The Impact of Direct Democracy on Society

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Table of Contents

Chapter I:
Introduction

1 Introduction

The topic of this dissertation, as its title suggests, is the societal impact of direct democracy, with a particular focus on Switzerland. This introductory chapter intends to shed light on this issue by providing a brief general description of the theoretical and empirical background and the general scientific approach the author intends to take. More issue-specific detail and in-depth information will be provided in the following chapters, each of which is dedicated to one aspect of this research problem.

'Direct democracy', or more precisely, direct legislation, implies the direct influence of the people on the political decision-making process whose outcomes gain shape in both laws and decrees of the parliament. This influence means that a modern democracy, which takes place solely through representative institutions of political decision-making, is complemented by direct-democratic institutions. The decisive institutions of direct legislation are referenda and initiatives, which in economic theory are thought of as influencing the political outcome in favor of the preferences of the citizenry (FELD and KIRCHGÄSSNER, 2001, BESLEY and COATE 2001).

'Societal impact' is a broad term that seemingly allows a huge scope of possible issues for analysis. To investigate this impact, I will use the so-called social indicators that can, on the one hand, be objectively measured but that, on the other hand, go beyond the mere measurement of financial or monetary aspects. Fortunately, these indicators must and therefore will be limited to those linked to public goods whose financial base for provision is directly affected by institutions of direct legislation[1]. This approach narrows the field of societal impact considerably, in this case to the following indicators: subjective well-being of residents, public safety, income redistribution, and education.

The approach of this dissertation is empirical and employs econometric methods of analysis. Such systematic methods have the advantage that the resultant scientific findings will be valid in a general sense, i.e. they will not be driven by single observations (statistical outliers) as might occur if a purely verbal-descriptive case study approach were chosen.

[1] In the case of Switzerland, these are typically those public goods provided by Swiss cantons in fields that enjoy a high degree of political and financial autonomy (see also the discussion in chapter VI on education). Such fields are especially police, education, culture, and welfare.

2 Theoretical and Empirical Background

According to some strands of economic theory, in a representative system, resources are wasted and allocations of goods and resources occur that deviate strongly from the median voter's position. On the one hand, overspending is caused by (a) politicians who exploit the budget (and implicitly the tax base) to satisfy the needs of the electorate in their local districts ('pork-barrel legislation')[2]; (b) the forming of broad coalition governments that leads to an inefficient expansion of budgets by the spending ministers ('budget as a common pool')[3]; or (c) logrolling in the political decision-making process, which brings about the financing of minority projects that would otherwise not have gained support from the parliamentary majority[4]. Moreover, (d) government administrations are headed by bureaucrats who aim at maximizing their budget and extracting rents, which might lead to a preference for those expenditure projects that also cause an increase in administrative spending (e.g. through the foundation of new departments, etc.)[5]. If the administrative budget is constrained, bureaucrats are thought to aim at increasing their personal administrative staff at the expenses of the resources available for carrying out their duties related to production as prescribed by law[6]. Whatever the case, according to these theoretical arguments in a purely representative system, expenses are undertaken that are not preferred by the median voter, leading possibly to an unwanted growth in budget, the provision of public goods that are not the first priority for or are supplied in a way undesirable to the median voter, or a deteriorating quality of the public good due to fewer means available for its provision.

Nevertheless, direct democratic institutions, many of their supporters argue, can serve as means to discipline the behavior of politicians and bureaucrats. Using a model of political economy, FELD and KIRCHGÄSSNER (2001) show that the mere existence of such institutions (playing the role of a 'credible threat') leads to an allocation of good and resources that is closer to the median voter's preferences than otherwise[7]. Therefore, three characteristics

[2] According to WEINGAST, SHEPSLE and JOHNSEN (1981), this leads to a preference of projects in districts of the winning party at the expense of those of the loosing party. See also TULLLOCK (1959).

[3] See ROUBINI and SACHS (1989), DE HAAN and STURM (1997), KONTOPOULOS and PEROTTI (1999), or VOLKERINK and DE HAAN (2001). See FELD et al., 2003 for an empirical test with Swiss data.

[4] See also BESLEY and COATE (1997, 1998) on inefficiencies in representative democracies, particularly through allowing the politicians to pursue their own goals between elections and the activities of interest groups. In addition, see BESLEY and COATE (2001) on the role of initiatives to break up bundling of projects.

[5] See NISKANEN (1975). See also BESLEY and COATE (2003) for representatives aiding bureaucrats to seek rents.

[6] See WILLIAMSON (1964).

[7] Deviation from the desired allocation could also be caused by the level of information asymmetry between politicians and citizens (KESSLER 2001), which might also be mitigated through the existence of institutions of

should be expected in direct democracies: first, less money is wasted on undesired projects because voters veto them through fiscal referenda and initiatives[8]; second, governmental budgets should be smaller than in less direct democratic systems because fewer financial projects and laws triggering new expenses are approved by the electorate if voters are fiscally conservative[9]; and third, public goods might be provided more efficiently as bureaucrats' discretionary power is also limited[10]. Smaller governmental budgets should in turn bring about lower income and/or property tax rates, thereby relaxing the individual's budget constraint in contrast to those of voters living in a more representative democratic system. In addition, because of the resulting increase in individual utility, persons living in a system with strong direct democracy should experience a higher level of well-being than those living under a purely representative political regime[11].

In fact, empirical evidence already partly corroborates most of these conjectures. In general, an allocation closer to the median voters' preferences in direct democracies is observed by POMMEREHNE (1978) for Switzerland[12] and MATSUSAKA (2000) for the U.S.A.[13]. As regards the budgetary and cost-reducing impacts of direct legislation, several studies based on typical public finance models for expenditure regressions show a spending and revenue lowering impact of direct democracy on combined cantonal and subfederal budgets, and an accumulation of less debt per capita in Swiss municipalities with stronger direct democratic

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direct legislation. For a discussion on the influence of interest groups, as well as why a mitigation of information asymmetry might take place in the Swiss political system particularly, see FELD and KIRCHGÄSSNER (2001) and KIRCHGÄSSNER (2000). A contrasting view on the impact of logrolling on the production of public goods is presented in BRETON (1996). He argues that in direct democracies an inefficiently low level of provision might be achieved because referenda on single projects prevent gains from vote trading. A strong counterargument can be found in FELD and KIRCHGÄSSNER (1998).

[8] For theoretical arguments, see BESLEY and COATE 1998, 2001, 2003; see also particularly chapter IV on redistribution.

[9] For theoretical arguments, see FELD and KIRCHGÄSSNER, 2001, and the discussion in chapters IV and VI.

[10] See the discussion in chapter VI on education and the references therein.

[11] See the various papers by FREY and STUTZER discussed in chapter III on happiness.

[12] As method, he compared the performance of an 'average voter' model with a 'median voter' model to explain levels of public expenditure for various subsamples grouped by the degree of direct democracy. The higher the degree of direct legislation, the better the explanatory power of the median voter variables. For additional supporting findings, see also GERBER (1996, 1999).

[13] See also FELD and KIRCHGÄSSNER (1998) for further references. Moreover, SCHALTEGGER and FELD (2001) show that stronger institutions of direct democracy (fiscal referenda) lead to a lower degree of centralization of expenses at the supra-local level. According to TIEBOUT (1956), decentralization brings about a more efficient allocation of goods and resources and a production of goods which is closer to the citizens' preferences. Hence, direct democracy might lead to efficiency gains via decentralization as transmission channel. This conjecture is supported by findings of FREY and STUTZER (2000, p. 928).

rights[14]. Similar empirical results have been obtained for the United States[15]. As regards the cost-reducing impact, POMMEREHNE (1983) reveals that institutions of direct democracy decrease the costs of garbage collection in Swiss municipalities. Further, and possibly as a result, FELD et al. (2003) show that income tax and property tax rates are lower in more direct democratic cantons[16]. In addition, FELD and SAVIOZ (1997) observe that GDP per capita is also greater in such cantons in Switzerland[17]. Given these results, the conjecture on the budgetary impact of direct legislation can be viewed as supported. A general corroboration of the shift of policy outcomes to the median voter position on nonbudgetary issues like parental consent laws and capital punishment is observed by GERBER for the U.S. (1996, 1999). Yet, given the spending dampening effect of direct democracy on budgets, how are public goods affected by direct legislation? [18] First, FELD and MATSUSAKA (2000), together with MATSUSAKA (1995a), discover that in direct democracies a stronger reliance on user charges exists that should make the quality of the public good more independent of the financial resources (tax revenue) of the cantonal government[19]. Second, as mentioned above, efficiency gains in the provision of a particular good are observed by POMMEREHNE (1983). Similarly, the findings presented in KIRCHGÄSSNER and HAUSER (2001) can be viewed as evidence that cost savings (induced through the fiscal referendum) occur rather in the expenditure for administration than in the financial resources available for the provision of public goods.

[14] See e.g. FELD and KIRCHGÄSSNER (2001) for the effect on cantonal and local revenue, expenditures and debt; FELD and KIRCHGÄSSNER (1999) for Swiss municipalities; FELD and MATSUSAKA (2003) on expenditure; KIRCHGÄSSNER and HAUSER (2001) on expenditure for administration; SCHALTEGGER (2001) for impact on various subfederal budget components; POMMEREHNE (1978) for Swiss municipalities; SCHNEIDER and POMMEREHNE (1983) on expenditure growth in Swiss municipalities, to name the most important contributions. An (incomplete but informative) overview can be found in FELD and KIRCHGÄSSNER 2000. The expenditure dampening impact of direct democratic institutions can be seen as evidence that "legislatures tend to spend much more than the median voter wants" (see FELD and MATSUSAKA, 2003, p. 2722). For evidence on the fiscal conservatism of voters in the U.S., see PELTZMAN (1992).

[15] See e.g. MATSUSAKA (1995a), KIEWIET and SZAKALY (1996), RUEBEN (1997), HOLCOMBE (1980) for U.S. states, and the work by SHADBEGIAN for local districts with TELs (see also the references in chapter VI). See also an overview in KIRCHGÄSSNER et al. (1999).

[16] In addition, the studies by POMMEREHNE and WECK-HANNEMANN (1996, 1989) reveal also a lower level of tax evasion (see next section for discussion). A decentralizing impact of direct democracy on tax revenue, income, and property tax revenues in particular, was detected in SCHALTEGGER and FELD (2001), i.e. in direct democratic cantons tax collection occurs more at the local level.

[17] See also FREITAG and VATTER (2000) for similar results, who find rather the use of institutions of direct legislation than their mere existence to be an important determinant.

[18] Lower spending alone is not a valid indicator of efficiency gains; it might be caused by inefficiencies through insufficient vote trading in the political decision-making process (see BRETON, 1996).

[19] See also POMMEREHNE (1978, 1983) and FELD and MATSUSAKA (1999).

Given these results, public goods do not appear to be negatively affected by the lower spending in more direct democratic political systems[20].

As argued earlier, because of all these detected impacts to the advantage of the citizens, a utility increasing effect can be expected. The works by FREY and STUTZER (2000, 2002) find empirical evidence in line with this conjecture using a cross section of 1992 data (LEU et al., 1997). These authors show first that direct democracy appears to lead to an increase in so-called outcome utility, which means happiness achieved through the resulting allocation of goods and resources. Moreover, they also reveal that the major part of the increase in life satisfaction can be attributed to so-called procedural utility, i.e. satisfaction gained through mere participation in the political process. A corroboration of the positive utility impact from another perspective might be deduced from the literature on tax evasion: In more direct democratic cantons, significantly less tax evasion occurs than in more representative democratic cantons, as the studies by POMMEREHNE and WECK-HANNEMANN suggest (1989, 1996) [21]. This finding could be viewed as an indicator of the satisfaction of tax payers with the policy-making carried out by their governments and administrations.

So far, literature speaking in favor of institutions of direct democracy has been examined. There are, however, also some theoretical and a few empirical studies[22] which shed light on its potential negative aspects which should not be neglected. Some of these arguments which follow have already been alluded to in the previous paragraphs. These counter-arguments can be grouped under the heading ‘cost aspect’, ‘interest groups’ and ‘political decision-making’.

As regards the costs of decision-making in direct democratic political systems, some economists suggest that a representative political system has an advantage over a direct democracy because it saves information costs and profits from efficiency gains through the division of labor between specialized politicians and the electorate (FELD and KIRCHGÄSSNER, 2003). Furthermore, the negative sides of direct legislation are also shown in a more specific theoretical model by MATSUSAKA and MCCARTY (2001) with includes a voter, a representative, preference uncertainty and costly initiatives. The authors' model suggests that

[20] A detrimental impact of direct democracy on health (infant mortality rate) and education (share of pupils with *maturité*/*Matura*) going beyond a budgetary one is found by BARANKAY (2002). These results, however, might suffer from misspecification as important fiscal and political determinants are missing in his model.

[21] See also more recent analyses by FELD and FREY (2001, 2002).

[22] Information on some empirical studies is provided in the footnotes of this section.

under specific circumstances the voter would be better off without an initiative than with such a means of direct legislation. Other costs involved with direct democratic decision-making are the costs of signature collection for initiatives or optional referenda. The higher the signature requirement is, that is the higher the costs of using this institution are, the less strong its correction of the political outcome will be. In line with this argument, a too high signature requirement will only induce costs to the voter without the general beneficial correctional impact (see FELD and KIRCHGÄSSNER, 2003, and the references therein). Another cost argument is provided by GRILLO (1997), who argues that given a substantially large size of the electorate, the decision-making, information and organizational costs of a direct democratic system would outweigh its benefits.[23]

In addition, a common critique is that in direct democracies there is an increased influence of even small, but well-organized interest groups on the political decision-making process and its outcome (see KIRCHGÄSSNER et al., 1999, p.20). Already during the parliamentary process of law-making the threat of taking the optional referendum - so the argument - is utilized by these groups to blackmail politicians.[24] In a theoretical model, (MATSUSAKA and MCCARTY, 2001) have shown that, when information is imperfect and asymmetric, it becomes easy for such groups to abuse these institutions. Hence, an allocation of goods and resources far away from the median voter's position is induced.[25]

Another group of arguments relates to the prerequisites of a participation of the electorate in direct democracies such that an allocation which is beneficial to society can be achieved. On the one hand, if voting took place purely for reasons of expression (BRENNAN and LOMASKY, 1993), the closeness of the political outcome to the median voter's preferences would be jeopardized (BRENNAN and HAMLIN, 2000). In such model, a representative political system is shown to mitigate these causes for misallocations of goods and resources. Related to this

[23] For counter-arguments, see KIRCHGÄSSNER et al., 1999, p. 33, and the references therein. Particularly, modern technology should decrease such costs.

[24] Consequently, BORNER et al. (1994, p. 129) call for a restriction of the referendum. In addition, they also suspect that the threat of taking the referendum serves as device to facilitate rent-seeking (ibid., p. 26 cont.).

[25] There is evidence by LASCHER et al. (1996) and CAMOBRECCO (1998) that government policy is not closer to the median voters' preferences in US states with initiatives. In MATSUSAKA (2001), however, it was shown that their results could be driven by misspecification of their empirical model. Evidence for an existing impact of interest groups was found by ZISK (1987, p. 90 cont.) for the US and SCHNEIDER (1985) for Switzerland. Counter-arguments based on casual observations and further empirical literature supporting either side is given in KIRCHGÄSSNER et al., 1999, pp. 25 cont. A general counter-argument is that the influence of interest groups exists also in representative political systems, but is more transparent in direct democracies (KIRCHGÄSSNER et al., 1999, p. 30).

aspect is the critique that citizens in direct democracies have no incentives to make well-informed decisions because the costs of information gathering are higher than the expected benefit (cf. DOWNS, 1957), which is particularly crucial for optional elements of direct legislation (see KIRCHGÄSSNER et al., 1999, p.20 and p.47 cont.).[26] Moreover, in an economic model, MARINO and MATSUSAKA (2005) show that politicians have an incentive to supply biased information to the citizenry in a world of partial delegation, as takes place in direct democracies, in order to prevent initiative- or referendum- induced budget cuts.[27] Another argument against direct democratic institutions, particularly referenda, is that they slow down the political decision-making process which makes this political system unable to react fast to economic or societal changes (see KIRCHGÄSSNER et al., 1999, p.20). This leads to what is called a 'Status Quo bias'.[28] Moreover, BRETON (1996) suggests that institutions of direct legislation prevent gains from logrolling, which implies vote trading, leading to a level of provision of public goods which is below the societally optimal level[29]. Finally, a related aspect is that means of direct legislation prevent binding compromises necessary for reaching international agreements.[30]

My own contribution to these societal aspects of influences of direct democracy will be twofold: first, after updating the index of direct democracy for the relevant time span, the analysis will use the new data to reinvestigate the happiness aspect of direct democracy; and second, the analysis will address some new transmission channels of direct democracy's influence on well-being, channels that are different from those mentioned above. Specifically, I will examine the impact of direct legislation on the public goods of income redistribution, public safety, and education. These empirical analyses will be carried out using Swiss cantonal data. The next section briefly summarizes why Switzerland is an appropriate country for studying such effects.

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[26] A similar argument is that a direct democracy requires a higher level of civic engagement than a representative political system, and a rational individual should not be expected to show such a level. For systematic and casual empirical counter-evidence against both arguments, see KIRCHGÄSSNER et al., 1999, pp. 53-68.

[27] See also related empirical evidence for the provision of public goods in districts with an override option in the United States, described in chapter VI.

[28] For counter-arguments and institutional solutions to this problem at the Swiss federal level (see KIRCHGÄSSNER et al., 1999, pp. 21 cont.). Particularly initiatives serve to 'accelerate' the political process. The Status Quo bias might also be positively interpreted as a higher degree of continuity and countability in policy-making (KIRCHGÄSSNER et al., 1999, p. 24).

[29] See also footnote 7. The paper by BARANKAY (2002) might serve as supportive empirical evidence.

[30] For counter-arguments, see KIRCHGÄSSNER et al., 1999, p. 33, and the references therein.

3 Switzerland as a Perfect Laboratory for Empirical Analysis

Few countries in the world offer direct-legislative opportunities to their people, particularly the opportunity to influence the daily political decision-making processes. Basically, only two nations offer an extensive range of such direct-democratic means: the United States of America and Switzerland[31]. The political system of Switzerland is shaped by two important characteristics. First, in this federal country, direct legislative institutions exist on all three levels – i.e. the federal level, the cantonal ('state') level, and the communal level. Second, Switzerland shows a very strong fiscal decentralization, which, in contrast to Austria and Germany, gives each level in the state its own tax sources. Therefore, a direct institutional link exists between the power to tax and the direct-legislative institutions that provide local citizens with the political means to influence both sides of the budget equally.

Thus, Switzerland can be viewed as a perfect laboratory for studying the impact of direct democracy on political outcomes (see also e.g. KIRCHGÄSSNER, 2002, 2000): each canton – or at the communal level, each commune – can be viewed as one observation with a varying degree of direct democracy laid down in its constitution. All the cantons and communes, however, share an identical macroeconomic and political framework at a higher level, so that some difficulties arising in a cross-national comparison can be avoided. This common framework is shaped by both policy at the federal level, international politics, and the economic situation of the rest of the world.

At the cantonal level, there exist two direct-legislative institutions that are of particular interest because they greatly affect the daily piecemeal works of politics: the fiscal referendum[32] and the statutory initiative (see FREY and STUTZER, 2000; see the review by KIRCHGÄSSNER 2002 for fiscal aspects). Whereas the fiscal referendum is of a reactive nature, the statutory initiative provides citizens with an agenda-setting power. Another difference between these institutions is their focus: the fiscal referendum deals with expenditure projects of the cantonal government – i.e. it is related more to decrees and by-laws; in contrast, the statutory initiative directly influences the law-making process – i.e. it is either used for

[31] In many other countries, however, such as Canada, Austria, Liechtenstein, Italy, Ireland, and France, a popular vote must only be held on constitutional amendments.

[32] An 'optional' fiscal referendum can be distinguished from a 'mandatory' one. They differ with respect to the expenditure threshold and the signature requirement, which must be met in the case of the latter. (See also chapter II).

placing new (proposed) laws on the political agenda or for revising or eliminating already existing laws[33]. Other important institutions of direct legislation are the constitutional initiative and the statutory referendum[34]. Chapter II of this dissertation describes in more detail the form that these four institutions take in the 26 cantons, how they have changed over recent years, and how they can be used to construct a measure of direct democracy for use in the subsequent analyses.

4 Structure of the Dissertation

The rest of this dissertation is organized as follows. The second chapter outlines and discusses in detail the development of important institutions of direct legislation between 1997 and 2003. The goal of this chapter is to provide a basis for an update of the composite index of direct democracy, which became necessary to carry out the empirical analyses presented in the subsequent chapters. The third chapter presents an analysis of the impact of direct democracy on individual well-being using a cross section of 1992 data and Swiss Household Panel data from 2000 to 2002. Chapter IV analyzes the impact of direct democracy on the efficiency of redistribution. The fifth chapter is dedicated to the question of how direct legislation affects levels of property crime, hate crime, and sexual offenses. Chapter VI presents an investigation into the impact of direct legislation on educational quality as measured by PISA-like test scores. These empirical chapters are structured like scientific papers and have already been presented as separate papers at various conferences; for this reason the reader might find some overlapping in the arguments presented. Finally, chapter VII concludes with a short summary and evaluation of the findings of this dissertation.

[33] The constitutional initiative aims at changing, eliminating, or adding a new constitutional law. Usually, the requirements for this initiative are the same as those for the statutory one, so that in econometric analyses the impact of the two cannot be distinguished. See also chapter II.

[34] In some cantons, even more specialized forms of referenda exist, such as those on highway expenditure or international treaties. See chapter II and TRECHSEL and SERDÜLT (1999) for more information.

Chapter II:

Direct Democracy 1997 – 2003

1 Introduction

This chapter describes the changes in direct democratic institutions that occurred in the 26 Swiss cantons between 1997 and 2003. In general, institutions of direct democracy are initiatives and referenda that are usually regulated in constitutional stipulations. This section contains a brief introduction to these instruments of direct democracy and a description of the so-called index of direct democracy that measures the strength of these institutions at the cantonal level in Switzerland.

On the one hand, initiatives constitute an element of active electoral participation in the political decision-making process. They allow the electorate to place a proposal for a law or constitutional change on the political agenda. Accordingly, we speak of a *statutory initiative* for altering laws and a *constitutional initiative* for a constitutional amendment. In Switzerland, any revision of the cantonal constitution must be approved by the electorate, i.e. it directly brings about a popular vote, as stipulated in the national constitution (art. 51, l Swiss Constitution (SC)). In case of an initiative that refers to a cantonal law, a popular vote must usually take place if the cantonal parliament makes a counterproposal, and only in some cantons does a popular vote generally follow an initiative (i.e. without a counterproposal). The requirement for any type of initiative is that a specific number of signatures is to be collected from among the electorate, and it is not uncommon for the signature requirement to increase proportionally to the importance of the legislative act to which the initiative pertains (e.g. law, partial revision of constitution, total revision of constitution).

Referenda, on the other hand, constitute a reactive element of direct legislation because they can only take the form of a reaction to preceding activities of the legislative body. Referenda at the cantonal level are, in general, applicable to laws, decrees, international and intercantonal treaties (concordats), and fiscal issues such as expenditure projects. Accordingly, we speak of a so-called *statutory referendum* for laws, decrees and by-laws (*Gesetzesreferendum*), an *administrative referendum* for administrative acts [35]

[35] An administrative act is not necessarily a decision made by an administration; the distinction between an administrative and a legislative act lies in the scope of applicability. In Switzerland, nomothetic acts are defined as acts always referring to the regulation of either obligation or rights of persons, organization of the state or duties and processes of administrations. Furthermore, administrative acts are not directly linked to financial expenses. The wording in the various cantonal constitutions related to legislative and administrative acts is not consistent between Swiss cantons. Administrative referenda are very difficult to identify as in many cantons administrative acts take the form of a legislative act. On the other hand, there is also an almost unmanageable variety of legal forms of legislative acts (see TRECHSEL and SERDÜLT 1999, pp. 14-15 and p. 31).

(Verwaltungsreferendum), a referendum for treaties (*Staatsvertragsreferendum*), and the *fiscal referendum* (*Finanzreferendum*). In theory, all referenda could exist in both a mandatory and optional form: a mandatory referendum triggers a popular vote automatically following a decision by the representative body, whereas an optional referendum must be held only if some specific requirements have been met by the electorate– usually a signature requirement[36]. In theory, all referenda can also exist in an ordinary or extraordinary form. If a referendum is ordinary, its (potential) application is directly stipulated in the constitution; it is, in a sense, part of the daily political process. An extraordinary referendum can be held if a minority of the representative organ of the canton demands it: in this case, the requirements for taking this extraordinary optional referendum will be stipulated in the legal act to which it refers. In general, not all types of referenda exist in one canton, and there is a huge variation in requirements between the Swiss states.

To ensure a minimum level of direct democracy in each Swiss canton, the federal constitution rules which institutions of direct legislation must exist at the cantonal level. Article 51, 1 SC states that 'each canton shall stipulate a democratic constitution', where 'democratic ' refers to the organization of the canton, particularly the division of power, and to the fact that the cantonal parliament is elected by the cantonal citizenry (EHRENZELLER et al. 2002, p. 624, no. 8). As regards institutions of direct legislation, as already mentioned above, only the constitutional referendum and the constitutional initiative (both for a partial and total revision of the constitution) are required in addition by art. 51, 1 SC (EHRENZELLER et al., 2002, p. 624, no. 9) [37]. Although at the federal level a statutory referendum is stipulated in its optional form (art. 141 SC), this institution is not prescribed for the Swiss cantons. Nevertheless, all Swiss cantons guarantee their citizens more direct democratic rights than the required minimum. In fact, the statutory referendum of direct legislation exists in all 26 Swiss cantons, as do most of the other institutions such as the fiscal referendum and the administrative referendum[38]. In addition, changes in cantonal territory are subject to approval through a popular vote of the affected populations and cantons (art. 53, 3 SC), as are secessions or unifications of cantons (art. 53, 2 SC).

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[36] In case of fiscal referenda, both forms require the meeting of a financial threshold in first place.

[37] Changes in the cantonal constitutions must be in accordance with the federal law that is ensured by the approval by the federal assembly (art. 51, 2 SC; art. 172, 2 SC). The federal assembly consists of two chambers: the federal parliament (*Nationalrat*) and the representatives of the 26 cantons (*Ständerat*) (art. 148, 2 SC). In general, any cantonal stipulation should not contradict federal law (federal law breaks cantonal law (art. 49, 1 SC)).

[38] For an overview of the (non)existence of the most important institutions of direct legislation in Swiss cantons, see LUTZ and STROHMANN (1998).

As regards the institutions of direct democracy to be analyzed in this chapter, they are restricted mainly to those that serve as a basis for constructing the so-called index of direct democracy as developed by STUTZER (1999). This index is an unweighted average of four subindices that measure the strength of four specific institutions of direct legislation; in particular, the initiatives for constitutional and statutory changes, the fiscal referendum (ordinary optional and mandatory) on expenditure projects, and the referendum for laws (and decrees)[39]. To follow this chapter's development more easily, the reader should recall that these subindices are based on an evaluation of the requirements for each institution, on the one hand, the signature requirements necessary for optional referenda and initiatives, and on the other, the financial threshold for the (optional and mandatory) fiscal referenda. In order to provide as complete a picture of constitutional changes in institutions of direct democracy as complete, however, the discussion also lists most of the changes in administrative referenda and any extraordinary statutory referenda. The administrative referenda are listed in the section on statutory referenda, because with respect to their political intention, they are closely related to the statutory referendum.

The original values of the index of direct democracy were presented by STUTZER (1999) and STUTZER and FREY (2000) for the years 1970, 1992, and 1996 and were then updated for the missing years between 1980 and 1998 by FELD and SCHALTEGGER (see their various articles in the list of references). The data used for constructing this index up until 1996 can be found in TRECHSEL and SERDÜLT (1999)[40] (hereafter cited as T/S in this chapter), in which the authors analyze the institutions of direct democracy and describe the changes from 1970 onwards (T/S 1999, p. 8). The index of direct democracy has since been employed in various time-series cross-sectional analyses of the impact of direct democracy and is thus essential[41]. For the purposes of this dissertation an update for the years 1997 – 2003 became necessary because the data used to investigate the effect on education, crime, and subjective happiness encompass only these more recent years[42]. It should also be noted that neither TRECHSEL and SERDÜLT (1999) nor STUTZER (1999) analyze the cantonal institutions of the so-called

[39] In many cantons, the requirements for the statutory and administrative referendum are identical. However, in some cantons they differ, and STUTZER (1999) used either their average or either one of them to construct the subindex of the statutory referendum.

[40] Sporadically, the description of the changes reaches 1998, particularly in the second part in TRECHSEL and SERDÜLT (1999) in which each single cantonal constitution is analyzed.

[41] E.g., see the literature cited in KIRCHGÄSSNER (2002, 2001, 2000).

[42] Differences between the values constructed by FELD and SCHALTEGGER (for 1997 and 1998) and my own are mainly due to the extrapolation of old values without taking into account changes in the underlying cantonal population. See also section 7 in this chapter.

Landsgemeinde cantons, i.e. those cantons that know no form of legislative representation or delegation but vote on everything in an open meeting[43]. These *Landsgemeinde* cantons excluded from the analysis in T/S (1999) are *Appenzell Innerrhoden*, *Appenzell Ausserrhoden*, *Nidwalden*, *Obwalden*, and *Glarus* because in 1996 they were either still a *Landsgemeinde* or had been so until recently (see T/S 1999, p. 7) [44].

The update of this index would not have been possible without the actual wording of the changes provided by courtesy of various cantonal legal services and administrations. Therefore, at this point I would like to thank all the various civil servants and honorary collaborators, most particularly, Mr. Jürg Fehner (ZH), Mr. Urs Rüegg (ZH), Ms. Elisabeth Vetter (LU), Ms. Kathrin Graber (LU), Ms. Judith Lauber, Mr Georg Zemp (LU), Mr. Heinz Bachmann (LU), Mr. Peter Huber (UR), Mr. René Zehnder (SZ) and Mr. Peter Gander (SZ), Mr. Notker Dillier (OW), Mr. Josef Baumgartner (NW), Mr. Hansjörg Dürst (GL), Mr. Bruno Zimmermann (ZG), Mr André Schoenenweid (FR), Ms. Yolanda Studer (SO), Dr. Denise Mangold (BS), Mr. René Bolliger (BL), Mr. Christian Ritzmann (SH), Mr. Joe Müggler (AR), Mr. Köbi Frei (AR), Gabriela Küpfer (SG), Mr. Walter Frizzoni (GR), Mr. Urs Meier (AG), Mr. Claudio Franscini (TI), Mr. Alex Depraz (VD), Ms. Séverine Despland (NE), and Mr. Jean-Jacques Tombet (GE). I also thank ALOIS STUTZER, the inventor of this index, for helpful comments and clarifications.

The rest of this chapter is organized as follows. The next section provides an overview of the changes in the cantonal constitutions from 1997 to 2003. The subsequent sections separately examine the institutional changes for the relevant direct democratic institutions and describe their actual provisions. Addressed by these sections are the constitutional initiative, the statutory initiative, the mandatory and the optional statutory referendum, and, finally, the mandatory and the optional fiscal referendum. The outcome of this research is organized in the form of tables intended to serve as a 'manual' for information on the relevant constitutional requirements and for further consultation. These tables also allow construction of further controls of direct democratic institutions, such as the signature requirements or the financial threshold requirements for various time points between 1997 and 2003. Finally, the implications of these changes for the index are described and the constructional changes of the index are discussed. The chapter concludes with a brief critique of the index.

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[43] In various papers by STUTZER and FREY (e.g. 2000) index values for the *Landsgemeinde* cantons are also reported, without, however, a detailed description of their relevant constitutional stipulations.

[44] Today (2003), 2 cantons (*Glarus* and *Appenzell Innerrhoden*) are still *Landsgemeinden*; *Nidwalden* ceased before 1996, *Appenzell Ausserrhoden* in 1997, and *Obwalden* in 1998.

2 Constitutional Changes between 1997 and 2003

The time span between 1997 and 2003 appears to have been a period favoring the adoption of new constitutions. Table 2.1 displays a list of all 26 cantons and their constitutional changes that affect those direct democratic institutions on which the index of direct democracy is based (state: 18th of August, 2004). The cantons FR, SH, SG, GR, TI, VD, and NE – about one fourth of all Swiss cantons – all passed new, totally revised constitutions between 1997 and 2004 (the canton of LU is also currently drafting a new constitution, which should be voted upon in 2007[45]). The reason for this 'clustering' could lie both in a "Year 2000" effect combined with a "200-years jubilee" effect of membership in the Swiss federation, which emerged from a confederation of single independent states. However, not all of these new constitutions necessarily brought about alterations in the existing direct democratic institutions: some of the new constitutions aimed primarily at rewriting the outdated nineteenth-century wording and giving it a more modern structure without altering its legal content. The rest of this section is devoted to an assessment of the amendments in the constitutional stipulations.

With respect to the mandatory and optional statutory referendum, there is a weakening of the direct democratic institution through a shift of the political power from the electorate toward the cantonal parliament. Quite often, the application of the mandatory statutory referendum has been restricted by the inclusion of a majority requirement of the cantonal parliament in the constitution (SO, BL, SH, AG) or by restriction of the issues to be regulated through legal stipulation (GL)[46]. Alternatively, the mandatory statutory referendum has been completely abolished and replaced by an optional statutory referendum, as happened in four cantons (ZH, OW, AR, GR (2004)). In general, such changes lead to a decline in the index of direct democracy in these cantons.

[45] See www.neueverfassung.lu.ch (7/10/2004) and personal communication with Mr. ZEMP (20/04/2005).

[46] In case the majority restriction serves only as a means for disciplining the cantonal parliament, the introduction of such a majority restriction has to be regarded as equivalent to a de facto elimination of the mandatory referendum. Only observation of the daily political usage over a longer period of time can provide a basis for a correct evaluation of this institution.

Table 1: Changes of the Institutions of Direct Democracy from 1997 to 2003

Canton	Institutional Changes between 1997 and 2003 (dates of effectiveness)	Comments	Index (1996)[47] (STUTZER, 1999)	Index (2003)
ZH	GRR*: 1 Jan 1999, FRR*: 1 Jan 1999		4.417	3.500
BE			3.020	3.021
LU			4.420	4.417
UR	VIR, GIR, GRR: 1 Oct 1997 FRR: 1 Oct 1997		5.290	5.125
SZ			4.990	4.927
OW	GIR, GRR*: 29 Nov 1998 FRR*: 8 June 1997, 29 Nov 1998	Abolishment of *Landsgemeinde* (29 Nov 1998)		4.625
NW				4.438
GL	GRR*: 5 May 2002 FRR: 5 May 2002	*Landsgemeinde*		5.500
ZG			4.420	4.479
FR		New constitution by 1 Jan 2005	2.850	2.792
SO	GRR*: 11 Dec 1998 FRR: 11 Dec 1998		5.670	5.250
BS	FRR: 1 Jan 1998	Change in cantonal law	4.400	4.396
BL	GRR*: 1 Jan 2000		5.690	5.479
SH	GRR*: 1 Jan 2003 FRR: 1 Jan 2003	New constitution by 1 Jan 2003	5.210	5.021
AR	GRR*: 1 June 2000	Abolishment of *Landsgemeinde* (28 Sept 1997)		4.917
AI	FRR: 28 Apr 2002	*Landsgemeinde*		5.438
SG	VIR, GIR: 1 Jan 2003 GRR: 1 Jan 2003 FRR: 1 Jan 2003	New constitution by 1 Jan 2003 *Einheitsinitiative*	3.580	3.521
GR		New constitution by 1 Jan 2004	4.750	4.833
AG	GRR*: 1 Jan 2003 FRR: 1 Jan 2003		5.460	5.438
TG			4.330	4.333
TI	GRR: 1 Jan 1998 FRR: 1 Jan 1998	New constitution by 1 Jan 1998	2.100	2.250
VD	VIR, GIR: 1 Sept 2003 FRR*: 1 Sept 2003, 29 Nov 1998	New constitution by 1 Sept 2003	2.420	2.417
VS			3.580	3.583
NE	GRR: 1 Jan 2002, FRR*: 1 Jan 2002	New constitution by 1 Jan 2002	2.190	2.729
GE			1.750	1.750
JU			4.020	3.708

The constitutions and the changes tracked for them are based on their newest versions as of the 18th of August, 2004. Updated cantonal constitutions can be obtained from http://www.admin.ch/ch/d/sr/13.html#131. In case of FR the source was http://www.fr.ch/constituante/doc/fichiers/proj_def/proj.pdf. Empty cells indicate that no relevant revision of the constitution or cantonal law had been observed. FRR: Fiscal Referendum, GIR: Statutory Initiative, GRR: Statutory Referendum, VIR: Constitutional Initiative. *indicates abrogation or restriction of a mandatory referendum.

[47] The value of ZÜRICH has been corrected. See section 7 of this chapter.

Looking at the development of the statutory initiative and the constitutional initiative, three interesting changes should be noted. First, more and more cantons list these two existing types of initiatives under the identical heading in their constitution in order to group them together and indicate them as two possible variations of a people's initiative. This integration is particularly true for most of the new constitutions, and is part of a trend to make constitutions more transparent to the average citizen. In the old constitutions, the description of the people's rights was subject to a strict constitutional structure that reflected the organization of the state as understood in the nineteenth century. Traditionally, revisions of the constitution (including revisions initiated by the people) were given a separate chapter in the back part of the constitution, not only symbolically almost outside the daily law-making process. The statutory initiative, in contrast, was placed in the middle, linked to the legislative process. For this reason, integrating both initiatives under one heading constitutes not only a formal change but indicates a change in the underlying philosophy and understanding of the purpose of a constitution.

Linked to this change is a second phenomenon: the introduction of the unitary initiative (*Einheitsinitiative*) (e.g. in *St. Gallen* art. 43 of the new constitution), which also aims to make it easier for the citizens to influence the legislative process. The unitary initiative is a binding motion of the electorate for which no specific legal form is required. During the readings (*Lesungen*) for the new cantonal constitution in *St. Gallen*, the introduction of the *Einheitsinitiative* was under hot debate and the most important aspects of this institution were named (PROTOCOL 2000, PROTOCOL 2000a). According to the various speakers, the advantages of this new type of initiative are that it is relatively easy to carry out and that it is the legislative organ and not the initiator who decides whether a change in cantonal law or in cantonal constitution is the appropriate (re)action. This latter means that unitary initiatives can no longer be turned down on the formal ground that the wrong level of law-making had been chosen by the initiators, e.g. the proposal of a change in cantonal law when an amendment of the constitution would have been correct and vice versa. Additionally, the *Einheitsinitiative* allows the development of the cantonal law and the constitution in a juristically consistent and systematic way.

Nevertheless, the *Einheitsinitiative* should not be seen as a perfect substitute for the traditional initiatives, because it serves purely as a device to induce the cantonal parliament to become active. Both statutory and constitutional initiatives are viewed as stronger instruments than the *Einheitsinitiative* because both the legal level of application and the content of the change are

precisely specified. Moreover, the *Einheitsinitiative* serves as a mere suggestion of the electorate, and the cantonal parliament can decide whether to follow it or not. If the parliament decides against, a referendum must be held. If it decides in favor and passes a law, this law is then subject to the optional statutory referendum. If a constitutional change is chosen, the cantonal people again have the final say in a popular vote. Overall, some speakers in the cantonal parliament of *St. Gallen* pointed out, a hierarchy of types of initiatives can be construed in which the unitary initiative is at the lowest level of influence. Because of this hierarchy of initiatives, differences in the signature requirements appear justified. There are, however, cantonal constitutions in which no differentiation is made between the number of signatures required for the unitary versus the statutory initiative[48].

The third phenomenon linked to the initiative but also to the fiscal referendum is that more and more cantons tend to regulate the time period for collecting signatures or the financial threshold in the constitution itself and not in a cantonal law on political rights of its citizenry. The advantage of this development could be increased transparency because now all requirements concerning one institution are laid down in the same legal act. Another advantage, depending on whether a mandatory statutory referendum exists in a canton or not, might be that a change in constitution must be approved by the electorate through a mandatory referendum[49], whereas an alteration of a cantonal law might only be subject to an optional statutory referendum. In this latter situation, political economic theory predicts that larger deviations from the median voter's preferences will occur than in the former (FELD and KIRCHGÄSSNER 2001). On the other hand, again depending on whether differences exist regarding the signature requirements for a statutory or constitutional initiative, it might have become more difficult for the electorate to change that requirement if regulated in a constitution. In political practice, however, between 1980 and 1998, time periods for collection or financial thresholds, whether regulated by a law or the constitution, were rarely changed over time. Additionally, for all cantons during our investigation period, regulation at a new, higher level of lawmaking failed to bring about a different (shorter) time period for signature collection.

[48] Because of its potentially low level of political influence and because it only complements the existing traditional initiatives, the *Einheitsinitiative* does not (yet) form part of the index of direct democracy. Only the future will show how important the institution becomes to the daily political process and whether it should be included in the index construction or not.

[49] As required by art. 51, 1 SC, see also section 1.

Table 1 provides information on how the index changed between 1996 (as found in STUTZER, 1999) and 2003 (based on own calculations). A more detailed discussion on such change is provided in section 7 of this chapter. It must also be noted that not all institutional changes automatically cause a change in the index of direct democracy: index points are awarded based on ranges of signature requirements or financial thresholds so that relatively small alterations do not necessarily translate into a change in category. In addition, the reader should keep in mind that each institution influences the composite index by only 25%: e.g. the change in signature requirement in *Basel-Land* (BL) for the optional statutory referendum caused a total change of just about 0.21 index points. At this point, I would like to emphasize that because the index is constructed on a yearly basis, revisions of the cantonal constitution becoming effective after April 1st are always counted as changes in the index of the subsequent year. Therefore, if a cantonal electorate approves a revision in September 1997, becoming effective the same date, it is the index value of the subsequent year 1998 that will be adapted accordingly. Moreover, it is also possible that several, contrasting institutional changes – either occurring simultaneously or subsequent to each other – might cause a compensating impact on the index of direct democracy. Finally, it should be noted that changes in the index can occur even if the institutional requirements do not change: such changes happen because some evaluations use per capita values, meaning that fluctuations in population often lead to a change in category.

3 The Initiative

3.1 Constitutional Initiative

Table 2: The Constitutional Initiative between 1997 and 2003

Canton	Signature Requirement	Articles in cant. constitution	Effective since	Time-limit for collection
Zürich (30 Mar 2004)	10,000	art. 29, 3 num.1	11 June 1969 (1 June 1969)	6 months art. 13, 2 *Initiativgesetz* (art. 126 *GPR*)
Bern (21 Oct 2003)	30,000 (TR) 15,000 (PR)	art. 58, 1 lit. a art. 58, 2	1 Jan 1995	6 months see also T/S 1999, p. 190

Table 2: The Constitutional Initiative between 1997 and 2003 (cont.)

Canton	Signature Requirement	Articles in cant. constitution	Effective since	Time-limit for collection
Luzern (21 Oct 2003)	5,000	art. 32, 1 art. 35 bis, 2	1 Jan 1994 (28 Nov 1993) 1 Sept 1970 (7 June 1970)	1 year art. 136 lit. a *SRG* See T/S 1999, p. 265.
Uri (1 Apr 2003)	600	art. 27, 1 art. 28, 2	1 Oct 1997 (8 June 1997)	None
Uri (old)	300	art. 27, 1 art. 28, 2	(since 1985)	None See T/S 1999, p. 362
Schwyz (18 Aug 2004)	2,000	art. 102, lit. a art. 103, lit. b	21 Dec 1899 (23 Oct 1898)	None See also T/S 1999, p. 307
Obwalden (22. Oct 2002)	500	art. 61, 1 lit. a (until Nov 1998: art. 63 num. 1)	19 May 1968	None 29 Nov 1998 (29 Nov 1998): abolishment of the *Landsgemeinde*) changes in 1998 do not affect institution
Nidwalden (28 Dec 2001)	1,000 (TR) 500 (PR)	art. 54, 4, num. 1 art. 54, 4, num. 2 (Art 54a Abs.4)	1 Dec 1996 (1 Dec 1996)	2 months
Glarus (30 Mar 2004)	1	art. 138, 3	1 May 1988 (1 May 1988)	None *Landsgemeinde*
Zug (1 Apr 2003)	2,000	art. 79, 2 art. 35, 1	2 Dec 1990 (2 Dec 1990)	None (Rules for the statutory initiative apply) See T/S 1999, p. 397
Freiburg (18 Aug 2004) new constitution by 1 Jan 2005	6,000	art. 41a art. 42, 2	1 Jan 2005 (16 May 2004)	90 days
Freiburg (old)	6,000	art. 78, 2 lit. b	3 Aug 1979 (24 Sept 1978)	90 days art. 115, 2 *PRG* art. 193, 3 *PRG* (old) see also T/S 1999, p. 214
Solothurn (30 Mar 2004)	3,000	art. 29, I lit. a art. 30, 3	1 Jan 1988 (8 June 1986)	18 months
Basel-Stadt (4 July 2000)	4,000	art. 28, 1	3 June 1991 (2 June 1991)	None See T/S 1999, p. 166
Basel-Land (22 Oct 2002)	1,000	art. 28, 1	1 Jan 1987 (4 Nov 1984)	None See also T/S 1999, p. 144

Table 2: The Constitutional Initiative between 1997 and 2003 (cont.)

Canton	Signature Requirement	Articles in cant. constitution.	Effective since	Time-limit for collection
Schaffhausen (21 Oct 2003) new constitution by 1 Jan 2003	1,000	art. 27, 1 lit. a	1 Jan 2003 (22 Sept 2002)	None
Schaffhausen (old)	1,000	art. 108 (TR) art. 107 (PR) art. 43, 2	6 Apr 1997 (6 Apr 1997) 1. June 1876 (14 May 1976) 20 May 1973 (29 Jan 1973)	None Change of 1997 concerns only TR and does not affect the VIR-subindex See T/S 1999, p. 290
Appenzell AR (3 Apr 2001)	300	art. 51	1 May 1996 (30 Apr 1995)	None
Appenzell IR (30 Mar 2004)	1	art. 7 bis art. 48	25 Apr 1982 (25 Apr 1982)	*Landsgemeinde*
St. Gallen (9 July 2002) new constitution by 1 Jan 2003	8,000	art. 41, 1 lit. a, b art. 45	1 Jan 2003 (10 June 2001)	5 months
St. Gallen (old)	8,000	art. 114 num. 2 art. 115	1926 see T/S 1999, p. 333	6 months
Graubünden (6 July 2004) new constitution by 1 Jan 2004	4,000	art. 12, 1	1 Jan 2004 (18 May 2003 / 14 Sept 2003)	1 year art. 53c, 1 *GPR*
Graubünden (old)	5,000	art. 54, 3	2 Mar 1980 (2 Mar 1980)	1 year art. 53c, 1 *GPR*
Aargau (30 Mar 2004)	3,000	art. 64, 1	1 Jan 1982 (28 Sept 1980)	1 year art. 54, 1 *GPR*
Thurgau (22 Oct 2002)	4,000	art. 26	1 Jan 1990 (4 Dec 1988)	6 months
Tessin (30 Mar 1999) new constitution by 1 Jan 1998	10,000	art. 83 (art. 119, 2 *LEDP*)	1 Jan 1998 (14 Dec 1997)	60 days art. 119, 1 *LEDP*
Tessin (old)	10,000	art. 54, 1 lit. c art. 56	1970 (31 May 1970)	60 days art. 3 *LIRR* see T/S 1999, p. 342
Waadt (21 Oct 2003) new constitution by 1 Sept 2003	12,000 (PR) 18,000 (TR)	art. 78 lit. a art. 79, 2	1 Sept 2003 (22 Sept 2002)	4 months (contradicts *LEDP*, art. 92, 1)

Table 2: The Constitutional Initiative between 1997 and 2003 (cont.)

Canton	Signature Requirement	Articles in cant. constitution.	Effective since	Time-limit for collection
Waadt (old)	12,000	art. 100	1961 (11 June 1961) see T/S 1999, p. 373	3 months art. 92, 1 *LEDP*
Wallis (21 Oct 2003)	6,000	art. 100, 1	1 June 1994 (24 Oct 1993)	None
Neuenburg (16 Oct 2001) new constitution by 1 Jan 2002	10,000 (TR) 6,000 (PR)	art. 101, 1 art. 102, 1	1 Jan 2002 (24 Sept 2000)	6 months art.105, 1 *LDP*
Neuenburg (old)	10,000 (TR) 6,000 (PR)	art. 83, 1 art. 84, 1 and 2	1959 see T/S 1999, p. 275	6 months art. 105, 1 *LDP*
Genf (21 Oct 2003)	10,000	art. 64 art. 65A	(7 Mar 1993)	4 months art. 89, 1 lit. c *LEDP*
Jura (4 July 2000)	2,000	art. 75, 1	1 Jan 1979 (20 Mar 1977)	12 months Art 89, 1 *LDP*

See also table 1. In the column 'Canton', the date of the version of the cantonal constitution (or the date of the download, the 18th of Aug, 2004) is displayed in brackets. TR denotes total revision and PR partial revision of the cantonal constitution; if not otherwise indicated, the signature requirement applies to both initiatives equally. Signature requirements relate to the corresponding cantonal electorate. The *Einheitsinitiative*, which does not enter the index of direct democracy, is not considered. In the column 'Effective since' the date of the popular vote is given in brackets. For an explanation of the abbreviation of the cantonal laws on political rights and their dates of enactment, see table A.1 in the Appendix.

3.2 Statutory Initiative

Table 3: Requirements for a Statutory Initiative between 1997 and 2003

Canton	Signature Requirement	Articles in cant. constitution	Effective since	Time-limit for collection
Zürich (30 Mar 2004)	10,000	art. 29, 3 num. 1	11 June 1969 (1 June 1969)	6 months see constitutional initiative
Bern (21 Oct 2003)	15,000	art. 58, 1 lit. b art. 58, 2	1 Jan 1995	6 months see also T/S 1999, p. 191
Bern (21 Oct 2003)	4,000	art. 41 bis, 1	1 Sept 1970 (7 June 1970)	1 year art. 136 lit. a *SRG*
Uri (1 Apr 2003)	600	art. 27, 1 art. 28, 2	1 Oct 1997 (8 June 1997)	None

Table 3: Requirements for a Statutory Initiative between 1997 and 2003 (cont.)

Canton	Signature Requirement	Articles in cant. constitution	Effective since	Time-limit for collection
Uri (old)	300	art. 27, 1 art. 28, 2	(since 1985)	None See T/S 1999, p. 362
Schwyz (18 Aug 2004)	2,000	art. 31, 2	21 Dec 1899 (23 Oct 1898)	None See T/S 1999, p. 308
Obwalden (22 Oct 2002)	500	art. 61, 1, lit. b	29 Nov 1998 (29 Nov 1998)	None Nov 1998: abolishment of the *Landsgemeinde*
Obwalden (8 June 1997)	1	art. 63, num. 2	19 May 1968	None
Nidwalden (28 Dec 2001)	250	art. 54, 4, num. 3 art. 54, 5	1 Dec 1996 (1 Dec 1996)	2 months
Glarus	1	art. 69, 1 art. 58, 1	5 May 2002 (5 May 2002)	None *Landsgemeinde* Only important regulation in form of law
Glarus (old)	1	art. 69, 1 lit. b art. 58, 1	1 May 1988 (1 May 1988)	None *Landsgemeinde*
Zug (1 Apr 2003)	2,000	art. 35, 2	2 Dec 1990 (2 Dec 1990)	None See T/S 1999, p. 397
Freiburg (18 Aug 2004) new constitution by 1 Jan 2005	6,000	art. 41 lit. b art. 42, 2	1 Jan 2005 (16 May 2004)	90 days
Freiburg (old)	6,000	art. 28 ter, art. 28 quarter	11 Mar 1921 (30 Jan 1921)	90 days art 115, 2 *PRG* art. 193, 3 *PRG* (old) see also T/S 1999, p. 215
Solothurn (30 Mar 2004)	3,000	art. 29, 1 lit. b art. 30, 3	1 Jan 1988 (8 June 1986)	18 months
Basel-Stadt (4 July 2000)	4,000	art. 28, 1	3 June 1991 (2 June 1991)	None
Basel-Land (22 Oct 2002)	1,000	art. 28, 1	1 Jan 1987 (4 Nov 1984)	None See also T/S 1999, p. 145
Schaffhausen (21 Oct 2003) new constitution by 1 Jan 2003	1,000	art. 27, lit. b	1 Jan 2003 (22 Sept 2002)	None
Schaffhausen (old)	1,000	art. 43, 2	20 May 1973 (29 Jan 1973)	None

Table 3: Requirements for a Statutory Initiative between 1997 and 2003 (cont.)

Canton	Signature Requirement	Articles in cant. constitution	Effective since	Time-limit for collection
Appenzell AR (1 Apr 2001)	300	art. 51	1. May 1996 (30 Apr 1995)	None
Appenzell IR (30 Mar 2004)	1	art. 7 bis	25 Apr 1982 (25 Apr 1982)	*Landsgemeinde*
St. Gallen (9 July 2002) new constitution by 1 Jan 2003	6,000	art. 42 art. 45	1 Jan 2003 (10 June 2001)	5 months
St. Gallen (old)	4,000	art. 49	1926 see T/S 1999, p. 334 11 Apr 1996 (*RIG*)	3 months art. 41, 1 *RIG*
Graubünden (6 July 2004) new constitution by 1 Jan 2004	3,000	art. 12, 2	1 Jan 2004 (18 May 2004, 14 Sept 2003)	1 year art. 53c, 1 *GPR*
Graubünden (old)	3,000	art. 3, 1	2 Mar 1980 (2 Mar 1980)	1 year art. 53c, 1 *GPR*
Aargau (30 Mar 2004)	3,000	art. 64, 1	1 Jan 1982 (28 Sept 1980)	1 year art. 54, 1 *GPR*
Thurgau (22 Oct 2002)	4,000	art. 26	1 Jan 1990 (4 Dec 1988)	6 months
Tessin (30 Mar 1999) new constitution by 1 Jan 1998	7,000	art. 37 art. 119, 3 *LEDP*	1 Jan 1998 (14 Dec 1997)	60 days art. 119, 1 *LEDP*
Tessin (old)	7,000	art. 59, 4 art. 56	1970 (31 May 1970)	60 days art. 3, 3 *LIRR* see T/S 1999, p. 343
Waadt (21 Oct 2003) new constitution by 1 Sept 2003	12,000	art. 78 lit. b art. 79, 2	1 Sept 2003 (22 Sept 2002)	4 months (contradicts *LEDP*, art. 92, 1)
Waadt (old)	12,000	art. 27, 1	1978 (4 Dec 1977) see T/S 1999, p. 373	3 months art. 92, 1 *LEDP* see also T/S 1999, p. 373
Wallis (21 Oct 2003)	4,000	art. 33, 1	1 June 1994 (24 Oct 1993)	None

Table 3: Requirements for a Statutory Initiative between 1997 and 2003 (cont.)

Canton	Signature Requirement	Articles in cant. const.	Effective since	Time-limit for collection
Neuenburg (16 Oct 2001) new constitution by 1 Jan 2002	6,000	art. 40, 1 art. 40, 2	1 Jan 2002 (24 Sept 2000)	6 months art. 105, 1 *LDP*
Neuenburg (alt)	6,000	art. 38, 2 art. 38. 3	1960 see T/S 1999, p. 276	6 months art. 105, 1 *LDP*
Genf (21 Oct 2003)	10,000	art. 64 art. 65B	(7 Mar 1993)	4 months art. 89, 1 c *LEDP*
Jura (4 July 2000)	2,000	art. 75,1	1 Jan 1979 (20 Mar 1977)	12 months art. 89, 1 *LDP*
See table 2.				

3.3 Comments on the Changes in Constitutional and Statutory Initiatives

Tables 2 and 3 provide an overview of the 26 Swiss cantons and their constitutional stipulations concerning the constitutional and the statutory initiative between 1997 and 2003. In several cantons, alterations in these institutions of direct legislation have occurred since 1997. In the constitution of *Zürich*, the time period available for constitutional and statutory initiatives is not fixed (art. 29, 4 CC)[50]; instead, it is stipulated that a cantonal law should determine this limit. As of the 1st of June, 1969, the time period for signature collection was set at 6 months for people's initiatives (art. 13, 2 of the *Initiativgesetz*). On the 1st of September, 2003, a new cantonal law on political rights (*Gesetz über die politischen Rechte, GPR*) was passed in the *Kantonsrat* that fixed an identical time period (art. 126, 1 *GPR*) and became effective the 1st of January, 2005. In other words, in terms of the requirements for statutory and constitutional initiatives, no changes that affect the index of direct democracy have been undertaken in *Zürich* since 1969, despite the enactment of a new cantonal law on political rights.

Uri is one of the cantons in which important changes in the requirements for the constitutional and statutory initiatives occurred after 1997. In this canton, the number of required signatures doubled from 300 to 600 as of the 1st October, 1997 (art. 28, 2 CC; art. 27, 1 CC), which

[50] CC stands for cantonal constitution.

caused a duplication of the relative signature requirement from 1.18 per voter to 2.36 per voter and, finally, lowered the subindex for either initiative from 1998 onwards. Since the identical stipulations are valid for both initiatives, an identical drop is noted for the subindex of both types of initiatives. The time period for collection, however, was not altered by this constitutional revision.

In the canton of *Obwalden*, on the 29th of November, 1998, the electorate decided against maintaining the status of *Landsgemeinde*. This choice meant that (a) the open vote in a yearly citizens' meeting was replaced by a secret vote at the ballot box and (b) the role of the representative organ of legislation was strengthened. As regards the constitutional initiative, however, no change in institutional requirements occurred after 1968 except for a formal renumbering of articles in the constitution. In contrast, as regards the statutory initiative, the abolishment of the *Landsgemeinde* in November 1998 brought about a substantially higher signature requirement than before. As a result, the stipulated number of supporters rose from 1 in June 1997 to 500 in November 1998, which caused a jump in the relative signature hurdle from about 0.005 to 2.27. The time period available for collection, however, remained the same. In effect, this change led to a substantial decline in the subindex for the statutory initiative.

For the canton of *Nidwalden*, there have been no apparent changes in the institutional requirements for either initiative since 1996. It might be interesting to point out, however, that in addition to the usual initiatives, the citizens can also make a counterproposal to an existing decision of the *Kantonsrat* either to revise the constitution or to make a new/change in cantonal law. This institution is very similar to the initiative – in a way it can be viewed as a reactive initiative. The requirements for the counterproposals are identical for the statutory initiative and the constitutional initiative, respectively (see art. 54a, 4 CC).

The canton of *Glarus* is one of the two cantons in which direct democracy in the form of the *Landsgemeinde* still exists. In this canton every citizen has the right to make a motion to the *Landsgemeinde*, the assembly of its citizens, on issues that concern the *Landsgemeinde* (art. 58, 1 CC). Hence, according to art. 58, 1 CC and art. 138, 3 CC, it takes one vote to launch a constitutional or statutory initiative. Art. 69 CC then specifies the fields on which the *Landsgemeinde* can exert its decision-making power, which is comprised of the constitution and cantonal laws. This stipulation, however, was amended in May 2002, leading to a

seemingly indirect restriction of the statutory initiative[51], because only fundamental regulations should be determined in the form of laws, whereas before 2002 any issue could have been in a statutory form. The intention of this new regulation was to give the cantonal parliament the power to regulate organizational issues, particularly the execution of the federal law, which in Switzerland is carried out by cantonal administrations, in the form of parliamentary decrees that, as administrative acts, cannot be challenged by direct democratic rights[52]. This change in the scope of the statutory initiative, however, does not affect the value of the index of direct democracy because no distinction is made between a statutory initiative with a wide or a narrow scope of application. In the same period, the requirements of the constitutional initiative remained unchanged.

In the canton of *Freiburg*, a new constitution took effect on the 1st of January, 2005. Regarding the institutional setup of either the statutory or the constitutional initiative, however, no change was introduced. Thus, the index of direct democracy remains unaffected. The sole difference to the old legal system is that for both types of initiatives the signature requirements are now explicitly stated in the constitution instead of being regulated exclusively by a cantonal law. This complete regulation at the constitutional level does not, however, diminish the power of the electorate to change these requirements through an initiative because in *Freiburg* it is (and was) as easy to launch a statutory initiative as to launch a constitutional one.

The people of *Schaffhausen* adopted a new constitution that came into force on the 1st of January, 2003. This new constitution is one of those total revisions that aimed to modernize the structure and wording without changing the legal content – at least as far as the initiatives are concerned. In the new constitution, both types of initiatives are now regulated in one article (art. 27 CC) rather than being dispersed within the constitution (art. 108, 107 old CC and art. 43 old CC). Therefore, no change is observed in the values of the subindices of either the constitutional or the statutory initiative.

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[51] As well as of the mandatory statutory referendum – see section 4.

[52] According to Mr. DÜRST, *Ratsschreiber* in the *Regierungskanzlei* of *Glarus*, in practice this restriction was only carried out to solve an academic battle over whether all executions of federal laws needed to be based on a so-called cantonal introductory law ('*Einführungsgesetz*') that would, under the old constitution, have been subject to a mandatory referendum. In political practice, he claims, no restriction of direct democratic rights was caused by this amendment in May 2002.

During the time period under investigation, the people of *St. Gallen* also adopted a new constitution in 2002, which took effect on the 1st of January, 2003. This new constitution brought about decisive changes for the statutory and constitutional initiatives. Overall, launching an initiative seems to have become more difficult under the new regulation. Specifically, in the new constitution, the time for collecting signatures was fixed at 5 months for either initiative (for reasons of 'harmonization'[53]) and the number of signatures was raised substantially (from 4,000 to 6,000) for the statutory and maintained at 8,000 for the constitutional initiative, respectively. Hence, the time requirement has become stricter for the constitutional initiative but more relaxed for the statutory one. As a consequence, the subindex for the constitutional initiative stayed the same[54], but the subindex for the statutory initiative declined from 4 points down to 3.66 points. One new feature in this constitution is the introduction of the so-called *Einheitsinitiative* (unitary initiative), which is easier to launch in comparison to a traditional statutory or constitutional initiative (4,000 signatures in 5 months, art. 43, 1 CC and art. 45 CC). The advantages and disadvantages of this kind of initiative have already been described in the previous section. As a final change, the minimum time gap between passing a new law and starting a new initiative appears to have been erased in the new constitution. Neither the unitary initiative nor the change in minimum time gap is reflected in the subindex for the statutory initiative.

Like many other cantonal people, the citizens of *Graubünden* also opted for a new constitution, which became effective on the 1st of January, 2004, but which is outside the scope of this investigation. The new stipulations brought about a rise in the people's empowerment through a decrease in the number of signatures necessary for a constitutional initiative (from 5,000 signatures down to 4,000). This development should be well reflected in a higher value of the subindex of the constitutional initiative (from 4.333 up to 4.666). Regarding the statutory initiative, however, the old requirements remained unchanged at 3,000 signatures. For both initiatives, the time for collecting signatures is set at one year in a cantonal law (art. 53c, 1 *GPR*), which has remained unaltered since 1962.

Also counted among the new cantonal constitutions has to be that of the Italian-speaking canton of *Tessin*, which was adopted in December 1997 and became effective on the 1st of January, 1998. Regarding both constitutional and statutory initiatives, no changes are observable in the constitutional stipulations. The sole observed change is that the time period

[53] See PROTOCOL 1 and PROTOCOL 2.

[54] As the index is constructed, both 180 and 150 days of time for collection fall in the same category.

available for collection is now regulated at the constitutional level and not (exclusively) through a cantonal law (art. 3, 3 *LIRR* for the old constitution; art. 137, 1 *LEDP* since 1 Jan, 1999). Hence, the index of direct democracy is unaffected. As the number of signatures for launching a constitutional initiative is significantly higher than the number necessary for a statutory one (10,000 vs. 7,000), it is now more difficult for the people to induce a change in the requirement of the time period for collection than prior to the new constitution. At this point, it should be noted that the information in T/S 1999, p. 343, on the signature requirement for the statutory initiative in the proposed constitution does not reflect the number actually set in the new constitution.

The canton *Waadt* also experienced the introduction of a new constitution, which entered into force on the 1st of September, 2003. As in many other cases, however, only small institutional changes were introduced through this process. The time period for collecting signatures for either initiative was increased from 3 months to 4 months (art. 79, 2 CC)[55]; however, the number of signatures was augmented to 18,000 for a total revision of the constitution but stayed the same for the partial revision and the statutory initiative. Since the index of direct democracy does not take into account a total revision of the constitution (which occurs less than once in a human lifetime on average), it is not affected by that latter change. Nevertheless, the subindices for either initiative should increase from 2004 onwards. In addition, as in many other cantons and in contrast to the old legal setup, the time period for collecting signatures is now regulated at the constitutional level. As, however, the requirements for changing a cantonal law and amending the constitution are identical in *Waadt*, regulating such an issue in the constitution does not weaken the institution-setting power of the people. The old regulatory setup facilitated, in theory, changes by the parliament to their advantage because cantonal laws were then subject only to an optional statutory referendum. The cantonal parliament, however, has never abused its power in the past because, in political practice, the stipulation of 3 months has remained unchanged, at least since 1978[56].

Finally, the people of *Neuenburg* also totally revised their old constitution and voted on a new one on the 16th of October, 2001, which took effect on the 1st of January, 2002. In the new

[55] The new constitutional stipulation contradicts the time limit laid down in the cantonal law on political rights (art. 92, 1 *LEDP*). The *Grand Conseil* of *Waadt*, however, amended this law on the 5th of April, 2005. This revision will come into force after a delay of 40 days if the electorate of *Waadt* does not carry out a statutory referendum to overrule this change.

[56] See T/S 1999, p. 375 cont. for an account of the institutional development of the initiative.

constitution, the stipulations are more transparently and logically structured, but this did not cause a change in the institutional setup for either initiative. Therefore, the two subindices of the statutory and the constitutional initiatives remain unchanged. Additionally in this case, the requirement of the time period, originally solely regulated in the cantonal law on political rights (*LDP)*, became part of the constitutional provisions.

4 The Statutory Referendum

4.1 Mandatory Statutory Referendum

Table 4: Mandatory Statutory Referendum Between 1997 and 2003

Canton	Article in cant. const.	Effective since	Remarks
Zürich (30 Mar 2004)	art. 30, num. 1 art. 30 bis, 1 art. 30 bis, 3	1 Jan 1999 (27 Sept 1998) 1 Jan 1999 (27 Sept 1998)	Non-existing for laws Extraordinary mand. stat. ref. for laws and decrees
Zürich (old)	art. 30, num. 1	1869	All formal laws, including treaties which have an impact on the cantonal legislation (see T/S 1999, p. 405)
Bern (21 Oct 2003)	art. 61, 2	22 Sept 2002	Extraordinary stat. mand. referendum on issues which are subject to the optional referendum
Luzern (21 Oct 2003)	art. 39, 1	1 Jan 1977 (5 Dec 1976)	Extraordinary mand. stat. ref. for laws and concordats
Uri (1 Apr 2003)	art. 24 lit. b art. 25, 4	1 Jan 1985 (28 Oct 1984)	All formal laws Extraordinary mand. stat. ref. for decrees of the Landrat
Schwyz (18 Aug 2004)	art. 30, 1 art. 32	1969 / 1970	All Laws Extraordinary mand. stat. ref. for decrees and by-laws enacted by the Kantonsrat
Obwalden (22 Oct 2002)	art. 58 lit. c art. 59, 2 lit. a	29 Nov 1998 (29 Nov 1998)	Only initiatives which are not accepted by the Kantonsrat Extraordinary mand. stat. ref. for laws

Table 4: Mandatory Statutory Referendum Between 1997 and 2003 (cont.)

Canton	Article in cant. const.	Effective since	Remarks
Obwalden (old)	art. 65	19 May 1968	All laws were passed in a popular vote at the ballot box, (*Landsgemeinde*)[57]. For optional referendum, see table 5
Nidwalden (28 Dec 2001)			Non-existing
Glarus (30 Mar 2004)	art. 69, 1	5 May 2002 (5 May 2002)	*Landsgemeinde*: only important regulations and provisions are passed in the form of laws
Glarus (old)	art. 69, 1 lit. b, c	1 May 1988 (1 May 1988)	*Landsgemeinde*: All laws and treaties, including those for execution of federal laws
Zug (1 Apr 2003)	art. 34, 4 art. 34, 1	2 Dec 1990 (2 Dec 1990)	Extraordinary mand. stat. ref. for laws and decrees of the *Kantonsrat*
Freiburg (18 Aug 2004) new constitution by 1 Jan 2005		1 Jan 2005 (16 May 2004)	Non-existing
Freiburg (old)		11 Mar 1921 (30 Jan1921)	Non existing (see also T/S 1999, p. 210)
Solothurn (30 Mar 2004)	art. 35 1, lit. c art. 35, 2; art. 35, 1 lit. k	11 Dec 1998 (29 Nov1998) 1 Jan 1988 (1 Jan 1988)	Laws which have been passed by less than 2/3 of the present members of the *Kantonsrat* Extraordinary mand, stat. ref. for decrees (no change)
Solothurn (old)	art. 35, 1 lit. d art. 35, 2; art. 35, 1 lit. k	1 Jan 1988, and before 1887 (1 Jan 1988 / (8 Jun 1986))	All laws See T/S 1999, p. 313. Extraordinary mand. stat ref. for decrees of the Kantonsrat
Basel-Stadt (4 July 2000)	(art. 28, 3) art. 29, 1	1890 / 1991 (1978 / 1979)	(all laws stemming from unformulated initiatives) Extraordinary mand. stat ref. for
Basel-Land (22 Oct 2002)	art. 30 lit. a art. 30 lit. b	1 Jan 2000 (7 June 1998) 1 Jan 2000 (7 June 1998)	Laws passed with less than 4/5 of the present members of the Landrat Extraordinary mand. stat. ref. for laws and nomothetic international treaties

[57] Personal communication with Mr. DILLIER of *Rechstdienst* of *Obwalden* (24/08/2004).

Table 4: Mandatory Statutory Referendum Between 1997 and 2003 (cont.)

Canton	Article in cant. const.	Effective since	Remarks
Basel-Land (old)	art. 30 lit. b	1 Jan 1987 (4 Nov 1984)	All laws and important decrees
Schaffhausen (21 Oct 2003) new constitution by 1 Jan 2003	art. 32 lit. c art. 32 lit. e art. 32 lit. i	1 Jan 2003 (22 Sept 2002)	All Laws which are not subject to an optional statutory referendum; i.e. those to which less than 4/5 of the present members of the *Kantonsrat* have agreed Decrees of the *Kantonsrat*, stipulated in a cantonal law Extraordinary mandatory statutory referendum for any decree of the *Kantonsrat*
Schaffhausen (old)	art. 42, 1 art. 42, 1 num. 6	8 June 1980 (1895, 1960, 1977, 1978)	All laws which are not subject to the optional statutory referendum (see T/S 1999, p. 18, p. 283) Decrees based on art. 23 extraordinary mand. stat. ref. for laws and decrees and possible treaties (see T/S. 1999, p. 283-284)
Appenzell AR (3 Apr 2001)	art. 60, 1 lit. d art. 60, 1 lit. h	1 June 2000 (21 May 2000) 1 June 2000 (21 May 2000)	Fundamental decrees (*Grundsatzbeschluss*) Extraordinary mandatory stat. ref. for issues subject to the optional stat. ref.
Appenzell AR (28 Sept 1997)	art. 60, lit. b, c	1 May 1996 (30 Apr 1995)	Mandatory statutory referendum for laws and treaties with law-creating character; voting at the ballot box
Appenzell IR (30 Mar 2004)	art. 1, 2 art. 19, 1 art. 20, 1	27 Apr 1873 (24 Nov 1872)	Landsgemeinde (i.e. mand. stat. ref. is existing)
St. Gallen (9 July 2002) new constitution by 1 Jan 2003	art. 49, 1	1 Jan 2003 (10 June 2001)	Extraordinary mandatory stat. ref. for laws and nomothetic international treaties
St. Gallen (old)	art. 47, 1	(16 Nov 1890)	Extraordinary mand. stat. ref on issues subject to the optional stat. ref. (T/S 1999; p. 327 cont.)

Table 4: Mandatory Statutory Referendum Between 1997 and 2003 (cont.)

Canton	Article in cant. const.	Effective since	Remarks
Graubünden (6 July 2004) New constitution by 1 Jan 2004	art. 16, num. 2 art. 16, num. 5, art. 16, num. 6	1 Jan 2004 (18 May 2003, 14 Sept 2003)	Treaties which affect the constitution extraordinary mand. stat. ref. for all issues regulated in art. 16
Graubünden (old)	art. 2, 2, num. 3 art. 2, 2 num. 4 art. 2, 2 num. 5	1 Jan 1894 (2 Oct 1892)	Organic Laws, Laws regarding the administration, cantonal laws and regulations regarding the execution of federal laws; decrees which establish new branches of cantonal administration de facto no exclusion of any law, see T/S 1999, p. 237
Graubünden (old) (cont.)	Art. 2, 2 num. 7	1 Jan 1894 (2 Oct 1892)	Extraordinary mand. stat. ref. for any other decrees not regulated in art. 2, 2 see also T/S 1999, p. 237 cont.
Aargau (30 Mar 2004)	art. 62, 1B, lit. e art. 62, 1B, lit. e	1 Jan 2003 (2 June 2002) 1 Jan 2003 (2 June 2002)	Laws and decrees subject to an optional referendum but not passed with absolute majority of the *Grosser Rat* Extraordinary mand. stat. ref. for the same laws and decrees
Aargau (old)	art. 62, b	1 Jan 1982	All Laws (as in T/S 1999, p. 124)
Thurgau (22 Oct 2002)	art. 22 art. 24, 2	1.Jan 1990 (4 Dec 1988)	Extraordinary mand. stat. ref. for laws and decrees see also T/S 1999, p. 347
Tessin (30 Mar 1999) new constitution by the 1 Jan 1998	art. 39	1 Jan 1998 (14 Dec 1997)	Art 135, 137 *LEDP*
Tessin (old)	art. 21, 2	29 Oct 1967	Only unformulated statutory initiatives (see T/S 1999, p. 339)
Waadt (21 Oct 2003) New constitution by 1 Sept 2003		1 Sept 2003	Non-existing
Waadt (18 Jan 2000)		29 Nov 1998	Non-existing

Table 4: Mandatory Statutory Referendum Between 1997 and 2003 (cont.)

Canton	Article in cant. const.	Effective since	Remarks
Waadt (old)		1981 (1885)	Non-existing (see T/S 1999, p. 369 cont.)
Wallis (21 Oct 2003)	Art. 31, 2 Art. 32, 2	1 June 1994 (24 Oct 1993)	Extraordinary mand. stat. ref for laws, decrees, and nomothetic treaties see also T/S 1999, p. 384
Neuenburg (16 Oct 2001) new constitution by 1 Jan 2002		1 Jan 2002	Non-existing
Neuenburg (old)		21 Nov 1858	Non-existing (see also T/S 1999, p. 271)
Genf (21 Oct 2003)		1993	Non-existing (see also T/S 1999, p. 230)
Jura (4 July 2000)	Art. 79	1 Jan 1979 (20 Mar 1977)	Extraordinary mand. stat. ref for all decisions (decrees, laws, etc.) of the parliament
See table 2. In some cases the dates of effectiveness were obtained from T/S 1999, particularly for ZH, FR, BS, SG, SO, TG, TI, VD and GE. For some selected cantons information on popular votes on international treaties have also been included.			

Table 4 describes the existence and scope of the mandatory statutory referendum. In many cantons, a mandatory statutory referendum exists that is applicable solely to very specific laws and contracts but not to all cantonal laws in general. For example, since 1999 in *Nidwalden*, the new mandatory referendum is applicable solely to administrative permissions on subterraneous exploitation or storage (art.52, 6. CC) and in *Bern* exclusively to intercantonal treaties and alterations in the cantonal demarcations (art. 61, 1 lit. c, d). In such cases, the mandatory referendum is treated and regarded as nonexistent for the construction of the index of direct democracy because these special cases do not form part of the daily political decision-making process. For this reason, I have usually excluded these mandatory referenda on treaties and special issues. Included however are the extraordinary statutory referenda because they might shed light on the entire range of statutory referenda (jointly with the optional statutory referendum) that exists in a canton. In general, however, the analysis of the mandatory statutory referendum addresses all those applicable to all cantonal laws, decrees and by-laws.

4.2 Comments on the Changes in the Mandatory Statutory Referendum

Between 1997 and 2003, a considerable constitutional change regarding the mandatory statutory referendum could be observed in *Zürich*: the mandatory statutory referendum was abolished and a optional statutory referendum was introduced instead. This change became effective on the 1st of January, 1999. To evaluate how this change affects the index for the statutory referendum, the interplay with the optional statutory referendum must be taken into consideration (see section 4.3), which caused a drop in the subindex of the statutory referendum (from 6 to 3.33). On the other hand, in 1999, an additional extraordinary mandatory referendum was introduced for laws and decrees (art. 30 bis, 1 and 3 CC).

In canton *Bern*, a restriction with respect to extraordinary mandatory referendum was introduced in September 2002. Before this change any law could be subject to such an extraordinary referendum if 120 members (out of 200) of the *Grosser Rat* so decided (see T/S 1999, p. 177). Now, however, it is stipulated that 100 members (out of 160, art. 72 CC) can put a law or decree on the ballot if it is subject to an optional referendum anyway according to art. 62. CC, to which all laws, international treaties, and most decrees are subject. Excepted are specific expenditure projects that are below the threshold for an optional fiscal referendum. This change, however, did not affect the index of direct democracy because the extraordinary mandatory statutory referendum does not form part of it.

In the canton of *Obwalden*, the abolishment of the *Landsgemeinde* in 1998 led to a dramatic change regarding the mandatory statutory referendum. Traditionally, before the popular vote on the 29th of November, 1998, the assembly of the cantonal electorate voted on all laws at the ballot box (art. 65 old CC) and by open ballot in case of optional statutory referenda on decrees of the *Kantonsrat* (art. 61 old CC). In the new partial revision of the constitution of 1998, this mandatory statutory referendum was eliminated and replaced by an optional statutory referendum for laws (art. 59, 1 CC), and the old optional referendum on decrees was completely abolished[58]. According to the revised constitution, a statutory initiative must be voted upon if the *Kantonsrat* does not agree or if it makes a counterproposal (art. 58 lit. c CC). According to art. 59, 2 lit. a, an extraordinary mandatory statutory referendum can be held if a third of the members of the *Kantonsrat* agree. This development led to a decline of the subindex of the statutory referendum in *Obwalden* from 6 in 1998 to 4.33 in 1999.

[58] Personal communication with Mr. DILLIER of the *Rechtsdienst* of *Obwalden* (24/08/2004).

A similar but less drastic revision of the cantonal constitution took place in canton *Glarus*, one of the still existing *Landsgemeinden*, which, on the surface, appears to have shifted power from the direct democratic town meeting to the representative legislative organ *Landrat* on the 5th of May, 2002. The mandatory statutory referendum was restricted to only 'fundamental and important regulations and provisions' (art. 69 CC), whereas, as implicitly expressed, the remaining legal acts can be determined in the form of decrees and by-laws by the *Landrat* (see also art. 82, 4 CC and 89, lit. b - d CC). In contrast, in the old constitution, the *Landsgemeinde* was responsible for the legislation of all types of laws (art. 69, 1 lit. b old CC), which also encompassed acts aimed at regulating the execution of federal laws. Accordingly, in the new version of article 89 CC, the *Landrat* is given the power to legislate for the execution of both federal and intercantonal law (art. 89, d). Further, it can enact decrees if empowered either by the *Landsgemeinde* or by the constitution (art. 89, lit. b and c CC). In practice, however, according to my source, this partial revision does not play a relevant role in the daily political decision-making in *Glarus* and thus, as already discussed in the section on initiatives, does not constitute a real restriction of direct legislative power of the electorate[59].

The people of the canton *Freiburg* have passed a new constitution that will come into force on the 1st of January, 2005. However, as regards the mandatory statutory referendum, no significant change between the old and new constitutions can be observed: in both, it is basically nonexistent.

In *Solothurn*, laws are subject to the mandatory statutory referendum when they have been passed by less than a two-thirds majority in parliament. This majority constraint was added in the constitutional referendum of the 29th of November, 1998, which became effective on the 11th of December, 1998 (see also T/S 1999, p. 312). Hence, since then, a mandatory statutory referendum has been taken place more rarely, in contrast to when the unrestricted version of this referendum was in force before November 1998. A similar case applies to *Basel-Land*, where the mandatory statutory referendum is applied to cantonal laws passed by less then four-fifths of the votes in the cantonal parliament (see also T/S 1999, p. 137). This constraint was added through a decision of the *Landrat* on the 12th of March, 1998, which became effective on the 1st of January, 2000. In the course of this revision, the extraordinary mandatory statutory referendum was also introduced (art. 30 b CC). In the two cases of

[59] See also footnote 52.

Solothurn and *Basel-Land*, the revisions in the cantonal constitutions in 1998 lead to a decline in the subindex value of the statutory referendum for both cantons (from 6 to 5.33 and from 6 to 5.17, respectively).

On the 1st of January, 2003, the new constitution of the canton *Schaffhausen* came into force, which imposes fewer restrictions on the mandatory statutory referendum than the old constitution. As with the old constitution, all laws not subject to an optional referendum (art. 32 lit. c CC) must be passed by the cantonal electorate. These include those laws not passed by more than four-fifths of the present members of the cantonal parliament (old constitution: '*Grosser Rat*'; new constitution: '*Kantonsrat*'). In contrast to the new constitution, however, the old regulations also exempted laws referring to cantonal administration and execution of federal laws from the mandatory referendum, as those laws did not bring about new expenses or new taxes (art. 42 old CC, T/S 1999, p. 283). Moreover, in the new constitution, both extraordinary mandatory and statutory referenda are applicable (art 32, 1 lit. h CC; art 33, 1 lit. f CC). This widening of the scope of the mandatory statutory referendum in the new constitution, however, does not affect its valuation on the index.

Decisive changes can also be observed after 1997 in the case of *Appenzell Ausserrhoden*. On the 28th of September, 1997 (effective as of the same date), the annual open *Landsgemeinde* meeting was replaced by secret voting at the ballot box. At that time, however, the principle character of the direct democratic institution remained unchanged (and thus did not affect the value of the index of direct democracy)[60]. On the 21st of May, 2000, however, a distinction between the optional and the mandatory referendum was introduced that includes the abolishment of the statutory mandatory referendum for both laws and international treaties and its replacement by an optional statutory referendum (art. 60 lit. b, c old CC, art. 60 bis CC, see also table 5, effective: 1st of June, 2000). Only fundamental decrees remained subject to a mandatory statutory referendum (art. 60, 1 lit. d CC). According to Mr. SIGRIST[61], this new mandatory referendum on fundamental decrees has been applicable only once since its introduction in June 2000 and plays a negligible role. He further stated that the laws referring to the execution of such a fundamental decree would again be subject to the optional statutory referendum. Through the same constitutional revision, an extraordinary mandatory referendum was introduced that can be applied to decrees of the *Kantonsrat* if a third of its

[60] The index of direct democracy does not take into account the type of voting (e.g. at the ballot box vs. by open ballot).

[61] Chief of *Rechtsdienst Appenzell Ausserrhoden*, personal communication (15/10/2004).

present members so demand (art. 60, 1 h CC). This change can be regarded as an improvement in the degree of direct democracy (although not one affecting the index) because prior to this last revision in 2000, decrees of the *Kantonsrat* were not subject to any referendum whatsoever (see 74, 2 old CC). At this point, it should also be noted that laws triggering a new expense are still subject to the mandatory fiscal referendum. Through the abolishment of the mandatory statutory referendum, however, a decline on the subindex for the statutory referendum was recorded (from 6 to 4.67).

The people of *St. Gallen* adopted a new constitution on the 10th of June, 2001, which came into force on the 1st of January, 2003. Regarding the mandatory statutory referendum, its extraordinary version continues to exist in the new constitution with identical requirements (a demand of one-third of the members of the cantonal parliament) but couched in revised and more modern wording ('*Kantonsrat*' (2003) vs. '*Grosser Rat*'). In both constitutions, the issues that are potentially subject to this type of referendum are identical to those potentially subject to an optional statutory referendum (art. 49, 1 CC; art. 47 old CC, see also T/S 1999, p. 327 cont.). It is readily apparent that these minor changes do not affect the index of direct democracy for the canton of *St. Gallen*.

The abolishment of the mandatory statutory referendum can also be observed in canton *Graubünden*, becoming effective on the 1st of January, 2004. Before this constitutional revision, basically all laws, even those only regulating the execution of federal laws, as well as decrees that established new branches of administration, were subject to the mandatory statutory referendum. In the new constitution, the new optional statutory referendum applies only for laws. In both the old and the new constitution, however, an extraordinary statutory referendum exists. As observed earlier, the abolishment of the mandatory statutory referendum for cantonal laws can be expected to cause a decline in the index of direct democracy from 2004 onwards.

In the canton of *Aargau*, both the diminishing power of the mandatory statutory referendum for laws and its strengthening for decrees can be noted simultaneously. Through a popular vote in June 2002, which became effective on the 1st of January, 2003, the application of the mandatory referendum for laws was restricted by the new requirement that it affect only those laws not passed by an absolute majority of all members of the *Grosser Rat* (art. 62, 1 b CC). Probably to ease this change, the possibility of an extraordinary mandatory referendum was

also introduced in the case that a quarter of all members of the cantonal parliament demanded it (art. 62, 1 lit. b CC). In the course of this revision, article 62 was appended a letter 'e' (art. 62, 1 e CC), which extends the mandatory statutory referendum to decrees of the *Grosser Rat* (according to art. 63, 1 lit. b - d and f CC). This extension means that this referendum is now applicable to decrees to which, prior to its revision, only the optional referendum applied. Its constitutional requirement is, again, that decrees have been passed with a majority below the absolute majority of the *Grosser Rat*. Analogously to art. 62, 1 lit. b CC, the extraordinary mandatory referendum was also applicable to such decrees. Given that the index of direct democracy assigns a higher value to a mandatory statutory referendum than to an optional statutory referendum, a decline in the index will be induced by the new majority requirement.

Among the remaining cantons, *Tessin*, *Waadt* and *Neuenburg* adopted new constitutions in 1998, 2003, and 2002, respectively. These, however, did not bring about any changes with respect to the nonexistence of the mandatory statutory referendum for cantonal laws. Hence, the degree of direct democracy as measured by the index was not affected.

4.3 Optional Statutory Referendum

Table 5: Optional Statutory Referendum Between 1997 and 2003

Canton	Signature requirement	Time-limit for collection	Articles in cant. const.	Effective since
Zürich (30 Mar 2004)	5,000	60 days	art. 30 bis, 1	1 Jan 1999 (27 Sept 1998)
Zürich (old)				Mandatory statutory referendum See table 4
Bern (21 Oct 2003)	10,000	3 months	art. 62, 2	1 Jan 1995 (6 June 1993)
Luzern (21 Oct 2003)	3,000	60 days	art. 39, 1 art. 40, 1	1 Nov 1969 (art. 40, 1) 1 Jan 1977 (art. 39, 1)
Uri (1 Apr 2003)	450	90 days	art. 25, 1 art. 25, 2 lit. b	1 Oct 1997 (8 Jun 1997) For by-laws; mandatory stat. ref. for laws see table 4
Uri (old)	300	90 days	art. 25, 1	Only for by-laws (see T/S 1999, p. 359) (1985) mandatory stat. ref. for laws see table 4

Table 5: Optional Statutory Referendum Between 1997 and 2003 (cont.)

Canton	Signature requirement	Time-limit for collection	Articles in cant. const.	Effective since
Schwyz (18 Aug 2004)	2,000	30 days	art. 31, 1	21 Dec 1899 (23 Oct 1898) Decrees and by-laws mandatory stat. ref. see table 4
Obwalden (22 Oct 2002)	100	30 days	art. 59, 1 lit. a art. 59, 2 lit. b	29. Nov 1998 (29.Nov 1998) Laws
Obwalden (old)	100	30 days	art. 61, no. 1 art. 73, 1	*Landsgemeinde*: optional statutory referendum for decrees of the *Kantonsrat*; mandatory stat. ref. see table 4
Nidwalden (28 Dec 2001)	250	2 months	art. 52a, .1	1 Dec 1996 (1 Dec 1996) Laws
Glarus (30 Mar 2004)	1	none	art. 57, 1 lit. b art. 58, 1	1 May 1988 (1 May 1988) *Landsgemeinde* mandatory stat. ref. see table 4
Zug (1 Apr 2003)	1,000	60 days	art. 34, 1 art. 34, 2	2 Dec 1990 (2 Dec 1990) laws and decrees
Freiburg (18 Aug 2004) new const. by 1 Jan 2005)	6,000	90 days	art. 46	1 Jan 2005 (16 May 2004) Laws
Freiburg (old)	6,000	90 days see also art. 130, 2 *PRG*	art. 28 bis, 1 art. 28 quarter	11 Mar 1921 (30 Jan1921) Laws and decrees
Solothurn (30 Mar 2004)	1,000	90 days	art. 36 1, lit. b	11 Dec 1998 (29 Nov 1998) Laws and decrees, which are not subject to the mand. Stat. ref.

Table 5: Optional Statutory Referendum Between 1997 and 2003 (cont.)

Canton	Signature requirement	Time-limit for collection	Articles in cant. const.	Effective since
Solothurn (old)	1,000	90 days	art. 36, 1 and 2	1988 – 1998 By-Laws, decrees mandatory stat. ref. see table 4 see T/S 1999, p. 314
Basel-Stadt (4 July 2000)	2,000	6 weeks	art. 29, 1 and 2	21 June 1979 (24 Sept 1978) laws and decrees
Basel-Land (22 Oct 2002)	1,000	8 weeks	art. 31, 1 lit. a art. 31, 1 lit. c	Decrees by the *Landrat* (*Planungsbeschluss*) 1 Jan 1987 (4 Nov 1984) Laws not being subject to the mand. Stat. ref. 1 Jan 2000 (7. Jun 1998)
Basel-Land (old)	1,000	8 weeks	art. 31, 1 lit. a art. 31, 1 lit. c	Decrees by the *Landrat*; 1 Jan 1987, (4 Nov 1984) Stat. mand. ref. see table 4 (see T/S 1999, p. 137 cont.)
Schaffhausen (21 Oct 2003) new constitution by 1 Jan 2003	1,000	90 days	art. 33, 1 lit. a art. 33, 1 lit. b	All laws to which at least 4/5 of the *Rat* have agreed Stat. mand. ref. see table 4 International treaties
Schaffhausen (old)	1,000	90 days	art. 42 bis, 1	8 Jun 1980 laws concerning organization of administration or execution of laws; all remaining laws to which at least 4/5 of the Rat have agreed
Appenzell AR (3 Apr 2001)	300	60 days	art. 60 bis	1 June 2000 (21 May 2000) Laws, treaties with law-like character

Table 5: Optional Statutory Referendum Between 1997 and 2003 (cont.)

Canton	Signature requirement	Time-limit for collection	Articles in cant. const.	Effective since
Appenzell AR (old)				1 May 1996 (30 Apr 1995) mandatory statutory referendum, see table 4
Appenzell IR (30 Mar 2004)				27. Apr 1873 (24 Nov 1872) Statutory Mandatory Referendum, see table 4
St. Gallen (9 July 2002) new constitution by 1 Jan 2003	4,000	40 days	art. 49, 1 lit. a, b art. 50, 1	1 Jan 2003 (10 Jun 2001) Laws and international treaties with law-like content
St. Gallen (old)	4,000	30 days	art. 47	Laws, general decrees not subject to art. 55 or not of an urgent nature T/S 1999; p. 327 cont.
Graubünden (6 July 2004) new constitution by 1 Jan 2004	1,000	90 days	art. 17, 1 num. 1 art. 17, 1 num. 2	Laws; International treaties with law-affecting content 1 Jan 2004 (18 May 2003, 14 Sept 2004)
Graubünden (old)				Non-existing 1 Jan 1894 (2 Oct 1892) Stat. mand. ref. see table 4
Aargau (30 Mar 2004)	3,000	90 days	art. 63, lit. a art. 63, lit. f art. 63, 1 lit. b (art. 62, 1 lit. b, lit. e)	1 Jan 2003 (2 Jun 2002) -Laws -Specific decrees of the *Grosser Rat* which are determined by law -Fundamental plans of state activity which are all not s.t. a mandatory referendum (see table 4) art. 40 *GPR*

Table 5: Optional Statutory Referendum Between 1997 and 2003 (cont.)

Canton	Signature requirement	Time-limit for collection	Articles in cant. const.	Effective since
Aargau (old)	3,000	90 days	art. 63, 1 lit. f art. 63, 1 lit. b art. 63, 1 lit. a	1 Jan 1982 Specific decrees of the *Grosser Rat* which are determined by law Treaties Fundamental plans of state activity art. 40 *GPR* see also T/S 1999, p. 124 cont.
Thurgau (22 Oct 2002)	2,000	3 months	art. 22	1 Jan 1990 (4 Dec 1988) laws, decrees on treaties see also T/S 1999, p. 346
Tessin (30 Mar 1999) new constitution by 1 Jan 1998	7,000	45 days	 art. 42 lit. a art. 42 lit. c	1 Jan 1998 (14 Dec 1997) laws and decrees treaties with nomothetic character
Tessin (old)	7,000	30 days	art. 60, I	(31 May 1970) laws and decrees See T/S 1999, p. 339 for further application
Waadt (21 Oct 2003) new constitution by 1 Sept 2003	12,000	40 days	art. 84 1, lit. a art. 84 1, lit. b	Laws and decrees Treaties with nomothetic content
Waadt (old)	12,000	40 days	art. 27, 2	1978 / (29. Nov 1998)* laws and decrees see also T/S 1999, p. 369 *change concerns fiscal referendum, see tables 6 and 7
Wallis (21 Oct 2003)	3,000	90 days	art. 31, 1 art. 31, 2	1 June 1994 (24 Oct 1993) laws and decrees, treaties with nomothetic content see also T/S 1999, p. 380 cont.

Table 5: Optional Statutory Referendum Between 1997 and 2003 (cont.)

Canton	Signature requirement	Time-limit for collection	Articles in cant. const.	Effective since
Neuenburg (16 Oct 2001) new constitution by 1 Jan 2002	4,000	40 days	art. 42, 1 art. 42 2 lit. a art. 42, 2 lit. e art. 42, 2 lit. g art. 42, 2 lit. c	1 Jan 2002 (24 Sept 2001) laws treaties extraordinary optional statutory referendum decrees based on people's initiatives
Neuenburg (old)	6,000	40 days	art. 39, 2 art. 120 *LDP*	1879, 1959 (see T/S 1999, p. 271) Laws and specific decrees
Genf (21 Oct 2003)	7,000	40 days	art. 53	1983 (7 Mar 1982) Laws See also T/S 1999, p. 221-222
Jura (4 July 2000)	2,000	60 days art. 94 *LDP*	art. 78 art. 78 lit. a art. 78 lit. c	(1 Jan 1979) 20 Mar 1977 Laws Treaties with nomothetic character See also T/S 1999, p. 250 cont.

See table 2.

Table 5 describes the legal Status Quo and recent amendments concerning optional statutory referenda referring to laws, decrees, or by-laws as they influence the degree of direct democracy in a canton as measured by the index of direct democracy. For reasons of completeness, I have also included information on the extraordinary optional statutory referendum. Usually, not reported are optional referenda referring to international or intercantonal treaties or on special issues. To understand the impact of these changes on the composite index of direct democracy, it must be recalled that in most cases, the subindex of the statutory referendum is constructed as an average of the optional and the mandatory statutory referendum, if both exist. Again, I will only include in the verbal description those cases in which alterations of the optional statutory referendum have occurred since 1997.

4.4 Comments on the Changes in the Optional Statutory Referendum

After 1997, many cantons introduced some alterations regarding the optional mandatory statutory referendum. To begin with, *Zürich* introduced such a referendum as a new institution replacing the abolished mandatory statutory referendum. The optional referendum is fitted out with the requirement of 5,000 signatures to be collected within 60 days. As discussed before, in the case of *Zürich*, this change in the constitution led to a decline of the index for the statutory referendum.

In *Uri*, the optional statutory referendum applies only to by-laws, not to laws. The signature requirement for this referendum was raised from 300 to 450 votes in June 1997. In this special case, the index of the statutory referendum is negatively affected by this development. For the treatment of *Uri* concerning the index of direct democracy, see the section 6.

The next canton in which a considerable development could be observed is *Obwalden*. In the course of the introduction of a (partly) representative system that became effective in 1998, *Obwalden* introduced an optional statutory referendum for laws as a new institution with a signature requirement of 100 and a collection time of 30 days but abolished the old optional referendum for decrees of the *Kantonsrat* (with the identical requirements). According to my source, as a result, decrees can no longer be challenged by a referendum[62]. This abolishment of the mandatory statutory referendum has led to a lower index value for the both the optional and the overall statutory referendum compared to the previous maximum value.

On the 1st of January, 2005, a new constitution entered into force in the canton of *Freiburg*. This new constitution did not, however, bring about any new stipulations regarding the optional statutory referendum (6,000 signatures within 90 days). The only difference lies in the fact that the time period for collection is now regulated at the constitutional not the statutory level.

The people of the canton *Solothurn* partially revised their constitution in November 1998. Before 1998, the optional statutory referendum was only applicable to decrees not to laws, to which the mandatory referendum applied (T/S 1999, p. 313 cont.). Now all laws and decrees of the cantonal parliament can be subject to an optional statutory referendum. The signature

[62] Personal communication with Mr. DILLIER, *Rechtsdienst* of *Obwalden* (24/08/2004).

requirement of 1,500 votes and a time limit for collection of 90 days were kept in the amended article. The value of the index for the optional statutory referendum reaches 4.67 for 1999 on, and the final value of the subindex for the statutory referendum was brought down from the maximum value of 6 in and prior to 1998.

In *Basel-Land*, the optional statutory referendum for laws was also introduced as a new institution because the use of the mandatory statutory referendum was restricted. The number of signatures required was set to 1,500 in a popular vote in 1998, and the time available for collection was stipulated as 60 days, identical to the requirements for the old optional statutory referendum for decrees, which still exists (see T/S 1999, p. 137, for the old constitution). These changes caused the index for the optional statutory referendum to decline because of the former restriction of the old mandatory statutory referendum before 2000.

In *Schaffhausen*, the new constitution of the 1st of January, 2003, brought about a restriction of the optional statutory referendum in comparison to the old constitution: only all those laws, including laws concerning administrative issues, to which more than four-fifths of the *Rat* have agreed are subject to this institution. In the old constitution, however, all laws related to administration and organization were subject to the optional referendum (because the remaining laws were subject to the mandatory referendum). The inclusion of 'administrative' laws in the scope of the mandatory referendum led to no change in the index of direct democracy.

In the canton of *Appenzell Ausserrhoden*, the electorate decided to introduce an optional mandatory referendum for laws in 2000, which was not known prior to this date. This optional referendum applies to laws and international treaties with a nomothetic impact and can be carried out if 300 signatures have been collected within 60 days after the publication of that law or treaty. Since this new optional referendum replaces an old mandatory referendum, a decline in the level of the overall index of the statutory referendum occurred (see also table 9).

On January 1st, 2003, a new constitution became effective in the canton *St. Gallen*. One of the prominent differences between the optional statutory referendum of the old and of the new constitution is the time available for the collection of the necessary signatures, which has been increased from 30 days to 40 days while the required number of signatures has been left unaltered. The scope of this referendum has been narrowed because, general decrees (*allgemeinverbindlicher Beschluss*) of the *Grosser Rat* (now: *Kantonsrat*) are no longer

subject to this referendum (and hence not subject to any referendum at all), whereas cantonal laws can still be challenged through a referendum (art 49, 1 lit. a CC; art. 47 old CC). In the new constitution, however, regulations concerning the wages of civil servants and teachers are explicitly exempted from the referendum (art. 49, 2 CC). On the other hand, the scope of the optional referendum has been widened as international and intercantonal treaties are now subject to it if their content is nomothetic (art. 49, 1 lit. b). The increase in the time available for collection has caused an increase in the subindex of the statutory referendum, which is also reflected in the total index (see table 9).

The electorate of *Graubünden* also voted on a new constitution both on the 18th of May and the 14th of September, 2003[63], which took effect on the 1st of January, 2004. Similar to the development observable in other cantons, the mandatory statutory referendum was replaced by an optional referendum in the new constitution. This new optional referendum is both applicable to cantonal laws and to treaties whose stipulations are nomothetic. The requirement of 1,500 signing supporters out of the electorate must be met within 90 days after publication. This constitutional change will cause the subindex of the overall statutory referendum to fall from its old level of 6 points from 2004 on.

Observable in the canton *Aargau* is one of the cases in which the scope of the optional mandatory referendum was broadened at the expense of the mandatory statutory referendum. On the 2nd of June, 2002, the electorate of *Aargau* opted for a revision of art. 63, 1 lit. a CC. This amendment led to an abolishment of the old unconstrained mandatory referendum by including an absolute majority constraint and by adding 'laws' as a new field of application of the optional referendum. Additional potential applications of the optional referendum are laid down in art. 63, 1 CC (except for lit. e), which also include (among others) decrees of the *Grosser Rat*, already known in the old constitution. The constitutional requirements for taking the optional statutory referendum remained unchanged (3,000 signatures in 90 days). As the index of direct democracy takes into account the statutory referendum for laws rather than for decrees, a decline in the level of the overall index of statutory referendum occurred due to this change, which also transmits to the total index (see table 9).

[63] Because the official result of the first popular vote on the vote system for electing the *Grosser Rat* appeared to be scanty and changed after a recount in May 2003, it was decided to hold a second popular vote in September 2003 for reasons of clarification (personal communication with Mr. FRIZZONI (10/09/2004)).

The cantonal electorate of *Tessin* passed a new constitution effective on the 1st of January, 1998, that raised the number of days available for collection of 7,000 signatures from 30 to 45 (art. 42 CC). The optional referendum is applicable to both laws and decrees. This change caused an increase in the index of the optional statutory referendum, which equals the (overall) index for the statutory referendum because no mandatory referendum is known in this canton (see also table 9 for the overall change).

In the canton of *Waadt*, a partial revision of the constitution took place in 1998 that, however, did not affect the optional statutory referendum for laws (and decrees), only the fiscal referendum (see tables 6 and 7). In 2003, the electorate passed a completely new constitution (effective the 1st of September, 2003)[64] that extended the application of the optional referendum of laws and decrees to international treaties and concordats in case their stipulations contradicted cantonal law or were of a law-giving character in general (art. 84 1, lit. b CC). In the old constitution, it was not possible for the people to challenge these treaties (see T/S 1999, p. 368 cont.). The requirements for taking an optional referendum, 12,000 signatures and 40 days, have remained unchanged since 1997, leaving the index unaffected.

Finally, in *Neuenburg*, a new constitution was also adopted in 2001 that entered into force as of the 1st of January, 2002. This new constitution brought first, a change in the scope of application of the optional (statutory) referendum and second, a facilitation of carrying it out. On the one hand, both in the old and new constitutions, the optional statutory referendum is applicable to laws, but the specific (general) decrees (art. 39, 2 old CC) are exempted from application in the new constitution. On the other hand, as regards requirements, the number of signatures was lowered from 6,000 down to 4,500, whereas the number of days for collecting them remains the same (*LDP* art. 120: for the old constitution; art. 42, 1 CC: for the new constitution); this new requirement is now entirely regulated in the constitution itself. New to the optional referendum, however, is its application to international and intercantonal treaties (art. 42, 2 e CC) that are either nomothetic or equivalent to a decree leading to expenses[65]. Finally, an extraordinary optional (statutory) referendum was also introduced on ”other acts of the *Grand Conseil*“ in the case that 35 of its members so decide (art. 42, 2 g CC). The decline in signatures in the new constitution has caused the subindex of the statutory referendum to rise for *Neuenburg* (see also table 9).

[64] The date of effectiveness given in the new constitution (art. 175 CC) was altered through a decree of the *Grand Conseil* of *Waadt* (2nd of July, 2003).

[65] For a discussion of the nonapplicability of the optional referendum to treaties in the old constitution, see T/S 1999, p. 272.

5 The Fiscal Referendum

The next two tables are devoted to the description of the development of both the mandatory (table 6) and optional fiscal referendum (table 7) on expenditure projects in the Swiss cantons between 1997 and 2003.

Economically, fiscal referenda refer to resolutions of the cantonal parliament that trigger expenses; these can be both administrative and legislative acts (from a legal perspective). In some cantons, the fiscal referendum refers explicitly to cantonal laws. In any case, these decrees and laws involve a substantial expense that must be borne by the cantonal budget and thus by the cantonal taxpayer. In some cases in which the optional fiscal referendum refers solely to decrees of the parliament, it is the statutory referendum that serves as a control device over expenses triggered by cantonal law. In this sense, the fiscal and the statutory referendum do constitute two institutions whose scope of application might partly overlap.

The following tables describe only fiscal referenda on expenditure projects (the most important type) and do not take into account the existence of either extraordinary fiscal referenda or fiscal referenda not related to expenditure projects, such as referenda on tax rates, acquisition or disposition of real estate, bond loans, and so on, because these latter do not form part of the index of direct democracy. For constructing the subindex, in the case of the fiscal referendum, usually the greater value of the points awarded to either the optional or the mandatory referendum is chosen.

5.1 Mandatory Fiscal Referendum

Table 6: The Fiscal Referendum – Mandatory (Ordinary) between 1997 and 2003

Canton	Financial threshold	Article in cant. const.	effective since	Remarks
Zürich (30 Mar 2004)			1 Jan 1999 (27 Sept 1998)	Non-existing
Zürich (old)	> 20 million (> 2 million)	art. 30 num. 2	6 June 1971	Decrees of the *Kantonsrat*
Bern (21 Oct 2003)			1 Jan 1995 (6 June 1993)	Non-existing See T/S 1999, p. 183

Table 6: The Fiscal Referendum – Mandatory (Ordinary) (cont.)

Canton	Financial threshold	Article in cant. const.	effective since	Remarks
Obwalden (22 Oct 2002)			29 Nov 1998 (29 Nov 1998)	Non-existing
Obwalden (8 June1997 - 29 Nov 1998)	> 1 million (> 200,000)	art. 61, num. 3	8 June 1997 (8 June 1997)	(*Landsgemeinde*) Laws and decrees
Obwalden (old)	> 300,000 (> 50,000)	art. 61, num. 3	8 June 1986 (8 June 1986)	(*Landsgemeinde*) Laws and decrees
Nidwalden (28 Dec 2001)	> 5 million (> 500,000)	art. 52 num.4	1 Dec 1996 (1 Dec1996)	Decrees of the *Landrat*
Luzern (21 Oct 2003)	>25 million (> 25 millions = 10 times annual expense)	art. 39 bis, 1 lit. c art. 39, 2 art. 39 bis, 3	1 Jan 1977 (5 Dec 1976)	Decrees and laws of the *Grosser Rat*
Uri (1 Apr 2003)	> 1 million (> 100,000 at least 10 years)	art. 24, lit. c art. 24, lit. d	1 Jan 1994 (28 Nov 1993)	Expenditure projects, i.e. decrees; Laws see table 4
Schwyz (18 Aug 2004)	> 250,000 (> 50,000)	art. 30, 2	31 Dec 1958	Decrees of the *Kantonsrat* See T/S 1999, p. 305
Glarus (30 Mar 2004)	> 1 million (> 200,000)	art. 69, 2 lit. b	5 May 2002 (5 May 2002)	(*Landsgemeinde*) Decrees
Glarus (old)	> 500,000 (> 100,000)	art. 69, 1 lit. d	1 May 1988 (1 May 1988)	(*Landsgemeinde*) Decrees
Zug (1 Apr 2003)			At least since 1970	Non-existing See also T/S 1999, p. 401
Freiburg (18 Aug 2004) new constitution by 1 Jan 2005	> 1% of the overall spending of the last cantonal budget	art. 45 lit. b	1 Jan 2005 (16 May 2004)	Decrees and laws of the *Grand Conseil* / *Grosser Rat*
Freiburg (old)	> 1% of the overall spending of the last cantonal budget	art. 28 bis	7 Oct 1986 (8 June 1986)	Laws and decrees of the *Grosser Rat* / *Grand Conseil*
Solothurn (30 Mar 2004)	> 5 million (> 500,000)	art. 35, 1 lit. e	11 Dec 1998 (29 Nov 1998)	Decrees of the *Kantonsrat*

Table 6: The Fiscal Referendum – Mandatory (Ordinary) (cont.)

Canton	Financial threshold	Article in cant. const.	effective since	Remarks
Solothurn (old)	> 2 million (> 200,000)	art. 35, 1 lit. e	1 Jan 1988 (8 June 1986) (new const.)	Decrees of the *Kantonsrat* See T/S 1999, p. 317
Basel-Stadt (4 July 2000)			Since 1954 or earlier	Non-existing See also T/S 1999, p. 161
Basel-Land (22 Oct 2002)			1 Jan 1987 and before	Non-existing See T/S 1999, p. 139
Schaffhausen (21 Oct 2003) new constitution by 1 Jan 2003	> 3 million (> 500,000)	art. 32 lit. e	1 Jan 2003 (22 Sept 2002)	Decrees of the *Kantonsrat*
Schaffhausen (old)	> 1 million (> 100,000) > 300,000 < 1 million (> 50,000 < 100,000)	art. 42 ter art. 42, 1 num. 2	(23 Apr 1989) (23 Apr 1989)	Decrees of the *Grosser Rat* Decrees of the *Grosser Rat* passed with less than 4/5 majority (see T/S 1999, p. 287)
Appenzell AR (3 Apr 2001)	> 5% of a taxing unit (> 1% of a taxing unit)	art. 60, 1 lit. e art. 76, 2 lit a, b	1 May 1996 (30 Apr 1995)	(28 Sept 1997: abolishment of *Landsgemeinde*); via voting at the ballot box
Appenzell AR (old)	> 5% of a taxing unit (> 1% of a taxing unit)	art. 60, 1 lit. e art. 76, 2 lit. a, b	1 May 1996 (30 Apr 1995)	(*Landsgemeinde*) Decrees of the *Kantonsrat*
Appenzell IR (30 Mar 2004)	> 1 million (> 200,000 for at least 5 years)	art. 7 ter, 1	28 Apr 2002 (28 Apr 2002)	(*Landsgemeinde*) Decrees of the *Grosser Rat*
Appenzell IR (old)	> 500,000 (> 100,000 for at least 5 years)	art. 7 ter, 1	25 Apr 1982	(*Landsgemeinde*) Decrees of the *Grosser Rat*
St. Gallen (9 July 2002) new constitution by 1 Jan 2003	> 15 million (> 1,500,000 for at least 10 years)	art. 48 lit. d art.6 *RIG*	1 Jan 2003 (10 June 2001); *RIG*: IV. NG 1 Apr 1997 (11 Apr 1996)	Laws and decrees; threshold specified in cantonal law *RIG*

Table 6: The Fiscal Referendum – Mandatory (Ordinary) (cont.)

Canton	Financial threshold	Article in cant. const.	effective since	Remarks
St. Gallen (old)	> 15 millions (> 1,000,000 for at least 10 years)	GS 14, 27; nGS 6, 38* art. 6 *RIG*	20 Jan 1924 (17 Nov 1923) *RIG*: IV NG 1 Apr 1997 (11 Apr 1996)	Laws and decrees, threshold specified in cantonal law *RIG* through last amendment IV NG see T/S 1999, p. 330
Graubünden (6 July 2004) new constitution by 1 Jan 2004	> 10 million (> 1 million)	art. 16, num. 4	1 Jan 2004 (18 May 2003, 14 Sept 2003)	Decrees of the *Grosser Rat*
Graubünden (old)	> 5 million (> 500,000)	art. 2, 2 num. 6 a	(28 Jan 1973)	Decrees of the *Grosser Rat*
Aargau (30 Mar 2004)	> 5 million (> 500,000)	art. 62, 1 lit. e	1 Jan 2003 (2 Jun 2002)	Decrees of the *Grosser Rat* if not passed with absolute majority
Aargau (old)			1 Jan 1982 (28 Sept 1980)	Non-existing See also T/S 1999, p. 127
Thurgau (22 Oct 2002)	> 3 million (> 600,000)	art. 23, 1	1 Jan 1990 (4 Dec1988)	Decrees of the *Grosser Rat*
Tessin (30 Mar 1999) new constitution by the 1 Jan 1998			1 Jan 1998 (14 Dec 1997)	Non-existing Also non-existing in 1997 See also T/S 1999, p. 341
Waadt (21 Oct 2003) new constitution by 1 Sept 2003			1 Sept 2003 (14 Apr 2003)	Non-existing
Waadt (18 Jan 2000)	> 20 million (> 2 million at least for 10 years)	art. 27, 2 bis	29 Nov 1998 (29 Nov 1998)	Decrees of the *Grand Conseil* is subject to the assembly of the communal electorate (*assemblées de commune*)
Waadt (old)			1885	Non-existing See T/S 1999, p. 371
Wallis (21 Oct 2003)			1 June 1994 (24 Oct 1993)	Non-existing See T/S 1999, p. 382

Table 6: The Fiscal Referendum – Mandatory (Ordinary) (cont.)

Canton	Financial threshold	Article in cant. const.	effective since	Remarks
Neuenburg (16 Oct 2001) new constitution by 1 Jan 2002			1 Jan 2002 (24 Sept 2000)	Non-existing
Neuenburg (old)	> 1.5 % (> 1.5 per mill) of total cantonal revenues	art. 39, 3	1992 (8 Dec 1991)	Laws and decrees See also T/S 1999, p. 273
Genf (21 Oct 2003)				Non-existing See also T/S 1999, p. 223
Jura (4 July 2000)	> 5% (> 5 per mill) of the revenues of the last budget	art. 77, lit. d art. 77, lit. e	1 Jan 1979 (20 Mar 1977)	laws and decrees, all expenses not determined by a law see also T/S 1999, p. 253

See table 2. In the column 'Financial threshold', the threshold for recurring expenses is displayed in brackets, while the expenditure threshold for nonrecurring expenses is displayed above. All numbers are in Swiss Francs.

* *Grossratsbeschluss* (decree of the *Grosser Rat* of *St. Gallen*) of the 17th of November, 1923.

5.2 Comments on the Changes in the Mandatory Fiscal Referendum

Since 1997, only a few developments regarding the mandatory fiscal referendum (of the ordinary type) could be observed. In most of the cantons, either the mandatory fiscal referendum was abolished and replaced by an optional fiscal referendum or the threshold was increased substantially. Certainly, such a development might cause a decline in the subindex for the fiscal referendum.

The most prominent change is probably that which occurred in *Zürich*. After years of the existence of a mandatory fiscal referendum (from at least 1970 on; see T/S 1999, p. 406), the electorate of *Zürich* decided to abolish it and keep only the optional fiscal referendum. This new constitutional amendment became effective on the 1st of January, 1999. An elimination of the mandatory fiscal referendum and its replacement by the optional fiscal referendum also occurred in *Obwalden* (effective: the 29th of November, 1998) after the threshold for the mandatory fiscal referendum for laws and decrees had already been increased substantially on the 8th of June, 1997, from 300,000 to 1 million Swiss Francs. A similar development could

also be observed in the canton of *Neuenburg*, where on the 1st of January, 2002, the new constitution brought about the nonexistence of the mandatory fiscal referendum. Before that total revision, such an institution was present for nonrecurring expenses of more than 1.5% of the last total cantonal revenues (art. 39, 3 old CC). Additionally, in the canton of *Waadt*, an elimination of a previously existing mandatory fiscal referendum took place in April 2003. The development of this institution in this particular canton is described in more detail below. An obvious decline is noticeable in the level of direct democracy for these cantons.

An increase in the threshold for the mandatory fiscal referendum could be observed in various cantons. One of them is *Glarus*, in which the threshold for decrees was raised from 500,000 to 1 million Swiss Francs on the 5th of May, 2002. The canton *Solothurn* also partially revised its requirements for a mandatory fiscal referendum for decrees of the *Kantonsrat* and raised the old financial threshold from 2,000,000 to 5,000,000 Swiss Francs in 1998, which is more than a doubling of the old threshold valid since 1988. Similarly, the new constitution of the canton of *Schaffhausen* brought about a tripling of the old threshold for decrees from 1 million to 3 million Swiss Francs for nonrecurring expenses from the 1st of January, 2003, onward. At this point, it is also worth pointing out that the special mandatory fiscal referendum with a substantially lower hurdle of 300,000 Swiss Francs for decrees passed with less than a four-fifths majority in the *Grosser Rat* was not kept in the new constitution of *Schaffhausen*. In the canton of *Appenzell Innerrhoden*, on the 28th of April, 2002, the old threshold for decrees was doubled and is now fixed at 1 million Swiss Francs. The people of the canton of *Graubünden* approved new and higher expenditure thresholds for the mandatory fiscal referendum for the new constitution (effective as of the 1st of January, 2004): the figure is now 10 million Swiss Francs for nonrecurring expenses triggered by decrees of the *Grosser Rat* – twice as high as before the change. In general, increases in thresholds led the subindex of the mandatory fiscal referendum decline.

Constitutional amendments that did not lead to an alteration of the legal stipulations with respect to the mandatory fiscal referendum also occurred in some cantons during this period. One is *Freiburg*, in which a new constitution entered into force on the 1st of January, 2005, which, other than a renumbering of articles, did not affect the fiscal referendum. Another example is *Appenzell Ausserrhoden* in which, in 1997, the *Landsgemeinde* was abolished without touching the requirements for the mandatory fiscal referendum. The last change concerning the financial hurdle in this canton occurred previously on the 1st of May, 1996,

when it was set at 5% of a taxing unit (which equaled 1,650,500 Swiss Francs in 2003)[66]. In the canton of *St. Gallen*, a new constitution also came into force (1st of January, 2003) that did not affect the requirements for the mandatory fiscal referendum. The financial hurdles for laws and decrees are stipulated in a cantonal law (art. 6 *RIG*) and since 1996 the figure has been 15 million Swiss Francs for nonrecurring expenses. From a political economy perspective, the regulation of hurdles at the statutory and not at the constitutional level is very interesting, as already discussed in the introductory section of this chapter on the statutory referendum. A new constitution that became effective in the canton of *Tessin* on the 1st of January, 1998, also led to no alteration with regards to the nonexistence of the mandatory fiscal referendum. Since legal stipulations do not change, the subindex of the fiscal referendum is unaffected.

In a few cantons, there has been an empowerment of the people through the introduction of a mandatory fiscal referendum. One of these cases is the canton of *Aargau*, in which, after years of nonexistence since 1982 (see T/S 1999, p. 341), a mandatory fiscal referendum was introduced – although its power is mitigated by the inclusion of a majority restriction. The evaluation of this restriction was discussed in the introductory section of this chapter. The new expenditure hurdles are fixed at 5 million Swiss Francs for nonrecurring expenses and at 600,000 Swiss Francs for recurring expenses. On the 29th of November, 1998, the people of *Waadt* also passed a partial revision of its constitution, introducing a new fiscal mandatory referendum with a financial threshold of 20,000,000 Swiss Francs for nonrecurring expenses (art. 27, 2 bis old CC), which had never before been known in the history of *Waadt* (see T/S 1999, p. 368 cont.). It must be noted, however, that this mandatory fiscal referendum did not lead to a vote of the people of the canton but of the assembly of the local communes. In the new constitution (effective: the 1st of September, 2003), however, this mandatory fiscal referendum was again abolished. Only law-changing financial measures by the government to aid a budgetary disequilibrium are now subject to a mandatory referendum (art. 83, 2 CC). This regulation seems to aim at restricting raises in income tax rates in order to equilibrate expenses and revenue (cf. art. 165, 2 CC). Through this stipulation, the electorate of *Waadt* can influence, at least indirectly, the level of income tax rates in times of overall economic hardship.

[66] For recurring expenses, the threshold equals 331,000 Swiss Francs in 2003.

5.3. Optional Fiscal Referendum

Table 7: The Fiscal Referendum – Optional (Ordinary) between 1997 and 2003

Canton	Sign. Requ.	Time-limit for coll.	Financial threshold	Article in cant. const./ effective since (date of vote)
Zürich (3 Mar 2004)	5,000	60 days	> 3 million (> 300,000)	art. 28 bis, 1 num. 1 art. 30 bis 1, 2 1 Jan 1999 (27 Sept1998) decrees of the *Kantonsrat*
Zürich (old)	5,000	45 days	> 2 million (> 200,000)	art. 30 num. 2 1 Jan 1996 (25 Sept 1994)
Bern (21 Oct 2003)	10,000	3 months	2 million (> 400,000)	art. 62, 1 lit. c 1 Jan 1995 / (6 Jun 1993) decrees of the *Grosser Rat*
Luzern (21 Oct 2003)	3,000	60 days	laws: > 10 million (> 1 million on average for 10 years) decrees: > 3 million (> 300,000 on average for 10 years)	art. 39, 2 1 Jan 1977 (5 Dec 1976) art. 39 bis, 1 lit. a (25 Jun 1995) art. 39 bis, 1 lit. b (27 Sept 1998) (revision only w.r.t. extraordinary fisc. ref.) art. 40 1 Nov 1969 (14 Sept 1969) art. 39 bis, 3 1 Nov 1969 (14 Sept 1969) Laws and decrees of the *Grosser Rat*
Uri (1 Apr 2003)	450	90 days	> 500,000 (> 50,000 for at least 10 years)	art. 25, 2 lit. c, d 1 Jan 1994 (28 Nov1993) art. 25, 1 1 Oct 1997 (8 June 1997) art. 25, 3 1 Jan 1995 (28 Oct 1984) " new expenses": laws and decrees see also T/S 1999, p. 359
Uri (old)	300	90 days	> 500,000 (> 50,000 for at least 10 years)	art. 25, 2 lit. c, d 1 Jan 1994 (28 Nov1993) art. 25, 1 1955 art. 25, 3 1 Jan 1995 (28 Oct 1984) " new expenses": laws and decrees see also T/S 1999, p. 359

Table 7: The Fiscal Referendum – Optional (Ordinary) (cont.)

Canton	Sign. Requ.	Time-limit for coll.	Financial threshold	Article in cant. const./ effective since (date of vote)
Schwyz (18 Aug 2004)				Non-existing Stat. ref not applicable to decrees of the *Kantonsrat*, but applicable to laws and by-laws[67] See also tables 4 and 5
Obwalden (22 Oct 2002)	100	30 days	> 1 million (> 200,000)	(vote at the ballot box) art. 59 1, lit. b art. 59 2, lit. b 29 Nov 1998 (29. Nov 1998) decrees of the *Kantonsrat*
Obwalden (8 June 1997 - 29 Nov 1998)	100	30 days	(> 500,000) (> 100,000)	(*Landsgemeinde*) art. 61 num. 4 8 June 1997 (8 June 1997) art. 71, 1 8 June 1997 (8 June 1997) decrees of the *Kantonsrat*
Obwalden (old)	100	30 days	> 100,000 (> 20,000)	(*Landsgemeinde*) art. 61 num. 4 8 June 1986 (8 June 1986) decrees of the *Kantonsrat* art. 71, 1 8 June 1986 (8 June 1986) decrees of the *Kantonsrat*
Nidwalden (28 Dec 2001)	250	2 months	> 250,000 (> 50,000)	art. 52 lit. a, 1 1 Dec 1996 / (1 Dec 1996)
Glarus (30 Mar 2004)				No optional fiscal referendum Mandatory referendum see table 6
Zug (1 Apr 2003)	1,000	60 days	> 500,000 (> 50,000)	art. 34, 1 art. 34, 2 2 Dec 1990 (2 Dec1990) laws and general decrees of the *Kantonsrat*
Freiburg (18 Aug 2004) new constitution by 1 Jan 2005	6,000	90 days	> 0.25% of the total expenditures of the last budget (approved account)	1 Jan 2005 (16 May 2004) art. 46, 1 lit. b art. 46, 2 All acts of the *Grand Conseil*
Freiburg (old)	6,000	90 days	> 0.25% of the total expenditures of the last budget (approved account)	art. 28 bis, 3 7 Oct 1986 (8 June 1986) art. 130, 2 *PRG* all laws and decrees also applicable: art. 28 ter see also T/S 1999, p. 211 cont.

[67] Personal communication with Mr. GANDER, *Staatsschreiber* of *Schwyz.*

Table 7: The Fiscal Referendum – Optional (Ordinary) (cont.)

Canton	Sign. Requ.	Time-limit for coll.	Financial threshold	Article in cant. const./ effective since (date of vote)
Solothurn (30 Mar 2004)	1,000	90 days	> 1 million (>100,000)	art. 36 1, lit. a art. 36, 2 1 Jan 1988 (8 June 1986)
Basel-Stadt (4 July 2000) (new)	2,000	6 weeks	> 1,5 million (or = sum of all annual expenses over all years)	art. 29, 3 lit. c 21 Jun 1979 (24 Sept 1978) see T/S 1999, p. 163 art. 22 *Finanzhaushaltsgesetz* (1 Jan 1998)
Basel-Stadt (old)	2,000	6 weeks	> 1 million (> 200,000)	art. 29, 3 lit. c 21 Jun 1979 (24 Sept 1978) see T/S 1999, p. 163 art 1, 1 *ArefG*
Basel-Land (22 Oct 2002)	1,000	8 weeks	> 500,000 (> 50,000)	Decrees of the *Landrat* art. 31, 1 lit. b art. 31, 1 art. 31, 3 1 Jan 1987 (4 Nov 1984)
Schaffhausen (21 Oct 2003) new constitution by 1st Jan 2003	1,000	90 days	> 1 million (> 100,000)	Decrees of the *Kantonsrat* art. 33, 1 lit. d art. 33 , 2 1 Jan 2003 (17 Jun 2002)
Schaffhausen (old)	600	60 days	> 300,000 (> 50,000)	Decree of the *Grosser Rat* if 4/5 of present members have agreed art. 42 ter, 1 art. 42 ter, 2 (23 Apr 1989)
Appenzell AR (3 Apr 2001)			1 May 1996 (30 Apr 1995)	Non-existing (*Landsgemeinde*) (28 Sept 1997: abolishment of the *Landsgemeinde* and replacement of open vote by voting at the ballot box)
Appenzell IR (30 Mar 2004)	200	30 days	> 250,000 > 50,000	art. 7 ter, 2 art. 7 ter, 3 (26 Apr 1992) (*Landsgemeinde*) Decrees of the *Grosser Rat*
St. Gallen (9 July 2002) new constitution by 1 Jan 2003	4,000	40 days	> 3 million (> 300,000 for at least 10 years)	art. 49, 1 lit. c; art. 50, 1 1 Jan 2003 (21 Jun 2001) art. 7, 1 *RIG*: IV NG (11 Apr 1996) Decrees of the *Grosser Rat*

Table 7: The Fiscal Referendum – Optional (Ordinary) (cont.)

Canton	Sign. Requ.	Time-limit for coll.	Financial threshold	Article in cant. const./ effective since (date of vote)
St. Gallen (old)	4000	30 days	> 3 million (> 300,000 for at least 10 years)	art. 47 art. 7, 1 *RIG*: IV NG 1 Apr 1997 (11 Apr 1996) higher upper limits through mandatory fiscal referendum decrees and laws
Graubünden (6 July 2004) new constitution by 1 Jan 2004	1,000	90 days	> 1 million (> 300,000)	art. 17, 1 art. 17, 1 num. 3 art. 17, 3 1 Jan 2004 (18 May 2003 / 14 Sept 2003) decrees of the *Grosser Rat* change in upper limits through mandatory fiscal referendum
Graubünden (old)	3,000	90 days	> 1 million (> 300,000)	art. 2, 2 num. 6 lit. b (28 Jan 1973) Decrees of the *Grosser Rat*
Aargau (30 Mar 2004)	3,000	90 days	> 5 million (> 500,000)	art. 63, 1 lit. d art. 63, 1 1 Jan 2003 (2 Jun 2002) decrees of the *Grosser Rat* art. 40 *GPR* (10 Mar 1992)
Aargau (old)	3,000	90 days	> 3 Mio (> 300,000)	art. 63, 1 lit. c art. 63, 1 Decrees of the *Grosser Rat* 1 Jan 1982 (28 Sept 1980) art. 40 *GPR*
Thurgau (22 Oct 2002)	2,000	3 months	> 1 million (> 200,000)	art. 23, 2 1 Jan 1990 (4 Dec1988) decrees of the *Grosser Rat*
Tessin (30 Mar 1999) new constitution by the 1 Jan 1998	7,000	45 days	> 1 million (> 250,000 for at least four years)	art. 42 lit. b 1 Jan 1998 (14 Dec 1997) decrees and laws ("*gli atti*")
Tessin (old)	7,000	1 month	> 200,000 (> 50,000 for at least 4 years)	art. 60, 2 (31 May 1970) See also T/S 1999, p. 338 cont.

Table 7: The Fiscal Referendum – Optional (Ordinary) (cont.)

Canton	Sign. Requ.	Time-limit for coll.	Financial threshold	Article in cant. const./ effective since (date of vote)
Waadt (21 Oct 2003) new constitution by 1 Sept 2003	12,000	40 days	No threshold	art. 84, 1 lit. a art. 84, 2 lit b art. 84, 3 1 Sept 2003 (14 Apr 2003) optional statutory referendum applies to decrees (see also table 5) exceptions: art. 84, 2 lit. b
Waadt (18 Jan 2000)	12,000	40 days	> 2 million (> 200,000 for 10 years)	art. 27, num. 2 29 Nov 1998 (29 Nov 1998) all decrees of the *Grand Conseil*
Waadt (old)	(12,000)	(40 days)	No threshold	optional statutory referendum applies (see table 5) see also T/S 1999, p. 369 art. 27, num. 2 1978
Wallis (21 Oct 2003)	3,000	90 days	> 0.75% (> 0.25 %) of the total gross expenditure of the last administration and investment account	decrees, but only for extraordinary expenses art. 31, 3, num. 2 art. 31 1, num. 3 1 June 1994 (24 Oct 1993)
Neuenburg (16 Oct 2001) new constitution by 1 Jan 2002	4,000	40 days	> 400,000 (acts of the *Grand Conseil*)	decrees which trigger expenses, laws triggering non-budgetary expenses art. 42, abs. 2 lit. b art. 42, 1 art. 42, 3 1 Jan 2002 (24 Sept 2000) Laws: opt stat. ref. applies (identical requirements)
Neuenburg (old)	6,000	40 days	art 39, 2 art. 120 *LDP* > 3 per mill of the approved budget (decrees of the *Grand Conseil*)	Simple decrees which trigger a new expense Laws: opt. stat. ref. applies (identical requirements)

Table 7: The Fiscal Referendum – Optional (Ordinary) (cont.)

Canton	Sign. Requ.	Time-limit for coll.	Financial threshold	Article in cant. const./ effective since (date of vote)
Genf (21 Oct 2003)	7,000	40 days	> 125,000 (> 60,000)	Laws triggering non-budgetary expenses art. 56 (2 Febr 1986) art. 53 (7 Mar 1982) art. 57 19 Feb 1960 (7 Feb 1960) art. 54, 1 19 Dec 1958 (7 Dec 1958) See also T/S 1999, p. 223-226
Jura (4 July 2000)	2,000	60 days	> 5 per mil (> 0,5 per mil) of the revenue of the last budget	art.78 lit. b All expenses not determined by a law 1 Jan 1979 (20 Mar 1977) art. 94 *LDP*
See tables 2 and 6.				

5.4 Comments on the Changes in the Optional Fiscal Referendum

Most of the constitutional revisions regarding the optional fiscal referendum after 1997 concern an alteration of the signature requirement or the financial threshold for this institution. Only in a few cantons was an elimination of the mandatory fiscal referendum (see section 5.2) accompanied by an adaptation/extension of the optional fiscal referendum.

After 1997, a major change regarding the optional fiscal referendum occurred in the canton *Zürich* when the mandatory fiscal referendum was abolished at the end of 1998 and only a revised version of the optional fiscal referendum remained. In the same partial revision of the cantonal constitution, the period allotted to the collection of the signatures was increased from 45 to 60 days. The financial threshold was augmented to 3,000,000 Swiss Francs in contrast to 2,000,000 Swiss Francs, which prevailed until the end of 1998 (see T/S 1999, p. 408 cont.). Despite this constitutional change, from 1999 on, the value of the index of the optional fiscal referendum for ZH reached 4, which it had already achieved in 1998 and 1997.

The canton *Luzern* also introduced a change with respect to optional fiscal referendum, but as this did not relate to any of the requirements that form the basis for the derivation of the index, it had no impact on the index value for the optional fiscal referendum. Only with

respect to the extraordinary fiscal referendum did a change occur concerning the number of members necessary to call in such an institution (art. 39 bis, 1 lit. b CC). This amendment became necessary because the size of the cantonal parliament had been reduced from 170 to 120 seats[68].

The canton *Uri* is one of the rare cases in which the index value for the optional fiscal referendum remained the same for the period between 1997 and 2001, although a partial revision with respect to the requirements did take place. From 1997 to 1998, the number of signatures requested was raised from 300 to 450 (see T/S 1999, p. 359), applicable to all optional referenda. This revision, however, neither changed the results of evaluation of the absolute quantity of signatures nor that of the relative number of signatures, because both numbers (still) fall into the identical categories before and after the change.

In the canton *Obwalden* the electorate voted successfully for a supersession of the *Landsgemeinde* status on the 8th of June, 1997. An optional fiscal referendum was introduced with a signature requirement of 100 voters to be collected within 30 days, and a financial threshold of 500,000 Swiss Francs for nonrecurring expenses was set. In 1998, this threshold was then again doubled to 1,000,000 Swiss Francs; the identical development occurred for recurring expenses. Hence, the index for the optional fiscal referendum was negatively affected in both 1998 and 1999. The overall index of fiscal referendum therefore reflects the (lower) value of the optional fiscal referendum as no mandatory referendum has existed since 1998.

The optional fiscal referendum was also revised in the canton of *Tessin* on the 14th of December, 1997, which became effective with the new constitution on the 1st of January, 1998. The number of signatures necessary for such a referendum was kept at 7,000, but the time allotted for their collection increased from 30 to 45 days (see also T/S 1999, p. 340). The financial threshold was also augmented to 1,000,000 Swiss Francs from the original 200,000 Swiss Francs (see also T/S 1999, p. 341). This development meant an increase in the value for the fiscal referendum from 1997 onwards.

[68] Personal communication with Mr. H. BACHMANN, *Rechtsdienst* of *Luzern* (21/01/2004).

Changes also occurred in *Waadt* starting in 1999. This canton (re)introduced the optional fiscal referendum as a new institution in its cantonal constitution[69], with a signature requirement of 12,000, 40 days allowed for their collection, and a financial threshold set at 2,000,000 Swiss Francs. These new characteristics of the optional fiscal referendum allowed canton *Waadt* to achieve an index value of 3 between 1999 and 2001 for the optional fiscal referendum. At this point, it should be noted that in canton *Waadt* before 1999, financial expenditure projects were (theoretically) covered by the statutory referendum (see T/S 1999, p. 371). However, according to my source[70], because the statutory referendum was never applied to financial issues, its potential scope was never fully exploited by the electorate (see also the description of the fiscal referendum in chapter III). Interestingly, the requirements for this new optional fiscal referendum were copied from those for the already existing optional statutory referendum, which shows the closeness of these two institutions. When STUTZER (1999) calculated the index values of direct democracy, however, he counted the *de iure* applicability of the optional statutory referendum toward the index of the fiscal referendum. However, in contrast to the new stipulation of 1999, the optional statutory referendum of the old constitution fixed no financial threshold. Therefore, a higher index value is observed for the optional fiscal referendum before 1999.

In October 2003, the people of *Waadt* adopted a new constitution that became effective on the 1st of September, 2003. Besides the abolishment of the mandatory fiscal referendum for expenditure projects, it introduced a '*référendum facultatif*' that serves both as a statutory referendum (art. 84, 1 lit. a CC) and as fiscal referendum. Exempted from this referendum are the cantonal budget, supplementary credits, bonds, bound expenses, and accounting (art. 84, 1 lit. b CC)[71]. This stipulation means, then, that unbound (non-budgeted) expenses – i.e. expenditure projects and also tax-related issues – can be subject to an optional (fiscal) referendum. The advantage of this stipulation, from a direct democratic perspective, is that, as in the old constitution before its November 1998 revision, no expenditure threshold exists.

According to the new constitution in *Neuenburg*, which became effective on the 1st of January, 2002, 4,500 voters can demand a *réferendum populaire* on a decree that triggers expenses within 40 days after its publication (art. 42, 2 lit. b CC); the same applies to any law which does not relate to the budget or the account (art. 42, 2 lit. a CC; art. 42, 3 CC). This

[69] An optional fiscal referendum also existed in the constitution of 1961 until 1978 (see T/S 1999, p. 371).

[70] Personal communication with Prof. G. KIRCHGÄSSNER, March 2004.

[71] This article resembles the regulation of art. 27, num. 2 ter, old CC for the mandatory referendum.

new optional (fiscal) referendum applies to acts (*actes*) of the *Grand Conseil*, the cantonal parliament (as in the old constitution, see T/S 1999, p. 273). On the surface, there appears to be no financial threshold for this referendum; however, the state organ responsible for making financial decrees or laws on expenses above 400,000 Swiss Francs is the *Grand Conseil* (the parliament), whereas for those below this threshold the responsibility falls to the *Conseil d'Etat* (the executive organ). Thus, the optional referendum appears only to apply to decrees with expenses above 400,000 Swiss Francs[72]. There are, however, some differences with respect to the stipulations in the old constitution: first, a reduction of the signature requirement has occurred (6,000 to 4,500); second, in the new constitution, the referendum is applicable to all acts that meet the threshold requirement, not only to decrees that trigger a 'new' expense as in the old constitution (art. 39, 2 old CC). It must also be noted, however, that there existed in the old constitution a mandatory referendum on expenditure projects for both laws and decrees that did not survive the recent total revision of the constitution (see table 6). For more details of how the system of financial referenda worked under the ancient rule, see T/S 1999, p. 272 cont.

6 Reconstruction of the Index between 1997 and 2003

The index of direct democracy is an unweighted average of four different subindices that reflect different constitutional settings regarding four institutions of direct legislation: the constitutional initiative, the statutory initiative, the fiscal referendum, and the statutory referendum. These four subindices are based on an assessment of the following requirements: the number of signatures (both absolute and relative), the time period for collection, and, in the case of the fiscal referendum, the financial threshold. Both STUTZER (1999) and STUTZER and FREY (2000) describe the construction of this index. The reader should note, however, that each article presents a different version of the index. The difference lies in the question of whether the expenditure threshold per electorate or per residential population is used in constructing the subindex of the fiscal referendum. Whereas the last paper uses the threshold per vote, STUTZER (1999) uses the threshold per capita, which includes not only the cantonal electorate but also foreign residents holding a permit of a year or longer[73]. The first version of the relative threshold seems to take into account the decision-making power of the single

[72] Personal communication with Ms. DESPLAND, *Chancellerie d'Etat* of *Neuenburg* (12/10/2004).

[73] In some cantons, foreign permanent residents account for up to 20% of the population. It takes 13 years of residence to gain Swiss citizenship.

citizen with respect to financial matters of the canton, while the second versions is based on the view of the threshold as a financial burden shared among all residents of the canton. As the studies of FREY and STUTZER also focus on the procedural utility gained by Swiss citizens in contrast to foreign residents, the use of the first version of this index makes sense in their research context. However, as regards the signature requirement, both versions of the index are constructed using the number of signature divided by the electorate. The correlation between these two index versions exceeds the value of 0.9. In the case of this study, the number of signatures per voter and the financial threshold per resident is used to reflect the financial burden. According to L.P. FELD, this latter setup was also chosen for the construction of the index between 1980 and 1997 carried out by his colleague CH.A. SCHALTEGGER.

Regarding the updating of this index, a few general comments need to be made. The constitutional initiative that forms part of the index refers exclusively to the partial revision of the constitution (see STUTZER 1999). In many cantons, it is easier to launch a partial revision than a total revision of the constitution. Concerning the mandatory and the optional statutory referendum, in general, this study only takes into account those referenda referring to all types of legislative acts, not those referring to administrative acts. Hence, the loss or gain in citizen power with respect to the latter should not affect the index of direct democracy for a canton. Distinction between the two, however, is almost unmanageable (see section 1), but, fortunately, in most of the cantons the referendum on laws, on decrees and on by-laws is equivalent in their requirements. Concerning the fiscal referendum, only the threshold of nonrecurring expenses is taken into account, and changes in thresholds for recurring expenses are not reflected in the index of direct democracy (see STUTZER 1999). The index does not distinguish whether the expense is a consequence of a law or of a decree; however, in daily political practice, expenditure projects induced by a decree are more often the object of a fiscal referendum. Changes that occur after April 1st in any given year are accounted for in the following year; for example, the changes in the constitutions of *Obwalden* in June 1997 and of *Glarus* in May 2002 influence the index of direct democracy only from 1998 and 2003 onwards, respectively. As regards the signature requirement, a month is counted as corresponding to 30 days, 6 weeks to 45 days, and so on.

Regarding the statutory referendum, 6 index points are awarded to those cantons in which a mandatory statutory referendum exists. In the case of an optional referendum, index points are

awarded based on its requirements (absolute and relative signature requirement, time period for collection). Usually there exists in Swiss cantons only one of the two types of statutory referenda referring to cantonal laws (and decrees), and hence the final subindex value for the statutory referendum is out of the question. The difficulty with this index lies in the question of whether decrees and by-laws of the cantonal parliament are regarded as 'laws' or as administrative acts, and whether they should be included in this index or not. This distinction differs from canton to canton, and a decision can only be made according to the legal and political practice. For example, in the case of *Uri*, both the mandatory and the optional referendum exist, although the optional referendum concerns only parliamentary by-laws not formal laws. In this case, STUTZER (1999), taking into account that both types exist in parallel, decided to calculate the average of both referenda (p. 5). For consistency, I have maintained the averaging of both institutions for the case of *Uri*. In addition, for the canton of *Schwyz*, the mandatory referendum applies to laws and the optional referendum to by-laws, so here again the average of the two referenda is used because some by-laws in particular areas aim at substituting for formal laws (STUTZER, 1999, p. 5). Again to maintain consistency, I will also follow this approach. In a few cantons (SO, BL, SH, AG), a majority restriction was introduced for the application of the mandatory statutory referendum during the period from 1997 to 2003; if this requirement is not met, the optional referendum applies. In these cases, I replicate the approach chosen by STUTZER (1999) and calculate the average of both types of statutory referenda. As the constitutional change in *Glarus* led de facto to no restriction of the use and scope of the mandatory statutory referendum for cantonal laws, only this institution will be taken into account for the index construction.

As regards the fiscal referendum, both the optional and the mandatory referenda are awarded index points separately and an index value is calculated for each. The higher value of the two is then chosen, which constitutes the subindex of the fiscal referendum. In general, no distinction is made as to whether the fiscal referendum applies to a law or to a decree. For most of the cantons, the threshold for expenditure projects induced by laws or decrees is identical. Only the canton of *Luzern* makes an exception: here, the considerably lower financial threshold for decrees is employed as done in STUTZER (1999). These thresholds have remained unchanged since 1995. In the canton of *Schaffhausen*, the fiscal referenda apply solely to decrees, not to laws; hence, the value of the subindex is based exclusively upon these constitutional stipulations. In the old constitution prior to January 2003, a lower threshold for the mandatory fiscal referendum was fixed when a decree was passed with less than a four-fifths majority. STUTZER (1999) did not take this specific regulation into account, probably

because it did not apply often in practice. Consequently, in the new constitution of *Schaffhausen*, this particular regulation was dropped, and no majority requirement any longer applies to any fiscal referendum.

In the canton of *Aargau*, a mandatory fiscal referendum was introduced in January 2003 after a time of nonexistence, but it was accompanied by a majority restriction. This new development is taken into account by averaging the index points for the optional and the mandatory fiscal referenda, analogously to similar cases for the mandatory statutory referendum. Regarding the canton of *Schwyz*, in which no ordinary optional fiscal referendum exists, STUTZER (1999) reported taking into account the optional fiscal referendum for state highways; in this case, the average points of the mandatory and the optional fiscal referendum (for state highways) is calculated and used as a fiscal subindex. Personally, I find this approach inconsistent because other cantons such as *St. Gallen* have similar optional fiscal referenda on special issues, particularly on infrastructure projects; for these cantons, however, this type of optional fiscal referendum is not taken into consideration during calculation of the fiscal subindex. Nevertheless, to keep the index consistent over time, I will maintain the special treatment for the canton *Schwyz*. In 1999, a new cantonal decree on streets (*Strassenverordnung*) was adopted by the *Kantonsrat* of *Schwyz* (effective: the 1st of January, 2000), which brought about a higher threshold for this optional fiscal referendum (new: art 20, 2 CC) to which an expense of more than 20,000,000 Swiss Francs is subject (prior to change: 2,000,000 Swiss Francs). As regards the signature requirement and the time for collection, the regulations have remained unchanged since 1972.

In STUTZER (1999), for the cantons *Wallis* and *Genf*, the requirements for the optional fiscal referendum were not taken into account during calculation of the subindex value for the fiscal referendum because "only non-budgetary or extraordinary" expenses are subject to this referendum (p.7). In STUTZER's opinion, this omission weakens this political institution so considerably that it leads to a de facto nonexistence. Since the publication of his calculations in 1999, the constitutional requirements in *Wallis* and in *Genf* have not changed (see tables 6 and 7). In the constitution of *Wallis*, in art. 31, 1 num. 2, it is explicitly stated that ordinary expenses are not subject to an optional (fiscal) referendum. A mandatory referendum exists only in its extraordinary form (since 1994) and hence does not influence the subindex of fiscal referendum. In the canton of *Genf*, a popular vote on any expenditure can be avoided by the legislature through the inclusion of this expenditure in the budget law (art. 54, 1 CC), so that the reasons for a considerable restriction of its use stated in STUTZER (1999) still apply (p.7).

Hence, in these cantons, the optional fiscal referendum has been awarded an index value of 1 as if this referendum were nonexistent.

Interesting also is the case of *Waadt*. As a very innovative step, this canton introduced a mandatory fiscal referendum in 1998. In contrast to the stipulations in the other constitutions, this mandatory fiscal referendum could only be used by the assembly of the communes, not by its cantonal electorate. I have, however, decided to regard this mandatory referendum as a fully valid mandatory referendum as if the electorate had been given the right to vote[74]. Since there are about 380 communes in the canton of *Waadt*, the size of each of them will be so small that a single citizen can be assumed to have quite a decisive (indirect) influence on the outcome of the mandatory fiscal referendum. In the new constitution of 1 Sept 2003, however, this mandatory fiscal referendum was again abolished.

Finally, and most importantly, I must point to the case of *Zürich*. Until the end of 1998, a mandatory fiscal referendum existed that was then replaced by an optional fiscal referendum. The existence of this important institution was overlooked by TRECHSEL and SERDÜLT in a first unpublished version of a table summarizing the mandatory fiscal referenda in Switzerland, on which STUTZER (1999) then based the calculation of the index for 1996[75]. As a consecutive fault, the subindex for the fiscal referendum in *Zürich* in STUTZER (1999) is based on incorrect information and is significantly lower then it should be (4.00 instead of 5.00). In STUTZER and FREY (2000), however, this mistake has been corrected and the index values reported for *Zürich* for 1992 are correct.

7 The Development of the Index of Direct Democracy from 1997 to 2003

Table 9 displays the values that the index takes on for the 26 cantons during the relevant years. In addition, for reasons of comparison in table 8, three different versions of the index are displayed: that published by STUTZER (1999) for 1996, that constructed by FELD and SCHALTEGGER for a synthetic panel running from 1980 to 1998, and, finally, my own calculations for the years 1997 and 1998. I note that in T/S 1999, and consequently STUTZER's

[74] Since in this case both mandatory and optional fiscal referenda are awarded identical points for the period in question, an averaging over the two institutions as an alternative would have brought about the identical subindex value. Also taking into account only the optional fiscal referendum would not have changed the value of the total index.

[75] In a personal communication with A. STUTZER, his mistake in STUTZER (1999) was admitted.

analysis, the so-called *Landsgemeinden* (OW, NW, GL, AR, AI) – i.e. those cantons that knew no representative power of legislation in or until shortly prior to 1996 – were excluded from the description of institutions.

Table 8: Comparison of Index Values of Direct Democracy

Canton	(1996) Stutzer	(1996) Feld	(1997) Feld	(1997) Fischer	(1998) Feld	(1998) Fischer
ZH	4.17	4.167	4.167	4.417	4.167	4.417
BE	3.02	3.021	3.021	3.021	3.021	3.021
LU	4.42	4.000	4.000	4.417	4.000	4.417
UR	5.29	5.125	5.125	5.292	5.125	5.125
SZ	4.99	4.656	4.656	4.990	4.656	4.990
OW		5.833	5.833	5.833	5.833	5.333
NW		5.000	5.000	4.438	5.000	4.438
GL		5.500	5.500	5.750	5.500	5.333
ZG	4.42	4.417	4.417	4.417	4.417	4.417
FR	2.85	2.854	2.917	2.854	2.917	2.854
SO	5.67	5.667	5.667	5.667	5.667	5.667
BS	4.4	4.396	4.396	4.396	4.396	4.396
BL	5.69	5.688	5.688	5.688	5.688	5.688
SH	5.21	5.000	5.000	5.208	5.000	5.208
AR		5.500	5.500	5.500	5.500	5.500
AI		5.250	5.250	5.375	5.250	5.375
SG	3.58	3.167	3.167	3.458	3.167	3.458
GR	4.75	4.500	4.500	4.833	4.500	4.833
AG	5.46	5.458	5.458	5.458	5.458	5.458
TG	4.33	4.333	4.333	4.333	4.333	4.333
TI	2.10	1.854	1.854	2.104	1.854	2.250
VD	2.42	2.417	2.417	2.417	2.417	2.417
VS	3.58	3.250	3.250	3.583	3.250	3.583
NE	2.19	2.188	2.250	2.188	2.250	2.188
GE	1.75	1.750	1.750	1.750	1.750	1.750
JU	4.02	3.708	3.708	3.708	3.708	3.708

A comparison of STUTZER's version with that of FELD for 1996 reveals that the value of *Zürich*, which was correct for 1992 (see e.g. STUTZER and FREY 2000) was perpetuated by FELD and maintained until 1998, when at least since 1996, it should have been substantially

higher (4.417 instead of 4.17). Minor differences can also be observed for the cantons LU, UR, SZ, SH, SG, GR, TI, VS, and JU. Unfortunately, I was not given access to the calculations by which FELD and SCHALTEGGER constructed their version of the index, so the causes for these differences cannot be traced. However, the main reason for the differences between their index and mine may lie in the fact that, according to a personal communication with CH.A. SCHALTEGGER, they also took into account the so-called recurring expenses in the index for the fiscal referendum. The original index developed by STUTZER (1999) was, however, constructed solely on the basis of one-time (i.e. non-recurring) expenditures and neglects the thresholds for recurring expenses. The reader should note, however, that in most of the cantons, the nonrecurring threshold equals the recurring threshold times the number of years in which it reoccurs, so the differences in the index resulting from different evaluation of financial thresholds are influential only in a few cantons. The correlation between the three versions of the index of direct democracy, however, exceeds the 90% level so that econometric analyses using either version should not produce substantially different results.

Table 9: Overview of the Index of Direct Democracy 1997 - 2003

Canton	1997	1998	1999	2000	2001	2002	2003
ZH	4.417	4.417	3.500	3.500	3.500	3.500	3.500
BE	3.021	3.021	3.021	3.021	3.021	3.021	3.021
LU	4.417	4.417	4.417	4.417	4.417	4.417	4.417
UR	5.292	5.125	5.125	5.125	5.125	5.125	5.125
SZ	4.990	4.990	4.990	4.927	4.927	4.927	4.927
OW	5.833	5.333	4.625	4.625	4.625	4.625	4.625
NW	4.438	4.438	4.438	4.438	4.438	4.438	4.438
GL	5.750	5.750	5.750	5.750	5.750	5.750	5.500
ZG	4.417	4.417	4.417	4.417	4.479	4.479	4.479
FR	2.854	2.854	2.792	2.792	2.792	2.792	2.792
SO	5.667	5.667	5.250	5.250	5.250	5.250	5.250
BS	4.396	4.396	4.396	4.396	4.396	4.396	4.396
BL	5.688	5.688	5.688	5.479	5.479	5.479	5.479
SH	5.208	5.208	5.208	5.208	5.208	5.208	5.021
AR	5.500	5.500	5.500	5.500	5.167	4.917	4.917
AI	5.375	5.375	5.375	5.438	5.375	5.438	5.438
SG	3.458	3.458	3.458	3.458	3.458	3.458	3.521
GR	4.833	4.833	4.833	4.833	4.833	4.833	4.833
AG	5.458	5.458	5.458	5.458	5.458	5.458	5.438
TG	4.333	4.333	4.333	4.333	4.333	4.333	4.333
TI	2.104	2.250	2.250	2.250	2.250	2.250	2.250
VD	2.500	2.500	2.500	2.500	2.500	2.500	2.500
VS	3.583	3.583	3.583	3.583	3.583	3.583	3.583
NE	2.188	2.188	2.188	2.188	2.188	2.729	2.729
GE	1.750	1.750	1.750	1.750	1.750	1.750	1.750
JU	3.708	3.708	3.708	3.708	3.708	3.708	3.708

Source: Own calculations

Comparing my own calculated values for 1997 with the values presented by STUTZER for 1996, only a few differences can be observed. The cantons in question (besides *Zürich*) are *Freiburg* (FR), *Graubünden* (GR), and *Jura* (JU). First, it should be noted that no new or altered constitutional stipulations were observed in these cantons between 1997 and 2003, which could affect the index of direct democracy. However, small variations in the index can occur even if constitutional revisions are absent: that is, since relative numbers also play a significant role, a change either in size of residential population or cantonal electorate can lead to different values of institutional subindices. In addition, changes are also possible when the expenditure thresholds are defined as a percentage of some types of cantonal budget, revenues, or investments known to vary over the years. In the case of FR, for example, the variation in the index is induced by the expenditure threshold for the optional fiscal referendum, whereas in the cases of GR and JU, it is mainly the change in the size of the electorate that makes the relative signature requirement tighter (JU: VIR, GIR, GRR, FRR) or looser (GR: VIR)[76]. (Since the index points vary with brackets, however, not all minor changes in a relative requirement are automatically reflected by an index change.)

8 Brief Critique of the Index

This chapter will conclude with a brief critique of the composite index of direct democracy as constructed by STUTZER (1999). The index of direct democracy has many advantages: it combines the necessary requirements for utilizing a particular right and makes cantons with contrasting institutional equipments comparable. The sole prominent disadvantage is the lack of an inflation adjustment: that is, a threshold introduced decades ago loses its strictness over time, whereas in other cantons a threshold defined based on an annual (approved) budget or some of its components not only tends to grow with inflation but also varies with business cycles. In practice, however, as can be seen in the case of *Freiburg*, such a change in the threshold based on cantonal budget size affects the final index only on a very small scale. In addition, the missing adjustment of thresholds for inflation has no major influence on the value of the index of direct democracy: such an adjustment would only be decisive if it led to a switch in the category for evaluation of the relative financial hurdle of the fiscal referendum. Independent of the mandatory or the optional fiscal referendum is affected, a switch in brackets causes an alteration of the final index of direct democracy of 0.25 index points because only the higher subindex value of the two enters the final composite index of direct

[76] All other requirements remained unaffected.

democracy. Hence, the majority of the cantons would not be strongly affected by an adjustment of threshold for inflation. In a synthetic panel, the missing adjustment for inflation will not lead to biased coefficients because the inclusion of year dummies filters out the ‘inflation effect’ for the affected cantons. A comparison of the index of direct democracy over time, however, is only useful for a description of leaps in direct democracy, not small changes.

9 Appendix

Table A.1: Cantonal Laws Related to Institutions of Direct Democracy

Canton	Law	Abbreviation	Date of Enactment
ZH	Gesetz über die politischen Rechte	*GPR*	1 Sep 2003 effective: 1 Jan 2005
ZH	Gesetz über das Vorschlagsrecht des Volkes	*Initiativgesetz* (outdated)	1 Jun 1969 effective until 1 Jan 2005
BE			
LU	Stimmrechtsgesetz	*SRG*	21 Oct 1988
UR			
SZ			
OW			
NW			
GL			
ZG			
FR	Gesetz über die Ausübung der politischen Rechte	*PRG*	6 Apr 2001 effective: 1 Aug 2001
	Gesetz übe die Ausübung der politischen Rechte	*PRG* (outdated)	18 Feb 1876 effective until 1 Aug 2001
SO			
BS	Finanzhaushaltsgesetz	*Finanzhaushalts-gesetz*	(16 Apr 1997) effective: 1 Jan 1998
BS	Gesetz über das Ausgabenreferendum	*ArefG* (outdated)	(29. June 1978) effective until 1 Jan 1998
BL			
SH			
AR			
AI			
SG	Gesetz über Referendum und Initiative	*RIG*	27 Nov 1967 effective: 1 Jan 1968
GR	Gesetz über die Ausübung der politischen Rechte	*PRG*	7 Oct 1962
AG	Gesetz über die politischen Rechte	*GPR*	10 Mar 1992
TG			
TI	Legge sull'esercizio dei diritti politici	*LEDP*	7 Oct 1998 effective: 1 Jan 1999
	Legge sull'iniziativa popolare, sul referendum e sulla revoca del Consiglio di Stato	*LIRR* (outdated)	22 Jan 1954
VD	Lois sur l'exercice des droits politiques	*LEDP*	16 May 1989 effective: 1 Jan 1990
VS			
NE	Lois sur les droits politiques	*LDP*	17 Oct 1984
GE	Lois sur l'exercice des droits politiques	*LEDP*	15 Oct 1982
JU	Lois sur les droits politiques	*LDP*	26 Oct 1978

Chapter III:

Happiness

1 Introduction[77]

A more democratic system, especially a system with direct popular rights, is likely to produce political outcomes that are closer to the preferences of the median voter than a less democratic system.[78] Consequently, *ceteris paribus*, a greater exposure to democracy can be expected to raise individuals' well-being. Not only does such exposure lead to political results that are acceptable to a large part of a population, but citizens' well-being may also arise from their participation in the political decision-making process and from the perceived extent of the procedural fairness of this process. In fact, the utility gained from procedural fairness is found to be quantitatively much larger than the utility gained from a (democratic) political outcome.[79]

Therefore, we expect empirical research to show that a higher level of democratization of a country or state leads to a higher level of self-reported happiness. However, the limited empirical evidence from previous cross-sectional studies only partly supports this proposition. Based on a sample of about 40 nations drawn from the World Values Survey, SCHYNS (1998) and VEENHOVEN (2000) find a positive and significant correlation between the Freedom House Democracy Index and self-reported happiness. However, this correlation becomes insignificant once the different national income levels are controlled for.[80] In another study based on the World Values Survey, INGLEHART and KLINGEMAN (2000) note that "[our] findings undermine any simplistic assumption that democratic institutions are the main determinant of human happiness" (p. 180).

Simplistic relations between income and happiness are, however, also questionable. As earlier papers by ABRAMOVITZ (1959) and EASTERLIN (1974) already indicate, income growth may have a positive effect on personal happiness in the short run but not in the long run. As soon as individuals adjust to their new situation, the level of happiness may settle down to the old

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[77] This chapter is based on the joint paper "Is It Culture or Democracy? The Impact of Democracy, Income, and Culture on Happiness" by D. DORN, G: KIRCHGÄSSNER, A. SOUSA-POZA, and myself, discussion paper No. 2005-12, University of St. Gallen. D. DORN and A. SOUSA-POZA contributed particularly to the second international analysis, which does not form part of this dissertation chapter. The paper was presented at the workshop on Happiness, Economics and Interpersonal Relationships, (4/12/2004), the Swiss Household Panel Conference (26/02/2005), the American Public Choice Society Conference (10/03/2005), the Annual Conference of the Swiss Society for Statistics and Economics (17/03/2005), and the European Public Choice Society Conference (3/04/2005).

[78] See, e.g., POMMEREHNE (1978) or, for a theoretical model, FELD and KIRCHGÄSSNER (2001).

[79] See STUTZER and FREY (2003).

[80] See also BJØRNSKOV (2003) for a similar result.

equilibrium.[81] Consequently, during recent decades, the average level of life satisfaction has remained constant in many countries despite considerable economic growth. Several studies provide evidence for this observation.[82] Moreover, EASTERLIN (1974) shows that countries with rather different GNP per capita – for example, West Germany and Nigeria, to mention the two most extreme examples – had nearly the same average personal happiness rating (p. 106).[83] Further, JUNGEILGES and KIRCHGÄSSNER's (2002) international study finds that higher income per capita and especially higher income growth lead to higher suicide rates of both sexes and in nearly all age groups. If suicide is interpreted as a measure of ill-being, this finding clearly contradicts the idea of a positive relation between personal income and happiness.[84] On the other hand, differences in economic status within a country have a clear and consistent impact on personal happiness.[85] Thus, to correctly capture the impact of income on happiness, it is necessary to distinguish between the general income level (or average income) within a society (and its development), and the relative economic position that an individual or family occupies in this society.[86]

Besides political freedom and economic well-being, the culture in which a person resides could also influence subjective well-being. For example, people in different cultures may value certain aspects of life differently and could, therefore, have different perceptions of their own individual well-being under the same objective circumstances.[87] This possibility is also noted by EASTERLIN (1974, p. 108). Several more recent papers examine this relationship.[88] STUTZER and FREY (2000, 2003), for example, use language as a proxy for culture, and the resulting coefficients are typically highly significant. The use of language variables to reflect culture can be justified because in society, language serves as an important transmission

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[81] See, e.g., EASTERLIN (2001).

[82] See, e.g., the papers cited in FREY and STUTZER (2002, p. 413), and also FRANK (1997), OSWALD (1997), MCBRIDE (2001), and EASTERLIN (2003). The long-term impact may even go in the reverse direction, from happiness to economic growth. See KENNY (1999).

[83] See also the graph in FREY and STUTZER (2002, p. 417), which appears to indicate that poorer countries have lower happiness ratings but that above a level of about 5,000 US dollars per capita (in 1995 PPP), there exists no obvious relation between GNP per capita and personal happiness.

[84] On the contrary, they found that – once income effects are controlled for – higher civil liberty consistently leads to lower suicide rates.

[85] For the impact of relative income on happiness see, e.g., D'AMBROSIO and FRICK (2004). That absolute income might, nevertheless, also have an impact on happiness is shown, e.g., by SCHYNS (2002).

[86] Another question is whether it is really income and not wealth together with income that matters. For this, see HEADY and WOODEN (2004) or HEADEY, MUFFELS and WOODEN (2004).

[87] For this, see, e.g., LIJPHART (1979) or the difference between Europeans and Americans with respect to (economic) inequality shown in ALESINA, DI TELLA and MACCULLOCH (2004).

[88] See, e.g., the contributions in DIENER and SUH (2000).

channel of culture and its embedded view of the world, the social system, and customs. At the individual level, the language spoken shapes human patterns of thought.[89] Moreover, as shown in other studies, cultural differences, represented by languages, have strong impacts on political behavior.[90] Consequently, such cultural variations may not only be reflected in institutional differences but also in how the individuals value the contributions of political institutions to their individual welfare. Among other factors, that are closely related to culture and that might have an impact on people's happiness are religion and the amount of social capital available in a society (see, e.g., FERRISS 2002 or BJØRNSKOV 2003).

Analyzing these relationships requires a valid measure of individual happiness; two such measures are commonly used. While nearly all authors speak of happiness, only some surveys truly question respondents about their personal happiness; the others ask about personal satisfaction or well-being. This latter holds true for all Swiss surveys. However, personal satisfaction on the one hand and happiness on the other are quite different aspects of personal life,[91] particularly (but not exclusively) for speakers of the German language. Nevertheless, the literature usually assumes that these two personal emotions are comparable insofar as they are both highly correlated with themselves and with other explanatory variables.[92] Therefore, as is common, the two terms are here used interchangeably.

The only scholars who, after controlling for income (and sometimes also for language), find a positive influence of democracy on subjective life satisfaction are FREY and STUTZER in their analyses for Switzerland.[93] The Swiss federal structure allows for wide variations in political institutions, especially in direct popular rights, between the 26 different cantons. Further, major cultural differences are reflected in the four different official languages[94]. Thus, Switzerland can be (and has been) perceived as a laboratory in which to study the effects of various degrees of political institutions on political and social outcomes.[95] Other factors that

[89] See, e.g., ALLIOT (1999) or LAZEAR (1999).

[90] See, e.g. LIJPHART (1979), who in a study of the structure of party affiliations in four multilingual countries (including Switzerland) concluded that "because language is a crucial differentiator among nations, it is bound to be a major cleavage and a main source of partisan differences in 'nations' that are not linguistically homogeneous" (p .453).

[91] For the difference between satisfaction and happiness see, e.g., LANE (1991, chapter 22) or VEENHOVEN (2000a).

[92] See, e.g. VEENHOVEN (2000).

[93] See, e.g., FREY and STUTZER (2000, 2000a, 2000b), as well as STUTZER and FREY (2003).

[94] Also the degree of federalism (between the cantonal and the local level) exhibits considerable variation.

[95] See, e.g., FELD and SAVIOZ (1997) for the impact of direct democracy on economic welfare, FELD and KIRCHGÄSSNER (2001, 2004) for its effect on public finances, or FELD et. al. (2004) for the effect of direct

are difficult to measure and make comparisons among countries difficult are irrelevant for an analysis within one country. Therefore, Switzerland seems very well suited for testing the impact of institutional differences and cultural background on perceived happiness. The only disadvantage in this respect is that the variation in level of democracy (and in other political institutions) is much smaller between Swiss cantons[96] than between units in an international sample that might include, for example, established democracies like the United Kingdom or the United States, as well as relatively weak democracies like that of Russia. Nevertheless, measured on an international scale, the extent of democratic rights (as well as the degree of federalism) is extremely high for all Swiss cantons. Thus, the fact that a significant impact of democracy is observed within Switzerland but not in a cross-national setting is very surprising. Rather, a stronger influence of democracy on happiness might be expected in a cross-national study than in a study of the single democratic country of Switzerland.

It might be argued that a cross-national analysis of subjective well-being is difficult because countries vary with regard to a variety of determinants, especially (as mentioned above) culture, that might influence individual happiness. As already stated, however, Switzerland is also divided into four different language regions with rather different cultures, with the borderline being mainly between the German- and non-German-speaking ('Latin') parts comprised of the French- and Italian-speaking Swiss.[97] Thus, cultural aspects, which are already shown to be a main source of differences in the political behavior in different countries, also play a major role within Switzerland. In fact, voting patterns in recent public elections reveal substantial differences among the different language regions within Switzerland.

This chapter takes a closer look at the relation between democracy and perceived subjective well-being, while also taking into account the impact of income and culture. First, I briefly review the empirical results for Switzerland obtained by FREY and STUTZER in their various contributions. Second, using a similar model to that of FREY and STUTZER (2000), in the

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democracy on income (re)distribution. While possible cultural impacts are not at the centre of these studies, all use a dummy for the French- and Italian-speaking cantons to take possible cultural differences into account.

[96] In addition, political rights of Swiss citizens vary only with respect to the cantonal and local levels. At the federal level, citizens from all cantons have the same political rights, i.e. with regard to such important policy fields as foreign policy, trade, defense, etc. Among the important fields of politics at the cantonal level are education, welfare, and police.

[97] The share of those speaking the fourth language, Rhaeto-Romanic, is about 1% of the Swiss population and, therefore, in our context negligible. Moreover, practically all of these people speak fluent German, the main language of *Graubünden*, the only canton in which Rhaeto-Romanic is spoken.

subsequent section I reestimate this relationship allowing for the relative income position of the individuals and also using new, more recent data from the Swiss Household Panel (SHP). Using this dataset and panel techniques that control for individual heterogeneity, no robust relationship is observed between democracy and happiness in Switzerland. The chapter concludes with some remarks about the differences between the results of the two datasets.

2 Previous Empirical Research for Switzerland

All empirical studies by STUTZER and FREY are based on a cross-section of approximately 6,000 households from a 1992 dataset collected by LEU et al. (1997) that is a representative sample of the Swiss population.[98] The dependent variable in these studies measures general life satisfaction on a scale from 1 to 10.[99] The set of explanatory variables, which is very similar across their various contributions, includes economic, sociodemographic, and institutional variables. In most of their studies, cultural determinants and/or macroeconomic variables are included for robustness checks. These cultural variables are either the language of the residential commune, as in STUTZER and FREY (2000), or the main language of the respective canton, as, e.g., in FREY and STUTZER (2000, 2000a, 2003). However, as in FREY and STUTZER (2000b) or FREY et al. (2001), cultural variables are sometimes missing.

The main variable of interest is an institutional variable that refers to the situation in 1992: direct democracy, which is measured by an index ranging from 1 to 6[100]. In accordance with the literature mentioned above, the authors assume that increased exposure to direct democracy leads to policy outcomes that are closer to citizens' preferences[101]. This proximity should, in turn, make them happier. The estimation method used in their analysis is primarily

[98] Actually, there is oversampling of two groups, the elderly and the poor. However, a representative sample can be obtained by either weighting the different groups, as FREY and STUTZER do in all their papers, or by eliminating the oversampled observations (which is possible due to the construction of the dataset). As will be shown below, these two methods can lead to quite different results.

[99] The first three categories are aggregated to increase the number of observations for the lowest category.

[100] This nonweighted composite index is comprised of four separate indices for (i) the constitutional initiative, (ii) the statutory initiative, (iii) the statutory referendum, and (iv) the fiscal referendum (See STUTZER (1999) or STUTZER and FREY (2000) for a detailed explanation of the construction). FREY and STUTZER (2000, p. 937), as well as STUTZER and FREY (2000b, pp. 32 cont.), present tables that show the distribution of this index over all Swiss cantons.

[101] In addition, in some of their papers (e.g. FREY and STUTZER 2000a), they tested the impact of fiscal federalism which is also predicted to be utility increasing. They found the degree of federalism to serve as a "transmission mechanism of direct democracy's beneficial effects" (p. 157) (i.e. the variable turned out not to be significant in combination with direct democracy). See also FREY and STUTZER (2000, p. 928).

a weighted ordered probit model with robust standard errors obtained through clustering at the cantonal level.[102]

In several contributions the authors show that the index of direct democracy appears to be robust to different estimation methods and to the inclusion of additional control variables; the positive coefficient of direct democracy stays significant at least at the 5% significance level.[103] In FREY and STUTZER (2000b), it is shown that the impact of direct democracy is also robust to controlling for those five cantons in which direct democratic rights are exerted through an open vote (*Landsgemeinden*). Measured by the index employed, these cantons are identical to those that enjoy the highest level of direct democracy. In a variation of this approach, a regression excluding these cantons is carried out that also results in the same positive finding for direct legislative rights.[104] As STUTZER and FREY (2000) show, the impact of the existence of direct democracy is also robust to controlling for measures of the actual use of these institutions, proxied by the number of cantonal referenda. Moreover, the inclusion of interaction variables between dummies for personal characteristics and the index of direct democracy reveals that the gains in happiness are quite evenly distributed among different socioeconomic groups[105]. In FREY and STUTZER (2000, p. 927), the problem of causality concerning direct democracy is addressed through economic historical reasoning.

In their most recent contribution to the analysis of direct democracy on happiness for Switzerland, STUTZER and FREY (2003) focus on procedural utility. They test the existence of procedural utility in the political process, which only Swiss citizens should be able to enjoy. Again, the happiness enhancing impact of direct democracy is observed. They also separate the gain in outcome utility from the gain in procedural utility by estimating separate but identical models for both Swiss citizens and foreign residents. They find that the increase in happiness attributed to procedural utility is more than three times greater than the experienced increase in outcome utility.[106]

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[102] In some papers, they also present weighted OLS estimations.

[103] See, e.g. FREY and STUTZER (2000a). In this paper, they report the estimates when all possible cantonal determinants of happiness taken into consideration are simultaneously included in the model, i.e. besides others national income per capita or the main language of the canton. The (positive) coefficient of direct democracy remains significant.

[104] See STUTZER and FREY (2000b, footnote 18).

[105] See FREY and STUTZER (2000) and FREY and STUTZER (2000c) regarding the poor.

[106] In the second part of this chapter, they use the first wave of the SHP (1999) to test the hypothesis that greater direct democratic power of citizens in a canton leads to the belief that they have greater political influence. Thus foreigners, who do not have these political participation rights, should believe less in their political

3 New Estimates for Switzerland

3.1 Cross section of 1992 and Balanced Panel 2000 – 2002

We reestimate the FREY and STUTZER (2000) model but deviate from their approach in two respects. First, following the literature mentioned in the introduction, we do not look only at the effect of (absolute) personal income but rather distinguish between the income level within a canton on the one hand and the relative income position of the individual on the other. Second, we do not use only the LEU (1997) dataset but also new data from the Swiss Household Panel (SHP), a longitudinal panel survey whose data are gathered annually using computer-assisted telephone interviewing (CATI). In this survey of selected households, the primary household representative must answer all questions on the personal questionnaire, while the remaining household members are only asked a particular selection of questions. For the first wave, a representative sample of 5,074 households from the Swiss population was recruited, meaning that a total of 12,937 individuals were personally interviewed in the autumn of 1999. For 2002, the sample size of the panel was equal to 3,690 households, comprised of 9,544 individuals. A total of 5,705 individuals were personally interviewed. The response rate was between 84 and 89% of all individuals contacted, a total of 9,544 individuals in these households. As the information on life satisfaction is only available for the last three waves, i.e. for the years 2000 to 2002, our analysis is restricted to these three waves.

To make this analysis compatible with the FREY and STUTZER (2000) study, we restrict the sample to individuals older than 20. For the balanced panel, we also eliminate observations with missing values in the control variables; e.g. occupational status, age, or family type, as well as disabled status. Once missing income variables in one wave are replaced by values in the previous or subsequent wave(s), and negative income values are set to zero, the number of remaining observations in the panel declines from 5,362 persons in 2000 to 4,534 interviewees in 2002.

The dependent variable is derived from a question on general satisfaction with whose exact wording is as follows:

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influence. To test this hypothesis, they included an interaction term between the variables 'political participation possibilities' and 'foreigner'. The authors find this hypothesis supported by their results.

In general, how satisfied are you with your life if 0 means 'not at all satisfied' and 10 means 'completely satisfied'?

To be able to compare the results with those of FREY and STUTZER (2000) and to avoid inference problems caused by too few observations, we aggregate the four lowest categories (0, 1, 2, 3) into a single category.[107] To make the coefficients of the income variables comparable to those in FREY and STUTZER (2000), which uses data of 1992, we deflate the income data of the SHP using the same year as base year. These steps reduce the sample to 3,301 individuals, i.e. 9,903 observations. To estimate the model, we first use an unweighted random-effects ordered probit model, thereby allowing for individual heterogeneity.[108] As a robustness test, we also carry out the identical estimation for the unbalanced panel, which leaves approximately 4,000 more observations. A fixed effects approach was not deemed appropriate because of the time invariance of the dependent variable for many individuals, as well as the de facto time invariance of the institutional variables of interest.[109] To control for other factors besides the income variables, we use the same explanatory variables as FREY und STUTZER (2000) and also include year dummies. In addition, however, we also employ dichotomous variables for various religious denominations and a dichotomous variable for a bad health state.

To test the relative income hypothesis and to allow for the likely nonlinearity of the income effect, we include subsistence income, measured as 40% of average income in the respective canton; the difference between actual and subsistence income; and the squares of these differences calculated separately for positive and negative differences. Assuming a positive but decreasing marginal utility of income, we expect a positive sign for the relative income

[107] As the scale ranges from 0 to 10, it includes one more category than the ones in the LEU-dataset used by FREY and STUTZER (2000). As the results in table A.2 of the Appendix show, the distributions over the remaining groups are quite similar for the three waves of the panel, the panel altogether, and the LEU data set. (In calculating these statistics, the data are assumed to be cardinal and not ordinal, as they actually are. However, FERRER-I-CARBONELL and FRIJTERS (2004) have shown that assuming cardinality or ordinality makes little difference.) – Descriptions of the variables, the distribution of the observations of the life satisfaction variable, and descriptive statistics of the index of direct democracy are given in tables A.2, A.3 and A.4 of the Appendix.

[108] The calculations have been performed by using the reoprob command in Stata, Version 8.1. (See for this FRECHETTE (2001, 2001a).) Points for the Gaussian-Hermite quadrature approximation are set at 30. – Since the three waves are each representative for Switzerland, estimation without weights seemed appropriate. For estimation of the three single cross sections separately, see section 3.2. The reoprob command does not allow clustering of aggregate level variables.

[109] Furthermore, with only three time periods, the coefficients in a fixed effects ordered probit would have been subject to a very severe bias. See for this, e.g., GREENE (2004).

and a negative sign for the (positive) difference between actual and subsistence income.[110] If only relative income matters, the coefficient of subsistence income should be zero. If absolute income matters, the coefficients of subsistence and relative income should be both positive and identical.

As is common in such studies, language – which can play an important role at different levels, institutional as well as individual – proxies for culture. Culture at the cantonal level can be represented by the dominant language, which may be a decisive covariate because it can, to a rather large degree, 'explain' the level of direct democracy,[111] meaning that cantonal culture might shape the very political institutions at the centre of this analysis. Because there are three dominant languages in Swiss cantons, three dummy variables are used. Similar arguments hold for local culture, but corresponding data are only available in the LEU dataset. Controlling for culture (i.e. language) at the individual level may also be important because the perception of the benefits of democratic institutions may vary with individual cultural background. Moreover, the perception of happiness and what contributes to personal satisfaction may differ among individuals with different cultural backgrounds. Because of the high percentage of foreigners (about 20% nationwide), increasing mobility across language regions, and a rising number of bilingual couples, the personal cultural background is frequently different from the dominant culture within a canton or local community. Therefore, we use the language of the family as the main variables representing the personal cultural background for the analysis of the SHP data. As the corresponding data are not available in the LEU data set, we use the culture of the local community as the second cultural variable for analysis[112].

Another possible variable to represent culture (that differs from language) is religion. In Switzerland, this holds because in the German- and French-speaking parts there are regions with large majorities of either Catholic or Protestant populations. Thus, religion and language are not highly correlated. To account for religious denominations, in the LEU data we use a

[110] Taking subsistence income and the difference between actual and subsistence income is under the null of the absolute income hypothesis observationally equivalent to using average income and the difference between actual and average income. Differences occur, however, with respect to the squared terms.

[111] An OLS regression of the index of direct democracy for the year 2000 on the three cantonal language variables yields the following result

$$\underset{}{\text{DEMO}} = \underset{(4.72)}{4.716\ \text{German}} + \underset{(2.75)}{2.753\ \text{French}} + \underset{(2.25)}{2.250\ \text{Italian}} + \hat{u},$$

with $R^2 = 0.612$ and 23 degrees of freedom. (The numbers in parentheses are the estimated t-statistics.)

[112] The definitions of all variables used in the following models are listed in table A.4 of the Appendix

dichotomous variable indicating whether an individual pays church taxes or not. Fortunately, the SHP data are richer and allow controlling for several individual religious professions.

Following the HENDRY approach, we start with the comprehensive model, for both of whose datasets ordered probit estimates are given in table A.2 of the Appendix.[113] For the SHP data, we show the results for both the balanced and unbalanced panel.[114] For the LEU dataset, we present the results of weighted estimates that take into account the oversampling, as well as estimates for the reduced representative sample. In all four cases, the squared income variable for those below the poverty line does not prove significant. Therefore, this variable is deleted from further estimations. In all four estimations the variables controlling for religious denominations are either not significant at all or only very weakly at the 10% level of significance[115]. For this reason, these cultural factors are excluded from the discussion of the estimation results. Moreover, the discussion of results is also restricted to the balanced panel and the full LEU dataset.

Table 1a shows the results of the models with the SHP data.[116] If no cultural variables are included, the index of direct popular rights has a significant positive impact as in the models of FREY and STUTZER. However, as soon as any cultural variables are taken into account, the significance vanishes completely: the corresponding t-statistics are very far from any conventional significance level. Thus, the significance in model (1) seems to be only a result of the omitted cultural variables.

It is hardly surprising that the effect of the index of direct popular rights is strongly reduced as soon as culture is included in the regression equation. The descriptive statistics in table A.3 of the Appendix and a corresponding analysis of variance show that the main variance of this index is between and not within the three language groups.[117] Taking into account that the language of a canton is truly independent of its extent of direct popular rights, it becomes

[113] For reference categories and chosen weights, see there.

[114] Similar results are obtained when the model is estimated for the three waves of the SHP separately (see also section 3.2). Additionally, testing for various functional forms of the index of direct democracy does not reveal a considerably significant effect when culture is controlled for.

[115] In both datasets the religious variables are also not jointly significant at the 5% level.

[116] We only present the results for the relevant variables. The complete results can be received from the authors on request.

[117] 61.3 % of the variance of the index of direct democracy is between and only 38.7 % is within the three language groups. If we add the French and Italian part as one, we still get 60.6 % between and only 39.4 % of the variance between the groups.

Table 1a: Personal Subjective Well-Being in Switzerland, 2000 – 2002

SHP Data, Balanced Panel, 9903 Observations

	Basic Model (1)	Model including cantonal culture (2)	Model including household culture (3)	Model including cantonal and household culture (4)
Direct democracy	0.049* (2.55)	0.019 (0.69)	-0.009 (0.35)	0.008 (0.29)
Subsistence Income	0.094 (0.82)	0.054 (0.44)	0.014 (0.12)	0.062 (0.51)
Relative Income	0.052*** (5.15)	0.052*** (5.17)	0.052*** (5.13)	0.052*** (5.10)
Income above poverty line squared	-0.001** (2.76)	-0.001** (2.77)	-0.001** (2.74)	-0.001** (2.73)
French-speaking canton		-0.112 (1.61)		0.104 (0.94)
Italian-speaking canton		-0.117 (0.91)		0.325(*) (1.66)
French-speaking family			-0.212*** (3.28)	-0.273** (2.61)
Italian-speaking family			-0.336** (2.93)	-0.547** (3.12)
Log of likelihood	-14'375.227	-14'373.905	-14'368.221	-14'366.660
		Wald Tests		
Subsistence Income = Relative Income	0.13	0.00	0.10	0.01
Joint significance of cantonal language variables		2.65		3.12
Joint significance of family language variables			14.04***	14.50***
Joint significance of democracy and cantonal language variables		9.16*		3.24
Joint significance of democracy and family language variables			20.57***	14.97**
Joint significance of democracy and all language variables				23.69***

The number in parentheses are the absolute values of the z-statistics of the estimated parameters. '***', '**', '*' or '(*)' show that the corresponding null hypothesis can be rejected at the 0.1, 1, 5, or 10% level, respectively. The Wald tests are χ^2 with 1, 2, 3, or 5 degrees of freedom, respectively.

obvious that omitting the language variables from the estimated equation results in a serious specification error that inflates the coefficient of the index of direct democracy, as well as its estimated significance level.

Relative income is always highly significant, but subsistence income is not. This finding is evidence in favor of the relative income hypothesis. However, when testing for the equality of the coefficients of subsistence and relative income, the null hypothesis can never be rejected. Thus, the results do not allow discrimination between the absolute and the relative income hypotheses. On the other hand, the coefficient of the squared relative income term is always – as expected – negative and statistically significant. This result is clear evidence for a decreasing marginal utility of income.

The signs of the cultural variables indicate that people in French- and Italian-speaking cantons and/or families are less satisfied with their overall situation than people living in German-speaking cantons and/or families (models (2) and (3)). However, if the different indicators are differentiated, it becomes clear that it is the language of the family and not the language of the canton that matters (model (4)). Once the language of the family is included, neither the cantonal language nor the index of direct democracy proves considerably significant. This observation does not only hold for the single variables, as can be seen in the z-statistics, but also for the combined hypotheses, as the results of the Wald tests show. French- and Italian-speaking people seem more critical of their situation than German-speaking people.

Table 1b shows the corresponding results from the LEU dataset,[118] which are quite different from those obtained using the SHP data. The effect of direct democracy is more robust in this dataset. When cultural variables are omitted, the coefficient of the index of direct democracy is significantly different from zero even at the 1% level. If only local culture is included, the significance vanishes (model (7)); however, if both sets of cultural variables are included, it is significant at the 10% level (model (8)); and if only the cantonal variables are included, it is still significant at the 5% level (model (6)).[119] As the results of the Wald tests show, contrary

[118] Model (5) corresponds to the second equation in FREY and STUTZER (2000, table 2, p. 927); however, the health status and denomination variables have been added. (They include the health status, e.g., in STUTZER and FREY (2000, 2003)). Using the same specification, we were able to exactly replicate their results. Thus, differences between their results and model (5) are due to the different specification of the income variables and the inclusion of the health status variable.

[119] The significance is approximately the same if the fiscal decentralization variable is included in the model, but it increases if the individual health status is deleted, giving z-values of 3.19, 2.37, 1.60, and 2.17 in models (5) to (8), respectively. On the other hand, the significance vanishes if the smaller, representative dataset is

Table 1b: Personal Subjective Well-Being in Switzerland, 1992

LEU Data, Full Cross Section, 6127 observations

	(5)	Model including cantonal culture (6)	Model including local culture (7)	Model including cantonal and local culture (8)
Direct democracy	0.080** (2.91)	0.050* (1.98)	0.042 (1.58)	0.041(*) (1.77)
Subsistence Income	-0.133 (1.35)	-0.210* (2.13)	-0.211* (2.05)	-0.219* (2.21)
Relative Income	0.024** (2.83)	0.025** (2.95)	0.024** (2.91)	0.025** (2.95)
Income above poverty line squared	-0.001*** (3.45)	-0.001*** (3.74)	-0.001*** (3.64)	-0.001*** (3.75)
French-speaking canton		-0.194** (3.09)		-0.047 (0.30)
Italian-speaking canton		0.190* (2.55)		0.436*** (3.34)
French-speaking family			-0.224*** (4.47)	-0.185 (1.33)
Italian-speaking family			0.157* (1.99)	-0.274** (3.02)
Log of likelihood	-10'032.007	-10'014.004	-10'011.961	-10'011.223
		Wald Tests		
Subsistence Income = Relative Income	2.61	5.89*	5.35*	6.29*
Joint significance of cantonal language variables		86.22***		14.00***
Joint significance of family language variables			158.60***	9.49**
Joint significance of democracy and cantonal language variables		87.22***		14.01**
Joint significance of democracy and family language variables			160.87***	11.17*
Joint significance of democracy and all language variables				261.84***

The number in parentheses are the absolute values of the z-statistics of the estimated parameters. '***', '**', '*' or '(*)' show that the corresponding null hypothesis can be rejected at the 0.1, 1, 5, or 10% level, respectively. The Wald tests are $\chi 2$ with 1, 2, 3, or 5 degrees of freedom, respectively. Full LEU sample has been estimated with individual weights. Robust standard errors obtained through clustering of cantons.

used. This gives the following z-values: 2.50, 1.67, 1.18, and 1.44. Thus, once individual culture is included, the index of direct popular rights never proves significant at any conventional level.

Table 2: Results for Different Population Groups

	German-speaking cantons	French-speaking cantons	German-speaking families	French-speaking families
	SHP Data 2000 – 2002, balanced panel			
Direct democracy	-0.004 (0.14)	0.256** (2.66)	-0.006 (0.22)	0.050 (0.68)
Subsistence Income	0.117 (0.89)	0.040 (0.10)	0.144 (1.11)	-0.235 (0.66)
Relative Income	0.046*** (3.86)	0.074** (3.17)	0.044*** (3.72)	0.078*** (3.34)
Income above poverty line squared	-0.001* (2.48)	-0.000 (0.20)	-0.001* (2.42)	-0.000 (0.20)
Log of likelihood	-9'600.8689	-4'038.6533	-9'495.1812	-4'052.0114
Number of observations	6716	2748	6670	2755
	Wald Tests			
Subsistence Income = Relative Income	0.29	0.01	0.58	0.76
	German-speaking cantons	French-speaking cantons	German-speaking families	French-speaking families
	LEU Data, 1992			
Direct democracy	0.029 (1.10)	0.054 (0.95)	0.039 (1.32)	0.039 (0.73)
Subsistence Income	-0.191(*) (1.82)	-1.000* (2.28)	-0.186(*) (1.73)	-0.729* (2.18)
Relative Income	0.024* (2.46)	0.061(*) (1.77)	0.025* (2.50)	0.060(*) (1.81)
Income above poverty line squared	-0.001*** (3.73)	-0.003*** (3.35)	-0.001*** (3.71)	-0.003*** (3.20)
Log of likelihood	-7'108.1302	-2'397.8161	-7'201.1989	-2'293.8294
Number of observations	4466	1378	4531	1308
	Wald Tests			
Subsistence Income = Relative Income	4.43*	5.50*	4.02*	5.40*

The number in parentheses are the absolute values of the z-Statistics of the estimated parameters. '***', '**', '*' or '(*)' show that the estimated parameter is significantly different from zero at the 0.1, 1, 5, or 10% level, respectively. The Wald tests are χ^2 with 1 degree of freedom. See also tables 1a and 1b.

to the results for the SHP dataset, both sets of cultural variables should be included. Thus, one interesting result of this analysis is that the index of direct popular rights is (at least marginally) significant if the full LEU dataset is used but not significant at all when more recent SHP dataset is used.

In addition, in model (1), subsistence income has an insignificant coefficient, but the test for equality of the coefficients between the subsistence income and the relative income cannot be rejected. If culture is included in the following models (2), (3) and (4), however, the coefficient of subsistence income always has a negative sign and is significant at the 5% level. The equality of the coefficients of subsistence and relative income, however, can always be rejected at the 5% level. This result, again, does not allow discrimination between the absolute and the relative income hypothesis. On the other hand, we also find decreasing marginal utility of income as in the SHP dataset.

There are also some differences between the results of the two datasets with respect to the cultural impact. First, whereas cantonal culture is insignificant in the SHP data, in the LEU data, both group indicators prove significant. This outcome might, however, be due to the fact that local culture is not a sufficiently appropriate substitute for personal culture. More surprising is the second difference. Taking the results of the SHP data, both French and Italian cantonal, as well as personal, cultures lead, *ceteris paribus*, to a lower degree of subjective well-being, if included separately in the model. In the LEU data, the result is identical for the French cantons and local communities but not for the Italian-speaking individuals, who seem happier than their French- and German-speaking counterparts.

Finally, we split the datasets and estimate the model for various cultural subgroups: i.e. for German- and French-speaking cantons and families.[120] These results for the SHP panel, as shown in table 2, seem to confirm that direct democracy has a life satisfaction increasing impact in cantons whose majority language is French but an insignificant one in German-speaking cantons. Splitting the SHP data according to languages of the household questionnaire, however, shows no significant impact of the degree of direct democracy on any cultural subpopulation. Once again, this result is different with respect to the LEU dataset. In German- as well as French-speaking local communities, direct democracy seems to have no

[120] Estimating separate equations for Italian culture is not possible as there is only one canton, *Tessin (Ticino)*, in which the main language is Italian. Moreover, because most Italian-speaking Swiss live in this canton, the equation for the subsample with Italian family culture is also dropped.

significant impact on personal well-being, even if the coefficients of the estimated parameters are positive in all four regressions.

Combining the results of the two datasets reveals that the findings on the effect of income on subjective well-being are consistent: Both are neither fully compatible with the relative income hypothesis, nor with the absolute income hypothesis. However, with respect to (direct) democracy and culture, major differences exist. While both show that culture has a strong impact on subjective well-being, according to the SHP data, Italian culture seems to have a negative impact on happiness, but according to the LEU data, it has a positive one. Additionally, while the SHP results strongly suggest that democracy itself has no statistically significant effect on happiness in Switzerland[121], results for the LEU data indicate a possible independent impact of direct democracy on personal well-being, even if the significances are fragile and strongly dependent on the respective specifications.

The question remains of why these differences exist. There are at least two possible reasons. One is that the weighting procedure does not really produce a representative sample. This presumption is supported by the fact that the results with the reduced sample do not – at least with respect to the impact of direct democracy – indicate significance and are thus compatible with the SHP data results. The second possible reason is that the perception of the Swiss population with respect to their valuation of the benefits of direct democracy might have changed between 1992, when the LEU data were collected, and the beginning of this century, as represented by the SHP data. In any case, as earlier mentioned, a finding of no significant impact of direct popular rights on personal well-being in these individual datasets in Switzerland does not indicate that there is no impact at all. First, the most important elements of Swiss direct democracy exist at the federal level, and they are identical for all Swiss people. Second, as mentioned previously, measured on an international scale, the extent of democratic rights in all Swiss cantons is extremely high. Thus, it may well be the case that democracy, and especially direct democracy, has a positive impact on the personal subjective well-being of the whole Swiss population, even though no statistical significance is found at the subfederal level.

[121] In the unbalanced SHP panel a statistical significance at the 10% level is reached (see table A.2 of the Appendix). Since in this estimation standard errors are not corrected according to the MOULTON-critique (1990), however, significance levels appear inflated.

3.2 Results for Single Waves, 2000 – 2002

One interesting extension of the analysis of these Swiss results is to look at the single cross sections rather than the whole panel. The advantage of single waves is that they contain more observations than the balanced panel alone. In this case, the number of observations ranges from 5,341 to 4,523, in contrast to the 3,301 individuals per wave in the balanced panel. This exercise also serves to reveal whether the results obtained in the balanced and unbalanced panels are driven by one single wave only; it also serves to account for sample attrition in the (balanced) panel. The process of variable creation and deletion is approximately the same as for the balanced panel[122]. Again, the model specification includes religious denomination of the individual. An ordered probit model is estimated with transversal individual weights and adjustment of standard errors through clustering of cantons. Results for the single waves are displayed in tables 3a, 3b, and 3c for the years 2000, 2001, and 2002, respectively.

A comparison of the results for all three waves reveals that direct democracy does not always exert the expected impacts and its influence varies over the three cross sections. Based on the balanced panel results in table 1a, it should affect happiness positively in the first model, and its coefficient should be positive in models (2) and (4), when measures of cantonal culture are included. In fact, such a significant happiness increasing influence is found in model (1) for each wave. Yet the estimation results reveal a negative coefficient in models (2) and (4) in the year 2001 wave[123], even though the predicted happiness raising influence is observed in the 2000 and 2002 waves. On the other hand, as previously observed in the balanced panel, controlling for (cantonal and/or personal) culture (see models (2), (3), and (4)) leads always to a breakdown in the link between direct popular rights and well-being in all waves.

[122] In contrast to the balanced panel, observations with missing income variables have been deleted. The results for the single SHP waves are similar to the ones for single waves in the balanced panel.

[123] A positive coefficient is observed in model (3) in the year 2002 wave which also contradicts the results in the balanced panel.

Table 3a: Personal Subjective Well-Being in Switzerland, 2000

SHP Data, Cross Section, 5341 Observations

	Basic Model (1)	Model including cantonal culture (2)	Model including household culture (3)	Model including cantonal and household culture (4)
Direct democracy	0.061** (3.15)	0.016 (0.72)	-0.000 (0.02)	0.009 (0.41)
Subsistence Income	-0.163 (1.20)	-0.258(*) (1.90)	0.296* (2.25)	-0.252(*) (1.81)
Relative Income	0.057*** (4.64)	0.057*** (4.70)	0.056*** (4.64)	0.056*** (4.62)
Income above poverty line squared	-0.002* (2.49)	-0.002** (2.59)	-0.002** (2.60)	-0.002** (2.58)
French-speaking canton		-0.163*** (3.72)		0.001 (0.01)
Italian-speaking canton		-0.162** (2.92)		0.169 (1.20)
French-speaking family			-0.241*** (6.94)	-0.223* (1.96)
Italian-speaking family			-0.322*** (3.67)	-0.411** (2.98)
Log of likelihood	-8'388.0864	-8'382.5347	-8'369.9752	-8'368.9366
		Wald Tests		
Subsistence Income = Relative Income	2.51	5.32*	7.19**	4.85*
Joint significance of cantonal language variables		14.21***		1.47
Joint significance of family language variables			48.19***	13.93***
Joint significance of democracy and cantonal language variables		25.54***		1.66
Joint significance of democracy and family language variables			81.43***	15.55**
Joint significance of democracy and all language variables				72.87***

The number in parentheses are the absolute values of the z-statistics of the estimated parameters. '***', '**', '*' or '(*)' show that the corresponding null hypothesis can be rejected at the 0.1, 1, 5, or 10% level, respectively. The Wald tests are χ^2 with 1, 2, 3, or 5 degrees of freedom, respectively. Estimation with transversal individual weights keeping sample size and with robust standard errors by clustering o cantons.

Table 3b: Personal Subjective Well-Being in Switzerland, 2001

SHP Data, Cross Section, 5004 Observations

	Basic Model (1)	Model including cantonal culture (2)	Model including household culture (3)	Model including cantonal and household culture (4)
Direct democracy	0.055*** (3.37)	-0.002 (0.09)	-0.005 (0.21)	-0.007 (0.34)
Subsistence Income	0.029 (0.29)	-0.007 (0.08)	0.007 (0.09)	0.007 (0.08)
Relative Income	0.036** (3.06)	0.035** (2.94)	0.035** (2.87)	0.035** (2.87)
Income above poverty line squared	-0.000* (2.07)	-0.000(*) (1.92)	-0.000(*) (1.86)	-0.000(*) (1.86)
French-speaking canton		-0.195*** (5.16)		-0.053 (0.91)
Italian-speaking canton		-0.165** (3.08)		0.018 (0.12)
French-speaking family			-0.226*** (5.10)	-0.181*** (3.24)
Italian-speaking family			-0.207* (2.15)	-0.224 (1.33)
Log of likelihood	-7'973.7579	-7'965.9563	-7'960.9757	-7'960.7507
		Wald Tests		
Subsistence Income = Relative Income	0.01	0.27	0.12	0.11
Joint significance of cantonal language variables		27.33***		0.89
Joint significance of family language variables			34.01***	10.68**
Joint significance of democracy and cantonal language variables		43.24***		1.09
Joint significance of democracy and family language variables			39.97***	12.43**
Joint significance of democracy and all language variables				46.11***

The number in parentheses are the absolute values of the z-statistics of the estimated parameters. '***', '**', '*' or '(*)' show that the corresponding null hypothesis can be rejected at the 0.1, 1, 5, or 10 % level, respectively. The Wald tests are χ^2 with 1, 2, 3, or 5 degrees of freedom, respectively. Estimation with transversal individual weights keeping sample size and robust standard errors by clustering of cantons.

Table3c: Personal Subjective Well-Being in Switzerland, 2002

SHP Data, Cross Section, 4523 Observations

	Basic Model (1)	Model including cantonal culture (2)	Model including household culture (3)	Model including cantonal and household culture (4)
Direct democracy	0.051*** (4.20)	0.034 (1.49)	0.021 (0.98)	0.035 (1.54)
Subsistence Income	-0.015 (0.18)	-0.054 (0.52)	-0.082 (0.80)	-0.032 (0.30)
Relative Income	0.068*** (8.61)	0.068*** (8.52)	0.067*** (8.19)	0.067*** (8.07)
Income above poverty line squared	-0.001*** (7.34)	-0.001*** (7.24)	-0.001*** (7.02)	-0.001*** (7.04)
French-speaking canton		-0.046 (0.73)		-0.015 (0.24)
Italian-speaking canton		-0.107 (1.43)		0.248* (2.31)
French-speaking family			-0.089 (1.53)	-0.051 (0.63)
Italian-speaking family			-0.292*** (4.20)	-0.424*** (4.13)
Log of likelihood	-7'195.3371	-7'194.5900	-7'186.9392	-7'184.6366
		Wald Tests		
Subsistence Income = Relative Income	1.00	1.39	2.08	0.86
Joint significance of cantonal language variables		2.67		5.73(*)
Joint significance of family language variables			17.73***	17.10***
Joint significance of democracy and cantonal language variables		28.33***		6.83(*)
Joint significance of democracy and family language variables			47.95***	28.81***
Joint significance of democracy and all language variables				72.34***

The number in parentheses are the absolute values of the z-statistics of the estimated parameters. '***', '**', '*' or '(*)' show that the corresponding null hypothesis can be rejected at the 0.1, 1, 5, or 10 % level, respectively. The Wald tests are χ^2 with 1, 2, 3, or 5 degrees of freedom, respectively. Estimation with transversal individual weights keeping sample size and robust standard errors by clustering of cantons.

In addition, also considerable differences are observed with respect to the income variable. First, if culture is included, the subsistence income is significant in the year 2000 wave, which supports the absolute income hypothesis, but with the wrong sign. In contrast, it is insignificant in the subsequent two waves, which is in line with the relative income hypothesis. In addition, for the year 2000, the hypothesis that the coefficients of subsistence income and relative income are equal is rejected if culture is included (see models (2) to (4) in table 3a), which is in line with the relative income hypothesis; however, this is not the case for the years 2001 and 2002, which, again, supports the absolute income hypothesis. Overall, neither the absolute nor the relative income hypothesis can be fully supported or rejected in any of the three waves. On the other hand, in all three waves, relative income appears always to increase happiness. Decreasing marginal returns to income can also be unambiguously observed in all three waves, although in the year 2001 wave the corresponding coefficient is only weakly significant (at the 10% level).

As expected, culture measured by language always appears to be an important determinant[124]. In the first two of the three waves, cantonal French or Italian culture is a decisive determinant of happiness if personal culture is not controlled for (see model (2)); however, in the year 2002 wave, the cantonal cultural variables are not even jointly significant. Nevertheless, the signs of the cultural factors point in the same direction in all three waves, corroborating the result obtained for the balanced panel and indicating a happiness decreasing influence. In all three waves, the speaking of French or Italian in the household exerts an almost always significantly negative influence on a person's happiness level, in contrast to households in which German is spoken (see model (3)). The observed signs equal those obtained in the balanced panel. These individual cultural determinants at the household level are in all three waves jointly significant. In model (4), the inclusion of both cantonal and personal measures of culture reveals changing signs and significance levels, particularly for the cantonal variables. In model (4), also jointly significant in all three waves are the measures of family culture, whereas the test results for cantonal culture are different in the year 2002 from the ones in the year 2000 and 2001 waves.

A brief look at the remaining individual determinants of happiness may give some indication of whether some of the changing results for the institutional, cultural, and income variables

[124] In the year 2000 and 2001 waves, religion never played a significant role. Only in the year 2002 wave particularly Protestants appear to be considerable happier (at the 5% level) in all specifications, and also Christ-Catholic and other Christian denominations in model (4).

result from the creation by the different sample sizes in the three waves of a sample selection bias[125]. In all three waves and all specifications, health status negatively influences the perceived level of well-being. For all three years, the positive coefficients of the three oldest age groups appear significant; however, results for 2000 and 2001 show that persons aged 40 or younger experience a considerably lower happiness level than the reference group of persons younger than 30. In addition, females are significantly happier in all three waves and also foreigners always seem less happy. In only two of the three waves does education matter for individual well-being. However, in all estimations, family status and household type show an identical pattern of influence. Unemployed persons do not appear unhappier in 2000 but in subsequent years, possibly as a result of the recession that began in and continued during that period. Housewives, however, are more satisfied with their lives in 2002 and 2000, but not in 2001. Finally, the employment status of 'other' and 'self-employed' again show identical signs and similar significance levels in all three single cross sections. In sum, the similarity of the remaining individual determinants between the three waves does not support the likelihood of a sample selection problem[126].

Overall, the results of the wave 2000 seem most to resemble the balanced panel results. Despite the differences between the estimation results for the single waves, one commonly observed trait is a more fragile and less decisive influence of cantonal culture compared to personal culture. Furthermore, from a statistical viewpoint, the cultural factor clearly dominates the institutional impact in all three single waves: a statistical insignificance of the institutional variable could be observed in all three waves when either measure of culture was included in the specification. In this respect, the findings for the balanced panel can be seen to be corroborated. To conclude, the fragility of some of the estimation results among the various waves, however, emphasizes the advantage of a panel estimation over a single cross section that might – as a whole – be a statistical outlier.

[125] FRIJTERS et al. (2004, p. 651 cont.) present an overview of the findings of previous empirical life satisfaction studies with respect to the sociodemographic determinants age, gender, health, marriage/divorce, education, unemployment and ethnicity.

[126] The complete results can be received from the authors on request.

4 Summary and Concluding Remarks

In this chapter, we analyze the impact of democracy on subjective well-being both in Switzerland and in an international comparison of 28 countries. Whereas FREY and STUTZER (2000, 2000b, 2003) show a positive influence of democracy on happiness in Switzerland, no other analysis of this problem exists that uses a microdata-based international regression analysis. In addition, by including language and religion variables, we control not only for various sociodemographic and economic determinants of life satisfaction but also for cultural influences.

Results for the two different datasets for the 26 Swiss cantons revealed that intercantonal differences in the levels of direct democracy no longer show a significant impact on personal happiness once cultural differences among the different Swiss regions are controlled for. most This is most probably because differences in democracy levels are small among Swiss cantons; thus, levels in the latter may not be sufficiently large to generate a statistical significance at any conventional level when the inclusion of cultural determinants diminishes variation in the democracy variable. However, this result varies among the regions: when only the French-speaking part is considered, the SHP data reveal a significant positive impact of democracy on happiness. This observation does not apply, however, to the German-speaking part.

In both investigations, culture also has a considerable impact on subjective well-being, one that in most cases is highly significant. With regard to cultural background, it appears that individuals speaking French or Italian seem less happy than those speaking a Germanic language. The questions of why this apparent difference exists and why people speaking certain other languages appear less happy remain open. Therefore, it is imperative that, studies on subjective well-being, even on a national level, control for individual culture.

Finally, relative income has a positive impact on happiness, but with diminishing returns. This finding is consistent with the usual assumption of decreasing marginal utility. Whether the absolute income of a person also has an impact on happiness cannot be unambiguously determined by this study. However, even if absolute income has an additional impact on happiness, the effect of relative income clearly dominates.

5 Appendix

Table A.1: Distribution of Life Satisfaction in the Balanced Panel, Extent of Direct Democracy and Fiscal Federalism in 2000

Canton	Categories								CS	DD
	10	9	8	7	6	5	4	3		
Aargau	18.1	20.7	37.8	14.4	3.1	4.0	1.0	0.9	8.9	5.46
Appenzell Innerrhoden	0.0	0.0	20.0	40.0	20.0	20.0	0.0	0.0	0.1	5.44
Appenzell Ausserrhoden	25.3	13.8	40.2	13.8	4.6	2.3	0.0	0.0	0.9	5.50
Bern	19.1	19.2	40.7	14.5	2.1	3.3	0.7	0.4	11.2	3.02
Basel-Stadt	18.9	21.5	40.7	10.4	3.0	4.7	0.3	0.3	3.0	5.48
Basel-Landschaft	23.8	18.8	35.8	13.3	3.4	4.3	0.6	0.0	3.3	4.40
Freiburg	14.3	16.0	39.3	15.5	6.8	5.8	1.5	0.8	4.0	2.79
Genf	16.6	12.6	38.4	17.9	6.7	5.7	0.4	1.7	4.8	1.75
Glarus	31.1	11.5	36.1	14.8	0.0	6.6	0.0	0.0	0.6	5.75
Graubünden	25.6	23.2	36.3	12.5	1.2	1.2	0.0	0.0	1.7	4.83
Jura[1]	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	3.71
Luzern	17.1	21.5	37.7	13.0	4.2	4.8	0.5	1.2	5.7	4.42
Neuenburg	18.0	16.1	38.7	14.6	5.9	5.0	1.0	0.6	6.3	2.19
Nidwalden	10.7	0.0	57.1	21.4	7.1	3.6	0.0	0.0	0.3	4.44
Obwalden	29.8	15.8	35.1	14.0	5.3	0.0	0.0	0.0	0.6	4.63
St. Gallen	22.2	21.1	35.2	13.5	3.9	3.3	0.6	0.2	4.9	3.46
Schaffhausen	9.8	20.6	44.1	18.6	1.0	3.9	1.0	1.0	1.0	5.21
Solothurn	16.2	20.1	38.3	16.6	3.5	3.0	1.6	0.7	4.4	5.25
Schwyz	19.4	20.0	40.6	13.3	1.1	5.0	0.0	0.6	1.8	4.93
Thurgau	14.4	19.5	40.1	18.3	3.1	3.9	0.4	0.4	2.6	4.33
Tessin	17.2	19.0	34.8	14.5	5.4	6.1	0.9	2.0	4.4	2.25
Uri	33.3	23.8	28.6	4.8	4.8	4.8	0.0	0.0	0.2	5.13
Waadt	14.5	20.5	40.4	13.5	4.6	4.2	1.0	1.3	9.5	2.50
Wallis	22.0	18.9	39.3	12.9	3.8	2.5	0.0	0.6	3.2	3.58
Zug	13.4	16.8	40.3	20.2	3.4	5.9	0.0	0.0	1.2	4.42
Zürich	17.6	19.7	37.6	15.4	4.3	3.6	0.6	1.0	15.5	3.50
Total Share	18.0	19.1	38.5	14.7	4.0	4.1	0.7	0.8		

CS is the cantonal share in observations in the balanced panel, DD the value of the index of direct democracy for the year 2000.

[1] There are no observations from the canton *Jura* in the balanced panel.

Table A2: Results for Switzerland, Full Model

	SHP Data		LEU Data	
	Balanced Panel	Unbalanced Panel	Full Sample	Representative Sample
Direct democracy	0.008 (0.29)	0.039(*) (1.67)	0.042(*) (1.80)	0.025 (1.16)
Good or average health	Reference category			
Bad health	-0.536*** (12.79)	-0.562*** (15.22)	-0.703*** (16.93)	-0.713*** (18.89)
Age 20-29	Reference category			
Age 30-39	0.045 (0.74)	-0.027 (0.51)	-0.033 (0.38)	-0.095(*) (1.68)
Age 40-49	0.018 (0.27)	0.023 (0.42)	0.069 (0.90)	0.048 (0.76)
Age 50-59	-0.028 (0.41)	0.002 (0.03)	0.044 (0.73)	-0.024 (0.45)
Age 60-69	-0.046 (0.59)	-0.060 (0.92)	0.299*** (3.91)	0.255*** (5.80)
Age 70-79	-0.031 (0.34)	-0.136(*) (1.77)	0.413*** (4.69)	0.398*** (5.01)
Age 80 and older	-0.186 (1.29)	-0.272* (2.22)	0.422*** (4.60)	0.492*** (6.00)
Male	Reference Category			
Female	-0.082(*) (1.73)	-0.008 (0.19)	0.062(*) (1.65)	0.070* (2.12)
Swiss	Reference Category			
Foreigner	-0.302*** (3.90)	-0.317*** (5.08)	-0.233*** (4.02)	-0.186** (3.16)
Low education	Reference Category			
Middle education	0.095(*) (1.71)	0.029 (0.63)	0.051 (1.23)	0.080*** (3.18)
High education	0.029 (0.42)	0.016 (0.27)	0.044 (0.79)	0.106** (2.57)
Single woman	-0.355*** (4.51)	-0.426*** (6.24)	-0.272*** (5.45)	-0.177*** (3.80)
Single man	-0.446*** (5.25)	-0.365*** (5.01)	-0.211*** (3.54)	-0.269*** (6.44)

Table A2: Results for Switzerland, Full Model (cont.)

	SHP Data		LEU Data	
	Balanced Panel	Unbalanced Panel	Full Sample	Representative Sample
Couple without children	Reference Category			
Couple with children	-0.140** (2.82)	-0.142*** (3.38)	-0.142*** (3.47)	-0.078** (2.68)
Single parent	-0.704*** (7.53)	-0.603*** (7.86)	-0.378*** (3.68)	-0.350*** (3.52)
Other private household	-0.311(*) (1.96)	-0.276 * (2.16)	-0.168 * (2.33)	-0.171*** (3.53)
Collective household	-0.136 (0.74)	-0.262(*) (1.89)	-0.382*** (3.25)	-0.267** (3.01)
Employed	Reference Category			
Self-employed	0.126(*) (1.77)	0.114(*) (1.82)	0.054 (1.22)	0.064(*) (1.67)
Housewife	0.332*** (5.18)	0.313*** (5.46)	0.130** (2.57)	0.056 (1.11)
Other employment status	0.429*** (8.27)	0.375*** (8.64)	-0.037 (0.55)	-0.053 (0.83)
Unemployed	-0.504*** (3.21)	-0.572*** (4.24)	-0.778*** (4.99)	-0.681*** (6.24)
Subsistence Income	0.061 (0.51)	-0.033 (0.42)	-0.223* (2.27)	-0.399*** (3.79)
Relative Income	0.052*** (5.07)	-0.043*** (5.03)	0.030** (2.88)	0.029*** (3.26)
Income above poverty line squared	-0.001** (2.74)	-0.001*** (3.21)	-0.001*** (3.46)	-0.001** (3.17)
Income below poverty line squared	0.033 (0.23)	0.003 (0.02)	0.127 (1.33)	0.031 (0.38)
German-speaking canton	Reference Category			
French-speaking canton	0.105 (0.95)	0.033 (0.35)	-0.040 (0.25)	-0.126 (0.73)
Italian-speaking canton	0.325(*) (1.66)	0.065 (0.42)	0.434*** (3.28)	0.270*** (3.67)
German-speaking household/ German-speaking local community	Reference Category			

Table A2: Results for Switzerland, Full Model (cont.)

	SHP Data		LEU Data	
	Balanced Panel	Unbalanced Panel	Full Sample	Representative Sample
French-speaking household/ French-speaking local community	-0.274** (2.62)	-0.233** (2.59)	-0.189 (1.36)	-0.112 (0.63)
Italian-speaking household/ Italian-speaking local community	-0.547** (3.12)	-0.342** (2.59)	-0.268** (2.93)	-0.127(*) (1.90)
Protestant (SHP)/ dummy for paying church taxes	0.101 (0.92)	0.074 (0.81)	0.047 (0.85)	0.086(*) (1.94)
Catholic (SHP)	0.072 (0.65)	0.059 (0.65)		
Christ-catholic (SHP)	0.117 (0.81)	0.078 (0.64)		
Other Christian religion (SHP)	0.241(*) (1.93)	0.206(*) (1.90)		
Other denomination/ Jewish / Muslim (SHP) / dummy for not paying church taxes	Reference category			
No religion (SHP)	-0.000 (0.00)	-0.045 (0.46)		
Dummy for the year 2000	0.218*** (7.88)	0.219*** (8.70)		
Dummy for the year 2001	0.114*** (4.22)	0.121*** (4.85)		
Dummy for the year 2002	Reference year			
Number of observations	9903	12967	6'127	5'107
Log of likelihood	-14'366.633	-19'335.282	-10'009.338	-8'531.3623
Rho	0.533***	0.545***		
Adjusted Mac Fadden's R2	0.015	0.016	0.040	0.036

The numbers in parentheses are the absolute values of the z-values of the estimated parameters. '***', '**', '*' or '(*)' show that the estimated parameter is significantly different from zero at the 0.1, 1, 5, or 10 percent level, respectively. Controls for the type (and size of) of commune are included but not reported. Full LEU sample has been estimated with individual weights. Both LEU samples also with robust standard errors obtained through clustering of cantons.

Table A.3: Descriptive Statistics of the Index of Direct Democracy

	Mean	Standard Deviation	Median	Minimum	Maximum
All cantons	4.168	1.182	4.420	1.750	5.750
German-speaking cantons	4.716	0.765	4.830	3.020	5.750
French-speaking cantons	2.753	0.773	2.645	1.750	3.710
Italian-speaking canton	2.250	0.000	0.250	2.250	2.250
French- or Italian-speaking cantons	2.681	0.731	2.500	1.750	3.710

Table A.4: Description of the Variables from the Swiss Household Panel

Variable	Definition	Based on / Source
Life satisfaction	8 categories, with the original categories 0, 1,2, and 3 forming the lowest	p0Xc44
Bad health	1 if subjective state of health is not good, 0 otherwise	1 if P0Xc01 >=3
Age	Year of interview - birth year of interviewee	200X – birth year
Age 30 – 39	1 if age is between 30 and 39, 0 otherwise	
Age 40 – 49	1 if age is between 40 and 49, 0 otherwise	
Age 50 – 59	1 if age is between 50 and 59, 0 otherwise	
Age 60 – 69	1 if age is between 60 and 69, 0 otherwise	
Age 70 – 79	1 if age is between 70 and 79, 0 otherwise	
Age 80 and older	1 if age is older than 80, 0 otherwise	
Female	1 if person is female, 0 otherwise	sex = 2
Foreigner	1 if person is foreigner, 0 otherwise (single, double or triple citizenship)	nat_1_X, nat_2_X, and nat_3_X
Middle education	1 if person completed secondary II education, 0 otherwise	educat0X = 4, 5, 6, or 8
High education	1 if person completed a tertiary education (university, university of applied science, Higher Master Craftsman's Diploma)	educat0X = 7, 9, 10
Single woman	1 if a single is female, 0 otherwise	Single = 1 & Sex = 2
Single man	1 if a single is male, 0 otherwise	Single = 1 & Sex = 1
Single	1 if a person lives alone without children, 0 otherwise	hldtyp0X = 1, 2 or 3
Couple with children	1 if an unmarried couple with children lives in the same household, 0 otherwise	hldtyp0X = 8, 9, 10 or 11
Single parent	1 if a single parent with child(ren) lives in this household, 0 otherwise	hldtyp0X = 4 or 5

Table A.4: Description of the Variables from the Swiss Household Panel (cont.)

Variable	Definition	Based on / Source
Collective household	1 if household is a collective household, 0 otherwise	hldtyp0X = 13
Self-employed	1 if a person is self-employed or employed in own company, 0 otherwise	p0Xw29 =3 or 4 & ┐(unemployed = 1, housewife = 1, or occupa0X = 3, 7, 8, or 10)
Housewife	1 if person is a housewife or a houseman, 0 otherwise	occupa0X = 6
Other employment status	1 if person works in the family, is an apprentice or a student, does military service, is retired or other	1 if (self-employed = 0 & housewife = 0 & unemployed = 0 & employed = 0)
Unemployed	1 if person is unemployed and either officially recorded or not, 0 otherwise	occupa0X = 9
Income	Monthly net income of the household, deflated to the reference year 1993 with the GDP deflator, divided by the equivalence scale of the Swiss Conference for Public Assistance.	i0Xeqsn /12*inflation index
Direct democratic rights	Index of direct democracy of the year of interview	Own calculations, based on STUTZER (1999)
French, Italian or German household language	Interview language of household questionnaire	hlingu0X (1 = French, 2 = German, 3 = Italian)
Protestant	1 if person is a housewife or a houseman, 0 otherwise	p0Xr01= 1
Catholic	1 if person is a Catholic, 0 otherwise	p0Xr01= 2
Christian Catholic	1 if person is a Protestant, 0 otherwise	p0Xr01= 3
Other Christian denomination	1 if person is a Christian Catholic, 0 otherwise	p0Xr01= 4
No denomination	1 if person has no official denomination, 0 otherwise	p0Xr01= 8

X stands for the year in which the person or household was interviewed (X = 0, 1, or 2, i.e. 2000, 2001 or 2002), p for personal and h for household questionnaire. Detailed information on the nomenclature used in the SHP surveys can be found at www.swisspanel.ch/shpdata/var_nom.php?lang=en&pid=25 (18.02.2005).

Chapter IV:
Income Redistribution

1 Introduction[127]

There is an intensive dispute in economics about the relationship between income distribution and economic growth (see BÉNABOU 1996 for a survey and BARRO 2000 for a differentiated analysis). Several authors particularly argue that an unequal income distribution induces the median voter to demand higher income taxes that adversely affect labor supply, private investment, risk-taking and hence economic growth (ALESINA and RODRIK 1992, 1994, PERSSON and TABELLINI 1994, PEROTTI 1993, 1996, SAINT-PAUL and VERDIER 1996, ALESINA and PEROTTI 1996, LEE and ROEMER 1998). According to theoretical arguments and empirical results (POMMEREHNE 1978, STEUNENBERG 1992, GERBER 1996, 1999), public policies are more in line with median voter preferences in direct than in representative democracies. As BESLEY and COATE (1997) show, candidates elected in a representative democracy have sufficient leeway to follow their individual goals once they are in office. This in turn opens possibilities for specific influences by interest groups and bureaucracies (BESLEY and COATE 1998, 2003). Referenda and initiatives provide instruments to selectively control representative and bind policy outcomes to citizens' preferences (FELD and KIRCHGÄSSNER 2001, BESLEY and COATE 2001). It could thus be expected that income distribution and redistribution in direct democracies differ from those in representative democracies.

The most recent literature dealing with impacts of direct democracy on fiscal policies has, however, mainly focused on expenditure, revenue and debt, which are substantially lowered by referenda or initiatives. This holds for the U.S. states and local jurisdictions for which MATSUSAKA (2004) and KIEWIET and SZAKALY (1996) provide the most convincing evidence as well as for Swiss cantons and local jurisdictions for which comparable evidence is provided by FELD and KIRCHGÄSSNER (1999, 2001, 2001a) and by FELD and MATSUSAKA (2003). With respect to the structure of public spending, SCHALTEGGER (2001) and VATTER and FREITAG (2002) find that mainly cantonal and local welfare and cantonal administrative spending are reduced by fiscal referenda. FELD and MATSUSAKA (2000) report that cantons with stronger direct democratic institutions on fiscal issues relatively more strongly rely on user charges than on broad-based taxes to finance spending.

[127] This chapter is based on the joint paper "The Effect of Direct Democracy on Income Redistribution: Evidence for Switzerland" by L.P. FELD, G. KIRCHGÄSSNER, and myself. This paper was presented at various conferences, e.g. the the American Public Choice Society Conference (11 – 14/03/2004), European Public Choice Conference (15 – 18/04/2004), and the Annual Conference of the German Economic Association (28/09 – 1/10/2004).

The latter results are explained by the argument that direct democracy enforces the benefit principle of taxation according to which public services provided by the government and tax prices charged to citizens should be equivalent. If spending control by the voter is strong, user charges can be more easily justified than broad-based taxes that also affect non-users of a particular public good. Hence, in cantons with strong direct democratic institutions, which rely less on taxes but more on fees and user charges as a source of revenue, less redistribution of income should occur. Similarly, welfare spending does not necessarily follow from the benefit principle: because its main purpose is to use tax revenue received from progressive income taxation to provide transfer income to the poor such that those paying for welfare do not receive a direct benefit from their payments.

These studies imply that direct democratic institutions reduce income redistribution through the public sector because less public funds are available and allocated for redistribution purposes. However, reducing the size of these funds used does not necessarily lead to a decrease in distributive gaps between the affluent and the needy if redistribution programs are better targeted in direct than in representative democratic systems. It may well be that transfers undertaken in representative democratic systems are much more determined by the rent-seeking activities of interest groups than by the normative goal of providing financial aid to the poor. In direct democratic systems, the stronger control of representatives may simply reduce the transfers provided to special interests and lead to a more effective and more targeted income redistribution from the rich to the poor.

In this chapter, a first attempt is made to study the impact of direct democracy on income redistribution, using data of the Swiss cantons. In a first step, the findings concerning the impact of direct democracy on broad-based taxes and welfare spending are established on the basis of an extended data set. In a second step, we estimate a model to explain the effect of direct democracy on income redistribution using panel data from the Swiss Federal Tax Office. We start with a brief review of political economy models of income redistribution in section 2 where we critically assess the simple benefit principle argument from above. The empirical studies on institutional determinants of income redistribution are summarized in section 3. In section 4, the Swiss institutions of direct democracy are introduced. The impact of direct democracy on welfare and taxation is econometrically analyzed in section 5. The estimation results of the impact of direct democracy on income redistribution are presented in section 6. Conclusions follow in section 7.

2 Political Economy Models of Income Redistribution

What affects income distribution is a long lasting controversy in economics. Following the theory of factor rewards (CHAMPERNOWNE and COWELL 1998), factor supplies and demands drive the compensation of labor and capital (abstracting from land) such that each factor is paid its marginal productivity while the exact share of each factor depends on the elasticity of substitution between factors. How the factor distribution translates into the personal income distribution is a matter of how much each person owns of the different production factors and on the ability to increase their marginal productivities, e.g., by education. Hence, property rights and the initial endowment with property play an important role. In addition, KEYNES (1936) emphasizes the impact of macroeconomic conditions for income distributions, i.e. the role of unemployment, inflation, business expectations, fiscal or monetary policies.

Starting from the distribution of primary personal income, income redistribution is undertaken either voluntarily by more affluent individuals or coercively by the state (see KIRCHGÄSSNER and POMMEREHNE 1992, BOADWAY and KEEN 2000 and HARMS and ZINK 2003 for surveys on income redistribution in democracies). Aside voluntary income redistribution, the models describing the determinants of coercive income redistribution in a democracy have in common that they mainly built upon the median voter theorem. According to ROMER (1975), ROBERTS (1977) and MELTZER and RICHARD (1981), income redistribution through taxes and transfers is the higher the more skewed the income distribution. Skewness of the income distribution could be measured by the ratio of mean to median income which provides a good intuition for the political mechanism underlying redistribution: The higher the mean as compared to median income the more the median income taxpayer (supposed to be equivalent to the median voter) can gain from taxing the rich.[128] The direction in which income is redistributed is, however, not determined. The median voter might form a coalition with the poor in order to exploit the rich (DOWNS 1957) or a coalition with the rich in order to exploit the poor (POMMEREHNE 1974) such that income redistribution occurs toward the median income position (STIGLER 1970, TULLOCK 1971). The rich might, however, also form a coalition with the poor against the middle income class which has the advantage for the rich of acquiring votes in the cheapest possible fashion. The poor have an incentive to join this

[128] Skewness of the income distribution is however not synonymous to income inequality. See LEE and ROEMER (1998) and BOADWAY and KEEN (2000). For example, two near symmetric income distributions having the same mean but different variances may have the same skewness (close to zero). The less dispersed income distribution could easily be more equal than the other.

coalition because they can expect higher transfers than in a coalition with the middle income class. The precondition for that coalition to emerge is that the poor realize that they gain relative to a coalition with the middle class. Given the possibility of these different coalitions, no clear-cut predictions on voting outcomes over income redistribution can be made (BOADWAY and KEEN 2000).

These arguments presume a voter turnout of 100% which is highly unrealistic. If individuals were rational, they would realize that their individual vote decisively influences voting outcomes only with a very small probability such that costs of voting usually exceed its expected benefits for each individual. It is the 'Paradox of Not Voting' that voter turnout is regularly high, in some countries even above 70%. If some people do not show up at the polls, it is the median person of those who actually do vote who determines the amount of income redistribution. Because high income people have a higher voter turnout than the poor, the median of the actual voting population is richer than that of the whole population (FREY 1971; LEE and ROEMER 1998). However, lower voter turnout does not fundamentally alter the cycling argument outlined above.

Moreover, this argument is not satisfactory because it abstracts from the obvious lack of explanatory power of the rational voter hypothesis in the prediction of voter turnout. BRENNAN and LOMASKY (1993) explain voting behavior by expressive motives. Like people cheer at a football match they go to the polling booth to express their opinion on a political question or on candidates. As income redistribution is concerned, this might either lead to pro-poor voting outcomes when voters express their fairness considerations, or to pro-rich voting outcomes when voters express their prejudice that being poor is a matter of laziness or voluntary choice. KIRCHGÄSSNER (1992, 1996; see also KLIEMT 1986) explains voting behavior by a theory of low cost decisions. In that case, the individual decision is more or less irrelevant for the one who makes that decision, while the collective decision has considerable consequences. With respect to income distribution, the theory of low cost decisions implies that voting for redistribution is cheap as the single vote has no influence on his own wealth position while giving direct payments to the poor is costly. Rational individuals may thus well vote for redistribution rather than give direct payments (KIRCHGÄSSNER and POMMEREHNE 1992).

These arguments do not provide any strong predictions on the extent of income redistribution in direct democracies. A general reluctance to redistribute excessively and enter voting cycles may stem from two sources. First, citizens in direct democracies are repeatedly involved in income redistribution exercises. This repeated interaction may lead to co-operative behavior as EPPLE and RIORDAN (1987) and ARTALE and GRÜNER (2000) show. Citizens may simply realize over time that voting cycles on income redistribution lead nowhere such that a consensus emerges among them according to which only a moderate redistribution takes place. This consensus may be sustained by credible threats to punish those groups in society that deviate from such an implicit 'understanding' (EPPLE and RIORDAN 1987, p. 43, HARMS and ZINK, 2003, p. 657). Second, tax base effects may restrict excessive income redistribution. As already MELTZER and RICHARD (1981) argue, an egalitarian income distribution does not result from tax-transfer-systems decided by the median voter because labor supply incentives are considered. It is not possible to redistribute income through marginal income tax rates close to (or sometimes even above) 100% without, abolishing work incentives. Tax base effects become even more important in open economies when the rich can migrate to jurisdictions with lower redistribution and the poor to those with higher income redistribution (for labor see EPPLE and ROMER 1991, for capital PERSSON and TABELLINI 1992).[129] Tax base effects hence provide for the credibility of threats in repeated interactions of citizens in direct democracies such that a moderate income redistribution can be a stable political outcome.

Since most countries in the world are not constituted as direct democracies, the political economy analysis of income redistribution in representative democracies is more relevant. In a citizen candidate model, BESLEY and COATE (1997) analyze electoral competition in a representative democracy. Division of labor favors the selection of a candidate with high abilities in politics. That candidate whose platform attracts a sufficient number of votes wins the race and is able to implement his preferred policy. Although there is an attachment to citizens' preferences through the selection process, candidates have sufficient room for maneuver to follow their own interests between elections. Aside any personal motives of office holders, which might in the extreme involve corruption, the interests may first stem from ideological dispositions (DIXIT and LONDREGAN 1998, ROEMER 1998) such that left wing parties impose higher marginal tax rates in progressive income tax schedules than right

[129] FELD (1997) argues that tax competition is lower in direct than in representative democratic jurisdictions because the legitimacy of income redistribution is higher in direct democracies. It might be that citizens, therefore, have a stronger attachment to their community, canton, or country.

wing parties. If additional ideological goals are considered like, e.g., religion, politicians may, however, refrain from too high marginal tax rates (ROEMER 1998). Second, representatives may follow the interests of their constituencies. As WEINGAST, SHEPSLE and JOHNSEN (1981) argue, representatives stem from specific districts and want to obtain benefits that are geographically concentrated in their district by spreading the costs over the whole population. Logrolling among representatives in parliaments ensures that pork barrel politics remains stable and income redistribution occurs from the districts of the loosing coalition to those of the winning coalition.

Third, representatives can be captured by special interest groups that engage in rent seeking activities. [130] That interest groups are important actors in social policy and income redistribution policies has long been recognized.[131] BESLEY and COATE (1998) more recently show that interest group influence is one source of inefficiency in their citizen candidate model. Rent seeking as such involves redistributing from those groups in society that are not successfully lobbying the government to those that are successful. One set of groups obtains benefits at the expense of another set of groups. In addition, inefficiencies might occur due to rent dissipation. Finally, representatives might follow the interests of the bureaucracy and redistribute income in their favor (BESLEY and COATE 2003).[132]

Given that these sources may lead to a potential deviation of policy outcomes from citizen preferences, the question arises whether these deviations in income redistribution are different in different constitutional environments. PERSSON and TABELLINI (2000) argue that majoritarian elections entail more targeted spending (local public consumption) due to pork barrel politics, less non-targeted spending (broad social spending like unemployment insurance) and a larger size of government (higher taxes) than proportional elections. The reason is not necessarily that proportional elections more strongly reflect the different preferences of citizens in a society, but that majoritarian elections involve stiffer competition for the individual districts. These results are exacerbated if combined with different political regimes. PERSSON and TABELLINI (2000) analyze policy outcomes in presidential and

[130] Surveys on rent seeking by interest groups are provided by TOLLISON (1982, 1996), BROOKS and HEIJDRA (1989), NITZAN (1994), EKELUND and TOLLISON (2001), MCCHESNEY (2001) and MUELLER (2003).

[131] An early, not well known discussion along these lines was provided by LIEFMANN-KEIL (1961, p. 101).

[132] Because the differences in income redistribution between direct and representative democracies is our main interest, we do not review other sources of influence that shape the political economy of income redistribution like e.g. social status or capital market imperfections. See HARMS and ZINK (2003). The reason is that we conjecture that the quality of their impact on income redistribution is not determined by the type of democratic systems.

parliamentary systems and find that presidential systems reinforce the income redistribution targeted to certain districts. A measure against too much redistribution in that sense is a system of checks and balances that prevents centers of power from colluding.

Instead of (or in addition to) enforcing citizens' preferences for income redistribution by electoral competition and checks and balances, instruments of direct democracy appear to provide a valuable alternative. According to these theoretical arguments, but also to empirical results (POMMEREHNE 1978, GERBER 1996, 1999), public policies are expected to follow median voter preferences more in direct than in representative democracies. Referenda and initiatives provide instruments to selectively control representative and bind policy outcomes to citizens' preferences (FELD and KIRCHGÄSSNER 2001). Given the argument that moderate income redistribution occurs in direct democracies due to tax base effects, and the arguments for additional sources of demand for income redistribution in representative democracies, it is expected that income redistribution is less pronounced in direct democracies. BESLEY and COATE (2001) argue that particularly popular initiatives enable citizens to unbundle legislative packages that combine different issues in log-rolling exercises. Referenda are an additional possibility to veto policies that are too far away from citizens' preferences. All in all, this might well result in direct democracies following the benefit principle of taxation more strongly than representative democracies. The benefit principle is, however, not the normative rule that guides decisions in direct democracy, but the outcome of complex political economy mechanisms.

3 Empirical Studies on Institutional Determinants of Income Redistribution

From these political economy considerations the clear prediction results that political institutions can matter for income redistribution. Not many empirical studies have, however, been conducted on the impact of institutional determinants on income redistribution[133]. There are even not that many which analyze the influence of political economy mechanisms on redistributive outcomes. Most empirical studies focus on spending (structure) or revenue (structure), but only a minority on final income distribution measures. The early studies have been conducted by sociologists and political scientists while economists only recently entered the scene. HEWITT (1977) seems to have been the first to include a measure for the level of

[133] An overview of the empirical literature on political democracy and redistribution with focus on franchise expansion from an economist's perspective can be found in GRADSTEIN and MILANOVIC (2004).

democracy as an explanatory variable in his set of determinants in order to analyze the impact of democratic institutions on redistribution. He defines democracy by three purely legalistic characteristics such as the election of the executive, universal suffrage, and, finally, the secrecy of ballot. For a cross-section of 25 countries in 1965, he finds that democratic experience had a negative, but insignificant impact on redistribution. For the top 5% and the top 20% income earners, however, the negative impact of democracy on their share in total income was both strong and significant at least at the 10% level. Thus, the author detects a negative relation between the length of democratic experience and income inequality; hence, more democratic structures are associated with more income redistribution.

In a panel data set of 4 waves between 1960 and 1975 on 48 countries across the world, PAMPEL and WILLIAMSON (1985) use a measure for the level of democracy as an explanatory variable in order to analyze the extent of pension expenditures relative to GDP. Their democracy variable reflects political competitiveness and political liberties as originally developed by BOLLEN (1980). Dividing the sample into different groups according to their level of democracy, the authors show that for nations with at least a middle level of democracy further economic development does not have a significant impact on pension spending, but the share of elderly persons becomes decisive. They also found that in states with a low level of democracy senior people do not exert political power on pension payments, regardless of the level of economic development. Obviously, the influence of senior residents grows substantially in democracy indicating an increasing political power channeled through political institutions.

PAMPEL and WILLIAMSON (1988) almost repeat the previous analysis with an enlarged set of pooled data (1950 – 1980, one wave every 5th year of 18 advanced industrial nations) and an extended set of explanatory variables which includes also the relative number of votes as well as electoral competition. In congruence with the interest-groups-politics approach, these variables are supposed to measure the political influence of the population on the political process (voting participation) and the impact of interest groups on policy outcomes (the more political competition, the stronger their influence). They find that both vote per population and electoral competition have a significant positive and robust influence on social welfare spending. Interestingly, the impact of ideology is weak and not robust in this model. The inclusion of state structure (e.g. federalism) does not change the impact of the political variables. Admitting interactions between percentage aged and voting participation, on the

one hand, and electoral competition , on the other hand, reveals a significant positive impact of both interaction terms which identifies these institutions as their channels of influence (while the coefficients of the political institutions themselves remain insignificant). This may indicate that the positive effect of democracy measured in their first paper captures the influence of the lobbying power of senior citizens, which appears to go far beyond their socio-demographic influence.

Differentiating the dependent variable social welfare in different components, PAMPEL and WILLIAMSON (1988) find among the political determinants a significant positive effect of voting participation on social insurance and family allowances, but an insignificant one on public assistance. Electoral competition, the measure of the power of lobbies, proves to be positive and significant only for social insurance and family allowance. The reason could be that social insurance is a contribution based system; hence, people have to be entitled by financial contributions whereas public assistance is given on the basis of means-tested needs. In the public assistance regression only unions and unemployment rate have a positive and significant coefficient. The share of the senior population is observed to exert a positive and significant impact on social insurance expenses and family allowances, but an insignificant one on public assistance. The authors interpret their results as supporting the interest-group-politics theory based on the strong interaction terms between the institutional variables and the percentage of elder residents. Hence, it can be concluded that democratic institutions help channeling the demands of particular interest groups.

Using data for 21 countries covering an extremely long time span from 1880 to 1930 in 10-year-distances, LINDERT (1994) investigates the impact of democracy, female suffrage, voter turnout and the frequency of executive turnover on total social transfers and its components, welfare and unemployment, pensions and health expenditure. He finds a significant positive influence of female suffrage as well as of executive turnover on total social transfers. The latter is significant and positive for all subcategories of social expenditure. Voter participation is positive and highly significant for total social transfers, and particularly for pensions and health payments, but not decisive for welfare and unemployment transfers. According to the author, the positive influence of voter participation can be explained either by the legal extension of suffrage also to lower income groups, which favor redistribution, or by the actual integration of social groups favoring progressive taxation and transfer payments into the

political process. In general, demographic influences (age structure and religion) are stronger determinants of redistribution than political variables.

LOTT and KENNY (1999) use panel data on 48 American states from 1870 until 1940. As 'democratic' variables they consider the existence of a literacy test, a secret ballot, the poll tax (a tax on each adult in the community), female suffrage, the additional turnout due to female suffrage and, finally, the additional turnout due to the poll tax. The authors believe (a) that the adoption of secret ballot prevented illiterate people from voting; and also (b) that the poll tax had a disenfranchising effect on blacks and poor whites in the United States as its payment was a perquisite for voting. The literacy test, however, aimed mainly at preventing immigrants with poor language skills from voting. The authors show that giving women the right to vote obviously increased voter turnout, which in turn exerted a positive effect on expenditure of social services. Furthermore, the literacy test had an insignificant coefficient. The additional turnout reduction due to the poll tax as well as the secret ballot, on the other hand, exerted significant and negative effects on social expenditure. This shows that these institutions do, indeed, prevent lower income groups, or, in general, groups favoring redistribution, from voting. With respect to total expenditures of the government (including education, social services, highways) the signs of the 'democratic' variables remain the same, although the significance levels of the coefficients alter. The existence of a poll tax has a negative, significant impact on total expenditure, but an insignificant influence on social services, thus weakly corroborating the hypothesis concerning its effect. The important result of this paper might be that democratic institutions can be realized in such a way that particular societal groups are disenfranchised, which does significantly affect redistributive spending.

The most recent contributions in the field of economics have been undertaken by macro-economists investigating the determinants of economic growth in the so-called fiscal policy-approach. Using a cross section of about 50 democratic and non-democratic countries, PEROTTI (1996) employs a 2SLS approach to simultaneously estimate (a) an economic model, which describes the effect of fiscal policy on growth, and (b) a political model, which comprises democratic institutions and income equality as instruments for the fiscal policy variables. In his political model, he employs alternatively as regressands various tax-related and social transfer variables; these are the average personal income tax, the marginal tax rate, expenditures for social security and welfare and, finally, expenditure for health and housing. His main result is that the interaction term between the democracy variable and the measure

of (in)equality proved to be significant for social security expenditures showing that an increase in inequality has a significant and positive effect in democracies on welfare spending: He also finds a significantly positive effect of inequality in democracies on the marginal income tax rate, but a negative one on the average income tax rate, although both not significant. With respect to the remaining regressands (e.g. health and housing expenditure, expenditure for education), the interaction term appears to be insignificant as well. Income equality solely exerts a dampening, but insignificant effect on public expenditure or tax rates. The share of population above 65 proves to have a positive and significant influence on marginal tax rates, the average tax on labor as well as spending for housing and health. These results show that particularly in democratic states higher levels of inequality lead to more public assistance, which is not the case for non-democracies and other types of redistributive expenditures.

PERSSON and TABELLINI (1994) estimate the impact of the median voter on redistribution, predicting a negative relationship between transfers and the middle quintile, their measure of income equality. They find a dampening impact of the middle income share on redistribution, some ratio of redistributive spending to GDP, in a cross section of around 1960 (13 countries)[134]. BASSET et al. (1999) re-estimate their model and do not find the previous result to be robust to differences in definitions of equality, sample size and the inclusion of the share of senior residents. Using the average ratio of transfers to GDP over the period 1970-1985, however, BASSET et al. (1999) were able to mildly corroborate a negative relation for democracies, using a specification similar to the one in PEROTTI (1996) and by including an institutional interaction term. Both the institutional interaction term with a negative sign and the coefficient of democracy with a positive one appear to be significant, whereas the equality measure is never. Again, the result obtained by PEROTTI (1996) is only mildly corroborated, as it is not robust to different measures of income equality: Democracy by itself appears to be favoring redistribution, but in case of low income inequality less redistribution occurs, when also the actual income distribution is included in the regression.

More recently, GRADSTEIN et. al. (2001) have empirically shown how ideology and political democracy interplay and how political institutions impact inequality: Political democracy appears to affect income inequality negatively in Judeo-Christian societies if inequality is high because the needy outvote the affluent. If, however, there is no need for redistribution,

[134] Significance was observed by BASSETT et al. (1999) after having corrected a small mistake in the data.

democracy does not appear to have a significant distributional effect. This is the case in non-Judeo-Christian societies which value income equality highly. No influence of democracy on income distribution might also be observed because these societies rely on informal transfers to a large extent.

To sum up, according to these findings, democratic institutions appear to serve as channels for transporting the interest of specific socio-demographic groups beyond their purely demographic influence. Furthermore, the design of institutions matters for the inclusion or exclusion of specific societal groups and thus influences redistribution. In addition, the macroeconomic literature suggests that the impact of inequality by itself as a component of the preferences of the median voter cannot be predicted, whereas the negative interaction between democracy and equality seems to be quite robust. None of these papers has, however, addressed the differential impact of democratic regimes on income redistribution. For a panel of OECD countries since the 1960's, MILESI-FERRETI, PEROTTI and ROSTAGNO (2002) study whether countries with majoritarian elections have different levels of transfer payments than countries with proportional elections and find that transfer payments are strongly positively related to the degree of proportionality. For a panel of 60 countries from 1960 to 1998, PERSSON and TABELLINI (1999, 2003) support these findings and also report evidence that welfare spending is lower in presidential systems. Most interesting, GRADSTEIN et. al. (2001) also showed that parliamentary systems exhibit a higher degree of income redistribution than purely presidential regimes. These results support the hypothesis that the impact of interest groups and bureaucracies on broad based income redistribution is less restricted in presidential systems.

4 Swiss Data on Direct Democracy and Income Distribution

Our study on the impact of direct democracy on income redistribution adds to this political economics literature. Before proceeding to the empirical study, it is necessary to briefly introduce the Swiss political system. Switzerland is an ideal laboratory to study the impact of direct democracy on policy outcomes. Aside its pronounced structure of fiscal federalism, Switzerland is known for its considerable variation of institutions of direct democracy. Most cantons have some form of semi-direct democracy with a parliamentary system with legislators elected according to a system of proportional party representation. Only two rural cantons (*Appenzell Innerrhoden* and *Glarus*) take political decisions in cantonal meetings

(*Landsgemeinde*). On the other hand, the cantons are shaped by different institutions of political participation rights (TRECHSEL and SERDÜLT 1999; FELD and MATSUSAKA 2003). Proposals can be initiated via the voter initiative, and new laws passed by the legislature are, to different degrees, subject to an optional or even a mandatory popular referendum. Fiscal referenda on policy decisions of sub-national governments have been of particular interest in the literature. In our empirical analysis, we use an overall index of direct democracy as proposed by FREY and STUTZER (2000). Because it consists of many different instruments of direct democracy[135], we refer the reader to that source. In order to contrast the index with one of its components, we have a closer look at the data for the fiscal referendum and the index of the year 1992. The relevant information on the fiscal referendum is provided in table 1.

There exists no fiscal referendum at the federal level, but with the exception of the canton of *Waadt* (VD)[136] all cantons know at least some kind of a fiscal referendum. 13 cantons have a mandatory as well as an optional fiscal referendum. In seven other cantons (BE, BS, BL, AG, TI, VS, GE) only the optional fiscal referendum exists , whereas in SZ, GL, ZG, AR, NE new spending projects have to pass the mandatory, but not an optional fiscal referendum. The fiscal referendum can be differentiated according to five categories: the fiscal referendum for public spending, public-sector bonds, taxes, holdings on enterprises, and purchases of real estate. In principle, there are threshold variations for non-recurring expenditures and for recurring expenditures. Five cantons (FR, AR, VS, NE, JU) determine thresholds as a percentage of last budget's expenditures. All others determine a specific amount instead. The number of signatures required to qualify for ballots and the time span within which the signatures have to be collected for the optional fiscal referendum are also very diverse among cantons. This number varies from 0.49% of all voters in the canton of *Obwalden* (OW) up to 4.28% in the canton of *Jura* (JU), while the time span for collecting the signatures ranges from 30 to 90 days.

Comparing the existence of different forms of fiscal referenda and their spending thresholds with the index of direct democracy (see table 1), it becomes clear that there is a certain correspondence. On the other hand, it becomes obvious that the index contains additional information based on the signature requirements for the two initiatives and the (optional) sta-

[135] It is constructed as an unweighted average of the indexes of the legislative initiative, the legislative referendum, the constitutional initiative, and the fiscal referendum.

[136] Also laws and decrees which trigger public spending are subject to an optional legislative referendum in the canton of *Waadt*, in which, however, this institution never seemed to have been applied to such cases.

Table 1: Fiscal Referenda and Direct Democracy in Swiss Cantons

Canton	Non-recurring expenditures[a]		Recurring expenditures[a]		Frey-Stutzer Index[b]
	Optional	Mandatory	Optional	Mandatory	(1992)
ZH	2-20	20	0.2-2	2	4.17
BE	2		0.4		3.50
LU	3-25	25	specific stipulations		4.48
UR	0.5	1	0.05	0.1	5.42
SZ		0.25		0.05	4.93
OW	0.5	1	0.1	0.2	5.58
NW	0.25	5	0.05	0.5	4.92
GL		0.5		0.1	5.50
ZG		0.5		0.05	4.42
FR	0.25 %	1 %	0.25 %	1 %	2.42
SO	1-2	2	0.1-0.2	0.2	5.42
BS	1		0.2		4.40
BL	0.5		0.05		5.69
SH	0.3-1	0.3	0.05-0.1	0.05	5.08
AR		5%		1%	5.50
AI	0.25	0.5	0.05	0.1	5.25
SG	3-15	15	0.3-1.5	1.5	3.40
GR	1-5	5	0.3-0.5	0.5	4.75
AG	3		0.3		5.46
TG	1	3	0.2	0.6	4.04
TI	0.2		0.05		2.10
VD					2.42
VS	0.75%		0.25%		3.42
NE		1.5%		1.5%	2.13
GE	0.125		0.06		1.75
JU	0.5 %	5%	0.05%	0.5%	3.71

Source: G. LUTZ and D. STROHMANN (1998); B.S. FREY and A. STUTZER (2000).

[a] In million Swiss Francs if not indicated otherwise.

[b] The index is constructed by the signature requirement as the number of signatures relative to the number of voters, by the days within which the signatures have to be collected and by the financial threshold as the per capita spending limit allowing for referendum (the values correspond to the year 1992).

tutory referendum. What is also striking are the differences between French- and Italian-speaking cantons and German-speaking cantons. The average index value of German-speaking cantons is with 4.9 considerable higher than that of French- and Italian-speaking cantons with 2.6. These cultural differences must hence be considered carefully in the empirical analysis.

5 The Impact of Direct Democracy on Welfare and Taxation

In order to test the impact of direct democracy on income redistribution, we follow a two step strategy. First, we analyze public expenditure and revenue as well as tax revenue and welfare spending as the two most important instruments for income redistribution at the Swiss cantonal level as a function of the direct democracy index and controls. Second, we analyze income distribution as measured by Gini coefficients of the (approximated) pre- and post-tax personal income distribution as well as the difference between both distributions. We thus propose the following basic model:

$$ID_{it} = \beta_0 + \beta_1 DIRDEM_{it} + \beta_2 V_{it} + u_{it} \quad (1)$$

where ID_{it} stands for the different dependent variables that are of interest in our study of income redistribution. More precisely, in this section we take a closer look at the log of real per capita spending and revenue at the cantonal and local levels as well as tax revenue and welfare and in the next section at Gini coefficients of the pre- and post-tax income distribution.

The model implies that ID_{it} is a function of direct democracy, as measured by the Frey-Stutzer index ($DIRDEM_{it}$) and a vector of control variables V_{it}. β_0 to β_2 are the parameters of interest while u_{it} denotes the error term. According to previous empirical work by FELD and KIRCHGÄSSNER (1999, 2001, 2001a), FELD and MATSUSAKA (2003, 2000), SCHALTEGGER (2001) and VATTER and FREITAG (2002), we expect a negative impact of direct democracy on public spending, revenue, tax revenue and welfare spending. The impact of direct democracy on the after-tax income distribution and redistribution is, however, ambiguous. It may well be that less funds are available for income redistribution, but that transfers are better targeted to the needs. The expected sign of the direct democracy index on the Gini coefficients is hence indeterminate.

V_{it} consists of variables capturing the structure of fiscal federalism such as fiscal decentralization measured by the share of local in total subfederal (i.e. cantonal and local) spending (revenue, tax revenue), tax competition measured by the inverse of the average of all other cantons income tax rates in the highest income tax bracket, weighted by the inverse of geographical distance between cantonal capitals, and unconditional grants which address the impact of vertical transfer payments from the federal government to cantonal governments.

The more fiscally decentralized a canton the less leeway exists for income redistribution because of migration incentives. Similarly, the intensity of tax competition restricts income redistribution at the cantonal level. Finally, unconditional grants help to finance additional spending and relax cantonal budget constraints.

Moreover, the log of national income disaggregated to the cantonal level is included according to the interpretation of WAGNER'S Law (1892) of a possible income effect on the demand for public goods, but also the for income redistribution as an insurance against risk. The ratio of urban population in a canton reflects the impact of population density on fiscal policy decisions of governments. In agglomerations, a concentration of poor people often occurs such that additional income redistribution has to be undertaken. The log of population takes into account economies of scale: Can larger cantons benefit from economies of scale in order to reach a lower level of public (welfare) expenditures when achieving an identical level of supply of public goods. A negative impact of this variable indicates that the larger the population the lower the level of the dependent fiscal variables. In addition, a variable incorporating fiscal constraints at the cantonal level is included. They can be seen as a supplementary instrument to limit the taxing power of policymakers and hence their ability to redistribute income (SCHALTEGGER, 2002). We also include a coalition variable in order to empirically evaluate the effect of broad based coalition governments on the exploitation of the budget as a fiscal commons. The argument that the tax base represents a fiscal commons that is exploited by too many spending ministers in the sense of pork barrel politics is developed by ROUBINI and SACHS (1989), DE HAAN and STURM (1997), KONTOPOULOS and PEROTTI (1999), or VOLKERINK and DE HAAN (2001). Moreover, the share of leftist parties in the government is considered in order to control for the ideological disposition to redistribute income. In line with the literature, we expect this variable to have a positive impact on (the instruments of) income redistribution. Finally, the share of the young and the share of the senior population in total population are controlled for in order to reveal the influence of the two groups which (supposedly) most strongly benefit from income redistribution measures by the state. We finally include the cantonal unemployment rate indicating the macroeconomic conditions and a French and Italian language dummy as controls.

The analysis uses annual data from 1980 to 1998 deflated to the year 1980. The subscript i = 1, ..., 26 indicates cantons and t = 1980, ..., 1998 indexes years. The empirical analysis is performed using a pooled cross-section time-series model. We follow FELD and KIRCHGÄSS-

Table 2: Expenditure, Revenue, Taxes, and Welfare Expenditure per Capita, 1980 – 1998, 494 Observations

	Public Expenditure	Public Revenue	Tax Revenue	Welfare Expenditure
Constant	0.621 (0.66)	0.772 (0.97)	1.688** (2.79)	8.218*** (5.81)
Direct democracy	-0.040* (2.25)	-0.043** (2.61)	-0.045** (3.17)	-0.148*** (4.73)
Fiscal decentralization	-0.570*** (3.91)	-0.596*** (4.33)	-0.060 (1.00)	0.141 (0.82)
Tax competition	-0.175* (2.50)	-0.177** (4.32)	-0.204*** (6.74)	-0.119 (1.17)
Log of unconditional grants	0.110* (2.27)	0.145** (3.19)	-0.047 (1.42)	-0.033 (0.51)
Log of national income	0.135 (0.99)	0.110 (0.89)	0.322*** (3.85)	0.224 (1.23)
Urbanization	0.141 (1.33)	0.110(*) (1.71)	0.460*** (7.26)	0.391* (2.47)
Log of population	-0.001 (0.03)	-0.001 (0.04)	-0.044** (2.62)	-0.088* (2.28)
Fiscal constraints	-0.001 (0.11)	0.004 (0.32)	0.020** (2.62)	-0.034 (1.25)
Number of parties in the cantonal government	0.024 (1.03)	0.022 (1.06)	-0.003 (0.24)	0.042 (1.59)
Share of leftist parties in the government	0.019 (1.44)	0.013 (1.22)	-0.037** (3.34)	-0.010 (0.47)
Share of young population	-0.005 (0.56)	-0.011 (1.35)	-0.029*** (5.32)	-0.044** (3.20)
Share of old population	0.015(*) (1.65)	0.010 (1.29)	-0.006 (1.15)	0.031* (2.23)
Dummy for French and Italian language	-0.108 (1.44)	-0.134* (1.96)	-0.031 (0.82)	-0.155 (1.58)
Unemployment rate	0.007 (0.43)	0.003 (0.22)	-0.000 (0.01)	-0.003 (0.11)
$\bar{R}^2$	0.734	0.744	0.918	0.866
SER	0.118	0.108	0.076	0.177
J.-B.	7.484*	33.206***	2.802	1.376

The numbers in parentheses are the absolute values of the estimated t-statistics, based on the Newey-West autocorrelation-consistent standard errors. '***', '**', '*' or '(*)' show that the estimated parameter is significantly different from zero at the 0.1, 1, 5, or 10% level, respectively. SER is the standard error of the regression, and J.-B. the value of the Jarque-Bera test for normality of the residuals.

NER (2001), who argue that despite the panel structure of the data the inclusion of fixed effects in the cross-section domain is inappropriate because the institutional variables reflecting the extent of direct democracy vary only very little or remain constant over time in most cantons. Accordingly, cantonal intercepts do not make sense as the captured impact on fiscal outcomes is either solely driven by the time variation or in case of time invariant variables, fixed effects are likely to hide the effect of institutional variables and render them insignificant. The consistency of OLS-estimates depends on the exogeneity of the regressands. In order to tackle the problem of possible endogeneity of the decentralization variable, we use an instrumental variable technique with cantonal fixed effects as instruments which are supposed to capture systematic differences in cantonal policy-making.[137] Finally, year effects to circumvent time dependency are included in the main regression and the autocorrelation consistent standard errors according to the Newey-West method are calculated.

Table 2 contains the estimation results for the fiscal policy variables. In all equations, direct democracy has the expected negative sign and is significant at least at the 5% significance level. The coefficient in the public revenue equation is a little bit greater than the one in the public expenditure equation, and the one in the tax revenue equation is of similar magnitude. But because of the log-form of the dependent variable no direct conclusion with respect to the size of the impact of direct democracy can be drawn. However, direct democracy reduces welfare spending much more significantly than the other fiscal policy variables. These results corroborate the earlier findings in the literature and are fully in line with the arguments above. The government obtains less funds in a direct democracy for redistribution of income.

The controls exhibit the expected influences in most cases, but also show interesting patterns of results. Fiscal decentralization is associated with significantly less spending and revenue, but does not significantly affect tax revenue and welfare spending. Tax competition leads to a significantly smaller public sector in spending, revenue, and also tax revenue terms while it does not significantly affect welfare spending. Unconditional grants from the federal level significantly relax the cantonal budget constraints in general, but neither significantly affect tax revenue nor welfare spending. The unemployment rate and the dummy for French and Italian-speaking cantons are largely insignificant. As expected, urbanization has a significantly positive impact on public revenue, tax revenue as well as welfare expenditure,

[137] We used the same instruments in all equations. This also holds for the estimates in table 3.

but we do not find a significant impact on total expenditure. National income only has a significant positive impact on tax revenue. The higher income in a canton the higher is tax revenue, an unsurprising result with progressive income tax schedules. Interestingly, fiscal constraints do not play a significant role in restricting general expenditure or revenue, or even welfare spending. They, however, appear to trigger higher tax revenue which means that tax cuts are less easily possible in cantons with fiscal restraints. Of the political variables, the number of parties in the government does not have any significant effect on fiscal policy, while the share of leftist parties in government is unexpectedly associated with significantly less redistribution in terms of taxes. Economies of scale do not appear to exist in the case of spending and revenue in general because the population variable is not significant. More populous cantons do have less tax revenue and welfare spending. The share of young people is associated with significantly lower tax revenue and lower welfare payments while the share of senior residents has a significantly positive impact on welfare and total public expenditure.

Generally speaking, the model explains the variation in fiscal policy quite well. More than 73% of the variance is explained. In the case of the expenditure and revenue equation, the Jarque-Bera test statistic indicates that the null hypothesis of normality of the residuals can be rejected. Analyzing the outliers and eliminating them from the sample does, however, not qualitatively alter the results[138].

6 The Impact of Direct Democracy on the Redistribution on Income

As contended above, lower levels of tax revenue and welfare spending do not necessarily imply that less income redistribution is achieved in direct democracy. If direct democracy has a more efficient government, these instruments may be more effectively targeted to the needy such that less funds are necessary to achieve a specific level of income redistribution. We therefore turn to the analysis of the income distribution as measured by Gini coefficients.

Data on income distribution are not generally available for Switzerland. There is one micro data set of 1992 (LEU et al., 1997) which could only be used as a single cross section[139]. Because the focus of our interest is on the impact of several cantonal variables on income

[138] The results are presented in table A.1 of the Appendix.

[139] The Swiss Household Panel (SHP) provides information on gross and net household income from 1999 – 2003, but does not distinguish federal and sub-federal tax payments.

redistribution, the cross section data are not enormously helpful however. They leave us with only 26 observations with respect to the direct democracy variable even if individual data provide the basis. For our analysis we, therefore, use panel data on the share of taxpaying households and their incomes in different income classes for the period 1980 – 1997 from the Swiss Federal Tax Office. FLÜCKIGER and ZARIN-NEJADAN (1994) use quite similar data for their analysis of the impact of macroeconomic policy on the income distribution in Switzerland. Since Swiss tax collection until recently has taken place on a biennial basis, the data set is a two years panel. Tax liability for periods t and t + 1 (taxation period) have been calculated on the basis of the average income of periods t – 1 and t – 2 (calculation period), while the payment was made over the periods t + 1 and t + 2 (payment period). Both pre-tax and post-tax distributions can only be approximated because the FTA data do not cover the true gross or taxable income, but are limited to the 'adjusted gross income' ("*Reineinkommen*")[140] and the actual tax payments. Therefore, we view the '*Reineinkommen*' as proxy for gross income and calculate a hypothetical net income by deducing the tax payment. Whether a person is entitled to financial support and how big the size of the actual transfer is exhibits significant variation between cantons. Some of these social transfers already directly affect the gross income and the adjusted gross income, some impact only the after-tax income of the needy households[141]. The estimation method and the basic model to be estimated is the one described in section 5. However, because income distribution might have an impact on voting behavior and, therefore, also on the composition of the government, the ideology of the government is instrumented with the identical set of variables described in previously.

The estimation results are presented in table 3. The first column contains the results for the approximated pre-tax Gini coefficient (in percentage points) as a function of the model described above. The direct democracy index has a negative impact on the pre-tax Gini coefficient which is highly significant at the 0.1% level. In cantons with more direct democracy income distribution before the federal government begins to redistribute income via taxes and further allowances is more equal[142]. Fiscal federalism does not have a consistent and clear-cut effect. The coefficients of fiscal decentralization, grants and tax competition are

[140] For a more precise definition and description of the relation between gross income, '*Reineinkommen*' and taxable income according to the Swiss laws of taxation, see HÖHN and WALDBURGER, 2001, p. 359.

[141] We have to thank EMANUEL LAUBER, tax inspector of the EstV/FTA for clarification (personal communication, 1st of June 2004).

[142] This observed impact may be possibly caused by welfare payments at the sub-federal level which already affect the adjusted gross income of Swiss households

not significantly different from zero. Fiscal constraints do not have a significant impact on the pre-tax income distribution; the significance of the estimated parameter is somewhat below the 10% level.[143] Of the political variables, only the share of leftist parties in government has a significant impact on pre-tax Gini coefficients of the cantons. The higher the share of leftist parties in government, the more equal is the income distribution. The number of coalition parties in government is not significantly different from zero. The share of young and old people from total population both have a significant negative impact on income distribution meaning that they are both associated with more equal distributions. Cantons with a larger population as well as French- and Italian-speaking cantons have significantly more equal income distributions while urbanization is associated with significantly higher income inequality. National income and unemployment do not have a significant effect.

The income distribution measured by post-tax Gini coefficients shows the same qualitative pattern of results. With respect to our variable of interest, this means that cantons with more direct democracy have also more equal post-tax income distributions. It is hence very interesting to look at the difference between pre- and post-tax income distributions which indicate to what extent the closing of the gap in income distributions is due to the different variables in our model. Note that a positive (negative) sign in the difference equation in columns (3) and (4) means that a variable has a positive (negative) impact on the size of income redistribution, i.e. more (less) income is redistributed. A positive (negative) impact also implies that redistribution is such that more (less) equalization of income distribution is achieved because the direction of redistribution is in all canton inequality decreasing[144]. The estimation results reveal that significantly less income redistribution occurs in direct democratic cantons. The coefficient is significant at the 0.1% level. The other variables reveal again the same pattern of results as before with the difference that tax competition now becomes marginally significant and has a positive impact. There is significantly less income redistribution in cantons with a higher share of leftist parties in the government, a higher share of old and young people, in more populous and in the French- and Italian-speaking cantons. On the other hand, there is significantly more income redistribution in the cantons with more intense tax competition (at the 10% level), and with a higher share of the urban population (at the 5% level).

[143] There is, however, a marginally significant negative effect if we exclude the outliers. This would imply that cantons with fiscal constraints have a more equal income distribution. The same holds for the equations of the post-tax distribution and of the difference. See for these results table A.2 of the Appendix.

[144] For all cantons in all years, post-tax income distribution minus pre-tax distribution has a negative sign.

Table 3: Inequality and Redistribution, 1981 – 1997, 234 Observations

(Gini-Coefficients in Percentage Points)

	Pre-Tax Gini-Coefficient	Post-Tax Gini-Coefficient	Difference	Difference
Constant	62.781*** (3.91)	58.843*** (3.98)	3.940** (2.96)	-1.371** (2.82)
Pre-tax Gini-Coefficient				0.085*** (18.20)
Direct democracy	-1.679*** (6.04)	-1.562*** (6.09)	-0.117*** (4.97)	0.025* (2.12)
Fiscal decentralization	2.756 (1.23)	2.678 (1.31)	0.078 (0.37)	-0.155* (2.21)
Tax competition	0.724 (1.17)	0.635 (1.10)	0.089(*) (1.95)	0.027 (1.49)
Log of unconditional grants	1.027 (1.28)	1.005 (1.37)	0.023 (0.31)	-0.064** (3.02)
Log of national income	2.545 (1.12)	2.311 (1.11)	0.234 (1.17)	0.019 (0.29)
Urbanization	3.359* (2.40)	3.080* (2.39)	0.279* (2.36)	-0.005 (0.11)
Log of population	-1.646*** (4.57)	-1.526*** (4.61)	-0.120*** (3.87)	0.020 (1.36)
Fiscal constraints	-0.323 (1.61)	-0.294 (1.60)	-0.029 (1.60)	-0.004 (0.81)
Number of parties in the cantonal government	-0.242 (0.91)	-0.221 (0.91)	-0.021 (0.90)	-0.001 (0.10)
Share of leftist parties in the government	-0.634** (2.81)	-0.577** (2.77)	-0.057** (3.07)	-0.004 (0.46)
Share of young population	-0.298(*) (1.80)	-0.273(*) (1.79)	-0.026(*) (1.82)	-0.001 (0.14)
Share of old population	-0.506*** (3.41)	-0.460*** (3.36)	-0.046*** (3.75)	-0.003 (0.85)
Dummy for French and Italian language	-3.428** (3.07)	-3.180** (3.10)	-0.250** (2.61)	-0.029 (1.32)
Unemployment rate	0.100 (0.42)	0.103 (0.47)	-0.003 (0.17)	-0.012* (2.09)
$\overline{R}^2$	0.673	0.672	0.672	0.958
SER	1.449	1.338	0.119	0.043
J.-B.	177.544***	165.945***	232.692***	19.369***

Comments see table 2.

It is most interesting to finally analyze the impact of the different variables on income redistribution conditioned on the pre-tax income distribution in the fourth column of table 3.[145] This method allows to identify which variables affect income redistribution when it may be most needed to close the gap between the rich and the poor. Again, the effect of direct democracy is very instructive: Conditioned on the pre-tax income distribution, cantons with direct democracy exhibit significantly more income redistribution (at the 5% level). If the pre-tax income inequality is relatively high, direct democratic cantons redistribute more income, otherwise they don't. Taken together, the impact of direct democracy is very intriguing: There are significantly less funds available for income redistribution, there is significantly less income redistribution, but if the pre-tax income inequality is relatively high, income redistribution in direct democratic cantons is significantly higher. This supports the hypothesis that welfare payments in direct democratic cantons are better targeted than in more representative democratic cantons: the money is more effectively used in direct than in representative democracies.

More fiscally decentralized cantons show a significantly lower level of income redistribution if the pre-tax income distribution is relatively higher, an effect that is slightly corrected by the (insignificant) impact of tax competition. Cantons which receive higher grants do redistribute less income. Cantons with more unemployment redistribute income significantly less. Overall, the model explains the variation of income distribution and income redistribution quite well. At least 67% of the variance can be explained. According to the Jarque-Bera test statistics, the null hypothesis of normality of the residuals can be rejected at high significance levels. An analysis of outliers and a sensitivity check of the estimates to outliers reveal again, however, that the results are quite robust.[146]

7 Concluding Remarks

Does direct democracy lead to less redistribution? Taking into account that – according to the results presented in this chapter but also to other empirical results – it leads to lower welfare expenditure per capita, one might draw such a conclusion. Such an apprehension is probably behind the strong rejection of introducing additional direct popular rights in Germany by

[145] In addition to the instruments mentioned above we employ the two year lagged value of the pre-tax Gini coefficient as an instrument for this variable.

[146] The results are again presented in the Appendix. See table A.2.

some political scientists: They fear the end of the welfare state. The possible effects might be largely overestimated by such fears; despite the largely developed direct popular rights the Swiss welfare state has not yet broken down, and a cutback of public welfare expenditure occurred in purely representative systems like the German one during recent years as well. Nevertheless, there might be – *ceteris paribus* – a negative impact of direct democracy on redistribution.

That public welfare expenditure is somewhat lower might, on the other hand, not necessarily compromise redistribution. Because public expenditure might be better tailored to the needs of the electorate in direct democracies, given the amount of public welfare expenditure its redistributive effect might be larger than in purely representative systems. Taking these two countervailing effects into account, it is theoretically open which of them dominates.

Our results provide an interesting picture. Looking just at the differences between the income distributions before and after taxation, it is shown that direct democracy has a negative impact on redistribution, as it is supposed by many of its critics. Furthermore, taking into account that redistribution is needed the more the more unequal the pre-tax distribution is, direct democracy has a significantly positive effect on redistribution. Moreover, analyzing its impact on the pre-tax and after-tax income distributions a strong inequality equalizing effect of direct legislation could be revealed. This indicates two things: first, efficiency gains are present in direct democracies when it comes to reducing income inequality. Second, the electorate is the more in favor of redistribution the more they assess it as being justified by the unequal starting conditions. Such a behavior will rather strengthen than weaken the welfare state.

The results which are presented in this chapter are, however, only first estimates. Further analyses have to follow. One obvious shortcoming of this study is that by using tax data those who do not pay taxes (and might be the poorest citizens in the society) are not included in our analysis. Another shortcoming of these tax data are that they are rather based on household than personal income and do not take into account the number of persons living in the same household. Such improvements might be made by estimating a similar model using the cross section data of 1992 from the study of LEU et al. (1997). As mentioned above, due to the very small number of observations and the cross-sectional nature we then, however, face other and not less severe problems.

Finally, one should take into account that here we only deal with the cantonal and local level and, there, only with the tax side. This side is, of course, very important for redistribution. Nevertheless, a large part of redistribution is done by public expenditure.[147] Moreover, the perhaps most strongly redistributive part of the Swiss welfare state is the first column of the old age pension system (AHV) which is located at the federal level. It is much more redistributive as, e.g., the corresponding German system. Nevertheless, not only its introduction but also all of its many revisions have finally been accepted in referenda. Thus, Switzerland with its direct democracy at all governmental levels can hardly be seen as an example where the welfare state is endangered by the existence of direct popular rights.

[147] See for this the corresponding estimates in KIRCHGÄSSNER and POMMEREHNE (1996) and FELD (2000).

8 Appendix

Table A.1: Expenditure, Revenue, Taxes, and Welfare Expenditure per Capita, 1980 – 1998 (Outliers excluded)

	Public Expenditure	Public Revenue
Constant	0.585 (0.63)	0.626 (0.86)
Direct democracy	-0.042* (2.19)	-0.038* (2.44)
Fiscal decentralization	-0.556*** (3.84)	-0.465*** (4.12)
Tax competition	-0.183** (2.67)	-0.188** (3.13)
Log of unconditional grants	0.113* (2.35)	0.163*** (3.78)
Log of national income	0.124 (0.93)	0.098 (0.86)
Urbanization	0.153 (1.48)	0.134 (1.60)
Log of population	-0.001 (0.01)	-0.000 (0.02)
Fiscal constraints	-0.001 (0.05)	0.006 (0.52)
Number of parties in the cantonal government	0.026 (1.15)	0.024 (1.26)
Share of leftist parties in the government	0.20 (1.54)	0.016(*) (1.66)
Share of young population	-0.005 (0.15)	-0.013(*) (1.81)
Share of old population	0.015(*) (1.75)	0.012(*) (1.83)
Dummy for French and Italian language	-0.107 (1.44)	-0.113(*) (1.79)
Unemployment rate	0.005 (0.34)	0.004 (0.36)
$\overline{R}^2$	0.741	0.744
Number of observations	493	484
SER	0.117	0.108
J.-B.	4.382	4.981(*)

Comments see table 2.

Table A.2: Inequality and Redistribution, 1981 – 1997 (Outliers excluded)

	Pre-Tax Gini-Coefficient	Post-Tax Gini-Coefficient	Difference	Difference
Constant	63.951*** (4.35)	59.976*** (4.42)	3.975** (3.33)	-1.489** (3.16)
Pre-tax Gini-coefficient				0.085*** (19.20)
Direct democracy	-1.647*** (6.18)	-1.534*** (6.23)	-0.113*** (5.06)	0.025* (2.17)
Fiscal decentralization	4.749* (2.43)	4.419* (2.45)	0.330* (2.11)	-0.181** (2.75)
Tax competition	0.832 (1.40)	0.732 (1.32)	0.100* (2.34)	0.024 (1.29)
Log of unconditional grants	0.654 (0.93)	0.680 (1.05)	-0.027 (0.44)	-0.56** (2.72)
Log of national income	3.385(*) (1.77)	3.039(*) (3.04)	0.346* (2.23)	0.013 (0.21)
Urbanization	3.717** (3.06)	3.383** (3.00)	0.334*** (3.48)	-0.013 (0.11)
Log of population	-1.717*** (5.07)	-1.588*** (5.09)	-0.129*** (4.53)	0.022 (1.60)
Fiscal constraints	-0.304(*) (1.71)	-0.278(*) (1.71)	-0.026(*) (1.78)	-0.001 (0.15)
Number of parties in the cantonal government	-0.363 (1.52)	-0.326 (1.48)	-0.037(*) (1.94)	0.001 (0.08)
Share of leftist parties in the government	-0.674*** (3.43)	-0.608** (3.32)	-0.65** (4.35)	-0.008 (1.09)
Share of young population	-0.333* (2.07)	-0.306* (2.07)	-0.028* (2.01)	0.001 (0.16)
Share of old population	-0.484*** (3.74)	-0.441*** (3.68)	-0.043*** (4.24)	-0.003 (0.79)
Dummy for French and Italian language	-2.726”* (2.92)	-2.556** (2.95)	-0.170* (2.37)	0.031 (1.3204
Unemployment rate	0.081 (0.44)	-0.063 (0.37)	-0.017 (1.23)	-0.009(*) (1.77)
$\overline{R}^2$	0.768	0.764	0.797	0.961
Number of observations	200	200	200	207
SER	1.175	1.092	0.091	0.041
J.-B.	0.428	0.742	1.589	3.194

Comments see table 2.

Chapter V:
Public Safety

1 Introduction[148]

Direct democracy is conjectured to lead to an allocation of resources that comes closer to the preferences of the median voter than does that in a representative democracy. In contrast to traditional economic theory, however, this study assumes that the median voter is subject to bounded rationality. Based on these two assumptions particularly, it is hypothesized that the voter (1) prefers that less money be spent on crime prevention measures and (2) favors protection against property crime over fighting violent crime. In this contribution, these hypotheses are developed in detail and empirically tested for the case of Switzerland, a country with strong variation in the degree of direct democracy and public safety at the cantonal level.

Based on an extensive review of the literature, it appears that this investigation is one of the first to combine and empirically test the median voter theorem of public choice with the theorem of bounded rationality developed by behavioral economists and economic psychologists. It is also the first analysis of an economic model of crime for Switzerland using an econometric panel method for cantonal-level data that employs a rich set of explanatory variables[149].

Theoretical models of political economy demonstrate that institutions of direct democracy lead to an allocation of goods and resources that comes closer to the median voter's preferences than the allocation achieved in a representative democratic system. Empirical studies for both the U.S. and Switzerland provide evidence that the provision of public goods is more efficient in direct democracies and government spending is lower. In the theoretical model world of political economy, the median voter is assumed to be a perfectly rational *homo economicus* who, on average, makes unerroneous predictions of event probabilities. On the other hand, economic psychologists have found that the rationality of an average human being is bounded: i.e., an individual's predicted probabilities differ systematically from actual

[148] A preliminary version of this paper was presented at the conferences of the British Society of Criminology, (6-9/07/2004), Portsmouth, GB, the European Society of Criminology, (25-29/08/2004), Amsterdam, Netherlands, the European Association of Law & Economics, Zagreb, Croatia, (23-25/09/2004), and the 56th Annual Meeting of the American Society of Criminology, Nashville, (17-20/11/2004). I thank Reiner Eichenberger, Simon Gächter, Martin Jäggi, Gebhard Kirchgässner, Friedrich Schneider and conference participants for helpful comments and suggestions.

[149] Using Swiss data, a purely time-series analysis can be found in FUNK and KUGLER (2002, 2003), who used as explanatory variables only two deterrents and either the number of unemployed (2003) or lagged crime rates (2002). A partial correlation analysis for two cross sections of 1960 and 1970 can be found in ZWICKY (1982)

average probabilities. An *optimism bias* induces the underestimation of the subjective probability for a bad event to occur, with, according to the literature on the *availability heuristic*, the bias becoming stronger when an event is less frequently observed than other events.

This study combines economic and psychological approaches by applying them simultaneously to the supply of the public good, 'public safety', defined by the level of crime in a specific region. Depending on the type of crime, some crimes are committed more often than others. Based on this observation and applying the 'optimism bias' theory and 'availability heuristic' of economic psychology, it is conjectured that voters prefer the prevention of more frequently occurring property crime relative to less frequently occurring violent crime. Therefore, it is hypothesized that direct democratic systems will reallocate given budgetary resources for police issues in favor of the protection of property at the expense of personal integrity. Further, since studies in economic psychology have shown that the true average likelihood for the whole society is also systematically underestimated, it is also conjectured that fewer financial means will be allocated through institutions of direct legislation to crime prevention and crime protection measures.

This study also assumes that governmental and administrative decision makers are better at forecasting than the common citizen, because empirical investigations by economic psychologists have detected that *debiasing strategies* – linked to bureaucratic procedures, continuous training, statistical information and continuous feedback – may reduce the underestimation of true probability. Because these findings seem consistent with the initial hypotheses, it is further conjectured that in more representative democracies, more resources will be devoted to public safety in general and, more specifically, available means allocated between several crimes will reflect the actual probability of their occurrence more closely than in direct democracies.

These hypotheses are tested using a synthetic panel of Swiss data on cantonal police expenditure and crime rates between 1986 and 2001. The regressors are selected according to the economic model of crime and public choice models of government expenditure. In anticipation of the empirical results, support is found for the boundedly rational median voter theorem: First, subfederal police expenditure is observed to be dampened by the degree of direct democracy, which subsequently translates also into a lower relative number of cantonal

policemen. Second, if the model takes the impact of direct democracy on police expenditure explicitly into account, property crime rates are shown to be considerably lower in direct democratic cantons than in more representative democratic ones, and, the assault rate is shown to be significantly higher. Finally, executive efficiency gains are detected in state provision of public safety against those crimes that are of lower priority to the median voter. These empirical results are interpreted as supporting this study's hypothesis of the strongly bounded rationality of the median voter and bureaucratic efforts to partly compensate for its redistributive effect on crime.

The rest of the chapter is organized as follows. Section 2 reviews relevant theoretical and empirical literature – particularly that by economic psychologists and behavioral economists, with which a public choice economist might not be too familiar – from which testable hypotheses are derived. Section 3 outlines the specification of the economic model of crime, and section 4 describes the data. The estimation results are then presented and discussed in section 5. Finally, section 6 concludes the report.

2 Political Economy and the Behavioral Economic Context: Derivation of Hypotheses

2.1. The Median Voter Theorem

Economic theory predicts that in a direct democracy, an allocation of resources is achieved that more closely approximates median voter preferences than such allocation in a representative democracy. Using a model with a one-dimensional ideology space, FELD and KIRCHGÄSSNER (2001) show that institutions of direct legislation force politicians to shift their policies toward the median voter's position[150]. In general, the stronger the institution of direct legislation (i.e. the easier it is for voters to make use of it), the stronger its influence on the decision-making process and the greater the size of this shift. In other words, bureaucrats of a Niskanen type (NISKANEN 1975, WILLIAMSON 1964) and politicians interested only in reelection are considerably limited in their budget maximizing or spending behavior. Accordingly, resources should be wasted less in more direct democratic systems than in more representative democratic systems, and the provision of public good should be more efficient at the societal level. In this context, it must be noted that the mere existence of such a direct

[150] This effect is found for both the mandatory and optional referendum, as well as the (statutory) initiative.

legislative institution itself serves as a sufficiently credible threat to influence the allocation of resources.

Empirical analyses of the impact of direct democracy for the U.S. and Switzerland on the provision of public goods provide evidence consistent with the median voter theorem. Investigations of a budgetary effect show a revenue and spending restraining influence (FELD and KIRCHGÄSSNER 2001, MATSUSAKA 1995), and POMMEREHNE (1983) provides empirical evidence for Switzerland that garbage collection is more efficient in direct democratic municipalities than in more representative democratic ones. Most recent research on welfare payments also shows them to be more efficiently targeted in direct democracies than in more representative ones (FELD et al., 2004). In sum, there exists ample empirical support for the median voter theorem (for a literature review and overview, see KIRCHGÄSSNER, 2000, 2001, 2002a, KIRCHGÄSSNER et al. 1999). Based on these findings, it is quite probable that, in more direct democracies, the allocation of goods and resources for the provision of the public good 'public safety' should also be closer to the median voter's preferences.

2.2 Optimism Bias

A preferred allocation of goods and resources may depend not only on the present economic situation but also on median voter predictions about the future and opinion of what is required to meet personal needs. Hence, a misprediction about future developments will also affect the demanded state production of public goods: the stronger the influence of a boundedly rational median voter on the political decision-making process, the more biased away from a (theoretically optimal) fully rational median voter's position the actual allocation of goods and resources will be. However, the typical model of the median voter assumes perfect rationality, which also implies perfect foresight. In this paper, in contrast, based on recent developments in the fields of behavioral economics and economic psychology, this traditional approach will be extended to assume only a partially rational voter.

One type of bounded rationality hypothesized by behavioral economists and particularly economic psychologists is *human judgment error*, meaning that actual judgments show systematic differences from unbiased forecasts (KAHNEMAN et al., 1982). Among these, there also exists an *optimism bias*, which means that individuals underestimate their own

probability of experiencing a bad event relative to the predicted risk for the average person[151]. In other words, the estimated ratio of the subjective to the societal likelihood is biased downwards.

Empirical studies conducted mostly by psychologists show the validity of the optimism bias hypothesis. In particular, empirical evidence supports the conjecture that human beings would rather systematically underestimate their own likelihood than overestimate the average person's risk. In other words, people assume their own probability to be lower than that of their peers (ARNOULD and GRABOWSKI 1981, CAMERER and KUNREUTHER 1989). Such an optimism bias is observed among college-age drivers for the estimated likelihood of being involved in a car accident (DEJOY 1989)[152], among college students for being sued after having committed a felony (WEINSTEIN 1980), or among male drivers (aged 17 – 72 years) for being caught after having committed felony drunk driving (GUPPY 1993), and among smokers for the health risk of smoking (HAMMAR and JOHANSSON-STENMAN 2004, PROKHOROV et al. 2003). In all, over 250 empirical studies provide support for the existence of the optimism bias (JOLLS 2004). Moreover, this bias can be quite substantial: test subjects' own estimated likelihoods are observed to be 20% to 80% lower than the predicted risk in society (JOLLS 1998). Based on this empirical evidence, the likelihood of becoming a crime victim should be systematically underestimated by a typical human being, making it systematically lower than the estimated risk for the average person.

In addition to the misprediction of personal probability, the general probability in the population of experiencing a negative event is systematically misjudged. An early study by LICHTENSTEIN et al. (1978) asked test subjects to assess annual relative average frequencies of various causes of death in the United States, including diseases, accidents, and natural disasters. Most interesting for this present study, the relative risk for an average person becoming the victim of murder or manslaughter was underestimated despite extensive coverage of this topic in the mass media (see section 2.4). However, the frequencies of bad events that affect many people simultaneously (e.g. natural disasters) or are connected to accidents evoked by (uncontrollable) technology (e.g. nuclear meltdowns) tended to be systematically overestimated. Apparently, test subjects deemed these crime related causes of

[151] A possible cause for positive and negative biases can be emotions such as anger or fear (LERNER et al. 2003).

[152] See also SVENSON et al. (1985); for motorcyclists see RUTTER et al. (1998).

death less probable than other causes (FISCHHOFF 2002)[153]. Following this argument, and assuming a high correlation between the estimated frequencies and predicted societal probabilities, the average person should also mispredict the average likelihood of any 'normal' property or violent crime occurring in society. Consequently, given the misjudgment about both personal and peer probability of becoming a crime victim, a boundedly rational median voter can be conjectured to prefer a lower level of crime prevention measures than if the decision-making process were unbiased.

Finally, the size of the optimism bias seems to depend on the rareness of the event. JOLLS (2004) and FISCHHOFF (2002) argue in the tradition of the literature on the *availability heuristic*[154] that infrequent events, those not often heard of or seen, are not easily imagined or kept in mind. Therefore, the likelihood of their occurrence is underestimated. As a result, there exists a causal relation between the degree of misprediction and the frequency of an event: the less frequently an event occurs, the lower its 'availability' and the higher the underestimation of its probability of occurrence[155] by a boundedly rational individual. Some empirical evidence supports this hypothesis. For example, availability heuristic helps explain the fact that people do not insure against natural disasters and car accidents – their subjective estimation of likelihood is negatively affected by absence of such experience in their own life or that of their friends, neighbors, and relatives (KUNREUTHER 1976). On the other hand, people are observed to update their estimated probabilities after experiencing a negative event either personally or in their immediate environment. For example, after being arrested once, offenders appear to substantially correct the subjective probability of arrest (LOCHNER 2003).[156] Therefore, the probability predicted for the types of events that occur more often should be closer to actuality than predictions for very rare events. In the case of this study, the probability of being murdered should be underestimated to a stronger degree than the risk of

[153] The probabilities of nuclear power accidents and other environmental catastrophes are usually overestimated (see also VISCUSI 1992; KURAN and SUNSTEIN 1999), which might be explainable by mass media coverage (COMBS and SLOVIC 1979). Most particularly, the likelihood of disease is strongly underestimated, a little less than the likelihood of accidents.

[154] See TVERSKY and KAHNEMAN (1973).

[155] In particular, the accuracy of subjective estimates depends on whether "(1) the exposures are proportional to actual frequencies; (2) the events are equally memorable; and (3) [the] people have reliable mental techniques for converting the availability of instances into summary estimates" (FISCHHOFF 2002). According to SCHWARZ and VAUGHN (2002), people usually do not investigate official statistics before answering a question on the likelihood of an event but rather rely on their own memory.

[156] However, this same study finds no impact of the experience of random individuals or their local surroundings, friends included, which contradicts the previous empirical finding of KUNREUTHER (1976).

being burglarized. Hence, the relative subjective probabilities for different crimes would be biased in favor of the more frequent crime[157].

2.3 Debiasing Strategies

Biases, such as misprediction of event probabilities, can be mitigated through *debiasing strategies* such as the influence of the social environment, available factual information, and bureaucratic decision making. For example, the use of protocols that prescribe a step-by-step procedure for experts can help alleviate these experts' judgment bias (MERKHOFER 1987, MORGAN and HENRION 1990, MORGAN and KEITH 1995) as can specific instructions on how to assess particular information (that might otherwise give rise to a bias or not lead to debiasing by itself) (CLARKSON et al. 2002). Peer reviews are also suitable for mitigating judgment errors: people tend to be more critical of other person's statements than of their own estimates (TAYLOR and BROWN 1988). Most particularly, according to psychological evidence, one potential remedy is to make more objective information available to test subjects (DASGUPTA and GREENWALD 2001) or provide training in research methods and statistics (LEHMAN et al. 1988). Other debiasing techniques are abundant practice and/or training, task restructuring, and feedback on performance (FISCHHOFF 1982). Such feedback may be thought of as the provision of appropriate statistical information on frequencies of rare events: individuals might then use this information to objectively evaluate their own estimate of probability[158]. In this specific case, such objective statistical information might address crime rates, clear-up rates, and conviction rates that could debias criminologists, police administrators, and politicians specializing in this issue. According to the empirical evidence, once given specialized training, daily practice, and bureaucratic procedures for assessing such information, these experts will probably be less subject to an optimism bias and/or availability heuristic than the average person.

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[157] A similar conclusion can be drawn from application of a variation of the *availability hypothesis*: According to this theory, only those probabilities are underestimated that do not exceed a particular critical level (JOLLS, 2004). An alternative explanation is given by *prospect theory*: a certainty effect can lead to an increase in the difference between the perceived probability of an infrequent and very frequent event. This theory leaves open whether one of the probabilities is underestimated or the other overestimated; what is important is that the gap in subjective likelihoods of occurrence is greater than the actual gap (KAHNEMAN and TVERSKY 1983).

[158] Experiments do indeed show that overconfidence is decreased if the framing for an assessment is generated by a random process and is thus not based on the subject's own prior beliefs, i.e. when the subject's own naiveté creates the framing for the next evaluation (WINMAN et al. 2004).

2.4 The Role of Mass Media[159]

It is often argued that the biased reporting of particularly severe and spectacular crimes in the mass media influences how people assess both the personal and societal risk of becoming a victim (SLOVIC 1986, JOHNSON and COVELLO 1987). Based on this argument, we would expect an increase in the availability of such crimes (KALICHMAN 1994), which might (partly) offset the low-frequency effect (see section 2.2). An assessment of the empirical psychological literature on the impact of mass media on risk perception, however, gives a rather mixed picture.

First, at least in some studies on mass media content, the reporting was shown to be less biased than usually conjectured (e.g., FREUDENBERG, et al. 1996).[160] Second, it is likely that the correlation between media content and risk perception found in earlier studies[161] was subject to a reversed causality[162]. Furthermore, even if media coverage increases availability of events[163], this effect might be rather short-term, but studies on the duration aspect are still missing. In general, influences can only be permanent if an event is recalled on a regular basis.[164] Third, as already mentioned in section 2.2, the two main important sources of information for forming an opinion are one's personal experience and that of relatives and friends (TYLER 1984), whereas third-hand information, such as media content, is suppressed by higher-order information (WIEGMAN and GUTTELING 1995)[165]. Finally, media coverage tends to influence the perception of societal risk more than individual risk, with individual risk remaining significantly underestimated (TYLER and COOK 1984, COLEMAN 1993)[166]. This finding is in line with the *impersonal impact hypothesis* (TYLER 1980).

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[159] This section is draws upon a survey written by WÅHLBERG and SJÖBERG (2000).

[160] The study by COMBS and SLOVIC (1979) is traditionally cited as evidence for a mass media bias in reporting.

[161] Particularly studies that are based on *cultivation theory* propose a link between the amount of media consumption and assessment of risks. However, more recent studies showed no link (Hirsch 1980).

[162] More recent research reveals that persons with higher apprehension tend to select more frequently programs with a crime-related content (WAKSHLAG et al. 1983). For more potential explanations, see WÅHLBERG and SJÖBERG (2000).

[163] MILBURN and MCGRAIL (1992) showed that news can be recalled more easily when it contained some dramatic parts. But both positive and negative coverage can lead to more concern and thus to higher risk perception (MORGAN et al. 1985).

[164] See the references cited in WÅHLBERG and SJÖBERG (2000). There is also a related literature on the relation between media coverage and fear of crime. It is, however, questionable how much fear of crime and estimation of probabilities of its occurrence are correlated.

[165] For the marginality of the impact of mass media on people's opinion, see e.g. VALLONE et al. (1985).

[166] Some correlation between the two probabilities, however, pertains (SJÖBERG et al. 1996).

Overall, the available literature on the impact of mass media on people's perception of risk suggests that this field of research is still under development. Empirical findings are inconsistent and contradicting. For this reason, no final conclusion can be made with respect to the development of my hypotheses and will furthermore be neglected.

Based on all of the above, the following hypotheses can be formulated:

Hypothesis 1:

> Institutions of direct democracy induce an allocation of scarce means for crime prevention which is in accordance with median voter preferences to a higher degree than in political systems without such institutions.

Hypothesis 2:

> Assuming the median voter to be on average boundedly rational, she or he systematically underestimates the probability of personally becoming a victim of a crime. Further, the median voter systematically mispredicts the risk of a less frequent crime to a higher degree than the risk of a more frequent crime.

Hypothesis 3:

> The median voter also systematically underestimates the average probability of the occurrence of one crime in society.

Hypothesis 4:

> Because of the bureaucratic manner of information gathering, processing, and decision making, trained administrators and experienced politicians tend to suffer less from optimism bias and availability heuristic than the electorate.

2.4 Derivation of Empirically Testable Hypotheses

The hypotheses developed above are not directly empirically testable using data on crime rates, police expenditure, and political institutions. However, combining the fundamental *Hypothesis 1* with the remaining three leads to the following testable hypotheses:

Testable Hypothesis 1:

The financial means available for the prevention and detection of crime is considerably lower in more direct democracies than in more representative democracies.
(Combination of hypotheses 1, and 3 or 2)

Testable Hypothesis 2:

In systems with strong direct democratic institutions, the median voter, because of his or her bounded rationality, induces a reallocation of given means towards those crimes that seem (subjectively) to occur more frequently than other crimes. Debiasing on the part of trained bureaucrats or experienced politicians, however, mitigates this effect in more representative democratic systems.
(Combination of hypotheses 1, 2, and 4)

Available for the empirical testing of these hypotheses are Swiss cantonal data on crimes against person, sexual integrity, and property between 1986 and 2001. Table 1 gives an overview of their occurrences in Switzerland.

Table 1: Summary Statistics of Cantonal Crime Rates, 1986 – 2001

Crime	Obs.	Mean	Std. Dev.	Min	Max
Homicide	416	2.016582	1.918033	0	13.55932
Rape	416	4.377733	3.602149	0	26.00049
Pickpocketing	416	16.58696	20.19978	0	111.4252
Robbery	416	18.32574	20.73591	0	121.2102
Other sex crimes	416	33.00213	23.91515	0	139.8345
Defalcation	416	33.16078	47.2319	0	734.2105
Assault	416	52.52116	36.77527	2.164346	232.4125
Fraud	416	111.9786	143.2382	0	2296.57
Burglary	416	784.0628	458.3655	80.68312	2577.983
Auto theft	416	1214.325	591.9845	54.63459	3243.841

A mean crime rate of 100 in a population of 100,000 equals a probability of 0.1% that one particular individual in this society will be victimized. Setting an (admittedly arbitrary) threshold at this level and defining crimes with probabilities above this level as frequent produces the following observations. According to this threshold, most violent and hate

crimes, such as killing, assault, rape, and other sex crimes, have a low average frequency. However, pickpocketing does not appear to occur as often as usually presumed, which might be caused by a low reporting rate. In addition, the crime rates of robbery and defalcation are quite low, at a probability of 0.018% and 0.033%, respectively.

The most frequent crimes reported are auto theft, with an objective probability of 1.214%, and burglary, with 0.78%. Fraud also exhibits a 0.11% likelihood of occurrence, but with a large standard deviation that reflects a strong variation over time and/or between cantons. The probability of becoming a victim of a property crime (e.g. burglary) is 15 times higher than the likelihood of being assaulted, and the risk of having a car stolen or misappropriated 607 times greater than the probability of being killed. Nevertheless, these latter pair of crimes is subject to a very low level of underreporting[167], so the data-derived ratio may be close to the actuality. In sum, the frequency ratios of the most prominent violent crimes to property crimes are considerably biased towards the latter.

Combining these mean levels of Swiss cantonal crime rates and their likelihood with the hypotheses developed above allows the formulation of the following testable hypothesis:

Testable Hypothesis 2a:

> In political systems with institutions of direct legislation, the median voter prefers and induces an allocation of relatively more resources for fighting most types of property crime than for fighting crime against persons.

3 Model

To test these hypotheses, the economic model of crime is estimated based on the work of BECKER (1968) and EHRLICH (1973)[168] and subsequent empirical contributions. This model can be formulated in a "structural form" but may also be reduced to a single equation (the "reduced form"). The analysis is carried out for both forms. In the structural form of the economic model of crime, the following two equations must be estimated:

[167] See footnote 180.

[168] Whereas BECKER's original model aimed to identify a socially optimal crime rate by equating the marginal cost of crime prevention and the marginal societal loss through criminal activities, it was EHRLICH (1973) who derived an individual's supply curve of offences.

$$(1)\ log\ O = f\ (D_1, D_2, Y, X_1, E_1, cult, inst) + \varepsilon.$$

$$(2)\ D_1 = f\ (Y, X_2, E_2, F, cult, inst) + \varepsilon,$$

where equation (1) represents a typical specification to estimate the supply of crime and equation (2) an auxiliary regression to determine the endogenous variable.

D_1 denotes the endogenous deterrence variable linked to the probability of punishment; in this case, the number of policemen per capita, which is driven by subfederal police expenditure[169]. D_2 represents the second deterrent related to the severity of punishment, measured by the share of suspended sentences among total sentences, which is treated as exogenous in this system[170]. The term *inst* stands for the degree of direct democracy in the respective canton, the variable of interest, whereas *cult* represents the cantonal main language as a cultural covariate. Traditionally, the wealth of the society, measured by national income (*Y*) is employed in both equations. In equation (1), the usual further exogenous sociodemographic (X_1) and economic determinants of crime (E_1) are added: welfare transfers per capita, income inequality in society, unemployment rate, closeness to an important border crossing, interaction of population between cantons, cantonal population, urbanization, and the share of 15 to 24-year-old and 25 to 29-year-old residents. Also included in equation (2) are mostly typical fiscal and political determinants (*F*) used in the field of public finance for government expenditure models. Here, they consist of fiscal decentralization, tax competition, federal transfers, a constitutional constraint that aims to balance the state budget, a measure of the ideology of the cantonal government, the size of the coalition in the cantonal executive. Also included are economic and sociodemographic determinants (E_2, X_2) like urbanization, cantonal population, the share of young residents between the age of 0 and 14, the share of

[169] Clear-up rates are not collected throughout the whole of Switzerland and are therefore not available. FUNK and KUGLER (2003) employ and proxy a conviction rate for mass crimes theft and robbery by dividing the number of convictions by the number of reported offenses. According to the experts in the crime section of the BFS, using this variable is not advisable since convictions are measured by sentences and offenses by either persons or cases. Further, heterogeneity in data collection (persons or cases) between Swiss cantons makes this 'conviction' rate incomparable. Moreover, this approximation does not work with infrequently observed crimes like homicides (years with 0 offense rates but positive conviction rates).

[170] For an alternative specification as a two-way causation model incorporating two different endogenous deterrence variables, see EIDE (1994) and CAMERON (1988). Assuming the severity of punishment as exogenous takes into account that no valid instrument exists in the data; furthermore, this approach follows the classical tradition applied in most of the criminometric literature.

residents between 15 and 24, and the share of persons aged 60 or older[171]. Since D_1 is endogenous in equation (1), it is instrumented with the determinants of police force size used in equation (2) to prevent bias in the whole coefficient vector[172].

In the reduced form, the endogenous variable D_1 is replaced with the exogenous determinants of equation (2). Hence, the complex model of crime shrinks to one single equation:

$$(3)\ log\ O = f\ (D_2, Y, X_1, X_2, E_1, E_2\ , F, cult, inst) + \varepsilon,$$

where the crime supply depends on all exogenous determinants of both equations (1) and (2).

The chosen approach of estimating both a structural form and a reduced form is useful because direct democracy, the variable of interest, is an exogenous determinant in both equations (1) and (2) of the structural form and also of the reduced form (3). Analyzing both forms allows separation of the direct institutional impact from its indirect impact: Whereas the structural form of the model allows observation of the direct impact of direct democracy on the endogenous deterrent (2) and also its direct influence on the crime rate (1), the reduced form permits an analysis of its combined direct and indirect institutional effect (3). With respect to the *Testable Hypotheses*, the first can be assessed with the help of equation (2) because it determines the institutional impact on expenditure. The second (2 and particularly 2a) must be tested using equation (1) since this specification takes into account the allocation of given means for crime prevention and crime detection (this specification analyzes only the reallocation between crimes). Finally, a comparison of the estimates of equations (1) and (3) help reveal the efficiency gains in the provision of public safety at the cantonal police level that might hint at the validity of the not directly testable *Hypothesis 4* (section 2.3).

According to the traditional economic model of crime, the potential criminal weighs the expected costs and expected gains of committing a [property] crime (EHRLICH 1973). Therefore, crime prevention policies can influence both sides of this decision-making process.

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[171] Splitting up the share of young residents into the share of foreigners and the share of Swiss citizens for both younger age groups did not seem appropriate because the shares of foreigners are too highly correlated (rho = 0.93). The main results for the democracy variable, however, are robust to such an alteration in specification. Estimation results are available from the author.

[172] In the traditional model specification, probability of punishment depends on the financial resources available, the crime rate, and some determinant of effective usage of these financial means like urbanization or level of education. For a discussion of the endogeneity problem, see WOLPIN (1980) and CAMERON (1988).

Higher clear-up and detection rates, here proxied by the number of police per capita, and a stricter severity of punishment increase the expected costs (BECKER 1986), whereas higher wealth raises the expected illegal income opportunities (EHRLICH 1973). The effect of income inequality appears to be undecided in the criminometric literature (FAJNZYLBER et al. 2002, BOURGUIGNON et al. 2003)[173], although criminal theory does predict an increasing impact of inequality on crime rates (CHIU and MADDEN 1998). Unemployment rate as a measure of missing legal income opportunities is supposed to raise the expected (net) gains from crime (WITTE and TAUCHEN 1994, DOYLE et al. 1999)[174]; however, welfare transfers increase the costs (loss of secure legal income) (STEVANS 1988). Agglomerations provide greater opportunities for committing a crime and ensuring greater anonymity, thus affecting both sides of the criminal's cost-benefit analysis equally in favor of the felony (GLAESER and SACERDOTE 1999). Young people below the age of 24 who are not yet part of the labor market or who have only a low (starting) income are found to be particularly prone to committing property and violent felonies (COHEN and LAND 1987)[175]. For Switzerland, this current study includes additionally the share of 25 to 29-year-olds because they constitute an important age group among convicted persons in some cantons[176]. This specification also includes the size of each cantonal population to take into account that smaller cantons might be systematically safer. In addition, to account for the geographic proximity of Swiss cantons, the model employs an econogeographic variable that measures the interaction between centers: interaction of canton *i* is defined as the sum of the population of canton *i* multiplied with the population of canton *j* weighted with the inverse of the absolute distance between the two cantons. In accordance with SAH (1991) and KELLY (2000), I assume that a higher degree of interaction between populations leads to more spillovers of criminality across groups and more potential offenders in a canton, more opportunities for committing a felony through greater anonymity, and a lower probability of neighborhood watch, thus raising the crime rate.

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[173] These proxies for legal and illegal income opportunities appear to be interchangeable. Inequality in income can both be interpreted in terms of legal and illegal income opportunities (see ENTORF and SPENGLER 2000 for further literature). Even in the most recent contributions, the empirical evidence only partly corroborates the positive impact of inequality on crime. See BOURGIGNON et al. (2003) and DYOLE et al. (1999) for a description of the various ambiguous results and differences in interpretation. The authors also emphasize that the results depend on the measure of inequality used.

[174] FUNK and KUGLER (2003) employ the absolute number of unemployed persons in a Swiss canton, which may be misspecified.

[175] Their analysis was carried out for motor vehicle theft and homicide. In addition, DOYLE et al. (1999) found a significant positive impact of level of wages in the low-skilled sector on various crimes, particularly on property crime, which this age group might proxy.

[176] In addition, COHEN and LAND (1987) found the age group of 25 – 29 particularly decisive for killings in the U.S. It should be noted that young persons below 29 are also more prone to victimization than higher age groups and therefore form a substantial part of crime demand (COHEN et al. 1981).

In addition, since Switzerland is a small country surrounded by several neighboring countries, the relatively greater wealth of Switzerland might attract an influx of foreign criminals who might exert a deleterious influence on p ublic safety. Therefore, a dichotomous variable is included indicating that a canton is closely situated to a border crossing rich in traffic.

A different way of interpreting the traditional determinants of crime supply is provided by behavioral economics and sociologists who emphasize that ecological factors such as opportunities for illegal activities, the institutional environment in which decisions are made, and peer group pressure are decisive. For example, it is argued that in segmented (dual) labor markets, workers in the secondary labor market face job conditions that are more similar to illegitimate activities than to normal employment (see PIORE 1968, and an empirical test by MYERS 1983). With respect to income inequality, it is believed that relative welfare or income comparisons[177] of those below the mean income with those above occur, creating a feeling of 'malevolence' and causing more crime (EHRLICH 1973). On the other hand, welfare transfers, if correctly targeted, might reduce this envy of the socially deprived (KELLY 2000). According to PUTNAM's (2000) social capital theory, higher social capital such as voluntary neighborhood watches and mutual control and trust brings about lower offense rates. In sum, in the behavioral approach, all types of sociodemographic and economic variables such as education, urbanization, age structure of population, income, expenditure for police, unemployment rates, wage levels, share of foreigners, and racial structure of population can be motivated and interpreted ecologically.

4 Data

Available for this study are Swiss macro data at the cantonal level, which can be used to estimate the aggregate supply of offense function. Crime rates are calculated on the basis of data provided by the Federal Office of Police (BAP) on cantonal occurrences of infractions and attempted infractions of criminal law from 1986 to 2001 per 100,000 residents[178].

[177] Hence, cases of utility interdependence are included that are not usually present in a neoclassical framework.

[178] In Switzerland, centralized collection of reliable cantonal data on crime started in 1984, and since 1986 they have been available electronically. Swiss cantonal police forces either count the number of victims or the number of cases. For example, a murder with four victims is recorded as a 'four' in some cantons and in the rest as 'one'. Also definitions of auto theft differ to a great extent. Therefore, cantonal crime levels are not directly comparable. Heterogeneity in reporting behavior was identified through a survey of the 26 cantonal police forces in cooperation with the Swiss conference of chiefs of cantonal police. These results are subject to strict confidentiality. Based on these findings, dichotomous control variables are constructed and included in equations (1) and (3). Most are highly significant. A replication of the German and French questionnaire can be found in the Appendix of this chapter.

Following the procedure of CHERRY and LIST (2002) and WEEDE (1981), the number of offenses is augmented by 1 to avoid the problem of crime rates of zero. The types of crime included in the study are crime against persons, property, morality, and decency, as well as white collar property crimes. In order to take into account measurement errors in the dependent variable and huge differences in relative numbers, all crime rates are logarithmized, as is common in the criminometric literature[179]. The latest theoretical and simulation-based findings show that this transformation ensures that the estimates will be reasonably accurate and not overly biased[180]. A detailed description of the crime categorization of felonies (as defined by the articles of the Swiss Criminal Code, *StGB*) is provided in the Appendix (table A.1).

Among deterrents, the severity of punishment is measured by the share of unsuspended sentences in total sentences[181], which is available from the Swiss Federal Statistical Office (BFS) only for several major crimes (theft, robbery, fraud, murder, bodily assault, sexual abuse, and rape). Swiss criminal law specifies minimum and/or maximum levels of punishment and allows for suspended and unsuspended sentences, hence still leaving room for the discretion of individual judges. There is reason to believe that the different cantons have developed distinguishable cultures in the local application of the Swiss Criminal Code, so that not only a variation over time but also between the 26 Swiss cantons can be expected[182]. As a second deterrent, the model uses the cantonal police force per capita, based on confidential data provided by the Federal Office of Police (BAP)[183], which proxies the probability of being

[179] To reduce the detrimental impact of measurement errors, also the number of policemen serving as proxy for clear-up rates was logarithmized. See also footnote 180.

[180] Regarding underreporting and the resulting measurement error which might bias the coefficients see EHRLICH (1996) and FAJNZYLBER (2002) for a theoretical investigation. For empirical evidence on reporting behavior depending on the victim's characteristics and the nature of the crime, see MACDONALD (2002). PUDNEY et al. (2000) show through a Monte Carlo simulation that simple OLS estimates are not seriously biased due to measurement errors in the dependent variable or the clear-up rates (see also LEVITT (1998) for similar results). In general, the smallest degree of underreporting can be expected for murder and robbery (see FAJNZYLBER et al. 2002.) and, in the case of Switzerland, auto theft and burglary for reasons of car and household insurance. High underreporting is observed for crimes with a social stigma, such as rape (MACDONALD 2002).

[181] Severe crimes, which are under consideration here, do not allow for fining the convicted.

[182] This assumption can be concluded from the fact that first, until recently, there was no mutual acknowledgment of advocate's licenses between cantons; second, criminal procedural law consists of cantonal laws that prevent intercantonal mobility of judicial personnel (EXPERTENKOMMISSION 1997, p. 25); third, it is mostly local long-term residents who are elected as judges in cantonal and local courts; and finally, there is a continuous effort by the Swiss Supreme Court to eliminate systematic differences in the sentencing practice of the Swiss cantonal courts (GIGER 2002, p. 257 cont., ROTH 2003, 3.3.7).

[183] Data were obtained with the explicit permission of all chiefs of cantonal police forces.

detected and arrested[184]. These data allow an identification of ordinary cantonal policemen and criminal detectives in charge of the detection of the more severe crimes[185]. Since the size of the police force is determined by cantonal policies, a considerable cross-sectional variation is expected[186]. The institutional measure of direct democracy is constructed based on STUTZER (1999). This measure is a composite index that ranges from 1 to 6, with 1 indicating the lowest degree of direct democracy. It encompasses all direct legislative institutions, such as the statutory initiative, the constitutional initiative, and the fiscal and statutory referendum.

All sociodemographic and expenditure information was obtained from the BFS. All monetary variables are deflated to the base year 1980 using the GDP deflator series provided by the State Secretariat for Economic Affairs (SECO). The remaining econogeographic, economic, and fiscal determinants are calculated using data from the BFS, the Swiss Federal Tax Administration (FTA), the Swiss Household Panel (SHP), the Federal Office of Spatial Development, and yearly issues of *L'Année politique Suisse*[187]. A more detailed description of all variables and their construction and descriptive statistics can be found in the Appendix (tables A.1, A.4, and A.11).

5 Empirical Results

5.1. Structural Equation I: Determinants of Police Expenditure

The first computational step – using a times-series cross-sectional panel of 26 cantons from 1986 to 2001 – analyzes the impact of direct democracy on combined cantonal and local spending on security issues (per capita), and particularly on the size of cantonal police forces

[184] See e.g. CORNWELL and TRUMBULL (1994) for the use of police per capita as a measure of the county's ability to detect crime.

[185] As in the U.S., there exists a communal and a state (i.e. cantonal) police force. According to MARTIN JÄGGI, chief commander of the cantonal police of *Solothurn* and President of the Swiss Conference of Cantonal Chiefs of Police, local police forces deal mainly with traffic issues and their contribution to crime detection is negligible.

[186] In Switzerland, not all cantons seem to report clear-up rates in their cantonal criminal statistics, and data on conviction rates have not been found. Data on the length of sentence are also unavailable.

[187] *L' Année politique Suisse*, HANS HIRTER et al., Institut für Politikwissenschaft an der Universität Bern (ed.), Bern: Institut für Politikwissenschaft, 1986–2001. For the years 1986 to 2001, some of the fiscal and political variables were obtained by courtesy of my colleagues G. KIRCHGÄSSNER, L.P. FELD, and Ch.A. SCHALTEGGER.

(per capita)[188]. In this specification, the fiscal decentralization variable is instrumented with cantonal fixed effects[189], and standard errors are robust and also adjusted for serial autocorrelation. Table 2 reports the results for different budgetary components of security expenditure. The first column represents the police expenditure estimation, while columns (2) to (4) represent more specifically the regressions on the size of the cantonal police force and its subcategories 'ordinary policemen' and 'criminal detectives'. Also included for reasons of comparison are the results for the aggregate total security expenditure and the judicial system and national defense expenditure, which are reported in columns (5) to (8) in the Appendix (table A.2).

The variable of interest, the degree of cantonal direct democracy, appears to exert an expenditure-lowering influence on police expenditure in the subfederal budgets in Switzerland (at the 1% level). In addition, with respect to the actual size of the cantonal police force, a considerable decreasing impact can be observed for all three police force size variables (at the 1% and 5% levels, respectively). Since work on crime prevention and detection is divided among the several types of policemen, which depends on the type of crime, a deterrence-lowering impact of direct democracy on all the different crime categories analyzed can be conjectured. Additionally, it is observed that in more direct democratic cantons fewer financial means are available for total security spending and judicial system expenditure. As regards national defense expenditure only, no such impact is detected, which might be the result of regulations at the federal level. A high (centered) R2 indicates a good fit of the model to the time-series cross-sectional data. For corroborating estimation results with outliers excluded, see table A.5 of the Appendix.

These empirical results are perfectly in line with the conjecture made in *Testable Hypothesis 1*. By systematically underestimating the average person's (or their own) probability of becoming victimized, median voters induce a police expenditure budget constraint in cantons whose institutional setup allows the voter to exert a stronger influence on the political decision-making process. The fewer means then available for crime prevention and crime detection translate directly into a lower number of police per capita. In more representative democracies, however, in which trained bureaucrats and specialized politicians, who make

[188] This time span is chosen to make it comparable with crime equation (1). Estimation of the widest time span possible (1984–2001) does not alter the results significantly. The identical results found here also hold for cantonal expenses only.

[189] Because of the way this variable is constructed, the dependent variable might influence its size.

more accurate predictions about crime rates, exert more influence on budgets, more money is spent on fighting crime[190].

Table 2: Security Expenditure 1986 – 2001

	(1) Police expenditure	(2) Police force	(3) Ordinary policemen	(4) Criminal detectives
Direct democracy	-0.115**	-0.093**	-0.068*	-0.210**
	(4.03)	(3.64)	(2.56)	(4.65)
Fiscal decentralization	-0.514**	-0.23	-0.23	-0.29
	(2.70)	(1.26)	(1.39)	(0.74)
Tax competition	-0.272**	-0.059	-0.102	0.113
	(3.55)	(0.88)	(1.49)	(0.87)
Federal transfers	0.145*	0.172**	0.115*	0.397**
	(2.26)	(2.87)	(2.03)	(3.31)
Constitutional constraint	-0.024(*)	-0.001	0.012	-0.033
	(1.72)	(0.03)	(0.66)	(1.10)
Conservative ideology	0.02	0.017	-0.123	0.549*
	(0.14)	(0.13)	(0.95)	(2.22)
Size of coalition	-0.013	-0.052*	-0.054*	-0.065
	(0.42)	(2.00)	(2.22)	(1.12)
Romance canton	-0.26*	-0.086	-0.063	-0.173
	(2.37)	(0.94)	(0.71)	(0.99)
Urbanization	0.00	-0.002(*)	-0.004**	0.004(*)
	(0.20)	(1.69)	(2.86)	(1.75)
National income	0.719**	0.579**	0.518**	0.860**
	(3.99)	(3.87)	(3.55)	(3.22)
Cantonal population	0.019	0.002	0.009	-0.012
	(0.57)	(0.08)	(0.30)	(0.28)
Residents 0 – 14	-0.044(*)	-0.081**	-0.102**	0.004
	(1.96)	(4.09)	(4.79)	(0.14)
Residents 15 – 24	0.049*	0.045*	0.024	0.099*
	(2.07)	(2.03)	(1.02)	(2.54)
Residents over 60	0.037**	0.047**	0.040**	0.068**
	(2.74)	(3.77)	(2.89)	(3.46)
Constant	-5.185**	-8.577**	-7.617**	-14.474**
	(3.50)	(6.83)	(5.63)	(7.33)
Observations	416	416	416	416
Centered R^2	0.82	0.83	0.82	0.51
Jarque-Bera χ-value	40.05***	1.69	1.57	210.50***

02SLS with heteroscedasticity and autocorrelation consistent standard errors for two lags. Absolute values of t-statistics are in parentheses. Endogenous variable: fiscal decentralization. (*) indicates significance at the 10%, * at the 5%, and ** at the 1% level. Year fixed effects are included but not reported.

[190] These empirical findings are in line with all the U.S American and Swiss efficiency literature on the impact of institutions of direct legislation on budgets (see section 2.1). Most particularly, SCHALTEGGER (2001), using a different specification, also finds a limiting institutional impact on subfederal total security spending for Swiss data between 1980 and 1989. The innovative contribution of this section lies in the theoretical argument.

Regarding the additional fiscal, political, and sociodemographic predictors in the model, an expenditure-lowering impact of fiscal decentralization and the fiscal break is observed on police expenditure but not on the number of policemen. Tax competition causes a decline only in the expenditure for police but not in the number of policemen. Financial lumpsum transfers from the federal government lead to both significantly higher police expenditure and a larger cantonal police force. Interestingly, a more conservative ideology of cantonal executives is associated with more criminal detectives per capita but is insignificant with respect to police expenditure and the remaining measures of cantonal police force. In contrast to the usual expectation, the size of the coalition, which measures government fragmentation, appears to cause a lower number of policemen, particularly ordinary policemen. In cantons in which the main culture is French or Italian, significantly less money is spent on police, leaving the number of policemen seemingly unaffected[191]. On average, a higher degree of urbanization is associated (albeit weakly) with a lower number of ordinary policemen and total policemen but also (weakly) with more criminal detectives. Moreover, wealthier cantons (in terms of national income) show a higher spending on police that appears to translate into significantly higher numbers of all types of policemen. Further, there is no significant linkage between the size of the cantonal population and police-related variables. Regarding the demographic structure of the canton, a significant negative relation is observed between almost all police variables (except criminal detectives) and the share of young residents below the age of 14[192], but a positive one (except ordinary policemen) is found for the share of residents between 15 and 24 years, criminally the most active period in life (e.g. COHEN and LAND 1987). Finally, in this model, senior residents appear to either demand or require more of the good 'public safety': their share is significantly positively associated with higher levels of spending on police issues and also with higher numbers of all types of police.

As a robustness test, this same analysis is performed with a specification that includes the natural log of the theft and assault crime rate lagged by two periods, because both police expenditure and the size of the police force might be determined also by cantonal crime rates. As expected, both crime rates affect the independent police-related variables strongly and positively. Most important, the police expenditure and police force lowering impact of direct democracy remains unchanged (all at the 1% level). The estimation outcomes for this

[191] A possible interpretation is that the (unobservable) technical equipment might be negatively affected.

[192] In Switzerland, the age of criminal responsibility is 7, but until the age of 16 no prison terms are applicable, but only measures according to art 82 – 88 *StGB*.

specification can be found in the Appendix (table A.3). In addition, estimation results of the original model with outliers excluded are displayed in table A.5 of the Appendix.

5.2 Structural Equation II: Determinants of Crime Rates

In the second step, structural equation (1) is estimated to determine the impact of direct democracy on cantonal crime rates based on an economic model of crime but taking into account that it also reduces the means available for cantonal police forces. Therefore, the (endogenous) police force variables are explicitly included in the crime regression, which makes the direct impact of direct democracy on various crime rates observable. Since these deterrents are subject to a potential simultaneity[193], they are instrumented with the exogenous variables in equation (2). This relation is analyzed for various categories of property crime, including 'white collar' property crime like fraud and defalcation, and violent crime[194]. All regressions include dichotomous determinants that control for heterogeneity in recording behavior by cantonal police forces, and the standard errors are adjusted for serial autocorrelation.

Property Crime

Table 3 displays the estimation results for property crime, particularly burglary, pickpocketing, auto theft, robbery, fraud, and defalcation. Regarding the variable of interest, in this model, the direct impact of direct democracy is observed to significantly decrease property crimes of burglary and auto theft and robbery (all at the 5% level or above). It does not, however, affect pickpocketing, fraud, and defalcation rates.

These important empirical results are perfectly in line with *Testable Hypothesis 2*. Auto theft and burglary offense rates are the two highest in the list of crime rates (see table 1). Based on this finding, it can be concluded that due to the high frequency of these property crimes, the median voter overestimates their true probabilities of occurrence (in comparison to violent crimes) and demands that more of the available resources be allocated to the prevention of auto theft and burglary. Rather puzzling, however, is the result for robbery because, based on its quite rare occurrence, there should be no overestimation of its relative probability.

[193] The significant impact of theft and assault crime rates on these deterrents in the empirical analysis in section 5.1 corroborates this conjecture.

[194] Robbery is counted among property crimes because its primary goal is to generate illegal income.

Table 3: Property Crime 1986 – 2001 Structural Form

	(1) Burglary	(2) Pickpocketing	(3) Auto theft	(4) Robbery	(5) Fraud	(6) Defalcation
Criminal detectives	-0.574*	0.164	-0.840*	-0.734*	0.735	1.141*
	(2.58)	(0.45)	(2.36)	(2.20)	(1.34)	(2.01)
Ordinary policemen	0.429**	1.121**	0.710**	1.219**	1.255**	-0.04
	(2.88)	(4.33)	(2.83)	(4.41)	(3.80)	(0.11)
Severity 139	-0.005	0.006	0.001			
	(1.62)	(1.20)	(0.17)			
Severity 140				0.000		
				(0.103)		
Severity 146					0.000	
					(0.019)	
Severity 138						-0.005
						(1.57)
Direct democracy	-0.204**	0.137	-0.214*	-0.186*	-0.008	-0.001
	(2.87)	(1.30)	(2.06)	(2.16)	(0.05)	(0.01)
Welfare transfers	0.283**	0.302*	0.304*	0.376**	-0.126	-0.349(*)
	(3.05)	(2.03)	(1.99)	(2.75)	(0.70)	(1.96)
Income inequality	0.14	0.169	0.028	0.004	-0.197	-0.263
	(1.33)	(1.00)	(0.20)	(0.02)	(0.92)	(1.08)
National income	0.545*	-0.857*	0.409	-0.216	-1.628**	-0.82
	(2.48)	(2.00)	(0.89)	(0.59)	(3.82)	(1.51)
Unemployment rate	0.032	0.043	-0.004	0.006	0.006	-0.039
	(0.97)	(0.61)	(0.07)	(0.11)	(0.07)	(0.46)
Closeness to border	-0.255**	0.494**	0.093	0.166(*)	0.019	-0.365*
	(3.19)	(4.72)	(0.80)	(1.70)	(0.11)	(2.23)
Interaction b. cantons	0.001	0.012(*)	0.011	0.035**	-0.009	-0.004
	(0.11)	(1.79)	(1.57)	(4.92)	(0.85)	(0.36)
Urbanization	0.010**	0.012**	0.006*	0.012**	0.010*	0.011*
	(4.41)	(3.73)	(1.97)	(3.99)	(2.37)	(2.43)
Cantonal population	0.121	0.22	-0.206	-0.401**	0.217	0.128
	(1.23)	(1.61)	(1.43)	(2.82)	(1.02)	(0.57)
Residents 15 – 24	0.139**	0.178**	0.174**	0.05	0.074	-0.136(*)
	(3.73)	(2.67)	(2.92)	(0.80)	(1.01)	(1.72)
Residents 25 – 29	0.045	-0.045	0.077	0.138	0.068	0.097
	(0.67)	(0.51)	(0.95)	(1.52)	(0.52)	(0.68)
Romance canton	-0.033	-0.307	-1.050**	-0.362*	-1.146**	0.102
	(0.27)	(1.44)	(5.74)	(2.08)	(3.75)	(0.29)
Constant	-0.179	7.210**	3.983*	8.456**	19.308**	13.934**
	(0.11)	(3.14)	(2.05)	(4.05)	(4.89)	(2.95)
Observations	416	416	416	416	416	416
Centered R^2	0.71	0.65	0.30	0.53	0.41	0.05
Jarque-Bera χ-value	0.20	48.20***	800.60***	9.57**	36.29***	75.30***

2SLS with heteroscedasticity and autocorrelation consistent standard errors for two lags. Absolute values of t-statistics are in parentheses. Endogenous variables: Criminal detectives and ordinary policemen. (*) indicates significance at the 10%, * at the 5%, and ** at the 1% level. Year fixed effects and measures of recording behavior are included but not reported.

Nevertheless, given that, according to availability heuristic frequencies, more severe and damaging crimes are more persistent in people's memories than simple crimes like pickpocketing, the optimism bias for robbery rates might not be as great as for simple theft, thus leading to higher efforts in more direct democratic cantons to fight this crime.

In line with the predictions, the deterrent 'criminal detectives' exerts a crime-dampening influence on burglary, auto theft, and robbery. No such effect, however, is prominent for the remaining property crimes, and for defalcation even a crime enhancing effect is revealed. The number of ordinary policemen also appears to increase property crime for the first five categories, but exerts an insignificant impact on defalcation.[195] In all six property crime categories, the coefficients of the severity of punishment are always rendered insignificant, which might be caused by the unsolved potential endogeneity[196]. For burglary and defalcation, however, the coefficients show at least the expected sign.

The economic determinants, however, reveal a different pattern of behavior. Welfare transfers are associated with higher levels of burglary, pickpocketing, auto theft and robbery, but not fraud and defalcation, which it decreases. This offense-raising result contradicts expectations[197]. The coefficient of income inequality is always rendered insignificant, but with the predicted sign in the first four crime categories. National income, the measure of a society's wealth, leads – as anticipated – to significantly higher burglary rates but, contrary to expectations, to lower pickpocketing and fraud rates. In this specification, the unemployment rate appears to be insignificant for all six types of property crime.

The impact of the econogeographic determinant of proximity of a canton to important border crossings, which measures the exposure of a canton to an influx of foreigners, unexpectedly decreases crimes of burglary and defalcation, but increases crimes of pickpocketing and robbery. This result indicates that although cases of serial burglaries committed by foreign gangs of thieves have occasionally been prominent in the Swiss media, in the synthetic panel

[195] It is generally known that a higher frequency of patrolling policemen and higher efforts to detect crime lead to higher reported and recorded crime rates in criminal statistics, even when the true crime rate might not have changed over time.

[196] Given that higher crime rates increase the severity of punishment, which, again, lowers the crime rate, the 'simultaneous' effect will be zero (insignificant).

[197] One possible explanation is that this variable serves as a proxy for the share of poor, uneducated persons in a canton who might be more prone to commit a crime. The tax data (until 1998) on which the inequality variable is based excludes persons with nontaxable income. Alternatively, welfare transfers might directly increase the propensity to commit crimes through the creation of disincentives for regular work, as shown in an economic model by IMROHOROGLU et al. (2000).

from 1986 to 2001, it is pickpocketing and robbery rather than burglary that is induced from abroad. Moreover, a more intense interaction between cantonal populations within Switzerland is associated with higher cantonal pickpocketing and robbery rates, which supports the prediction about this determinant.

As regards the sociodemographic determinants, as expected, a higher degree of urbanization is associated with higher levels of property crime for all categories. Robbery rates decrease in the size of the canton measured by cantonal population. However, no such effect can be detected for the remaining property crimes. Also in line with the original BECKER model, a higher share of young persons between 15 and 24 causes higher rates of the 'blue collar' property crimes of burglary, pickpocketing and auto theft, but weakly lower rates of the 'white collar' offenses of defalcation[198]. The coefficients of the share of persons aged between 25 and 29 years appear to be insignificant for all property crimes, which is also in line with the traditional economic theory of crime.

Finally, Romance culture, as measured by the main cantonal language being Italian or French, leads to significantly less auto theft, robbery, and fraud. As people's reporting behavior may be shaped by the dominant cantonal culture, it is possible that this variable captures such differences. In general, the centered R^2 of between 0.30 and 0.71 indicates quite a good fit of the model for all crimes reported in columns (1) through (5). For defalcation, however, no such statement can be made (based on a centered R^2 of 0.052). Table A.6 of the Appendix reports estimation results with outliers excluded[199].

Violent Crime

Table 4 reports the estimation results for crimes against persons and against morality and decency. Columns (1) to (3) indicate the hate crimes of homicide and assault and a combined category for both crimes, while columns (4) and (5) show the results for two types of sexual offenses: rape and crimes against sexual integrity. The combined category (3) is constructed to take into account the fact that in some cantons attempted homicides (category 1) might be counted as completed severe assaults (category 2)[200].

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[198] The exclusion from the labor market of persons of this age might serve as one explanation.

[199] As table A.6 shows, a significant crime lowering impact of direct democracy on defalcation rates can be observed (at the 5% level).

[200] Because CHERRY and LIST (2002) and WITHERS (1988) show that aggregation of different crimes can lead to a bias in the coefficients, the estimation results for this artificial category must be interpreted with caution.

The variable of interest, direct democracy, appears to have an offense-raising impact on assault (2) and the combined assault and homicide rate (at the 1% and 5% levels, respectively). The remaining types of offenses are not affected by direct democracy. Only in the other sex crime regression is a t-value close to the 10% level of significance observed, again with a positive sign of the coefficient[201].

This empirical result is also in line with *Testable Hypothesis 2*. Obviously, in direct democratic cantons, available means for public safety are allocated in such a way that higher rates of assault and (possibly) other sex crimes are admitted. Thus, the median voter appears to particularly disfavor the prevention of assault, possibly because of its low frequency, which induces the systematic underestimation of its occurrence (compared to property crime). Unexpectedly, homicide and rape rates do not appear to 'dis-benefit' from reallocation of given means in direct democratic cantons despite their low probability of occurrence. One possible explanation is that they are both rather severe crimes always reported by the media and therefore the severity might exert an increasing influence on the subjective assessment of their probability of occurrence, somewhat offsetting the decrease of the low-frequency impact (see section 2.4).

Among the deterrents in general, the police force variables do not appear to influence the offense rate in the predicted manner: For all types of hate and sex crimes, the number of ordinary policemen per capita is associated with higher crime levels. In addition, criminal detectives affect hate and assault crime rates positively, but a weak decreasing tendency emerges for their impact on sex crimes[202]. As with property crime, the coefficients of the severity of punishment variables for homicide, assault, rape, and sexual offenses are insignificant in all estimations, which might be the result of its endogeneity.

[201] Excluding outliers, however, the coefficients for both sex crime categories become positive, but are not significant (see table A.7 of the Appendix).

[202] See also footnote 195.

Table 4: Violent Crime and Sex Crime 1986–2001 Structural Form

	(1) Homicide	(2) Assault	(3) Hate Crime	(4) Rape	(5) Sex Crime
Criminal detectives	-0.037	0.516**	0.518**	-0.54	-0.610(*)
	(0.21)	(2.72)	(2.70)	(1.59)	(1.66)
Ordinary policemen	0.528**	0.753**	0.745***	1.581**	1.433**
	(2.73)	(3.96)	(3.71)	(4.23)	(3.51)
Severity 111	0.001				
	(0.97)				
Severity 122 123		0.003			
		(1.41)			
Severity			0.001		
			(0.70)		
Severity 187				-0.002	0.001
				(1.20)	(0.33)
Direct democracy	-0.042	0.191**	0.160*	-0.014	0.145
	(0.63)	(2.59)	(2.31)	(0.18)	(1.48)
Welfare transfers	0.023	0.361**	0.323***	0.075	0.102
	(0.21)	(3.38)	(3.33)	(0.75)	(0.84)
Income inequality	0.088	-0.108	-0.077	0.058	-0.180
	(0.69)	(0.91)	(0.69)	(0.39)	(1.14)
National income	-0.298	-0.738**	-0.772**	-0.578(*)	-0.559(*)
	(1.09)	(2.66)	(2.90)	(1.81)	(1.87)
Unemployment rate	-0.016	0.024	0.012	0.027	-0.064
	(0.40)	(0.53)	(0.29)	(0.58)	(1.19)
Closeness to border	0.197(*)	-0.346**	-0.299***	0.136	-0.006
	(1.96)	(3.68)	(3.55)	(1.27)	(0.06)
Interaction between cantons	0.009	-0.002	-0.002	0.030**	0.016*
	(1.56)	(0.48)	(0.27)	(3.91)	(2.07)
Urbanization	0.000	-0.006*	-0.005(*)	0.003	0.012**
	(0.01)	(2.12)	(1.93)	(1.17)	(3.82)
Cantonal population	-0.195(*)	0.047	0.034	-0.536**	0.006
	(1.68)	(0.47)	(0.23)	(4.12)	(0.04)
Residents 15 – 24	-0.021	-0.100*	-0.075	0.169*	0.226*
	(0.45)	(1.97)	(1.56)	(2.15)	(2.44)
Residents 25 – 29	0.043	0.025	0.005	0.163*	-0.034
	(0.61)	(0.24)	(0.05)	(1.97)	(0.35)
Romance canton	-0.330(*)	0.025	0.001	-0.412*	-0.088
	(1.90)	(0.15)	(0.01)	(2.05)	(0.42)
Constant	7.243**	16.007**	16.040***	11.727**	5.371*
	(4.16)	(8.32)	(7.81)	(5.74)	(2.45)
Observations	384	416	416	416	416
Centered R2	0.29	0.57	0.57	0.33	0.33
Jarque-Bera χ-value	2.41	44.84***	19.56***	19.06***	33.99***

See table 3..

As regards the economic determinants, an offense rate enhancing influence of welfare payments on assault and hate crime is observed. Again, this effect is contrary to prediction by the economic model of crime[203]. Also contrary to expectations, the coefficients of income inequality are rendered insignificant, while national income exerts a crime decreasing impact on assault, hate crime, and all sexual offenses. Thus, again contrary to BECKER's model of crime, the unemployment rate does not appear to be of any importance for the type of offenses under investigation. Proximity to traffic-rich border crossings to neighboring countries appears to be weakly associated with higher homicide rates (at the 10% level) but significantly lower assault and hate crime rates (at the1% level, respectively). Interestingly, more interaction between the cantonal populations does not affect any crime against person, but does exert a strong raising influence on both sexual offense rates (at the 1% and 5% level, respectively). These last finding is in line with the prediction that higher mobility increases the opportunities for committing a crime, as well as the anonymity that in turn lowers detection probability.

Regarding the sociodemographic variables, a higher degree of urbanization is associated with lower assault and hate crime rates[204] but higher sex crime rates, this last being perfectly in line with the economic theory of crime. Cantonal population exerts a crime-decreasing influence on homicide and rape. The share of residents 15 to 24-year-olds is associated with fewer assault offences but a more frequent occurrence of sexual offenses, which latter finding is supported by economic theory[205]. As conjectured, the share of 25 to 29-year-old residents appears only to positively affect rape rates.

As regards Romance culture, a negative linkage between Latin language and homicide and rape rates is observed. Again, unobserved reporting or recording behavior could be captured by this variable. With respect to the goodness of fit of the model, on average lower levels of the centered R^2 are observed for these crimes than for property crimes. This finding is not surprising as the economic rational choice model of crime was developed to explain property crime rather than violent crimes, whose emotional aspects it may not capture. Again, estimation results with outliers excluded can be found in table A.7 of the Appendix.

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[203] For an explanation, see footnote 197.

[204] It is possible that in urbanized areas, reporting rates of less severe cases of assaults are lower.

[205] The crime-dampening impact on assault might be explained by a lower propensity of this age group to report such cases to the police.

In sum, in the structural model, direct democracy appears to exert a reducing impact on property crime and an increasing impact on offenses against person, particularly assault. Obviously, even taking into account that protection of person against assaults will be neglected, the median voter prefers that relatively more of the available means be devoted to fighting the property crimes of auto theft, burglary, and robbery. This finding corroborates *Testable Hypothesis 2a* that postulates a reallocation of available means for fighting more frequently occurring property crime at the expense of less frequent crimes against person.

5.3 Reduced Form: Determinants of Crime

In the reduced form, presented in equation (3), the endogenous variables for police per capita are replaced with their exogenous determinants; hence, this specification also includes fisco-political variables that determine government expenditure. This makes it possible to observe the combined direct and indirect impact of direct democracy on public safety as well as the total effect of those variables which form part of both the auxiliary and the main regression in the structural version of the model.

The reduced form of the model is, again, estimated for all the types of property, hate, and sex crimes under investigation. The replacement of the endogenous factors with the exogenous determinates of equation (1) seemingly increases the goodness of fit of the reduced form of the model: The centered R^2s have increased substantially in comparison with the structural form, particularly for the two sexual offenses. Again, estimation results with outliers excluded are given in tables A.8 and A.9 of the Appendix.

Property Crime

Table 5 reports the estimation results for property crime. Direct democracy exerts a crime-reducing impact on burglary, auto theft, and fraud rates. No significant influence can be observed, however, on pickpocketing, robbery, and defalcation rates. Comparing these estimation results for the total institutional effect in the reduced from of the model with those of the structural model reveals that the offense rate lowering impact is present in both forms for burglary and auto theft. With respect to robbery, the significance of direct democracy vanishes in the reduced form, probably because its lowering direct effect is offset by the reporting rate increasing impact of ordinary policemen. As regards fraud rates, the opposite is

observed: the direct impact of political institutions appears to be irrelevant, whereas the combined influence significantly lowers the offense rate[206].

For severity of punishment, a crime lowering impact on defalcation is observed that is perfectly in line with the prediction of the economic model of crime but that contradicts the previous results from the structural form of the model; on auto theft, even a (weakly) positive impact can be observed. As regards the remaining economic, sociodemographic, and econogeographic determinants, the discussion will focus on a description of the differences in impact between the reduced and the structural form. In contrast to the structural form, welfare transfers lose their (albeit) weak lowering impact on defalcation rates, but remain significant for the first four categories of property crime. Interestingly, income inequality becomes a statistically decisive determinant of pickpocketing, indicating a (weakly) positive relation between higher income inequality and simple theft rates, contrasting the results of the structural form. The direction of this influence is in line with the economic model of crime. In addition, the coefficients of cantonal levels of national income are rendered insignificant for burglary and pickpocketing, but its fraud lowering influence pertains. The unemployment rate is now strongly positively associated with burglary rates, which supports BECKER's theory.

As regards the remaining econogeographic and sociodemographic determinants, in the reduced form, the closeness of a canton to important border crossings is no longer an important determinant of pickpocketing, but for robbery rates a change in direction of impact is observable, while the crime reducing impact on defalcation and burglary stays the same. Further, compared to the structural form results, an interaction between cantonal populations appears now to be significantly crime reducing for burglary rates but strongly crime increasing for fraud and defalcation, in addition to the already observed crime raising influence on pickpocketing and robbery. Moreover, in contrast to the previous results obtained from the structural form estimation, the degree of urbanization in a canton no longer raises burglary and fraud rates, but weakly decreases auto theft rates. The coefficients of cantonal population now become significant for burglary, fraud, and defalcation, but insignificant for robbery. Most interesting, in this specification, the share of young persons between 15 and 24 years is now decisive for robbery rates, but has no impact on burglary and defalcation; in contrast, its positive influence on pickpocketing and auto theft remains the

[206] Excluding outliers, also a dampening impact of direct legislation on defalcation rates is detected, whereas the one on auto theft rates looses its significance. See also section 5.4.

same. It is for robbery and fraud offences that a significant crime increasing effect of a higher share of residents aged 25 to 29 years is observed, in contrast to the finding for the reduced form. These offense raising impacts are in line with the underlying economic theory. In terms of the cultural determinant, the (reported) crime rate lowering influence is now also prominent for the occurrence of burglary (besides the already observed effect on auto theft and fraud), but this impact has lost its significance for robbery. In general, contradictions of the economic model of crime may result from the fact that in the reduced form the combined impact for many covariates is estimated, whereas in the previously estimated structural form only the direct crime-related influence is calculated.

In the reduced form, the exogenous variables of the expenditure equation are also included in the crime equation. These additional variables are sociodemographic, fiscal or political. Since these determinants usually do not form part of the traditional economic model of crime, no prediction has been made because their influence occurs indirectly through their impact on police expenditure. In this reduced specification, the share of people over 60 exerts a crime dampening impact on burglary and auto theft rates but a crime increasing impact on pickpocketing and robbery rates[207]. The share of persons below the age of 14 appears to lead to less burglary, auto theft, or fraud[208]. Among the fiscal variables, fiscal decentralization is associated with less robbery but is not significant with respect to any other property crime. Further, as shown in columns (1), (2), (5) and (6), tax competition appears to lead to lower mostly non-violent property crime rates. Finally, a stricter debt break causes a more frequent occurrence of pickpocketing, fraud, and defalcation. With respect to the political variables, more conservative governments seem to favor the prevention of burglary and (weakly) of fraud, whereas a larger coalition size is weakly associated with higher burglary rates.

[207] Based on the social capital theory (PUTNAM 2000), the retired persons positively contribute which should lower crime rates (through, e.g. neighborhood watching). On the other hand, they might also form an important part of crime demand.

[208] In explanation, it should be noted that children in this age group are simply not physically, mentally, or socially capable of committing some types of property crime.

Table 5: Property Crime 1986–2001 Reduced Form

	(1) Burglary	(2) Pickpocketing	(3) Auto Theft	(4) Robbery	(5) Fraud	(6) Defalcation
Severity 139, 140, 146, 138	-0.002	0.004	0.005(*)	0.000	0.002	-0.006*
	(0.94)	(0.85)	(1.74)	(0.46)	(0.73)	(2.52)
Direct democracy	-0.158**	0.088	-0.142*	-0.074	-0.204*	-0.167
	(3.67)	(1.04)	(2.26)	(1.21)	(2.06)	(1.55)
Welfare transfers	0.142*	0.479**	0.176(*)	0.323**	0.141	0.034
	(2.15)	(3.67)	(1.80)	(3.04)	(0.93)	(0.20)
Income inequality	0.079	0.309(*)	0.043	0.154	-0.276	-0.212
	(0.93)	(1.80)	(0.36)	(1.21)	(1.40)	(1.00)
National income	-0.114	0.116	0.249	0.479	-1.233*	-0.146
	(0.49)	(0.25)	(0.70)	(1.44)	(2.27)	(0.25)
Unemployment rate	0.063*	0.022	0.013	-0.057	0.103	-0.013
	(1.97)	(0.34)	(0.30)	(1.23)	(1.40)	(0.16)
Closeness to border	-0.241**	0.167	0.09	-0.161(*)	-0.221	-0.456**
	(3.75)	(1.33)	(0.93)	(1.79)	(1.41)	(2.86)
Interaction b. cantons	-0.006*	0.016**	0.004	0.018**	0.016*	0.024**
	(1.99)	(2.92)	(0.86)	(4.46)	(2.34)	(3.39)
Urbanization	0.004	0.015**	-0.006(*)	0.015**	0.005	0.015*
	(1.64)	(3.05)	(1.68)	(4.15)	(0.80)	(2.31)
Cantonal population	0.200**	0.066	-0.136	-0.065	-0.379*	-0.551**
	(3.21)	(0.54)	(1.46)	(0.74)	(2.54)	(3.55)
Residents 0 – 14	-0.078**	-0.026	-0.200**	0.063	-0.222**	-0.078
	(2.73)	(0.47)	(4.24)	(1.55)	(3.39)	(1.09)
Residents 15 – 24	0.048	0.256**	0.112*	0.142*	-0.025	-0.089
	(1.23)	(3.31)	(2.06)	(2.53)	(0.27)	(0.91)
Residents 25 – 29	0.036	0.097	0.054	0.226**	0.247*	0.205
	(0.70)	(0.94)	(0.68)	(3.03)	(2.06)	(1.57)
Residents over 60	-0.051*	0.125**	-0.092*	0.165**	-0.010	-0.001
	(2.24)	(2.77)	(2.33)	(5.10)	(0.19)	(0.01)
Fiscal decentralization	0.338	-0.382	-0.333	-0.856**	0.755	0.549
	(1.56)	(0.89)	(1.03)	(2.73)	(1.52)	(1.01)
Tax competition	-0.199(*)	-0.565*	0.071	0.004	-1.018**	-0.760**
	(1.71)	(2.47)	(0.42)	(0.03)	(3.82)	(2.60)
Federal transfers	0.045	-0.204	-0.450**	-0.405**	0.413(*)	0.02
	(0.45)	(1.04)	(3.14)	(2.80)	(1.76)	(0.08)
Constitutional constraint	0.02	0.205**	0.02	0.038	0.305**	0.287**
	(0.61)	(3.26)	(0.37)	(0.84)	(4.10)	(3.59)
Conservative ideology	-0.656**	-0.129	-0.262	-0.049	-0.732(*)	0.232
	(3.50)	(0.35)	(0.99)	(0.18)	(1.70)	(0.50)
Size of coalition	0.069(*)	-0.06	-0.096	0.015	-0.125	-0.099
	(1.68)	(0.74)	(1.64)	(0.25)	(1.27)	(0.97)
Romance canton	-0.371*	-0.334	-1.043**	0.014	-1.934**	-0.172
	(2.35)	(1.07)	(4.68)	(0.06)	(5.34)	(0.44)
Constant	5.808**	-6.225	15.559**	-4.642	13.256*	10.499(*)
	(2.68)	(1.46)	(4.47)	(1.51)	(2.55)	(1.95)
Observations	416	416	416	416	416	416
Centered R2	0.81	0.65	0.59	0.67	0.47	0.35
Jarque-Bera χ-value	2.90	50.62***	1424.00***	46.71***	31.38***	56.78***

OLS with heteroscedasticity and autocorrelation consistent standard errors for two lags. Absolute values of t-statistics are in parentheses. (*) indicates significance at the 10%, * at the 5%, and ** at the 1% level. Year fixed effects and measures of recording behavior are included but not reported.

Violent Crime

Table 6 displays the results for violent crime when the total - i.e. the combined direct and indirect - impact of direct democracy is analyzed in the reduced form of the model of crime. Again, the estimation outcomes for violent and sex crimes are reported in columns (1) to (5).

Of highest interest are the estimation results for the degree of direct democracy in Swiss cantons. As already observed in the structural equation specification, a considerable offense increasing influence can be observed on the assault and hate crime rates. However, a strong increasing effect on sex crime stands out that contradicts the estimation results reported for the structural form. Apparently, the combined direct and indirect influence of direct democracy leads to more sex crime, while the direct impact of direct democracy is insignificant. Since the number of ordinary policemen significantly increases (reported) crime in the structural form[209], however, the combined crime raising influence of direct legislation on sex crime is an interesting result that might be explained using the findings for the reduced form.

As already observed for the reduced form, the severity of the punishment variable is not significant for any of the violent and sexual offenses. As regards the remaining determinants of the traditional model of crime, the analysis will again focus on the differences between the outcomes for the reduced and the structural from of the model. Welfare payment exhibits the identical pattern in both forms, as does income inequality and the unemployment rate. The level of national income in cantons, however, exerts different influences in each form: in the reduced form, it is found to be positively associated with hate crime and sex crime, whereas in the structural form it appears to significantly decrease almost all offense rates, except homicide rates.

Among the econogeographic variables, the influence from abroad reveals a significantly lowering impact on rates of sexual offenses, an outcome not obtained in the structural form, while for the remaining crime types the observed effect stays unchanged. On the other hand, the coefficient of interaction of cantonal populations is rendered positive and strongly significant for all violent crimes, as previously conjectured, and also for the occurrence of rape. Only the latter impact coincides with the one already observed in the structural model;

[209] For sex crimes, the positive impact of ordinary policemen appears to offset the quantitatively smaller crime reducing influence of criminal detectives found in the reduced form of the model.

for other sex crimes, however, where previously an enhancing effect was revealed, no significant influence is found. With respect to the sociodemographic determinants traditionally included in an estimation of the economic model of crime, the degree of urbanization is seen to be nondecisive for any type of offense, which contradicts the results obtained for the structural form for assault, hate crime and sex crime. Moreover, in the reduced form, less violent crimes and rape occur in bigger cantons, but more sexual offenses belonging to the category 'other sex crime'. In contrast, the coefficient of cantonal population was almost always rendered insignificant in the structural form, except a lowering impact in the rape and homicide regressions. Further, in the reduced form, the impact of the cantonal share of residents 15 – 24 differs only with respect to other sex crimes, where it no longer appears important. Again, a higher ratio of persons between the age of 25 and 29 are positively associated with rape. In addition to the crime lowering effect on homicide and rape already observed in the structural form, cantonal culture also appears to be an important, but offense rate raising determinant for assault, hate crimes, and other sex crimes in the reduced form.

Again, estimation of a reduced form model reveals the impact of otherwise only indirectly effective determinants of crime; i.e. the exogenous factors exclusively employed in structural equation (1). The share of residents below the age of 14 does not appear at all decisive for any crime, whereas the share of persons over 60 exerts a crime raising influence on all offenses against person or morality and decency. Among the fiscal and political variables, the degree of cantonal fiscal decentralization and the amount of federal transfers are observed to be irrelevant; however, tax competition is found to be crime lowering for homicide, assault, hate crimes, and rape but weakly rate increasing for sex crimes. Moreover, the fiscal constraint appears to lead strongly to more assault, hate crimes, and sexual offenses, but no such effect is detected for homicide. In addition, a more conservative government has a significantly stronger propensity to fight the commitment of homicide and rape in comparison to the remaining crime types on which no decisive influence is revealed. Finally, the more fragmented the government, the more it is associated with lower levels of other sex crimes.

Table 6: Violent Crime and Sex Crime 1986–2001 Reduced Form

	(1) Homicide	(2) Assault	(3) Hate Crime	(4) Rape	(5) Sex Crime
Severity 111, 122 - 123. combined severity, 187	0.000	0.001	0.001	-0.001	0.001
	(0.27)	(0.39)	(0.72)	(1.07)	(0.84)
Direct democracy	-0.057	0.237**	0.195***	-0.002	0.212**
	(0.92)	(3.82)	(3.44)	(0.04)	(3.19)
Welfare transfers	-0.017	0.499**	0.444***	0.052	0.103
	(0.17)	(5.45)	(5.32)	(0.61)	(1.01)
Income inequality	0.155	0.109	0.113	0.159	-0.102
	(1.23)	(0.94)	(1.07)	(1.35)	(0.74)
National income	0.104	0.509	0.517(*)	0.066	0.756*
	(0.33)	(1.64)	(1.81)	(0.22)	(2.08)
Unemployment rate	-0.04	0.034	0.020	-0.021	-0.074
	(0.89)	(0.78)	(0.50)	(0.49)	(1.49)
Closeness to border	0.235*	-0.535**	-0.468***	-0.201*	-0.294**
	(2.27)	(5.81)	(5.58)	(2.40)	(2.88)
Interaction b. cantons	0.015**	0.013**	0.015***	0.015**	0.000
	(2.81)	(3.02)	(3.73)	(3.84)	(0.06)
Urbanization	0.000	0.005	0.005	0.005	0.006
	(0.12)	(1.38)	(1.55)	(1.60)	(1.40)
Cantonal population	-0.296**	-0.437**	-0.478***	-0.272**	0.356**
	(2.60)	(4.41)	(5.07)	(3.39)	(3.66)
Residents 0 – 14	0.039	-0.003	-0.005	0.035	-0.008
	(0.80)	(0.09)	(0.15)	(0.91)	(0.18)
Residents 15 – 24	0.026	-0.094(*)	-0.067	0.168**	0.089
	(0.42)	(1.75)	(1.35)	(2.92)	(1.29)
Residents 25 – 29	0.125	0.115	0.109	0.255**	-0.027
	(1.57)	(1.55)	(1.60)	(3.60)	(0.32)
Residents over 60	0.076*	0.086**	0.094***	0.175**	0.124**
	(2.05)	(2.80)	(3.28)	(6.09)	(3.57)
Fiscal decentralization	0.008	-0.262	-0.200	0.402	0.371
	(0.03)	(0.88)	(0.74)	(1.40)	(1.08)
Tax competition	-0.558**	-0.513**	-0.487**	-0.306*	0.319(*)
	(2.88)	(2.95)	(3.01)	(2.00)	(1.73)
Federal transfers	-0.13	-0.143	-0.123	0.009	-0.103
	(0.97)	(1.04)	(1.00)	(0.07)	(0.66)
Constitutional constraint	0.034	0.355**	0.321***	0.082*	0.110*
	(0.74)	(7.24)	(7.18)	(1.98)	(2.19)
Conservative ideology	-0.625*	0.197	0.182	-0.569*	-0.202
	(2.46)	(0.79)	(0.80)	(2.30)	(0.68)
Size of coalition	-0.019	-0.078	0.182	0.028	-0.264**
	(0.34)	(1.38)	(1.58)	(0.53)	(4.13)
Romance canton	-0.511*	0.410(*)	0.379(*)	-0.411(*)	0.504(*)
	(2.28)	(1.90)	(1.92)	(1.87)	(1.91)
Constant	0.283	5.700(*)	5.728*	-5.156(*)	-5.909(*)
	(0.08)	(1.90)	(2.11)	(1.88)	(1.78)
Observations	384	416	416	416	416
Centered R^2	0.34	0.65	0.57	0.50	0.49
Jarque-Bera χ-value	4.02	40.20***	17.39***	13.12**	39.00***

See table 5.

5.4 Comparison of the Results for the Reduced and Structural Forms

Whereas the reduced form reveals the combined direct and indirect effect of direct democracy on crime (B), the structural form explicitly makes the direct impact observable (A). A comparison of the results for both forms makes it possible to draw conclusions about the unobserved indirect effect (the difference between B and A, B - A)[210]. As shown in section 5.2, the effect of direct democracy significantly dampens police expenditure and reduces police force size; therefore, a crime increasing unobserved indirect impact would be expected. On the other hand, gains in executive efficiency[211] in the provision of the public good 'public safety' are detectable that could (over)compensate for the fewer resources available. Based on the hypotheses of bounded rationality, some debiasing of both police administrators and regular policemen can be conjectured that might explain which crimes efficiency gains at the cantonal police level might be observed for and which not. Table 7 briefly summarizes the different influences of direct legislation detected for both forms of the model:

The conjectured crime increasing indirect influence of direct legislation is strongly corroborated for robbery, sex crime and weakly corroborated for assault and hate crimes[212]. In these cases, fewer available financial means for crime prevention and crime protection do lead to higher crime rates through the subfederal budgetary channel.

In the case of fraud – and possibly also for burglary and auto theft[213] – the indirect impact through the budget even appears crime reducing, a finding that contradicts expectations. Even though fewer financial means are made available for police issues at the cantonal and communal level, these particular crimes are negatively affected. This is possibly a case of executive super-efficiency in which fewer resources are allocated in such a way that the public good gains in quality, at least with respect to these crimes. In other words, even though

[210] This discussion is based on the estimation results of the previous regressions in tables 3 to 6, not the ones listed in the Appendix. The conclusions, however, do not change considerably when the regression results with outliers excluded are taken into account. Affected is the institutional impact on defalcation and auto theft, which in turn, changes the conclusion on efficiency only in the last case. Furthermore, for any crime the differences (B – A) might always be insignificant, which would be interpreted as hinting at efficiency gains.

[211] Efficiency in the production of public safety at the level of the police forces, i.e. executive efficiency, should not be confused with the question of an efficient allocation of goods and resources at the societal level.

[212] The indirect effect might also be insignificant.

[213] Additionally, in the case of an insignificant indirect impact on burglary and auto theft, the result would also be interpreted as supporting efficiency gains (see next paragraph).

the policemen in direct democratic cantons are fewer, they carry out their work in a more efficient way than their peers in more representative democratic cantons[214].

Table 7: Influence of Direct Democracy in the Reduced and Structural Forms

Crime type	Direct Effect (Structural Form)	Combined Direct and Indirect Effect (Reduced Form)	Indirect Effect
	A	B	B – A
Burglary	Negative	Negative	Negative or insignificant
Pickpocketing	Insignificant	Insignificant	Insignificant
Auto theft	Negative	Negative	Negative or insignificant
Robbery	Negative	Insignificant	Positive
Fraud	Insignificant	Negative	Negative
Defalcation	Insignificant	Insignificant	Insignificant
Homicide	Insignificant	Insignificant	Insignificant
Assault	Positive	Positive	Positive or insignificant
Hate crime	Positive	Positive	Positive or insignificant
Rape	Insignificant	Insignificant	Insignificant
Sex crime	Insignificant	Positive	Positive

An unambiguously insignificant indirect influence of direct democracy through the budget is detected not only on the very severe crimes against persons – i.e. homicide and rape – but also on defalcation. It must be concluded that this indirect impact through reduction in police expenditure does not exert a decisive influence, which also runs contrary to expectations.

[214] A decrease in reporting rates for these crimes is also possible, probably because there are fewer policemen to report to. This interpretation, however, is not very trustworthy as, in order to obtain their insurance benefits, victims have a strong incentive to report burglaries and auto thefts particularly.

Therefore, it is again suggested that, in more direct democratic cantons, police forces must have increased their efficiency in fighting these crime to offset their lower number.

In sum, comparing the results of the reduced form of the model with those of the structural form reveals efficiency gains at the police level for some crimes in more direct democratic cantons. Obviously, the disguised indirect impact of direct legislation is not fully reflected by the estimates obtained for the police force deterrents in the structural form of the economic model of crime[215]. Efficiency gains in execution can be observed for severe crimes against person (rape, homicide) and the either infrequent or not so severe property crimes of fraud, defalcation, and pickpocketing[216]. Most interesting, this group is comprised of those types of offenses for which no direct institutional crime dampening impact can be observed in the structural form; i.e. those crimes that would potentially suffer from a reallocation of available means as preferred by the median voter. The interpretation made here is that practitioners and police administrators try to compensate for the potentially crime increasing redistributive effect of direct democracy on some crimes through efficiency gains achieved in their production process. Such unambiguously compensating efficiency gains are, however, not observable for assault and sex crime. It may be that, since assault and other sex crimes are on average much less severe than homicide or rape, not attempting to achieve efficiency gains in their prevention is rational. Overall, *Hypothesis 4*, which proposes that bureaucrats and practitioners are less subject to optimism bias and the availability heuristic than the common voter, appears to be corroborated by the observed executive efficiency gains for less frequent crimes.

6 Conclusion

This chapter has presented an analysis of the relation between a boundedly rational median voter and the allocation of means for crime prevention and crime detection through estimation of an economic model of crime in the tradition of BECKER (1968) and EHRLICH (1973). Given the newest empirical findings in the field of economic psychology, the median voter is conjectured to suffer from an optimism bias when predicting the probability of personally

[215] In the structural form, other aspects like the equipment of the police forces are omitted.

[216] The number of frauds is one candidate for being seriously affected by cantonal heterogeneity in data collection. Recording the number of victims or even sent letters instead of reported cases increases the number over 100 times. Additionally, the variation appears quite strong. Finally, fraud rates have risen recently through the use of the Internet. These facts might explain why no crime rate decreasing impact of direct democracy is found in the structural model of crime.

becoming a victim of crime. Moreover, a severe misprediction is also hypothesized regarding the average probability in society, meaning that fewer financial means should be spent on police issues in more direct democratic cantons than otherwise. When such bias is combined with availability heuristic, the boundedly rational median voter should mispredict the likelihood of occurrence to a greater extent for less frequent crimes than for more frequent felonies. Given the actual crime rates in Switzerland, a preference for fighting property crimes compared to preventing violent crimes is predicted.

Using a synthetic panel of Swiss cantonal crime rates from 1986 to 2001 and a set of determinants, a structural and a reduced form of the model is estimated. In general, the empirical evidence corroborates the hypotheses. First, estimation of the structural form reveals an expenditure dampening and police force reducing impact of direct democracy. In addition, once the availability of financial means is controlled for, direct democracy is found to significantly decrease burglary, auto theft, and robbery rates but also to increase assault rates. Estimating the reduced form and comparing it with the results of the structural form reveals executive efficiency gains in the provision of public safety for most crimes that do not benefit from a reallocation of given means induced by the median voter. This finding is in line with the hypothesis that the optimism bias and availability heuristic are less prominent for practitioners and police administrators.

In sum, it is again shown that in direct democracies there exists an allocation of goods and resources that appears to be consistent with the median voter's preferences, independent of whether the median voter preference is objectively and societally the best allocation or not. Hence, this result is in line with the traditional public choice literature on the impact of institutions on political decision making and political outcomes. In this study, it is also shown that administrators, who in the case of crime prevention can be considered less subject to human misjudgments, prefer an allocation closer to the objectively and economically best allocation. In particular, the additional resources devoted to meet the median voter's preferences appear to be obtained through executive efficiency gains in fighting those crimes that are less important to the median voter.

For future research, it would appear valuable to combine approaches from the fields of public choice and economic psychology and relax the perfect rationality assumption to make economic models more suitable for explaining real world events.

7 Appendix

Table A.1: Crime Categories as Codified in the Swiss Criminal Code (StGB)

Crime in estimation output	Articles in StGB	Description of article
Homicide	111 – 116	111: killing 112: murder 113: manslaughter 114: euthanasia 115: assisting suicide 116: infanticide
Assault	122 – 123	122: mayhem 123: malicious injury
Defalcation	138	138: defalcation
Pickpocketing	139	139: theft
Burglary	139	139: theft
Auto theft	139	139: theft
Robbery	140	140: robbery
Fraud	146	146: fraud
Rape	190	190: rape of a female person
Sex crime	187 – 189, 191 – 194, 198	187: sexual abuse of children 188: sexual abuse of minor adult dependants 189: sexual assault 191: rape of defenseless persons 192: sexual abuse of dependants in institutions of correction 193: sexual abuse of dependants 194: exhibitionism 198: sexual harassment

Table A.2: Security Expenditure 1986 – 2001

	(5) Security	(6) National defense	(7) Judicial system
Direct democracy	-0.068**	0.033	-0.061*
	(3.09)	(0.96)	(2.30)
Fiscal decentralization	-0.407**	0.449*	-0.361(*)
	(2.70)	(2.54)	(1.82)
Tax competition	-0.194**	0.069	-0.207**
	(3.47)	(0.59)	(2.90)
Federal transfers	0.050	0.042	0.011
	(1.11)	(0.54)	(0.19)
Constitutional constraint	0.006	0.018	0.039**
	(0.57)	(0.81)	(2.65)
Conservative ideology	0.003	0.139	-0.028
	(0.03)	(1.10)	(0.24)
Size of coalition	0.004	-0.046	0.045(*)
	(0.18)	(1.14)	(1.68)
Romance canton	-0.152*	0.215	-0.193*
	(2.10)	(1.48)	(2.19)
Urbanization	-0.001	-0.010**	0.004*
	(0.51)	(3.61)	(2.16)
National income	0.697**	0.539**	0.453**
	(5.61)	(2.63)	(2.79)
Cantonal population	0.039(*)	-0.034	0.088**
	(1.69)	(1.06)	(3.13)
Residents 0 – 14	-0.011	-0.016	-0.012
	(0.72)	(0.62)	(0.74)
Residents 15 – 24	0.010	-0.129**	0.049*
	(0.49)	(3.78)	(2.03)
Residents over 60	0.034**	-0.031(*)	0.045**
	(3.22)	(1.91)	(3.96)
Constant	-4.176**	-0.568	-6.192**
	(3.89)	(0.35)	(5.34)
Observations	416	416	416
Centered R^2	0.77	0.68	0.80
Jarque-Bera χ-value	0.80	210.00***	7.03*

2SLS with heteroscedasticity and autocorrelation consistent standard errors for two lags. Absolute values of t-statistics are in parentheses. Endogenous variable: fiscal decentralization. (*) indicates significance at the 10%, * at the 5%, and ** at the 1% level. Year fixed effects are included but not reported.

Table A.3: Security Expenditure 1986 – 2001 with Lagged Crime Rates

	(1) Police expenditure	(2) Police force	(3) Ordinary police	(4) Criminal detectives	(5) Security	(6) Defense	(7) Judicial system
Direct democracy	-0.125**	-0.102**	-0.075**	-0.228**	-0.078**	0.027	-0.075**
	(5.61)	(5.02)	(3.34)	(5.40)	(5.69)	(0.81)	(4.34)
Assault crime rate	0.085**	0.064**	0.038*	0.144**	0.080**	0.064(*)	0.109**
	(3.77)	(3.69)	(2.13)	(3.22)	(5.66)	(1.89)	(5.40)
Theft crime rate	0.160**	0.215**	0.226**	0.141	0.150**	-0.05	0.156**
	(3.50)	(5.58)	(4.60)	(1.56)	(4.04)	(0.50)	(2.67)
Fiscal decentralization	-0.328(*)	-0.056	-0.094	-0.017	-0.232*	0.499**	-0.145
	(1.96)	(0.36)	(0.65)	(0.05)	(2.48)	(2.61)	(1.16)
Tax competition	-0.226**	-0.004	-0.047	0.163	-0.151**	0.065	-0.159**
	(3.86)	(0.07)	(0.80)	(1.35)	(3.87)	(0.54)	(2.86)
Federal transfers	0.172**	0.199**	0.138**	0.434**	0.073(*)	0.05	0.043
	(2.79)	(3.71)	(2.77)	(3.67)	(1.96)	(0.66)	(0.84)
Constitutional constraint	-0.025*	-0.005	0.006	-0.031	0.004	0.023	0.039**
	(1.99)	(0.34)	(0.41)	(1.08)	(0.62)	(1.01)	(3.49)
Conservative ideology	0.028	0.039	-0.093	0.541*	0.011	0.115	-0.026
	(0.25)	(0.37)	(0.87)	(2.41)	(0.16)	(0.93)	(0.33)
Size of coalition	0.01	-0.029	-0.035(*)	-0.033	0.026(*)	-0.038	0.072**
	(0.42)	(1.41)	(1.75)	(0.60)	(1.82)	(0.99)	(3.63)
Romance canton	-0.302**	-0.127	-0.093	-0.245	-0.196**	0.189	-0.251**
	(3.22)	(1.59)	(1.20)	(1.43)	(3.76)	(1.37)	(3.70)
Urbanization	0.000	-0.002(*)	-0.004**	0.005*	0.000	-0.009**	0.004**
	(0.06)	(1.75)	(3.02)	(2.29)	(0.16)	(3.27)	(3.05)
National income	0.483**	0.339**	0.311*	0.543*	0.474**	0.470*	0.181
	(3.31)	(2.88)	(2.56)	(2.09)	(5.91)	(2.24)	(1.39)
Cantonal population	-0.032	-0.056*	-0.046(*)	-0.071(*)	-0.009	-0.035	0.033(*)
	(1.24)	(2.54)	(1.95)	(1.77)	(0.62)	(1.08)	(1.66)
Residents 0 – 14	-0.033(*)	-0.066**	-0.084**	0.011	-0.001	-0.022	-0.003
	(1.71)	(3.83)	(4.23)	(0.42)	(0.10)	(0.82)	(0.20)
Residents 15 – 24	0.037(*)	0.029	0.006	0.090**	-0.001	-0.124**	0.038(*)
	(1.96)	(1.58)	(0.29)	(2.71)	(0.07)	(3.32)	(1.93)
Residents over 60	0.024(*)	0.035**	0.031*	0.050**	0.022*	-0.037*	0.029**
	(1.96)	(2.90)	(2.15)	(2.87)	(2.55)	(2.23)	(3.12)
Constant	-5.303**	-8.963**	-8.156**	-14.258**	-4.284**	-0.139	-6.193**
	(4.20)	(8.20)	(6.35)	(8.72)	(5.61)	(0.09)	(7.40)
Observations	416	416	416	416	416	416	416
Centered R^2	0.82	0.83	0.82	0.51	0.86	0.68	0.87

2SLS with heteroscedasticity and autocorrelation consistent standard errors for two lags. Absolute values of t-statistics are in parentheses. Endogenous variable: fiscal decentralization. (*) indicates significance at the 10%, * at the 5%, and ** at the 1% level. Year fixed effects are included but not reported. Crime rates are lagged by two periods.

Table A.4: Summary Statistics of Explanatory Variables

Variable	Obs.	Mean	Std. Dev.	Min	Max
Direct democracy	416	4.261	1.211	1.583	5.833
Real welfare transfers (no log)	416	0.255	0.147	0.0488	0.866
Income inequality	416	1.214	0.253	0.746	1.898
National income (no log)	416	27.009	6.049	18.608	53.997
Unemployment rate	416	2.243	1.907	0	7.800
Closeness to border	416	0.269	0.444	0	1
Interaction b. cantons	416	16.505	17.157	0.901	81.450
Urbanization	416	59.142	24.041	14.063	100
Population (no log)	416	266505.9	276036.3	13137	1228628
Residents 0 – 14	416	18.291	2.210	11.268	23.172
Residents 15 – 24	416	13.127	1.738	10.073	17.874
Residents 25 – 29	416	7.533	0.861	5.161	9.495
Residents over 60	416	18.366	2.888	11.584	27.073
Fiscal decentralization	416	0.394	0.176	0.004	0.978
Tax competition (no log)	416	0.233	0.078	0.096	0.419
Federal transfers (no log)	416	302.022	115.906	150.452	914.377
Constitutional constraint	416	0.339	0.863	0	3
Conservative ideology	416	-0.098	0.184	-0.6	0.4
Size of coalition	416	3.310	0.871	1	5
Romance canton	416	0.269	0.444	0	1
Severity art. 111	384[217]	73.309	30.23066	0	100
Severity art. 122/123	416	17.444	13.91414	0	100
Severity art. 187	416	19.040	18.46005	0	100
Severity art. 139	416	25.514	9.470814	0	66.7
Severity art. 140	416	42.141	24.84547	0	100
Severity art. 146	416	23.711	14.37432	0	100
Severity art. 138	416	19.065	17.89831	0	100
Criminal detectives per capita (no log)	416	0.0003689	0.0001769	0.0000484	0.0010211
Ordinary policemen per capita (no log)	416	0.0015139	0.0005898	0.0005924	0.0039329

[217] In two cantons, the severity of punishment variable is missing. For this reason, in the homicide regressions only 384 observations are used.

Table A.5: Security Expenditure 1986 – 2001, Outliers Excluded

	(1) Police expenditure	(4) Criminal detectives	(6) National Defense	(7) Judicial system
Direct democracy	-0.117**	-0.203**	0.044(*)	-0.057*
	(4.25)	(5.53)	(1.88)	(2.22)
Fiscal decentralization	-0.437*	-0.512	0.650**	-0.445*
	(2.48)	(1.56)	(4.65)	(2.36)
Tax competition	-0.269**	0.122	0.178**	-0.218**
	(3.81)	(1.18)	(2.79)	(3.21)
Federal transfers	0.140*	0.332**	0.036	-0.009
	(2.26)	(3.31)	(0.73)	(0.15)
Constitutional constraint	-0.022	-0.018	-0.011	0.041**
	(1.64)	(0.79)	(0.89)	(2.94)
Conservative ideology	0.042	0.437*	0.082	-0.015
	(0.31)	(2.45)	(0.84)	(0.14)
Size of coalition	-0.013	-0.152**	-0.084**	0.041
	(0.47)	(3.77)	(3.10)	(1.62)
Romance canton	-0.244*	-0.049	0.374**	-0.175*
	(2.53)	(0.38)	(4.28)	(2.14)
Urbanization	0.000	0.001	-0.015**	0.003
	(0.21)	(0.62)	(10.39)	(1.50)
National income	0.732**	0.939**	0.700**	0.560**
	(4.46)	(4.32)	(4.63)	(3.52)
Cantonal population	0.014	-0.025	0.003	0.088**
	(0.44)	(0.71)	(0.14)	(3.29)
Residents 0 – 14	-0.046*	-0.006	-0.052**	-0.009
	(2.10)	(0.22)	(3.14)	(0.55)
Residents 15 – 24	0.047*	0.055	-0.143**	0.035
	(2.08)	(1.46)	(6.94)	(1.47)
Residents over 60	0.037**	0.055**	-0.034**	0.046**
	(2.88)	(2.99)	(3.10)	(4.29)
Constant	-5.095**	-12.740**	-0.072	-6.252**
	(3.53)	(7.34)	(0.06)	(5.61)
Observations	412	396	389	410
Centered R^2	0.78	0.60	0.83	0.82
Jarque-Bera χ-value	3.72	3.04	3.95	4.17

2SLS with heteroscedasticity and autocorrelation consistent standard errors for two lags. Absolute values of t-statistics are in parentheses. Endogenous variable: fiscal decentralization. (*) indicates significance at the 10%, * at the 5%, and ** at the 1% level. Year fixed effects are included but not reported.

Excluded outliers are: in regression (1): Uri (1989, 1990), and Glarus (1991) and Tessin (1988) in regression (4): Zürich (1986 - 1996), Uri (1986 - 1990), Schwyz (2001), Graubünden (1986), Thurgau (2000) and Jura (1998); in regression (6): Luzern (1999), Uri (2000), Obwalden (1988), Nidwalden (1986 - 1988, 1999, 2000, 2001), Glarus (1994), Freiburg (1989, 1990), Solothurn (1992 - 1995), Appenzell Innerrhoden (1994, 1996, 1997), Tessin (1986), Jura (1986, 1987, 1990, 1998 - 2001), and, finally, in regression (7): Glarus (1988 - 1992) and Appenzell Innerrhoden (2001).

Table A.6: Property Crime, Outliers Excluded, Structural Form

	(2) Pickpocketing	(3) Auto Theft	(4) Robbery	(5) Fraud	(6) Defalcation
Criminal Detectives	0.129	-0.372*	-0.898**	0.620	1.521**
	(0.42)	(2.02)	(2.67)	(1.16)	(2.96)
Ordinary Policemen	1.052**	0.427**	1.274**	1.322**	0.384
	(4.78)	(3.05)	(4.77)	(4.09)	(1.23)
Severity 139	0.01	0.003			
	(1.40)	(1.00)			
Severity 140			0.000		
			(0.02)		
Severity 146				-.001	
				(0.35)	
Severity 138					-0.003
					(1.13)
Direct democracy	0.053	-0.107(*)	-0.218*	-0.014	-0.218*
	(0.57)	(1.78)	(2.50)	(0.10)	(2.02)
Welfare transfers	0.261*	0.155	0.457**	-0.077	-0.279(*)
	(2.12)	(1.50)	(3.30)	(0.44)	(1.89)
Income inequality	0.105	0.046	-0.026	-0.16	-0.004
	(0.64)	(0.51)	(0.17)	(0.76)	(0.02)
National income	-0.455	0.547**	-0.051	-1.537**	-1.526**
	(1.11)	(2.73)	(0.14)	(3.67)	(3.15)
Unemployment rate	0.026	0.033	-0.013	0.02	-0.059
	(0.44)	(0.90)	(0.26)	(0.27)	(0.78)
Closeness to border	0.408**	0.053	0.147	0.031	-0.399**
	(4.49)	(0.69)	(1.50)	(0.18)	(3.26)
Interaction between cantons	0.010(*)	0.002	0.038**	-0.007	-0.016
	(1.71)	(0.44)	(4.98)	(0.70)	(1.55)
Urbanization	0.013**	0.005*	0.013**	0.009*	0.012**
	(4.24)	(2.40)	(4.30)	(2.23)	(3.25)
Cantonal population	0.218(*)	-0.034	-0.482**	0.2	0.088
	(1.84)	(0.42)	(3.25)	(0.95)	(0.46)
Residents 15 – 24	0.191**	0.151**	0.087	0.095	-0.096
	(3.25)	(3.93)	(1.38)	(1.33)	(1.40)
Residents 25 – 29	-0.021	0.155**	0.171(*)	0.061	-0.016)
	(0.27)	(3.07)	(1.88)	(0.47)	(0.15)
Romance canton	-0.395*	-0.937**	-0.360*	-1.164**	-0.471(*)
	(2.25)	(8.19)	(2.02)	(3.81)	(1.87)
Constant	5.269*	2.496*	7.327**	18.601**	23.583**
	(2.52)	(2.26)	(3.48)	(4.82)	(6.61)
Observations	404	400	411	415	394
Centered R^2	0.73	0.64	0.53	0.44	0.29
Jarque-Bera χ-value	3.75	3.90	3.94	2.85	3.98

2SLS with heteroscedasticity and autocorrelation consistent standard errors for two lags. Absolute values of t-statistics are in parentheses. Endogenous variables: Criminal detectives and ordinary policemen. (*) indicates significance at the 10%, * at the 5%, and ** at the 1% level. Year fixed effects and measures of recording behavior are included but not reported. Excluded outliers are displayed in table A.10.

Table A.7: Violent Crime and Sex Crime, Outliers Excluded, Structural Form

	(2) Assault	(3) Hate Crime	(4) Rape	(5) Sex Crime
Criminal Detectives	0.432*	0.430*	-0.487(*)	-0.899*
	(2.58)	(2.57)	(1.95)	(2.31)
Ordinary Policemen	0.849**	0.834**	1.275**	1.715**
	(5.60)	(4.96)	(4.70)	(4.13)
Severity 122 123	0.001			
	(0.69)			
Severity		0.000		
		(0.32)		
Severity 187			-0.002	0.001
			(1.40)	(0.37)
Direct democracy	0.137*	0.111(*)	0.052	0.086
	(2.42)	(1.93)	(0.78)	(0.90)
Welfare transfers	0.360**	0.336**	0.102	0.111
	(3.78)	(3.95)	(1.15)	(0.87)
Income inequality	-0.129	-0.121	-0.058	-0.122
	(1.11)	(1.12)	(0.52)	(0.81)
National income	-0.691**	-0.739**	-0.111	-0.643*
	(2.77)	(3.14)	(0.48)	(2.05)
Unemployment rate	-0.025	-0.022	0.005	-0.071
	(0.67)	(0.63)	(0.11)	(1.33)
Closeness to border	-0.353**	-0.306**	0.204*	0.053
	(4.24)	(4.00)	(2.47)	(0.47)
Interaction between cantons	-0.004	-0.003	0.028**	0.022**
	(0.96)	(0.47)	(4.48)	(2.67)
Urbanization	-0.003	-0.003	0.004*	0.012**
	(1.45)	(1.50)	(2.01)	(4.07)
Cantonal population	0.081	0.045	-0.480**	-0.139
	(0.89)	(0.36)	(4.32)	(0.93)
Residents 15 – 24	-0.06	-0.045	0.157*	0.269**
	(1.51)	(1.16)	(2.53)	(2.91)
Residents 25 – 29	-0.098	-0.088	0.09	0.025
	(1.37)	(1.30)	(1.32)	(0.27)
Romance canton	0.037	-0.013	-0.154	-0.189
	(0.24)	(0.09)	(0.97)	(0.94)
Constant	16.014**	16.281**	8.500**	5.915**
	(9.60)	(8.90)	(5.39)	(2.72)
Observations	406	406	401	407
Centered R^2	0.62	0.63	0.44	0.32
Jarque-Bera χ-value	2.21	2.56	2.78	3.87

See table A.6.

Table A.8: Property Crime, Outliers Excluded, Reduced Form

	(2) Pickpocketing	(3) Auto Theft	(4) Robbery	(5) Fraud	(6) Defalcation
Severity 139, 140, 146	0.004	0.001	0.002(*)	0.001	-0.006**
	(1.09)	(0.45)	(1.70)	(0.38)	(3.40)
Direct democracy	0.042	-0.017	-0.035	-0.192*	-0.300**
	(0.61)	(0.47)	(0.66)	(1.97)	(3.28)
Welfare transfers	0.441**	0.130*	0.385**	0.16	0.171
	(4.01)	(2.30)	(4.56)	(1.07)	(1.25)
Income inequality	0.275(*)	0.059	0.108	-0.228	-0.023
	(1.92)	(0.89)	(0.98)	(1.18)	(0.13)
National income	0.161	0.197	0.174	-1.176*	-0.396
	(0.42)	(0.99)	(0.60)	(2.21)	(0.83)
Unemployment rate	-0.018	0.091**	-0.036	0.113	-0.03
	(0.34)	(3.57)	(0.92)	(1.56)	(0.46)
Closeness to border	0.088	0.017	-0.127	-0.212	-0.575**
	(0.84)	(0.33)	(1.62)	(1.38)	(4.32)
interaction b. cantons	0.012**	0.000	0.015**	0.015*	0.016**
	(2.70)	(0.17)	(4.49)	(2.31)	(2.76)
Urbanization	0.016**	-0.005*	0.019**	0.004	0.021**
	(3.89)	(2.36)	(5.86)	(0.70)	(4.08)
Cantonal population	0.141	0.05	-0.024	-0.344*	-0.553**
	(1.38)	(0.95)	(0.32)	(2.34)	(4.32)
Residents 0 – 14	0.012	-0.118**	0.116**	-0.207**	-0.033
	(0.25)	(4.52)	(3.29)	(3.22)	(0.56)
Residents 15 – 24	0.241**	0.005	0.118*	-0.005	-0.017
	(3.77)	(0.17)	(2.38)	(0.05)	(0.21)
Residents 25 – 29	0.164(*)	0.111*	0.237**	0.241*	0.238*
	(1.93)	(2.56)	(3.67)	(2.05)	(2.21)
Residents over 60	0.141**	-0.091**	0.180**	0.002	0.087(*)
	(3.75)	(4.18)	(6.51)	(0.03)	(1.82)
Fiscal decentralization	-0.468	-0.562**	-0.682*	0.792	0.701
	(1.32)	(3.15)	(2.51)	(1.62)	(1.56)
Tax competition	-0.656**	-0.219*	0.079	-1.084**	-0.617*
	(3.46)	(2.31)	(0.55)	(4.12)	(2.55)
Federal transfers	-0.200	0.075	-0.175	0.414(*)	0.301
	(1.23)	(0.89)	(1.38)	(1.81)	(1.47)
Constitutional constraint	0.169**	0.028	0.033	0.304**	0.347**
	(3.25)	(0.98)	(0.84)	(4.15)	(5.23)
Conservative ideology	-0.387	-0.471**	-0.01	-0.843*	0.646(*)
	(1.26)	(3.24)	(0.04)	(1.99)	(1.67)
Size of coalition	-0.027	-0.132**	0.093(*)	-0.105	-0.087
	(0.41)	(4.03)	(1.79)	(1.09)	(1.03)
Romance canton	-0.435(*)	-1.077**	-0.022	-1.982**	-0.326
	(1.65)	(8.80)	(0.11)	(5.57)	(1.00)
Constant	-8.449*	9.602**	-6.770*	11.710*	6.422
	(2.38)	(4.94)	(2.56)	(2.29)	(1.44)
Observations	401	389	404	415	402
Centered R^2	0.73	0.79	0.73	0.49	0.49
Jarque-Bera χ-value	4.36	3.39	4.05	2.71	0.55

OLS with heteroscedasticity and autocorrelation consistent standard errors for two lags. Absolute values of t-statistics are in parentheses. (*) indicates significance at the 10%, * at the 5%, and ** at the 1% level. Year fixed effects and measures of recording behavior are included but not reported. Excluded outliers are displayed in table A.10.

Table A.9: Violent and Hate Crime, Outliers Excluded, Reduced Form

	(2) Assault	(3) Hate Crime	(4) Rape	(5) Sex Crime
Severity 122-123, (111, 122, 123 combined), 187	0.001	0.001	-0.001	0.001
	(0.38)	(0.69)	(0.91)	(0.94)
Direct democracy	0.193**	0.169**	-0.001	0.193**
	(3.33)	(3.18)	(0.02)	(3.17)
Welfare transfers	0.489**	0.467**	0.049	0.058
	(5.84)	(6.10)	(0.61)	(0.62)
Income inequality	0.086	0.098	0.147	-0.168
	(0.81)	(1.02)	(1.33)	(1.34)
National income	0.444	0.409	0.131	0.886**
	(1.52)	(1.55)	(0.46)	(2.64)
Unemployment rate	0.017	0.005	-0.03	-0.042
	(0.42)	(0.14)	(0.77)	(0.92)
Closeness to border	-0.517**	-0.465**	-0.192*	-0.286**
	(6.11)	(6.08)	(2.43)	(3.03)
Interaction between cantons	0.013**	0.013**	0.013**	(0.000)
	(3.12)	(3.56)	(3.59)	(0.00)
Urbanization	0.004	0.004	0.006(*)	0.004
	(1.25)	(1.18)	(1.95)	(1.03)
Cantonal population	-0.439**	-0.434**	-0.236**	0.323**
	(4.86)	(5.03)	(3.14)	(3.62)
Residents 0 - 14	-0.03	-0.035	0.048	-0.015
	(0.81)	(1.04)	(1.31)	(0.34)
Residents 15 - 24	-0.081	-0.066	0.176**	0.049
	(1.64)	(1.45)	(3.26)	(0.77)
Residents 25 - 29	0.058	0.045	0.224**	0.016
	(0.82)	(0.71)	(3.36)	(0.20)
Residents over 60	0.073*	0.071**	0.174**	0.122**
	(2.57)	(2.68)	(6.46)	(3.80)
Fiscal decentralization	-0.104	-0.164	0.264	0.568(*)
	(0.38)	(0.67)	(0.97)	(1.80)
Tax competition	-0.497**	-0.478**	-0.282*	0.428*
	(3.13)	(3.23)	(1.97)	(2.53)
Federal transfers	(0.156	-0.139	-0.003	-0.132
	(1.23)	(1.21)	(0.03)	(0.92)
Constitutional constraint	0.334**	0.304**	0.062	0.116*
	(7.44)	(7.45)	(1.59)	(2.51)
Conservative ideology	0.139	0.096	-0.581*	-0.115
	(0.61)	(0.46)	(2.50)	(0.43)
Size of coalition	-0.088(*)	-0.094*	0.044	-0.288**
	(1.68)	(1.99)	(0.87)	(4.91)
Romance canton	0.363(*)	0.322(*)	-0.399(*)	0.549*
	(1.81)	(1.77)	(1.93)	(2.28)
Constant	7.349**	7.494**	-5.820*	-4.951
	(2.67)	(3.01)	(2.26)	(1.63)
Observations	410	409	413	408
Centered R^2	0.68	0.70	0.52	0.54
Jarque-Bera χ-value	3.17	0.19	3.49	0.42

See table A.8.

Table A.10: Deleted Observations as Outliers in the Crime Regressions

	Structural Form	Reduced Form
Assault	Obwalden (1988), Nidwalden (1996), Zug (1993), Freiburg (1993), Basel-Land (1992), Schaffhausen (1990), Tessin (1986, 1987, 1988), Jura (1986).	Schwyz (1987), Nidwalden (1996), Appenzell Innerrhoden (1999), Tessin (1986, 1988), Jura (1986).
Hate Crime	Uri (1990), Obwalden (1988), Nidwalden (1996), Zug (1993), Freiburg (1993), Schaffhausen (1990), Tessin (1986 – 1988), Jura (1986).	Schwyz (1987), Obwalden (1988), Nidwalden (1996), Aargau (2001), Tessin (1986, 1988), Jura (1986).
Rape	Nidwalden (2001), Zug (1993, 1995, 1998, 1999) Basel-Land (1990, 1993 - 1996), Aargau (2001), Thurgau (2001), Waadt (2000), and Jura (1992).	Zug (1999), Basel-Land (1995), Thurgau (2001).
Sex Crime	Uri (1994), Nidwalden (1994), Appenzell Innerrhoden (1990, 1992, 1999), Aargau (1992), Thurgau (2000), Tessin (1989), Jura (1998).	Nidwalden (1994), Freiburg (1993), Appenzell Ausserrhoden (1990), Appenzell Innerrhoden (1993, 1996, 1999), Aargau (1992, 1997).
Pickpocketing	Schwyz (1990, 1992, 2000), Obwalden (1996), Zug (1995 – 1998), Solothurn (1995), Basel-Land (2000), Waadt (1989, 1990).	Schwyz (1989, 1990, 1992, 2000), Obwalden (1996), Nidwalden (1990), Zug (1997), Solothurn (1995), Basel-Land (2000), Schaffhausen (1989), Aargau (1988), Waadt (1986, 1989, 1990, 1998).
Auto Theft	Uri (1988), Obwalden (1998), Zug (1996 – 1999), Wallis (1995 – 2001).	Schwyz (1986, 1987, 1999 – 2001), Obwalden (1998), Glarus (1986 – 1988), Zug (1996, 1997, 1998, 1999), Solothurn (1990, 1991, 1994), Appenzell Ausserrhoden (1993, 1996, 1997), Wallis (1995 – 2001).
Robbery	Uri (1998), Schwyz (1986), Zug (1995, 1996), Graubünden (1986).	Uri (1992), Schwyz (1986, 1989), Obwalden (1996), Glarus (1987, 1997), Zug (1988, 2000), Appenzell Ausserrhoden (1989, 1990, 1995, 2000).
Fraud	Nidwalden (1996).	Nidwalden (1996).
Defalcation	Uri (1989 – 1991), Nidwalden (2000), Schaffhausen (1990, 1996), Aargau (1986 – 1999), Thurgau (2000), Genf (1999).	Nidwalden (2000), Schaffhausen (1996), Graubünden (2001), Aargau (1989 – 1999).

Table A.11: Description of Variables

Variable	Formula	Type of variable	Source
Deterrents			
Criminal detectives	Log (criminal detectives / population)	continuous	BAP, full time equivalents
Ordinary policemen	Log(ordinary policemen / population)	continuous	BAP, full time equivalents
Severity of punishment (art. 111, 122/123, 138, 139, 140, 146, 178 of Swiss criminal code)	Unsuspended sentences / total sentences	continuous	BFS
Institutional determinant			
Direct democracy	Index from 1 (minimum) to 6 (maximum)	continuous	Own calculations / STUTZER (1999)
Economic variables			
Welfare transfers	Log(deflated welfare payments / population)	continuous	BFS, SECO (deflator)
Income inequality	Mean household income / median household income	continuous	FTA (1986 - 1998; biannually); SHP (1999 - 2001; annually)

Table A.11: Description of Variables (cont.)

Variable	Formula	Type of variable	Source
National income	Log(deflated national income in 1000 Sfr/ population)	continuous	BFS;
Unemployment rate	Officially recorded unemployed / labor force	continuous	SECO, BFS (labor force)
Federal transfers	Log (deflated federal transfers / population)		BFS, SECO (deflator)
Fiscal variables			
Fiscal decentralization	1-(cantonal total expenditure /cantonal + local expenditure)	continuous	Own calculations, BFS
Tax competition	Tax competition for canton i = [Sum (tax(j)* inverse distance (ij))]/ 25	continuous	Own calculations, FTA
Constitutional constraint	Index from 1 to 3 (strictest)	categorical	G. KIRCHGÄSSNER
Conservative ideology	Share of rightist parties in executive – share of leftist parties	continuous	Own calculations based on issues of Année Politique Suisse
Size of coalition	Number of parties / independent members in government	continuous	Own calculations based on issues of Année Politique Suisse

Table A.11: Description of Variables (cont.)

Variable	Formula	Type of variable	Source
Econogeographic variables			
Closeness to border	At least one of the ten most important border crossings is closely located to the canton; importance is given if >6,000 cars per day in 2001	dichotomous	Federal Office of Spatial Development;
Interaction between cantons	$I(ij) = (Pop_i * Pop_j)/\text{absolute distance}_{ij}$;	continuous	Own calculations, BFS (population), www.michelin.de (distances)
Sociodemographic variables			
Urbanization	Residents in urbanized areas (>10,000 inhabitants)/ population	continuous	BFS
Cantonal population	Log(permanent residential population at the end of the year[218])	continuous	BFS

[218] The BFS defines permanent residents as Swiss people and foreigners holding a C- or B-permit. Seasonally admitted residents are excluded.

Table A.11: Description of Variables (cont.)

Variable	Formula	Type of variable	Source
Residents 0 – 14 years	Residents aged 0 – 14 years / residential population	continuous	BFS
Residents 15 – 24 years	Residents aged 15 – 24 years / residential population	continuous	BFS
Residents 25 – 29 years	Residents aged 24 – 29 years / residential population	continuous	BFS
Residents over 60 years	Residents aged over 60 years / residential population	continuous	BFS
Cultural factor			
Romance canton	Canton with either Italian or French language	dichotomous	Own calculation
Dependent variable			
Crime rate	Log((number of delicts +1)/population*1000)	continuous	BAP, BFS (population)

Questionnaires

Fragebogen über die Erstellung der polizeilichen Kriminalstatistik

Klassifikation der Delikte Nummer 1, 2, 4, 5, 6, 7, 9, 17, 18 der polizeilichen Kriminalstatistik, wie sie monatlich dem Bundesamt für Polizei (**BAP)** gemeldet wird (Delikte gegen die Person, gegen das Eigentum und gegen die sexuelle Integrität).

Bitte beantworten Sie die Fallfragen (ab Seite 2) so, als ob Sie die Delikte für das monatliche Meldeblatt an das BAP aufnehmen würden. Bitte kreuzen Sie richtige Antworten an bzw. geben Sie die entsprechende Zahl an. Gerne dürfen Sie Kommentare und Bemerkungen hinzufügen (siehe auch die letzte Seite). Alle Angaben werden selbstverständlich vertraulich behandelt.

Allgemeine Fragen:

Wenn ein (1) Verbrechen mit mehreren Opfern (x) begangen wurde, wird dies in dem monatlichen Meldeblatt für das BAP aufgenommen als:

a) ein (1) begangenes Verbrechen
b) als (x) begangene Verbrechen (x = Zahl der Opfer)
c) dies ist von Delikt zu Delikt unterschiedlich

Wenn ein schweres und ein leichteres Delikt in Tateinheit begangen worden sind, erscheint auf dem Meldeblatt

a) lediglich das schwere Verbrechen
b) lediglich das leichte Delikt
c) beide Delikte (a) und b))
c) kann man so pauschal nicht sagen

Ist Ihre Erfassungsmethode für das BAP von heute die selbe wie die des Jahres **1998** ?

a) ja / mit hoher Wahrscheinlichkeit
b) nein
c) Ich weiss es nicht.

Ist Ihre Erfassungsmethode von heute die selbe wie die des Jahres **1986** ?

a) ja / mit hoher Wahrscheinlichkeit
b) nein
c) Ich weiss es nicht.

Gab es eine wichtige Änderung zwischen 1986 und heute in der polizeilichen Erfassungspraxis für das BAP? Dann geben Sie bitte das ungefähre Jahr an (mehrere Nennungen möglich):

Halten Sie die Erfassungspraxis (in den Gemeinden) in Ihrem Kanton für einheitlich?

a) ja (relativ homogen)
b) nein (relativ heterogen)
c) Frage ist nicht anwendbar

Delikt 1 / Delikt 2 (Mord / Totschlag / Körperverletzung)

Fall 1:
2 Kriminelle töten zusammen ein Opfer.
Dieses Verbrechen wird dem BAP gemeldet als:

a) 1 Tötungsdelikt (Delikt 1)
a) 2 Tötungsdelikte (Delikt 1)

Fall 2:
1 Krimineller tötet eine Familie von 4 Personen.
Dieses Verbrechen wird für das BAP erfasst als:

a) 1 Tötungsdelikt
a) 4 Tötungsdelikte

Fall 3:
Ein Amokläufer tötet 1 Person und verletzt leicht einen Passanten.
Dieses Verbrechen wird für das BAP erfasst als:

a) 1 (vollendetes) Tötungsdelikt
b) 1 Körperverletzung
c) 1 (versuchtes) Tötungsdelikt
d) als Kombination aus _____ und _____und _____

Delikt 4 / Delikt 5 / Delikt 6 (Diebstahl / Entreissdiebstahl / Einbruchdiebstahl)

Fall 4:
Eine Person auf einem fahrenden Moped entreisst einer Passantin ihre Handtasche.
Dieses Verbrechen wird dem BAP gemeldet als:

a) Entreissdiebstahl (Delikt 5)
b) Diebstahl (Delikt 4)
c) eine Kombination aus a) and b)

Fall 5:
Eine Person entdeckt, dass Ihre Geldbörse aus ihrer Hosentasche verschwunden ist. Sie ist überzeugt, dass diese gestohlen worden sein muss.
Dieses Delikt wird für das BAP erfasst als

a) Diebstahl (Delikt 4)
b) Entreissdiebstahl (Delikt 5)
c) eine Kombination aus a) und b)

Fall 6:
Eine Person meldet der Polizei, dass das Schloss der Haupteingangstüre ihres Wohnhauses beschädigt und dass eine Fensterscheibe zerschlagen worden ist. Sie glaubt, dass jemand erfolglos versucht hat, in ihr Haus einzubrechen.
Dieses Delikt wird für das BAP aufgenommen als

a) Sachbeschädigung
b) (versuchter) Einbruchdiebstahl
c) als Kombination aus a) und b)

Fall 7:
Eine Verbrecherbande aus 4 Personen bricht in derselben Nacht in 2 Wohnhäuser ein und stiehlt einige wertvolle Gemälde.
Dieses Verbrechen wird dem BAP gemeldet als:

a) _____ Einbruchdiebstahl(e) (Delikt 6)
b) _____ Sachbeschädigung(en)
c) eine Kombination aus a) und b)
d) keines, sondern_____

Delikt 7 (Autodiebstahl) / Delikt 9 (Betrug)

Fall 8:
Eine Person verschickt 500 Briefe, um eine lukrative Geldanlage mittels eines Schneeballsystems vorzutäuschen.
Dieses Verbrechen wird dem BAP gemeldet als:

a) 500 Betrugsfälle / Betrügereien
b) 1 Betrug

Fall 9:
Eine Bande von 3 Kriminellen stiehlt 10 Autos innerhalb der letzten 2 Wochen.
Dieses Verbrechen wird für das BAP aufgenommen als

a) _____ Autodiebstahl/ Autodiebstähle (Delikt 7)

Frage:
Was wird neben dem Diebstahl von Automobilen noch in die Kategorie 7 aufgenommen:

a) Diebstahl ganzer Schiffe / Boote
b) Diebstahl von Schiffsmotoren
c) Schidiebstahl
d) Entwendung von Fahrzeugen zum blossen Gebrauch
e) Fahrraddiebstahl
f) Diebstahl von Motorrädern, Kleinmotorrädern und Motorfahrrädern
g) andere, nämlich_____

Delikt 17 / Delikt 18 (Vergewaltigung / Unzuchtsdelikte)

Fall 9:
Eine Frau kommt zur Polizei und behauptet, sie sei vergewaltigt worden. Aufgrund ihrer Zeugenaussage ist nicht klar, ob es sich um eine Vergewaltigung (Art. 190) oder 'lediglich' um eine sexuelle Nötigung (Art. 189) handelt.
Dieses Verbrechen wird für das BAP aufgenommen als

a) Vergewaltigung (Delikt 17)
b) Unzuchtsdelikt (Delikt 18)
c) sowohl a) als auch b)

Fall 10:
3 Männer vergewaltigen 2 Frauen am selben Ort zur selben Zeit.
Dieses Verbrechen wird für das BAP aufgenommen als

a) _____ Vergewaltigung(en)

Vielen Dank für Ihre Kooperation !!!

Raum für zusätzliche Kommentare:

Datum:.....

Name:.....

Korps:.....

Questionnaire concernant le relevé des infractions pour la statistique policière de la criminalité

Classifications des infractions numéro 1, 2, 4, 5, 6, 7, 9, 17, 18 de la statistique policière de la criminalité comme elle est communiquée mensuellement à l'Office Fédéral de la Police (**OFP**) (infractions contre la personne, contre la propriété et contre l'intégrité sexuelle).

Veuillez, s.v.p., répondre aux questions concernant les affaires criminelles modèles (page 2 cont.), comme si vous enregistriez ces infractions dans la statistique policière mensuelle pour l'OFP. Cochez les réponses correctes et remplissez les chiffres correspondants. Vous pouvez aussi commenter les questions / réponses (voir aussi la dernière page). Toutes les informations seront traitées confidentiellement.

Questions générales:

S'il y a plusieurs (x) victimes d'une (1) infraction, cette infraction est communiquée à l'OFP comme:

a) une (1) infraction commise
b) (x) infractions commises
c) ça dépend de la nature du crime

Si une infraction grave et une infraction légère ont été commises durant le même crime (Tateinheit), ce cas est retenu dans la statistique policière de la criminalité comme:

a) un crime grave
b) un crime léger
c) un crime grave et un crime léger (a) et b))
d) une réponse globale n'est pas possible

Votre méthode actuelle pour retenir des infractions pour l'OFP, est-elle encore la même que celle de **1998** ?

a) oui / plutôt oui
b) non
c) Je ne sais pas.

Votre méthode actuelle pour retenir des infractions pour l'OFP, est-elle encore la même que celle de **1986** ?

a) oui / plutôt oui
b) non
c) Je ne sais pas.

Est-ce qu'il y a eu un changement important entre 1986 et aujourd'hui concernant la méthode pour retenir des infractions pour les envoyer à l'OFP ? S.v.p. indiquez l'année(s) approximative(s) du changement (plusieurs indications possibles):

Est-ce que vous jugez la pratique de retenir les infractions (dans les communes) dans votre canton homogène?

a) oui (plutôt homogène)
b) non (plutôt hétérogène)
c) cette question n'est pas applicable

Délit 1 / délit 2 (homicide/lésion corporelle)

Cas model 1:
2 criminels assassinent ensemble une victime.
Ce crime serait communiqué à l'OFP comme:

a) 1 homicide (délit 1)
a) 2 homicides (délit 1)

Cas model 2:
1 criminel assassine une famille de 4 personnes.
Ce crime serait retenu pour l'OFP comme:

a) 1 homicide
a) 4 homicides

Cas model 3:
Un fou furieux tue une (1) personne et lèse légèrement un passant.
Ce crime serait retenu pour l'OFP comme:

a) 1 homicide (consommé)
b) 1 lésion corporelle
c) 1 homicide (tentative)
d) la combinaison de _____ et _____ et _____

Délit 4 / délit 5 / délit 6 (vol/vol a l'arraché/cambriolage)

Cas model 4:
Une personne sur un moto arrache un sac à main d'une passante.
Ce crime serait communiqué à l'OFP comme:

a) vol à l'arraché (délit 5)
b) vol (délit 4)
c) la combinaison de a) et b)

Cas model 5:
Une personne constate que son porte-monnaie a disparu de sa poche. Elle est convaincue qu' il a du être volé.
Ce délit serait retenu pour l'OFP comme:

a) vol (délit 4)
b) vol à l'arraché (délit 5)
c) la combinaison de a) et b)

Cas model 6:
Une personne déclare à la police qu' on a endommagé la serrure de sa maison et qu' on a fracassé la vitre d'une fenêtre. Elle croit que quelqu'un a tenté de cambrioler sa maison sans succès.
Ce délit serait retenu pour l'OFP comme:

a) dommage à la propriété
b) cambriolage (tentative)
c) la combinaison de a) et b)

Cas model 7:
Un gang de 4 personnes cambriole 2 immeubles d'habitation dans la même nuit et en dérobent plusieurs peintures de grande valeur.
Ce crime serait communiqué à l'OFP comme:

a) _____ cambriolage(s) (délit 6)
b) _____ dommage(s) à la propriété
c) la combinaison de a) et b)
d) rien de tout, mais_____

Délit 7 (vol de véhicules) / délit 8 (escroquerie)

Cas model 8:
Une personne a envoyé 500 lettres pour simuler un placement d'argent lucratif au moyen d'un système de la boule de neige.
Ce crime serait communiqué à l'OFP comme:

a) 500 escroqueries
b) 1 escroquerie

Cas model 9:
Un gang de 3 criminels a volé 10 voitures pendant les deux dernières semaines.
Ce crime serait retenu pour l'OFP comme:

a) _____ vol(s) de véhicules (délit 7)

Question:
Les catégories suivantes de vols, sont-elles comprises dans les vols d'automobiles ?

a) vol de bateaux / de barques entiers
b) vol de moteurs de bateaux
c) vol de skis
d) vol d'usage / soustraction de véhicule automobile dans le dessein d'en faire usage
e) vol de bicyclettes / cycles
f) vol de motocyclettes (légers) et cyclomoteurs
g) outres, comme_____

Délit 17 / délit 18 (viol / infractions contre l'intégrité sexuelle)

Cas model 10:
Une femme signale à la police qu'elle a été violée. Sur la base de son témoignage c'est n'est pas évident si cet incident constitue un viol (art. 190) ou 'seulement' une contrainte sexuelle (art. 189).
Ce crime serait communiqué à l'OFP comme:

a) viol (délit 17)
b) délit contre l'intégrité sexuelle (délit 18)
c) les deux a) et b)

Cas model 11:
3 hommes violent 2 femmes dans la même localité en même temps.
Ce crime serait retenu pour l' OFP comme:

a) _____ viol(s)

Nous vous remercions pour votre coopération.

Espace pour des commentaires complémentaires:

date :.....

nom :.....

corps.....

Chapter VI:
Education

1 Introduction

Direct democracy is conjectured to lead to lower educational spending and thus to lower academic achievement. In this chapter, this assumption is examined for the case of Switzerland, a country with strong variation in the degree of direct democracy at the cantonal level. To provide supportive evidence for the hypothesized negative impact, the analysis focuses specifically on the impact of direct democracy on educational budgets and its effects on student achievement in reading.

The unexpectedly mediocre performance of Swiss students in the 2000 international PISA study has rekindled discussion about improving the Swiss educational system[219]. At the same time, because of the economic recession, cuts in federal, cantonal, and local budgets have become necessary that have also affected the funding available for public education. The ongoing debates about school reforms are further complicated by the fact that Swiss voters have an important influence on fiscal and budgetary issues through direct legislation. In general, direct legislative institutions restrict the financial means available to the government for the provision of schooling (BRADBURY et al. 2001; SCHALTEGGER 2001; FISCHER 2005). Similarly, in the U.S., measures to cut property taxes have been enacted through popular initiatives at the state level. Because American school budgets rely primarily on this local tax as their revenue source, the question of how and whether these newly introduced tax caps affect student performance has arisen to create a new field of empirical research in the U.S. Hence, the underlying question is whether citizens' control over the school budget necessarily leads to a lower quality of this public good or not. Since Swiss cantons are heterogeneous with respect to the degree of direct democracy, and quite autonomous in their policies on public education, Switzerland seems especially suitable for such an analysis.

This chapter aims at contributing to these recent discussions both in Switzerland and the U.S. by analyzing the impact of direct democracy on the quality of public education in Switzerland using national individual data on ninth graders acquired simultaneously with data collection for the 2000 OECD-PISA study. This chapter is also the first to analyze (a) the ways that political institutions affect public schooling provision in Switzerland and (b) the institutional impact on an entire conditional distribution of test scores.

[219] The average test score of 494 for Switzerland was below the international mean of 500 for the PISA study.

In anticipation of the empirical results, direct democracy is first shown to considerably restrict the financial resources available for public education in Swiss cantons. Since the combined cantonal and local school expenses are the main source for public schooling in Switzerland, this limiting impact on the subfederal school budget can be considered crucial. Subsequently, it is observed that if an educational production function based on personal and class characteristics is estimated, direct democracy leads to a considerable decline in student performance in reading, but not in mathematics or natural science. This result for reading is very similar to that obtained by researchers for the U.S. However, following the inclusion of revenue-driven input variables, the link between direct democracy and academic achievement breaks down for reading, while a performance raising influence is detected for mathematics. Therefore, it can be concluded that the major (negative) impact of direct legislation must occur solely through the school budgetary channel. Beyond its financial impact, no further additional effect of direct democracy on student performance can be observed.

The rest of the chapter is organized as follows. Section 2 provides a brief overview of the Swiss political and public educational system and presents related findings for the United States. Section 3 describes the data and model, and outlines the chosen estimation methods and the measure of direct democracy. Sections 4 and 5 present a discussion of the estimation results. Sections 6 and 7 conclude. Detailed regression outcomes are provided in the Appendix.

2 Education Quality and Direct Democracy

2.1 Direct Democracy

In modern (semi)direct democracies, a representative democracy is complemented by direct democratic institutions. The most prominent cases are Switzerland and the United States, which are both also shaped by a very strong fiscal decentralization, with each level having its own sources of tax revenue. Therefore, there exists a direct institutional link between the power to tax and the direct legislative institutions that provide citizens with the political means to influence both sides of the budget equally. In Switzerland, popular rights can be exerted at all three levels of the state (federal, cantonal, and communal). Since cantons (and communes) differ with respect to the degree of direct democracy in their constitutions, it is

possible to analyze the impact of a change in the degree of direct democracy on a particular policy outcome (FELD and KIRCHGÄSSNER 2001) like the provision of schooling.

In Switzerland, cantons are not only responsible for public education but bear the financial burden for its provision. Concerning the overall costs of compulsory education, the federal government contributes only 0.2%, whereas the cantons bear 38.8% and the communes 61.1%[220]. With respect to the communes, they mostly finance primary schools. In general, in all 26 Swiss cantons, two types of advanced education can be distinguished: basic education and education to meet advanced requirements (e.g. university preparation). Usually, the second type can only be entered on a selective basis. Nowadays, Swiss canton school curricula in primary and secondary stages of education are harmonized to a great extent[221]. Also included in cantonal authority is the general responsibility for teacher education, particularly for primary schools, which takes place in specialized teacher seminaries. Compulsory education finishes with the ninth grade, usually at the age of about 15. The fundamental regulations of public education, particularly school organization and the financial contributions of each level, are laid down in various cantonal laws on education.

2.2 Theoretical Background

Institutions of direct legislation, argue their many supporters, serve as a means to discipline politicians and bureaucrats, who are assumed to behave in a Niskanen-like manner rather than as benevolent dictators. Specifically, these bureaucrats exercise monopoly power and aim at maximizing their budgets[222]. For example, there is evidence for the U.S. that people in favor of the introduction of property tax limits actually believed that these budget cuts would lead to efficiency gains (TEMPLE 1996), hence an allocation of goods that is closer to the citizens', particularly the median voter's, preferences (POMMEREHNE 1978)[223]. In practice, a reliance on user charges in more direct democratic cantons was observed that makes the quality of the public good more independent of the financial resources of the cantonal government (FELD

[220] Appropriate information on this issue can be found at www.educa.ch, the Federal Statistical Office, www.bfs.admin.ch or in the annual issues of the STATISTISCHES JAHRBUCH DER SCHWEIZ, Bundesamt für Statistik (ed.), Neue Zürcher Zeitung.

[221] Such a curriculum includes the cantonal main language, a first foreign language, mathematics, writing, religion, history and civics, natural sciences, applied arts, needlework, music, and sports.

[222] See ROMER and ROSENTHAL (1978, 1979, 1982, 1983) and ibid. with MUNLEY (1992).

[223] See FELD and KIRCHGÄSSNER (2001) for a theoretical model that applies to fiscal referenda, KIRCHGÄSSNER (2000, 2001, 2002) for a review of ample empirical evidence, FELD and SAVIOZ (1997) for perspective on growth, and HUG (2004) for a corroborating meta-analysis.

and MATSUSAKA 1999)[224]. However, in the case of compulsory public schooling, this solution is (politically) not an option.

The limitation of the budget through direct legislation can give rise to bureaucratic adaptive behavior. Two different strategies are suggested in the theoretical literature. The first, according to Niskanen's theory of bureaucracy (e.g. INMAN 1979), is a substitution of budget maximization with a (relative) increase in administrative staff at the expense of the resources available for production of the public good (WILLIAMSON 1964). The rationale for this strategy is that a large personal staff gives the bureaucrat a feeling of importance and power (DOWNS 1967). When facing the decision to cut either administrative or instructional spending, a Leviathan-like administrator can be expected to choose the latter. For example, empirical U.S. studies show that a tax limit that imposes a limit on school budget growth leads to a spending shift from instruction to administration (FIGLIO 1997, 1998)[225].

Alternatively, FIGLIO and O'SULLIVAN (2001) propose a manipulative bureaucratic behavior in which the administrator deliberately allows student performances to decline by allocating fewer financial resources to instruction than to administration. The goal is persuade the electorate that budget reduction has a deleterious impact on the quality of education so that it overrules the previous tax limit vote. FIGLIO and O'SULLIVAN (2001), using expense data for police, fire protection, and general administration from 5,150 U.S. cities, show that in those cities with a so-called override option, the deterioration in public service was larger than in cities without this option[226]. Moreover, they observe the same phenomenon with respect to teacher-administrator ratios in school districts with an override option[227]. Moreover, Bradbury et al. (2001) show that communities with a history of passing overrides had the fastest school spending growth compared to communities without such an option.

[224] For the U.S., see MATSUSAKA (1995).

[225] The same authors find this phenomenon to be mitigated in some cases by strong competition between jurisdictions.

[226] It is, however, questionable whether a change in the ratio of administration to production costs provides sufficient evidence for one of the two theories. If instruction costs are more variable than administrative costs, in the short term only a cut in instruction costs might be practical.

[227] A third, alternative explanation would be that if the school administration were already working efficiently prior the decision to cut its budget, a decline in the quality of the public service should be revealed even though a benevolent school administrator was in charge.

2.3 U.S. Empirical Evidence on Public Schooling

In the last two decades, so-called tax revolts – for example, California's famous Proposition 13 in 1978 – have taken place in about 20 U.S. states[228], pushed through by means of direct legislation, particularly statutory initiatives at the state level[229]. These revolts aim mostly at reducing the level and growth of property taxes[230] that create important revenue at the local level (CARD and PAYNE 2002, DOWNES et al. 1998, DYE and MCGUIRE 1997). Therefore, such a change has a direct negative effect on school budgets (BRADBURY et al. 2001, SHADBEGIAN 2003) that does not necessarily translate into an equally large cut (in relative terms) in the teaching and administration components of school spending (e.g. DYE and MCGUIRE 1997), particularly if, as in the U.S., the administrative body of a school has the power to allocate financial resources quite autonomously (FIGLIO 1997).

During the 1990s, empirical multivariate analyses of the impact of newly introduced tax limits on student performance were carried out to test an educational production function[231]. In a cross-state analysis using individual data, FIGLIO (1997) finds a substantially lowering influence of tax limits on student performance – particularly on science, social studies, and reading examinations – but less robust results for mathematics. Using individual data, DOWNES and FIGLIO (1997) again find a sizeable and significant decline in statewide mean student performance in states with a tax limit compared to states without such a limit in mathematics but not in reading. A contradictory finding is reported by DOWNES et al. (1998) in an analysis of the effect of a local property tax cap on student performance in elementary schools in the Chicago metropolitan area. The authors find only a weak and small negative impact on student performance in mathematics and no effect on reading test scores. One explanation for this contradictory finding may be that, because affected and unaffected school districts were in close proximity to each other, these neighborhood schools constituted a single supply of public schooling for their inhabitants, which created strong competition. This competition between schools is conjectured to have mitigated the otherwise detrimental impact of tax limits (DOWNES and FIGLIO 1999, HOXBY 2000) and to have restricted the rent-seeking behavior by bureaucrats (DYE and MCGUIRE 1997, FIGLIO and O'SULLIVAN 2001).

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[228] The source ACIR (1995) provides a catalogue of existing tax limits.

[229] A brief overview of the theoretical background of this development can be found in DOWNES and FIGLIO (1997).

[230] See FIGLIO and RUEBEN (2001), FIGLIO (1997), DOWNES and FIGLIO (1997)

[231] Earlier contributions to this topic from the 1970s until the very early 1990s, some empirical but most informal, are described in DOWNES and FIGLIO (1999). These contributions, however, suffer from methodological shortcomings.

To identify the channels through which the deleterious impact of these tax limits occurred, their influence on various revenue-driven input factors of the educational production function has also been analyzed. Significant evidence is found that new tax limits brought about less educational spending (BRADBURY et al., 2001, SHADBEGIAN, 2003), larger class sizes, higher student-teacher ratios (SHADBEGIAN 2003, FIGLIO 1998), and lower wages for beginning instructors (FIGLIO 1997, POTERBA and RUEBEN 1995)[232] that cause potentially well-qualified teachers to leave the profession (FIGLIO and RUEBEN 2001, FIGLIO 1997a). Moreover, in contrast to voter expectations (TEMPLE 1996), administrator preferences remain unchanged (DOWNES 1996); that is, administrators still overstaff their administrations (DOWNES 1996) and reduce instructional expenses relative to administrative expenses (FIGLIO 1998) or fail to reduce administrative spending at all (FIGLIO 1997). Finally, the introduction of a property tax limit does not appear to have induced an increased service level of local public school provision (FIGLIO 1998). In sum, these findings provide evidence that the rent-seeking behavior of school bureaucrats is not limited by a cut in local school budget.

2.4 Direct Democracy and Educational Spending in Switzerland

As a first step in this present analysis, the impact of direct democracy on combined local and cantonal spending for public schools in Switzerland is determined. For this purpose, a model of government spending is estimated. This influence will then be taken into account in the structural form of the educational production function as the indirect effect of direct legislation on public education through endogenous variables (see section 3.1).

Various previous studies investigate the effects that direct democratic institutions in Switzerland and in the U.S. have on the combined cantonal and communal budgets. For Switzerland, these studies show that direct democracy leads to both smaller revenue and smaller expenditure (KIRCHGÄSSNER 2000, 2001, 2002). Regarding educational expenses, SCHALTEGGER (2001) documents an expenditure lowering impact of direct democracy on almost all components of cantonal and communal expenditure[233]. It is not surprising that this limiting impact is most noticeable in those policy areas in which Swiss cantons are granted

[232] SHADBEGIAN (2003) found no significant effect of tax limits on teachers' average wages but provided no analysis of wages of starting teachers. In an older contribution, however, he showed that stringent tax limits had a decreasing effect on local government wages, which also comprised the wages of teachers (SHADBEGIAN 2000).

[233] Since SCHALTEGGER (2001) employs a different model specification, this analysis could not be based solely on his results. Rather, this estimation might be viewed as an omitted variable test for his estimation.

political autonomy by the Swiss constitution (art. 3 of the Swiss constitution[234]); specifically, education, the health system, and culture (GERMANN 2002). The weak influence of the federal government is rooted in the very structure and division of power of the Swiss state developed in the nineteenth century. For the U.S., a restraining property tax revenue effect of (stringent) local tax limits on the local (school) budget and revenue is detected (SHADBEGIAN 1999, 2003). Regarding the appropriation effort for the financing of higher education, ARCHIBALD and FELDMAN (2004) also find a considerable lowering impact at the U.S. state level[235]. Based on these findings, it is here hypothesized that direct democracy should have a spending restraint impact on the 'educational spending' component of the subfederal budget in Switzerland.

In this model, government expenditure is regarded as a function of fiscal decentralization (defined as share of local expenses in total cantonal and local expenses), urbanization of the canton, average national income, cantonal population, tax competition, a fiscally effective constitutional 'break', the share of young and old people (< 20 years, > 60 years, respectively), and government ideology (with positive values indicating a conservative position). Most important, government spending is thought to be dependent on the degree of direct democracy and cantonal culture, measured by a dummy for French- and Italian-speaking cantons. A prediction of the impact of these controls and their theoretical foundation can be found in FELD and KIRCHGÄSSNER (2001). The dependent variable is the combined cantonal and local budgetary expenses for schooling per capita, which have been logarithmized[236].

All the budgetary, (macro)economic, and sociodemographic controls were obtained from the Federal Statistical Office (BFS). These aggregate data form a synthetic panel, with 26 cantons as observational units per year between 1980 and 1998. The estimation technique is a two-stage least squares (2SLS) approach: both ideology of government and fiscal federalism are treated as endogenous since higher educational spending might allow formerly disenfranchised groups go to the polls and induce a shift in government ideology. Further, the

[234] Available at http://www.admin.ch/ch/d/sr/c101.html (10/11/2004).

[235] This study uses a broad definition of tax limits and included all existing types: both local and state property tax limits, formal restrictions of state expenditures and of state revenue, statutory and constitutional tax limits, moving base and fixed base versions. State spending for higher education appears to be more sensitive to the introduction of tax limits than other components of the state budget.

[236] A combination of cantonal and communal spending must be employed because in every single canton the financing of schooling is shared differently between the canton and its communes.

dependent variable, the combined local and cantonal expenditure for education, forms part of the denominator of the fiscal decentralization measure, which serves as a predictor. These variables are instrumented with cantonal fixed effects. Newey-West standard errors correct for heteroscedasticity and serial autocorrelation. All monetary variables are deflated to the year 2000.

Table 1: Determinants of Educational Spending in Swiss Cantons, 1980 – 1998

Variable	Coefficient	t-value
Direct democracy	-0.054**	-2.65
Cantons with Italian or French main language	-0.078	-1.43
Fiscal decentralization	-0.440***	-5.04
Tax competition	-0.052	-1.17
Log of lumpsum transfers	0.028	0.78
Constitutional constraint (fiscal break)	0.003	0.36
Conservative ideology of government	-0.138*	-2.35
Log of national income	0.157(*)	1.92
Urbanization	0.238***	3.31
Log of cantonal population	0.018	1.06
Share of young people	-0.012	-1.39
Share of old people	-0.024***	-3.26
Constant	6.802***	5.72
Adjusted R^2	0.83	
F-statistic	80.280	
Jarcque-Bera test (χ-value)	7.161*	
Observations	494	

2SLS estimation with Newey-West standard errors. ***indicates significance at the 0.1% level, **at the 1% level, *at the 5% level and (*)at the 10% level, respectively. Estimation with year dummies.

The results indicate that direct democracy restricts the subfederal spending for education (see table 1): its coefficient is negative and significant at the 1% level. Fiscal decentralization also exhibits the expected dampening impact, whereas educational expenses rise with the degree of urbanization. In addition, the more retired persons reside in a canton, the fewer financial resources are available for compulsory schooling. Moreover, higher wealth in a canton

weakly induces more spending for public schooling. In this model, more conservatively oriented governments tend to have a lower level of educational expenses. The remaining controls are not significant at any conventional level. An adjusted R^2 of about 0.83 confirms the good explanatory power of the model. The normality of the distribution of the residuals can be rejected. An exclusion of statistical outliers leads to identical empirical results with respect to the impact of direct democracy on schooling expenses and for most of the remaining predictors (see table 11.5 of the Appendix).

3 Data and Model

3.1 The Model: Structural and Reduced Form

This analysis of the impact of direct democracy on education assumes education in public schools to be an output of an educational production process in which several factors ('inputs') play a role[237]. These factors can be grouped according to the following criteria: school characteristics, classroom-related characteristics, peer characteristics, and, most important, student background information (individual and family). The selection of these determinants is based on empirical (econometric) analyses of the PISA results and hence reflects a typical model specification chosen by educational economists[238]. In addition, the model employs sociodemographic determinants at the cantonal level and is augmented by a cultural production factor that reflects the mentality of the citizens in the school's location to take into account the language gap that divides Switzerland. Finally, this model also contains an institutional variable that is the variable of interest. As dependent variable, the model uses the weighted likelihood estimate on reading provided in the dataset.

In this analysis, a *reduced* form and a *structural* form of the model will be distinguished. The structural form includes revenue-driven 'endogenous' input factors that are not part of the reduced form, which by definition is composed of exogenous variables only[239]. These endogenous input factors, determined by cantonal and communal school expenses and shown in section 2.4 to be negatively influenced by the level of direct democracy, are teacher

[237] The most extensive set of such determinants originates from the work of SUMMERS and WOLFE (1977).

[238] See FUCHS and WOESSMANN (2004), FERTIG (2003), FERTIG and SCHMIDT (2002), FERTIG and WRIGHT (2004). For specifications chosen by public economists, see DOWNES and FIGLIO (1997) and FIGLIO (1997).

[239] The second equation of this structural form is approximated in section 2.4 by the regression of educational expenditure on direct democracy.

qualification, teacher shortages, total hours of schooling, student-teacher ratio, availability and quality of instruction material, and state of school building or availability of space. The reduced form includes the combined direct and indirect influence of political institutions, whereas the structural form includes only the direct influence because the indirect influence is filtered out. Whereas both forms are estimated in the U.S. literature, the reduced form is used more frequently. Nevertheless, the impact of tax limits is found to significantly lower performance for both forms.

In the structural form, the relation between student performance and all its predictors outlined above can be expressed by the following function:

$$performance = f(democracy, culture, individual, peers, school, canton, school\ inputs),$$

where *democracy* denotes direct democratic institutions, and *culture* the main regional culture of the school location, which accounts for differences in people's mentality. *Individual* denotes the student's individual and family characteristics such as gender and parents' education. *Peers* stands for peer group characteristics that aim at measuring the external effects of the peer group on an individual's academic performance: i.e. individual academic performance might depend on the intellectual potential of peers [240]. *School* denotes school/class-related characteristics like the selectivity of the institution or problems with class discipline. C*anton* represents cantonal sociodemographics, which serve two purposes: first, they can be viewed as proxies for missing individual and peer group variables in class (e.g. religion); second, they account for the sociodemographic determinants of the demand for public goods [241]. Finally, *school inputs* denote revenue-driven school inputs as described above. A description of variables can be found in table 11.1 of the Appendix.

For predicting the impact of the sociodemographic and peer controls, the reader is referred to the literature cited in the fields of educational economics and public finance (e.g. WINSTON and ZIMMERMAN 2003, FIGLIO 1997). As regards the structural model, the influence of revenue-driven inputs on student performance is amply described in the educational

[240] In small classes, there might even exist a feedback relation and continuing interaction between the one and the other(s) (for empirical literature on peer effects, see ZIMMER and TOMA (2000), SUMMERS and WOLFE (1977), KERCKHOFF (1986), EPPLE et al. (2003), HOXBY (2000a), RANGVID (2004)).

[241] See e.g. FELD, et al. (2004) for a justification.

economics literature. In general, empirical findings on the decisiveness of these budget-related input factors tend to disagree or suffer from confounding factors[242].

3.2 Direct Democracy

The estimations employ a composite index of direct democracy that measures the degree of direct democracy at the cantonal level in Switzerland. It is an unweighted average of four subindices that evaluate the power of the constitutional initiative, the statutory initiative, the fiscal referendum, and the statutory referendum. This index takes on values between 1 and 6, with 6 indicating the highest degree of empowerment of the cantonal electorate. Nevertheless, this index measures the presence of these institutions rather than their actual use[243]. Its exact construction is demonstrated in STUTZER (1999) for the year 1996. The values for the year 2000 are displayed in the Appendix (table 11.2).

Defining public schooling as a public good, this analysis first poses the question of whether and how direct democracy affects the provision of this public good at the cantonal level. Based on the U.S. experience, a performance lowering impact in both forms of the model is anticipated.

3.3 The Data

The individual, family, and school related determinants are taken directly from the national PISA study. These data were collected jointly with the 2000 OECD-PISA study using identical questionnaires and subject tests, with a primary focus on reading performance. However, in contrast to the OECD-PISA study, the population of the national study includes all ninth graders[244]; hence, student ages range considerably. The procedure for data collection and treatment was roughly identical for both the Swiss national sample and the PISA sample. The primary sampling units are schools not cantons (FERTIG 2003), and the dataset provides a

[242] For literature on the impact of financial resources in general available to schools on education, see HANUSHEK (1996), DOWNES (1992), HANUSHEK and SOMERS (1999), LUDWIG and BASSI (1998), CARD and PAYNE (2002); for the impact of student-teacher-ratio on academic achievement, see HANUSHEK (1998), KRUEGER (2002), BUCKINGHAM (2003b), and FIGLIO (1999); for the influence of teachers' wages, see HANUSHEK et al. (1999, 2001), FIGLIO (1999), and finally, for quality of teaching see HANUSHEK et al. (1998), HANUSHEK (2003), BUCKINGHAM (2003b, 2003a).

[243] The existence of such an institution is already sufficient to induce a change in allocation of resources because it serves as a credible threat in a game theoretical context (FELD and KIRCHGÄSSNER 2001).

[244] The OECD-PISA study focuses on pupils aged 15.

weighted likelihood estimate of a student's performance that is a weighted average of correctly answered responses, with the weights reflecting the level of difficulty of the question (HAMBLETON and SWAMINATHAN 1985, WARM 1989). The matching of schools and students also makes it possible to construct classroom-based peer variables.

The national study sample is preferred over the original PISA sample for two reasons: first, it covers almost all cantons[245]; second, the administration of the questionnaires to complete classes instead of age groups allows the construction of peer group/class mate predictors. Moreover, even though there exists an extension ('the French sample') to the national study dataset that covers only French-speaking regions[246], the national study oversampled students from the German-speaking cantons of *Bern*, *Zürich*, and *St. Gallen*. Therefore, to prevent an overrepresentation, both datasets are merged and all observations with missing values deleted. In a second step, to prevent endogeneity in the peer group variables, all students who attended test language classes with less than 20 peers are also excluded. The mean of the reading test score was originally normalized at 500, with a standard deviation of 90 for the whole national dataset, but because of the deletion process, the new mean is about 530, with a standard deviation of approximately 80 based on a final sample of 3,411 observations. For more descriptive statistics also for the other two test subjects, see tables 11.3 and 11.4 of the Appendix.

3.4 Methodology

Both the reduced and the structural model are estimated using two different econometric methods. The first approach uses ordinary least squares (OLS) and corrects the standard errors of the coefficients for heteroscedasticity. It also assumes that students who attend identical schools share common factors both at the school and cantonal level – for example, condition

[245] No observations exist from the cantons *Appenzell Innerrhoden* and *Uri*. To the author's knowledge, there is no educational institute providing a ninth-grade education in the first canton.

[246] The extension dataset differs from the national sample solely in that entire classes were administered the questionnaires, whereas in the national sample, students were drawn randomly from selected classes. The original dataset included 4,833 students in 105 schools belonging to both samples. For the data, the codebooks, and further information, see SIDOS 2000 (2004). A more detailed description of data sources for the determinants of education can be found in the Appendix (table A.7). This table also provides information on the chosen base categories. The procedure of normalization follows that for the international PISA sample (FERTIG 2003).

of the school building and political institutions in the canton – and it therefore employs clustering at the school level (MOULTON 1990)[247].

The second approach applies a quantile regression method that, rather than estimating the conditional mean function of the dependent variable as in OLS, estimates various conditional functions for (predetermined) different portions of the test score distribution. Hence, this method provides a more complete picture of how the predictors influence the response variable over its distribution. It also becomes possible to uncover significant impacts on the tails that leave the mean unchanged, in contrast to an OLS regression that would render the determinant in question insignificant. The quantile regression method is also more efficient if the distribution of the dependent variable deviates from the normal distribution in its higher moments. The interpretation of a coefficient is the same as for an OLS estimate but is only valid only for a particular quantile. As the specified quantile increases, the portion of sample observations included (sorted in an ascending order) rises. In this model, the 10th, 25th, 50th, 75th, and 90th quantiles of the test score distribution are selected for analysis[248].

Some determinants employed are subject to potential simultaneity: e.g. besides peer effects, performance at home or a higher age could be proxies for bad grades at school. Additionally, the selection of pupils into different school types and classes is not fully taken into account with these estimation methods. Both might induce a bias in the estimated coefficients. An instrumentation of endogenous variables or a correction for sample selection, however, cannot be carried out because the PISA data do not provide the necessary exogenous instruments (for a discussion, see also RANGVID 2004, GRADDY and STEVENS 2003).

[247] This estimation method is also applied by educational economists to the analysis of PISA results using an international sample containing several countries. However, no literature appears to exist using a multilevel analysis approach for such data.

[248] For an introduction to the quantile regression method, see KOENKER and BASSET (1978), KOENKER and HALLOCK (2001), BUCHINSKY (1998), CADE and NOON (2003).

4 Estimation Results for Reading

4.1 Direct Democracy and OLS

Table 8.1 of the Appendix displays the OLS estimation results for reading for both the reduced form and the structural form that includes the educational input factors at the school and class level that are financed through cantonal sources.

In the reduced form, direct democracy exerts a performance lowering impact on an average student from among the 3,411 students that remain in the sample (significance at the 5% level). As regards the size of the impact, it appears to be considerable. Since this index ranges from 1 to 6, the maximum reduction in the test score for an average student due to direct legislative institutions is about 46 difficulty adjusted test score points. Given the standard deviation of about 80 points, this impact is quite substantial. Nevertheless, compared to the sizes of the impact of the remaining determinants, the effect of direct democracy on academic performance is not considerable. Stronger by far are, for example, the effects of high parental income, age, gender, the situation and learning environment of the pupil at home, and the selectivity of the attended school. In the structural form, the OLS estimate of the index of direct democracy is also negative, but in contrast to the reduced form, it is far from being significant at any conventional level. For an average student from among the 2,969 students, political institutions do not exert any significant impact on reading test scores if revenue-driven input factors are explicitly taken into account. The adjusted R^2 of around 0.26 for both forms indicates a good fit of this model for a cross section.

As regards the remaining controls in the OLS regression, most of the individual, family, peer, and school characteristics coefficients are robust to the inclusion and exclusion of educational revenue-driven input factors. The most prominent change can be observed for the selectivity of school, which becomes smaller in size and insignificant if these revenue-driven inputs are controlled for. Selectivity is defined as students being admitted to a particular school based either on their previous grades or an entrance exam. This result could mean that the advantage of more selective schools in Switzerland issues from a better quality of particular inputs. Overall, most of the individual and family variables show signs already obtained in similar studies on the determinants of school performance using the PISA results, e.g. the better performance of females (e.g. FERTIG 2003, FERTIG and SCHMIDT 2002, FERTIG and WRIGHT

2002)[249]. One exception is the achievement dampening effect of higher levels of education of parents, which might be caused by some collinearity between them and higher levels of income[250]. The control 'homework feedback' also shows an unexpected negative sign. Whereas a positive impact on a pupil's performance would be conjectured, the grading of homework or its inclusion in the final mark obviously leads to a reduction in pupil's performance[251]. Regular testing does not considerably influence a student's academic achievement. Both the higher age of a student and discussion of the student's performance at home can be viewed as proxies for low performance at school, whereas younger age could indicate excellent performance[252].

In terms of peer group effects, the heterogeneity measure in difficulty adjusted test scores seems to capture the most important peer group effects, so that the coefficients of the shares of female and of foreign peers are rendered insignificant[253]. The heterogeneity measure employed here is defined as the peer group's mean performance divided by its standard deviation. Thus, a positive estimate indicates that a better peer group performance and/or a smaller variation in class causes an increase in the single student's test score. In both the reduced and the structural form of the model, a positive and significant coefficient is observed; this result is supported by the externality/peer group effect theory (see section 3.1). The measured peer effect might also be a proxy for the school type, which the selectivity variable is intended to control for. Given that the heterogeneity measure for reading ranges between approximately 3 and 16, the quantitative size of this effect varies between about 20 and 110 test score points, which is quite a considerable range.

As regards cantonal characteristics, most cantonal variables are insignificant for the expected mean of the difficulty adjusted test score in reading. One exception is the share of poor persons in both models, defined as persons unable to afford savings of 100 Swiss Francs per

[249] In value-added analyses, the effect of gender is reported insignificant (e.g. GRADDY and STEVENS 2003). Due to the cross-sectional structure of the data, such an analysis is not possible in this case.

[250] Although the numerical correlation coefficient does not exceed 0.3 in absolute value, to the analysis takes into account that the income variable is also constructed on the basis of educational control. (The highest positive correlations are found between tertiary education of father and the two highest levels of income (~0.3), followed by the number of books with a low level (hisei2) and high levels of income (hisei6 and hisei7) (~-0.2 and ~0.2, respectively). It should also be noted that the empirical studies on the PISA results use an identical set of family background controls.

[251] A convincing explanation is better left to specialists in pedagogy and psychology.

[252] As already noted in the methodology section, the potential endogeneity cannot be corrected because the dataset does not provide suitable instruments.

[253] An omission of the coefficient in reading renders the remaining two peer variables significant.

month. This variable exhibits a significant decreasing impact on an average student's test scores, although not a very large one. In the structural model, Protestantism is weakly associated with higher test scores. The logarithm of the cantonal population is significant in the reduced model, indicating that student performance is worse in more populated cantons, but it is not decisive in the structural form.

As regards the endogenous input variables for schooling, no access to a PC at school appears to be detrimental for an average student's academic achievement. However a shortage of teachers does not affect the average student's achievement at any conventional level, although the negative sign points in the predicted direction. Tertiary qualification of a school's teachers, on the other hand, is associated with a higher test score in reading. Nevertheless, the coefficients of the remaining input factors are not significant for an average student at any conventional level.

In sum, a significant performance decreasing impact of direct democracy is observed in the reduced form and an insignificant coefficient in the structural form. Given that direct democracy is shown to dampen subfederal expenditure for public schools and thus potentially restrain spending for revenue-driven inputs at the school and class level, it can be conjectured that the disappearance of its significance in the structural form is due to the inclusion of these input factors. For policy makers, this finding means that fewer financial means available at the cantonal level for public schooling do translate into lower student performance, whether directly (quasi-automatically) or through the strategic acts of school bureaucrats. The indirect effect of direct legislation has a negative direction and dominates the direct one in the reduced form. It can also be concluded that revenue-driven inputs are important for school performance in reading, a noteworthy epiphenomenon of the empirical results[254]. The next step is to take a closer look at the influence of this predictor on various portions of the conditional test score distribution in reading.

[254] See also the discussion in section 6.

4.2 Quantile Regression Results

The Reduced Form of the Model

The quantile regression reveals a negative sign over the distribution of test scores in the reduced form specification for the impact of the composite index of direct democracy on academic achievement (see table 8.2 of the Appendix). The variable of interest is significant for all estimated quantiles, but the strongest impact can be observed for the 75th and 50th percentiles. Significance levels for the outer quantiles drop down to the 10% level. At this point, it should be noted that such a drop in significance levels can be expected given a rising deviation from the median at both tails of the conditional distribution, because the t-statistic is more likely to be confounded by single statistical outliers. A test of joint nonsignificance reveals a joint significance of this institutional impact at the 5% level for all estimated quantiles. The results also reveal the similarity in size of the coefficients for the mean and the median[255].

A quantile regression technique makes it possible to examine the development of the size of the coefficient over the entire distribution, as well as to test whether a seeming change in size is statistically significant. A graphic representation of the development of the coefficient of direct democracy over the entire conditional test score distribution is presented in figure 1[256]. On the ordinate, the value of the coefficient is measured in test score points, while the abscissa depicts the percentiles of the distribution in 1% increments. The estimated coefficient for each percentile is plotted as a continuous line and its 95% confidence intervals, as a shaded area. The OLS estimate of direct democracy is depicted as a horizontal dashed line; in this case, intersecting the x-axis at -7.9. As figure 1 illustrates, the impact of direct democracy does not appear to vary greatly between quantiles, particularly for the middle part of the test score distribution. Further, the coefficient(s) appear(s) to fluctuate closely around the dashed line. A closer inspection also shows the insignificance of the coefficient in the very low and very high percentiles at the 5% level. The result of a Wald test for equality of the entire distribution leads to the conclusion that, given the personal, family, and peer characteristics of a student, each individual is equally (negatively) affected by direct democratic institutions.

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[255] The author is aware of the fact that this difference in significance could have been caused either by the fact that standard errors are not bootstrapped in the OLS regression or because no clustering of cantons was possible in the quantile regressions.

[256] According to JOAO PEDRO W. DE AZEVEDO, author of the user-written command *grqreg* with which the plots were created, coefficients for the entire distribution are estimated with confidence intervals based on the bootstrapped standard errors of the last regression results (personal communication, 8th of February, 2005).

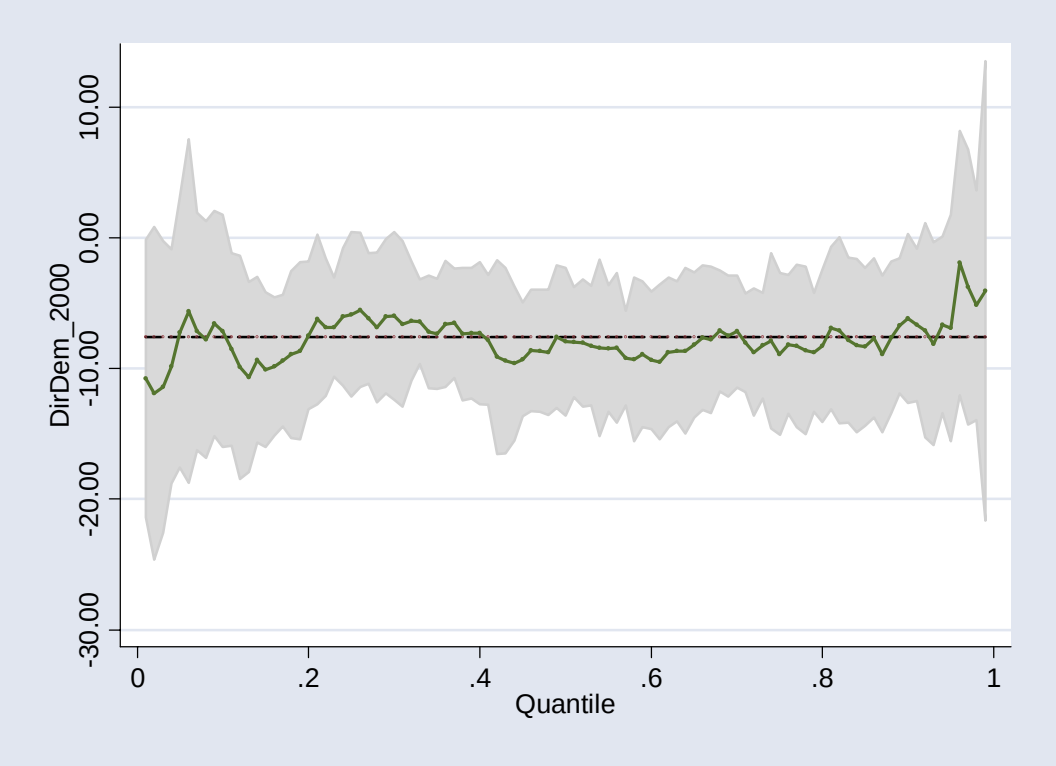

Figure 1: Reading; Reduced Form

For the coefficients of the remaining individual and cantonal sociodemographic controls and their variance over the test score distribution, the interested reader is referred to the Appendix. Worthwhile factors for exploration include the changing impact of higher income classes, number of siblings, discussion of student performance in the home, the share of foreign peers in class, as well as of the share of Protestants in the cantons. Also noteworthy is that, again, the sign for higher education is contrary to expectation, possibly for the same reasons as stated before. The impact of most variables, however, resembles the results obtained in the OLS model.

The Structural Form

Table 8.3 of the Appendix displays the quantile regression results for the structural form; i.e. after the inclusion of various input measures whose quality and/or quantity are probably determined by the given financial resources. The structural form allows differentiation between the impact of direct democracy through a mere lack of financial resources and an impact beyond this purely financial influence. In other words, it is potentially possible to identify a reallocation of given resources between students in the same grade. However, in all

quantiles, the index of direct democracy is rendered insignificant, which parallels the OLS regression results. Figure 2, in which the coefficient of direct democracy fluctuates around 0 and its confidence interval always appears to include the 0-line, corroborates this finding for the entire distribution. Based on a successful Wald test for equality of estimated coefficients, the hypothesis of joint nonsignificance cannot be rejected. Hence, no redistributive impact of direct democracy between students can be observed in reading.

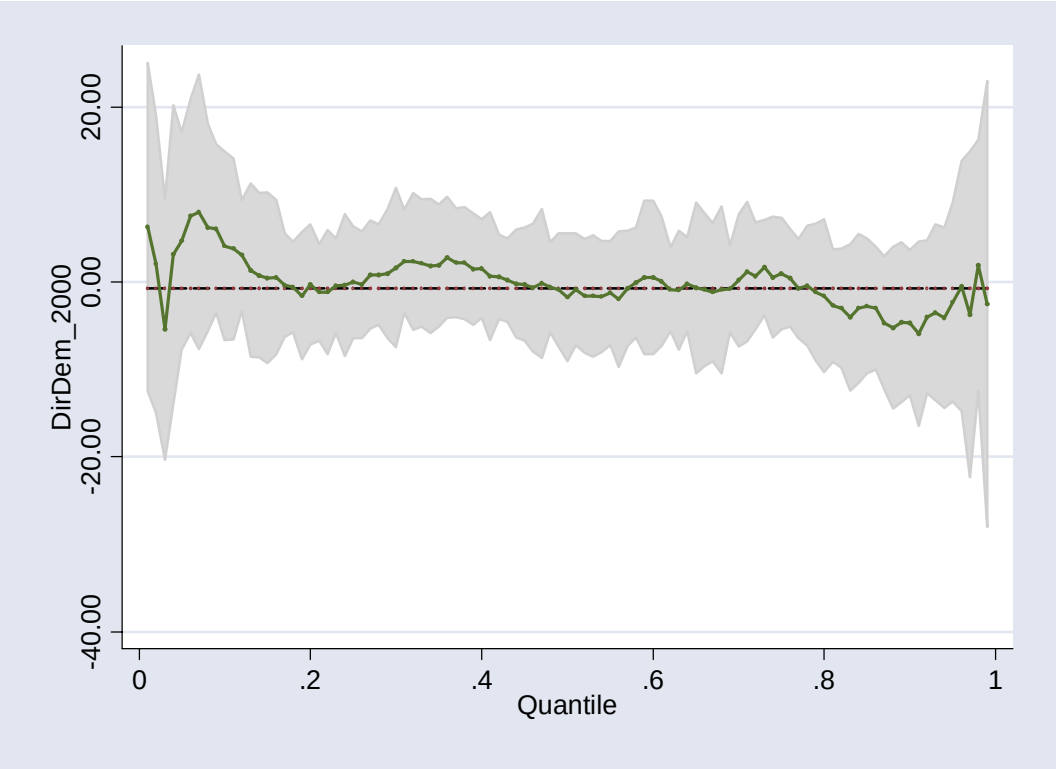

Figure 2: Reading; Structural Form

Similar effects to those already detected in the OLS model are observed for the various input measures in the structural model. Again, significant coefficients can only be observed for the equipment of the school with PCs and the tertiary qualification of teachers. The first input factor appears to be detrimental only for the students in the lower part of the test score distribution (10th and 25th quantiles), whereas the second factor affects all students except for those in the 90th quantile, which might be due to the low number of observations. In addition, a higher number of total teaching hours seems to benefit most pupils in the lower 25th quantile, but only weakly (significance at the 10% level). None of the other coefficients of input factors reach statistical significance.

With respect to the cantonal predictors, a similar pattern to the OLS results of the structural model is observed. Moreover, the results are also similar to those obtained in the reduced model that show the input factors at the school level to be quite uncorrelated with the sociodemographic determinants at the cantonal level. As in the reduced model, the convincingly significant coefficients are those for the share of Protestants, the logarithm of the cantonal population, and the share of poor people, with the latter experiencing a decline in significance from the 0.1% level in the reduced model down to the 5% or 10% level in the structural model for almost the entire distribution. This finding indicates that the strong negative impact of poor persons observed in the reduced model must occur through the cantonal budget transmission channel, probably because politicians face a trade-off between supporting needy persons and financing cantonal education. The identical observation holds true for the cantonal population, which was strongly significant in the reduced model but lost considerable significance in the structural model, particularly for the lower and middle quantiles. This finding can again be interpreted to mean that the impact of the cantonal size for lower and middle quantiles obviously occurs through financial channels related to school expenditure. In contrast, for the higher quantiles, a different transmission channel seems to be at work for this effect not filtered out in the model specification.

Several robustness checks assess whether the observed influence of direct democratic institutions in both forms depends on the inclusion or exclusion of particular controls or is sensitive to a potentially unsolved endogeneity problem. In general, the results of the original model specification are insensitive to slight variations in the specification, but the exclusion of peer effects appears to cause an omitted variable bias. In addition, imputation of the median and admission of class sizes of ten or more students, which increases the number of observations to roughly 9,000, do not alter the main findings. In the reduced form, direct democracy again shows a performance lowering effect, which disappears in the structural form.

5 Estimation Results for Mathematics and Natural Science

The reduced and structural forms are also tested for mathematics and natural science using both OLS and quantile regression methods. The measure of peer performance, the qualification of teachers, and the shortage of teachers are also adjusted according to the two subjects taught. In addition, classes with less than 20 students are eliminated on the basis of

the corresponding subject. The rest of this section discusses the results for both mathematics and natural science.

5.1 Results for Mathematics

The Reduced Form

The OLS results for the reduced form reveal an insignificant impact of direct democracy for an average student from among the 1,846 students tested (see table 9.1). The quantile regression results corroborate this observation for all estimated quantiles. Additionally, a test of joint nonsignificance for all estimated quantiles cannot be rejected.

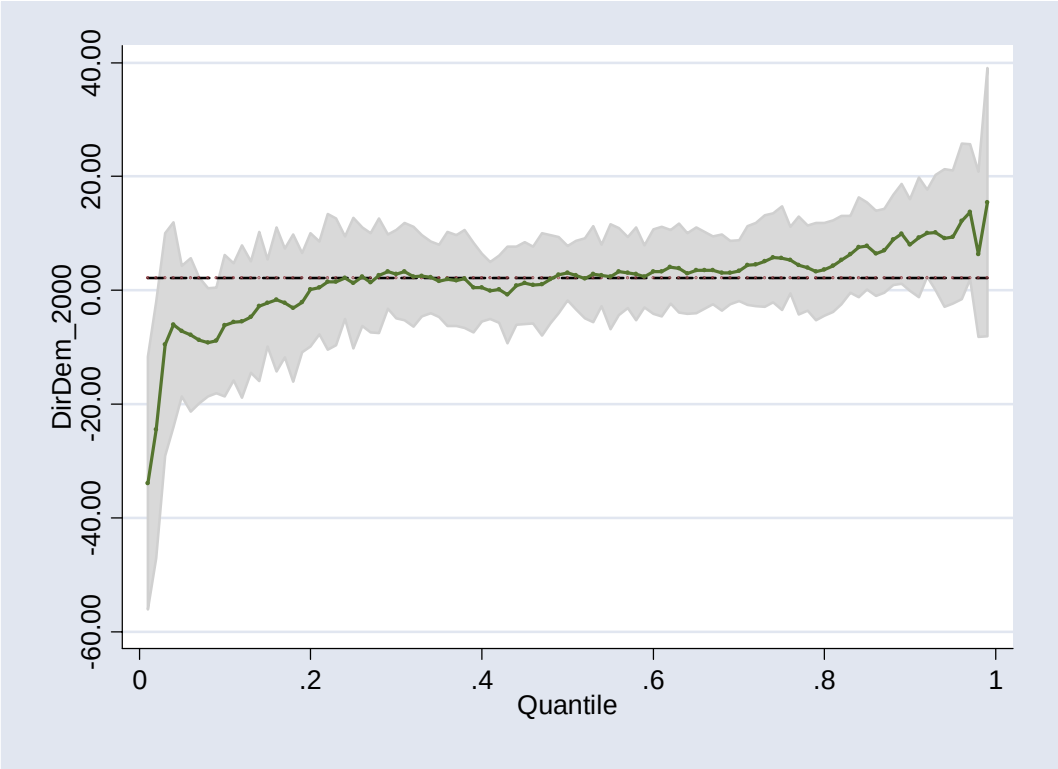

Figure 3: Mathematics; Reduced Form

In the OLS model, among the remaining sociodemographic, school peer, and cantonal input factors, female students are observed to perform about 30 test score points worse than their male peers in all quantiles. Students in selective schools perform weakly better than their peers in other school types, which is in line with the results for reading. Less heterogeneity in class leads to higher test scores for the average student, as observed for reading. In contrast, however, the share of foreign peers in class also exerts an achievement lowering impact,

whereas the ratio of female peers does not appear influential. Among the cantonal controls, once again, a higher ratio of poor people and more population exhibit a performance declining impact for an average student. In contrast to reading, the share of residents with at least a high school degree and the share of Protestants are strongly associated with higher test scores. Similar results are obtained in the quantile regressions. For a detailed description of the empirical results for the OLS and the quantile regressions, see the Appendix on mathematics, table 9.2

Figure 3 illustrates the development of the impact of direct democracy over the entire conditional test score distribution in the reduced form. Again, the dashed line depicts the OLS coefficient of about +2.2, while the continuous line represents the coefficients for all quantiles. The confidence interval always crosses the 0-line, indicating that the coefficient never reaches significance at the 5% level. In contrast to the results for reading, however, a clear upward trend of the coefficient is observable, starting with a negative value of -6 in the lowest percentile and reaching about +8 in the 90th percentile. The H_0-hypothesis of jointly equal coefficients in the estimated quantiles, however, is not rejected[257].

The Structural Form

The OLS estimate in the structural form reveals a significant performance enhancing effect on test scores in the structural form (at the 5% level; see table 9.1). The size of this coefficient is about 11.6, which in a canton with the theoretically highest level of direct democracy of 6 index points would translate into a sizeable effect of about 70 adjusted test score points, almost as large as the standard deviation of 80 points in mathematics.

The quantile regression estimates reveal different sizes of impact and varying significance levels (see table 9.3). In the lower portions of the conditional test score distribution, the size of the coefficient takes on the small, and thus insignificant, values of 1.1 and 5.3. In the 50th, 75th, and 90th quantiles, however, direct democracy affects student performance considerably, with maximum test score points of 64.5, 76.8, and 148.8 (significant at the 5%, 5%, and 0.1% levels, respectively). A test of joint nonsignificance reveals that the estimates are jointly significant at the 1% level, meaning that a test for equality of coefficients should reveal whether the seeming differences in sizes are statistically significant. The test shows

[257] A pairwise Wald test for equality of coefficients does show that some of these differences are statistically significant, particularly between the lowest estimated quantile (the 10th) and the two highest (the 75th and the 90th, respectively). Both, however, are only significant at the 10% level.

that the H_0-hypothesis of equality can be rejected at the 10% level. In addition, a pairwise Wald test reveals significant differences between the pairs in the 10th, 25th, 50th, and 75th percentiles and those in the 90th percentile (all significant at the 5% level) but not among the lower and middle estimated quantiles. These results can be viewed as weak evidence for a redistributive impact of direct democracy on given resources to the advantage of students in the 90th percentile of the conditional test score distribution; i.e. to the advantage of students who are already performing well.

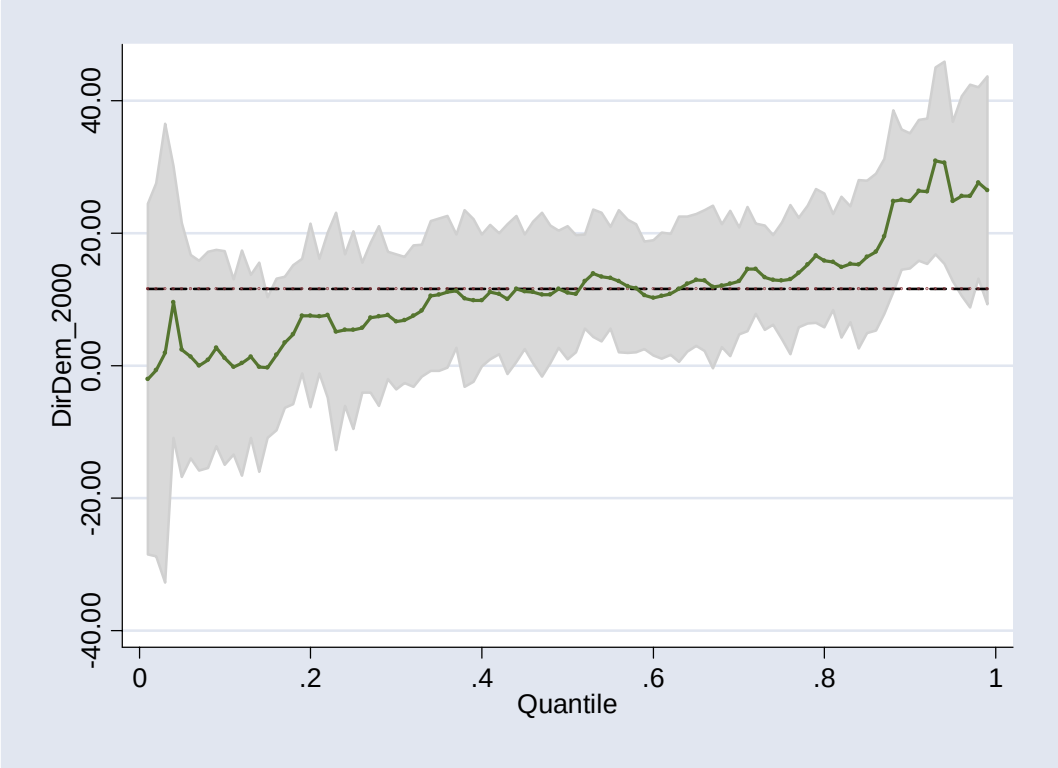

Figure 4: Mathematics; Structural Form

Figure 4 depicts the development of the size of the impact of direct democracy over the entire conditional test score distribution. As in the reduced form, there is a clear upward trend of the coefficient. The OLS coefficient crosses the ordinate at almost 12 test score points, and the continuous line starts with 1.10 in the 10th quantile and finishes with almost 25 in the 90th quantile.

As regards the remaining sociodemographic, peer and cantonal controls in the OLS model, again, female students perform significantly worse than their male peers, whereas the average students in selective schools do not perform significantly better than their peers in other school types. This result contrasts with the finding in the reduced model, again possibly

identifying the budgetary channel as the transmission channel. As in the reduced form, a performance enhancing impact of peer performance and a test score lowering influence of foreign peers can be observed. Again, the ratio of female peers does not appear to be a decisive factor.

Among the cantonal controls, the share of Protestants and of high school graduates still reveals an achievement enhancing impact, whereas the share of poor persons has become insignificant compared to the reduced model. As in the case for reading, the latter result indicates that the detrimental impact of poor persons occurs through the financial-budgetary channel of school expenses. This influence of the highly educated might indicate a preference for a better education in mathematics at the cantonal level. As in the case for reading, the inclusion of revenue-driven input variables makes the effect of the population disappear. The remaining cantonal predictors do not appear to be influential. As regards the revenue-driven input variables, influences similar to those obtained for reading are found; most particularly, the share of mathematics teachers with a tertiary education appears to have a test score raising influence on students, and the coefficient of the ratio of the tertiary staff in the teaching personnel is also positive and close to statistical significance for the average student. Moreover, no remaining input controls, including the equipment of schools with PCs, are of importance for the average student in mathematics. Again, the findings in the quantile regressions are very similar to those for the OLS. For more detailed estimation results, see the Appendix on mathematics.

Reflection on the findings for mathematics – particularly a comparison of the results for both forms of the model – leads to the conclusion that the insignificance of the coefficient of direct democracy in the reduced form must have been caused by the mutual cancellation of a positive (direct) influence and an assumedly negative one induced by the revenue-driven input factors. Hence, in the case of mathematics, the results reflect both a performance dampening indirect impact of direct democracy through the budget and an achievement raising direct impact. This latter finding could indicate that in more directly democratic cantons, the teaching of mathematics is favored. A second conclusion might be that fewer financial means at the subfederal level do matter for student performance. As the impact of the direct legislation changes through control of these inputs, which are also influenced by this institution, these inputs must play a decisive role in academic achievement.

Overall, the robustness tests corroborate the findings for the original specification quite well. In addition, imputation of the median to replace missing values increases the number of observations to roughly 10,000. Using several model specifications, the results are shown not alter considerably[258]. Again, in the reduced form, direct democracy does not appear to affect student performance considerably, while in the structural form it appears to be performance enhancing. Students in the 90th quantile of the conditional test score distribution particularly appear to profit from such a reallocation of given resources in mathematics.

5.2 Results for Natural Science

The Reduced Form

In the reduced form, the OLS estimate of the coefficient of direct democracy on adjusted student test scores indicates that for the average student from among the 1,410 students, there is no significant effect of direct legislation on student performance in natural science (see table 10.1 of the Appendix). The quantile regression estimation results show that the impact of direct democracy in the conditional test score distribution develops from a negative score for the lowest estimated quantile (-7.67) to a positive one (3.05) for the highest quantile. With respect to significance, only a statistically weak effect is found for the 25th quantile. As expected, the hypothesis of joint nonsignificance for the estimated percentiles cannot be rejected. As regards the impact of the remaining controls, the reader is referred to the Appendix for natural science (see table 10.2).

The graph for natural science (figure 5) illustrates the development of the influence of the index of direct democracy for the entire conditional test score distribution when revenue-driven inputs are not controlled for. Again, the dashed line represents the OLS estimate with a size of about -3, while the continuous line reveals the coefficients of the index of direct democracy for the various percentiles of the distribution. It is obvious that the impact of direct democracy never reaches a significance level of 5% or higher because the 95% confidence interval always intersects the 0-line. A Wald test for equality of all estimated quantiles leads

[258] In the reduced form, the OLS estimate is always insignificant for all specifications and sample sizes. In the structural form, exclusion of classes with less than twenty students again corroborates the main findings (about 2600 observations). The exclusion of classes with less than 10 students and the inclusion of a class size variable, which is highly significant (at the 0.1% level), also leads to identical results (5000 observations). The omission of class size, however, appears to cause an omitted variable bias which causes a decline in significance levels of the democracy variable (14.5% in the OLS regression, but still significant at the 10% level for the 50th and 75th quantiles) and a rejection of joint significance for the estimated quantiles.

to no rejection of the H_0-hypothesis[259]. Thus, both the graph and the tests support the observation that in the reduced form, the impact of direct democracy is not decisive for student performance in natural science. Further, this impact is identical in size for all students independent of their individual, peer, and family characteristics.

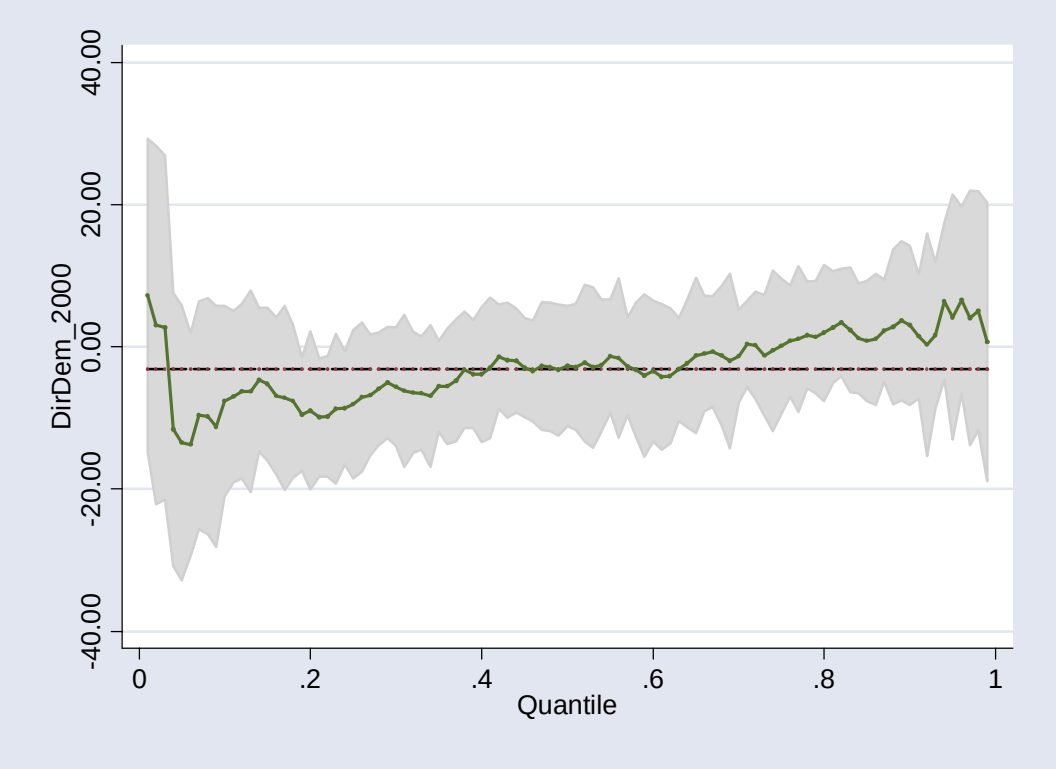

Figure 5: Natural Science; Reduced Form

As regards the impact of the sociodemographic, peer, and cantonal variables in the OLS model, one of the most interesting findings is probably the systematically worse performance of female students compared to their male peers. This observation is identical to that made for the mathematics results but, again, is in contrast to the outcome for reading. As already observed for mathematics and reading, selectivity is significantly associated with higher test scores. This impact of selectivity on natural science appears to be far stronger in both size and level of significance then that observed for mathematics. The peer effects for natural science mirror those for mathematics: again, more homogeneity in class performance influences the student test score positively, whereas the ratio of foreign peers exerts a (weakly) opposite influence, and the coefficient of the share of females is insignificant. With respect to the cantonal determinants of quality of education, an achievement increasing effect on the

[259] A pairwise Wald test for equality of the estimated quantiles reveals a weakly significant difference (at the 10% level) between the 25th quantile and the 90th quantile only.

average student is found for the share of Protestants and a performance lowering effect for the share of poor persons. Moreover, cantonal population appears influential. In addition, these results are similar to the findings for mathematics. In contrast to mathematics, however, no aggregate peer impact seems to exist for the share of highly educated persons, but a deleterious impact is observed for the share of people over 60. For natural science, quantile regression results are close to the findings in the OLS model. For more detailed regression output tables, the reader is referred to the Appendix for natural science (table 10.2).

The Structural Form

The structural form tests the institutional influence of direct democracy when revenue-driven inputs are controlled for. For an average student among the 1,100 test takers, the coefficient of the variable of interest is positive but insignificant (see table 10.1). The quantile regression results in the structural form for the estimated quantiles corroborate the OLS results by revealing no significant impact for any quantile (see table 10.3). In addition, the hypothesis of a joint significance for all estimated quantiles cannot be supported, which corroborates the observation that the institutional impact is negligible for the entire distribution. This result can be interpreted to mean that filtering out the influence through the subfederal budgetary channel reveals no impact beyond the one; that is, a reallocation of the means between students with differing levels of academic achievement.

As figure 6 shows, for the entire distribution, the coefficient of the institution of direct legislation fluctuates around the OLS estimate, which intersects the ordinate at 6.25. A test for equality of coefficients for the all estimated quantiles jointly shows that the effect of direct democracy is of equal size for all students[260]. Again, interested readers can find more detailed information on the remaining determinants in the Appendix (see table 10.3).

As regards the remaining sociodemographic, peer, and cantonal covariates in the OLS model, as already observed in the reduced form of the model, female student gender is associated with lower academic achievement. In contrast to the structural form result, the selectivity of school is found not to be influential. This observation is similar to that for mathematics. Again, it is conjectured that it is through the resource channel at the school and class level that selectivity unfolds its influence in the reduced model.

[260] In addition, a pairwise test for equality for the estimated percentiles was never rejected.

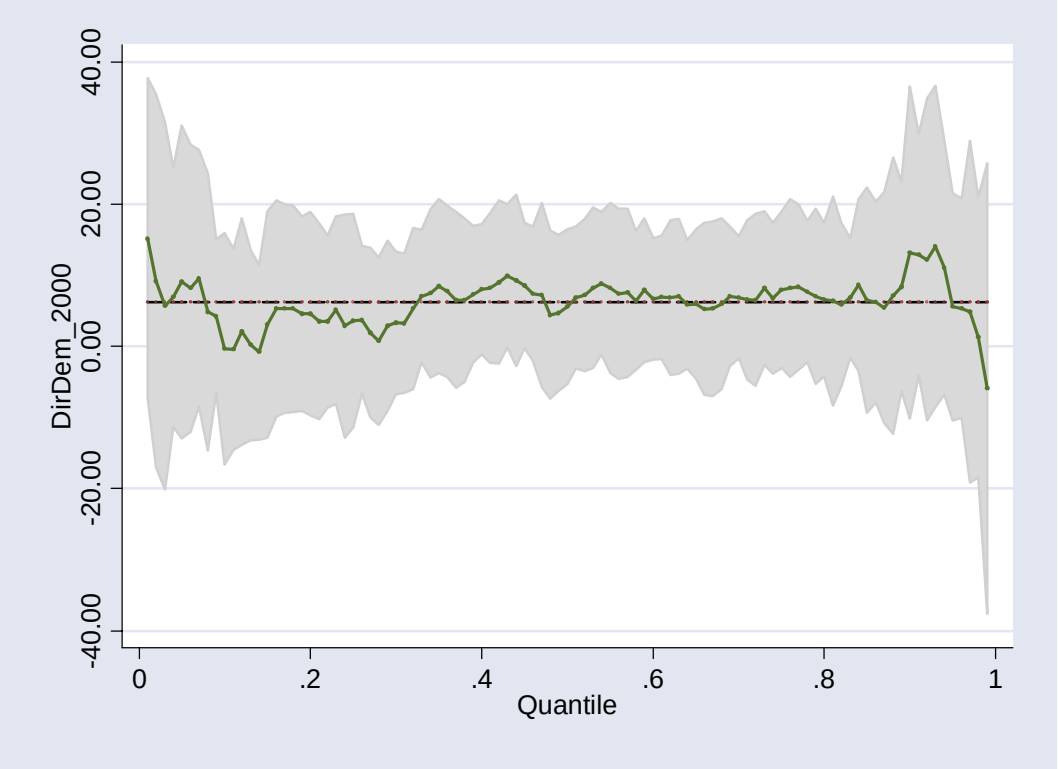

Figure 6: Natural Science; Structural Form

As regards peer effects, most interestingly, the impact of peer performance disappears in the structural form for natural science, which was never observed for reading or mathematics. Also in contrast, the share of female peers exerts a small but significant deleterious effect on the average student's test scores. As in mathematics but in contrast to reading, the share of foreign peers is also associated with lower academic achievement. In terms of the cantonal characteristics, a Protestant culture appears beneficial for students in natural science, an effect that is very similar to the one detected in the reduced model and for mathematics. Neither the share of well-educated persons nor of poor persons appears decisive. In the latter case, it can again be hypothesized that the breakdown of this link in the reduced form indicates that the poor persons' influence occurs through the budgetary channel. Additionally, this finding is in line with the results for mathematics. The share of people over 60, however, as already observed in the reduced form, still impacts students test scores significantly and in a deleterious way.

Patterns similar to those for reading and mathematics are also observed for some factors of the input endogenous variables that form part of the structural form. The qualification of (natural science) teachers appears to be a very decisive predictor of student performance. In contrast to reading and mathematics, however, a shortage of (natural science and overall) teachers also

appears to be influential in the expected direction, while more hours of schooling improves academic achievement. Further, the lack of instructional material appears to be strongly associated with lower test performance in natural science. As in mathematics but in contrast to reading, however, the equipment of the school with PCs is not an influential factor of test scores in natural science. Again, OLS estimates are very similar to the quantile regression results. For further information, the reader should consult table 10.3 of the Appendix.

To summarize the findings for natural science, institutions of direct legislation do not appear to influence student performance in any way. A comparison of the findings for the reduced and structural forms reveals no achievement decreasing impact through the budgetary channel in the reduced form, nor is any reallocative effect observable once revenue-driven inputs are controlled for. One possible interpretation is that in natural science, in contrast to reading and mathematics, there is not link between fewer subfederal means available for schooling, ultimately constrained by direct democracy, and the endogenous input variables which are influential for academic achievement - in other words: in natural science, there is no link between the equipment of school and direct democracy for students in the ninth grade. Moreover, because no significant institutional effect is observable even though the number of observed students for natural science is similar to the number of mathematics test-takers, it is unlikely that the observed insignificance is caused by a small sample size. Thus, it is concluded that stronger empowerment of the people has no effect whatsoever on student performance in natural science in the ninth grade.

Testing several model specifications through the inclusion and exclusion of particular covariates and varying the sample sizes reveals patterns similar to those observed in the original models and the 'original' sample size. In addition, imputation of the median to replace missing values and admission of class sizes in natural science of ten or more students, which increases the number of observations to roughly 4,700, do not alter the main findings considerably[261]. In general, the results support the finding that the institutional impact of direct democracy is negligible both in the reduced and structural form of the model.

[261] In the reduced form, the OLS estimate is insignificant, whereas for some quantiles a negative impact significant at the 10% level can be observed. In contrast to the OLS estimation, however, standard errors of the quantile regression estimates are not corrected by clustering of schools which leads to an inflation of significance levels. The hypothesis of a joint significance for the estimated quantiles, however, is rejected. Significance of the institutional coefficient(s) in the structural form was never found.

6 Comparison of the Results for Reading, Mathematics and Natural Science

The empirical results for reading, mathematics, and natural science reflect completely different findings and elicit interesting interpretations. In particular, differences occurred between mathematics and natural science even though both subjects were taken by a comparable number of students and despite the fact that both subjects are linked to logical thinking. These results are discussed in more detail below with respect to the direct and indirect impact of direct democracy on overall student performance in a subject, the allocation of resources within a class for a particular subject, and, finally, the allocation of given resources between subjects.

For both reading and mathematics, the indirect effect of direct legislation appears to be test score lowering, and for both subjects, the lack of available financial means for public education at the subfederal level appears responsible. This finding is in line with the findings of similar studies for the U.S. In addition, these results indicate that equipping schools with physical and human capital resources is important for student performance in these two subjects. This result contradicts empirical literature by education economists who found no link between school resources and student performance[262]. Such an observation, however, is not made for natural science, possibly because, in contrast to the mathematics and reading taught from the very beginning of compulsory schooling, physics and chemistry are introduced into the school curricula only in higher grades[263].

No reallocative impact *within the same subject* through institutions of direct legislation on student performance is detected for reading and natural science. For mathematics, however, direct legislation is observed to cause a reallocation of available resources, one that strongly favors students in the top of the conditional test score distribution (90th quantile). This finding contrasts with what could be expected if the median voter's child were in the median portion of the conditional test score distribution. It would, however, support this theory if the preferences of parents of outstanding performers were such that they demanded greater effort in public education for their child than was being demanded by classmates' parents.

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[262] This debate was launched by the review of HANUSHEK (1986) and his various later analyses (e.g. HANUSHEK et al. 1996) who found no relationship. Contradictory results are obtained, however, by more recent studies (GRADDY and STEVENS 2003; PAN et al. 2003a, 2003b) and the new evidence cited there.

[263] Depending on the canton, physics and chemistry are introduced either in the 7th or 8th grade.

Regarding the direct impact of direct democracy on student test scores, the findings indicate a performance enhancing effect for mathematics and an insignificant one for reading and natural science. This result is completely in contrast to the performance lowering influence of tax limits detected when endogenous input variables are controlled for. This observation may be related to the finding by DOWNES et al (1998) of a deleterious effect for both the reduced and the structural form of the educational production function[264].

A conjecture about reallocation *between test subjects* is possible if the results in the structural forms for several test subjects are compared. Based on such a comparison, it might be concluded that a reallocation of given means between school subjects, particularly a shift of given resources to mathematics, could have occurred. However, given that the regression results reflect only a small portion of the school curriculum, it is unclear whether the additional resources for mathematics have been withdrawn from these two specific school subjects or from other school subjects. What is clear, however, is that this reallocation is to the advantage of mathematics in more directly democratic cantons.

7 Conclusion

For the U.S., FIGLIO (1997) shows that local tax limits which reduced local revenue and spending lead to larger class sizes, lower teacher wages, and worse teacher quality but not to any reduction in administrative personnel. These results suggest that instead of the expected efficiency gains in the provision of this public good of schooling, a decline in the quality of educational service must have occurred. This decline in academic achievement is indeed reflected and corroborated by recent analyses of the impact of tax limits on student performances in the U.S. (e.g. FIGLIO and RUEBEN 2001). Such tax limits were demanded by and implemented through citizen's initiatives, an institution of direct legislation, in various U.S. states. Based on these results for the U.S., a test score decreasing impact of direct democracy on student achievement would not be surprising for Switzerland, as its political institutions have been shown to restrain subfederal expenses for schooling.

This study aims to provide such evidence for similar or dissimilar effects of direct legislation on public education in Switzerland. Using a cross section of individual data on student

[264] The endogenous input factors are student-teacher ratios in the district, mean teaching experience of teachers, and the share of highly qualified teachers.

performance in Switzerland obtained from a national study accompanying the OECD-PISA study, an educational production function augmented by institutional determinants of direct democracy is estimated. This model is similar to those employed both in public finance studies and in analyses by educational economists. Two major variations of the production function are estimated: first, a reduced form that excludes endogenous input factors at the school and class level (the reduced form) and second, a structural version that includes these factors (the structural form). This model specification makes it possible to distinguish the direct institutional impact from an indirect impact. Employed as a measure of direct democracy is a composite index that indicates the degree of overall empowerment of citizenry.

In the reduced form of the model, the findings from the OLS regression indicate that a higher degree of direct democracy leads to lower performance by students in reading, but does not affect mathematics and natural science. The quantile regression analysis shows a conspicuous performance reducing effect for direct democracy across the entire conditional test score distribution in reading, which is equally strong for the estimated quantiles. However, after the inclusion of variables controlling for various revenue-driven input factors at the school and class level, the negative influence of direct democracy disappears completely for reading, whereas a positive one emerges for mathematics. From this result, it is concluded that school and class input factors whose quality is dependent on the school district's financial equipment are important for student academic achievement in mathematics and reading. Moreover, this finding indicates that the test score lowering impact occurs through the subfederal budget, which is very similar to results obtained for the U.S. Finally, the estimation outcomes for the structural form also suggest that (a) there is no institutional influence on the distribution of given resources among students in the same class in reading and natural science, but in mathematics, and (b) there exists no deleterious effect that goes beyond the purely budgetary impact for reading and mathematics. In case of natural science, no budgetary impact seems present at all. This last outcomes contradict the U.S. results in which a performance lowering impact in the structural form of the model is also found for various test subjects.

Future research should address the impact of direct democracy at the school district level because communal governments have a substantial say in schooling issues and financing in Switzerland. In addition, selection into treatment at the school district level should be taken into account. In terms of the primary research question addressed in this chapter, the

application of a standard methodological approach from the field of public finance reveals that direct democracy has a substantial impact on public education, particularly on Swiss student performance in reading. Based on the empirical results presented in this chapter, the Swiss electorate is advised to avoid further cuts in spending for public education and to increase the share of teachers with a tertiary education in their schools.

8 Appendix: Reading

Table 8.1: OLS Regressions for Reading

	Reduced Form		Structural Form	
	Coeff.	t	Coeff.	t
Direct democracy	-7.595*	-1.98	-0.695	-0.16
Latin region	1.073	0.09	8.579	0.69
hisei2	4.497	0.54	5.974	0.72
hisei3	18.009*	2.52	15.840*	2.12
hisei4	15.707*	2.13	13.714(*)	1.86
hisei5	30.095*	3.25	31.338***	3.34
hisei6	26.767***	3.37	25.991**	3.22
hisei7	34.188***	3.54	31.298**	3.12
No income data	0.535	0.07	-2.335	-0.28
Number of siblings	-0.774**	-2.64	-0.815**	-2.61
Old student	-28.223***	-3.23	-27.748**	-2.69
Young student	11.081***	3.67	10.715***	3.43
Books at home	9.075***	10.58	8.574***	9.26
No late arrival	2.263	0.76	2.962	0.95
No PC at home	-15.107***	-3.30	-13.151**	-2.70
Female	17.610***	7.68	15.878***	6.65
Both parents work	0.096	0.04	-0.913	-0.37
Intact family	0.466	0.15	0.117	0.03
Native	2.198	0.41	0.211	0.04
Foreign parents	-6.019	-1.23	-5.468	-1.02
Second generation	2.575	0.79	3.234	0.88
Non-test language	-14.755**	-3.15	-17.529***	-3.44
Parents low education	-11.286*	-2.18	-8.318	-1.60
Parents medium education	-13.724***	-4.69	-11.770***	-3.87
Mother tertiary education	-11.794***	-3.70	-11.589***	-3.44
Father tertiary education	-2.997	-1.05	-1.147	-0.38
Discuss politics	8.129**	2.61	8.922**	2.70
Listen to music	-5.950	-0.99	-3.936	-0.64
Discuss performance	-8.861***	-3.49	-8.975***	-3.49
Main meal	1.763	0.32	1.832	0.31
Regular talking	5.752*	2.06	5.982*	2.02
Village school	-4.811	-0.46	-3.071	-0.29
Small town school	-3.694	-0.68	0.790	0.12

Table 8.1: OLS Regressions for Reading (cont.)

	Reduced Form		Structural Form	
	Coeff.	t	Coeff.	t
City school	12.025	1.29	2.897	0.26
Private school	1.876	0.24	-0.996	-0.09
Selective school	13.290**	2.84	5.960	1.18
Regular tests	5.652	0.99	4.813	0.80
Homework feedback	-13.482***	-4.56	-11.524***	-3.84
Problem discipline	-12.062***	-3.83	-12.061***	-3.93
Coefficient reading	7.391***	4.26	5.597**	2.90
Ratio of female	0.135	0.57	-0.043	-0.16
Ratio of foreign peers	-0.653	-1.50	-0.587	-1.23
High education	1.539	1.47	1.332	1.14
Old people	-0.284	-0.13	-2.324	-0.99
Unemployment rate	7.160	0.83	5.722	0.58
Protestants	0.560	1.49	0.742(*)	1.74
Muslims	2.209	0.71	1.292	0.37
No religion	-0.943	-0.72	0.419	0.28
Poor persons	-2.214***	-4.16	-1.668*	-2.39
Urbanization	-0.200	-0.62	-0.322	-0.78
Log (population)	-25.102**	-2.69	-15.487	-1.34
Poor conditions 1			0.287	0.03
Poor conditions 2			-9.700	-0.77
No PC at school			-7.158*	-2.32
Shortage teacher			-2.735	-0.24
Tertiary reading			20.146(*)	1.97
Tertiary staff			25.380*	2.15
Total hours			0.030	0.89
Student-teacher ratio			-0.495	-0.79
Constant	746.308***	6.77	597.282***	4.38
F-Test	29.06		28.38	
Adjusted R^2	0.26		0.26	
Number of observations	3411		2969	

OLS Regression with robust standard errors obtained through clustering of schools (176/149 schools). Observations with missing values and a class size of less than 20 have been deleted.

Table 8.2: Quantile Regression for Reading without Revenue-Driven Inputs (Reduced Form)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
Direct democracy	-7.141(*)	-1.82	-5.865(*)	-1.96	-7.964**	-2.82	-8.903**	-3.01	-6.167(*)	-1.70
Latin region	22.213	1.56	14.442	1.46	-8.798	-0.99	-9.811	-1.05	-8.472	-0.74
hisei2	-0.232	-0.02	-0.408	-0.03	5.007	0.51	6.468	0.50	4.014	0.28
hisei3	26.436*	2.08	26.441*	2.47	16.123(*)	1.75	11.487	0.96	0.180	0.01
hisei4	22.743(*)	1.81	20.393(*)	1.86	13.657	1.47	12.236	1.04	-0.536	-0.04
hisei5	34.291*	2.30	24.236*	2.03	21.543(*)	1.85	24.805(*)	1.77	33.050(*)	1.89
hisei6	37.234**	2.98	28.497**	2.60	17.563(*)	1.80	20.244(*)	1.67	24.844(*)	1.75
hisei7	37.512*	2.10	43.201***	3.50	27.437*	2.36	24.965(*)	1.82	17.616	1.10
No income data	2.338	0.17	6.363	0.53	0.403	0.04	0.191**	0.01	-3.395	-0.23
Number of siblings	-0.477	-0.96	-0.370	-0.80	-0.819(*)	-1.92	-1.075***	-3.55	-1.581**	-3.13
Old student	-35.165**	-2.66	-37.738***	-3.19	-26.379*	-2.32	-18.025	-1.57	-24.077*	-2.13
Young student	14.250**	2.78	9.677*	2.31	11.292**	3.11	13.785***	3.43	12.441*	2.47
Books at home	8.409***	5.05	8.870***	6.86	9.005***	7.67	9.603***	8.28	10.063***	8.26
No late arrival	2.386	0.49	4.405	1.12	-0.815	-0.23	-3.279	-0.98	0.322	0.08
No PC at home	-11.906	-1.29	-12.282*	-2.04	-12.134(*)	-1.85	-10.832(*)	-1.72	-9.469	-1.43
Female	21.619***	4.65	17.007***	5.22	17.446***	5.56	12.901***	4.06	17.228***	4.58
Both parents work	-1.729	-0.39	4.949	1.43	1.935	0.62	1.464	0.43	0.523	0.13
Intact family	8.657	1.31	1.722	0.36	-0.235	-0.05	-4.204	-0.93	-2.206	-0.44
Native	7.340	0.78	0.210	0.03	-4.748	-0.81	2.005	0.31	8.614	1.06
Foreign parents	-5.619	-0.68	-9.580	-1.40	-9.848	-1.53	-5.638	-0.77	3.619	0.48
Second generation	13.700*	2.42	2.874	0.68	2.947	0.70	1.051	0.26	-5.988	-1.28

Table 8.2: Quantile Regression for Reading without Revenue-Driven Inputs (Reduced Form) (cont.)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
Non-test language	-16.348(*)	-1.77	-12.174(*)	-1.82	-11.120(*)	-1.93	-14.362*	-2.49	-13.413(*)	-1.88
Parents low education	-3.668	-0.43	-10.496	-1.59	-13.301*	-2.12	-10.272	-1.53	-13.848(*)	-1.68
Parents medium education	-15.970**	-3.08	-15.484***	-4.04	-15.510***	-4.49	-12.511***	-3.43	-7.220(*)	-1.65
Mother tertiary education	-13.246*	-2.15	-8.288(*)	-1.83	-10.942**	-2.69	-11.590**	-2.93	-10.639(*)	-1.93
Father tertiary education	-5.393	-1.05	-3.238	-0.76	-2.068	-0.56	-5.061	-1.35	-5.281	-1.06
Discuss politics	12.842*	2.02	5.758	1.25	7.709	1.56	9.862*	2.15	12.506*	2.04
Listen to music	6.068	0.53	-7.995	-0.86	-9.145	-1.02	-8.141	-1.03	-10.694	-0.83
Discuss performance	-14.802**	-3.00	-6.992*	-2.04	-8.589**	-2.63	-7.321*	-2.12	-6.968(*)	-1.79
Main meal	17.790(*)	1.92	5.006	0.52	-2.303	-0.43	1.603	0.22	-6.203	-0.76
Regular talking	4.435	0.92	7.441*	1.98	3.367	0.95	4.456	1.27	3.228	0.78
Village school	-5.047	-0.46	-9.265	-1.18	-11.108	-1.44	8.274	0.95	4.132	0.51
Small town school	-0.026	0.00	-2.290	-0.54	-2.122	-0.57	-2.717	-0.66	-6.062	-1.23
City school	25.348*	2.13	19.234*	2.42	7.168	1.17	9.837	1.28	3.415	0.41
Private school	-0.244	-0.02	2.366	0.26	5.334	0.79	5.155	0.71	5.462	0.64
Selective school	15.652***	3.29	14.758***	4.20	10.898**	2.96	10.871**	2.83	11.743**	2.69
Regular tests	-2.801	-0.46	4.838	0.98	3.728	0.73	5.985	1.24	11.859(*)	1.93
Homework feedback	-11.686*	-2.35	-6.567	-1.60	-11.062***	-3.51	-10.289**	-2.72	-12.762**	-3.15
Problem discipline	-13.087**	-2.93	-11.285**	-3.13	-9.638**	-2.94	-12.329***	-3.40	-18.062***	-4.33
Coefficient reading	7.367***	4.67	6.920***	6.07	8.121***	7.68	7.361***	6.59	5.955***	4.43
Ratio of female	0.264	0.57	-0.060	-0.31	0.130	0.72	0.419*	2.16	0.306	1.41
Ratio of foreign peers	-1.281***	-3.88	-0.986***	-3.50	-0.394	-1.59	-0.234	-0.77	-0.182	-0.54

Table 8.2: Quantile Regression for Reading without Revenue-Driven Inputs (Reduced Form) (cont.)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
High education	1.668	1.26	1.382	1.35	1.503	1.58	1.236	1.42	0.691	0.63
Old people	-0.207	-0.07	-2.435	-1.17	0.622	0.28	-0.025	-0.01	-0.781	-0.27
Unemployment	13.934	1.45	5.138	0.70	7.941	1.32	7.361	1.02	15.673(*)	1.85
Protestant	0.827(*)	1.94	0.597(*)	1.86	0.368	1.23	0.833**	2.85	1.246***	3.95
Muslim	3.330	0.95	2.682	0.95	1.813	0.71	3.420	1.46	3.235	1.14
No religion	-0.112	-0.07	-1.467	-1.17	-1.252	-1.09	-1.483	-1.52	-1.204	-0.98
Poor	-3.756***	-5.01	-1.994***	-3.59	-1.822***	-3.45	-1.931**	-3.14	-2.504***	-3.86
Urbanization	-0.558	-1.12	0.140	0.41	-0.186	-0.61	-0.115	-0.41	-0.223	-0.60
Log (population)	-29.095**	-2.61	-27.056**	-2.70	-23.843**	-2.78	-30.616***	-3.97	-32.067***	-3.61
Constant	702.680***	5.37	736.512***	6.56	736.482***	7.73	847.780***	8.87	919.927***	8.08
Pseudo R^2	0.20		0.16		0.14		0.13		0.16	

Quantile regression for the 10th, the 25th, the 50th, the 75th, and the 90th quantiles. Standard errors are bootstrapped (1,000 replications). Observations with missing values and with a class size of less than 20 students have been deleted. 3,411 observations.

Table 8.3: Quantile Regression for Reading Including Revenue-Driven Inputs (Structural Form)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
Direct democracy	4.168	0.83	-0.001	0.00	-1.759	-0.53	0.966	0.29	-4.620	-1.00
Latin region	27.427	1.55	6.609	0.63	6.735	0.62	4.869	0.43	-1.190	-0.09
hisei2	7.893	0.50	7.497	0.57	5.858	0.55	10.464	0.76	7.946	0.51
hisei3	28.115(*)	1.89	23.921*	1.98	11.883	1.18	12.296	0.94	2.950	0.20
hisei4	25.477	1.63	17.758	1.41	9.892	0.98	14.638	1.14	4.918	0.34
hisei5	38.552*	2.12	29.371*	2.15	17.447	1.42	32.093*	2.17	42.113*	2.13
hisei6	37.487*	2.43	28.277*	2.25	17.409(*)	1.65	24.104(*)	1.85	31.643*	2.09
hisei7	32.207(*)	1.65	35.223*	2.30	18.777	1.48	34.275*	2.24	30.750(*)	1.68
No income data	-2.169	-0.13	0.168	0.01	-1.231	-0.11	5.511	0.38	-0.569	-0.04
Number of siblings	-0.889	-1.48	-0.628	-1.14	-0.428	-1.06	-1.315***	-3.98	-1.935***	-3.49
Old student	-42.0783	-2.33	-27.080(*)	-1.91	-30.785*	-2.48	-26.527(*)	-1.95	-25.361(*)	-1.83
Young student	9.192(*)	1.68	6.109	1.45	9.328*	2.34	10.870*	2.52	12.325*	2.19
Books at home	7.394***	4.01	8.119***	6.02	9.479***	7.38	9.534***	8.16	10.067***	6.90
No late arrival	9.956(*)	1.93	5.926	1.29	-1.264	-0.31	-4.090	-1.06	-0.075	-0.02
No PC at home	-9.467	-0.93	-11.243	-1.58	-12.416(*)	-1.75	-12.269(*)	-1.81	-7.440	-0.93
Female	17.176***	3.83	16.597***	4.59	14.939***	4.29	11.754***	3.27	17.776***	4.28
Both parents work	-2.909	-0.61	3.225	0.83	-0.431	-0.13	-0.581	-0.15	1.538	0.34
Intact family	9.881	1.45	2.868	0.53	-1.373	-0.28	-4.119	-0.79	-0.230	-0.04
Native	-0.156	-0.02	1.959	0.24	-2.173	-0.33	0.296	0.04	5.672	0.61
Foreign parents	-8.527	-0.88	-4.008	-0.52	-4.260	-0.56	-6.747	-0.82	1.124	0.12
Second generation	11.637*	1.98	5.297	1.03	5.650	1.26	2.239	0.46	-3.170	-0.58

Table 8.3: Quantile Regression for Reading Including Revenue-Driven Inputs (Structural Form) (cont.)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
Non-test language	-19.271*	-2.14	-14.434(*)	-1.89	-13.200*	-2.10	-13.893*	-2.14	-14.858*	-2.03
Parents low education	-0.893	-0.09	-10.193	-1.46	-10.526	-1.56	-9.948	-1.39	-12.256	-1.40
Parents medium education	-13.458*	-2.44	-12.419**	-3.04	-9.051*	-2.27	-12.825**	-3.06	-4.227	-0.87
Mother tertiary education	-10.485(*)	-1.69	-11.555*	-2.11	-10.494*	-2.42	-13.057**	-2.89	-12.876*	-2.22
Father tertiary education	-6.425	-1.18	-0.554	-0.12	2.173	0.53	-4.974	-1.19	-4.375	-0.82
Discuss politics	16.077**	2.56	8.338	1.54	9.177(*)	1.68	11.014*	2.41	13.576*	1.96
Listen to music	6.285	0.55	-5.897	-0.61	-5.519	-0.65	-6.637	-0.82	-13.205	-0.95
Discuss performance	-14.163**	-3.01	-8.236*	-2.24	-7.066*	-2.00	-7.280(*)	-1.90	-5.507	-1.23
Main meal	11.101	1.14	9.395	1.02	-8.204	-1.27	4.872	0.65	-0.658	-0.06
Regular talking	4.931	1.01	4.895	1.25	3.773	1.05	4.202	1.13	2.677	0.59
Village school	-2.491	-0.23	-14.228	-1.60	-9.416	-1.11	14.142	1.56	12.378	1.36
Small town school	5.228	0.81	2.581	0.52	0.053	0.01	7.015	1.26	-1.274	-0.20
City school	5.379	0.38	6.971	0.65	-2.643	-0.35	-2.568	-0.29	1.714	0.17
Private school	-4.807	-0.34	5.044	0.40	1.904	0.24	-11.225	-1.19	4.288	0.36
Selective school	9.208	1.64	7.531	1.63	2.560	0.58	-0.157	-0.03	7.725	1.27
Regular tests	-1.961	-0.27	3.118	0.62	5.072	0.91	4.426	0.78	8.797	1.14
Homework feedback	-11.331*	-2.34	-5.499	-1.29	-10.354**	-2.97	-5.626	-1.41	-13.749**	-2.96
Problem discipline	-16.128*	-3.56	-11.503**	-2.99	-12.732***	-3.60	-11.502**	-3.11	-19.205***	-4.38
Coefficient reading	5.875**	3.02	5.416***	3.90	5.975***	4.51	6.438***	4.63	5.918***	3.78
Ratio of female	-0.054	-0.18	0.051	0.22	-0.259	-1.24	0.129	0.56	0.122	0.46
Ratio of foreign peers	-0.789(*)	-1.87	-0.876**	-2.72	-0.686*	-2.19	0.226	0.58	0.008	0.02

Table 8.3: Quantile Regression for Reading Including Revenue-Driven Inputs (Structural Form) (cont.)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
High education	0.278	0.18	0.738	0.63	1.423	1.31	1.719	1.64	0.769	0.67
Old people	-1.393	-0.50	-4.136(*)	-1.74	-1.470	-0.62	-1.303	-0.49	-2.146	-0.68
Unemployment	19.306(*)	1.73	9.140	1.19	5.206	0.65	2.054	0.23	11.415	1.17
Protestant	0.879*	1.96	0.489	1.49	0.833*	2.29	1.063**	3.17	1.389***	3.52
Muslim	-0.088	-0.02	-1.883	-0.66	2.702	0.89	3.239	1.19	6.744(*)	1.79
No religion	2.655	1.51	1.097	0.79	-0.272	-0.21	-1.651	-1.45	-1.347	-0.90
Poor	-3.160***	-3.58	-1.321*	-2.20	-1.445*	-2.12	-1.315(*)	-1.85	-1.687(*)	-1.93
Urbanization	-1.053(*)	-1.89	-0.338	-0.83	-0.180	-0.54	0.119	0.34	-0.050	-0.12
Log (population)	-10.315	-0.79	-7.655	-0.77	-21.362*	-2.06	-25.664**	-2.99	-33.117**	-2.90
Poor conditions 1	9.562	0.89	0.736	0.08	8.175	0.94	-4.365	-0.53	-12.415	-1.31
Poor conditions 2	-13.062	-0.79	-6.122	-0.49	-9.447	-0.79	-9.785	-0.76	-13.771	-0.86
No PC at school	-9.800(*)	-1.65	-8.881*	-1.99	-1.966	-0.47	-4.638	-1.06	-7.881	-1.43
Teacher shortage	-17.794	-1.53	-10.907	-1.23	5.125	0.71	8.580	1.07	7.948	0.71
Tertiary reading	23.746(*)	1.92	26.727**	3.14	21.799**	2.56	13.595(*)	1.73	11.069	1.05
Tertiary staff	29.565*	2.10	21.325(*)	1.82	26.327**	2.58	31.932**	3.10	15.983	1.40
Total hours	0.030	0.70	0.058(*)	1.86	0.028	1.03	-0.006	-0.17	0.004	0.12
Student-teacher ratio	-0.750	-0.94	-0.972	-1.40	-0.401	-0.69	-1.056	-1.46	0.904	0.98
Constant	433.718**	2.79	453.589***	3.76	673.089***	5.35	743.483***	6.70	880.562***	6.31
Pseudo R^2	0.22		0.17		0.14		0.14		0.16	

Quantile regression for the 10th, the 25th, the 50th, the 75th, and the 90th quantiles Standard errors are bootstrapped (1000 replications). Observations with missing values and with a class size of less than 20 students have been deleted. 2,969 observations.

9 Appendix: Mathematics

Table 9.1: OLS Regression for Mathematics

	Reduced Form		Structural Form	
	Coeff.	t	Coeff.	t
Direct democracy	2.220	0.52	11.582*	2.33
Latin region	26.780*	1.99	37.743*	2.56
hisei2	-1.501	-0.14	-4.443	-0.41
hisei3	12.751	1.25	6.534	0.60
hisei4	0.267	0.03	-4.423	-0.43
hisei5	9.950	0.94	7.288	0.65
hisei6	12.849	1.28	8.314	0.80
hisei7	27.998*	2.23	27.284*	2.15
No income data	-9.630	-0.84	-15.622	-1.34
Number of siblings	-0.232	-0.53	0.094	0.21
Old student	-24.749*	-2.35	-25.151*	-2.16
Young student	6.849	1.55	7.265	1.57
Books at home	8.890***	8.17	8.540***	7.12
No late arrival	4.280	1.09	5.301	1.18
No PC at home	-22.038**	-2.81	-16.707(*)	-1.88
Female	-27.889***	-8.43	-28.822***	-8.38
Both parents work	0.963	0.26	-1.684	-0.42
Intact family	2.926	0.59	6.075	1.17
Native	-2.324	-0.33	1.119	0.15
Foreign parents	-15.773*	-2.25	-10.107	-1.35
Second generation	-4.854	-0.96	-4.794	-0.92
Non-test language	-16.148*	-2.16	-15.995(*)	-1.91
Parents low education	-3.878	-0.51	-6.177	-0.74
Parents medium education	-13.288***	-3.42	-12.330**	-3.04
Mother tertiary education	-8.908	-1.64	-8.060	-1.43
Father tertiary education	-3.177	-0.70	-4.284	-0.89
Discuss politics	2.099	0.46	2.555	0.50
Listen to music	-8.093	-0.88	-9.065	-0.95
Discuss performance	-10.117**	-2.73	-7.556(*)	-1.86
Main meal	6.685	0.94	8.208	1.04
Regular talking	3.720	0.97	2.343	0.59
Village school	-32.619**	-2.97	-25.467**	-2.74
Small town school	-10.232	-1.52	-6.170	-0.94

Table 9.1: OLS Regression for Mathematics (cont.)

	Reduced Form		Structural Form	
	Coeff.	t	Coeff.	t
City school	5.480	0.45	1.905	0.12
Private school	-7.076	-0.72	-5.211	-0.60
Selective school	9.123(*)	1.69	3.302	0.69
Regular tests	2.972	0.54	-1.391	-0.24
Homework feedback	-20.408***	-4.70	-19.054***	-4.15
Discipline problem	-2.706	-0.63	-5.117	-1.19
Coefficient mathematics	3.642*	2.34	4.220***	3.24
Ratio of female peers	0.222	0.81	-0.093	-0.37
Ratio of foreign peers	-1.141**	-2.68	-1.223*	-2.56
High education	2.844*	2.10	3.392*	2.59
Old people	0.465	0.13	-3.210	-0.91
Unemployment rate	6.234	0.68	3.254	0.32
Protestants	0.805(*)	1.88	1.064*	2.53
Muslims	2.602	0.67	-0.520	-0.13
No religion	-1.995	-1.24	-0.525	-0.30
Poor persons	-2.270**	-2.91	-1.488	-1.57
Urbanization	0.068	0.15	0.120	0.24
Log (population)	-19.773(*)	-1.72	-9.486	-0.78
Poor conditions 1			3.075	0.23
Poor conditions 2			-2.871	-0.24
No PC at school			-5.650	-1.18
Shortage teacher			-1.981	-0.16
Tertiary mathematics			23.373*	2.21
Tertiary staff			20.806	1.59
Total hours			0.071(*)	1.86
Student-teacher ratio			-0.158	-0.24
Constant	678.331***	5.06	452.594**	3.16
F-Test	12.98		15.97	
Adjusted R^2	0.20		0.23	
Number of Observations	1846		1596	

OLS Regression with robust standard errors obtained through clustering by schools (166/141 schools). Observations with missing values and a class size of less than 20 have been deleted.

Table 9.2: Quantile Regression for Mathematics without Revenue-Driven Inputs (Reduced Form)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
Direct democracy	-6.182	-1.10	1.270	0.27	3.032	0.86	5.675	1.59	7.969	1.63
Latin region	7.884	0.44	24.168(*)	1.77	34.891**	2.86	37.471***	3.18	33.240*	2.13
hisei2	-0.964	-0.04	5.178	0.35	7.650	0.53	-0.641	-0.04	-16.266	-0.85
hisei3	22.655	0.91	19.985	1.60	14.063	1.03	13.193	0.85	6.303	0.34
hisei4	10.859	0.44	-1.142	-0.08	0.269	0.02	-1.800	-0.11	-10.769	-0.54
hisei5	11.636	0.42	6.807	0.37	11.717	0.70	10.734	0.60	-5.614	-0.25
hisei6	19.262	0.77	9.122	0.66	11.834	0.80	20.576	1.27	8.220	0.42
hisei7	43.173	1.55	24.552	1.43	33.660(*)	1.93	23.963	1.39	1.766	0.07
No income data	-8.870	-0.34	-10.062	-0.66	-4.985	-0.31	-5.159	-0.30	-10.362	-0.46
Number of siblings	-0.145	-0.17	0.034	0.06	-0.483	-0.97	-0.362	-0.51	-0.452	-0.58
Old student	-25.482	-1.30	-34.568*	-2.15	-18.275	-1.05	-32.452**	-2.71	-53.650**	-3.15
Young student	8.552	1.03	8.484	1.38	3.154	0.59	4.481	0.72	5.891	0.77
Books at home	6.911**	2.74	6.255***	3.30	9.949***	6.08	8.766***	5.23	10.324***	4.37
No late arrival	7.460	1.07	4.696	0.80	6.832	1.33	4.443	0.88	2.808	0.39
No PC at home	-34.039*	-2.38	-31.027**	-2.70	-15.052(*)	-1.81	-17.873(*)	-1.90	-12.300	-0.96
Female	-38.814***	-6.50	-32.787***	-6.86	-27.603***	-6.10	-18.859***	-4.15	-20.438**	-3.11
Both parents work	5.226	0.74	1.817	0.34	1.650	0.34	-0.232	-0.05	0.107	0.02
Intact family	9.765	1.09	6.194	0.74	-2.499	-0.40	-5.628	-0.79	-5.865	-0.66
Native	0.415	0.03	-6.652	-0.54	-3.014	-0.33	1.114	0.12	-12.847	-0.91
Foreign parents	-13.902	-1.04	-15.096	-1.38	-23.312**	-2.65	-4.503	-0.47	-18.860(*)	-1.75
Second generation	-5.344	-0.54	-1.293	-0.17	-4.966	-0.76	-0.788	-0.12	-5.030	-0.66

Table 9.2: Quantile Regression for Mathematics without Revenue-Driven Inputs (Reduced Form) (cont.)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
Non-test language	-21.235	-1.59	-21.178*	-2.07	-10.553	-1.25	-17.477(*)	-1.81	-9.212	-0.90
Parents low education	7.269	0.59	1.484	0.15	-9.782	-1.10	-17.951*	-2.00	-6.731	-0.48
Parents medium education	-20.587**	-2.98	-16.337**	-2.87	-14.837**	-2.78	-5.020	-0.94	-3.287	-0.45
Mother tertiary education	-21.731**	-2.58	-7.119	-0.91	-3.255	-0.45	-10.308	-1.63	-4.374	-0.47
Father tertiary education	-5.650	-0.80	0.219	0.03	-5.402	-0.95	1.802	0.32	5.085	0.65
Discuss politics	0.115	0.01	3.489	0.43	0.230	0.03	4.458	0.68	-0.680	-0.08
Listen to music	2.007	0.10	-8.128	-0.61	-17.842	-1.28	-10.312	-0.87	-13.345	-0.85
Discuss performance	-7.167	-1.00	-11.957*	-2.19	-11.636*	-2.38	-12.753**	-2.74	-17.779**	-2.74
Main meal	17.957	1.33	11.982	1.12	10.113	1.17	3.942	0.40	5.239	0.41
Regular talking	-0.250	-0.04	10.631	1.88	4.115	0.79	1.107	0.22	1.189	0.17
Village school	-34.066*	-2.18	-30.874**	-2.62	-31.713**	-2.87	-14.331	-1.45	-40.886***	-3.48
Small town school	-10.461	-1.32	-11.061(*)	-1.79	-8.522(*)	-1.68	-9.184(*)	-1.71	-13.958	-1.64
City school	-0.478	-0.02	17.551	1.61	10.231	0.92	-0.143	-0.01	-15.060	-1.19
Private school	16.106	1.20	3.584	0.33	-15.419	-1.49	-12.129	-0.94	-1.415	-0.11
Selective school	2.047	0.29	9.964(*)	1.66	13.453**	2.69	10.145	1.79	-1.579	-0.23
Regular tests	-13.185	-1.53	-4.249	-0.59	9.971	1.61	7.114	0.99	7.158	0.67
Homework feedback	-15.988*	-2.08	-21.009***	-3.70	-21.045***	-3.92	-18.326***	-3.49	-15.133*	-2.15
Problem discipline	2.951	0.42	-1.940	-0.34	-2.548	-0.52	-6.117	-1.29	-12.677*	-2.08
Coefficient mathematics	3.224*	2.06	3.133*	2.28	3.449**	2.72	5.849***	4.23	6.084**	2.77
Ratio of female peers	-0.046	-0.13	0.345	1.18	0.458(*)	1.68	0.217	0.77	-0.021	-0.06
Ratio of foreign peers	-1.468**	-2.91	-1.606***	-3.82	-1.090**	-2.80	-0.270	-0.72	-0.272	-0.50

Table 9.2: Quantile Regression for Mathematics without Revenue-Driven Inputs (Reduced Form) (cont.)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
High education	2.973	1.56	1.376	0.88	2.612(*)	1.81	3.976**	3.05	4.415*	2.51
Old people	-2.129	-0.55	-4.336	-1.26	1.695	0.39	5.838(*)	1.69	7.836(*)	1.65
Unemployment rate	12.184	0.91	10.302	1.09	2.891	0.34	5.942	0.65	1.156	0.09
Protestants	0.796	1.60	1.007**	3.00	0.888*	2.19	0.943*	2.03	0.603	1.27
Muslims	4.022	0.90	0.761	0.23	4.245	1.12	4.562	1.18	4.134	1.03
No religion	-1.270	-0.60	-1.555	-0.93	-2.649(*)	-1.83	-3.105(*)	-1.99	-2.861	-1.52
Poor persons	-3.003**	-2.87	-2.026**	-2.61	-2.023(*)	-1.95	-2.709***	-3.29	-1.966(*)	-1.73
Urbanization	-0.134	-0.21	0.331	0.64	0.295	0.67	-0.022	-0.05	-0.022	-0.04
Log (population)	-29.590*	-2.00	-19.901(*)	-1.90	-24.789*	-2.20	-17.679	-1.58	-9.096	-0.72
Constant	811.463***	4.60	727.519***	5.55	687.969***	5.25	555.037***	4.02	488.471**	3.09
Pseudo R^2	0.15		0.14		0.13		0.13		0.12	

Quantile regression for the 10th, the 25th, the 50th, the 75th, and the 90th quantiles. Standard errors are bootstrapped (1,500 replications). Observations with missing values and with a class size in natural science of less than 20 students have been deleted. 1,846 observations.

Table 9.3: Quantile Regression for Mathematics Including Revenue-Driven Inputs (Structural Form)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
Direct democracy	1.103	0.14	5.326	0.89	10.961*	2.24	12.767*	2.54	24.816***	3.83
Latin region	2.708	0.14	12.154	0.67	48.398***	3.58	48.053***	3.45	46.662**	3.06
hisei2	-2.873	-0.11	6.587	0.34	-0.001	0.00	-5.518	-0.36	-22.759	-1.16
hisei3	18.679	0.82	21.598	1.22	5.641	0.38	3.222	0.23	-0.518	-0.03
hisei4	0.718	0.03	9.288	0.52	-8.353	-0.56	-12.045	-0.87	-11.045	-0.55
hisei5	3.284	0.12	23.803	1.02	5.701	0.32	8.962	0.53	-1.487	-0.07
hisei6	5.345	0.23	16.873	0.93	3.760	0.24	11.989	0.81	3.218	0.16
hisei7	29.109	1.10	44.554*	2.05	29.366	1.63	15.408	0.95	6.521	0.24
No income data	-11.722	-0.48	-12.459	-0.59	-14.993	-0.89	-9.097	-0.52	3.813	0.17
Number of siblings	0.463	0.54	0.541	0.79	-0.426	-0.68	-0.356	-0.47	-0.480	-0.55
Old student	-11.635	-0.55	-32.042	-1.57	-19.748	-0.99	-34.641*	-2.03	-52.359*	-2.00
Young student	11.605	1.38	6.588	0.92	1.226	0.20	6.101	0.90	6.745	0.85
Books at home	7.450**	2.72	6.124**	2.97	9.206***	5.16	9.182***	4.89	11.337***	4.56
No late arrival	12.609(*)	1.72	9.313	1.45	1.727	0.31	2.786	0.48	-4.190	-0.52
No PC at home	-37.626**	-2.56	-19.068	-1.56	-13.201	-1.38	-15.205	-1.54	-7.312	-0.51
Female	-31.683***	-5.18	-33.300***	-6.35	-26.771***	-5.55	-18.343***	-3.57	-21.269***	-3.33
Both parents work	1.714	0.23	-4.377	-0.74	-4.433	-0.86	-1.923	-0.34	-4.741	-0.64
Intact family	14.418	1.45	11.840	1.33	-2.492	-0.37	-2.447	-0.31	-6.689	-0.74
Native	5.292	0.34	1.317	0.11	-5.473	-0.49	6.776	0.63	-16.856	-1.13
Foreign parents	-11.203	-0.86	-8.613	-0.74	-24.807*	-2.32	-0.675	-0.07	-19.469(*)	-1.70
Second generation	-7.052	-0.73	-1.528	-0.19	-12.136	-1.63	-1.921	-0.28	-2.430	-0.27

Table 9.3: Quantile Regression for Mathematics Including Revenue-Driven Inputs (Structural Form) (cont.)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
Non-test language	-9.658	-0.68	-18.676(*)	-1.83	-20.858*	-2.16	-16.484(*)	-1.65	-5.471	-0.43
Parents low education	1.419	0.10	-3.545	-0.35	-8.450	-0.85	-18.982(*)	-1.76	-5.843	-0.37
Parents medium education	-22.634**	-3.03	-16.028*	-2.54	-11.825*	-2.20	-5.182	-0.91	-0.873	-0.13
Mother tertiary education	-16.514(*)	-1.72	-6.935	-0.95	-6.778	-0.97	-7.010	-0.94	-6.868	-0.74
Father tertiary education	-6.483	-0.80	-7.063	-1.11	-5.465	-0.90	-2.254	-0.33	6.212	0.71
Discuss politics	-5.734	-0.61	4.263	0.50	-1.808	-0.25	3.608	0.47	8.321	0.87
Listen to music	-11.395	-0.67	-14.060	-1.01	-14.275	-0.96	-11.298	-0.83	-7.873	-0.45
Discuss performance	3.049	0.41	-4.370	-0.75	-7.006	-1.44	-10.833(*)	-1.94	-22.159**	-3.10
Main meal	19.004	1.47	11.211	0.87	4.904	0.48	9.139	0.86	1.048	0.07
Regular talking	-3.317	-0.46	9.291	1.42	1.398	0.26	-3.002	-0.55	0.492	0.07
Village school	-30.301(*)	-1.91	-23.462(*)	-1.65	-21.638(*)	-1.82	-11.246	-1.07	-22.569(*)	-1.87
Small town school	-8.155	-0.83	-5.873	-0.75	-6.186	-1.02	-5.075	-0.82	-1.196	-0.13
City school	8.978	0.40	12.806	0.92	10.606	0.83	3.596	0.31	-23.947(*)	-1.77
Private school	5.870	0.40	0.893	0.08	-10.995	-1.00	-6.948	-0.56	-5.172	-0.37
Selective school	-2.740	-0.30	-1.919	-0.26	9.947	1.66	7.491	1.16	1.962	0.22
Regular tests	-7.738	-0.81	-5.746	-0.71	2.730	0.37	1.772	0.22	-1.526	-0.14
Homework feedback	-22.410**	-2.70	-19.059**	-3.11	-18.683***	-3.26	-18.388***	-3.34	-11.123	-1.52
Discipline problem	-4.919	-0.64	1.307	0.21	-3.276	-0.64	-7.627	-1.45	-9.926	-1.57
Coefficient mathematics	2.843	1.33	2.842	1.62	3.803**	2.68	5.055***	3.35	6.113*	2.37
Ratio of female peers	-0.010	-0.02	0.068	0.20	-0.142	-0.49	-0.144	-0.44	-0.358	-0.96
Ratio of foreign peers	-1.859*	-2.38	-1.743***	-3.43	-1.282**	-2.94	-0.473	-0.96	-0.141	-0.24

Table 9.3: Quantile Regression for Mathematics Including Revenue-Driven Inputs (Structural Form) (cont.)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
High education	1.833	0.80	1.776	1.03	2.442	1.63	4.917**	2.93	5.785**	2.99
Old people	-6.140	-1.38	-6.314	-1.56	-3.958	-0.92	1.483	0.39	0.196	0.04
Unemployment rate	8.652	0.64	8.073	0.65	1.565	0.16	7.899	0.67	1.595	0.11
Protestants	0.660	1.33	0.680	1.55	1.375***	3.18	1.627***	3.39	1.434*	2.28
Muslims	0.771	0.16	-1.406	-0.34	-0.068	-0.02	4.085	1.01	-2.429	-0.50
No religion	0.891	0.30	1.433	0.69	-1.559	-0.96	-3.532(*)	-1.89	-2.919	-1.40
Poor persons	-1.253	-1.05	-1.424	-1.33	-1.109	-1.03	-2.408*	-2.43	-0.798	-0.61
Urbanization	-0.265	-0.32	-0.355	-0.53	0.445	0.87	0.371	0.65	0.896	1.55
Log (population)	-12.298	-0.59	-0.214	-0.02	-16.676	-1.42	-25.489*	-1.97	-17.996	-1.26
Poor conditions 1	-10.837	-0.54	7.515	0.42	-1.319	-0.11	6.586	0.51	6.575	0.45
Poor conditions 2	27.850	1.26	-1.384	-0.08	-12.745	-0.72	3.455	0.20	-30.795	-1.39
No PC at school	-5.127	-0.59	-11.987	-1.54	-8.201	-1.39	-6.610	-0.94	4.858	0.56
Teacher shortage	5.725	0.40	-2.757	-0.23	4.332	0.45	-7.934	-0.85	-16.573	-1.22
Tertiary mathematics	51.826***	3.28	24.753(*)	1.89	17.357	1.49	14.842	1.40	31.509*	2.45
Tertiary staff	-5.885	-0.28	27.775	1.47	34.614*	2.23	13.884	1.05	5.280	0.29
Total hours	0.077	1.19	0.044	0.93	0.045	1.22	0.052	1.29	0.073	1.50
Student-teacher ratio	-0.661	-0.65	-0.131	-0.16	-0.449	-0.48	0.101	0.11	0.080	0.08
Constant	540.222*	2.02	441.658*	2.36	600.131***	4.03	579.891***	3.60	477.995**	2.61
Pseudo R^2	0.17		0.16		0.15		0.14		0.14	

Quantile regression for the 10th, the 25th, the 50th, the 75th, and the 90th quantiles. Standard errors are bootstrapped (1,500 replications). Observations with missing values and with a class size in mathematics of less than 20 students have been deleted. 1,596 observations.

10 Appendix: Natural Science

Table 10.1: OLS Regression for Natural Science

	Reduced Form		Structural Form	
	Coeff.	t	Coeff	t
Direct democracy	-3.128	-0.63	6.255	1.39
Latin region	30.738*	2.09	6.950	0.46
hisei2	-18.209	-1.15	-26.336	-1.58
hisei3	1.891	0.13	-10.887	-0.68
hisei4	-0.969	-0.06	-16.220	-0.99
hisei5	25.826	1.51	10.176	0.56
hisei6	21.398	1.39	6.325	0.38
hisei7	32.522(*)	1.96	19.064	1.06
No income data	-14.473	-0.90	-20.495	-1.16
Number of siblings	-1.358(*)	-1.73	-1.872*	-2.13
Old student	-21.996	-1.41	-19.335	-1.15
Young student	10.746(*)	1.65	7.802	1.00
Books at home	7.879***	4.97	6.666***	3.62
No late arrival	17.796***	3.33	17.255**	2.87
No PC at home	-14.857	-1.49	-24.519*	-2.47
Female	-18.157***	-4.54	-21.516***	-4.82
Both parents work	-3.436	-0.89	0.646	0.14
Intact family	7.967	1.44	10.941(*)	1.75
Native	18.718(*)	1.70	11.629	0.92
Foreign parents	-8.991	-0.78	-15.866	-1.09
Second generation	4.463	0.68	-0.820	-0.12
Non-test language	-5.894	-0.62	-12.885	-1.19
Parents low education	-10.944	-1.21	-4.962	-0.56
Parents medium education	-17.858***	-3.44	-15.786**	-2.78
Mother tertiary education	-4.382	-0.72	-1.681	-0.25
Father tertiary education	-5.291	-1.17	-0.785	-0.15
Discuss politics	4.714	0.86	2.770	0.45
Listen to music	9.846	0.75	21.285	1.34
Discuss performance	-12.514**	-2.66	-8.716(*)	-1.78
Main meal	-1.778	-0.18	-5.469	-0.52
Regular talking	-2.183	-0.52	0.130	0.03
Village school	-20.445(*)	-1.81	-19.693	-1.50
Small town school	-6.104	-0.79	-6.177	-0.81
City school	17.477	1.16	18.253	1.04

Table 10.1: OLS Regression for Natural Science (cont.)

	Reduced Form		Structural Form	
	Coeff.	t	Coeff	t
Private school	2.186	0.26	11.749	1.51
Selective school	17.758**	2.82	1.953	0.25
Regular tests	11.837	1.26	7.537	0.83
Homework feedback	-10.158*	-2.02	-8.329	-1.44
Discipline problem	-11.850**	-2.60	-4.504	-0.95
Coefficient natural science	4.876*	2.36	1.606	0.66
Ratio of female peers	-0.356	-1.11	-0.868*	-2.20
Ratio of foreign peers	-1.015(*)	-1.82	-1.038(*)	-1.93
High education	-0.621	-0.43	-0.216	-0.13
Old people	-7.291*	-2.26	-10.033**	-2.73
Unemployment rate	1.882	0.15	-0.755	-0.05
Protestants	1.025*	2.11	1.014(*)	1.95
Muslims	1.884	0.50	-1.566	-0.36
No religion	-0.643	-0.36	1.288	0.58
Poor persons	-1.579*	-2.13	-0.451	-0.42
Urbanization	0.334	0.84	-0.062	-0.15
Log (population)	-25.123*	-2.14	-12.017	-0.88
Poor conditions 1			6.668	0.46
Poor conditions 2			-41.770*	-2.02
No PC at school			4.068	0.65
Teacher shortage			-33.854*	-2.47
Tertiary Science			25.256*	2.37
Tertiary staff			39.016**	2.61
Total hours			0.097(*)	1.83
Student-teacher ratio			0.960	1.30
Constant	884.143***	6.61	664.068***	4.16
F-Test	16.13		33.47	
Adjusted R^2	0.25		0.29	
Number of observations	1410		1126	

OLS regression with robust standard errors obtained through clustering by schools (157 schools). Observations with missing values and a class size of less than 20 have been deleted

Table 10.2: Quantile Regression for Natural Science without Revenue-Driven Inputs (Reduced Form)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
Direct democracy	-7.668	-1.08	-8.105(*)	-1.81	-2.702	-0.60	0.102	0.02	3.052	0.56
Latin region	23.925	1.02	24.685	1.51	34.196*	2.13	43.662*	2.42	24.046	1.20
hisei2	18.192	0.53	-16.347	-0.88	-21.155	-1.22	-9.598	-0.45	-19.095	-0.69
hisei3	47.900	1.48	5.878	0.35	1.030	0.07	17.408	0.88	-14.504	-0.57
hisei4	46.898	1.43	-0.672	-0.04	0.572	0.04	5.943	0.30	-26.053	-1.00
hisei5	52.135	1.29	31.577	1.55	14.278	0.78	24.123	1.01	15.166	0.49
hisei6	73.672*	2.23	20.383	1.17	21.058	1.32	25.921	1.26	3.524	0.13
hisei7	83.204*	2.15	36.642(*)	1.68	27.842	1.47	30.615	1.32	6.239	0.18
No income data	23.238	0.62	-10.606	-0.53	-16.859	-0.88	2.891	0.12	-23.048	-0.85
Number of siblings	-1.069	-0.72	-0.783	-0.89	-1.461(*)	-1.66	-1.169	-1.35	-1.388	-1.32
Old student	-38.491	-1.16	-30.285	-1.58	-17.624	-0.68	10.355	0.58	-0.578	-0.04
Young student	3.081	0.29	13.119	1.54	7.181	0.82	7.517	0.97	11.932	1.04
Books at home	6.494(*)	1.91	8.720***	3.87	7.726***	3.63	8.711***	3.91	9.592***	3.47
No late arrival	30.102**	3.05	16.838*	2.12	18.391**	2.63	4.024	0.59	12.426(*)	1.65
No PC at home	16.707	1.29	-12.746	-1.20	-29.327*	-2.50	-30.552**	-2.89	-29.986*	-2.16
Female	-21.175**	-2.69	-19.311**	-3.09	-22.508***	-4.15	-19.166***	-3.44	-18.084*	-2.56
Both parents work	6.535	0.72	-3.456	-0.56	-2.379	-0.42	-4.743	-0.73	-10.254	-1.29
Intact family	2.831	0.23	7.640	0.88	5.932	0.67	10.141	1.37	8.864	0.89
Native	48.299*	2.20	17.710	1.23	5.689	0.47	10.547	0.94	-2.370	-0.13
Foreign parents	2.378	0.12	-3.523	-0.27	-16.742	-1.34	-29.717*	-2.36	-23.802	-1.49
Second generation	15.142	1.50	10.159	1.11	3.360	0.44	-2.983	-0.42	-9.661	-0.87

Table 10.2: Quantile Regression for Natural Science without Revenue-Driven Inputs (Reduced Form) (cont.)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
Non-test language	-7.871	-0.41	-12.892	-1.02	-0.820	-0.06	5.278	0.40	-0.618	-0.05
Parents low education	-38.094	-1.61	-11.194	-0.71	-5.072	-0.42	-5.091	-0.42	-20.558	-1.35
Parents medium education	-35.475***	-3.74	-20.690**	-2.72	-8.098	-1.23	-7.788	-1.23	-13.412(*)	-1.72
Mother tertiary education	-15.724	-1.43	0.800	0.09	-3.504	-0.45	-1.983	-0.26	-9.908	-1.01
Father tertiary education	-14.113	-1.46	-6.266	-0.80	-2.586	-0.40	-3.513	-0.52	-6.365	-0.78
Discuss politics	8.800	0.74	4.706	0.50	1.636	0.19	8.777	1.11	7.763	0.69
Listen to music	-0.548	-0.03	-14.796	-0.84	-3.706	-0.19	14.894	1.00	1.332	0.05
Discuss performance	-14.735(*)	-1.67	-13.374(*)	-1.80	-7.711	-1.20	-12.634*	-2.07	-15.571(*)	-1.91
Main meal	10.022	0.51	-0.674	-0.05	-6.299	-0.48	-7.318	-0.82	2.081	0.18
Regular talking	3.170	0.36	2.389	0.39	-3.951	-0.64	-4.285	-0.74	-5.737	-0.81
Village school	-24.427	-1.25	-20.149(*)	-1.67	-20.939	-1.57	-12.984	-1.16	-12.948	-0.86
Small town school	1.579	0.14	-11.794	-1.52	-9.572	-1.32	-4.529	-0.61	-2.643	-0.25
City school	1.236	0.05	5.034	0.22	25.843	1.27	23.779	1.49	13.501	0.61
Private school	-6.097	-0.35	10.464	0.75	10.591	0.96	-5.948	-0.59	-8.323	-0.57
Selective school	15.228(*)	1.72	16.660*	2.28	18.967**	2.66	17.453**	2.58	12.238	1.41
Regular tests	19.585	1.43	18.001(*)	1.77	10.309	1.07	5.635	0.57	-3.442	-0.27
Homework feedback	5.279	0.60	-12.558(*)	-1.82	-11.284	-1.54	-13.152(*)	-1.83	-14.765(*)	-1.73
Discipline problem	-12.826	-1.45	-9.625	-1.53	-10.372(*)	-1.70	-18.503**	-3.08	-18.434*	-2.52
Coefficient natural science	3.354	1.06	4.852*	2.38	6.044**	3.08	4.495*	2.02	6.823*	2.40
Ratio of female peers	-0.314	-0.55	-0.253	-0.72	-0.297	-0.93	-0.346	-1.01	-0.585	-1.31
Ratio of foreign peers	-0.615	-0.90	-1.418***	-3.25	-1.400**	-2.78	-1.088(*)	-1.94	-0.506	-0.67

Table 10.2: Quantile Regression for Natural Science without Revenue-Driven Inputs (Reduced Form) (cont.)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
High education	-1.379	-0.51	-0.317	-0.16	-1.891	-1.20	0.432	0.25	3.020	1.35
Old people	-6.461	-0.88	-7.204	-1.56	-7.428(*)	-1.76	-2.586	-0.64	1.923	0.44
Unemployment rate	2.006	0.12	-7.824	-0.58	0.927	0.08	4.835	0.35	14.295	0.81
Protestants	0.817	1.10	0.922(*)	1.87	0.776*	1.98	1.292**	2.78	0.721	1.23
Muslims	4.603	0.68	1.028	0.23	2.008	0.58	5.943	1.53	2.475	0.51
No religion	-0.102	-0.04	-0.091	-0.05	0.526	0.35	-2.545	-1.41	-3.237	-1.41
Poor persons	-2.313	-1.40	-1.860(*)	-1.82	-1.412	-1.49	-2.012(*)	-1.93	-1.433	-1.20
Urbanization	0.422	0.61	0.469	0.85	0.342	0.76	0.551	1.04	-0.057	-0.10
Log (population)	-34.727*	-1.98	-26.746*	-2.21	-17.145(*)	-1.79	-31.944*	-2.52	-23.616	-1.35
Constant	847.703***	4.05	878.293***	5.36	820.816***	6.27	911.757***	6.07	790.745***	3.99
Pseudo R^2	0.19		0.18		0.16		0.15		0.16	

Quantile regression for the 10th, the 25th, the 50th, the 75th, and the 90th quantiles. Standard errors are bootstrapped (1000 replications). Observations with missing values and with a class size in natural science of less than 20 students have been deleted. 1,410 observations.

Table 10.3: Quantile Regression for Natural Science Including Revenue-Driven Inputs (Structural Form)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
Direct democracy	-0.349	-0.04	3.590	0.57	5.624	1.00	7.973	1.41	13.179	1.61
Latin region	-49.684*	-2.10	-18.261	-0.95	18.636	1.07	37.696(*)	1.94	55.552**	2.57
hisei2	15.494	0.46	-24.616	-1.13	-32.866(*)	-1.72	-25.373	-1.15	-40.191	-1.48
hisei3	32.949	1.02	-6.015	-0.28	-3.173	-0.18	-10.986	-0.52	-38.014	-1.57
hisei4	28.565	0.87	-9.447	-0.45	-7.630	-0.43	-16.546	-0.78	-47.827(*)	-1.94
hisei5	49.230	1.29	28.754	1.14	5.602	0.27	2.690	0.10	-10.751	-0.33
hisei6	48.165	1.46	8.686	0.39	12.532	0.68	5.105	0.24	-23.589	-0.91
hisei7	89.089*	2.35	37.999	1.53	19.621	0.93	12.972	0.50	-24.288	-0.63
No income data	42.696	1.13	-7.708	-0.31	-15.461	-0.73	-13.408	-0.52	-43.255	-1.56
Number of siblings	-0.613	-0.39	-0.505	-0.42	-1.428	-1.61	-0.963	-0.80	-2.085(*)	-1.75
Old student	-15.846	-0.59	-34.456(*)	-1.66	0.604	0.02	-1.408	-0.08	-2.771	-0.15
Young student	10.647	0.87	8.382	0.83	10.127	1.10	-0.171	-0.02	3.008	0.25
Books at home	8.308*	2.31	6.479*	2.21	6.862**	2.93	6.379*	2.51	6.462(*)	1.90
No late arrival	34.569**	2.94	16.798(*)	1.84	19.212**	2.60	9.125	1.16	15.828(*)	1.69
No PC at home	2.253	0.14	-9.348	-0.73	-33.596**	-2.73	-36.155**	-2.81	-38.189*	-2.54
Female	-25.156**	-3.09	-18.090**	-2.66	-26.397***	-4.41	-21.960***	-3.33	-16.857*	-2.21
Both parents work	11.314	1.17	0.546	0.07	1.344	0.22	-3.926	-0.57	0.201	0.02
Intact family	1.402	0.11	6.584	0.61	10.829	1.18	4.541	0.48	3.435	0.33
Native	60.175*	2.06	19.665	1.16	-7.516	-0.56	11.883	0.83	5.688	0.26
Foreign parents	17.782	0.81	-5.241	-0.31	-38.623*	-2.47	-23.185	-1.49	-27.194	-1.41

Table 10.3: Quantile Regression for Natural Science Including Revenue-Driven Inputs (Structural Form) (cont.)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
Second generation	-3.726	-0.34	-0.751	-0.07	0.583	0.07	-8.643	-0.92	-12.267	-0.96
Non-test language	-35.496(*)	-1.65	-20.902	-1.25	4.996	0.34	-6.084	-0.44	5.521	0.35
Parents low education	-38.046(*)	-1.72	-14.404	-0.88	-1.170	-0.09	-0.516	-0.04	-27.905(*)	-1.69
Parents medium education	-33.955**	-3.12	-24.413**	-2.74	-7.710	-1.06	-4.227	-0.58	-3.416	-0.40
Mother tertiary education	-3.211	-0.26	-0.709	-0.07	-0.577	-0.07	2.744	0.30	9.556	0.75
Father tertiary education	-8.995	-0.83	-3.974	-0.44	-3.295	-0.45	-7.743	-1.02	0.225	0.02
Discuss politics	-1.769	-0.14	-6.244	-0.58	-0.148	-0.02	5.797	0.60	9.529	0.78
Listen to music	-20.442	-0.81	-12.316	-0.53	5.261	0.28	26.799	1.32	28.367	0.71
Discuss performance	-11.038	-1.07	-4.202	-0.50	-2.413	-0.38	-12.074(*)	-1.80	-14.648	-1.61
Main meal	10.828	0.54	-1.615	-0.11	-13.200	-1.08	-8.742	-0.80	1.162	0.09
Regular talking	-2.762	-0.31	2.452	0.32	0.206	0.04	2.374	0.36	-0.565	-0.07
Village school	-55.343*	-2.55	-45.081*	-2.37	-0.214	-0.01	-5.719	-0.41	7.661	0.45
Small town school	-18.726	-1.61	-15.835	-1.64	-3.892	-0.42	2.264	0.23	14.437	1.28
City school	-22.678	-0.57	38.774	1.63	24.526	1.27	31.989	1.46	29.730	1.05
Private school	23.530	1.04	37.381*	2.47	12.965	1.15	-2.219	-0.18	-29.093(*)	-1.75
Selective school	-17.119	-1.40	-4.357	-0.42	8.516	1.11	4.078	0.50	3.913	0.34
Regular tests	14.751	1.03	15.260	1.23	15.491	1.43	9.013	0.74	-13.103	-0.84
Homework feedback	6.197	0.66	-5.635	-0.66	-10.167	-1.39	-19.244*	-2.46	-11.809	-1.17
Discipline problem	-3.843	-0.43	-7.270	-1.05	-6.966	-1.10	-9.283	-1.38	-5.834	-0.71
Coefficient natural science	-4.081	-0.89	-1.266	-0.38	3.720	1.40	4.278	1.43	8.146*	2.37

Table 10.3: Quantile Regression for Natural Science Including Revenue-Driven Inputs (Structural Form) (cont.)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
Ratio of female peers	-1.748*	-2.54	-1.072*	-2.08	-0.694(*)	-1.74	-0.815(*)	-1.87	-0.531	-0.96
Ratio of foreign peers	-1.799*	-2.09	-2.042**	-3.48	-1.342*	-2.38	-0.717	-1.00	0.547	0.63
High education	1.332	0.41	0.303	0.11	-0.525	-0.22	0.669	0.26	1.950	0.58
Old people	-7.777	-0.86	-14.358*	-2.34	-8.376	-1.57	-6.717	-1.26	-0.417	-0.06
Unemployment rate	11.210	0.49	2.175	0.13	-4.250	-0.29	-9.219	-0.55	-3.240	-0.15
Protestants	0.979	1.12	1.291*	2.16	1.050*	2.14	1.065*	1.97	0.910	1.34
Muslims	-10.339	-1.32	-3.636	-0.64	-0.774	-0.17	2.960	0.60	7.312	1.12
No religion	5.393	1.41	1.859	0.60	1.548	0.66	-1.608	-0.62	-4.608	-1.27
Poor persons	-2.476	-1.18	-1.404	-0.92	-0.618	-0.43	-0.162	-0.12	-0.291	-0.17
Urbanization	-1.493	-1.69	-0.374	-0.48	0.224	0.37	0.619	1.04	0.700	1.03
Log (population)	-3.506	-0.14	-17.776	-1.01	-14.198	-1.17	-16.597	-1.19	-22.204	-1.17
Poor conditions 1	-51.514*	-2.07	-21.928	-1.11	21.593	1.14	18.442	1.13	22.430	1.19
Poor conditions 2	-57.630	-1.23	-39.101	-1.10	-63.711(*)	-1.93	-30.922	-0.91	-36.484	-0.97
No PC at school	8.291	0.60	0.691	0.07	9.791	1.22	5.124	0.61	-1.104	-0.11
Teacher shortage	-38.262(*)	-1.72	-32.346	-1.63	-32.586*	-2.24	-27.201(*)	-1.73	-17.926	-0.95
Tertiary science	17.137	1.02	18.168	1.41	29.903**	2.58	26.854*	2.14	26.252(*)	1.83
Tertiary staff	57.610*	2.24	57.703**	3.10	31.536*	2.09	33.029*	1.99	37.787(*)	1.76
Total hours	0.235*	2.99	0.115*	2.16	0.071	1.43	0.072	1.26	0.016	0.24
Student-teacher ratio	1.532	1.01	1.481	1.28	1.240	1.22	-0.244	-0.22	-0.701	-0.47
Constant	376.348	1.17	782.396***	3.55	667.196**	3.88	694.026***	3.52	684.400**	2.96

Table 10.3: Quantile Regression for Natural Science Including Revenue-Driven Inputs (Structural Form) (cont.)

	q10		q25		q50		q75		q90	
	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t	Coeff.	t
Pseudo R^2	0.25		0.22		0.20		0.18		0.19	

Quantile regression for the 10th, the 25th, the 50th, the 75th, and the 90th quantiles. Standard errors are bootstrapped (1,000 replications). Observations with missing values and with a class size in natural science of less than 20 students have been deleted. 1,126 observations.

11 Appendix: Descriptive Statistics

Table 11.1: Description of Variables

Variables	Description	Source
Dependent variable	WARM estimate (weighted likelihood estimate) in reading, mathematics or natural science: difficulty adjusted test score	National PISA study
Direct democracy	Index of direct democracy from 1 (min.) to 6 (max.) in 2000	Own calculation based on STUTZER (1999)
Latin region	1 if language community is either French- or Italian-speaking, 0 otherwise (cantreg)	National PISA study
Individual and family variables		
hisei2	PISA International Socio-Economic Index of Occupational Status of the parents as a proxy of income, 28 - 37 index points	National PISA study
hisei3	38 - 47 index points	National PISA study
hisei4	48 - 57 index points	National PISA study
hisei5	58 - 67 index points	National PISA study
hisei6	68 - 77 index points	National PISA study
hisei7	>78 index points	National PISA study
No income data	1 if missing value in hisei-Index (hisei), 0 otherwise	National PISA study
Number of siblings	Number of siblings (nsib)	National PISA study
Old student	1 if student older than 204 months / 15 years (age), 0 otherwise	National PISA study
Young student	1 if student younger than 180 months / 17 years (age), 0 otherwise	National PISA study
Books at home	Number of books at home (st37q01)	National PISA study
No late arrival	1 if student claims never to have arrived late for school in the last two school weeks (st29q03), 0 otherwise	National PISA study

Table 11.1: Description of Variables (cont.)

Variables	Description	Source
No PC at home	1 if student never has access to a PC at home (it01q01), 0 otherwise	National PISA study
Female	1 if student is female, 0 otherwise (st03q01)	National PISA study
Both parents work	1 if both parents work, either full time or part time (st07q01, st06q01), 0 otherwise	National PISA study
Intact family	1 if student usually lives with father and mother (st04q01, st04q03), 0 otherwise	National PISA study
Native	1 if country of birth is Switzerland (st16q01), 0 otherwise	National PISA study
Foreign parents	1 if country of birth of both father and mother is not Switzerland (st16q02, st16q03), 0 otherwise	National PISA study
Second generation	1 if only one parent is born abroad (st16q02, st16q03), 0 otherwise	National PISA study
Non-test language	1 if language spoken at home is not test-language (st17q01), 0 otherwise	National PISA study
Parents low education	Father and/or mother completed only primary education or did not go to school (fisced, misced)	National PISA study
Parents medium education	Father and/or mother completed lower secondary level (fisced, misced)	National PISA study
Mother tertiary education	Mother completed tertiary education (misced)	National PISA study
Father tertiary education	Father completed tertiary education (fisced)	National PISA study
Discuss politics	1 if student regularly discusses political or social issues with parents (st19q01), 0 otherwise	National PISA study
Listen to music	1 if student regularly listens to classical music together with parents (st19q03), 0 otherwise	National PISA study
Discuss performan	1 if student regularly discusses school performance with parents (st19q04), 0 otherwise	National PISA study
Main meal together	1 if several times a week parents eat main meal with student (st19q05), 0 otherwise	National PISA study
Regular talking	1 if several times a week parents spend time just talking to the student (st19q06), 0 otherwise	National PISA study
School and class variables		
Village school	1 if school is located in a village (< 3000 E) (sc01q01), 0 otherwise	National PISA study
Small town school	1 if school is located in a small town (3000 - 15,000) (sc01q01), 0 otherwise	National PISA study
City school	1 if school is located in a city (100,000 to 1,000000) (sc01q01), 0 otherwise	National PISA study
Private school	1 if school is private, 0 otherwise (sc03q01)	National PISA study

Table 11.1: Description of Variables (cont.)

Variables	Description	Source
Selective school	1 if admission to school is always based on student's record of academic performance including placement tests, 0 otherwise	National PISA study
Regular tests	1 if students are assessed four or more times a year using standardized or teacher developed tests, 0 otherwise (sc16q01, sc16q02)	National PISA study
Homework feedback	1 if homework is counted as part of mark or teachers grade homework most of the time or always (st32q07, st32q03), 0 otherwise	National PISA study
Discipline problem	1 if in most lessons or in every lesson, students don't listen to what the teacher says, students don't start working for a long time after the lesson begins, there is noise and disorder, or at the start of class more than five minutes are spent doing nothing (st26q13, st16q14, st26q16, st26q17)	National PISA study
Peer variables		
Coefficient reading / mathematics / natural science	Peers' mean performance divided by peers' standard deviation in test subject	Calculation based on National PISA study
Ratio of females	Share of female students in student's peer group	Calculation based on National PISA study
Ratio of foreign students	Share of students born abroad in student's peer group	Calculation based on National PISA study
Cantonal variables		
High education	Share of cantonal residents with a tertiary education or a high school degree	Swiss Federal Statistical Office
Old people	Share of cantonal residents older than 65 years	Swiss Federal Statistical Office
Unemployment	Cantonal unemployment rate	Swiss Federal Statistical Office
Protestant	Share of Protestant residents in canton	Swiss Federal Statistical Office
Muslim	Share of Muslim residents in canton	Swiss Federal Statistical Office
No religion	Share of residents with no religious affiliation	Swiss Federal Statistical Office
Poor people	Share of persons who cannot afford savings of 100 Swiss Francs per month	Own calculation based on Swiss Household Panel, wave 2000
Urbanization	Share of residents living in agglomerations with at least 100,000 inhabitants	Swiss Federal Statistical Office
Log (population)	Natural logarithm of the cantonal residential population	Swiss Federal Statistical Office

Table 11.1: Description of Variables (cont.)

Variables	Description	Source
Revenue-driven inputs		
Poor conditions 1	1 if school suffers from poor building, poor heating and/or inadequate space (sc11q01 sc11q02 sc11q03), 0 otherwise	National PISA study
Poor conditions 2	1 if school suffers from a lack of instructional material and /or a poor library [a lot] (sc11q04 sc11q06), 0 otherwise	National PISA study
No PC at school	1 if student has no access to PC at school (it01q02), 0 otherwise	National PISA study
Teacher shortage	1 if a shortage of teachers in general and/or test subject teachers in particular [Some/a lot] (sc21q01, sc21q02/ sc21q03/ sc21q04), 0 otherwise	National PISA study
Tertiary reading / mathematics / natural science	Proportion of language teachers with tertiary education at school (propread / propmath / propscie)	National PISA study
Tertiary staff	Proportion of teachers with a tertiary education of teaching staff at school (propqual)	National PISA study
Total hours	Total number of schooling hours per year (tothrs)	National PISA study
Student-teacher ratio	Student-teacher ratio as school size divided by number of teachers (stratio)	National PISA study

Notes: In parentheses are the names of the variables on which the determinants of student performance are based. These labels are identical to those used in the OECD-PISA study conducted by the OECD in 2000. The questionnaires used for the Swiss national study are also identical to those used for the PISA study with the exception of a few questions which are irrelevant to this model specification. These labels also provide information about which questionnaire contained the original question. The first two letters either indicate 'st' for student questionnaire, 'it' for the information technology questionnaire, or 'sc' for the school questionnaire. The first two digits then stand for the number of the general issue, and 'qXX' for the related single question. The following variables have already been derived and computed by the issuing institution: *wleread*, *hisei*, *nsib*, *miscedu*, *fiscedu*, *stratio*, *tothrs* and are already part of the dataset. More information on the construction of these variables can be obtained from the issuing institution at http://www.sidos.ch/data/projects/pisa/ (13/04/2004). Base categories are schools in small towns (15,000 to 100,000 inhabitants), a low parental income (*hisei1*: below 28 index points), and a high but not tertiary education of parents (*misced* = 4 or 5, *fisced* = 4 or 5).

Table 11.2: Indices of Direct Democracy for the Year 2000

	VIR	GIR	GRR	FRR	Direct Democracy
Zürich	3.333	3.333	3.333	4.000	3.500
Bern	2.667	2.667	3.000	3.750	3.021
Luzern	4.667	5.333	3.667	4.000	4.417
Uri	5.333	5.333	5.333	4.500	5.125
Schwyz	5.333	5.333	4.667	4.375	4.927
Obwalden	5.333	5.333	4.333	3.500	4.625
Nidwalden	4.000	4.333	4.667	4.750	4.438
Glarus	6.000	6.000	6.000	5.000	5.750
Zug	5.000	5.000	3.667	4.000	4.417
Freiburg	3.000	3.000	2.667	2.500	2.792
Solothurn	5.333	5.333	5.333	5.000	5.250
Basel-Stadt	4.667	4.667	4.000	4.250	4.396
Basel-Land	6.000	6.000	5..167	4.750	5.479
Schaffhausen	5.333	5.333	5.167	5.000	5.208
Appenzell Ausserrhoden	6.000	6.000	6.000	4.000	5.500
Appenzell Innerrhoden	6.000	6.000	6.000	3.750	5.438
St. Gallen	3.333	4.000	3.000	3.500	3.458
Graubünden	4.333	5.000	6.000	4.000	4.833
Aargau	5.667	5.667	6.000	4.500	5.458
Thurgau	4.000	4.000	4.333	5.000	4.333
Tessin	1.333	2.667	2.000	3.000	2.250
Waadt	2.333	2.667	2.000	3.000	2.500
Wallis	4.333	5.000	4.000	1.000	3.583
Neuenburg	2.667	2.667	1.667	1.750	2.188
Genf	2.000	2.000	2.000	1.000	1.750
Jura	4.667	4.667	3.000	2.500	3.708

VIR indicates index of constitutional initiative, GIR index of statutory initiative, GRR index of statutory referendum, and FRR index of fiscal referendum, respectively. Swiss cantons appear in so-called historical sequence and in German denomination.

Table 11.3: Distribution of Observations for Reading in Swiss Cantons

	Reduced form	Structural form	Complete data set (French & national sample)
Zürich	157	134	1102
Bern	364	332	1062
Luzern	63	54	256
Uri	-	-	-
Schwyz	25	25	109
Obwalden	18	18	120
Nidwalden	-	-	39
Glarus	8	8	20
Zug	11	11	78
Freiburg	567	510	998
Solothurn	18	18	113
Basel-Stadt	42	41	173
Basel-Landschaft	50	26	200
Schaffhausen	-	-	46
Appenzell Ausserrhoden	-	-	27
Appenzell Innerrhoden	-	-	-
St. Gallen	370	240	1061
Graubünden	20	20	155
Aargau	210	167	470
Thurgau	62	50	207
Tessin	-	-	903
Waadt	220	204	1101
Wallis	350	346	1046
Neuenburg	362	362	869
Genf	319	228	919
Jura	175	175	722
Students in German-speaking cantons	1418	1144	5126
Students in French-speaking cantons	1993	1825	5655
Sum	3411	2969	11796

Swiss cantons appear in so-called historical sequence and in German denomination.

Table 11.4: Descriptive Statistics for Reading, Mathematics and Natural Science

Variable	Obs.	Mean	Std. Dev.	Min	Max
Full sample					
wleread	11781	500.6531	89.52575	23.89	884.49
wlescie	6557	500.6293	95.24182	168.60	830.09
wlemath	6545	531.7902	90.78325	202.14	815.90
National sample					
wleread	7979	498.2971	92.53197	27.60	884.49
wlescie	4443	497.6009	95.63797	168.60	830.09
wlemath	4440	529.5631	94.06256	202.14	815.90
Reduced form sample					
wleread	3411	534.2979	77.08092	98.22	812.88
wlescie	1410	533.6809	87.79413	186.60	804.54
wlemath	1846	560.9948	79.62094	202.14	815.9
Structural form sample					
wleread	2969	535.5002	76.71042	166.01	812.88
wlescie	1126	537.611	88.60316	168.60	804.54
wlemath	1596	560.9478	80.14506	202.14	815.9

Table 11.5: Determinants of Educational Spending in Swiss Cantons, Outliers Excluded, 1980–1998

Variable	Coefficient	t-value
Direct democracy	-0.056**	2.75
Cantons with Italian or French main language	-0.087	1.62
Fiscal decentralization	-0.420***	5.11
Tax competition	-0.058	1.35
Log of lump sum transfers	0.025	0.69
Constitutional constraint (fiscal break)	0.005	0.54
Conservative ideology of government	-0.141*	2.48
Log of national income	0.158(*)	1.95
Urbanization	0.242***	3.40
Log of cantonal population	0.016	0.93
Share of young people	-0.013	1.51
Share of old people	-0.024***	3.39
Constant	6.863***	5.82
Adjusted R^2	0.83	
F-statistic	83.64	
Jarcque-Bera test (χ-value)	4.52	
Observations	491	

2SLS estimation with Newey-West standard errors. ***indicates significance at the 0.1% level, **at the 1% level, *at the 5% level and (*) at the 10% level, respectively. Estimation with year dummies.

Chapter VII:

Conclusion

Conclusion

This dissertation began with the statement that economic theory predicts that a direct democracy should result in greater citizen happiness compared to more representative democratic systems because it enables an allocation of goods and resources that is closer to citizens' preferences. Transmission channels of direct democracy may include higher personal income induced by shrunken government budgets and more efficient allocation of public goods caused by the taming of Leviathan-like bureaucrats and politicians.

In this dissertation, I investigate the impact of direct democracy on happiness and explore additional channels of transmission beyond those already detected in previous studies. Most particularly, as channels of transmission, I focus on income redistribution, student performance, and public safety. These individual studies have been realized using cantonal data on redistribution, crime rates, police and educational expenditure, and individual data on student performance and life satisfaction for Switzerland.

As a first step, reported in chapter II, I carry out an update of the composite index of direct democracy for the years 1997 to 2003, necessitated by the many revisions of cantonal constitutions that took place during this period. Despite these amendments, the index values appear to be very stable over time. Most of the empirical analyses outlined in the remaining chapters are performed using this updated index. Chapter III reinvestigates the impact of direct democracy on happiness using a more recent dataset than the one employed in previous studies; namely, Swiss panel data from 2000 to 2002. It is shown that there is no longer any statistically significant relation between direct democracy and life satisfaction in the Swiss data. This breakdown for the Swiss data is possibly caused by the inclusion of variables that control for culture which removes too much variation of the institutional variable to allow conventional significance levels. Therefore, statistical insignificance does not mean that in real life direct democracy has no (positive) impact on subjective well-being in Switzerland.

Based on these results, the next natural step is to search for possible channels of transmission. First to be analyzed was the impact of direct legislation on income redistribution. Based on a traditional public choice approach and using a time-series cross-sectional panel from 1980 to 1998, it is shown that direct democracy leads to fewer financial resources available for income redistribution and a smaller absolute volume, which was actually redistributed (as measured by the difference between the pre- and after-tax income distribution). This policy

should lead, on average, to a higher after-tax income for citizens. Moreover, the after-tax income inequality appears to be lower in more direct democratic cantons than in those with a more representative political system. Finally, it is also shown that citizens in direct democracies are flexible enough to actually increase their financial volume for redistribution if the pre-tax income inequality is high. Based on these results, it can be concluded that, *ceteris paribus*, redistribution is carried out more efficiently in direct democracies than in more representative democratic systems, e.g. through that needy persons in the lowest quantile(s) profit more from redistribution.

Another transmission channel of direct democracy on happiness is the public safety policy carried out by the Swiss cantons. Using a bounded rationality theory combined with a public choice analysis and estimating an economic model of crime with Swiss cantonal data from 1986 to 2001, I show that in direct democracies, scarce resources are allocated in such a way that the median voter's preferences are better met than in more representative systems. First, the results corroborate the hypothesis that the median voter prefers fewer means to be spent on crime prevention and investigation because he or she underestimates the probability of occurrence. Second, the conjecture that because of bounded rationality the voter prefers the fighting of property crime to the prevention of hate crime is found to be in line with my regression analysis outcomes. These additional costs of fighting crime – particularly property-specific crimes – appear to be financed through efficiency gains in the prevention of those crimes that are less prominent in the median voter's memory. Hence, my findings support both primary hypotheses: (a) direct democracy leads to an allocation of goods and resources closer to the median voter's preferences and (b) it induces gains in efficiency.

Finally, I also investigate the impact of direct democracy on public education of ninth graders using a cross-section of individual data from 2000 for reading, mathematics, and natural science. The results for the first two subjects indicate that an educational expenditure-restraining impact of direct democracy appears to translate into lower student performance. For both subjects, it is mainly teacher qualifications that play a dominant role among the revenue-driven school input factors. Separating the direct impact from the indirect budgetary effect reveals a performance increasing influence of direct democracy on mathematics; however, the direct impact on reading becomes insignificant. This finding is very important in that it contrasts with the negative results found for a similar institution in a comparable political setting for the U.S. Beyond the negative budgetary impact, no further performance

lowering impact is detected, which might indicate the nonexistence of Leviathan-like behavior by the school administration. The positive effect on reading might either hint at a redistribution of given means among subjects taught at school or an unobserved engagement of parents or family culture that favors mathematics. In general, in the case of the provision of the education, I find no evidence that the median voter's preferences are not addressed by the school administration.

In sum, although in my reestimation for Switzerland the happiness increasing impact of direct democracy in Swiss cantons ranges only from weak to insignificant, I identify up to three further transmission channels of direct legislative institutions. In all, the median voter's preferences are found to be more closely adhered to in more direct democratic cantons than in more representative democratic systems, and no evidence for the existence of a Leviathan-like bureaucracy is revealed. Hence, the basic hypotheses of public choice regarding the utility increasing allocative effects of direct democracy can be viewed as more or less supported by these empirical findings on redistribution, public safety, and education.

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