Department of Economics

Four Essays in Applied Economics

Christoph Basten

Thesis submitted for assessment with a view to obtaining the degree of Doctor of Economics of the European University Institute

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CONTENTS

Part I

Summary

The four papers constituting this thesis cover a range of different topics, thus reflecting the breadth of issues I have worked on in the course of my PhD. All seek to rely as little as possible on prior assumptions, and to approximate as far as the context allows the ideal of a fully randomized experiment. The papers are listed in the chronological order in which I have worked on them, and are now briefly discussed in turn.

Chapters 1 and 2, which are joint work with Frank Betz[1], investigate the extent to which the Protestant Reformation has influenced respectively preferences for leisure (chapter 1) and preferences for redistribution and state intervention (chapter 2) in present-day Switzerland. Both focus on a region in the South-West of Switzerland, part of which was forced in the 16th century to convert to Protestantism whereas the other part was to remain Catholic. 15th century population figures along with a wide range of qualitative evidence allow us to demonstrate the equality of the two regions before the event, and hence the causal nature of the present-day differences found. By complementing our Instrumental-Variable analysis with a Spatial Regression Discontinuity Design that focuses on those municipalities situated nearby the historical religious border, we are furthermore able to address potential concerns about remaining confounders in the domains of geography, proximity to France, and proximity to the nearby city of Geneva.[2]

The first paper, on preferences for leisure, speaks to the very active recent literature that seeks to empirically test Weber [1904]'s famous hypothesis whereby the Protestant Reformation has led to a more pronounced "work ethic" and thereby increased economic prosperity. It stands out from prior work in the literature for employing direct measures of "work ethic" as well as for focusing specifically on those "Ascetic" strands of Protestantism which Weber [1904]'s hypothesis is based on. We find that traces of a "Protestant work ethic" can be found until the present-day, in attitudes as well as actual working behavior, but we do not find significant effects on present-day income per capita. Looking beyond the Weber debate, this evidence is interesting also as an example of how a historic event, through the channel of culture, can have effects that persist until the present day.

The second paper moves from self-regarding to political preferences, analyzing attitudes on the extent to which the government should intervene in the market, and the extent to which it should redistribute the returns generated therein. We confirm the hypotheses implied by the works of the sociologists Esping-Andersen [1990] and Manow [2002] whereby

[1] Then affiliated with the European University Institute (EUI), now with the European Central Bank

[2] Chapters 1 and 2 share a common methodology. It is fully described in both, so that each chapter is readable on its own, but readers may wish to skip the methodology section the 2nd time.

ceteris paribus "Ascetic Protestantism" has led to weaker preferences for redistribution and government intervention than Roman Catholicism. These differences are then found to manifest themselves also in a range of economic outcomes, such as measures of the inequality of pre-tax income within each municipality. They can be seen to refute Marx [1904]'s view whereby existence determines consciousness rather than consciousness existence.

Chapters 3 and 4, joint work with Andreas Fagereng and Kjetil Telle[3], focus on the labor market and household finance, with very direct implications for public policy. Both make use of Norwegian administrative data, which are based on the highly reliable and comprehensive Norwegian tax registers, and cover the universe of Norwegian tax payers. Chapter 3 uses these data to track the paths of income, wealth and holdings in different asset classes around the year of job loss. Panel data techniques and the complementary use of information on plant downsizings allow us to find clear evidence that households start to accumulate additional savings and to reallocate these to less risky and more liquid asset classes from about 2 years before their job loss, subsequently deplete these and further savings to cushion consumption while unemployed, and finally start rebuilding their savings to the initial level, all within +/- 4 years of their job loss. We also find that the extent to which households employ such consumption smoothing strategies is smaller for those with lower initial holdings and for the young, that it does not seem to vary significantly with unemployment spell length, and that it is higher (sic) for those whose income shock is more permanent.

The above-mentioned evidence on the importance of prior holdings is evidence in favor of liquidity constraints, but is only tentative due to the likely endogeneity of these prior holdings. This limitation is addressed in the 4th chapter. This investigates the responsiveness of job search duration, which can be interpreted as a form of consumption, to quasi-randomly assigned lump-sum severance payments. Identification is achieved by exploiting discontinuities in the available amounts along the age and time dimensions. We thus find that that each NOK 1,000 of severance pay lowers the propensity to start a new job on any day within the first 24 months of job loss by about 1.4% relative to the group not receiving any severance pay. Such an effect has previously only been investigated and found for Austria, where the generosity of unemployment insurance is low relative to most other OECD economies other than the US. Our finding that such an effect exists even in a country with as high and equitable private wealth and as generous unemployment insurance as Norway suggests that liquidity constraints are likely to be a relevant phenomenon also in many other OECD economies.

[3]Andreas Fagereng is associated with the EUI and Statistics Norway (SSB), and Kjetil Telle with SSB.

Part II

Four Essays in Applied Economics

CHAPTER 1

MAX WEBER'S "PROTESTANT ETHIC" AT WORK

1.1 Introduction

Does culture, and in particular religion, matter for economic growth? If it does, through which channels? The past decade has seen a resurgence of interest of economists in this question, which in previous decades had been the preserve of the other social sciences. In particular, the availability of appropriate micro data and of new methods for causal inference have motivated innovative empirical work to probe what Acemoglu et al. [2002] called the "most famous link between culture and economic development", namely the proposition by Weber [1904] whereby Protestant regions had economically outperformed Catholic ones, and that the reason was to be found primarily in a "Protestant ethic" of harder work, entrepreneurship and higher rates saving and of investment. Thus Becker and Woessmann [2009] undertook a fascinating empirical investigation of the Weber hypothesis with data from 19th century Prussia that led them to two findings: On the one hand, they confirmed a positive causal impact of Protestantism on economic growth. On the other hand though, they argued that most of this effect could be explained by a positive impact of Protestantism on education, leaving little space for the work ethic channel postulated by Max Weber.

The present paper distinguishes itself from this previous work in three respects. Firstly, by using data from Western Switzerland we are able to focus specifically on those "Ascetic branches of Protestantism" – including, according to Weber, Calvinism, Pietism, Methodism and Baptism, but not Lutheranism – which Max Weber was primarily concerned with: While in the 19 century Prussian sample used by Becker and Woessmann [2009] 94% of Protestants were Lutheran (Becker and Woessmann [2009], footnote 8), Western Switzerland was initially reformed in the style of Zuerich-based Zwingli and was then soon converted to Calvinism, since John Calvin was based in Geneva, just outside our region of interest. Secondly, we are able to use a direct measure of work ethic, rather than relegating work ethic to be the residual effect of Protestantism not explained by other channels. Finally, our study focuses on whether traces of a "Protestant work ethic", and resulting impacts on economic prosperity, can still be found today.

Identifying the causal effect of a cultural factor like religion is complicated by potentially

severe omitted variable and endogeneity problems: For instance, England and Italy differed on many dimensions already before England turned its back on Roman Catholicism. A useful first step towards clean identification is to restrict analysis to a within-country context so as to keep fixed already some factors like general climate or legal systems. In a Continental European context, identification is also somewhat facilitated by the fact that the present-day religious compositions of different regions do still largely follow those of previous centuries and that those were chosen by regional rulers rather than individuals themselves, a principle known by the slogan "Cuius regio, Eius religio" – "Whose rule His religion". Of course that does not rule out that the choice made by those rulers was in turn endogenous, for instance because rulers were somewhat responsive to the preferences of their subjects. Our first step in addressing these concerns is to focus on a region that, as we can show with historic data pre-dating the Protestant Reformation, was initially homogeneous – including along the important dimension of language, Switzerland's other main cultural divide–, but was then split into two parts, with the West being exogenously subjected to the teachings of Ascetic Protestantism, whereas the East remained subjected to those of Roman Catholicism. The focus on this region alleviates concerns about prior differences, and instrumenting the present-day influence of respectively Protestantism and Catholicism with the historical assignment of religion takes care also of more recent patterns of migration which may be at least partly endogenous to economic factors.

Three concerns however remain also after adopting this Instrumental-Variable strategy: Firstly, the Eastern, Catholic part becomes increasingly mountainous as one moves away from the common border. This implies a higher average altitude in the Catholic part, which might have had its own effect on work habits and chances for economic success. Secondly, the Western, Protestant part does on its Western side share a border with France, which again might have had effects of its own. And finally, in a similar vein, there is cosmopolitan, rich Geneva in the South-Western corner. While the many rich foreign residents, some of whom reside on the shores of lake Geneva outside Geneva itself and inside our sample region, are not allowed to vote in the referenda which we use as a measure of preferences, their presence may nonetheless have impacted preferences of those eligible to vote. To address these three concerns, we complement our Instrumental-Variable strategy with a Spatial Regression Discontinuity Design, focusing on municipalities nearby the historical religious border.

Measuring work ethic by voting in six federal referenda that were used for a similar purpose in Bruegger et al. [2009], we find a causal impact of Ascetic Protestantism on Work Ethic

that has persisted until today: Individuals in historically Protestant regions show between 5 and 15 percentage points lower support for proposals like shorter working hours or longer paid holidays. These results are found both in Instrumental-Variable regressions and in Spatial Regression Discontinuity regressions across all relevant bandwidths. They are also confirmed in regressions for Switzerland at large, thus showing that the external validity of our results is not confined to the narrow region that constitutes our quasi-experiment. We then confirm these results using individual-level data on actual working hours and the propensity to be an entrepreneur from the Swiss Labor Force Survey (SLFS). At the same time, differences in economic prosperity as measured by municipalities' average income before taxes is found in Instrumental-Variable estimates, but does not pass the test of our Regression-Discontinuity Design. In sum, for the region the data suggest that Weber's Protestant Ethic is still at work, but at least today and in present-day Switzerland it no longer leads to greater economic prosperity. Both results must be seen as conservative estimates of the full causal effect of different religions on preferences and economic outcomes, because the two regions, while becoming separate cantons as a consequence of the quasi-experimental events, were and are still part of the same country, and thus subjected to many equalizing forces.

The remainder of this paper is structured as follows: Section 1.2 presents Weber [1904]'s theory of a Protestant ethic and Section 1.3 introduces our data. Section 1.4 then presents our empirical strategy, from the need for quasi-experimental variation in religion, through our quasi-experimental setup and evidence of equality at the baseline, to the implementation of our Spatial Regression Discontinuity Design. Section 1.5 then presents our results and Section 1.6 concludes.

1.2 Weber's hypothesis of a Protestant work ethic

The hypothesis of a Protestant work ethic was famously stipulated in Weber [1904], where he writes of it:

> "Labour came to be considered in itself the end of life, ordained as such by God. St. Paul's 'He who will not work shall not eat' holds unconditionally for everyone. Unwillingness to work is symptomatic of the lack of grace" (Chapter 5)

This is then contrasted with the mediaeval Christian doctrine, which he considers to have remained dominant within Roman Catholicism, and which he describes as follows:

> "Here the difference from the mediaeval view-point becomes quite evident. Thomas Aquinas also gave an interpretation of that statement of St. Paul. But for him

> labour is only necessary natural reason for the maintenance of individual and community... Moreover, it holds only for the race, not for every individual"

To further illustrate the Protestant perspective, Weber quotes a number of statements made by Benjamin Franklin, among which the following:

> "Remember that time is money. He that can earn ten shilling a day by his labour, and goes abroad, or sits idle, one half of that day, though he spends but sixpence during his diversion or idleness, ought not to reckon that the only expense; he has really spent, or rather thrown away, five shillings besides." (p.48)

At the same time, Weber repeatedly makes it very clear that he is referring here to Calvinism rather than Lutheranism:

> "But not all the Protestant denominations seem to have had an equally strong influence in this direction. That of Calvinism, even in Germany, was among the strongest it seems, and the reformed faith more than the others seems to have promoted the development of the spirit of capitalism, in the Wupperthal as well as elsewhere. More so than Lutheranism" (Ch. I, p.44) ... "It is hardly necessary to point out that Luther cannot be claimed for the spirit of capitalism in the sense in which we have used that term above, or for that matter in any sense whatever... And Luther himself would, without doubt, have sharply repudiated any connection with a point of view like that of Franklin" (p.82) ... "Although the Reformation is unthinkable without Luther's own personal religious development, and was spiritually long influenced by his personality, without Calvinism his work could not have had permanent concrete success" (p.87)

In short, Max Weber clearly hypothesizes that specifically Ascetic Protestantism will lead to lower Preferences for Leisure, or a stronger "work ethic" as he calls it. In Section 1.5 below we shall see whether this prediction holds true in present-day Switzerland.

1.3 Data

To measure preferences for leisure, we use the fraction of citizens in each municipality voting for shorter working hours, earlier retirement or longer holidays in six Swiss referenda. Swiss voters go to the polls three to four times a year to vote directly on a number of specific policy proposals. The typical referendum voter has a good deal of experience with

this decision process[1], and typically each referendum is preceded by a thorough discussion in the national and local media of the issues at stake. Thus voters can be considered to be truly voting on the issue under consideration, rather than using referenda for instance to express their general level of satisfaction with the present government, as sometimes happens in referenda held in other countries. As a result, the referendum data can be considered to provide a measure of preferences in the spirit of the Paradigm of Revealed Preferences, a measure that may be considered more meaningful than those that can be obtained from mere survey data.

Data on the referenda are available at the municipality level for all referenda held after 1980. Since then, there have been six referenda dealing with working time regulations, three about the extensive (to work or not to work) and three about the intensive (how many hours to work) margin. A list of the precise questions and the year of the respective referendum is contained in the table of summary statistics, Table 1.1. The same six referenda are also used in Bruegger et al. [2009], who skillfully investigate differences in preferences for leisure along Switzerland's other main cultural divide, the linguistic one. Unlike us, they do not need to focus on a specific region, because they can take the geographical distribution of the different linguistic groups as given. In our case this is not possible, but identification is nonetheless feasible by use of our quasi-experiment together with the Spatial Regression Discontinuity Design. Another difference between our work and that of Bruegger et al. [2009] is that we are able to rely on the pre-existing Weber literature for specifying why our independent variable might have a causal effect on preferences for leisure.

As a proxy for the present-day influence of Protestantism, we use each municipality's share of Protestants, as recorded every ten years by the Swiss census.[2] Since our referenda go back to 1985, we take our measure of Protestantism from 1980, the latest census to predate all six referenda used. It is worth noting that today each municipality contains not only Catholics and Protestants, but also a few individuals without religious affiliation (on average between 1 and 3% of a municipality's inhabitants, see Table 1.1), as well as a few individuals with other religious affiliations. Since we have only one instrument, namely an indicator of whether or not a municipality was forced to convert to Protestantism in the 16th century, we need to add those groups to either the Catholic or the Protestant group. The results displayed are based on the former strategy, but due to the limited quantitative

[1] Between 1970 and 2003, Switzerland held 292 referenda, on average 8.48 per annum, which amounted to 40% of all referenda held worldwide over this period; See Kaufman and Waters [2003], Appendix A.

[2] All census-based data are available free of charge from the Swiss Federal Office of Statistics, www.bfs.admin.ch

importance of such other groups in our sample our results are robust to which choice we make here. The census also gives us a number of additional covariates. To avoid controlling for a variable that could have been one channel for the effect of Protestantism, our main specifications control only for those covariates that have plausibly been predetermined before the Reformation, namely altitude in 1000m above sea level as a measure of geography, and historic language.[3] We have also repeated the same regressions controlling for additional covariates such as a municipality's age structure, educational composition or working sector composition, and the results thus obtained yielded coefficients that were slightly but not significantly smaller than those here displayed.

The additional economic outcomes population and population density (inhabitants per kilometer squared) have also been obtained directly from the census. Data on each municipality's average income per capita as well as the Gini coefficient as a measure of the inequality of the income distribution within each municipality have been taken from Ecoplan [2004] and are based on tax statistics from 1995/6. In addition, we have been able to find 15th century population figures at the municipality level in Ammann [1937], based on visitations by the Papal delegations. It is worth noting that the availability of such disaggregated data is not standard for that time: The frequently used reference for historical population figures in Europe, Bairoch et al. [1988], for instance covers only the three relatively larger cities of Lausanne, Vevey and Fribourg. Finally, we have computed driving distances from each municipality to the religious border in km, using ArcGIS and maps provided by the Swiss Federal Office for Topography.

Finally, we complement our referenda with micro-data from the Swiss Labour Force Survey (SLFS). This is an annual survey of Swiss households first fielded in 1991. The data come from the 2009 wave, and contain observations on 49,498 individuals[4]. As the survey is mainly concerned with labor market issues, it does not contain respondents' religious affiliation. We can however use information on individuals' municipality of residence to merge in information on the municipality's share of Protestants, as given in the census data. This regressor should then be interpreted as a measure of the intensity of "Protestant" values amongst each individual's geographic peers. The dependent variables for this set of regressions are chosen to match the content of the referenda: To start with, we consider an indicator for whether the individual is in the labor force, and the number of hours worked per week conditional of

[3] As we explain below, most of our region is homogeneously French-speaking. In our main specification we control for the predominantly German language in those few municipalities affected, but dropping those municipalities entirely gives essentially the same results.

[4] For documentation of the dataset, see www.slfs.bfs.admin.ch

being in the labor force; then an indicator for whether or not the individual is retired before age 65. Finally, we define and use an entrepreneurship indicator that equals one if and only if the individual is self-employed and has employees, where the latter requirement is imposed to make sure that we capture "entrepreneurs by conviction" rather than "entrepreneurs by necessity". We have run these regressions first for both genders, and then for males only so as to take out any differences in behavior due to differential patterns of female emancipation.

1.4 Empirical Strategy

1.4.1 The Need for Quasi-Random Variation in Religion

When investigating the causal effect of different religions, one cannot simply compare countries with different religions, as these will also differ on many other accounts. But even within the same country, there is the concern that differences in religion between different groups of citizens may be endogenous, i.e. depend on unobserved third variables that may at the same time be correlated with the outcomes under investigation, thus biasing any estimates of the causal effect of religion on our outcomes. A feature typical of most European countries is that today's religious compositions of the population depend largely on the choices made not by citizens themselves, and not even by their own ancestors, but rather on choices made in the course of the 16th century – in most of present-day Germany by local noblemen, in the case of much of present-day Switzerland by local councils composed of the most prominent citizens.

This of course raises the question what made these local rulers or councils choose as they did. At first sight, their choices depended largely on geo-political considerations. Thus the historian Gordon [2002] writes:

> "There was nothing inevitable about what happened in Zurich in the 1520s. Opposition to the reformer remained strong and it was only through a few close relationships ... that Zwingli was able to sustain his position ... With Zwingli leading from Zurich, there was no chance of the movement making any headway in Uri, Schwyz, Unterwalden, Zug, and Lucerne. Bad memories of Zurich's earlier hegemonic aspirations quickly reappeared. That is why the principal battlegrounds were not in any of the Confederates ... but in the Mandated Territories which they administered jointly." (Gordon [2002]).

However, closer analysis reveals that the more mountainous regions were more likely to remain Catholic than the less mountainous ones. A major reason, it turns out, was the fact

that Protestant reformer Zwingli forbade his followers to engage in mercenary service, on which however the more rural regions depended for their income. This of course poses a challenge for identification of a causal effect of religion on attitudes and outcomes today: If for instance returns to hard work have traditionally been higher in the less mountainous regions, this alone may have led to a stronger work ethic there, which would lead to an upward-bias in our estimates of the effect of Protestantism on work attitudes. Conversely, one may imagine that the tougher living conditions in the mountains have led to a stronger work ethic, which would downward-bias our estimates.

A second concern stems from the fact that, since the Reformation was initially and during most of the territorial competition with Catholicism a German-speaking movement[5], its spread is largely restricted to the German-speaking parts of Switzerland, whereas the French- and Italian-speaking areas are predominantly Catholic. This might lead one to worry that differences in work attitudes might at least partly be due to different cultural influences from respectively the German- and the French- or Italian-speaking neighboring countries. In fact, Bruegger et al. [2009] show precisely that individuals in the French- and Italian-speaking parts of Switzerland do on average place a higher value on leisure than those in the German-speaking parts. Of course one can try to control for such confounding factors, but the worry must remain that such controls fail to fully capture the confounding influences.

Trying to control for these factors, and in particular for geography, cannot provide a fully satisfactory solution, because the available variables like altitude above sea level are unlikely to capture all relevant features of geography. Furthermore, once we have recognized that choices of local elites were not fully random, it seems likely that they depended also on other factors of which we are unaware. What we need therefore is a good quasi-experiment, i.e. a quasi-random assignment of different religions to two otherwise identical regions. This is indeed what we use in this paper, and explain in the following subsection.[6]

1.4.2 The Natural Experiment I: Exogenous Assignment

We focus on a region in Western Switzerland that, as we shall show, was homogeneous until the beginning of the 16th century, but was then split into two parts, with the West being forced to adopt Protestantism, and the East to remain Catholic. In this section we

[5] French-speaking John Calvin got involved only after the death of Zwingli and only after the "Second Battle of Kappel" between Catholic and Protestant cantons.

[6] Further accounts of the assignment of religion in Switzerland in general can be found in Moeller [1978], Schaab [1993], and Schindling and Ziegler [1989].

first expound the details of the imposition, and then investigate whether the two parts were identical in all other respects.

Until 1513 the entire region was a subject territory of the Roman Catholic kingdom of Burgundy. Then however it was jointly conquered by the Swiss Confederacy, at the time a lose self-defense alliance of otherwise independent republics. For the next 23 years the region was then jointly ruled by these Confederates, as can be seen in the map in Figure 1.2. In 1536 however, the two most powerful members of the alliance, the city republics of Berne and Fribourg, decided to pay the others off and split the region amongst the two of them: The Eastern half fell to neighboring Fribourg, and the Western half to Berne.

As the latter had recently become Protestant, the new rulers – in order to facilitate governance of their new territories – imposed Zwinglian Protestantism everywhere, so that the new religious authorities would all be based in Berne rather than Rome, and could more easily be persuaded to preach citizens to obey also the worldly authorities of Berne. Of course one might think that the region would have become Protestant anyway, but the historical accounts suggest otherwise: As late as 1534 the deliberative assembly of Vaud, meeting at Moudon, decided explicitly that they would like to remain Catholic.[7] When they were nonetheless forced to become Protestant, peasants started rioting, and the hitherto powerful local authorities lost their jobs, as Berne preferred to replace them with loyal authorities educated in the Protestant faith. As Bruening [2005] writes:

> "[The Reformers] found little sympathy for the evangelical cause in Vaud. Late medieval Catholicism was alive and well in the region, where the people did not even seem to be aware of the Reformation before 1525... [and] much less enthusiastic about its central messages."

A major reason for the ignorance of the Reformation until that point, as Bruening [2005] points out, was language: Most of the Reformation documents had been published in German, and those few Lutheran pamphlets published in French had all been printed in either Paris or Antwerp, but none in French-speaking Switzerland. Furthermore, the Catholic duke of Savoy, while defeated by Berne, lingered constantly on the horizon, threatening to take back the territory and to restore religious obedience to Rome:

> "Catholicism was deeply ingrained among the people of Vaud, and the possibility of returning to Savoyard rule encouraged them in their resistance to the new ...

[7] For details, see Feller [1953], p.379

religion."(Bruening [2005])[8]

One initially homogeneously Catholic region had thus been split into one henceforth Protestant and one Catholic part. Three districts within those two regions were somewhat special though and need to be dealt with specifically: Three "Common Lordships", places where important battles in the Burgundy Wars had taken place, were henceforth ruled jointly by the Protestant city republic of Berne and the Catholic one of Fribourg. As a consequence, citizens of these districts were allowed to freely choose their religion. These districts –Echallens-Orbe, Grandson and Murten– are marked with yellow needles in the map in Figure 1.1. To restrict the quasi-experiment to include only municipalities which had their religions imposed by external rulers, these three districts have been dropped from the sample. Since this concerns only a few municipalities, the results obtained without dropping them are however very similar.

1.4.3 The Natural Experiment II: Equality at the Baseline

We have demonstrated that before the new assignment of religion, the Western half had not been more predisposed to Protestantism than the Eastern half. For the experiment to be valid however, it is necessary that the regions were identical also in other respects, for if they differ in terms of some of the variables dealt with in the previous subsection, such as geography or language, we cannot exclude the possibility that this difference has differentially influenced our outcome variables.

The key variable we are interested in for this purpose is income per capita. No direct measure thereof is typically available for the period before 1500, but a proxy commonly used in research on economic history is population size: Thus Bairoch [1991] (Chapter 1) and Vries [1984] (p.164) argue that only areas with high agricultural productivity and a developed transportation network could support larger populations. Furthermore, Acemoglu et al. [2002] present evidence that there is a close association between urbanization and income per capita before as well as after industrialization. Implementing this measure at a level as disaggregated as the municipalities in our region of interest however is often not feasible, because frequently used datasets on population figures, such as Bairoch et al. [1988], cover only a few larger places, in our region Lausanne, Vevey and the city of Fribourg. Fortunately though Ammann [1937] does contain more disaggregated data, based on a count of the

[8] Another excellent account of Bernese rule of the Vaud, on which we have drawn on is provided in Holenstein et al. [2006].

number of fire places conducted in the course of Papal visitations in the 15th century. T-tests fail to reject the Null Hypothesis that these numbers were on average the same across the two regions (Table 1.1), and a regression discontinuity analysis does not find evidence of a jump in the variable at the border between the two regions (Figure 1.5).

We also look at a number of qualitative factors, starting with political structures. As has been pointed out above, until the experiment both parts shared a common structure of governance, until 1513 as subject territory of Burgundy, and from 1513 through 1536 as a common subject territory of the Swiss Confederacy, as illustrated in Figure 1.2.[9] Secondly, we look at Switzerland's other main cultural divide, language: Here both regions lie almost entirely within the French-speaking part of Switzerland, apart from the district (Swiss administrative level between municipality and canton) of Sense in the Catholic half.[10] To ensure that this does not bias our results, we control for this difference in all our regressions with a dummy for whether the municipality has historically been German-speaking.[11] We have also conducted the same set of analyses fully excluding the two traditionally German-speaking districts from the sample, i.e. restricting our sample to French-speaking municipalities only, and doing so yields very much the same results.

As a result, our baseline specification takes the form

$$Y_m = \alpha + \beta P_m + \gamma A_m + \delta G_m + \varepsilon_m \tag{1.1}$$

where Y_m is the outcome for municipality m, P_m is its share of Protestants, and A_m and G_m are the two available covariates that can be thought to have been fixed prior to our quasi-experiment, altitude and an indicator for German language respectively. It is worth emphasizing that within the sub-sample of this region the simple least-squares estimates will already be free of the historical selection issues discussed above and so OLS estimates of the coefficient β should already get us fairly close to the true effect. One confounding factor that remains however will be selective migration in recent years. Therefore our second step is to estimate Equation 1.1 instead by Instrumental-Variables, instrumenting the present-day share of Protestants with the historical assignment of religion.

[9] By contrast, they became separate cantons, i.e. Swiss states, afterwards and as a consequence of the quasi-experiment.

[10] As well as Murten (now part of the District du Lac), which however we have already dropped from the sample for being a "Joint Territory", see above.

[11] We use this rather than today's share of inhabitants with German as first language in order to avoid picking up recent migration patterns, which must be considered as potentially endogenous to present-day outcomes.

The IV estimates should get us even closer to the truth, but one potential confounder that still remains, given our arguments above and a look at the satellite view presented in Figure 1.1, is geography: By and large this is identical across the two regions, with however one exception: In the Eastern, Catholic part, the municipalities farthest away from the border are situated in more mountainous territory. This can also be seen in Figure 1.4, which plots average altitude against distance from the border. While we can control for altitude per se, this will only imperfectly capture geography, because some municipalities may not be located so high themselves and yet be hard to reach from other towns and villages because of higher mountains surrounding them. Relatedly, one may worry that outcomes in the Western part are for some municipalities influenced, for better or worse, by their proximity to present-day France, or in the South West by their proximity to the city and canton of Geneva, even though these themselves are not in our sample. Our strategy for making sure that our final results are not biased by any of these three factors is to complement our Instrumental-Variable estimates with a Spatial Regression Discontinuity Design, which attaches greater weight to the municipalities close to the common border and which are hence identical in terms of those three factors. In the following subsection we explain this strategy more detail.

1.4.4 A Spatial Regression Discontinuity Design

The Spatial Regression Discontinuity Design (RDD) is based on the idea of focusing on municipalities near the border between the assigned and the not assigned region. Then even if, as discussed above, the two regions differ in their average geography, distance to France or distance to Geneva, the municipalities near the border will be equally affected by all of these factors, so that any discontinuity in outcomes between the two groups can credibly be attributed to the different treatment assignments. This manifests itself in a discontinuity of our treatment variable, a municipality's fraction of Protestants today, as a measure of the present-day influence of Protestant teaching. This fraction of Protestants does not jump from zero to one, because of conversion, secularization and migration, but it still jumps significantly. Hence we have what Trochim [1984] has called a "Fuzzy" (as opposed to "Sharp") Regression Discontinuity Design, where the estimate of the effect of Protestantism on our outcomes is given by the ratio of the reduced-form or intention-to-treat impact of assignment on outcomes divided by the first-stage impact of assignment on present-day treatment intensity.

More formally, let S_i denote the driving distance in km from municipality i to the border,

with distances in the Protestant region West of the border being coded as negative and those in the Catholic region East of the border being coded as positive values, so as to mirror the maps in Figures 1.2 and 1.1. Then if $\overline{s}$ is the discontinuity point, here normalized to $\overline{s} = 0$, the key requirement for a RDD is that the propensity of treatment *marginally* below and that *marginally* above $\overline{s}$ are significantly different: $\Pr(I = 1|\overline{s}^-) \neq \Pr(I = 1|\overline{s}^+)$. Figure 1.3 shows that this is clearly the case. By contrast, the hypothetical outcomes respectively without and with treatment, Y_0 and Y_1, *conditional on distance* S must be fully smooth around the discontinuity, i.e. $E[Y_0|\overline{s}^+] = E[Y_0|\overline{s}^-]$ and $E[Y_1|\overline{s}^+] = E[Y_1|\overline{s}^-]$.Of course, we observe each municipality only either as assigned to treatment or not assigned to treatment, so this smoothness condition cannot be tested directly. However, a useful test is to check whether baseline characteristics and pre-experiment outcomes evolve smoothly around the threshold. Figure 1.4 illustrates this for altitude: It shows both how the two regions differ in their average altitude, one motivation for us to implement a Spatial Regression Discontinuity Design here, and how altitude does however evolve smoothly around the threshold, validating our setup. Similarly, Figure 1.5 shows that also the number of fire places per municipality in 1416, as a measure of economic prosperity before the experiment, evolves very smoothly around the threshold.

As the number of observations directly *on* the border is limited, one choice to be made in the context of a Regression Discontinuity Design is which bandwidth to use. Smaller bandwidths are more credible in using fewer observations far away from the border, but on the other hand larger bandwidths allow to rely on more observations and thus promise greater statistical precision. Once we use observations also from outside the immediate neighborhood of the border, we need to flexibly control for distance from the border per se, thus changing Equation 1.1 into

$$Y_m = \alpha + \beta P_m + \theta(S_m) + \varepsilon_m \tag{1.2}$$

where $\theta(S_m)$ is a control function for the distance. Even then, our choice of the bandwidth might still matter, and so we probe the robustness of that choice by implementing our estimations for all bandwidths between 5km and 50km. We find our results to be very robust to that choice, as can be seen in Tables 1.5 and 1.6. They display for respectively referenda and other outcomes the results for bandwidths 5km, 10km, 20km, 30km, 40km and 50km, which we shall discuss in greater detail in Section 1.5 below. All graphical illustrations are for a bandwidth of 10km, but as the tables show, results for different bandwidths look quite similar in the relevant respects.

1.5 Results

A first, suggestive view can be gained from the summary statistics in Table 1.1, which compares the 330 municipalities in the traditionally Protestant part to the 155 in the traditionally Catholic one. Support for the more leisure option is between 2 and 7 percentage points lower here, while population size and density, mean income and income inequality are all higher. The picture is roughly confirmed by the OLS results in Table 1.2, which is based on regressions of the different referenda and economic outcomes on the share of Protestants in 1980, as a measure of the influence of Protestant thought: The prediction implied by the estimates is that in a fully Protestant municipality support for the more-leisure option will be between 7 and 18 percentage points lower, average income CHF 2,668 higher and the Gini measure of inequality will be 0.08 points higher, although population size and density will be somewhat lower. The regressions also confirm the need to control for altitude and language, both of which have statistically significant coefficients in most specifications.

As we move to the Instrumental-Variable estimates in Table 1.3, coefficient sizes in the referenda regressions become smaller, thus confirming the relevance of endogeneity like endogenous migration patterns also in our quasi-experimental sample, but they clearly remain statistically and economically significant, with coefficients between 5 and 14 percentage points. Coefficients for the economic outcomes in fact become larger, with an effect of CHF 6,085 on average income, 0.13 on the Gini coefficient, plus 1,233 inhabitants on the population size, and 0.19 on population density. The Reduced-Form or Intention-to-Treat coefficients behind these, displayed separately in Table 1.4, are naturally smaller, given the first-stage relationship of 0.64 (so a traditionally Protestant municipality now has on average 64 percentage points more Protestants than a traditionally Catholic one), but remain economically significant also on their own.

The most interesting results however, given our arguments in the Section 1.4 above, are those using the Fuzzy Spatial Regression Discontinuity Design. They are displayed in Table 1.5 for the referenda and in Table 1.6 for the other outcomes. For the referenda the previous results are qualitatively confirmed: The smallest coefficients, obtained with a bandwidth of 5km only, are if anything slightly larger and the largest ones, obtained with bandwidths of 30km or 40km, are a good deal larger. We see that for some referenda the size of the point estimate does vary considerably across bandwidths, but the implied qualitative pattern, namely a more pronounced "work ethic" in the traditionally Protestant municipalities, seems rather robust. Graphically this can be seen in Figures 1.6 through 1.11.

The same results were also found to hold for Switzerland as a whole, when we estimate

simple OLS regressions controlling not only for historical language and altitude, but also for area, population density, average age, education structure, the share of foreigners, unemployment rate and average income: 1.9 shows representative results of such regressions using as dependent variable the pro-leisure vote share averaged across all our referenda (columns (1) and (2)), and then using the first principal component thereof (columns (3) and (4)). In both cases the first regression is without and the second regression is with canton fixed effects. As can be seen the results are very similar to those obtained for our quasi-experiment. That shows that our results are not specific to the main sample used, but possess external validity also beyond that area.[12]

This is however not the case for the outcomes population, population density and income per capita: Here even the sign varies across bandwidths, and estimates of the effect on income are not statistically significant for any of the bandwidths. This finding can also be seen graphically in Figure 1.12, which shows that income falls to the right of the border in the Catholic region, likely connected to the more mountainous geography there, whereas it rises to the left, likely reflecting the increasing proximity to Geneva, but there is no evidence of a discontinuity at the border. By contrast, Figure 1.13 illustrates and the bottom of Table 1.6 shows for various bandwidths that inequality does jump at the border, according to the Wald estimates by 0.11 units of the Gini coefficient, which ranges between 0 and 1 with 1 denoting a perfectly equal and 1 a perfectly unequal (one person earns everything) distribution. This last finding is in line with those in Basten and Betz [2011b].[13]

Finally, Tables 1.7 and 1.8 show respectively the OLS and the IV and ITT results from our analyses of the Swiss Labor Force Survey data. The dependent variables are respectively an indicator for being in the labor force, an indicator for being retired, weekly working hours conditional on being in the labor force, and an indicator for being self-employed with employes. The main regressor of interest is the share of Protestants in the municipality, which in the Instrumental-Variables version is instrumented with an indicator of whether the municipality was assigned to Protestantism in the 16th century. We control for a third-order polynomial in age, for the exogenous regressors altitude and traditional language of the municipality, and for calendar year fixed effects (omitting the year 2000). Standard errors

[12] They also suggest that after adding this long list of controls the endogeneity of religion is not as much of an issue also in Switzerland at large as one might have feared, but this of course is something we know only ex post.

[13] Another possible confounding factor arises from foreign millionaires settling all along the "golden" shore of Lake Geneva also outside Geneva, and who would not be allowed to vote but might partially show up in the income figures. Gini results however remain also when we restrict the bandwidth to 5km around the border, which does not touch the surroundings of the shore.

are clustered by municipality, and displayed in brackets are the corresponding P-values. The regressions displayed here are for males only to take out any effects of differential norms on female emancipation, but the results using both genders are similar. Furthermore, the regressions are for individuals aged 18-85; official retirement age is 65, but our sample includes both individuals retired before 65 and individuals still in the labor force beyond age 65, both of which is possible and connected to respectively lower or higher retirement pensions; results of the retirement regressions in column 2 are thus driven by both.

The predictions of IV (OLS) are that in a fully Protestant municipality individuals will ceteris paribus be 8.9 (8.4) percentage points more likely to be in the labor force, and 7 (7.8) percentage points less likely to be retired. They are also predicted to work 0.182 hours less (3.36 hours more) per week conditional on being in the labor force and to be 2.7 (7.7) percentage points more likely to be entrepreneurs, although the latter two estimates are not statistically significant. Some caution is due in interpreting these regressions, as the behavior here reflected will partly be due to choices on the employee side and partly due to labor demand and regulations, although the latter are set democratically (through referenda like those used above, as well as by electing candidates with certain political platforms) and can thus be also be thought to reflect one channel of the treatment effect.[14] For what they are worth though, these results do provide an additional confirmation of the picture that emerged above whereby traces of a "Protestant ethic" can indeed still be found in present-day Switzerland.

1.6 Conclusion

We have identified the causal effect of "Ascetic Protestantism" on preferences for leisure and economic prosperity in present-day Switzerland. The results are supportive of Max Weber's famous hypothesis whereby Ascetic Protestantism has cultivated a stronger "work ethic". By contrast, we do not find robust evidence of effects on present-day economic prosperity. The results have been obtained by comparing two parts of an initially homogeneous region which in the 16th century were subjected to different religions for reasons that, as we have demonstrated, were unrelated to any factors with a potentially differential impact on our present-day outcomes of interest. An additional layer of robustness has been added through the implementation of a Spatial Regression Discontinuity Design around the historic

[14] For instance the choice of the retirement age will likely depend also on the sectoral and educational composition of the different local economies, so these may act as a channel in addition to the more direct channel of work ethic alone. Hence we see these results merely as suggestive complements to our results on referenda voting,

religious border. The results are shown to be not only internally, but also externally consistent, with confirmations of the same patterns in micro-data and in data for Switzerland as a whole. They are most fascinating, since our results must be considered to be conservative estimates, seeing that our treatment and control regions are nowadays part of the same country: If the two regions had become separate countries, their trajectories would arguably have diverged more.

To our knowledge, this is the first paper on the issue that uses direct measures of "work ethic" and also the first to focus on the "Ascetic branches" of Protestantism. The paper speaks not only to the recent empirical literature on Max Weber's classic, but also shows more widely the continued relevance of historical events and of culture for present-day economic outcomes.

1.7 Appendix

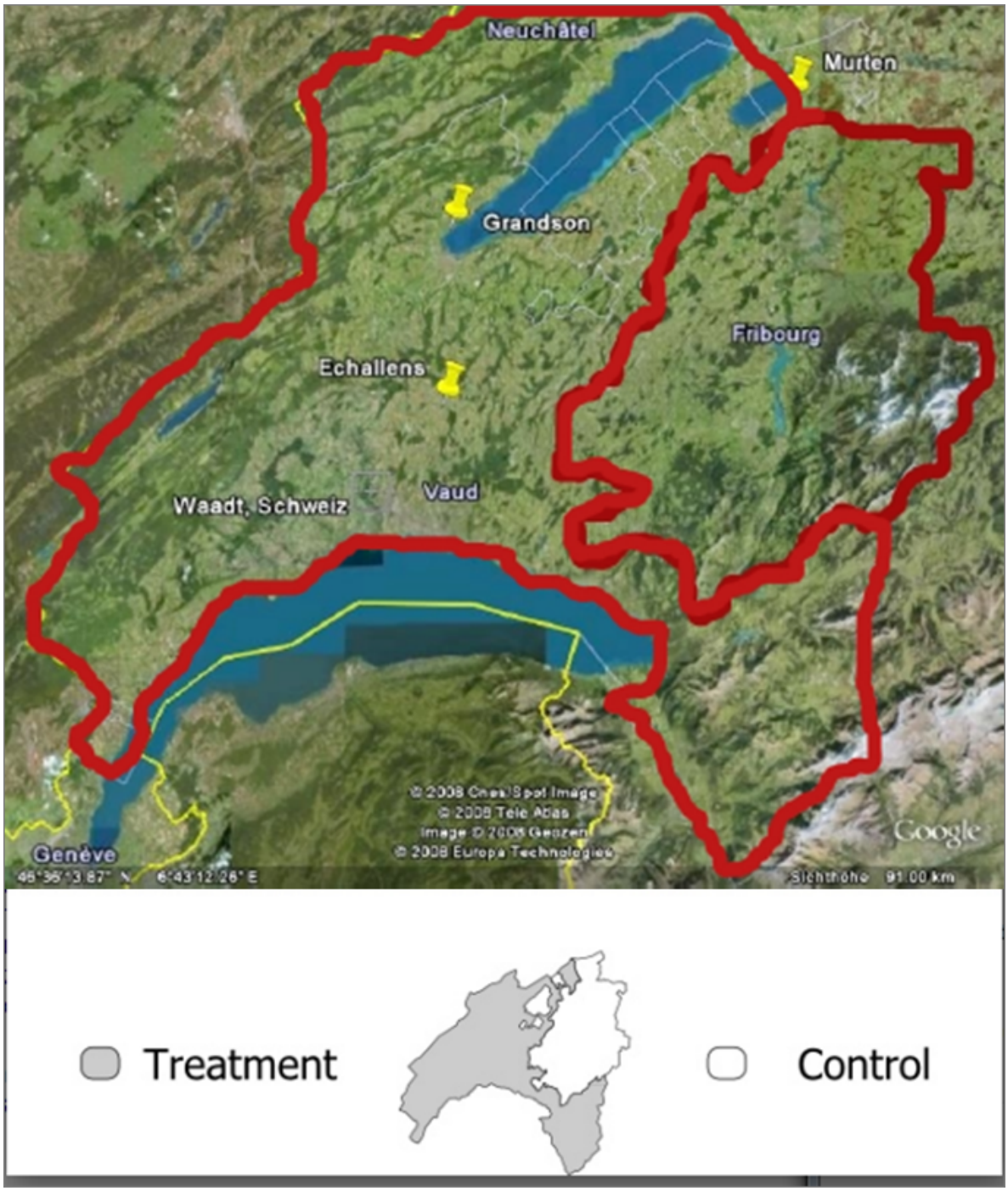

Figure 1.1: Satellite image of the region in which the quasi-experiment was set.

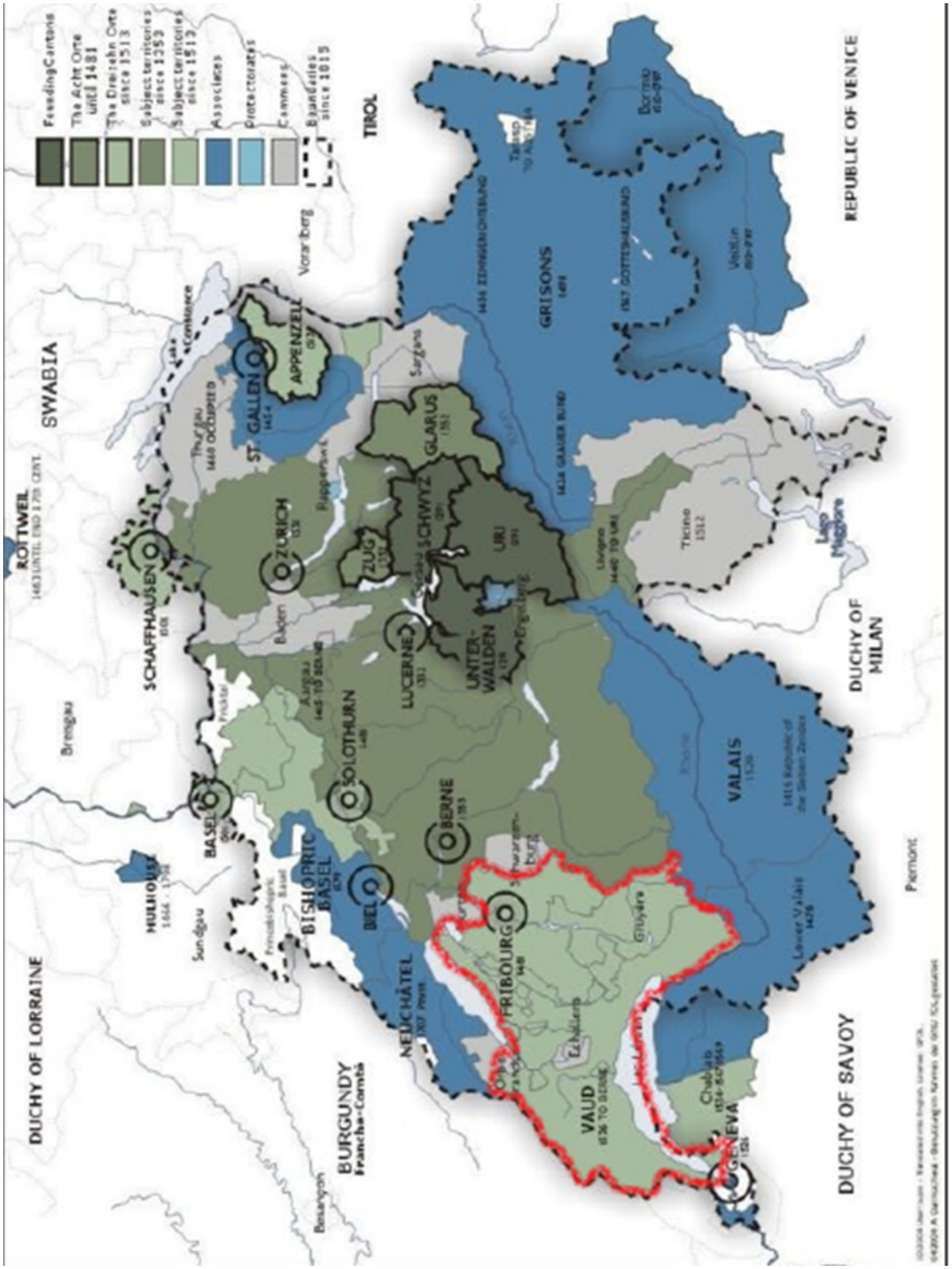

Figure 1.2: This map depicts the political setup prior to the quasi-experiment.

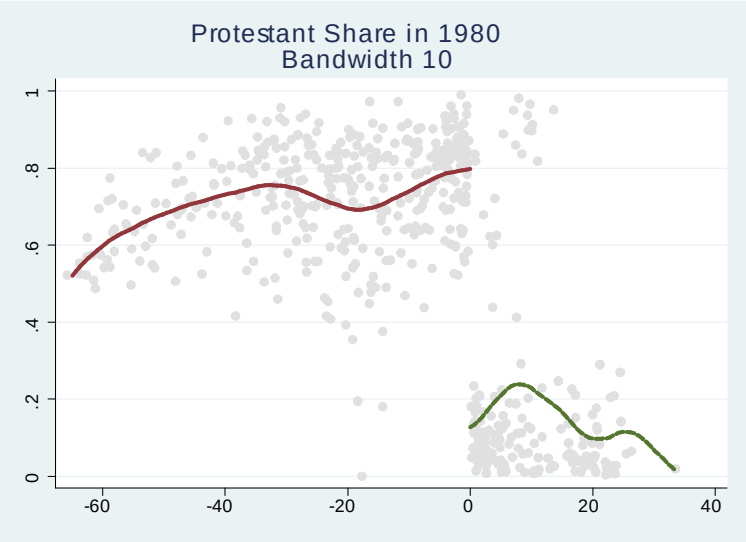

Figure 1.3: Graphical representation of the Regression Discontinuity Design with the dependent variable Share of Protestants in 1980. Local linear regressions with a bandwidth of 10km as specified in Eq. 1.2 and the text.

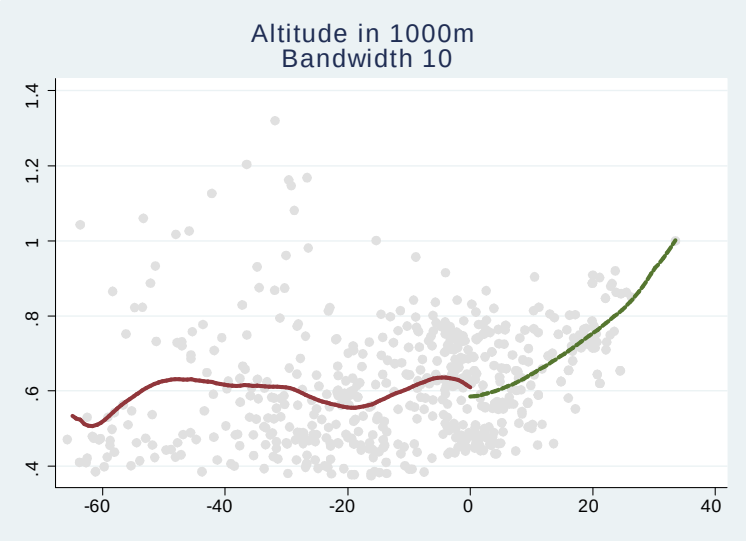

Figure 1.4: Graphical representation of the Regression Discontinuity Design with the dependent variable Altitude in 1000m above sea level. Local linear regressions with a bandwidth of 10km as specified in Eq. 1.2 and the text.

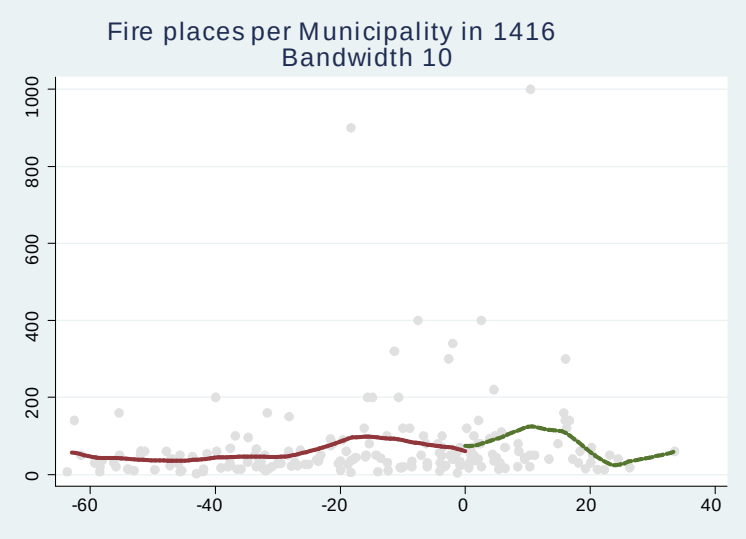

Figure 1.5: Graphical representation of the Regression Discontinuity Design with the dependent variable Number of Fire Places in 1416. Local linear regressions with a bandwidth of 10km as specified in Eq. 1.2 and the text.

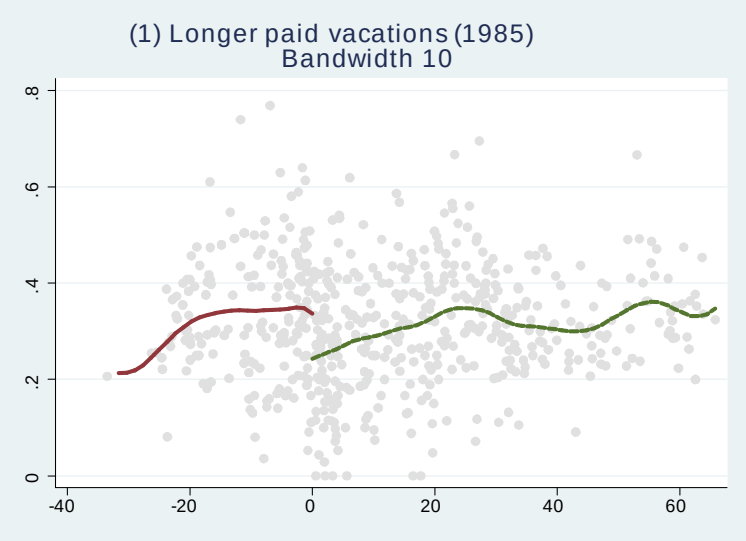

Figure 1.6: Graphical representation of the Regression Discontinuity Design with the dependent variable Referendum IM 1 (see Table 1.1). Local linear regressions with a bandwidth of 10km as specified in Eq. 1.2 and the text.

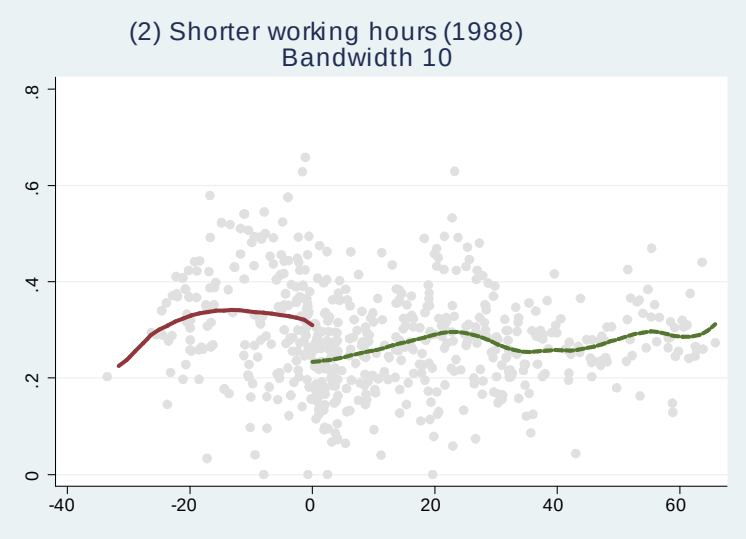

Figure 1.7: Graphical representation of the Regression Discontinuity Design with the dependent variable Referendum IM2 (see Table 1.1). Local linear regressions with a bandwidth of 10km as specified in Eq. 1.2 and the text.

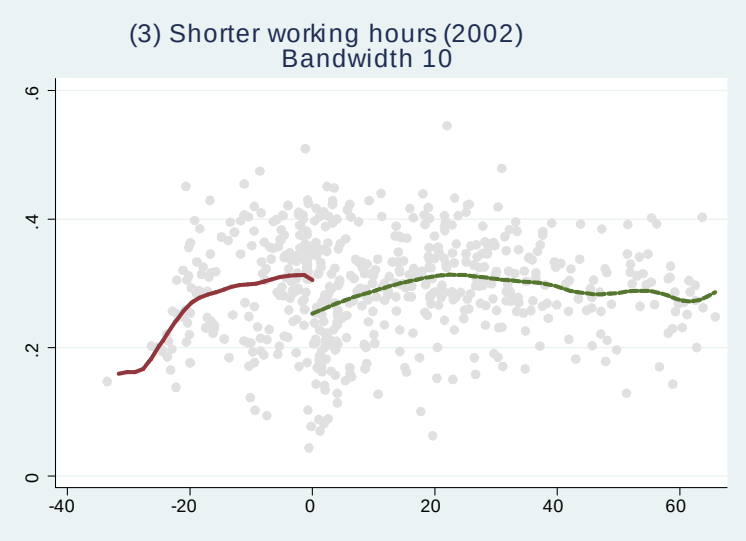

Figure 1.8: Graphical representation of the Regression Discontinuity Design with the dependent variable Referendum IM3 (see Table 1.1). Local linear regressions with a bandwidth of 10km as specified in Eq. 1.2 and the text.

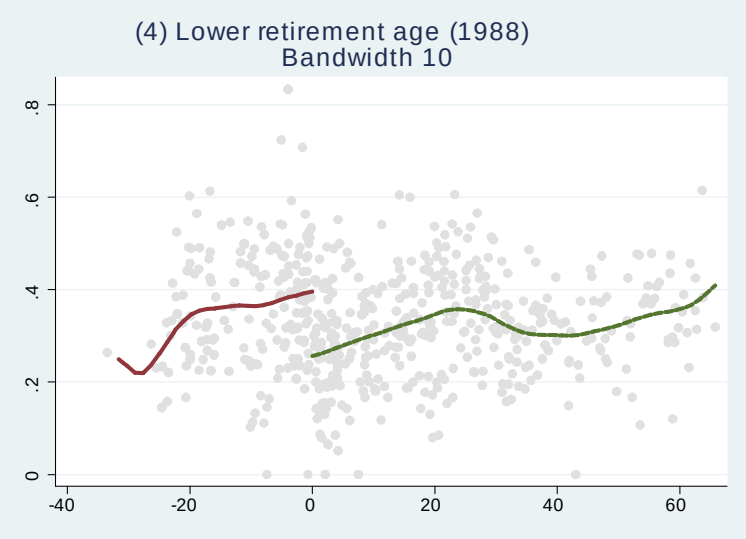

Figure 1.9: Graphical representation of the Regression Discontinuity Design with the dependent variable Referendum EM1 (see Table 1.1). Local linear regressions with a bandwidth of 10km as specified in Eq. 1.2 and the text.

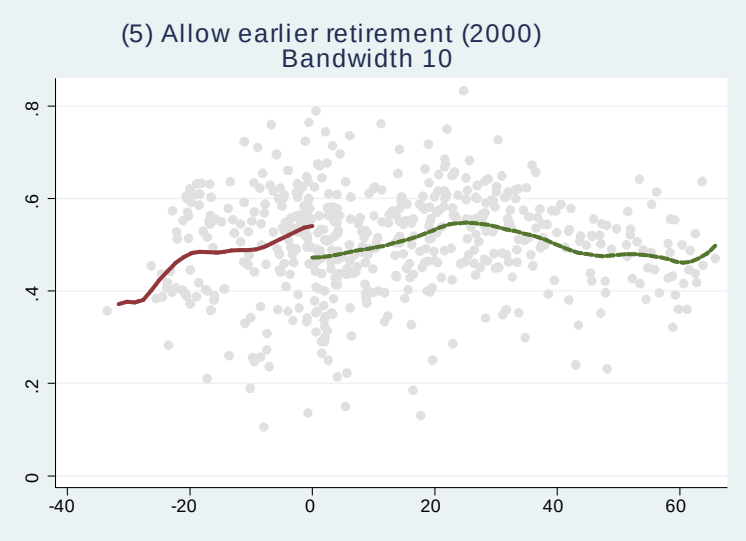

Figure 1.10: Graphical representation of the Regression Discontinuity Design with the dependent variable Referendum EM2 (see Table 1.1). Local linear regressions with a bandwidth of 10km as specified in Eq. 1.2 and the text.

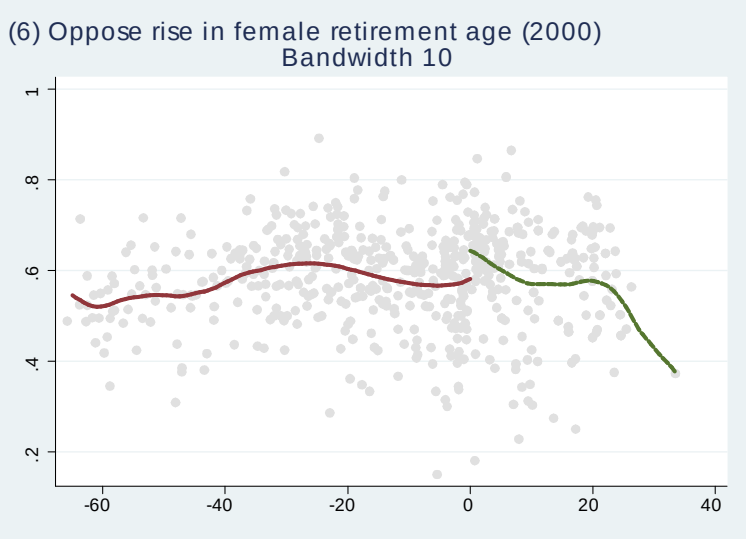

Figure 1.11: Graphical representation of the Regression Discontinuity Design with the dependent variable Referendum EM3 (see Table 1.1). Local linear regressions with a bandwidth of 10km as specified in Eq. 1.2 and the text.

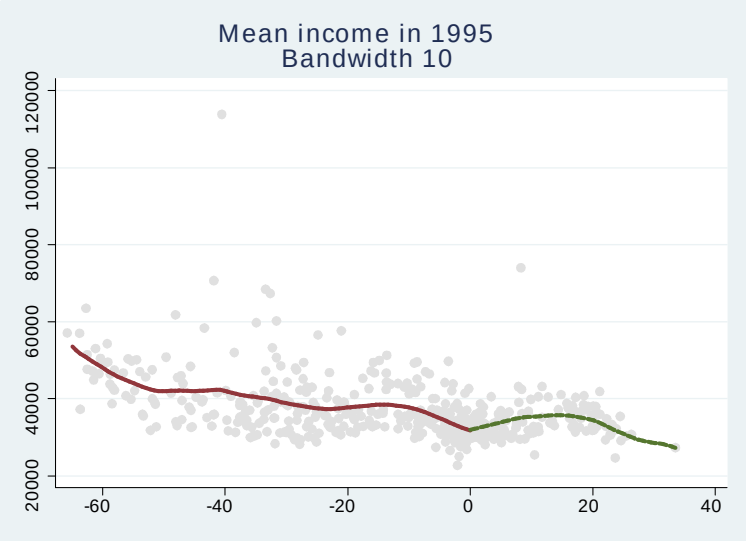

Figure 1.12: Graphical representation of the Regression Discontinuity Design with the dependent variable Mean Income in 1995 (see Table 1.1). Local linear regressions with a bandwidth of 10km as specified in Eq. 1.2 and the text.

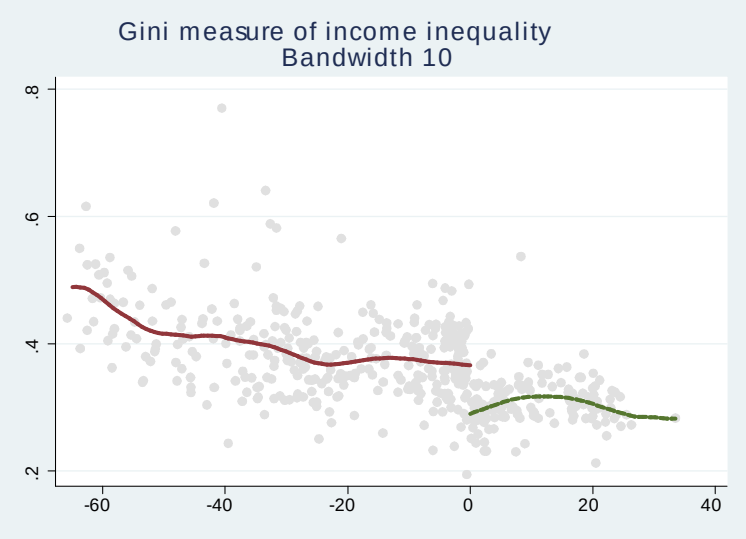

Figure 1.13: Graphical representation of the Regression Discontinuity Design with the dependent variable Gini Coefficient of Income Inequality in 1995 (see Table 1.1). Local linear regressions with a bandwidth of 10km as specified in Eq. 1.2 and the text.

Table 1.1: Summary Statistics

	Protestant			Catholic			Compared
	N	Mean	SD	N	Mean	SD	T
Baseline Characteristics							
Fire places before the Quasi-Experiment	142	62.99	95.49	72	76.04	127.24	-0.77
Altitude (1000m)	330	0.59	0.17	155	0.67	0.13	-5.76
German-speaking	330	0.00	0.00	155	0.26	0.36	-9.10
Treatment Intensity Today							
Share of Protestants (1980)	330	0.73	0.13	155	0.11	0.15	43.79
Share of Catholics (1980)	330	0.20	0.11	155	0.86	0.15	-47.81
Share w/o religion (1980)	330	0.04	0.03	155	0.01	0.01	15.14
Intensive Margin Preferences							
(IM1) Longer paid vacations (1985)	330	0.31	0.12	155	0.35	0.13	-3.36
(IM2) Shorter working hours (1988)	330	0.27	0.09	155	0.34	0.11	-6.75
(IM3) Shorter working hours (2002)	330	0.29	0.08	155	0.31	0.08	-2.08
Extensive Margin Preferences							
(EM1) Lower retirement age (1988)	330	0.31	0.11	155	0.38	0.13	-5.82
(EM2) Allow earlier retirement (2000)	330	0.50	0.10	155	0.52	0.10	-1.97
(EM3)Oppose rise in fem. retirement age (2000)	330	0.58	0.10	155	0.61	0.10	-3.33
Economic Outcomes							
Population size	330	1,848	7,455	155	1,071	1,541	1.81
Density (Inhabitants/sqkm)	330	0.33	0.77	155	0.16	0.19	3.71
Mean Income (in CHF, 1996)	330	38,265	8,701	155	34,001	4,705	6.99
Income Inequality (Gini, 1996)	330	0.39	0.07	155	0.31	0.04	17.45

Table 1.2: Simple OLS regressions of Referenda and Outcomes on the Present-Day Share of Protestants

	Protestant	Altitude	German	Constant	R2	N
Referenda						
Intensive Margin 1	-0.14	-0.16	-0.02	0.50	0.14	485
	(.000)***	(.000)***	(.393)	(.000)***		
Intensive Margin 2	-0.15	-0.15	0.04	0.46	0.25	485
	(.000)***	(.000)***	(.045)**	(.000)***		
Intensive Margin 3	-0.07	-0.08	-0.05	0.39	0.09	485
	(.000)***	(.000)***	(.002)***	(.000)***		
Extensive Margin 1	-0.18	-0.08	-0.06	0.48	0.18	485
	(.000)***	(.021)**	(.012)**	(.000)***		
Extensive Margin 2	-0.11	0.03	-0.13	0.56	0.11	485
	(.000)***	(.286)	(.000)***	(.000)***		
Extensive Margin 3	-0.12	0.04	-0.13	0.64	0.13	485
	(.000)***	(.153)	(.000)***	(.000)***		
Econ. Outcomes						
Population (2000)	-1,542	-4,774	-318	5,408	0.02	485
	(.002)***	(.005)***	(.587)	(.000)***		
Population Density (2000)	-0.24	-1.26	-0.08	1.19	0.10	485
	(.000)***	(.000)***	(.192)	(.000)***		
Mean Income (CHF, 1996)	2,668	-16,100	805	45,358	0.12	485
	(.003)***	(.000)***	(.382)	(.000)***		
Gini, 1995	0.08	-0.07	-0.02	0.36	0.20	485
	(.000)***	(.000)***	(.005)***	(.000)***		

Referenda as in T. 1.1; Share of Protestants from 1980 census, altitude in 1000m, "German" explained in text

P-values in parentheses based on robust SEs; * 0.10 ** 0.05, *** 0.01

Table 1.3: IV Results: Instrumenting the Present-Day Share of Protestants with the Historical Assignment of Religion

	Protestant	Altitude	German	Constant	R2	N
Referenda						
Intensive Margin 1	-0.08	-0.15	0.01	0.46	0.12	485
	(.000)***	(.000)***	(.553)	(.000)***		
Intensive Margin 2	-0.10	-0.14	0.06	0.43	0.24	485
	(.000)***	(.000)***	(.001)***	(.000)***		
Intensive Margin 3	-0.05	-0.08	-0.03	0.37	0.08	485
	(.000)***	(.000)***	(.027)**	(.000)***		
Extensive Margin 1	-0.14	-0.08	-0.03	0.46	0.18	485
	(.000)***	(.030)**	(.143)	(.000)***		
Extensive Margin 2	-0.08	0.03	-0.11	0.54	0.10	485
	(.000)***	(.208)	(.000)***	(.000)***		
Extensive Margin 3	-0.10	0.04	-0.12	0.63	0.12	485
	(.000)***	(.122)	(.000)***	(.000)***		
Econ. Outcomes						
Population (2000)	1,233	-4,342	1,246	3,526	0.00	485
	(.034)**	(.005)***	(.000)***	(.000)***		
Population Density (2000)	0.19	-1.19	0.17	0.90	0.06	485
	(.001)***	(.000)***	(.000)***	(.000)***		
Mean Income (CHF, 1996)	6,085	-15,500	2,731	43,040	0.11	485
	(.000)***	(.000)***	(.001)***	(.000)***		
Gini, 1995	0.13	-0.06	0.01	0.33	0.17	485
	(.000)***	(.001)***	(.424)	(.000)***		

Referenda as in T. 1.1; Share of Protestants from 1980 census, altitude in 1000m, "German" explained in text

P-values in parentheses based on robust SEs; * 0.10 ** 0.05, *** 0.01

Table 1.4: Intention-To-Treat Effect: Direct regressions of Referenda and Outcomes on the Historical Assignment of Religion

	Protestant	Altitude	German	Constant	R2	N
Referenda						
Intensive Margin 1	-0.05	-0.16	0.01	0.46	0.06	485
	(.000)***	(.000)***	(.712)	(.000)***		
Intensive Margin 2	-0.07	-0.15	0.06	0.43	0.15	485
	(.000)***	(.000)***	(.004)***	(.000)***		
Intensive Margin 3	-0.03	-0.08	-0.04	0.37	0.05	485
	(.000)***	(.000)***	(.019)**	(.000)***		
Extensive Margin 1	-0.09	-0.09	-0.04	0.45	0.09	485
	(.000)***	(.020)**	(.081)*	(.000)***		
Extensive Margin 2	-0.05	0.02	-0.12	0.54	0.06	485
	(.000)***	(.365)	(.000)***	(.000)***		
Extensive Margin 3	-0.06	0.03	-0.12	0.63	0.08	485
	(.000)***	(.267)	(.000)***	(.000)***		
Econ. Outcomes						
Population (2000)	791	-4,217	1,321	3,564	0.02	485
	(.033)**	(.005)***	(.000)***	(.000)***		
Population Density (2000)	0.12	-1.17	0.18	0.91	0.09	485
	(.001)***	(.000)***	(.000)***	(.000)***		
Mean Income (CHF, 1996)	3,904	-14,900	3,102	43,227	0.15	485
	(.000)***	(.000)***	(.000)***	(.000)***		
Gini, 1995	0.08	-0.05	0.01	0.34	0.31	485
	(.000)***	(.003)***	(.042)**	(.000)***		

Referenda as in T. 1.1; Share of Protestants from 1980 census, altitude in 1000m, "German" explained in text

P-values in parentheses based on robust SEs; * 0.10 ** 0.05, *** 0.01

Table 1.5: Fuzzy Spatial RDD Estimates for the 6 Preference for Leisure Referenda

		5km	10km	20km	30km	40km	50km
IM1	ITT	-0.059	-0.111	-0.114	-0.125	-0.120	-0.114
		(0.173)	(0.002)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
	Wald	-0.088	-0.155	-0.163	-0.185	-0.181	-0.175
		(0.164)	(0.001)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
IM2	ITT	-0.078	-0.101	-0.106	-0.115	-0.108	-0.104
		(0.015)**	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
	Wald	-0.115	-0.141	-0.151	-0.170	-0.164	-0.159
		(0.019)**	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
IM3	ITT	-0.061	-0.077	-0.076	-0.079	-0.077	-0.074
		(0.152)	(0.001)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
	Wald	-0.090	-0.108	-0.109	-0.117	-0.116	-0.113
		(0.153)	(0.001)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
EM1	ITT	-0.143	-0.154	-0.152	-0.155	-0.148	-0.141
		(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
	Wald	-0.211	-0.215	-0.217	-0.229	-0.223	-0.216
		(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
EM2	ITT	-0.089	-0.090	-0.089	-0.094	-0.091	-0.085
		(0.020)**	(0.001)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
	Wald	-0.131	-0.125	-0.128	-0.138	-0.137	-0.131
		(0.019)**	(0.001)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
EM3	ITT	-0.078	-0.086	-0.088	-0.092	-0.091	-0.087
		(0.014)**	(0.002)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
	Wald	-0.115	-0.120	-0.127	-0.136	-0.138	-0.133
		(0.014)**	(0.002)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***

Estimation of Eq. 1.2; Dependent variables are the same referenda as in T. 1.1

The Endogenous Regressor is the share of Protestants from 1980 census

P-values in parentheses based on SEs obtained through bootstrap; * 0.10 ** 0.05, *** 0.01

Table 1.6: Fuzzy Spatial RDD Estimates for Economic Outcomes

		5km	10km	20km	30km	40km	50km	N
Population	ITT	42.418	-478.599	-1298.319	247.359	864.793	1095.800	485
		(0.881)	(0.089)*	(0.330)	(0.421)	(0.033)**	(0.017)**	
	Wald	62.851	-646.421	-1921.300	375.902	1346.887	1707.968	
		(0.883)	(0.086)*	(0.331)	(0.427)	(0.035)**	(0.018)**	
Pop. Den.	ITT	0.036	-0.130	-0.042	0.017	0.131	0.165	485
		(0.157)	(0.071)*	(0.501)	(0.791)	(0.026)**	(0.004)***	
	Wald	0.053	-0.176	-0.062	0.026	0.204	0.257	
		(0.175)	(0.073)*	(0.499)	(0.793)	(0.028)**	(0.006)***	
Mean Inc.	ITT	-261.500	-14.781	97.736	599.426	577.250	261.192	485
		(0.786)	(0.988)	(0.869)	(0.412)	(0.442)	(0.734)	
	Wald	-387.471	-19.963	144.633	910.925	899.049	407.107	
		(0.786)	(0.988)	(0.869)	(0.407)	(0.446)	(0.733)	
Gini	ITT	0.075	0.079	0.069	0.068	0.064	0.060	485
		(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	
	Wald	0.111	0.107	0.102	0.103	0.099	0.093	
		(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	

Estimation of Eq. 1.2; Dependent variables are the same outcomes as in T. 1.4 above.

The Endogenous Regressor is the share of Protestants from 1980 census

P-values in parentheses based on SEs obtained through bootstrap; * 0.10 ** 0.05, *** 0.01

Table 1.7: Swiss Labor Force Survey regressions: OLS

	In Labor Force	Retired	Weekly Hours	Entrepreneur
Share of Protestants	0.084	-0.078	3.363	0.077
	(.001)***	(.000)***	(.008)***	(.000)***
Age	0.09	-0.041	0.31	0.013
	(.000)***	(.000)***	(.344)	(.000)***
Age2	-0.002	0.001	0.004	0
	(.000)***	(.000)***	(.623)	(.001)***
Age3	0	0	0	0
	(.000)***	(.001)***	(.071)*	(.087)*
German-speaking	0.046	-0.014	1.658	0.007
	(.001)***	(.272)	(.017)**	(.604)
Altitude in 1000m	0.033	-0.013	2.949	0.06
	(.140)	(.451)	(.005)***	(.000)***
Constant	-0.459	0.606	29.35	-0.258
	(.000)***	(.000)***	(.000)***	(.000)***
N	10,882	10,882	8,370	10,882

Columns 1,2,4 are indicator variables; Observations from 1991-2009, with year fixed effects

P-values in parentheses based on standard errors clustered by municipality; * 0.10, ** 0.05, *** 0.01

Coeff. on Protestantism gives difference between 100% Protestant and 100% Catholic municipality

Table 1.8: Swiss Labor Force Survey regressions: IV and ITT

IV

	In Labor Force	Retired	Weekly Hours	Entrepreneur
Share of Protestants	0.089	-0.07	-0.182	0.027
	(.020)**	(.005)***	(.926)	(.298)
Age	0.09	-0.041	0.321	0.013
	(.000)***	(.000)***	(.327)	(.000)***
Age2	-0.002	0.001	0.004	0
	(.000)***	(.000)***	(.637)	(.001)***
Age3	0	0	0	0
	(.000)***	(.001)***	(.073)*	(.082)*
German-speaking	0.047	-0.013	1.279	0.002
	(.001)***	(.317)	(.057)*	(.896)
Altitude	0.033	-0.013	2.824	0.058
	(.049)**	(.004)***	(.648)	(.144)
Constant	-0.46	0.603	30.445	-0.243
	(.000)***	(.000)***	(.000)***	(.000)***
N	10,882	10,882	8,370	10,882

ITT

	In Labor Force	Retired	Weekly Hours	Entrepreneur
Treated	0.029	-0.023	-0.059	0.009
	(.021)**	(.006)***	(.926)	(.314)
Age	0.091	-0.041	0.319	0.013
	(.000)***	(.000)***	(.329)	(.000)***
Age2	-0.002	0.001	0.004	0
	(.000)***	(.000)***	(.635)	(.001)***
Age3	0	0	0	0
	(.000)***	(.001)***	(.073)*	(.079)*
German-speaking	0.054	-0.018	1.265	0.004
	(.001)***	(.179)	(.078)*	(.776)
Altitude in 1000m	0.053	-0.028	2.784	0.064
	(.053)*	(.005)***	(.650)	(.144)
Constant	-0.471	0.611	30.472	-0.246
	(.000)***	(.000)***	(.000)***	(.000)***
N	10,882	10,882	8,370	10,882

Columns 1,2,4 are indicator variables; Observations from 1991-2009, with year fixed effects

P-values in parentheses based on standard errors clustered by municipality; * 0.10, ** 0.05, *** 0.01

Coeff. on Protestantism gives difference between 100% Protestant and 100% Catholic municipality

Table 1.9: OLS Work Ethic Regressions for all of Switzerland

	(1)	(2)	(3)	(4)
Share Protestants	-0.0887	-0.1068	-0.7063	-0.8616
	(0.021)**	(0.000)***	(0.021)**	(0.000)***
German-speaking	-0.1407	-0.0752	-1.1577	-0.6207
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
Altitude	-0.1267	-0.0826	-1.0287	-0.6763
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
Area	-0.0075	0.0005	-0.0602	0.0039
	(0.179)	(0.797)	(0.174)	(0.796)
Pop. density	0.001	0.0014	0.0085	0.0115
	(0.253)	(0.000)***	(0.215)	(0.000)***
Mean age	0.004	-0.0008	0.0312	-0.0057
	(0.074)*	(0.256)	(0.075)*	(0.288)
Post-sec. educ.	-0.1721	0.0075	-1.2439	0.181
	(0.166)	(0.900)	(0.208)	(0.708)
Tertiary educ.	-0.2204	-0.1836	-1.6342	-1.3646
	(0.223)	(0.004)***	(0.264)	(0.008)***
Share foreigners	0.1139	0.1206	0.898	0.9717
	(0.100)*	(0.000)***	(0.106)	(0.000)***
Unemployment	0.0142	-0.0571	0.0835	-0.5341
	(0.944)	(0.688)	(0.958)	(0.640)
Taxable income	-0.012	-0.0059	-0.1237	-0.0695
	(0.682)	(0.583)	(0.603)	(0.424)
Constant	0.5846	0.5615	2.1236	1.8519
	(0.064)*	(0.000)***	(0.392)	(0.026)**
R2	0.55	0.7	0.55	0.71
N	2,587	2,587	2,587	2,587

Dependent variable in (1) and (2) is mean of pro-leisure vote shares across referenda,
in (3) and (4) the 1st principal component; (2) and (4) with canton (state) fixed effects
P-values in parentheses based on robust standard errors clustered by canton; stars as above.

CHAPTER 2

RELIGION AND POLITICAL PREFERENCES

2.1 Introduction

Why do countries differ so persistently in the extent to which governments intervene in the market, both before surplus is realized, and afterwards by redistributing it? Can these differences be explained by economic factors alone, or do political preferences also play a role? In particular, do such preferences depend on factors commonly denoted as "culture"?[1] These questions are inherently difficult to answer, because such factors, if at all relevant, are likely to interact with formal political institutions and the economy, and their causal impact is therefore hard to isolate empirically. This is particularly true in a cross-country context, where cultural differences are bundled with many other differences, for instance in geographic endowments or legal systems. Therefore our first step has been to focus on one particular cultural factor, religion, and restrict our analysis to the within-country context of Switzerland, in which some regions have historically been Catholic and others Protestant.[2]

But even within Switzerland, the mere observation of a correlation between political preferences and religion is not sufficient to establish causality: While the present-day religious compositions of Swiss or, for that matter, European regions depend largely on those of past centuries, the initial choice of religion made in the 16th century, after the Protestant Reformations, may be correlated with some other, unobserved factors that also affect political preferences today. One such factor that, as it turns out, was relevant in the Swiss case, is geography: In the 16th century, those members of the Swiss Confederation, then a lose self-defense alliance, which were situated in the more mountainous parts were, for lack of other economic opportunities, significantly more dependent on earning their living with the supply of mercenary service to various European powers. The Protestant Reformers however considered mercenary service unethical and urged their followers to renounce it. As a consequence, the more mountainous regions were more hesitant to adopt the Reformation.

[1] The first paragraph of Section 2.2 provides a precise definition of this term.

[2] In particular, adherents of "Reformed" – or, in the terms of Weber [1904], "Ascetic"– Protestantism. This was started by Zwingli in Zurich and Calvin in Geneva, and on Weber [1904]'s definition includes also Pietism, Methodism and Baptism, but not the Lutheran variant common in Germany or Scandinavia. Thus, unless stated otherwise, "Protestant" shall henceforth always refer to the Reformed type. For further discussion of this distinction, refer to the second-last paragraph of Section 2.3, as well as to Basten and Betz [2011a].

If however geography and the resulting economic opportunity set did or do also exert a direct influence on political preferences, then geography is a possible omitted variable, and it is not clear whether we can sufficiently control for it. Therefore mere correlations between the spread of different religions and different political preferences today do not allow us to rule out that, rather than being independent causes of behaviour, both are merely the consequences of underlying economic variables, as famously argued by Marx [1904][3]. To credibly answer the question of whether culture acts as an independent source, we need a natural experiment in which the assignment of religion can plausibly be considered as ignorable with respect to any other factor likely to impact political preferences today.

The experiment we exploit consists of the split, in the 16th century, of a hitherto homogenous region in South-Western Switzerland into two parts, and the imposition on one of them of Reformed Protestantism. Given that, as we shall show, the two regions can otherwise be considered identical in terms of such factors as formal political institutions, language or economic prosperity at the baseline, and since furthermore we have strong evidence that people in the region that was then made Protestant were by no means more welcoming to Protestant ideas than were their neighbours, we can instrument our measure of the present-day influence of Protestant thought with an indicator variable for whether the municipality is situated in the region initially assigned to Protestantism. To ensure furthermore that results are not biased by the start of the Pre-Alps in the East, proximity to France in the West, or proximity to the city and lake of Geneva in the South West, we additionally implement a Spatial Regression Discontinuity Design (RDD) in which we focus on those municipalities located near the historical religious border.

To measure political preferences, we use Swiss federal referenda on specific proposals for redistribution and state intervention. The collective choice of redistributive schemes in Switzerland can be described by an extension of the standard Meltzer and Richard [1981] political economy model of redistribution. In the original model, each individual votes for more redistribution if this would give her a monetary net gain and otherwise votes against it. In the extension, as set forth for instance in Persson and Tabellini [2002], the voter's choice may by contrast additionally depend on a political preference parameter. Although our analysis does not depend on this or any other model, our investigation can be regarded

[3] "The totality of these relations of production constitutes the economic structure of society, the real foundation, on which arises a legal and political superstructure and to which correspond definite forms of social consciousness. The mode of production of material life conditions the general process of social, political and intellectual life. It is not the consciousness of men that determines their existence, but their social existence that determines their consciousness.", Preface to "A Contribution to the Critique of Political Economy", 1859.

as tracing out a possible source of such a political preference parameter. Thinking of our results in terms of this type of model is illuminating in particular because the political institutions of Switzerland, with their many elements of direct democracy, come unusually close to the social choice mechanism of "Pure Majority Voting" frequently assumed in the political economy literature. Beyond these measures of preferences, we also investigate the causal effect of Protestantism on two measures of the inequality of (pre-tax) incomes.

We find that Protestantism tends to foster weaker preferences for state intervention and redistribution than Catholicism: Depending on the measure, our instrumental variable estimates predict support for redistribution to lie up to 10 percentage points lower in a fully Protestant municipality than in a fully Catholic one, and support for government intervention up to 25 percentage points lower. Given that the aggregate share of yes votes in many referenda is frequently close to the 50% threshold, such differences can clearly be pivotal for which policies a region or, when aggregated at the federal level, a country ends up with. This relevance is underlined by the the finding that Protestant municipalities do end up exhibiting significantly greater degrees of income inequality. These findings are remarkable because we investigate differences between two variants of the same larger religion, and within the same country: Arguably the effects of more different religions and in regions with separate national governments can be assumed to be even larger than those found in our setup. Our findings therefore provide strong evidence that culture and historical events matter for political preferences and the resulting policies and economic outcomes.

The remainder of this paper is structured as follows. Section 2.2 discusses key prior works on respectively culture and political preferences in economics. Section 2.3 then looks at previous work in the other social sciences to derive some hypotheses of how and why different religions should be expected to have nurtured different political preferences. Subsequently, Section 2.4 introduces our data and Section 2.5 our empirical strategy. Section 2.6 presents our results and Section 2.7 concludes.

2.2 Previous Literature

A review of much recent work on culture and economics is provided in Guiso et al. [2006], who also make two useful methodological suggestions. The first is to conceive of culture as "those customary *beliefs and values* that ethnic, religious, and social *groups transmit fairly unchanged* from generation to generation" (italics added). This definition, with a view to reducing the scope for confounding factors, implies a focus on factors with much persistence over time, such as in Europe religion. For our question, the use of this definition demands to

exclude for instance the calls for government intervention that evolved rather spontaneously in response to the recent financial crisis. To the same end, they proposed that, rather than looking only at reduced-form regressions of economic or political outcomes on measures of culture, one explore each step in the hypothesized causal channel in turn, a suggestion we follow also in the present article when we go from the historic imposition of Protestantism through a measure of the present-day influence of "Protestant" culture and present-day political preferences to present-day policies and inequality. The value of thus seeking to trace out a channel step by step becomes apparent also from Nunn [2009]'s comprehensive survey of papers on the importance of historic events for economic development. As both of these reviews make clear, most work on both culture and history to this day has focused on the outcome of economic prosperity rather than on how such prosperity is then being distributed [4], in particular on the empirical side.

An interesting exception to this is Alesina and Glaeser [2004], who show that differences between the US and the average European country in the size of welfare states can be related to different beliefs about prospects of upward mobility, results consistent with the notion of a mutual reinforcement of beliefs and actual policies, as illustrated in a number of theoretical papers such as Benabou and Tirole [2006] and Alesina and Angeletos [2005]. This notion can help to explain why differences in types of welfare state are so persistent, although it remains silent on what put countries into those different equilibria in the first place.

A comprehensive range of possible determinants of Preferences for Redistribution (henceforth PfR), from standard economic variables like income to cultural factors like religion, is explored in Alesina and Giuliano [2009], using survey data from America's General Social Survey (GSS). A closer focus on culture as a determinant of PfR furthermore can be found in Luttmer and Singhal [2011], who establish causality by what Fernandez [2007] has termed the "Epidemiological Approach": By analyzing the attitudes of immigrants within Europe from different countries of origin who now live in the same country, they manage to obtain variation in culture while keeping fixed formal institutions and the economy. The fact that they find attitudes to remain correlated with those in the country of origin into the second generation after immigration makes it unlikely that this is merely the result of slow updating of beliefs about the functioning of markets and formal political institutions. Methodologically closest to our paper is Alesina and Fuchs-Schuendeln [2007], who exploit the natural experiment provided by German separation and reunification to show the effect on prefer-

[4] The distribution of economic payoffs does of course feed back into economic growth, by providing incentives for differentially growth-conducive activities.

ences for government intervention and redistribution of having lived under the Communist regime of East Germany.

Finally, there are two papers related to ours methodologically, albeit investigating different outcomes. Becker and Woessmann [2009], by instrumenting the spread of Lutheran-Protestant thought with the distance from Luther's city of residence, Wittenberg, show how this thought has affected economic prosperity in 19th century Prussia by promoting higher levels of education. Bruegger et al. [2009], by exploiting a discontinuity in work ethic at the language frontier, show that in Switzerland regional differences in unemployment can at least partly be explained by differences in work norms correlated with the country's different languages.

2.3 Theory: Religion and Political Preferences

Starting from a standard political economy model of redistribution or state intervention, one might ask: Why should religion matter for which policy individuals vote for? Researchers in social sciences other than economics on the other hand have long seen reasons for why it should, but have typically fallen short of showing empirically that there is indeed a causal effect from religion to preferences, rather than merely a correlation. Before we attempt to tackle this challenge empirically, it is worthwhile to take a look at that literature in order to obtain a first intuitive understanding of the relationship to be investigated.

A useful starting point here is Max Weber's famous "The Protestant ethic and the Spirit of Capitalism". While the main focus of this classic is to explain how Ascetic Protestantism's focus on hard work and saving has facilitated the rise of capitalism and the achievement of economic prosperity (on which see also Basten and Betz [2011a]), it also bears important, hitherto largely unexplored implications for political choices. Thus Weber characterizes Ascetic Protestantism as follows:

> "Labour came to be considered in itself the end of life, ordained as such by God. *St. Paul's 'He who will not work shall not eat' holds unconditionally for everyone.* Unwillingness to work is symptomatic of the lack of grace" (Chapter 5, italics added)

This is then contrasted with the mediaeval Christian doctrine, which he considers to have remained dominant within Roman Catholicism, and which he describes as follows:

> "Here the difference from the mediaeval view-point becomes quite evident. Thomas Aquinas also gave an interpretation of that statement of St. Paul. But for him *labour* is only necessary natural reason for the maintenance of individual and community... Moreover, it *holds only for the race, not for every individual*" (Chapter 5, italics added)

The differences thus characterized can be seen as the first hypothesis on how different branches of the Christian religion have advocated different approaches to dealing with unemployment or other issues of social security policy.

A more recent literature on the relationship between different religions and social policy is that started by Esping-Andersen [1990]'s comparative analysis of welfare states, in which he contrasts the "Liberal" (minimum) type of welfare state, said to be characteristic of the Anglo-Saxon countries, with two larger types, the "Social-Democratic" or "Universal" one typical of the Scandinavian countries and the "Conservative" type found in much of Continental Europe. Building on this classification, Manow [2002] then links the origins of these three stylized types explicitly to the influence of respectively Reformed Protestant, Lutheran Protestant and Catholic social teaching, where the first and third are relevant in our Swiss setting. In particular, he describes the impact of Reformed Protestantism on the development of the welfare state as follows:

> "If we describe reformed Protestantism's contribution to western welfare state development as 'negative' this refers first of all to the strongly anti-statist position of the protestant free churches and other reformed currents of Protestantism (Dissenters, Calvinists, Baptists etc.). Their emphasis on self help, autonomy of the holy local congregation, strict state/ church separation (with a church that was conceived to be de-centralized, local, democratic, congregationalist instead of episcopal in character), of individual asceticism and prudentia transported a strict anti-state program (often reinforced by traumatic experiences of religious persecution) that - in multiple ways – had a retarding effect on welfare state development."

He then points out how this has come to influence politics in countries such as Switzerland, the Netherlands, Britain and later the US. Specifically about Switzerland he writes:

> "Compare this to the Swiss case: ... a crucial element in this was the religiously influenced opinion that voluntary collective self-help should have the

> right of way before any state help sets in (Luebbert 1991: 49). Moreover, in Switzerland the reformed Conservative Party, later also the Calvinist Evangelische Volkspartei, at several points of time organized successful referenda against central government's social legislation... Not Adam Smith or John St. Mill, but Calvin and Zwingli were the central intellectual reference points in 'liberal' Switzerland."

In these considerations, Manow follows Weber in emphasizing the importance of distinguishing between the different variants of Protestantism ("if we want to investigate the role of religion in welfare state development, it is critical to differentiate between Lutheran and reformed currents of Protestantism") and points out that the anti-state stance just described is characteristic of Reformed but not of Lutheran Protestantism. This implies that Switzerland is indeed the ideal testing ground for the hypotheses of Weber and Manow, because here we have within the same country a historical coexistence between Catholicism and Reformed Protestantism.

That said, one may wonder whether the impact of different religions is really still present today, given the significant waves of secularisation that most European countries have undergone in recent decades. Geser [2008] however hypothesizes that "collectivisation on the basis of religion provides an ideal ground for the sedimentation of traditional mentalities ... which remain in place even after the disappearance of manifest religiosity. There is much to suggest that while the work ethic and thriftiness found in [the South-West German province of] Wuerttemberg has Pietist roots, it does no more require religious support for its persistence today"[5].[6]

2.4 Data

2.4.1 Swiss Referenda as a Measure of Preferences

To measure political preferences, we use the fraction of citizens in each municipality voting for more (or against less) redistribution, and for more (or against less) government

[5]The translation is ours. In the German original he writes: "Vergemeinschaftungen auf religioeskonfessioneller Basis bieten eine ideale Grundlage fuer die Sedimentierung traditionell verfestigter Mentalitaeten und Habitualisierungen, die selbst beim Verschwinden manifester Religiositaet noch lange wirksam bleiben. So spricht z.B. vieles dafuer, dass die in Wuerttemberg noch heute vorfindliche Arbeitsethik und Sparmoral zwar pietistische Urspruenge hat, heute aber keiner religioesen Stuetze mehr bedarf". For more on this, see also Geser [2004]

[6]For further sociological work on the political impact of different religions, see in particular Greeley [1989], as well as the works cited in Manow. Specifically on Catholic Social Theory, see the papal encyclicals Rerum Novarum (1939) and Quadrogesimo Anno (1979), as well as Nell-Breuning [1980].

intervention in the economy in nine different Swiss referenda. Here we explain first the general advantages of using Swiss referenda as a measure of preferences, and then discuss the content of the nine referenda used here.

The Swiss system of direct democracy, with its many referenda, is one of the rare cases that come close to the system of "Pure Majority Rule" often assumed in political economy models (for an example, see Persson and Tabellini [2002]): Firstly, rather than only electing representatives who then choose policies, citizens vote directly on specific policy proposals and every citizen has one vote. Secondly, in the referenda discussed below citizens' choice set comprised only two alternatives, for or against, thus excluding the possibility of strategic voting. Since furthermore Swiss voters participate in referenda rather frequently[7] and each referendum is preceded by a thorough discussion in the national and local media of the issues at stake, voters can be considered to be truly voting on the issue under consideration, rather than using referenda for instance to express their general level of (dis-) satisfaction with the present government, as often happens in referenda held in other countries. And thirdly, the setup can validly be considered as one with an "Open Agenda", given that in Switzerland many issues must by constitution automatically be submitted for referendum, and many other issues can and frequently are demanded by citizens to be submitted for referendum. As a result, the referenda provide a measure of preferences in the spirit of the Paradigm of Revealed Preferences, a measure that may be considered more meaningful than those that can be obtained from mere survey data. Previous papers that used them for that purpose include Bruegger et al. [2009] and Basten and Betz [2011a].

The Swiss Federal Office of Statistics[8] provides data on voting at the municipality level for all referenda held from 1980 onwards, and an overview of these referenda is available in the "Political Atlas of Switzerland" (2004). Of these, we have selected all referenda that proposed unambiguously either more or less redistribution or government intervention, and in which these issues were not mixed with other questions. This left us with five referenda on redistribution (henceforth R1-5) and four on government intervention (henceforth R6-9). Table 2.1 lists all 9 referenda together with the question voted on and the year in which they were held.

R1, held in 1992, asked citizens whether to lower health insurance contributions for the poor, and can thus be interpreted as a measure of solidarity on the basis of income.

[7] Between 1970 and 2003, Switzerland held 292 referenda, on average 8.48 per annum, which amounted to 40% of all referenda held worldwide over this period; See Kaufman and Waters [2003], Appendix A.

[8] www.bfs.admin.ch

R2 of 1997 proposed cuts in unemployment benefits and thus dealt with redistribution on the basis of employment status. R3 of 1995 put to a vote the expansion of an obligatory and universal insurance for the old, widows and handicapped ; it can thus be interpreted as dealing with redistribution on the basis of age, widow and health status. R4 of 2007 furthermore proposed to solve funding shortages in the disability benefit scheme by cutting benefits for the disabled, as opposed to raising the required level of contributions. Finally, R5 dealt with the introduction of an obligatory and progressively financed health care system.

R6 of 1981 suggested that firms were inherently tempted to abuse the market at the cost of consumers, implying a need for the government to intervene. R7 of 1986 focused more specifically on the market for housing, asking the government to prevent the charging of excessive rents. R8 of 1998 suggested that the market for agricultural products could, if left to itself, neither provide sufficient quality nor fair prices to consumers, and demanded that the government step in and pay subsidies for ecological farming. Finally, R9 of 2003 proposed to regulate more strongly how and which rental prices could be charged.

Given the selection of municipalities explained in Section 2.5 below, we have for all referenda held between 1982 and 2005 a total of 330 municipalities assigned to Protestantism and 155 not assigned, and which hence remained Catholic.. Referendum R6, held in 1981, had 2 observations less since the municipalities of Kleinboesingen and Villarbeney became independent only in 1982, and referenda R4 and R5, both held in 2007, had 21 observations less, because some municipalities lost their independence in 2005-6.[9]

2.4.2 Other Variables

As a proxy for the present-day influence of Protestantism, we use each municipality's share of Protestants, as recorded every ten years by the Swiss census. Since our referenda go back to 1981, we take our measure of Protestantism from 1980, the latest census to predate all referenda used. It is worth noting that today each municipality contains of course not only Catholics and Protestants, but also a few individuals without religious affiliation (on average between 1 and 3% of a municipality's inhabitants, see Table 2.2), as well as a few individuals with other religious affiliations. Since we have only one instrument, whether or not a municipality was forced to convert to Protestantism in the 16th century, we need to add those groups to either the Catholic or the Protestant group. The results displayed are

[9] These are Agriswil, Arrissoules, Autavaux, Bollion, Chapelle (Broye), Cordast, Delley, Donatyre, Esmonts, Forel (FR), La Tour-de-Trême, Lussy (FR), Montbrelloz, Portalban, Praratoud, Rossens (VD), Seiry, Sédeilles, Vesin, Villarbeney, Villarimboud.

based on the former strategy, but due to the still limited quantitative importance of such other groups our results are robust to which choice we make here. The census also gives us a number of additional covariates. To avoid controlling for a variable that could have been one channel for the effect of Protestantism, our main specifications control only for those covariates that have plausibly been predetermined before the Reformation, namely altitude in 1000m above sea level as a measure of geography, and historic language.[10] We have also repeated the same regressions controlling for additional covariates such as a municipality's age structure, educational composition or working sector composition, and the results thus obtained yielded qualitatively the same coefficients. Furthermore, we have computed driving distances from each municipality to the traditional religious border in km, using ArcGIS and maps provided by the Swiss Federal Office for Topography; the resulting measure constitutes the running variable for the Spatial RDD part of our analyses.

To formally test equality at the baseline, we have looked for pre-experiment data on economic prosperity. No data on GDP per capita are generally available at a disaggregated level for the period before 1500, but a proxy commonly used in research on economic history is population size: Thus Bairoch [1991] (Chapter 1) and Vries [1984] (p.164) argue that only areas with high agricultural productivity and a developed transportation network could support larger populations. Furthermore, Acemoglu et al. [2002] present evidence that there is a close association between urbanization and income per capita before as well as after industrialization. Implementing this measure at a level as disaggregated as the municipalities in our region of interest however is often not feasible, because frequently used datasets on population figures, such as Bairoch et al. [1988], cover only a few larger places, in our region Lausanne, Vevey and the city of Fribourg. Fortunately though Ammann [1937] does contain more disaggregated data, based on a count of the number of fire places conducted in the course of Papal visitations in the 15th century.

After looking at individuals' preferences as evidenced by their voting behaviour in referenda, we are interested in investigating whether the different preferences result also in different policies or economic outcomes. For the former, we look at the marginal income tax burden imposed on citizens by cantons and municipalities (on top of the common tax burden imposed by the federal government). While the Federal Office of Statistics provides public data on these only for the largest 20% of municipalities, data for all municipalities were available from Schuler et al. [2007]. The fact that cantons and municipalities have some autonomy

[10] Most of our region is homogeneously French-speaking. In our main specification we control for the predominantly German language in those few municipalities affected, but dropping those municipalities entirely gives essentially the same results.

in choosing their top tax rates does in principle make these an interesting measure of policy, in particular because an informational brochure for Swiss citizens, Steuerkonferenz [2002], explains the local tax autonomy explicitly with the great cultural heterogeneity within the country.[11] Nonetheless our data on top tax rates do not allow for statistically precise results, because most of the variation in therein happens at the canton rather than the municipality level, in that each canton chooses a "basic tax rate" for different income levels, and then each municipality in the canton chooses a multiplier ("Steuerfuss") thereon. Therefore we essentially have only 2 observations here. Nonetheless, we provide as additional information summaries of the top tax rates in Table 2.2, but caution that it is not possible to compute a standard T statistic to test whether the difference is statistically significant.[12]

The same problem does not exist for our measures on the inequality of a municipality's income distribution, which we have been able to obtain from Ecoplan [2004] and which are based on tax statistics from 1995/6. The first is the Gini coefficient as a standard measure of inequality: Its theoretical boundaries are 0 and 1, with 0 expressing a fully equitable distribution in which everyone is earning the same amount, and 1 expressing a fully unequal distribution in which one person is earning everything. As a second measure, we use the difference between mean and median income, rescaled to be bounded between 0 and 1. It is important to emphasize that both measures are based on pre-tax inequality and hence not directly affected by progressive income taxes. It will however likely depend on other egalitarian policies such as equal opportunities for education. In addition, one may hypothesize that it depends on the extent to which citizens work hard and hence translate the unequal initial opportunities given to them by nature and by the rules and norms of society into different levels of income. The latter channel would be more in the spirit of Max Weber's Protestant Ethic, but would also lead to the prediction of greater pre-tax inequality in Protestant municipalities.

To test the robustness of our results and especially to test the external validity of our results for all of Switzerland, we have also used a set of further control variables, all of which were available directly from Federal Office of Statistics. These are for each municipality the area in km squared, the population in 1000, the population share aged below 20, the population share aged above 60, the share of foreign residents, the unemployment rate, the average

[11] "A reduced autonomy of the members of the Federation [i.e. cantons and municipalities] however would imply a strengthening of central power, and one must ask whether a Switzerland governed centrally would even be able to survive, for after all its *raison d'être is not based on a common and homogeneous ethnicity, language or territory, but only on common history and politics in a federation.*" (p. 23, the translation into English being ours).

[12] The summary provided is for the case of a married couple without children, but the pictures emerging from the top tax rates charged for singles or married couples with children are qualitatively similar.

income before taxes, the shares of inhabitants in each of four different education categories (minimum schooling, secondary schooling, some post-secondary education, completed tertiary education), the shares with each of ten different types of socio-economic status (such as top management, self-employed, academic, etc.), and finally indicators for nine different municipality types (such as centre, suburban, etc.).

2.5 Empirical Strategy

2.5.1 The Need for Quasi-Random Variation in Religion

When investigating the causal effect of different religions, a comparison of outcomes across countries with different religions – as conducted for instance by Manow [2002] or many of the works cited therein, is likely to suffer from omitted-variable bias, as these countries will also differ on many other accounts such as geography, climate and so on. But even within the same country, there is the concern that unobserved third variables are simultaneously correlated with religion and our dependent variables. It is hence worthwhile to start with a consideration of what determined the present-day religious compositions of European and in particular Swiss regions.

A feature typical of most European countries is that the present-day religious composition of different regions depends largely on the choices made already in the 16th century, shortly after the Protestant Reformations expanded the set of religions to choose from. In most of present-day Germany these choices were made by local noblemen, in the case of much of present-day Switzerland by local councils composed of the most prominent citizens. This of course raises the question what made these local rulers or councils choose as they did. At first sight, their choices depended largely on geo-political considerations. Thus the historian Gordon [2002] writes:

> "There was nothing inevitable about what happened in Zurich in the 1520s. Opposition to the reformer remained strong and it was only through a few close relationships ... that Zwingli was able to sustain his position ... With Zwingli leading from Zurich, there was no chance of the movement making any headway in Uri, Schwyz, Unterwalden, Zug, and Lucerne. Bad memories of Zurich's earlier hegemonic aspirations quickly reappeared. That is why the principal battlegrounds were not in any of the Confederates ... but in the Mandated Territories which they administered jointly." (Gordon [2002]).

However, closer analysis reveals that the more mountainous regions were more likely to remain Catholic than the less mountainous ones. A major reason, it turns out, was the fact that Protestant reformer Zwingli forbade his followers to engage in mercenary service, on which however the more rural regions depended for their income. This of course poses a challenge for identification of a causal effect of religion on attitudes and outcomes today: If for instance survival in the mountains required a greater degree of solidarity, this alone may explain the different voting patterns found there nowadays and would lead to an upward-bias in our estimates of the effect of Protestantism on preferences for redistribution. Conversely, one may imagine that individuals there have always led relatively more solitary lives, or lives centered more around the family as opposed to larger social groups, leading to lower preferences for redistribution and hence downward-bias in our estimates.

A second concern stems from the fact that, since the Reformation was initially and during most of the territorial competition with Catholicism a German-speaking movement[13], its spread is largely restricted to the German-speaking parts of Switzerland, whereas the French- and Italian-speaking areas are predominantly Catholic. This might lead one to worry that differences in political preferences might at least partly be due to different cultural influences from respectively the German- and the French- or Italian-speaking neighbouring countries. Trying to control for these factors, and in particular for geography, is unlikely to provide a fully satisfactory solution, because the available variables like altitude above sea level are unlikely to capture all relevant features of geography. What we need therefore is a good quasi-experiment, i.e. a quasi-random assignment of different religions to two otherwise identical regions. This is indeed what we use in this paper, and explain in the following subsection.[14]

2.5.2 The Natural Experiment I: Exogenous Assignment

We focus on a region in the South-West of Switzerland that, as we shall show, was homogeneous until the beginning of the 16th century, but was then split into two parts, with the West being forced to adopt Reformed Protestantism, and the East to remain Catholic. In this section we first expound the details of the imposition, and then investigate whether the two parts were identical in all other respects.

[13] The Swiss Reformation was started by Zwingli in German-speaking Zurich, and French-speaking John Calvin became involved only after the death of Zwingli and only after the "Second Battle of Kappel" between Catholic and Protestant cantons.

[14] Further accounts of the assignment of religion in Switzerland in general can be found in Moeller [1978], Schaab [1993], and Schindling and Ziegler [1989].

Until 1513 the entire region was a subject territory of the Roman Catholic kingdom of Burgundy. Then however it was jointly conquered by the Swiss Confederacy, at the time a lose self-defence alliance of otherwise independent republics. For the next 23 years the region was then jointly ruled by these Confederates, as can be seen in the map in Figure 2.2. In 1536 however, the two most powerful members of the alliance, the city republics of Berne and Fribourg, decided to pay the others off and divide the region amongst the two of them: The Eastern part fell to neighbouring Fribourg, and the Western one to Berne.

As the latter had recently become Protestant, the new rulers – in order to facilitate governance of their new territories – imposed Zwinglian Protestantism everywhere, so that the new religious authorities would all be based in Berne rather than Rome, and could more easily be persuaded to preach citizens to obey also the worldly authorities of Berne. Of course one might think that the region would have become Protestant anyway, but the historical accounts suggest otherwise: In fact, as late as in 1534 the deliberative assembly of Vaud, meeting at Moudon, decided explicitly that they would like to remain Catholic.[15] When they were nonetheless forced to become Protestant, peasants started rioting, and the hitherto powerful local authorities lost their jobs, as Berne preferred to replace them with loyal authorities educated in the Protestant faith. As Bruening [2005] writes:

> "[The Reformers] found little sympathy for the evangelical cause in Vaud. Late medieval Catholicism was alive and well in the region, where the people did not even seem to be aware of the Reformation before 1525... [and] much less enthusiastic about its central messages."

A major reason for the ignorance of the Reformation until that point was language: Most of the Reformation documents had been published in German, and those few (Lutheran) Protestants pamphlets published in French had all been printed in either Paris or Antwerp, but none in French-speaking Switzerland. Furthermore, the Catholic duke of Savoy, while defeated by Berne, lingered constantly on the horizon, threatening to take back the territory and to restore religious obedience to Rome:

> "Catholicism was deeply ingrained among the people of Vaud, and the possibility of returning to Savoyard rule encouraged them in their resistance to the new government and religion." (Bruening [2005])[16]

[15] For details, see Feller [1953], p.379

[16] Another excellent account of Bernese rule of the Vaud, on which we have drawn significantly, is provided in Holenstein et al. [2006].

One initially homogeneously Catholic region had thus been split into one henceforth homogeneously Protestant and one homogeneously Catholic part. Three districts within those two regions were somewhat special though and need to be dealt with specifically: Three "Common Lordships", places where important battles in the Burgundy Wars had taken place, were henceforth ruled jointly by the Protestant city republic of Berne and the Catholic one of Fribourg. As a consequence, citizens of these districts were allowed to freely choose their religion. These districts –Echallens-Orbe, Grandson and Murten– are marked with yellow needles in the map in Figure 2.1. To restrict the quasi-experiment to include only municipalities which had their religions imposed by external rulers, these three districts have been dropped from the sample. Since this concerns only a few municipalities, the results obtained without dropping them are however very similar.

2.5.3 The Natural Experiment II: Equality at the Baseline

We have shown that before the new assignment of religion, the West had not been more predisposed to Protestantism than the East. For the experiment to be valid however, it is necessary that the regions were identical also in other respects that might be correlated with present-day political preferences

We start with the number of fire places per municipality before the experiment. T-tests fail to reject the Null Hypothesis that population size was on average the same across the two regions (Table 2.2), and a regression discontinuity analysis does not find evidence of a jump in the variable at the border between the two regions (Figure 2.8).

We also look at a number of qualitative factors, starting with political structures. As has been pointed out above, until the experiment both parts shared a common structure of governance, until 1513 as subject territory of Burgundy, and from 1513 through 1536 as a common subject territory of the Swiss Confederacy, as illustrated in Figure 2.2. Secondly, we look at Switzerland's other main cultural divide, language: Here both regions lie almost entirely within the French-speaking part of Switzerland, apart from the district (Swiss administrative level between municipality and canton) of Sense in the Catholic half.[17] To ensure that this does not bias our results, we control for this difference in all our regressions with a dummy for whether a municipality's traditional primary language has been German. We use this rather than today's share of inhabitants with German as first language in order

[17] As well as Murten (now part of the District du Lac), which however we have already dropped from the sample for being a "Joint Territory", see above.

to avoid picking up recent migration patterns, which must be considered as potentially endogenous to present-day outcomes. We have also conducted the same set of analyses fully excluding the two traditionally German-speaking districts from the sample, i.e. restricting our sample to French-speaking municipalities only, and doing so yields very much the same results.

As a result, our baseline specification takes the form

$$Y_m = \alpha + \beta P_m + \gamma A_m + \delta G_m + \varepsilon_m \tag{2.1}$$

where Y_m is the outcome for municipality m, P_m is its share of Protestants, and A_m and G_m are the two available covariates that can be thought to have been fixed prior to our quasi-experiment, altitude and an indicator for German language respectively. It is worth emphasizing that within the sub-sample of this region the simple least-squares estimates will already be free of the historical selection issues discussed above and so OLS estimates of the coefficient β should already get us fairly close to the true effect. One confounding factor that remains however will be selective migration in recent years. Therefore our second step is to estimate Equation 2.1 instead by Instrumental-Variables, instrumenting the present-day share of Protestants with the historical assignment of religion.

The IV estimates should get us even closer to the truth, but one potential confounder that still remains, given our arguments above and a look at the satellite view presented in Figure 2.1, is geography: By and large this is identical across the two regions, with however one exception: In the Eastern, Catholic part, the municipalities farthest away from the border are situated in more mountainous territory. This can also be seen in Figure 2.8, which plots average altitude against distance from the border. Now while we can control for altitude per se, this will only imperfectly capture geography, because some municipalities may not lie so high themselves and yet be badly connected to other towns and villages because of higher mountains surrounding them. Relatedly, one may worry that outcomes in the Western part are for some municipalities influenced, for better or worse, by their proximity to present-day France, or in the South West by their proximity to the city and canton of Geneva, even though these themselves are not in our sample. Our strategy for making sure that our final results are not biased by any of these three factors is to complement our Instrumental-Variable estimates with a Spatial Regression Discontinuity Design, which attaches greater weight to the municipalities close to the common border and which are hence statistically identical in terms of those three factors. In the following subsection we explain this strategy in a bit more detail.

2.5.4 A Spatial Regression Discontinuity Design

The Spatial Regression Discontinuity Design (RDD) is based on the idea of focusing on municipalities near the border between the assigned and the not assigned region. Then even if, as discussed above, the two regions differ in their average geography, distance to France or distance to Geneva, the municipalities near the border will be equally affected by all of these factors, so that any discontinuity in outcomes between the two groups can credibly be attributed to the different treatment assignments. This manifests itself in a discontinuity of our treatment variable, a municipality's fraction of Protestants today, as a measure of the present-day influence of Protestant teaching. To be sure, this does not jump from zero to one, because of conversion, secularization and migration, but it still jumps significantly. Hence we have what Trochim [1984] has called a "Fuzzy" (as opposed to "Sharp") Regression Discontinuity Design, where the estimate of the effect of Protestantism on our outcomes is given by the ratio of the reduced-form or intention-to-treat impact of assignment on outcomes, divided by the first-stage impact of assignment on present-day treatment intensity.

More formally, let S_i denote the driving distance in km from municipality i to the border, with distances in the Catholic region east of the border being coded as negative and those in the Protestant region west of the border being coded as positive values (so this is reverse from the maps, but in line with the OLS and IV regressions, where Protestantism is defined as treatment and Catholicism as no treatment). Then if $\overline{s}$ is the discontinuity point, here normalized to $\overline{s} = 0$, the key requirement for a RDD is that the propensity of treatment *marginally* below and that *marginally* above $\overline{s}$ are significantly different: $\Pr(I = 1|\overline{s}^+) \neq \Pr(I = 1|\overline{s}^-)$. This is shown by Figure 2.8. By contrast, the hypothetical outcomes respectively without and with treatment, Y_0 and Y_1, *conditional on distance* S must be fully smooth around the discontinuity, i.e. $E[Y_0|\overline{s}^+] = E[Y_0|\overline{s}^-]$ and $E[Y_1|\overline{s}^+] = E[Y_1|\overline{s}^-]$.Of course, we observe each municipality only either as assigned to treatment or not assigned to treatment, so this smoothness condition cannot be tested directly. However, a useful test is to check whether baseline characteristics and pre-experiment outcomes evolve smoothly around the threshold. Figure 2.8 illustrates this for altitude: It shows both how the two regions differ in their average altitude, one motivation for us to implement a Spatial Regression Discontinuity Design here, and how altitude does however evolve smoothly around the threshold, validating our setup. Similarly, Figure 2.8 shows that also the number of fire places per municipality in 1416, as a measure of economic prosperity before the experiment, evolves very smoothly around the threshold.

As the number of observations directly *on* the border is limited, one choice to be made in the context of a Regression Discontinuity Design is which bandwidth to use. Smaller bandwidths are more credible in using fewer observations far away from the border, but on the other hand larger bandwidths allow to rely on more observations and thus promise greater statistical precision. Once we use observations also from outside the immediate neighbourhood of the border, we need to flexibly control for distance from the border per se, thus changing Equation 2.1 into

$$Y_m = \alpha + \beta P_m + \gamma A_m + \delta G_m + \theta(S_m) + \varepsilon_m \tag{2.2}$$

where $\theta(S_m)$ is a control function for the distance. Even then, our choice of the bandwidth might still matter, and so we probe the robustness of that choice by implementing our estimations for all bandwidths between 5km and 50km. We find our results to be very robust to that choice, as can be seen in Tables 2.6 and 2.7. They display for respectively referenda and other outcomes the results for bandwidths 5km, 10km, 20km, 30km, 40km and 50km, which we shall discuss in greater detail in Section 2.6 below. All graphical illustrations are for a bandwidth of 10km, but as the tables show, results for different bandwidths look quite similar in the relevant respects.

2.6 Results

A first overview can be gained from the summary statistics in Table 2.2: They tell us that in 8 of the 9 referenda considered (the exception being R5, on the obligatory, progressively financed health insurance) support for the more redistributive or more interventionist policy option was between 2 and 14 percentage points higher in the traditionally Catholic than in the traditionally Protestant municipalities. In line with this, inequality of pre-tax income is higher in the Protestant region, both on the Gini (by more than 12 pooled standard errors) and on our alternative measure based on the difference between mean and median income (by more than 2 pooled standard errors), and we also find higher top tax income tax rates in the Catholic municipalities, although for the latter measure we cannot be sure about its statistical significance, due to the caveats discussed in Section 2.4 above.

These first impressions are confirmed by the OLS regressions reported in 2.3. Now, after controlling for differences in altitude and traditional language, support for the more redistributive or more interventionist policy option is lower in the Protestant municipalities on all nine measures: By between 1 and 9 percentage points for redistributive policies, and by between 3 and 27 percentage points for interventionist policies. In line with this, the Gini

coefficient is predicted to be 0.063 units (i.e. about one standard deviation) higher and the alternative inequality measure 0.003 units higher (only about 1/30 standard deviation, and not statistically significant).

As we move to the IV estimates, displayed in Table 2.4, coefficients tend to be about a fourth smaller and are not statistically significant for R3 and R5, but other than that maintain the same message. The estimated effect of Protestantism on our two measures of income inequality becomes even larger and statistically significant for both measures, with differences now amounting to respectively 0.12 units (about 2 standard deviations) and 0.027 units (about 1/3 of a standard deviation). The picture is also confirmed by the corresponding Intention-to-Treat or Reduced-Form coefficients displayed in Table 2.5, which are smaller (given that share of Protestants in a traditionally Protestant municipality is nowadays no more 100 percentage points higher, but only about 62 percentage points) but nonetheless convey qualitatively the same message.

Finally we move to the estimates obtained by Fuzzy Spatial Regression Discontinuity Design and reported in Tables 2.6 and 2.7 for respectively referenda and income inequality. Exact coefficient sizes vary somewhat across bandwidths and are in some cases slightly smaller and in others slightly larger than those obtained by Instrumental-Variables estimation, but by and large they also confirm the earlier picture whereby Protestantism has moderated preferences for redistribution and government intervention relative to Catholicism. Effects on the Gini coefficient remain striking, while the effect on our alternative measure of inequality loses its statistical and economic significance, although the sign does still remain positive.

2.6.1 Extension to all of Switzerland

As extensively argued above, analyses of the impact of religion conducted with data from all of Switzerland are problematic due to the likely endogeneity of religion in this setting. Therefore we have made the case for restricting analysis to the small region in the West of Switzerland for which these problems do not apply. At the same time however, one might wonder whether the results obtained are specific to this small region, or whether they possess external validity also beyond. To find out, we have conducted all the analyses presented above also on the full Swiss dataset.

As this is no more a valid quasi-experiment, the case can be made for using a more extensive list of controls such as average income, educational composition of the population,

and others. On the other hand, some of these variables might also be channels of the effect of Protestantism, rather than confounding factors, so that controlling for them would take out part of the very effect we are seeking to estimate. That said, we have tried different specifications, both with a minimal and with an extended set of controls. The extended set of controls included the municipality's area in km squared, population in 1000,the population share aged below 20 and that aged above 60, the share of foreign residents, the unemployment rate, the average income before taxes, 4 education categories (minimum schooling, secondary schooling, some post-secondary education, completed tertiary education), 10 types of socio-economic status (such as top management, self-employed, academic, etc.) , 9 municipality types (such as centre, suburban, etc.), and canton fixed effects. The results remained qualitatively unchanged, and so Table 2.8 presents, representatively for all, the results obtained with only the minimum set of controls used above, plus canton fixed effects. The fact that statistically significant effects of between 3 and 10 percentage points are found on all 9 referenda can be interpreted as evidence that our main results obtained and discussed above are not specific to the quasi-experimental region exploited for our Instrumental-Variable and Regression Discontinuity Design analyses above.

2.7 Conclusion

This paper has shown different religions to lead to different preferences for redistribution and state intervention in the economy. The natural experiment described above has enabled us to cleanly identify this effect as being causal. While the use of referendum data does already make for a strong argument that these differences are highly economically significant, the case becomes even stronger through the evidence that these different preferences are also associated with levels of actual income inequality. This is particularly so since we find that municipalities influenced by Reformed Protestantism have at the same time higher pre-tax inequality and a less redistributive tax system.

The results are remarkable not least because our setup is in many respects one in which differences in religion should be expected to matter less than in other parts of the world: We have compared merely two varieties of the same religion Christianity, within a very small region of the same country, and on what is arguably the world's most secularized continent. It thus seems fair to expect that in other parts of the world the impact of religion on political preferences and resulting policies and outcomes will be at least as large as what we have found within this setup.

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2.8 Appendix

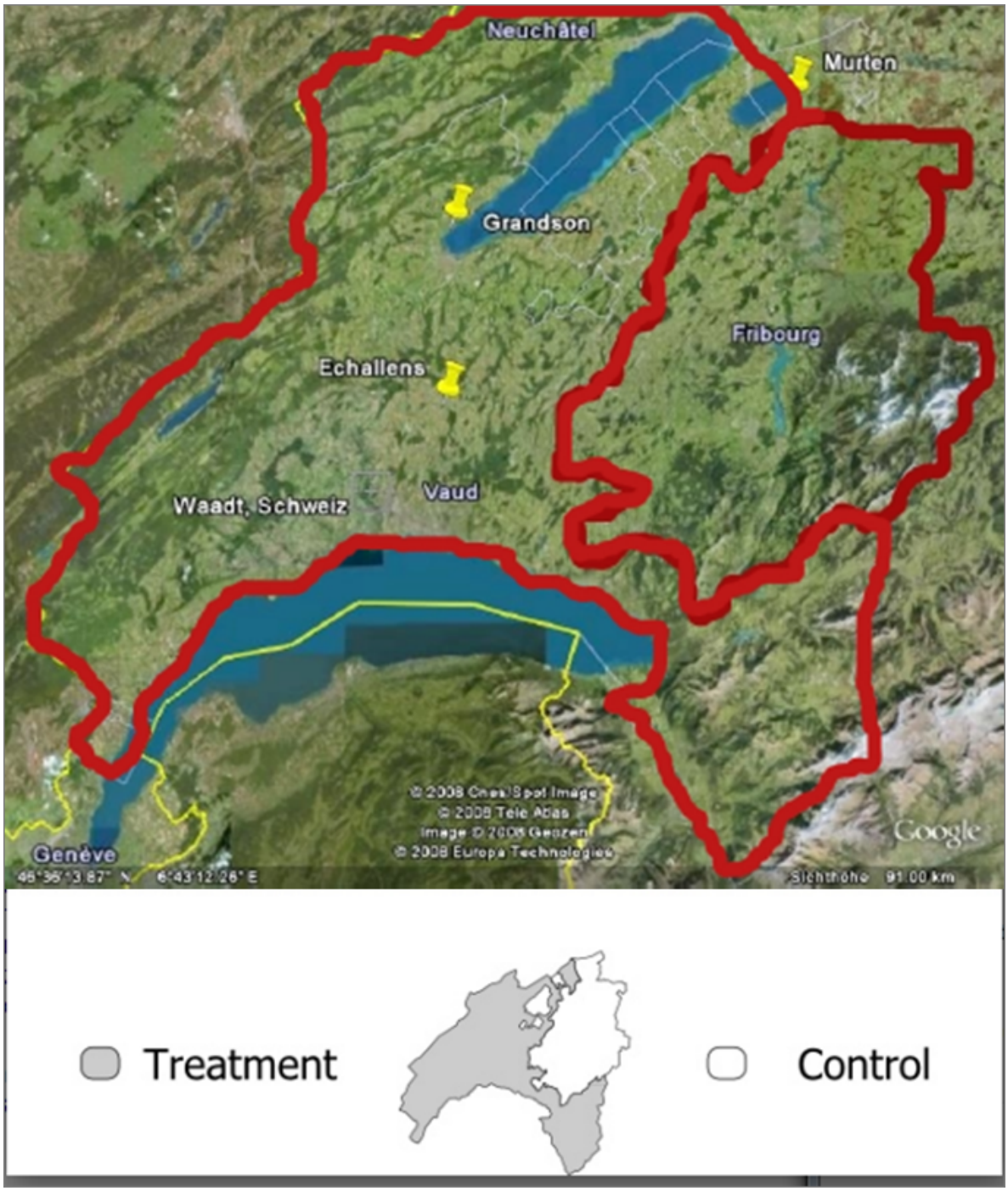


Figure 2.1: Satellite image of the region in which the quasi-experiment was set.

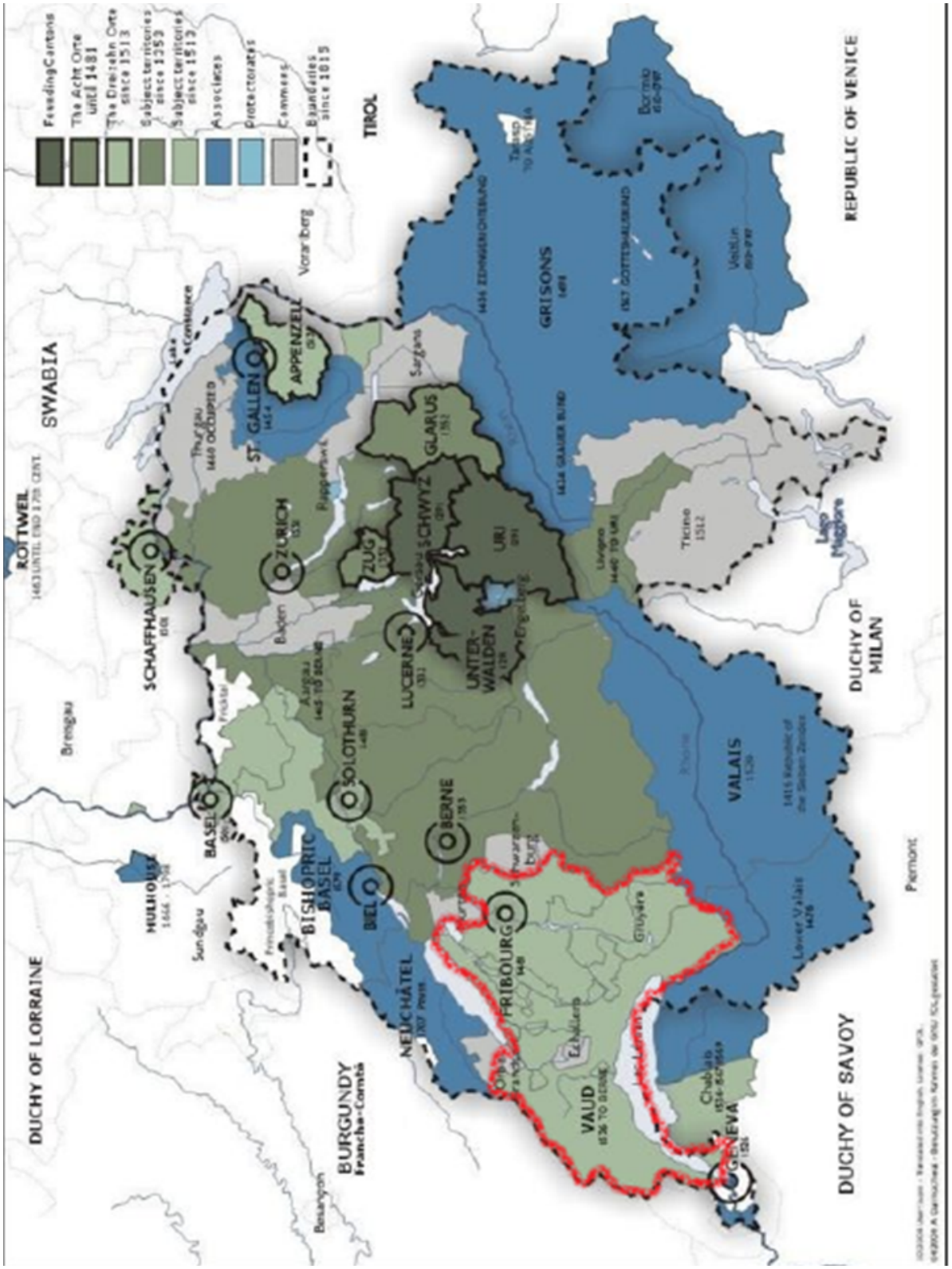
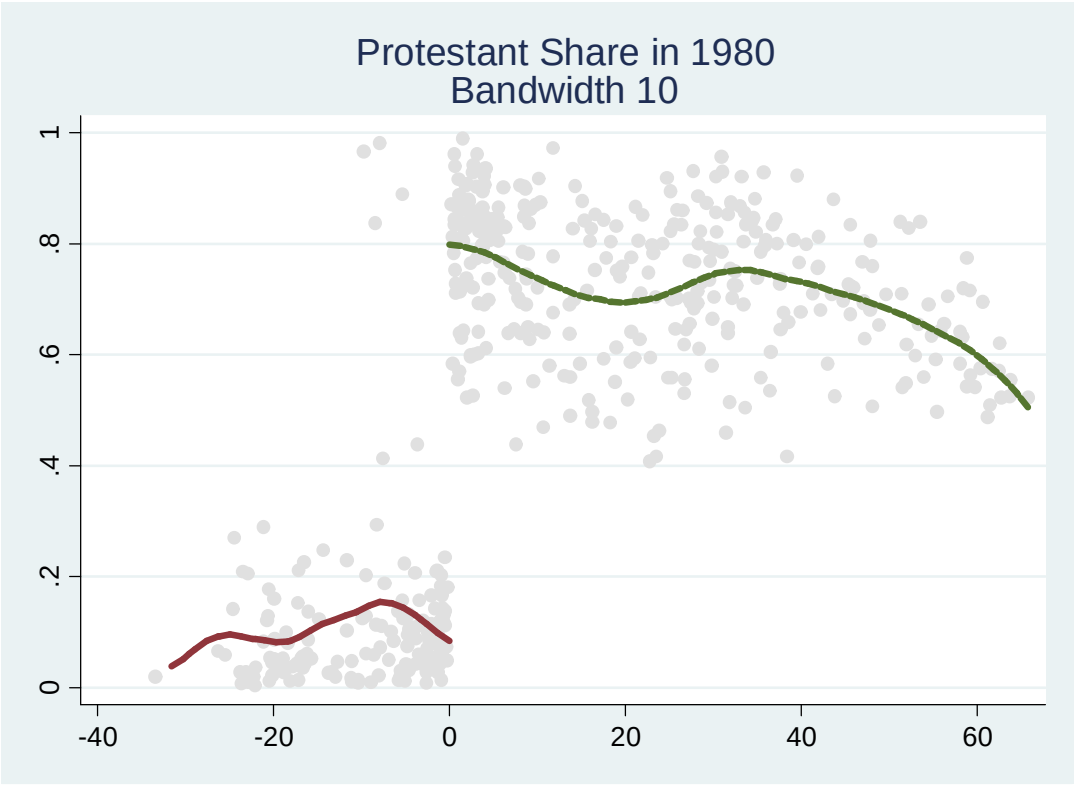
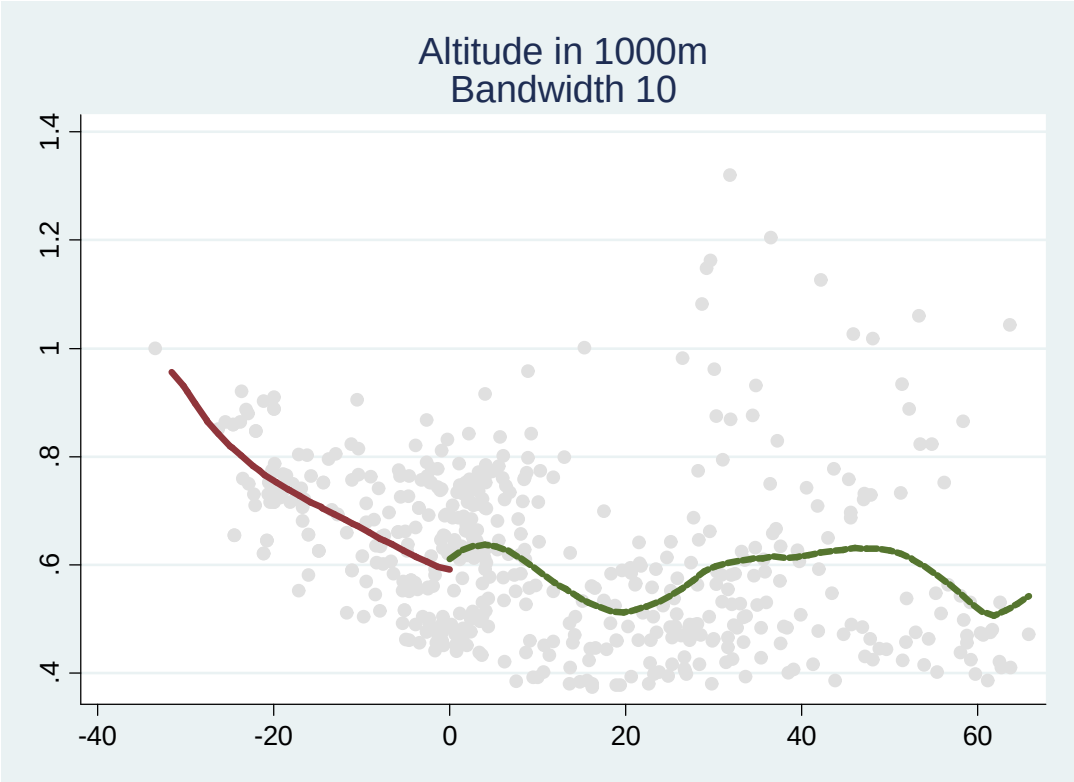


Figure 2.2: This map depicts the political setup prior to the quasi-experiment.



Graphical representation of the Regression Discontinuity Design, using as dependent variable our measure of treatment, each municipality's share of Protestants as recorded in the 1980 census. Local linear regressions with a bandwidth of 10km as specified in Equation 2.2.



Graphical representation of the Regression Discontinuity Design with the dependent variable "Altitude above sea level in 1000m". Local linear regressions with a bandwidth of 10km as specified in Equation 2.2.

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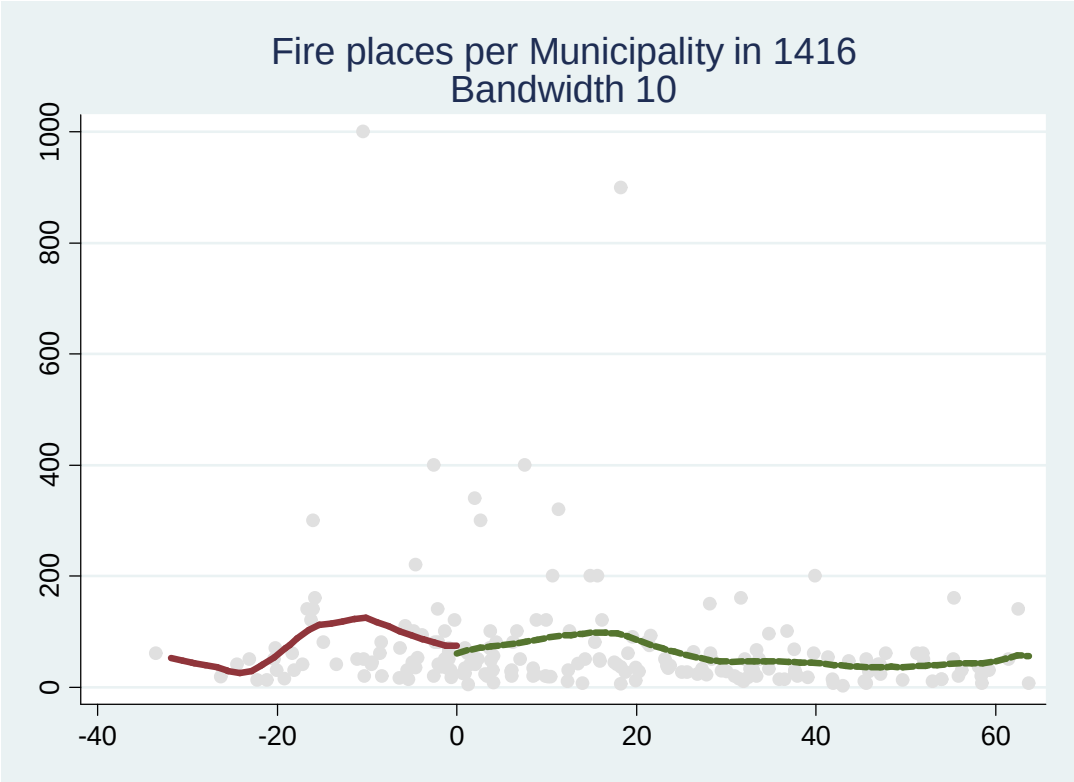


Graphical representation of the Regression Discontinuity Design with the dependent variable "Number of Fire Places in 1416". Local linear regressions with a bandwidth of 10km as specified in Equation 2.2.

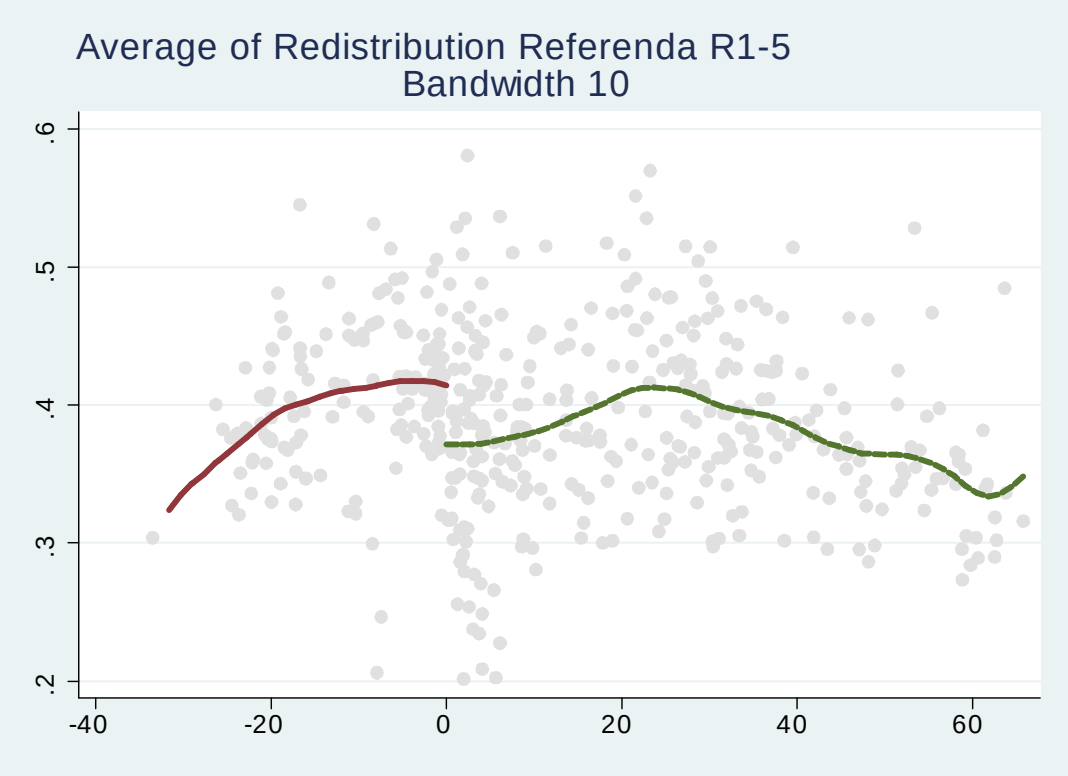


Figure 2.3: Graphical representation of the Regression Discontinuity Design with the dependent variable "Average Share of Votes in Favor of More Redistribution". Local linear regressions with a bandwidth of 10km as specified in Equation 2.2.

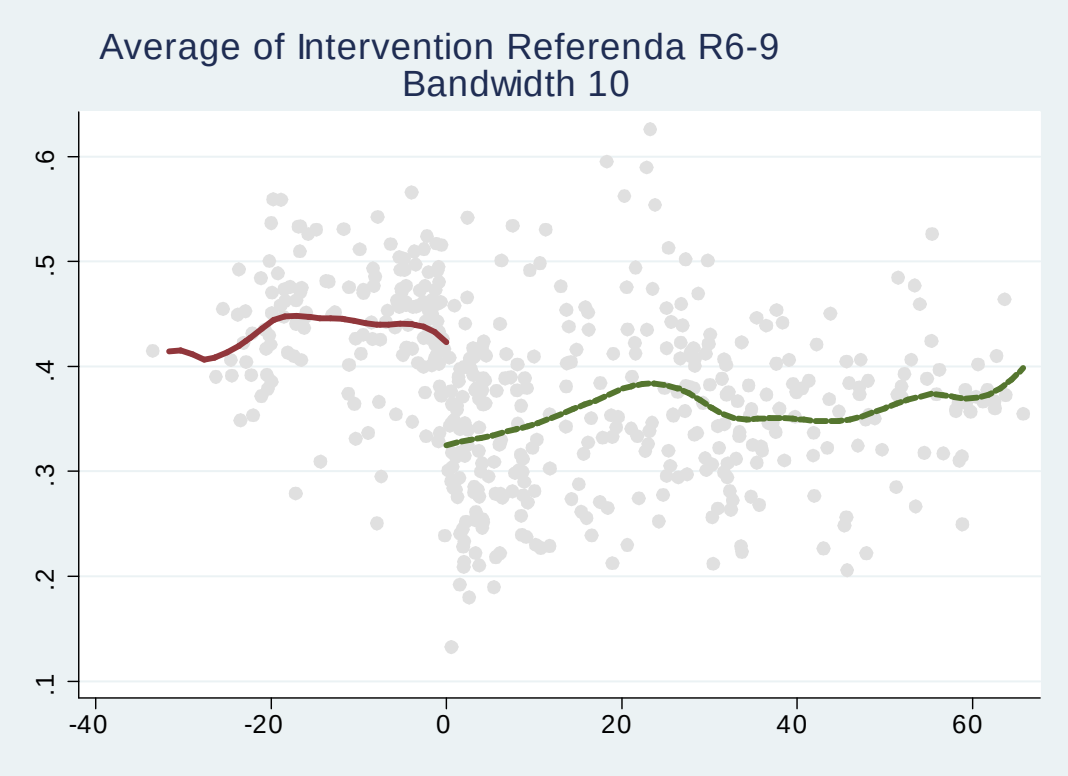


Figure 2.4: Graphical representation of the Regression Discontinuity Design with the dependent variable "Average Share of Votes in Favor of more Government Intervention". Local linear regressions with a bandwidth of 10km as specified in Equation 2.2.

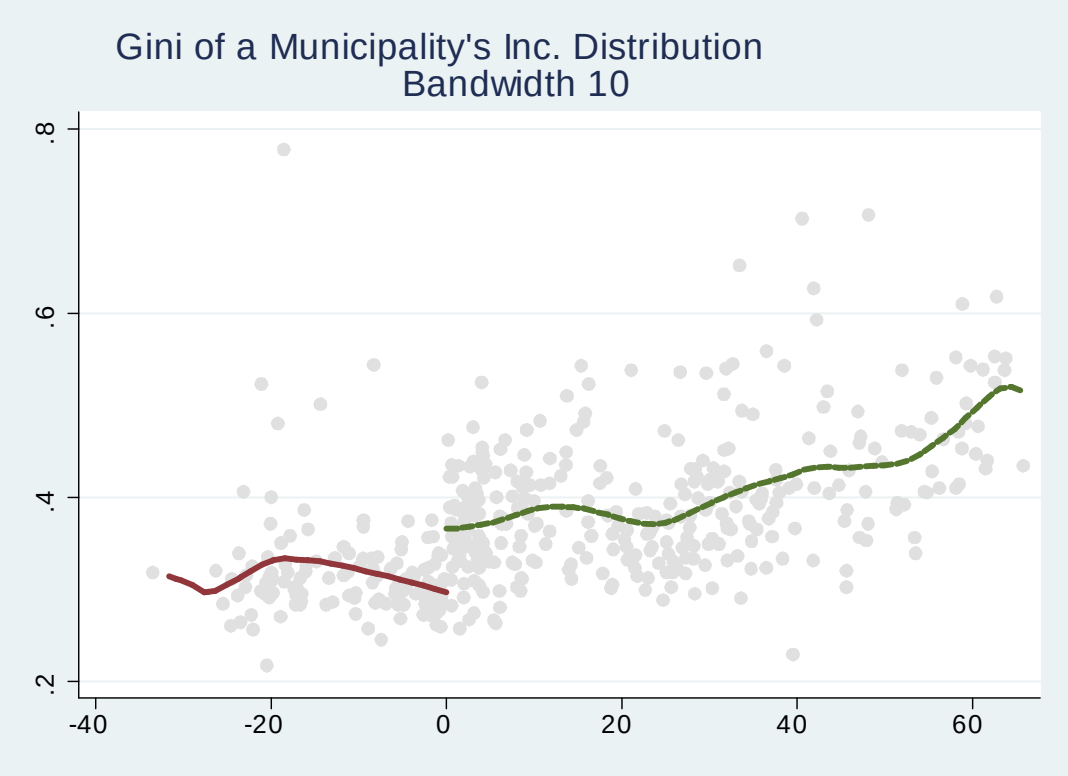


Figure 2.5: Graphical representation of the Regression Discontinuity Design with the dependent variable "Gini Coefficient of Income Inequality within each Municipality as of 1995". Local linear regressions with a bandwidth of 10km as specified in Equation 2.2.

Table 2.1: Overview over the Referenda Used

	Referendum	Year
R1	Allow Poor to pay lower health insurance contributions	1992
R2	(Reject) 3% Cut of daily Unemployment Allowance	1997
R3	Make oldage and disability insurance obligatory and universal	1995
R4	(Reject) Cut of Benefits for the Disabled	2007
R5	Progressively financed universal care insurance	2007
R6	Ask government to prevent firms from abusing market	1981
R7	Ask government to protect tenants from excessive rent prices	1986
R8	Farm subsidies to further ecological farming and lower prices	1998
R9	Regulate more strongly how and which rent rates can be charged	2003

Table 2.2: Summary Statistics for the 2 parts of our Quasi-Experimental Region

	Protestant			Catholic			Comp.
	N	Mean	SD	N	Mean	SD	T
Assigned Protestant	330	1.00	0.00	155	0.00	0.00	
Prot. Share (1980)	330	0.73	0.13	155	0.11	0.15	43.79
Cath. Share (1980)	330	0.20	0.11	155	0.86	0.15	-47.81
No Rel. Share (1980)	330	0.04	0.03	155	0.01	0.01	15.14
Altitude	330	0.59	0.17	155	0.67	0.13	-5.76
German	330	0.00	0.00	155	0.26	0.36	-9.10
Fire Places in 1600	142	62.99	95.45	72	76.04	127.24	-0.77
Population	330	1,848	7,455	155	1,071	1,541	1.81
Pop. Density	330	0.33	0.77	155	0.16	0.19	3.71
Avg. Age	330	38.29	2.91	155	36.74	2.26	6.39
Avg. Inc. (2000)	330	24,763	5,409	155	21,010	4,270	8.26
Unemp. Rate (2000)	330	0.03	0.02	155	0.02	0.01	5.92
Share non-college HE	330	0.15	0.04	155	0.12	0.03	7.72
Share w/ college degree	330	0.10	0.06	155	0.05	0.03	10.15
R1	330	0.23	0.09	155	0.29	0.08	-7.09
R2	330	0.59	0.10	155	0.61	0.10	-2.53
R3	330	0.25	0.08	155	0.27	0.08	-2.00
R4	326	0.44	0.09	138	0.50	0.09	-6.88
R5	326	0.41	0.09	138	0.38	0.09	3.54
R6	330	0.48	0.13	153	0.62	0.13	-11.19
R7	330	0.51	0.12	155	0.65	0.09	-14.80
R8	330	0.10	0.05	155	0.13	0.05	-5.74
R9	330	0.32	0.10	155	0.36	0.08	-3.75
Top Tax Rate	330	16.5900	1.0500	155	18.8200	1.2600	n/a
Gini Coeff. Of Incomes	329	0.40	0.07	138	0.32	0.06	12.03
Ineq.	329	0.09	0.08	138	0.07	0.09	2.09

"Ineq." computed as the difference between mean and median income, scaled to lay between 0 and 1.

Table 2.3: OLS

	Protestant	Altitude	German	Cons	R2	N
R1	-.082	-.048	.078	.315	.168	485
	(.000)***	(.105)	(.000)***	(.000)***		
R2	-.122	-.068	-.096	.712	.134	485
	(.000)***	(.017)**	(.000)***	(.000)***		
R3	-.031	.031	.019	.253	.031	485
	(.014)**	(.119)	(.169)	(.000)***		
R4	-.091	.059	-.018	.469	.108	464
	(.000)***	(.013)**	(.311)	(.000)***		
R5	-.011	.085	-.123	.362	.111	464
	(.405)	(.000)***	(.000)***	(.000)***		
R6	-.265	-.090	.021	.721	.337	483
	(.000)***	(.008)***	(.395)	(.000)***		
R7	-.256	-.085	-.009	.743	.355	485
	(.000)***	(.006)***	(.674)	(.000)***		
R8	-.033	-.023	.057	.137	.152	485
	(.000)***	(.113)	(.000)***	(.000)***		
R9	-.105	-.000	-.081	.398	.105	485
	(.000)***	(.989)	(.000)***	(.000)***		
Gini	.063	-.026	-.044	.358	.122	467
	(.000)***	(.371)	(.000)***	(.000)***		
Ineq.	.003	-.097	.007	.140	.031	467
	(.708)	(.000)***	(.635)	(.000)***		

Share of Protestants from 1980 census; referenda R1-9 as explained in T. 2.1; "German" variable, Gini and Inequality as explained in the text, Altitude in 1000m above sea level

P values in parentheses based on robust SEs; *0.10 ** 0.05 *** 0.01

Table 2.4: IV

	Protestant	Altitude	German	Cons	R2	N
R1	-.061	-.045	.090	.301	.164	485
	(.000)***	(.126)	(.000)***	(.000)***		
R2	-.078	-.061	-.071	.681	.117	485
	(.000)***	(.030)**	(.000)***	(.000)***		
R3	-.006	.035	.032	.236	.023	485
	(.617)	(.078)*	(.015)**	(.000)***		
R4	-.101	.058	-.024	.476	.107	464
	(.000)***	(.015)**	(.158)	(.000)***		
R5	.009	.088	-.111	.348	.106	464
	(.466)	(.000)***	(.000)***	(.000)***		
R6	-.222	-.084	.045	.691	.330	483
	(.000)***	(.013)**	(.059)*	(.000)***		
R7	-.253	-.085	-.007	.741	.355	485
	(.000)***	(.006)***	(.732)	(.000)***		
R8	-.023	-.021	.062	.130	.148	485
	(.009)***	(.139)	(.000)***	(.000)***		
R9	-.077	.004	-.066	.379	.098	485
	(.000)***	(.873)	(.000)***	(.000)***		
Gini	.120	-.017	-.011	.319	.082	467
	(.000)***	(.548)	(.302)	(.000)***		
Ineq.	.027	-.093	.021	.123	.025	467
	(.003)***	(.000)***	(.121)	(.000)***		

Share of Protestants from 1980 census; referenda R1-9 as explained in T. 2.1; "German" variable, Gini and Inequality as explained in the text, Altitude in 1000m above sea level

P values in parentheses based on robust SEs; *0.10 ** 0.05 *** 0.01

Table 2.5: ITT

	Protestant	Altitude	German	Cons	R2	N
R1	-.039	-.051	.086	.300	.130	485
	(.000)***	(.097)*	(.000)***	(.000)***		
R2	-.050	-.069	-.076	.679	.050	485
	(.000)***	(.024)**	(.000)***	(.000)***		
R3	-.004	.035	.032	.236	.019	485
	(.619)	(.092)*	(.020)**	(.000)***		
R4	-.066	.046	-.033	.476	.105	464
	(.000)***	(.052)*	(.060)*	(.000)***		
R5	.006	.089	-.111	.348	.110	464
	(.468)	(.000)***	(.000)***	(.000)***		
R6	-.142	-.107	.032	.685	.216	483
	(.000)***	(.006)***	(.199)	(.000)***		
R7	-.162	-.110	-.022	.733	.283	485
	(.000)***	(.002)***	(.224)	(.000)***		
R8	-.015	-.023	.061	.130	.130	485
	(.009)***	(.111)	(.000)***	(.000)***		
R9	-.049	-.004	-.070	.377	.046	485
	(.000)***	(.883)	(.000)***	(.000)***		
Gini	.079	-.003	-.001	.319	.210	467
	(.000)***	(.907)	(.922)	(.000)***		
Ineq.	.018	-.090	.023	.123	.037	467
	(.003)***	(.000)***	(.081)*	(.000)***		

Share of Protestants from 1980 census; referenda R1-9 as explained in T. 2.1; "German" variable,
Gini, Inequality and Tax Progressiveness measures as explained in the text, Altitude in 1000m.above sea level
P values in parentheses based on robust SEs; *0.10 ** 0.05 *** 0.01

Table 2.6: Fuzzy RD, Referenda

		5	10	20	30	40	50
R1	ITT	-0.073	-0.071	-0.078	-0.082	-0.078	-0.074
		(0.027)**	(0.002)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
	Wald	-0.107	-0.100	-0.112	-0.121	-0.117	-0.113
		(0.022)**	(0.001)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
R2	ITT	-0.087	-0.092	-0.097	-0.100	-0.097	-0.094
		(0.027)**	(0.004)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
	Wald	-0.129	-0.129	-0.139	-0.147	-0.147	-0.144
		(0.023)**	(0.003)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
R3	ITT	-0.038	-0.022	-0.026	-0.031	-0.029	-0.027
		(0.156)	(0.285)	(0.090)*	(0.030)**	(0.011)**	(0.020)**
	Wald	-0.055	-0.030	-0.038	-0.045	-0.044	-0.041
		(0.158)	(0.286)	(0.088)*	(0.029)**	(0.011)**	(0.020)**
R4	ITT	-0.056	-0.062	-0.071	-0.082	-0.081	-0.079
		(0.111)	(0.005)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
	Wald	-0.084	-0.088	-0.103	-0.121	-0.123	-0.120
		(0.102)	(0.004)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
R5	ITT	-0.000	0.009	0.008	0.003	0.000	0.002
		(0.997)	(0.667)	(0.619)	(0.835)	(0.980)	(0.886)
	Wald	-0.000	0.013	0.012	0.004	0.001	0.002
		(0.997)	(0.669)	(0.622)	(0.836)	(0.980)	(0.885)
R6	ITT	-0.117	-0.158	-0.183	-0.195	-0.190	-0.186
		(0.031)**	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
	Wald	-0.172	-0.221	-0.262	-0.288	-0.287	-0.285
		(0.028)**	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
R7	ITT	-0.122	-0.142	-0.150	-0.162	-0.161	-0.159
		(0.007)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
	Wald	-0.180	-0.198	-0.215	-0.239	-0.244	-0.243
		(0.005)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
R8	ITT	-0.030	-0.031	-0.027	-0.021	-0.018	-0.016
		(0.095)*	(0.008)***	(0.002)***	(0.014)**	(0.017)**	(0.042)**
	Wald	-0.045	-0.043	-0.039	-0.031	-0.027	-0.025
		(0.092)*	(0.007)***	(0.002)***	(0.015)**	(0.017)**	(0.041)**
R9	ITT	-0.066	-0.063	-0.071	-0.077	-0.070	-0.065
		(0.031)**	(0.004)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
	Wald	-0.096	-0.089	-0.102	-0.113	-0.106	-0.100
		(0.027)**	(0.003)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***

For explanations, see bottom of next Table.

Table 2.7: Fuzzy RD, Outcomes

		5	10	20	30	40	50
Gini	ITT	0.070	0.070	0.064	0.069	0.067	0.065
		(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
	Wald	0.104	0.099	0.093	0.102	0.102	0.099
		(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
Ineq	ITT	0.003	0.003	0.002	0.003	0.002	-0.001
		(0.705)	(0.620)	(0.679)	(0.545)	(0.724)	(0.838)
	Wald	0.005	0.004	0.003	0.005	0.003	-0.002
		(0.705)	(0.621)	(0.678)	(0.546)	(0.728)	(0.839)

Bandwidths 5-50 given by distance to the frontier in km, dependent variables as in previous Tables.

Endogenous Regressor is the Share of Protestants as measured in the 1980 census.

P-values in parentheses based on SEs obtained through bootstrap; * 0.10 ** 0.05, *** 0.01

Table 2.8: Effect of Protestantism on Preferences for all of Switzerland

	R1	R2	R3	R4	R5	R6	R7	R8	R9
Prot.	-0.096	-0.096	-0.067	-0.042	-0.025	-0.127	-0.102	-0.078	-0.065
	(.000)***	(.000)***	(.000)***	(.000)***	(.021)**	(.000)***	(.000)***	(.000)***	(.000)***
Altitude	0.015	-0.076	-0.026	-0.026	-0.021	-0.093	-0.014	-0.058	-0.036
	(.166)	(.000)***	(.002)***	(.003)***	(.029)**	(.000)***	(.186)	(.000)***	(.000)***
German	0.074	-0.074	-0.021	-0.050	-0.161	0.016	0.024	0.059	-0.073
	(.000)***	(.000)***	(.005)***	(.000)***	(.000)***	(.169)	(.024)**	(.000)***	(.000)***
Constant	0.345	0.593	0.296	0.416	0.359	0.662	0.628	0.234	0.363
	(.000)***	(.000)***	(.000)***	(.000)***	(.000)***	(.000)***	(.000)***	(.000)***	(.000)***
R2	0.376	0.573	0.399	0.388	0.733	0.401	0.424	0.471	0.456
N	2,794	2,795	2,795	2,721	2,721	2,789	2,795	2,795	2,795

9 referenda as listed in Table 2.1; Prot. is the share of Protestants in 1980; Altitude measured in 1000m above sea level;

German is indicator variable for the historical language; P-values in parentheses based on robust SEs; *0.10 **0.05, ***0.01

Each regression uses all Swiss municipalities existent in the year of the respective referendum; these vary due to splits and mergers.

All regressions control for canton fixed effects; Results qualitatively unchanged when controlling also for: area, population, share aged <20 and >60, share of foreigners, unemployment rate, mean income, 4 educ. categories, 10 types of socio-economic status, 9 municipality types (details in text).

CHAPTER 3

WEALTH DECUMULATION DURING UNEMPLOYMENT: EVIDENCE FROM A SCANDINAVIAN WELFARE STATE.

3.1 Introduction

How much can and do households use their savings to maintain consumption *after* job loss? Are households able to anticipate job losses and prepare specifically in the years *before* by saving more? Do they change the risk or liquidity *structure* of their portfolio? The "Optimal Unemployment Insurance" framework introduced by Baily [1978] and further developed by Chetty [2006] made it clear that private saving and dissaving can be seen as the main substitute for publicly provided unemployment insurance.[1] The extent to which households fill the income gap that remains after the receipt of unemployment insurance by decumulating prior savings gives some indication of how good a job unemployment insurance is doing at insuring household consumption. These are important questions, yet the topic of household portfolio responses to unemployment has so far been little explored, chiefly due to the limited availability of data on the wealth of the unemployed.

The main exception is Gruber [2001], who uses data from the American Survey of Income and Program Participation (SIPP) that allow him to observe self-reported wealth once before and once after job loss. This provides insights both into the adequacy of savings before job loss, and into the extent to which this wealth is drawn upon following that job loss. Thus Gruber finds that in the US the median job loser has sufficient assets to cover two thirds of the typical income loss, or equivalently to *fully* finance 5.4 weeks of unemployment, but one third of the workers in his sample cannot even replace 10% of their income loss. Against this backdrop, the *contributions of this paper are threefold.*

Firstly, we provide an analysis of the use of wealth around job loss that *addresses important sources of bias* which can arise if households with above- or below-average ability to draw upon private savings are particularly likely to become unemployed, or if job losses are particularly likely in years with non-average portfolio performance. To do so, we use

[1] This captures the benefit side of the Optimal UI framework. To determine whether the current level of UI is optimal, one needs to know also its Moral Hazard cost, which we do not address in this paper, but which has been estimated for Norway in Roed and Zhang [2003].

annual panel data to explore the within-household paths of income, financial wealth, and components of financial wealth through nine "relative" years around the job loss year "U", from U-4 to U+4, while controlling also for calendar year fixed effects. For the *average* household in our baseline specification, we find a financial wealth depletion amounting to some $2,500 between the peak in year U-1 and the trough in year U+2, corresponding to some 16% of the pre-tax and 25% of the after-tax shortfall of income in those years relative to the scenario of an unchanged income path.[2]

Interestingly, about half of that amount is accumulated as additional savings just before, between U-4 and U-1, and the other half is rebuilt straight after, in years U+3 and U+4, so that at the end of our panel the average household's financial wealth is roughly back at the level in U-4. This time pattern is interesting also in the contect of the debate on whether household saving behavior is better characterized by the common interpretations of Friedman [1957]'s Permanent-Income/Life-Cycle-Hypothesis framework, in which households saving for retirement have infinite horizons and discount rates as low as 4%, or by models of Precautionary or Buffer-Stock-Saving as in Deaton [1991] or Carroll [2001], where they optimize over a horizon of only a few years and save primarily to buffer against shocks like unemployment.[3] Our finding that half of the wealth depletion in between U-1 and U+2 is financed by additional saving between U-4 and U-1 and the other half by additional saving between U+2 and U+4 can be interpreted as supporting the view that significant consumption smoothing is in fact done over a shorter horizon. This impression is further confirmed by our second contribution.

Secondly, we decompose the financial wealth variable to investigate possible portfolio reshufflings in anticipation of the job loss: We split households' financial portfolios into the asset classes "stocks and mutual funds" on the one hand and "bonds and bank deposits" on the other hand, and then track the time paths of holdings in each asset class separately. We find that between years U-4 and U-1 the *average* household shifts close to $2,000 from stocks and funds into bonds and deposits, amounting to about 80% of the amount eventually depleted. This is consistent with the predictions of papers that add "human wealth", i.e. the net present value of all future labor income, to the household portfolio, with the implication that when human wealth is perceived to become more risky, financial wealth needs to be

[2] For a preview, see Figure 3.2

[3] In fact, Carroll [2001] points out that Friedman himself did not have in mind an infinite horizon either. He quotes him as saying "It would be tempting to interpret the permanent component [of income] as corresponding to the average lifetime value... It would, however, be a serious mistake to accept such an interpretation". He then cites Friedman [1963] as assuming a subjective discount rate of 33.3% rather than the 4% typically assumed in Certainty-Equivalence Models.

made less risky.Bodie et al. [1992] present such considerations in the context of a life-cycle model[4], where households save and invest for retirement, but similar considerations can be seen to apply to saving and investment over a shorter time horizon. It needs to be stressed though that on average bonds and deposits are arguably not only less risky but also more liquid, and so our present evidence cannot reveal to what extent the reshuffling is motivated by risk considerations, and to what extent by the strife to make the portfolio more liquid before depleting it.[5] On either interpretation though, evidence on portfolio reshuffling prior to job loss is novel in the literature, and should hence be seen as an interesting starting point for further research on this issue.

In addition to our main analyses, we consider the possibility that our household and year fixed effects might not suffice to make job loss exogenous. This would be the case if potential confounding factors are not only year or household but also year*household specific. For instance a personal crisis might at the same time reduce the usability of the portfolio and increase the propensity lose the job, or fantastic asset returns might at the same time improve the usability of the portfolio and thereby make individuals more willing to quit their jobs. To address this issue, we repeat our main analyses for a subsample of households whose job loss occurred as part of major plant downsizings. We find that the individuals struck by downsizings fare worse, and that this is likely not due to other differences in the subsample composition. This suggests that to the extent to which unemployment in our main sample is still endogenous, it is more likely to occur amongst the better-prepared than amongst the worse-prepared households.

Thirdly and finally, as a way to explain what determines the amount of wealth depleted, we exploit heterogeneity across households along four dimensions: We compare sets of households which differ in prior holdings (as a proxy for –possibly endogenous–liquidity constraints), age, unemployment spell length, and permanent income changes. We find that those with low prior wealth deplete less wealth, suggesting tentatively that part of our sample may be liquidity constrained, an issue we take up more rigorously in a companion paper, Basten et al. [2011a]. In line with this, we find that the older half of our sample, who have accumulated higher levels of prior wealth, draw upon their resources more. Thirdly, wealth depletion relative to the annual income drop is quite similar amongst those who are unemployed for a longer period than amongst those with only a short unemployment spell.

[4] "At any given age in the life cycle, the riskier is an individual's human capital, the lower will be his financial investment in risky assets." Bodie et al. [1992], section 5, proposition 6.

[5] The issue of portfolio liquidity is considered in Otsuka [2005] who emphasizes that significant parts of the precautionary savings against unemployment are held in illiquid assets, due to the higher returns offered there.

And finally, we see more rather than less wealth depletion amongst those whose income drop is more permanent, contradicting the predictions of the Permanent-Income Hypothesis whereby individuals should smooth consumption around temporary but not around permanent income shocks.

The remainder of this paper is structured as follows: Section 3.2 starts by introducing our data and general methodology. Section 3.3 then presents the results on wealth level changes before and after job loss, and Section 3.4 those on changes in the portfolio structure. In Section 3.5 we explore how depletion results differ for households who lose their job in the course of plant downsizings, and Section 3.6 explores how they differ across a number of relevant subsamples. Section 3.7 concludes.

3.2 Data and Methodology

We use administrative data from Norwegian tax registers that cover the universe of Norwegian taxpayers. Four features make this data set ideal for our purposes: Firstly, tax register data are likely more reliable than survey data, an issue that has previously been found to be of particular importance for data on income and wealth, as well as for data on unemployment spells, both of which are frequently recalled imperfectly or mis-reported.[6] Secondly, observing households in a panel format for a total of 13 years allows us to distinguish household and calendar year fixed effects from what happens in the different years around job loss. Finally and importantly, we are able to merge information on employment status and earned income with information on household financial wealth, and with information on employment at the plant level, a *sine qua non* for identifying downsizings. For our main analyses, we follow Gruber [2001] in focusing on financial wealth, which includes stocks, bonds, bank deposits and mutual funds. This is in line with the arguments in Chetty and Szeidl [2007] whereby fixed transaction costs make it not worthwhile to liquidate a house to pay for an unemployment spell. Besides, measures of the *value* of a house, and its changes from year to year, possess limited reliability. We do however, additionally use information on *whether* households enter or exit the real estate market, which we do observe with sufficient precision.

We focus on the unemployment incidences of married husbands[7], and restrict the age range from 29 to 60, so as to exclude households still in full-time education or with access to

[6] For an example of the effects of mis-reporting in household surveys, see Meyer et al. [2009]

[7] Even in Norway, job loss of the wife typically implies a less drastic shock to the household's budget, and is therefore less interesting to investigate for our present purposes.

early-retirement schemes. To ensure that everyone in our sample has access to at least one full year of unemployment insurance (UI) benefits at the 62.4% replacement rate, we drop households whose pensionable income in the year before job loss fell below the threshold required for UI eligibility. This threshold is very low: It is updated every year, but in 2010 it lay at NOK 109,322, or about $ 17,000.[8] For the same purpose, we exclude households with business income, as their unemployment benefits are calculated under slightly different rules.

Our panel data cover the 13 years from 1995 until 2007. This allows us to trace households over a considerable time span both before and after their job loss, which we identify from the receipt of unemployment benefits. For employees with benefit spells in several calendar years, we use as "base year" the first such year. To identify the effect of different "relative years" around job loss separately from those of different calendar years, we need to take "base years" (years of job loss) from different calendar years. If we use more different base years we get to observe a greater number of households, whereas a lower number of base years allows us to track them for a greater number of relative years. Trading off these two benefits, we have use job losses occurring between 1999 and 2003. This allows us to track our dependent variables for 4 years before job loss, taken from 1995 and 2002, and 4 years after job losses, taken from 2000 until 2007. To ensure that our comparison of income and wealth in those different relative years is not biased by differences in the sample composition, we require our panel to be fully balanced across the 13 different calendar years, leaving us with 8,667 households, or 112,671 household year observations.

Finally, we follow Chetty [2008] in excluding from our main analyses those who after the spell return to their old plant, on the reasoning that in Norway many of these will know upon job loss or even before that they are going to return to their old employer and under which conditions, thus removing the uncertainty about future labor income. This reduces the sample size from 8,667 to 5,513 households.[9] These are distributed fairly equally across our five base years[10]. To make our results robust to outliers, we top-code both income and wealth at the 99th percentile of their distributio within each calendar year. Furthermore, we consistently use 2004 as the omitted calendar year category, and convert NOK amounts

[8] For more information on this "basic amount", or "Grunnbeløpet i Folketrygden", see www.nav.no/english. For more information on the data, see- http://www.ssb.no/english/mikrodata_en/datacollection/fdtrygd

[9] In our illustrative initial cross sectional analysis, we exploit the full balanced sample of households satisfying the marriage and age requirements mentioned above. For the whole sample period this implies some 2,100,241 household-year observations.

[10] The number of spell starts in years 1999-2003 is respectively: 1,141 (1999), 1,040 (2000), 896 (2001), 1,172 (2002) and 1,264 (2003).

into US dollars at 2004 exchange rates, with $1 corresponding to about NOK 6.7, so that all monetary variables will be displayed in 2004 US dollars. For some placebo analyses to be discussed below, we have additionally drawn a sample of households from the general population who, instead of losing their job at some point, are employed in the same plant over the 13 year sample period. These 50,095 households are then assigned random "base years" from 1999-2003 matching the distribution of the unemployed sample.

We are interested in how households' income and, as a consequence, financial wealth and its components are affected by job loss. As we shall also demonstrate below, simple cross-sectional regressions of our outcome variables on unemployment status are inadequate to answer that question, because those likely to experience a job loss are likely to have different income and wealth levels also cross-sectionally, and because calendar years with many job losses are likely to coincide also with non-average portfolio returns in the financial markets. The first requirement to identify our effect of interest therefore is the use of panel data, allowing us to control for both household and calendar year fixed effects. These considerations lead to the following baseline specification:

$$Y_{i,t} = \alpha_i + \beta(RY_{i,t}) + \gamma_t + \varepsilon_{it} \tag{3.1}$$

Here $Y_{i,t}$ denotes our different outcome variables to be explained below, α_i is a household fixed effect for household i, γ_t is a vector of dummies for our 13 calendar years, and $RY_{i,t}$ is a vector of dummies for the nine relative years of and around job loss.[11] Because we use job losses from five different "core years", 1999-2003, we are able to separately identify the calendar and the relative year fixed effects. By estimating this equation both for annual income and for household financial wealth, we are then able to plot the average time paths of both variables, and then to relate changes in financial wealth to changes in annual income. In the same fashion we split financial wealth into different asset classes, and then apply the same methodology to investigate intra-portfolio reshufflings. On top of absolute wealth in dollars, we have also used wealth scaled by average annual income and the log of that, and regressions with these give qualitatively very similar results.

3.3 Results on Prior Holdings and Depletion

We first relate our work to the existing literature, comparing our measures to the most recent literature on prior holdings. We then perform simple cross sectional analysis for

[11] Given our RY dummies, the additional household fixed effect is no more necessary for unbiasedness, but helps to increase precision. Standard errors are clustered at the household level.

earnings, financial wealth and the subcomponents of households financial wealth. Finally we investigate the time path of these variables. Our results indicate moderate levels of depletion relative to income short falls, but we also see significant movements in the sub categories of the portfolios in relation to job losses.

3.3.1 Prior Holdings

Before analyzing how much wealth households *use* during unemployment, it is instructive to look at how much they *have*, as well as to compare these prior holdings with the ones which Gruber [2001] reports for his American sample. We do this in Table 3.1, which reports pre-layoff wealth holdings first for our placebo sample of individuals who never lose their job throughout our 13-year panel, then for our main sample of job losers, and finally for those who lose their job in the course of a major plant downsizing, a subsample which we will analyze in more detail in Section 3.5 below. We compare prior holdings in these three samples with each other, but also with the three sub-samples used by Gruber [2001]. All are scaled by weekly earnings, thus allowing us to compute how many weeks of unemployment a household could finance at a zero savings rate, both with and without unemployment benefits. It also allows for comparisons between our and Gruber's samples, without the need to first deflate or convert them into another currency.

The first thing that stands out in this table is that, both for those who do not and for those who do lose their jobs, scaled prior holdings are until the median of their respective distribution higher in the Norwegian than in the US sample, whereas above the median the reverse is true. So we may infer that in Norway households in the lower half of the wealth-over-earnings distribution are on average better off than their counterparts in the US. For the median job loser, prior holdings amount to 9.23 weeks of earnings. Taking into account that everyone in our sample can get 62.42% of previous income replaced through unemployment insurance, this implies that in theory the median job loser could afford to fully replace his income for at least 24.55 weeks without having to resort to any borrowing, and to fully replace prior consumption even longer if his previous consumption level was lower than his income.

Given our stipulation in the introduction that private savings can be seen as a substitute for publicly provided UI, one may wonder why many Norwegians do not reduce their savings to about the American level, given the higher and longer UI, and given that their retirement benefits would also seem significantly safer in light of the country's continuing oil revenues. On the other hand though, the higher savings do fit with Norway's longer average

unemployment durations[12], in whichever direction the causality between the two may be.

However, even in Norway the bottom 10% of the distribution hold only 84% of a weekly income in financial wealth, implying that with UI benefits amounting to 62% of prior wages they could fully maintain consumption levels for about two weeks, or slightly longer if previously they used to consume less than their full income, although this seems highly unlikely given the low level of savings we observe. This suggests that for all the generous UI system and the generally high and equitable wealth, liquidity constraints might be an issue for at least some unemployed even in Norway, a result that we will confirm in the results on wealth use below. Beyond those results, it is also instructive to compare holdings between our three different sub-samples: Here we see that in general those losing their jobs in the subsequent year have lower holdings than those who remain employed, suggesting that to some extent it is the less prepared who lose their jobs. The comparison between downsizing and other unemployed yields no general pattern, because at the bottom of the respective distributions the downsizing unemployed seem to be doing somewhat better, whereas at the top the reverse is true.

3.3.2 Cross-sectional analysis of Depletion

To demonstrate the relevance of our methodology, as explained above, it is illustrative to start with some simple cross-sectional regressions of income and financial wealth on unemployment. The results are displayed in Table 3.2, first with all years pooled and then for each core year separately. Using this methodology, the average household is predicted to have a $10,000 lower annual husband income, and between $6,000 and $10,000 lower financial wealth. The problem with interpreting this as the "effect" of unemployment is of course that the kind of households who experience unemployment may on average have lower incomes and financial wealth to start with. In addition, it is likely that the years in which many households experience unemployment are also those in which both incomes and wealth suffer relative to more booming years. Indeed, these suspicions will be confirmed once we obtain the fixed-effect results below and compare the cross-sectional results to those.

3.3.3 Income and wealth paths of the average household

Figure 3.1 displays the time paths around the year of unemployment for our sample of 5,513 households defined in the previous section. Two measures of income for the husband are available and used as outcome variables when estimating Equation 3.1: Firstly the sum

[12] See for instance http://stats.oecd.org/Index.aspx?DataSetCode=AVD_DUR

of all pre-tax pensionable labor income, and secondly total (including capital) income after tax. Pure labor income after tax cannot be measured, because tax dues in Norway depend jointly on labor and capital income. The latter measure is more noisy due to the capital income component, but is nonetheless shown for the average household to roughly follow the same time path as labor income. Since the need for using financial wealth will depend on changes in disposable income, the latter measure is more appropriate and so our main analyses will henceforth focus on this measure. We plot each relative year coefficient together with the constant, both obtained by estimating Equation 3.1. Looking at the pictures of income paths, the first thing worth noting is how our calendar year controls manage to take out baseline year-on-year growth in income: Throughout the four years preceding the job loss, the income curves are pretty much flat. In this first graph we plot results for both income measures, using the left-side axis for pre-tax and the righ for after-tax income. This illustrates nicely how the progressive tax system cushions some of the drop: As about one-third of income is taxed away, so is essentially one-third of a temporary income loss, and slightly more for someone who during his year with unemployment moves into a lower tax bracket.[13] Focusing henceforth on after-tax income then, we find that the drop in income in the year of job loss as well as in the subsequent year is about $5,000 or 10% relative to the income level of previous years. By year U+4, the average household manages to close about 80% of that gap.

Figure 3.2 then displays together the time paths of Income After Tax (right axis) and financial wealth (left axis). Whereas in the income graph, given household fixed effects and standard errors clustered at the household level, only the drop around job loss is statistically significant at conventional levels, the financial wealth graph has several interesting features. To begin with we see a distinct increase (significant at the 5% level) in holdings from U-4 until U-2, suggesting that at least some of the households do sense the greater risk of job loss and respond with increased saving. This is consistent with the predictions of a Buffer Stock Savings model as in Carroll [1997], where households smooth consumption over a horizon of a few years, accumulating and maintaining in good years a buffer-stock of wealth over permanent income, which they can then draw upon in cases like periods of unemployment.

Even though we detect these increased savings specifically between U-4 and U-2, as well as between U+2 and U+4, whereas we detect dissaving from U-1 through U+2, the skeptical reader might be worried that what we are plotting here reflects simply some general life-cycle

[13]Note that for income tax purposes, UI benefits in Norway are taxed under the same rules as regular labor income.

related saving or asset returns achieved in the stock market which our controls might fail to properly account for. To address such potential concerns, we have created a placebo sample of workers with high attachment to their plant but a with similar age distribution. We have then conducted the same analyses for income and financial wealth for this placebo sample. Figures 3.4 and 3.5 plot for respectively income and financial wealth the time paths for both the treated and the placebo group.While for the treated group we have the distinct patterns just discussed, the income graph for the placebo group is entirely flat, and the financial wealth graph is almost entirely flat, with none of the small remaining movements being statistically significant at any of the conventional levels. This is clear evidence in favor of our methodology, because with the larger sample size of our placebo sample any remaining patterns would certainly be detected.

3.4 Results on the Portfolio Structure

We have now seen that the average household in our sample increases savings in the years leading up to job loss, depletes part of that wealth in the years straight afterward, and starts resaving toward the end of our panel. Household wealth however consists not just of a single type of asset, but can be saved and invested in different asset classes, with stocks and mutual funds promising higher expected returns, and bonds and bank deposits promising lower riskiness and greater liquidity. As Bodie et al. [1992] emphasize, the choice of the risky share in the financial portfolio is likely to depend on the riskiness of what he denotes as "human wealth", the discounted sum of future labor income. They point out that households that have a target risky share for their total wealth, i.e. the sum of human and financial wealth, will respond to increased riskiness of their human wealth by making their financial wealth less risky. While theirs is a life-cycle model of saving, in which the purpose of saving and investment is to prepare for old age, the same considerations can be seen to apply to savings made in order to prepare for accidents like unemployment. The prediction then is for households who perceive a greater risk of unemployment, as our previous results suggest they do, to move some of their financial wealth from more to less risky assets, and revert that reshuffling only once the uncertainty about future labor income starts to fade away.

To investigate whether this is indeed the case, we next split household financial wealth into two asset classes, one including stocks and mutual funds and the other bonds and bank deposits. Figure 3.3 plots the predicted time paths of the two asset groups, as estimated with separate implementations of Equation 3.1. The holdings of stocks and funds are measured

on the right axis and those of bonds and deposits on the left axis, and while the levels clearly differ across the two asset classes (In U-4 the average household holds about $23,500 worth of bonds and deposits[14] and about $7,250 worth of stocks and funds, implying a "risky share" of stocks and funds over total financial wealth of about 30%), the axes are constructed such that the same vertical distance covers $1,000. This allows us to see that the reduction in the holdings of stocks and funds that occursthe between U-4 and U-1 coincides with a build-up of bonds and bank deposits, where both developments are significant at the 1% level or smaller. While the values predicted with the separate regressions for the two asset classes and all financial wealth do not exactly add up due to the separate sets of calendar year fixed effects to account inter alia for different asset returns, the graph clearly supports the view that financial wealth is reallocated within the portfolio. It is worth noting at this point that the assets which are on average less risky are also on average more liquid (especially since most of the bonds&deposits assets are in fact deposits), so that the results here presented cannot unambiguously prove that all reshuffling is motivated by riskiness as opposed to liquidity considerations, as for instance implied by Otsuka [2005].

In the data section above we have argued for focusing for our main analyses on financial wealth only, due to the limitations in measuring the appropriate market *value* of real estate holdings as well as the more significant transaction costs associated with trading a house. Nonetheless some households might sell their house in order to have more liquidity available right now, in order to have more liquidity available in the coming years in case it takes longer to find a new job or the next job is significantly worse-paid or less secure. Beyond portfolio considerations, they might also sell their house if finding a new job requires them to move to a different place. Since we are able to observe with precisions which households do hold some real estate, this is just interesting for an additional analysis, in which we repeat our estimation of Equation 3.1, but this time use as dependent variable an indicator variable for home ownership. To ensure the robustness of our procedure, we again conduct the same analysis both for our main sample and for the placebo sample. Both graphs are displayed in Figure 3.8, for the treated sample as solid line and measured along the left axis, and for the placebo sample as dotted line and measured along the right axis. Ownership levels differ across the two groups, with about 10 percentage points higher ownership amongst the placebo sample in year U-4. More interestingly, while the graph for the placebo sample is entirely flat, the treated sample experuences an economically and statistically (at the 5% level) significant decrease in ownership by about 2.5 percentage points between U-4 and U, a bit over half of which is reverted afterwards, i.e. about half of these have bought a new,

[14] Thereof $22,000 in deposits and only $1,500 in deposits, not separated in the graph.

most likely different (smaller, or differently located) house bu U+4, whereas the other half is for the time being renting. It is worth emphasizing that most of the sales occur already before the point of job loss. This may be because in most employment contracts individuals need to be notified at least 6 months prior to their job loss, but as a note of caution we need to stress that in some cases third factors such as family crises may be the common cause of selling the house and quitting the job. To look into this issue in more detail, the next section focuses on job losses that occur as part of major plant downsizings.

3.5 Addition 1: Plant Downsizings

One challenge affecting any paper on the effect of job loss is the possible endogeneity of the latter. The fixed-effects specification explained above allows us to fully control for any time-invariant personal characteristics, as well as any person-invariant year characteristics. But what if the specific households experiencing unemployment are affected by events specifically in the years in which they also become unemployed? For instance, if households with a greater ability and willingness to spend wealth on being unemployed invest less effort into retaining their jobs, then prior wealth holdings and income replacement out of wealth during the spell may paint too rosy a picture of the situation of the unemployed. Conversely, if personal non-job problems cause both lower savings rates in the years preceding job loss and this job loss itself, then the picture we get may be too dark as a representation of the causal effect of job loss alone. Also, the years in which many job losses occur, are likely characterized by non-average returns on assets that also affect household wealth. To deal with such potential problems, we identify a subset of job losers whose job loss occurs in the course of a major employment reduction in their plants, thus reflecting difficulties at the plant level as opposed to developments specific to the individual or household in question. This empirical strategy has been frequently used in labor economics to identify the effects of displacement on long-term earnings, started by Jacobson et al. [1993] and used more recently for instance in Wachter et al. [2009] and, in a Norwegian context, in Huttunen et al. [2006]. By contrast, it has to our knowledge so far not been applied to analyzing the effect of unemployment on household wealth. We apply it here to the level of household financial wealth, but similar considerations could also be applied to the above analysis of the portfolio composition.

Before we can proceed to present the results, we need to discuss the choices to be made in defining plant downsizings. Two crucial choices here are to be made on respectively the minimum number of employees in a plant, and the minimum fraction of employees being laid off. The trade-off here is between more restrictive choices that will more drastically cut the

number of remaining observations and hence the degree of precision that can be achieved, and less restrictive choices that allow to retain a greater number of observations, but come at the cost of making the exogeneity of job losses to individuals less credible. In the previous literature, Jacobson et al. [1993] and Wachter et al. [2009] picked respectively a minimum employee number of 50 and a minimum downsizing rate of 30%. Huttunen et al. [2006], pointing out that average firm and plant sizes in Norway are significantly smaller than for instance in the US, require instead only five employees. While we agree that the use of a smaller minimum plant size seems sensible in a Norwegian context, we want to avoid defining a lay-off of as few as two out of five employees as a plant downsizing. Therefore we require plants to employ at least ten employees in one of the core years or the last year before our core year period (1998). Additionally, we raise the requirement for the downsizing rate from 30% to 50%. In light of findings in Huttunen et al. [2006] whereby it is rather common for Norwegian firms to just move workers from one of its plants to another, a development which arguably does not always reflect trouble at the firm level and often does not lead to unemployment, we compute this downsizing rate without counting employees who leave a plant merely to continue working at another plant of the same firm. Furthermore, we require that the plant has existed for at least four years prior to the observed downsizing, and that it has not already been downsized, according to the above conditions, within the last three years. When applying all these requirements to the 5,513 cases of unemployment starts in the above-mentioned unemployment analysis, we are left with 1,075 households where the husband starts receiving unemployment benefits in the course of severe mass layoffs at his plant.

The results are depicted for respectively income after tax and financial wealth in Figures 3.9 and 3.10. To facilitate a formal comparison of the patterns experienced by respectively downsizing and non-downsizing unemployed, we have run our regressions on the full sample including both groups, and have interacted each of the coefficients in 3.1 with an indicator variable for whether or not the job loss occurred as part of a downsizing. The income paths of the two groups are remarkably similar in both shape and size, and for neither of the relative years is there a significant (on the 10% level) difference between the two lines. As indicated for the general sample above, the income drop around year U is significant at the 1% level using household fixed effects and clustering at the household level for both groups. Its interesting to note that income-wise both groups seem to recover equally well in the years after their job loss.

Financial wealth paths, as displayed in Figure 3.10, by contrast do differ somewhat:

The "downsizing unemployed" are about $4,000 poorer at the start of our observations in U-4, the difference being significant at the 1% level. According to our point estimates, they also conduct less additional saving between U-4 and U-2, although this difference is not statistically significant at the 10% level. Finally, the amount depleted between U-1 and U+2 is about $800 higher in absolute terms (less relative to the starting level) and is statistically significant at 1% for the non-downsizing, but only at 10% for the downsizing group. Most strikingly, while the non-downsizing group rebuilds its wealth from U+2 onward so as to return to roughly its U-4 level by U+4, as discussed above, the financial wealtperh of the downsizing unemployed remains permanently lower by about $2,000 or 7% until at least U+4. This is interesting seeing that their income recovery was no worse than that of the non-downsizing sample.

A potential concern in interpreting these differences is that beyond the procedure of their job loss individuals in the downsizing sample may differ also in their education, sector, etc, seeing that downsizings are differently common in different industries. To investigate this, we first produced separate summary statistics for the two subsamples, displayed in Table 3.5. This reveals that downsizings are particularly likely to affect those in the engineering sector and those with education until the further secondary levels. Therefore we have repeated the above analysis using instead of the downsizing sample a sample not affected by downsizing but with further secondary education and separated from jobs in the engineering sector. Interestingly, the patterns found here (not displayed, but available on request) looked very much like those of the non-downsizing sample, suggesting that the differences discussed in the previous paragraph are indeed due to the differences in the mode of job loss, as opposed to sample differences in education and/or sector of employment.

So if we use the patterns detected in the downsizing sample as a benchmark for what happens in response to purely involuntary job loss, triggered neither by a sound financial position of the household nor by household-specific personal , non-job difficulties, then this section suggests that if anything then the former pattern of endogeneity is the more prevalent one in our general sample. In other words, most unemployed in Norway manage to cope with their job loss, voluntary or involuntary, fairly well, and some of them may be able to risk temporary unemployment precisely because they are financially prepared for it, but a subsample who lose their job due to bad performance of their plant end up with what seems to be a permanently lower level of wealth, difference being not huge but still non-negligible.

3.6 Addition 2: Determinants of Wealth Depletion

In Section 3.3 above we have documented that the average household depletes significantly less wealth during unemployment than would be possible given our overview of prior holdings. In order to better understand the determinants of wealth depletion, and to allow for some, if lose, connection to the more theoretical literature, we now exploit some of the heterogeneity in the sample, comparing income and wealth paths between 4 sets of subsamples: (i) low vs. high prior holdings, (ii) old vs. young job losers, (iii) long vs. short unemployment spell, and (iv) big vs. small (or no) permanent income drop, where we have always split the sample by quartiles of the distribution of the respective variable. We now discuss each of these 4 analyses in turn.

3.6.1 The Role of Prior Wealth, as a Proxy for Liquidity Constraints

One question that arises from our full-sample analyses is whether the amount of wealth depleted, relative to the shortfall in income, is loss a matter of needs during unemployment, depending for instance on the relevance of home production or the time available for bargain hunting, or whether it is more a matter of how big prior holdings are[15], with less depletion done by those with lower prior holdings. One approximate way of assessing this is to see how the amount depleted during unemployment varies with prior holdings. This is of course not an exact measure of liquidity constraints, since two individuals with the same current level of financial wealth may have different leeway to borrow from their banks, but it is the best proxy for liquidity constraints available in our data set. We compute for each household the ratio of financial wealth holdings and annual income in year U-1, and then split the sample between those for whom this ratio was below and those for whom it was above the sample median. The former group can a priori and ceteris paribus be considered as more and the latter as less liquidity-constrained. We then interact the dummies for the different relative years with an indicator variable capturing which of the two groups a household was in. The resulting predicted paths of both income and wealth are displayed in Figure 3.11: While the income paths of the two groups are very similar, the less constrained households do nonetheless deplete wealth amounts that are about 50% higher than those depleted by the more constrained households. This provides some indication that cash-on-hand may matter for the degree of consumption smoothing. Nonetheless, it is important to point out that these results cannot be considered as conclusive, because the amounts of wealth held in our present setup are potentially endogenous. In a forthcoming companion paper, Basten et al.

[15] These are of course partly endogenous to behavior. One factor that exogenously influences the amount individuals have been able to accumulate until the point of job loss is age, see next subsection.

[2011a], we address this issue more rigorously by exploiting discontinuities in eligibility for severance payments, so as to obtain exogenous variation in cash-on-hand.

3.6.2 The Role of Age at Job Loss

Figure 3.8 predicts income and wealth paths separately for those whose age at the time of job loss was below and those for whom it was above the sample median of 45. The first thing that is interesting here is that the income paths are surprisingly similar, i.e. at least when splitting at the median age we see no evidence that those losing their job at higher ages suffer from longer unemployment durations or lower subsequent wages. Nonetheless, the old have significantly higher savings to start with and as a consequence are observed to deplete about 50% more of it during their unemployment spells, allowing them to achieve significantly more consumption smoothing. The life-cycle stage in which individuals lose their job, assuming that this job loss is exogenous, thus constitutes one factor that may be thought to exogenously influence the extent to which individuals have private savings available for consumption smoothing.

3.6.3 The Role of Unemployment Spell Length

Figure 3.8 splits the sample into those whose unemployment spell length was below and those for whom it was above the median. Note that we observe the spell length only for 1,937 of the 4,664 households of our full sample. The finding here is that those with longer spells tend to be those with higher initial wealth as well as greater absolute depletion amounts, however when we scale depletion amounts by the sizes of the income shortfalls, behavior is not all that different: In years U and U+1 those with short spells have income shortfalls of about \$3,000 compared to \$8,000 for those with long spells. In the same period, those with short spells deplete about \$1,250 of assets and those with long spells about \$2,500, so the degree of consumption smoothing within the year of job loss and the subsequent year is similar.[16] So it does seem to be the case that those with longer spells can afford smaller degrees of consumption smoothing per month of unemployment.

3.6.4 The Role of Permanent Income Changes

Next we would like to know whether depletion is lower amongst those for whom the income drop turns out ex post to be more permanent. As a proxy for the permanent income

[16] The income measure displayed here is pre-tax income, but the relationship whereby the income shortfall of short-spell households is about 3/8 as large as that of the long-spell households holds also when measured in terms of after-tax income.

change, we take the ratio of average annual income in U+3 and U+4, where the large majority of households has returned to a regular job, to average annual income in years U-4 and U-3. Then we split our sample at the median of the distribution of this measure. Figure 3.12 plots the predicted time paths of income and wealth separately for our two subsamples. On the right side of the panel we see the income paths, which differ between the two subsamples by construction. The interesting part then is the left column, which displays the wealth path for those with relatively high new incomes in the upper, and the path for those with relatively low new incomes in the lower half: The finding, interestingly, is that if anything those households with more negative permanent income shocks do deplete more wealth. This is in contrast to the predictions of the Permanent-Income Hypothesis, whereby households should keep consumption smooth when experiencing purely temporary, but not under permanent income shocks. Instead it is consistent with models of Buffer-Stock saving, in which households seek to maintain a certain target ratio of financial wealth over permanent income, so that a decrease in permanent income implies also a gradual reduction in the holdings of financial wealth.

3.7 Conclusion

We have demonstrated the need to control for year and household fixed effects when estimating the effect of job loss on income and wealth, and have then shown that the average Norwegian household covers between 16 and 25% of the income shortfall in the 3 years of and after job loss by drawing down its financial wealth. We have shown that about half of this amount is additionally saved in the 2-3 years before and half in the 2-3 years after the lower-income period, so that at the end of our 9-year panel financial wealth of the average household is almost as high as at the start. We have also shown that a good part of the wealth depleted after job loss is shifted from stocks and funds into bonds and deposits in the 2-3 years before job loss. On a more methodological side, we have demonstrated that in our full sample households with above-average prior wealth holdings are more likely to become unemployed. Finally, we have shown that wealth depletion is higher amongst those with higher prior wealth and is also higher among the old than among the young, is as high for long-term than for short-term unemployed on an annual basis, and is no lower for those with permanent than for those with merely temporary shcoks.

3.8 Appendix

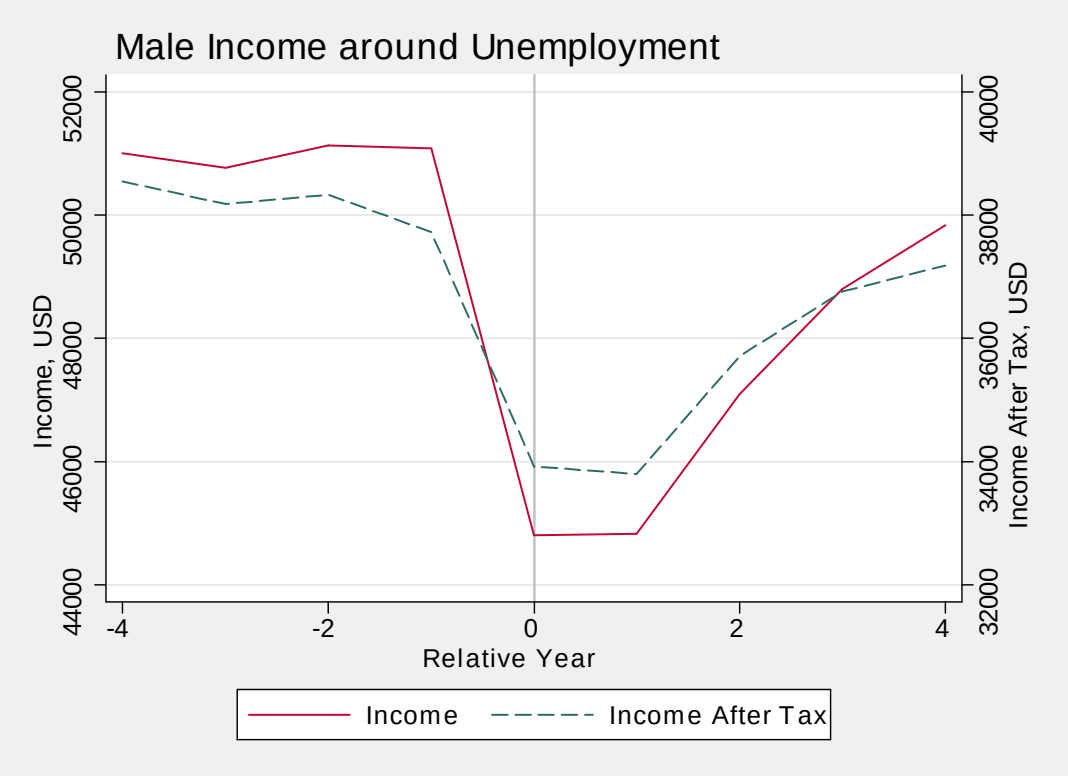


Figure 3.1: Equation 3.1 is estimated with Male Income and Income After Tax as LHS variables, using a sample of 5,513 households as defined in Section 3.2. Displayed are the coefficients of the relative year dummies plus the constant. Both income drops around Relative Year 0 are significant at the 1% level (using with household fixed effects and clustering at the household level). Income After Tax is reported on the RHS axis.

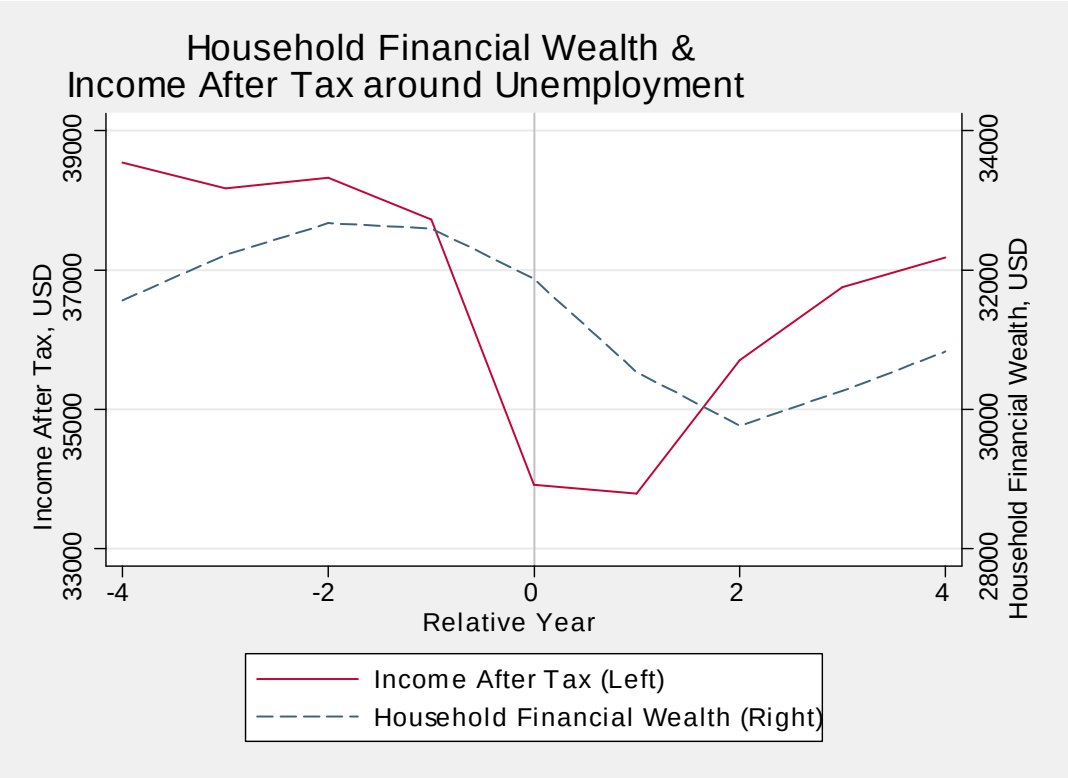


Figure 3.2: Equation 3.1 is estimated with Income After Tax and Household Financial Wealth as LHS variables, using a sample of 5,513 households as defined in Section 3.2. Displayed are the coefficients of the relative year dummies plus the constant. The income drop around Relative Year 0 is significant at the 1% level (using household fixed effects and clustering at the household level). The build up in financial wealth in years before Relative Year 0 is significant at the 10% level. The drop around RY0 is significant at the 1% level. Income After Tax is reported on the RHS axis.

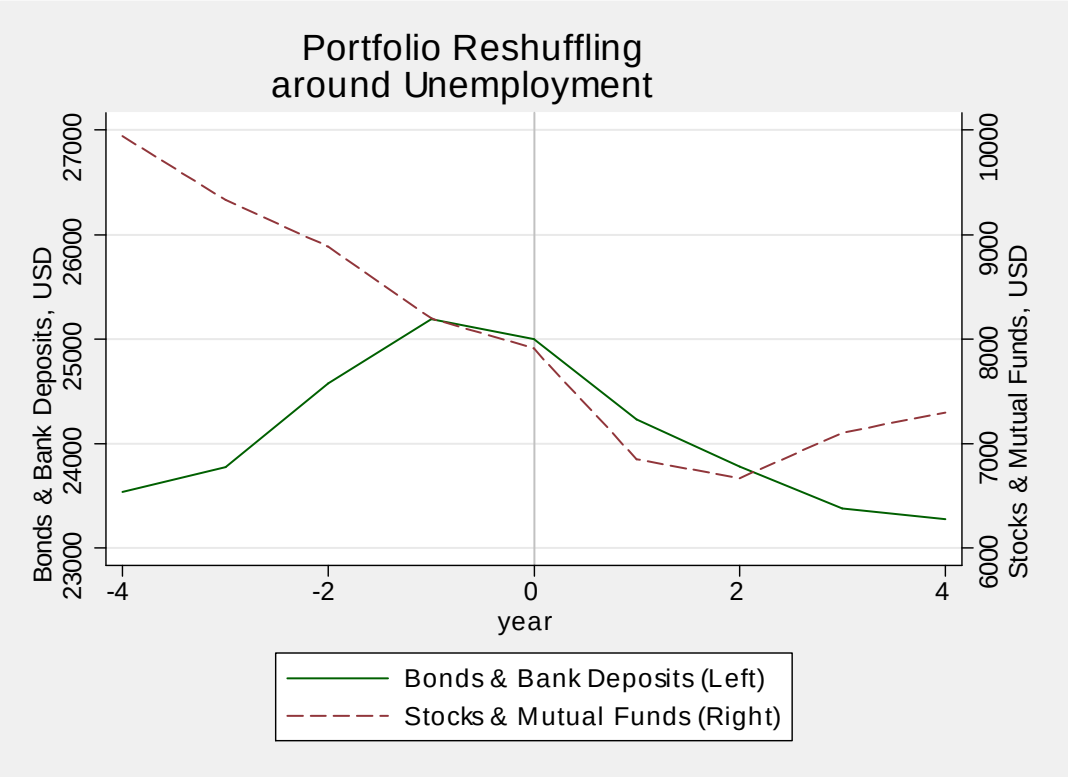


Figure 3.3: Equation 3.1 is estimated using to sub groups of Household Financial Wealth as LHS variables, using a sample of 5,513 households as defined in Section 3.2. This is Bonds & Bank Deposits and Stocks & Mutual Funds. Displayed are the coefficients of the relative year dummies plus the constant. Both the build up in the Bonds & Bank Deposits and the reduction in Stocks & Mutual Funds in the years prior to Relative Year 0 are significant at the 1% level. So are the simliar drops in both variables after Relative Year 0.

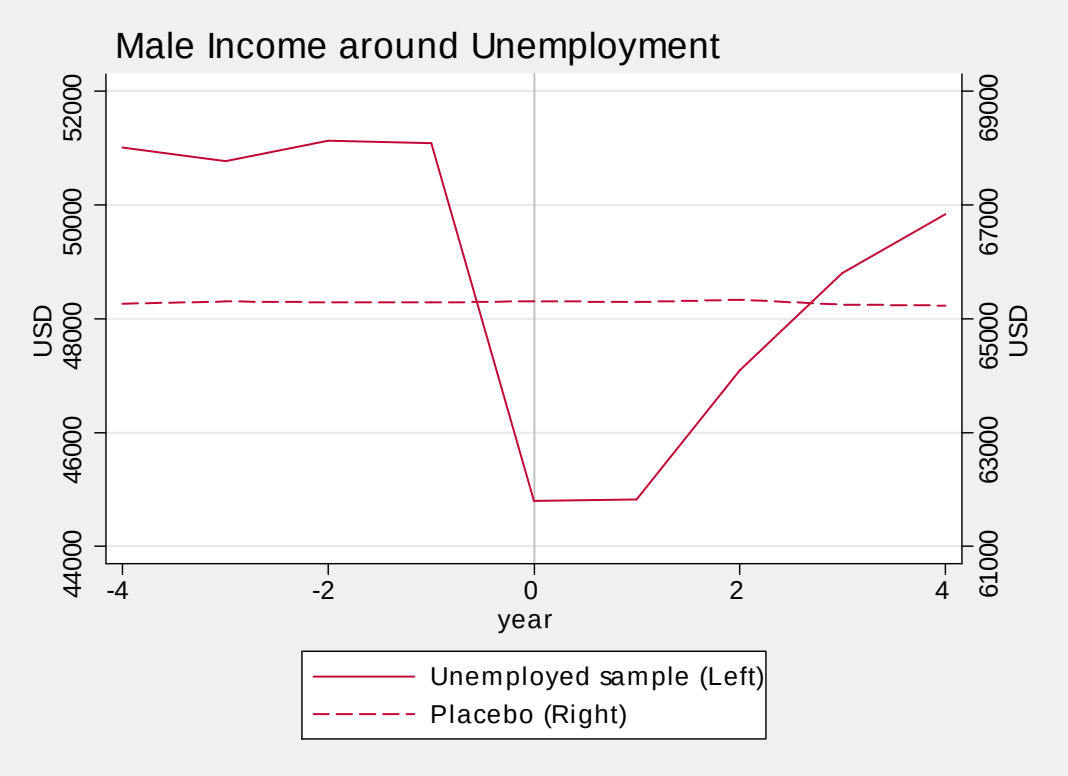


Figure 3.4: The solid line corresponds to the Income line in Figure 3.1. The dotted line is Equation 3.1 estimated on a placebo group of households with high attachment to their working plant, as defined in Section 3.2. Neither of the movements in this line are signficiant at the 10% level.

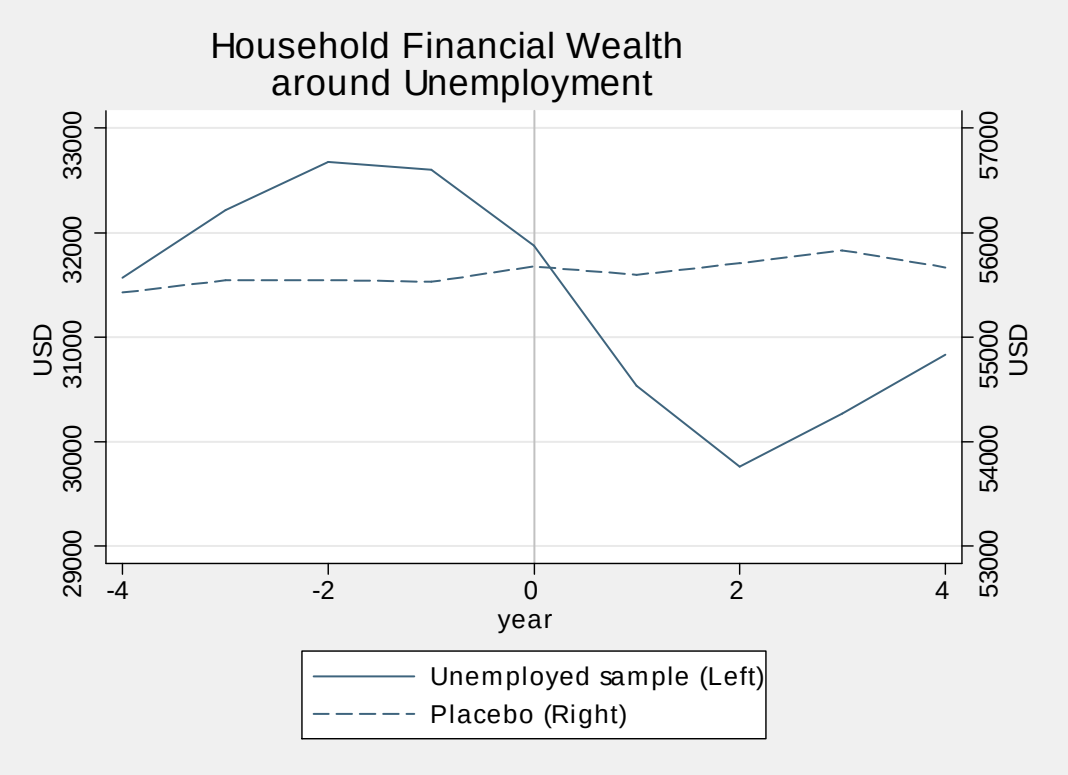


Figure 3.5: The solid line corresponds to the Household Financial Wealth line in Figure 3.2. The dotted line is Equation 3.1 estimated on a placebo group of households with high attachment to their working plant, as defined in Section 3.2. Neither of the movements in this line are signficiant at the 10% level.

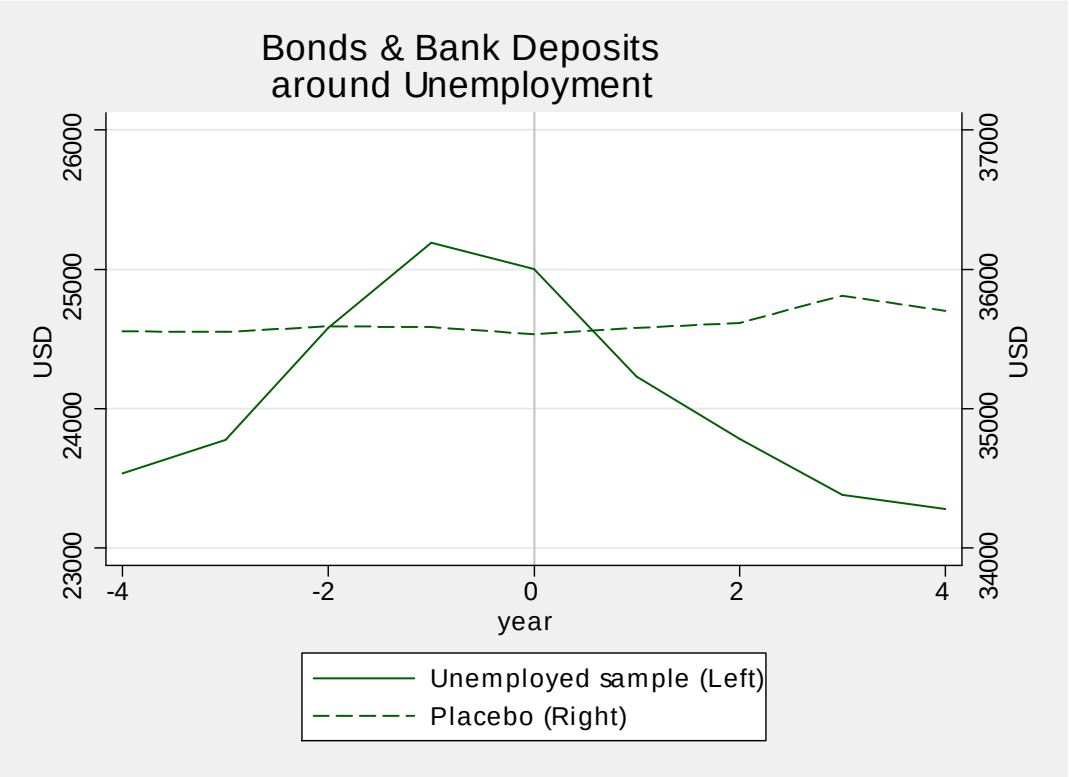


Figure 3.6: The solid line corresponds to the Bonds & Bank Deposits line in Figure 3.3. The dotted line is Equation 3.1 estimated on a placebo group of households with high attachment to their working plant, as defined in Section 3.2. Neither of the movements in this line are signficiant at the 10% level.

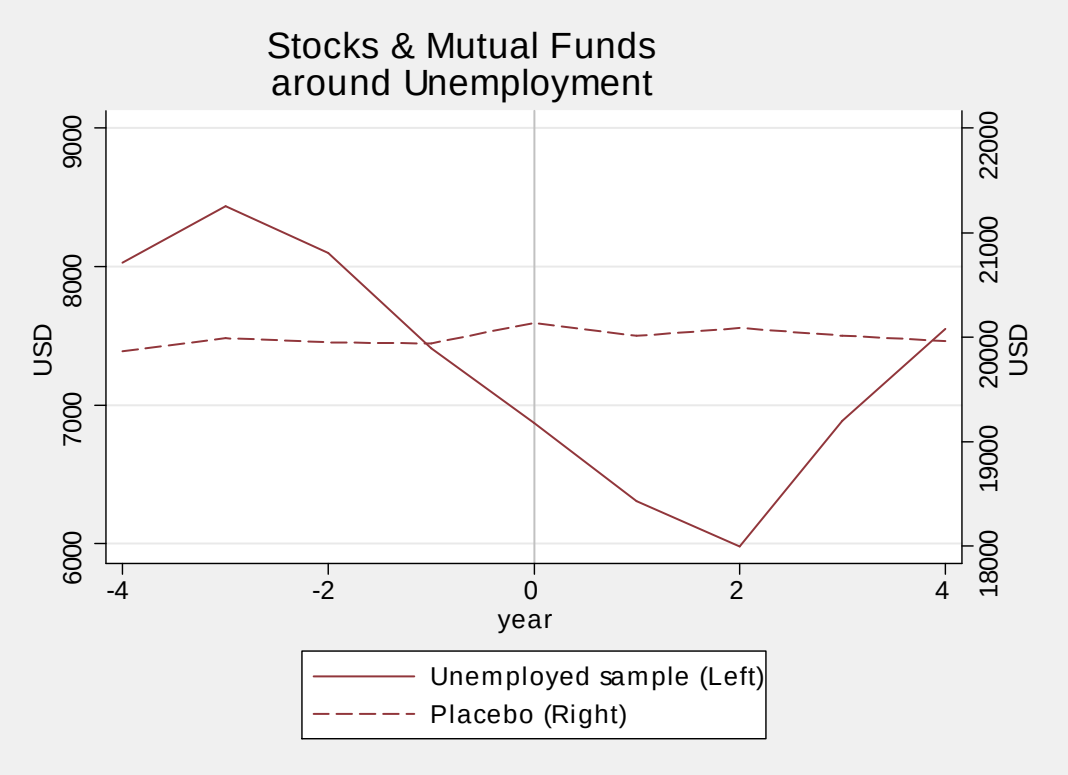


Figure 3.7: The solid line corresponds to the Stocks & Mutual Funds line in Figure 3.3. The dotted line is Equation 3.1 estimated on a placebo group of households with high attachment to their working plant, as defined in Section 3.2. Neither of the movements in this line are signficiant at the 10% level.

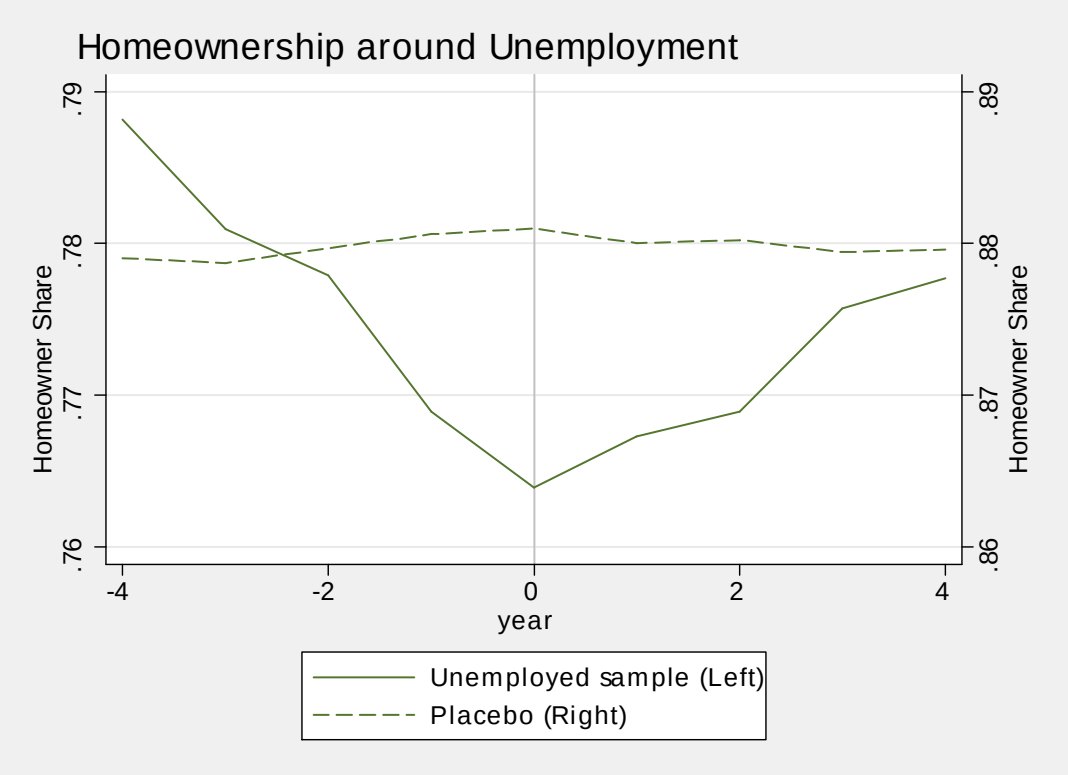


Figure 3.8: The solid line is 3.1 estimated using a dummy variable for household home ownership as the LHS variable, on a sample of 5,513 households as defined in Section 3. The decrease in the years prior to Relative Year 0 is significant on the 5% level. The dotted line is Equation 3.1 estimated on a placebo group of households with high attachment to their working plant, as defined in Section 3.2. Neither of the movements in this line are signficiant at the 10% level.

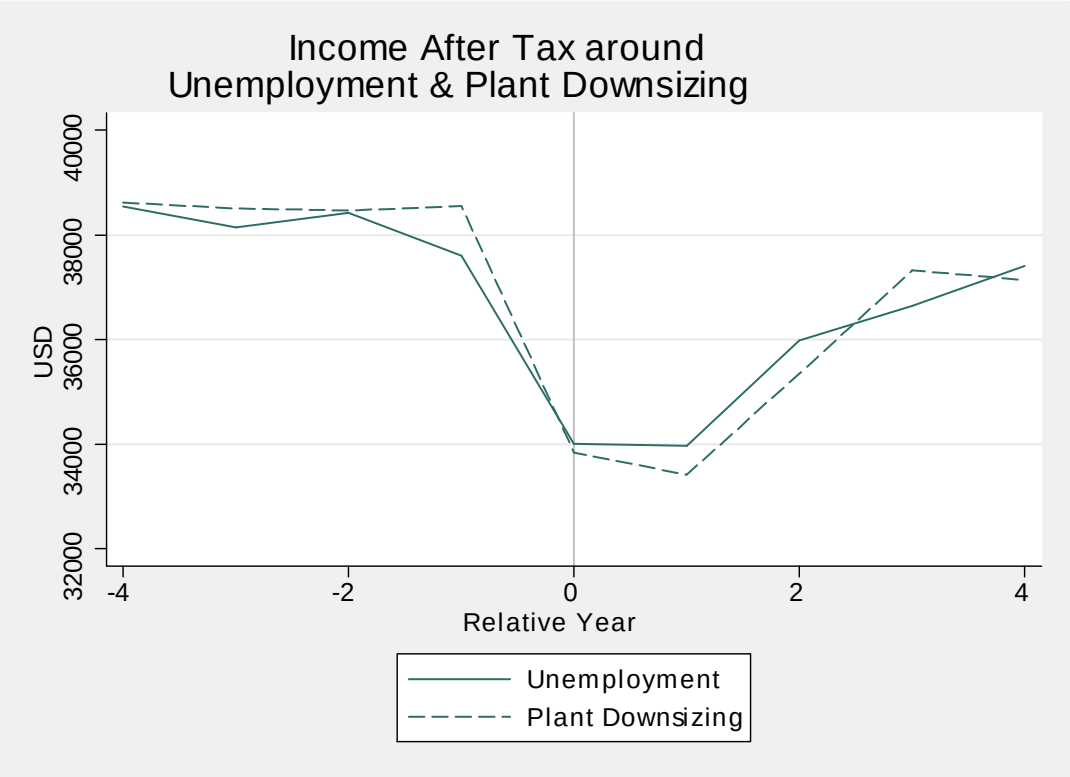


Figure 3.9: Equation 3.1 is estimated with Income After Tax as LHS variable using an interaction term, indicating whether or not a household was hit by a Plant Downsizing in relation to its Unemployment spell. Of the sample of 5,513 households defined in Section 3.2, 1,075 of the households are in the Plant Downsizing group. Both calendar and relative year dummies are interacted with the Downsizing dummy. The Income drops for both groups are significant at the 1% level around Relative Year 0. The difference in levels between the two groups is not significant the 10% level (using household fixed effects and clustering at the household level).

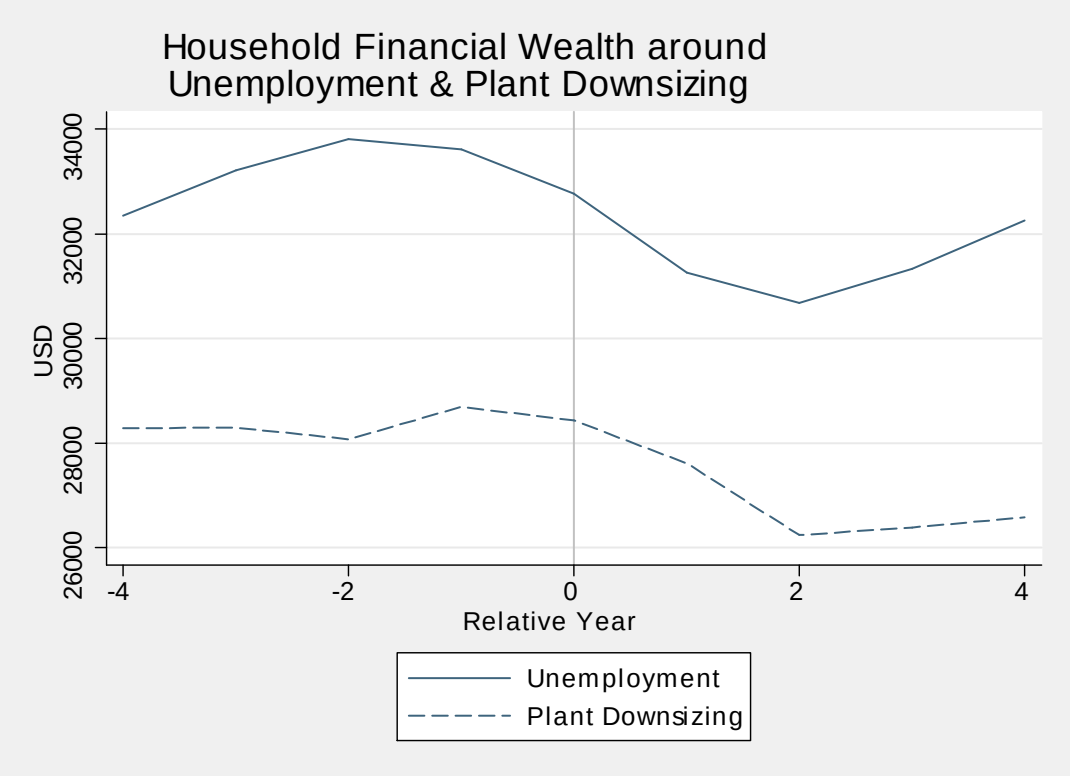


Figure 3.10: Equation 3.1 is estimated using with Household Financial Wealth as LHS variable with an interaction term, indicating whether or not a household was hit by a Plant Downsizing in relation to its Unemployment spell. Of the sample of 5,513 households defined in Section 3.2, 1,075 of the households are in the Plant Downsizing group. Both calendar and relative year dummies are interacted with the Downsizing dummy. For the Unemployment group, the build up in FW prior to job loss is significant at the 10% level, where as the depletion after job loss is at the 1% level (using household fixed effects and clustering at the household level). For the downsizing group the reduction around job loss is significant at the 5% level. The level difference between the two curves is significant at the 1% level.

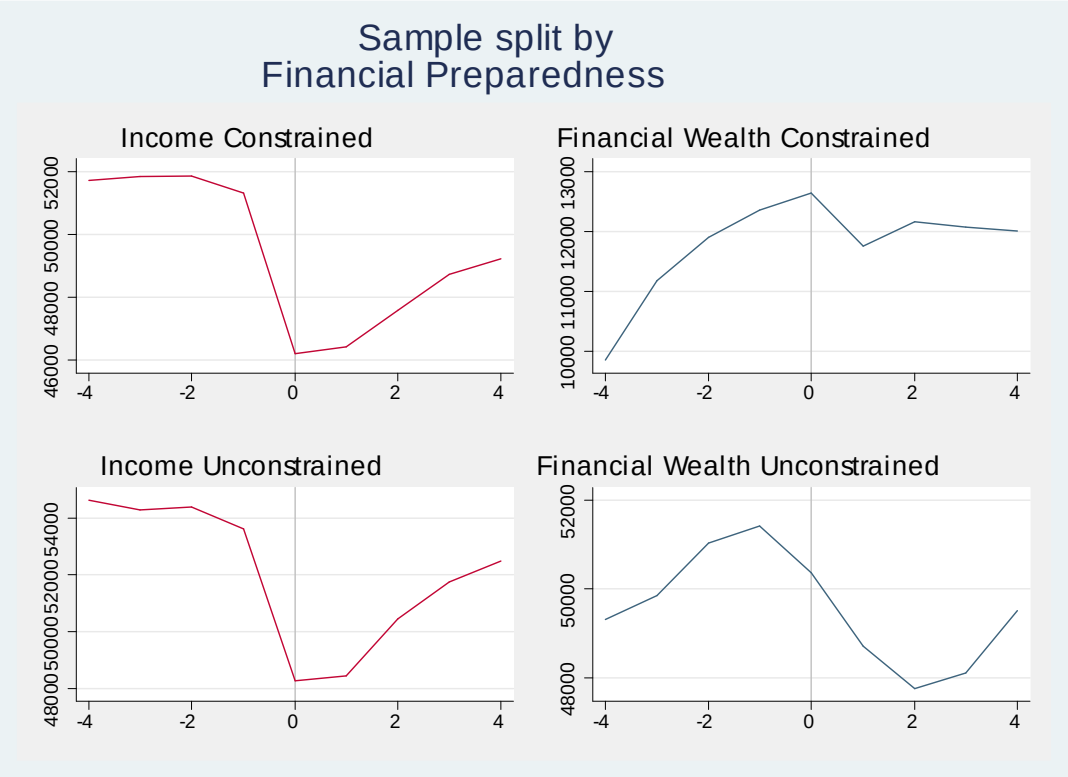
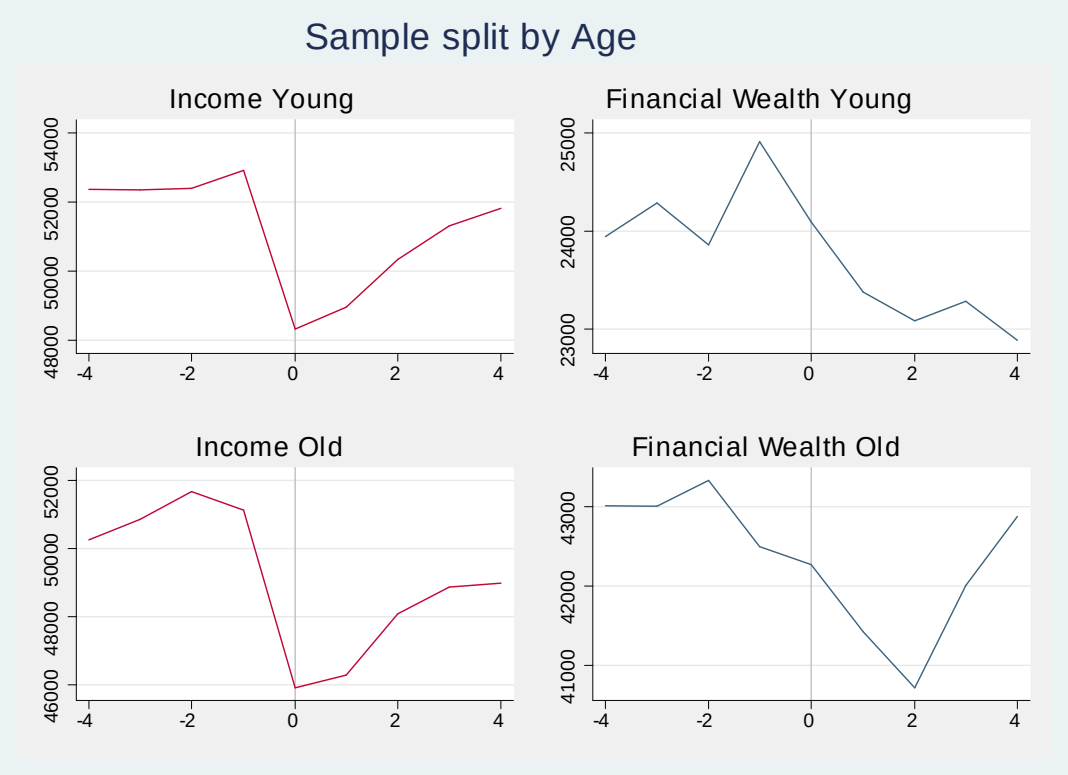
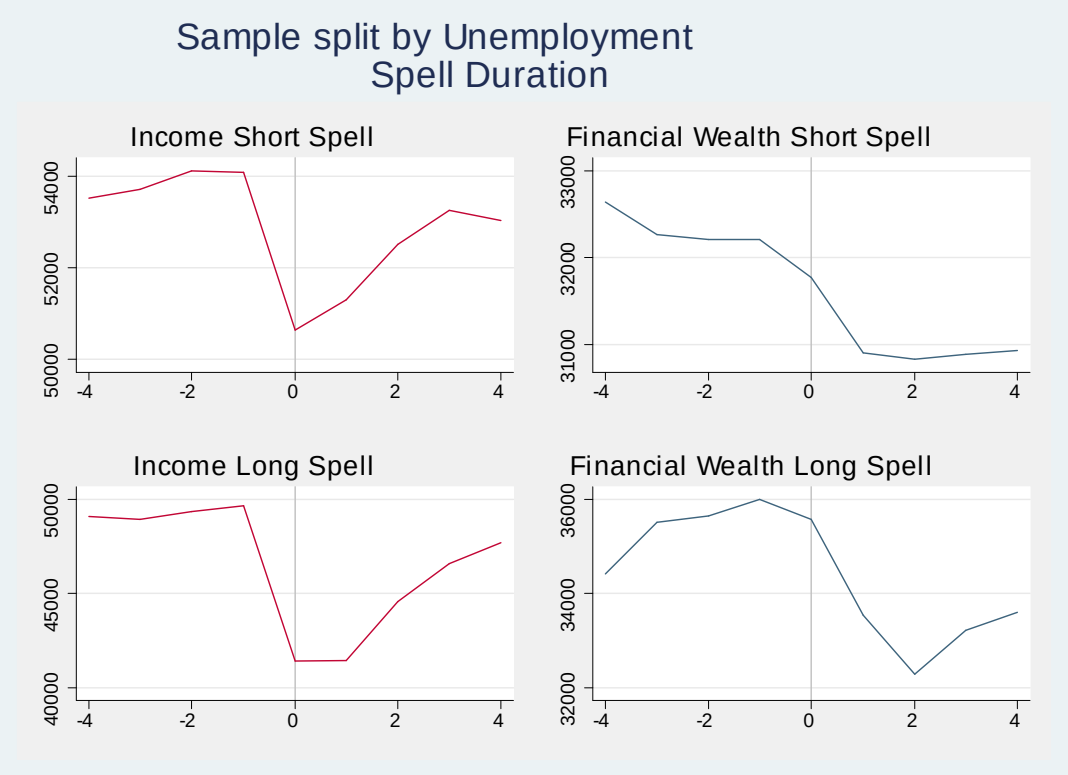


Figure 3.11: Male income (red, left) and household financial wealth (blue, right) for the 2nd (top) vs. the 3rd (bottom) quartile of distribution of financial wealth over annual wages in U-1.



Male Income (left, red) and household financial wealth (right, blue) for the 2nd (top) vs. the 3rd (bottom) quartile of the distribution of age (split at the median age of 45).



Male Income (left, red) and household financial wealth (right, blue) for the 2nd (top) vs. the 3rd (bottom) quartile of the distribution of unemployment spell length.

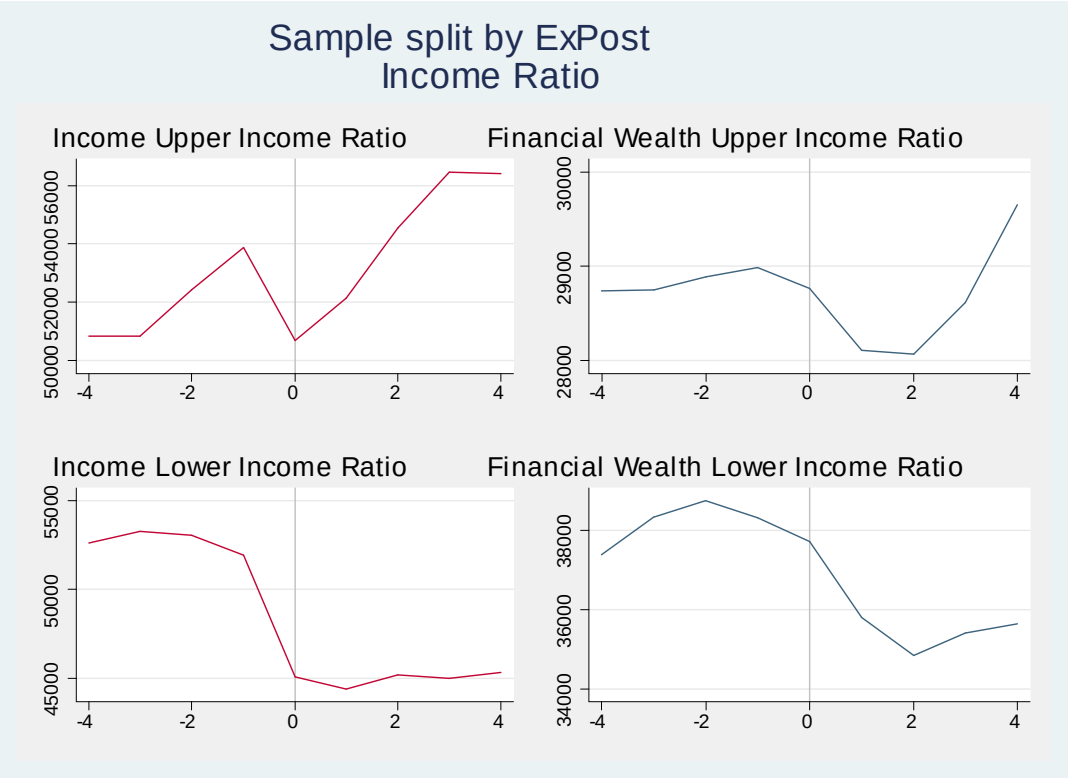


Figure 3.12: Male Income (left, red) and household financial wealth (right, blue) for the 3rd (top) vs. the 2nd (bottom) quartile of the distribution of next (in U+3 and U+4) relative to previous (in U-4 and U-3) income.

Table 3.1: Prior Holdings

Financial Wealth over Weekly Earnings in the Year before Job Loss
Gruber, 2001 US

	P10	P25	P50	P75	P90	N
Employed	0.3	2.39	10.24	38.76	118.6	22,112
Unemployed	0	0.34	5.35	33.73	159.58	15,070
Labor Force Leavers	0	1.89	15.72	92.82	423.3	21,660
Our Norwegian samples						
Always Employed	2	6.21	17.56	46.72	126.77	131,241
Unemployed	0.75	2.85	9.05	25.31	61.97	5,513
Unemployed through DS	1.16	3.39	9.88	24.48	53.98	1,075

Last line: Job losses in the course of plant downsizings, as defined in Section 3.5

Table 3.2: Cross-sectional Analysis

		Male Income	Household Financial Wealth
Pooled	Unemployment	-10,849	-8,331
		(294)***	(736)***
	Constant	58,999	37,472
		(68)***	(170)***
	N	807,785	807,785
1999	Unemployment	-11,143	-8,521
		(566)***	(1,386)***
	Constant	51,058	27,604
		(58)***	(144)***
	N	161,557	161,557
2000	Unemployment	-9,970	-11,046
		(594)***	(1,504)***
	Cons	53,190	31,711
		(63)***	(161)***
	N	161,557	161,557
2001	Unemployment	-11,941	-10,192
		(747)***	(1,808)***
	Constant	56,000	34,118
		(69)***	(167)***
	N	161,557	161,557
2002	Unemployment	-10,142	-6,891
		(712)***	(1,803)***
	Constant	58,992	37,457
		(72)***	(183)***
	N	161,557	161,557
2003	Unemployment	-11,235	-5,544
		(677)***	(1,720)***
	Constant	60,858	41,587
		(76)***	(192)***
	N	161,557	161,557

Table 3.3: Income paths

	Non-DS Unemployed	DS Unemployed
U-4	293.4	78.13
	(249.0)	(479.4)
U-3	-102.2	362
	(336.0)	(785.3)
U-2	174.5	45.15
	(406.5)	(860.8)
U-1	-647.4	949.6
	(488.8)	(898.1)
U	-4245.9	-161
	(462.6)***	(1101.1)
U+1	-4279.8	-551.7
	(486.5)***	(1692.1)
U+2	-2261.9	-638.2
	(503.8)***	(905.5)
U+3	-1608.9	679.5
	(446.2)***	(986.5)
U+4	-849.8	-262.8
	(441.5)	(734.6)
Constant	38251.7	
	(333.0)***	
N	71,669	
* p<0.05	** p<0.01	*** p<0.001

Table 3.4: Household Financial Wealth

	Non-DS Unemployed	DS Unemployed
U-4	2231.6	-4063.7
	(542.3)***	(1004.6)***
U-3	3096.5	-4916.5
	(706.8)***	(1368.8)***
U-2	3693.5	-5734.9
	(807.5)***	(1738.2)***
U-1	3494.1	-4922
	(919.1)***	(2023.3)*
U	2652.1	-4335.5
	(1007.2)**	(2274.6)
U+1	1142.4	-3646
	(1021.9)	(2256.6)
U+2	561.9	-4429.4
	(954.3)	(2148.9)*
U+3	1213.9	-4934.7
	(906.2)	(2052.4)*
U+4	2141.3	-5672.7
	(870.2)*	(1772.4)**
Constant	30118.9	
	(678.1)***	
N	71,669	
* p<0.05	** p<0.01	*** p<0.001

Table 3.5: Summary Statistics to Compare Downsizing against other Job Losers

	Non-Downsizing			Downsizing				
	N	Mean	SE	N	Mean	SE	Difference	T
Age	7280	43.930	0.065	1387	44.237	0.145	-0.307	-1.93
Layoff year	7280	2,000	0	1387	2,000	0	0	-6
Male income	7280	48,326	245	1387	51,771	515	-3,445	-6
Female income	7280	23,864	195	1387	24,362	444	-498	-1
Financial Wealth	7280	24,589	589	1387	21,110	1,007	3,479	3
Home Ownership	7280	0.777	0.005	1387	0.831	0.010	-0.053	-4.77
Spell duration	5563	220	4	1131	175	7	45	5
Education Type								
General	7118	0.211	0.005	1366	0.189	0.011	0.022	1.93
Humanities / Art	7118	0.073	0.003	1366	0.049	0.006	0.024	3.69
Education	7118	0.014	0.001	1366	0.015	0.003	-0.002	-0.45
Social / Legal	7118	0.009	0.001	1366	0.004	0.002	0.005	2.76
Econ / Admin	7118	0.097	0.004	1366	0.078	0.007	0.020	2.45
Engineering	7118	0.539	0.006	1366	0.619	0.013	-0.080	-5.52
Health / Sports	7118	0.012	0.001	1366	0.004	0.002	0.008	3.98
Primary	7118	0.012	0.001	1366	0.010	0.003	0.003	0.97
Transport/Service	7118	0.032	0.002	1366	0.034	0.005	-0.002	-0.39
Education Length								
None	7070	0.004	0.001	1368	0.001	0.001	0.002	1.96
Elementary	7070	0.006	0.001	1368	0.003	0.001	0.003	1.52
Secondary	7070	0.136	0.004	1368	0.135	0.009	0.001	0.12
Upper Secondary	7070	0.277	0.005	1368	0.257	0.012	0.020	1.55
Further Secondary	7070	0.349	0.006	1368	0.420	0.013	-0.071	-4.89
Lower College	7070	0.059	0.003	1368	0.041	0.005	0.018	3.05
Higher College	7070	0.124	0.004	1368	0.110	0.008	0.015	1.59
University	7070	0.042	0.002	1368	0.031	0.005	0.011	2.13
Research	7070	0.003	0.001	1368	0.003	0.001	0.000	0.03

Table 3.6: Comparing the Constrained and Unconstrained

	More Constrained		Less Constrained		
	Mean	SE	Mean	SE	T
Financial Wealth	2,504	36.23	30,669	703.85	-39.96
Male Income	41,505	244.98	40,780	351.01	1.69
House Ownership	0.76	0.01	0.77	0.01	-0.44
Days Unemployed	211.81	4.33	212.70	4.20	-0.15
Spouse Employed	0.74	0.01	0.78	0.01	-5.32

CHAPTER 4

CASH-ON-HAND AND THE DURATION OF JOB SEARCH: QUASI-EXPERIMENTAL EVIDENCE FROM NORWAY

4.1 Introduction

Should the generosity of unemployment insurance be increased, to allow individuals to search longer and find a better-fitting job? Or should it be reduced, so as to decrease the severity of Moral Hazard? These questions continue to ignite heated political debates across OECD economies. Much of the academic literature for a long time seemed to support the latter contention: Papers like Katz and Meyer [1990] or Lalive et al. [2006] had shown that more generous unemployment insurance (UI) did prolong unemployment duration, and the common interpretation was one of pure Moral Hazard: Individuals take longer to find a new job because someone else is paying. However, Chetty [2008] and Card et al. [2007a] ingeniously pointed out that part of the effect of UI on job search duration could be an income rather than a price effect: Individuals would like to choose a longer search duration also if they had to entirely finance it themselves – but they cannot do so because financial markets fail to lend them the necessary liquidity. For a given severity of Moral Hazard, greater liquidity constraints call for higher UI in order to correct that market failure[1]. To investigate the empirical relevance of this consideration for Austria, Card et al. [2007a] estimate the effect on job search duration of severance payments which by definition have no price effect because they are granted lump-sum: They find it to be positive and interpret this as evidence that liquidity effects do indeed matter, with the highly relevant implication that the generosity of the unemployment benefits paid in Austria during the sample period was below its optimal level. The finding is argued to be indicative also for the US, since the two countries share many relevant labor market characteristics, including relatively low UI with a maximum duration (in normal times) of six months and a replacement rate of around 50%.

What however does this imply for most other OECD economies, where prevailing benefit levels and durations are significantly more generous? Does the finding of liquidity constrained

[1] This is under the assumption that the government is not able to cure the market failure more directly by providing the necessary lending.

job losers apply there as well, or is UI there too generous, as many politicians and citizens would suspect? The present paper seeks to make a step in the direction of answering this question by investigating whether a causal effect of lump-sum severance payments on job search duration can be found also in Norway, whose UI generosity is near the upper bound of all OECD economies – with replacement rates of 62.4% and maximum benefit durations of at least two years for most job losers – while at the same time the average household holds comparatively high levels of wealth relative to annual income[2]. If we find an effect of cash-on-hand on job search duration even here, then it seems plausible that similar findings would be made also in many other OECD economies.

To identify the causal effect of lump-sum severance payments, we exploit discontinuities in the amount individuals are eligible for along the age and time dimensions, as agreed upon between the Confederation of Norwegian Enterprise, "Næringslivets Hovedorganisasjon" (NHO), and the Norwegian Confederation of Trade Unions, "Landsorganisasjonen i Norge" (LO). Henceforth we shall refer to these agreements as the "(LO-NHO) scheme". Under these schemes, the amount of severance pay granted, displayed in Table 4.1, becomes non-zero from age 50 onward, after which it increases every two years until age 58 and every single year thereafter until age 62, and then decreases each year until the final potential labor market year at age 66. In addition, within our period of observation, 1993-2008, the amounts for different age groups were discontinuously increased three times. The agreements cover all individuals who are involuntarily separated from one of the agreement's member plants after 10 or more years of tenure. While the variable tenure, which would otherwise also have been interesting for use as the running variable for a Regression Discontinuity Design, is not observed with sufficient precision, we fortunately do observe both layoff date and age precise to the day. This allows us to flexibly control for both of these running variables per se, thus identifying the causal effect of different amounts of severance pay eligibility only off of the discontinuities listed in Table 4.1 and displayed graphically for the age dimension (averaged across the 4 periods) in Figure 4.1. To fully exploit the duration structure of our data, we combine this identification strategy with a Cox Proportional Hazard model.

We find that on average each NOK 1,000 of severance pay lowers the job starting probability on any day within the first 2 years by about 1.4% relative to the baseline group aged just below 50 and who are hence not eligible for any severance pay. For the group aged just above 50 who in the most recent period received NOK 18,000, this implies a 25% lower job finding hazard. Similar results are obtained when we focus on the discontinuity around age

[2]For details on this, see Basten et al. [2011b].

50 alone, and are also confirmed with Censored Normal Regressions using completed job search duration as the dependent variable. The results are robust to many different kinds of control function as well as different ways of censoring the job search duration, and the functioning of our control function is confirmed by a number of suitable placebo regressions.

In a complementary section, we discuss the possibility that part of the reduced-form effect of severance pay, rather than being evidence of liquidity constraints, may result from Mental Accounting behavior: Households might have a higher propensity to consume goods or leisure out of money mentally classified as "income" than out of money classified as "wealth". If they then classify the severance payments – which they receive when regular labor income drops – as income, we may see a causal effect of such payments on job search duration even amongst households who are not formally liquidity-constrained. To empirically test the plausibility of the liquidity interpretation, we interact the severance pay amount with indicators for the different terciles of households' wealth distribution, on a wide range of different wealth measures, and test whether the size of the effect is decreasing in prior wealth. While we do find the effect to be slightly decreasing in a number of wealth measures, the patterns are not statistically significant. Unfortunately we lack the necessary precision to make a conclusive statement on this issue and need to postpone this to future research. In practice though, we argue that the policy implications of Mental Accounting behavior, which can be interpreted as "internal liquidity constraints" are likely the same as those of external Liquidity Constraints.

The remainder of the paper is structured as follows: Section 4.2 explains our Empirical Strategy and Section 4 introduces the data we use to implement it. Section 4.4 presents the general results on the effect of lump-sum severance payments on job search duration. Section 4.5 addresses theoretically and empirically the possibility of Mental Accounting behavior, and Section 4.6 concludes.

4.2 Empirical Strategy

To identify the causal effect of lump-sum severance payments on job-search duration, we exploit agreements first established in 1966 between Norway's Confederation of Trade Unions, "Landsorganisasjonen i Norge" (LO), and the Confederation of Norwegian Enterprise, "Næringslivets Hovedorganisasjon" (NHO) on severance payments ("Sluttvederlag", SLV) to workers losing their jobs. Under these agreements, employees are eligible for severance pay if and only if they are aged above 50 and have at least 10 years of tenure in their current plant or at least 15 years in several member plants. Furthermore, as Table

4.1 shows, the amount changes discontinuously, i.e. from one day to the next, at several other age thresholds as well as across four different time periods. A simple regression of job search duration on the theoretical severance pay amounts would in this setup not allow us to identify the causal effect of severance pay, because for instance in the age range up until age 62 severance pay amounts are increasing in age, but age is likely to be correlated with search duration also through channels other than the severance pay amounts. Furthermore, nominal amounts are higher in later years, so that differences across years in the tightness of the labor market or in the price level could lead our estimates to be biased.

4.2.1 The Control Function

However, since we are able to observe the day of layoff and each individual's age precise to the day, we are able to deal with this issue by controlling flexibly for any other factors correlated with age or time per se, and identify the causal effect of severance pay eligibility off of the discontinuous variation at the various age thresholds. In essence, we estimate the following equation for job search duration $dur_{i,t}$:

$$dur_{i,t} = \alpha + \beta * SP_{i,t}^{assigned} + f(age_{i,t}, time_t) + \varepsilon_{i,t} \tag{4.1}$$

To ensure that our control function does indeed fully take out the effects of any factors correlated with age or time other than the severance payments, we repeat our analyses with a wide range of different control functions for age, and always include also a complete set of calendar year fixed effects. We then test the functioning of our control functions on a placebo sample with similar age and tenure structure but coming from employers that were not participating in the severance pay scheme. As we discuss in more detail in the Section 4.4 below, we do find an "effect of severance payments" in our placebo sample *before* adding the control function, confirming the need for the latter, but not afterwards. We interpret this as suggesting that our control functions are indeed doing a good job.

In particular, we use specifications with six different control functions. Following Card et al. [2007a] and the papers cited therein, we start with a third-order polynomial in age, but for our most basic specification we use only a single third-order polynomial in age for all intervals shown in Table 4.1 and for all four periods with different SP amounts. Next, we expand this to four separate polynomials, one for each of the four periods. In a third step, we replace the third-order polynomial with a Linear Spline (LS) in age: A separate linear function with a separate slope is then estimated for each age interval with a different severance pay amount. In a fourth step, we use instead a Restricted Cubic Spline (RCS),

which allows the slopes to be nonlinear even within each of the intervals. The exact formulas of both kinds of spline are given in the appendix. Finally, in a fifth and sixth step we allow respectively for four separate Linear Splines and for four separate Restricted Cubic Splines, so as to take account of the different severance pay regimes listed in Table 4.1. Table 4.4, which we discuss in further detail in Section 4.4 below, shows that our estimator of interest is relatively robust to which control function we use. Therefore our main discussion of results will focus on the most basic specification, controlling only for the third-order polynomial in age along with the set of calendar year fixed effects.

4.2.2 Cox duration analysis and Censored Normal Regression

We combine the identification strategy discussed above with a Cox Proportional Hazards Model (see Cox [1972]). This is based on the concept of the hazard rate, which for any given period is defined as the number of individuals starting a new job divided by the current number of individuals not holding one. It is intended to reflect the propensity to start a new job in a given period, conditional on not having done so until that point in time. Using this hazard rate, the Cox analysis then estimates the following equation

$$h(t|\mathbf{x}_i) = h_0 \exp(\mathbf{x}_i \boldsymbol{\beta}_x) \tag{4.2}$$

or equivalently in logs

$$ln(h_t|\mathbf{x}_i) = \log h_0 + \beta X \tag{4.3}$$

where h_0 is the baseline hazard, i.e. the propensity to start a new job conditional on not having done so up until now, for some baseline reference group with $x_i = 0$. The key assumption behind this specification is that the effect of covariates is proportional across all time periods (days in our case): $\frac{d \log h_t}{dX} = \beta = \frac{d \log h_s}{dX} \forall s, t$. Our non-parametric Figures 4.7 and 4.8 where this assumption has not been imposed but curves for individuals with different values of the covariates are nonetheless mostly parallel, confirm that this is a valid approximation of reality. At the same time, use of the Cox model has the advantage that we can leave the baseline hazard unspecified. Hence our results will be independent of whether the latter is increasing, decreasing or constant over time, i.e. whether there is positive, negative or no "duration dependence".[3] Combining this with our discontinuity design then requires us to include in the set of regressors X both the assigned amount of severance pay

[3]For an introduction to Cox Proportional Hazard models and alternative methods of survival analysis, see for instance Cleves et al. [2008].

S_i and our control function in age and time per. Doing so then gives an equation of the following form:

$$h(t|\mathbf{x}_i) = h_0 \exp[\beta_T S_i + f(age, time)] \tag{4.4}$$

In addition to our Cox analyses, we also estimate the effect of severance pay on completed job search durations. To take into account that these durations have been censored, an issue we discuss in more detail below, we estimate these equations by Censored Normal Regression instead of the more standard Ordinary Least Squares. In contrast to Ordinary Least Squares estimates, the CNR ones prove relatively robust to how we censor the durations, albeit not as much as the Cox estimates, which confirms our choice of focusing on the latter.

4.2.3 Testing for Manipulation of the Threshold

When seeking to identify a causal effect off of one or several discontinuities, two major challenges do typically arise. The first is potential manipulation of the threshold: In our context, employers or employees might systematically try to make the separation happen just before or just after the employee reaches a new integer age at which the severance pay amount changes, and if that is possible for some individuals then one has to worry that in those cases in which it works individuals may be either more or less keen to return to work than in the cases where it does not work. In considering whether this is an issue for us, a qualitative investigation of the context makes such "gaming" appear relatively unlikely: As severance payments under the LO-NHO scheme are made by the LO-NHO fund rather than the individual employer, and since employers' future dues to that fund are not "experience-rated", i.e. do not depend on the fund's past payouts to the company's ex-employees, the employer has no particular incentive to lay off individuals just before they turn 50, 52, and so on. At the same time, the administration of the fund will work to ensure that employers do not systematically lay off their employees just after the respective thresholds.

This said, we do of course wish to search empirically for any evidence of potential threshold manipulation To do so, Figures 4.2, 4.3 and 4.4 plot the frequency of separations in our final sample (as defined in Section 4.3) by age, using respectively quarterly, monthly and weekly age bins. Figures 4.5 and 4.6 then repeat the same exercise, with respectively monthly and weekly bins, focusing around the age 50 discontinuity only, which we analyze in more detail below. As can be seen, there is no evidence of separations spiking either just before or just after severance payments increase or decrease, confirming the more qualitative picture given above.

4.2.4 Other Discontinuities and Censoring

The second major threat to identification is the possibility that other things also change at the thresholds. In general our research has not detected any such changes at the discontinuities under consideration, except however for two of our three larger discontinuities at respectively ages 60 and 62: Norwegian employees who lose their job at or after age 62 are then eligible to receive unemployment benefits continuously until the official retirement age of 67, rather than just for the usual maximum duration of 2 years. As a consequence, someone losing his job at or after age 60 can first fully exhaust the standard maximum duration of 2 years and then make use of the other rule. A possible way to deal with this is to censor job search durations before those critical thresholds: Then individuals separating from their jobs only after reaching age 60 or 62 will not be part of the analysis at all, and those who separate say N days before but are still without a job when reaching the threshold will only be used to compute the hazard rate, i.e. the number of job finders relative to the number currently without a job, only on his first N days but not thereafter. Table 4.5 shows the results of estimating the most basic specification of our Cox regressions censoring at respectively the formal retirement age of 67 and the potentially problematic ages 62 and 60. It also varies the job search duration after which we censor between 2 years, the maximum duration for which everyone in our sample can receive UI benefits, and 6 months, the maximum duration for which individuals can receive UI in Austria and hence the censoring duration used in Card et al. [2007a]. It turns out that the effect of severance payment in our sample is economically and statistically significant under all possible censoring choices. To be on the safe side however, we henceforth make the most conservative choice and censor already at age 60. The finding that our results do not hinge on other things changing at thresholds 60 and 62 is also confirmed in the results section when we conduct a simpler Regression Discontinuity Design exploiting only the third larger discontinuity, at age 50.[4]

4.3 Data and Measurement

4.3.1 Data Sources and Sample Definition

We use administrative data from Norwegian tax registers that cover the universe of Norwegian taxpayers. Our records on job separations are taken from Norwegian social security registers and we start with all job separations occurring between 1993 and 2008 out of plants that were party to the severance pay agreement drawn up between the labor union, LO, and

[4] In addition to age 60 and a duration of 2 years, search time is naturally censored at the end of our panel, on 31 December 2008.

the employer organization, NHO. For the placebo sample we employ all the same restrictions, but take only individuals from plants that did not participate in the agreement. In both cases, we retain those aged between 45 and 66 at the day of leaving their job (although our main results are only based on individuals aged below 60, see Section 4.2), because eligibility for any severance payment starts only at age 50, and because 67 is the regular retirement age. We drop all those entering early retirement. Early retirement is possibly endogenous and therefore might lead to bias if our results depended on individuals eligible for early retirement, but our choice of censoring durations at age 60 takes care of this. As indicated above, individuals are eligible for severance pay only if the separation is declared to be involuntary, a status which we cannot observe directly. We drop individuals who are observed to start a new job just on the subsequent calendar day. Many of these job "changes" are purely administrative records of individuals who merely switch to a different position within the same firm. Some are changes to a new employer and in principle a subset of these might be cases in which the individual was laid off from the previous job and able to secure a directly following new job already within the 3-month notice period. Such an individual might be eligible for severance pay. However, it seems reasonable to assume that the majority of such cases would take more than one day before starting a new job, and that the set of those starting a new job already the next day will therefore almost entirely be composed of individuals leaving their old job voluntarily precisely because they wish to start a new job. We have however also tried our main regressions on the larger sample still including those who start a new job just the next day, and the results are qualitatively unchanged. Furthermore, for our main sample we drop those registered as being out of their job for precisely one specific calendar *year* and which we think are likely to be data errors, but again the results hold also when we do not impose this rule. Finally, while Card et al. [2007a] drop those returning to their old firm, on the grounds that these are likely to know about this return already at the point of leaving and will thus likely not be truly searching, we retain them in our sample for our main results as we think that the sample of individuals allowed to return will be endogenous, and since those whose firm offers them a subsequent job lose their severance pay eligibility only if this new job starts already within 3 months of separating from the old job. But in the results sections below we will also display the results obtained when excluding returners in addition; we find that the estimated effects become very slightly smaller then. Furthermore, we require individuals to have earned at least the (very low) "minimum amount" (Grunnbeløpet) required for eligibility for at least two years of UI, so that our sample is homogeneously eligible for the same UI. Finally, the results displayed in Tables 4.4 ff. are for males only. In the subsample of females the effect

is also found, but the average effect is much weaker and not statistically significant, and so we focus on the sample of males so as to obtain more precise point estimates, also for our subsequent analyses for which we stratify the sample by tercile of wealth.

4.3.2 Restricting the Sample by measured Tenure

As we have pointed out above, eligibility for LO-NHO severance pay requires a continuous tenure of at least 10 years in the current plant, or a combined tenure of at least 15 years in plants that were all members of the scheme. Hence in order to correctly estimate the size of the severance pay effect, we would like to include in our sample all those who satisfy this requirement and exclude all those who do not. This is complicated somewhat by the fact that we do not observe everyone's past job history for 15 or more years, and that we do not observe which tenure interruptions –such as maternity leave and some specific types of sick leave – were for severance pay eligibility purposes counted as continuation of the previous job. This is also why we cannot implement a Regression Discontinuity Design with tenure as the running variable, as we might otherwise have wanted to do. For our identification strategy pointed out above it means that we are able to identify a subsample of individuals for whom we know that they satisfied the tenure requirement. By contrast, amongst those for whom we do not know this some will and some will not have fulfilled the requirement. To get our point estimates of the effect of severance pay right, we make the conservative choice of using only those individuals for whom tenure is known to be at least 10 years.[5] This restriction shrinks our sample size to about 10% and so the question arises to what extent the sample on which our analyses are based is still representative for the larger population without the tenure restriction. To answer this question, Table 4.2 presents summary statistics first for the full sample and then for the subsample used. We find that our subsample is on average about 9 years older, more educated and has higher income and wealth. A priori, one would expect these factors to lead to lower liquidity constraints, and so it may be expected that our results must be interpreted as lower bounds on the effect that would be found if our quasi-experiment and data allowed us to identify the severance pay effect also for those with lower measured tenure.

4.3.3 Intention-to-Treat vs. Wald Estimators

A last restriction from our data is that, since severance payments are not taxable, we do not observe the amounts actually received, as would be necessary to compute the "Wald"

[5] Using the full sample results in estimates of the severance pay effect that are about half as high as thoe we display here.

estimate of the effect of actual severance pay on job search duration, i.e. to instrument actual with hypothetical payments. Instead, like Card et al. [2007a], we can only estimate the Reduced-Form or Intention-to-Treat effect of severance pay eligibility, which constitutes again a lower bound on the effect of actual severance pay. We can however expect compliance amongst those eligible to be very high, since the claim forms are automatically sent to the LO-NHO joint office by the employer, together with the layoff notification.

4.3.4 Defining the duration of Job Search

The previous literature analyzing the duration of unemployment or job search duration has commonly considered two measures thereof. The first is simply the period for which an individual is officially registered as unemployed, the second is the time between lay-off and the start of a new regular job, and which Card et al. [2007a] denote "non-employment duration". Based on the findings in Card et al. [2007b], they argue for the former, on the grounds that in their Austrian sample many individuals will leave the unemployment register after six months not because they have actually found a job, but only because their benefits expire so that they no longer have an incentive to keep registering at the agency.

In our setting this is less of a problem, because all employees in our sample are eligible for up to two years of benefits. Yet there are good reasons for us to also focus on the latter measure: As Bratsberg et al. [2010] and some of the papers cited therein point out, Norway has four times as many individuals receiving disability insurance as individuals receiving unemployment insurance, and many of the former would be assigned the latter label in other countries. At the same time, Autor and Duggan [2007] show that liquidity effects are as relevant an issue for those on disability insurance as for those on unemployment insurance. Furthermore, the Mental Accounting scenario considered in Section 4.5 also applies also to those on disability insurance. Hence we use as outcome measure the duration from lay-off until the start of a new job, thus capturing both individuals receiving unemployment benefits and those receiving disability benefits. An added benefit of doing so is that it provides us with a larger sample size and hence ceteris paribus allows for greater statistical precision.

4.3.5 Measuring Household Wealth

In view of the previous literature on liquidity constraints of households, the most suitable definition of wealth should be financial wealth – including deposits, bonds, stocks and mutual funds, but not real estate – and measured at the household rather than the individual level. Nonetheless it is conceivable that transaction costs for stocks and bonds are so high that

households use only deposits, or that transaction costs for real estate are so low that they can also use their real estate, or that many married individuals keep their budgets sufficiently separate that individual holdings matter more than a household's total holdings. Fortunately, our dataset is comprehensive enough that we can use total wealth, financial wealth and deposits alone, and each of these both at the individual and at the household level.[6] Of course how long someone can sustain the household with a given amount of savings will depend on the monthly expenditures such as monthly rent, insurance payments etc., which in turn will largely depend on prior income. On these grounds we subsequently repeat the same analyses stratifying the sample by the terciles of different "preparedness" measures, for which we scale the different wealth measures by the average income earned in the last three years before job loss.

4.4 Results

4.4.1 Non- and Semi-Parametric Graphical Analysis

Before implementing the Cox model, which assumes that the treatment effect is the same in each stage of the job search duration, we start with non-parametric Kaplan-Meier survival curves, plotting the fraction of individuals still without a new job against the days elapsed since the separation. Figure 4.7 plots these for individuals eligible for different amounts of severance pay and shows that on any day since the separation those eligible for higher amounts were indeed more likely to still be without a job than those eligible for lower amounts. Now this might of course be entirely due to other factors correlated with age and hence also correlated with the severance pay amounts. To inquire whether this is the case, Figure 4.8 continues with a semi-parametric analysis, plotting the same curves after controlling for our basic control function with a third-order polynomial in age and a complete set of calendar year dummies. To obtain these curves, we have first estimated a Cox model on the control function only, and have then used the resulting baseline hazard to plot Kaplan-Meier curves adjusted for the control function. We can clearly see that the pattern with higher "survival" probabilities for those eligible for higher severance pay persists also after adjusting for the control function. Now one may of course suspect that the control function is simply not comprehensive enough. To test this, we now repeat the same two analyses for our placebo sample (more on which below). The results of doing so, first without and then with control function, are displayed respectively in Figures 4.9 and 4.10. Interestingly we see the qualitatively the same pattern as in our main sample before adding the control function,

[6] Here we add to the holdings of the individual under consideration those of the spouse, if any.

confirming the need for the latter. After adding it however, the survival rates are no longer monotonously increasing in the severance pay amounts, indicating that our control function is working. The more formal analysis provided below will confirm this. Also noteworthy is the fact that the curves are not always parallel, but are so most of the time, thus vindicating our subsequent use of the Proportional Hazards model.

4.4.2 Cox Proportional Hazard Analysis

Table 4.4 shows the results of our main regressions for the different control functions discussed above. We find that for each NOK 1,000 of severance pay the propensity to find a job the next day is between 1.2 and 1.82% lower than for the comparison group of those aged just below 50 and who are therefore not eligible to receive any severance pay. All estimates are statistically significant at the 5% level or lower, except for the one obtained with four separate Linear Splines, which is significant at 6.5%. Focusing on our most basic control function, we obtain a point estimate of 1.44%, which implies a total effect of about 25% for the NOK 18,000 (after-tax) payment available to those individuals aged just above 50 in the most recent period.

To confirm that this coefficient is not just reflecting other, correlated factors not taken out by our control function, we repeat our analysis on the placebo sample. This has been obtained by imposing all the same restrictions as for our main sample, except that we now take only those plants that were not part of the LO-NHO scheme. Table 4.3 shows summary statistics for the two samples, which reveal that the individuals in the placebo sample have practically the same age, but tend to be somewhat more educated. Income and wealth differences however do are not significantly different economically. When we repeat the Cox regressions for this sample, displayed in Table 4.6, we find either a positive (instead of negative, as in the main sample), or statistically insignificant effect for all control functions, confirming the validity of our estimation strategy.

4.4.3 Focusing on the Discontinuity around Age 50

In addition to our main sample, which now focuses on individuals aged between 45 and 60, we have also conducted a more standard Regression Discontinuity analysis focusing only on those aged between 48 and 52, where the two-year bandwidth has been chosen with a view to the fact that, as discussed above, the severance pay amount increases again from age 52 onwards. The results for the resulting subsample are displayed in Table 4.9: Column (1), based on regressions without control function, implies an effect of 25%, which corresponds

to what we have computed on the basis of the full sample above. When we add year fixed effects and respectively one 3rd-order polynomial in age for all 4 years or 2 separate ones for respectively those aged below and those aged above 50 on the day of job separation, this estimate increases to 46%, as displayed in columns (2) and (3). Columns (4) through (6) then repeat the same three analyses in a placebo setup in which we use individuals aged between 48 and 50, who are hence all ineligible for any severance pay, and estimate the effect of being aged 49 or higher. None of the three specifications finds an effect that is statistically significant at any reasonable level. Finally, columns (7) through (9) repeat again the same analysis now for a sample with the same age restrictions as columns (1) through (3), but now again for the placebo sample of individuals separating from non-LONHO plants. Again, no significant effect is found here, confirming the validity of the findings in our main sample.

4.4.4 Analyses of Completed Durations

Table 4.7 presents the results of our Censored Normal Regressions with completed job search duration as the dependent variable. For comparability, sample and control functions are exactly the same as in the Cox regressions reported in Table 4.4 above. Depending on the control function used, we find an effect of between 4 and 5.6 days. The point estimate obtained with our most basic specification, 4.2, implies a total effect of 75 days or about 2.5 months for the NOK 18,000 payment received at age 50. Table 4.8 explores how robust these findings are to how we censor the job search duration and shows that durations are still fairly robust to the maximum age but not to whether we censor after 6 months or 2 years of completed duration, with estimates obtained with early censoring amounting to only 1.5-1.8 instead of 4.0-5.6 days. That suggests that the point estimates obtained with Cox regressions are more reliable, even if the direction and statistical significance of the effect remain robust also here.

4.5 Mental Accounting instead of Liquidity Constraints?

In the previous sections we have shown that the causal effect of lump-sum severance payments on job search duration which Card et al. [2007a] found for Austria is robustly present also in Norway, making it plausible that the finding applies also to other OECD economies. To this point we have simply adopted their interpretation of this reduced-form effect as evidence of liquidity constraints, but in this complementary section we shall discuss also a possible alternative interpretation based on Mental Accounting.

Since this alternative hypothesis is an adaptation to the context of job loss and severance

payments of the ideas advanced in Shefrin and Thaler [1988], we start by summarizing the latter. At the core of that paper is the idea that individuals behave *as if* there coexisted two selves: A myopic "doer self" that is always concerned only with the current period, and a "planner self" concerned with maximizing a function of lifetime doer utilities. If the choices of consumption each period were left to the doer self, too much would be consumed in early periods, leading to a sub-optimal lifetime path of consumption. Restricting current consumption to a level below what is available in any given period however costs willpower. To address this problem, the "planner self" is then assumed to place constraints on future consumption choices already in advance, either through external commitment devices like pension plans or internal ones like rules-of-thumb.

One such rule is Mental Accounting: Rather than considering all money as fungible, households mentally assign all funds to different "mental accounts". The simplest version contains one account for "Current Income" (C), one for "Current Assets" (A) and one for "Future Income" (F). The rule-of-thumb then has the Marginal Propensity to Consume (MPC) – the fraction of each additional dollar consumed right away – be highest for money classified as C, lower for A, and lowest for F. In practice, households are likely to have more than just those three accounts, and different households will have different accounts, for their kids' education or other purposes. Furthermore, exactly which consumption choices this classification results in will depend on the exact "framing", i.e. on which categories each account is defined to include and over which horizon each account is to be balanced. This categorization into three main accounts however is thought to be a good first approximation for the average household.

Building on this categorization, we suggest that if households do indeed classify money into I, A or F and are more willing to consume out of I than out of A or F, then the consumption of job-search time is likely to respond to severance pay even for households who are not formally, or externally, liquidity-constrained, because the severance pay, in contrast to prior savings, is likely to be classified as "Current Income", seeing that it is paid out after lay-off precisely when regular income drops to the UI replacement rate. Put differently, households may implicitly understand the severance payments as specifically intended to be used for maintaining consumption while searching for an adequate new job, whereas prior savings are instead understood to be reserved for different purposes such as retirement or children's education. In the words of Shefrin and Thaler [1988]: "households treat components of their wealth as nonfungible, even in the absence of credit rationing." It is worth noting that conceptually Mental Accounting is in fact quite similar to standard

liquidity constraints in that in both cases households would have the necessary (lifetime) wealth to increase spending now, yet cannot do so because the wealth is not available at that specific point in time or for that specific purpose. The difference is firstly that Mental Accounting arises through constraints that are internal rather than external, and secondly that – given the individual's temptation to spend excessively absent any commitment devices – the internal constraints are optimal as a second-best solution.[7]

Just like the problem of liquidity constraints has a first-best policy solution, lending, and a possible second-best, increasing UI, there are several possible policy responses to the phenomenon of Mental Accounting. The first-best, given individuals' risk of myopia, would be to provide an external commitment device that still constrains myopic spending but does a better job at allowing higher spending if and only if that can be expected to increase lifetime utility, for instance by allowing the individual to find a financially or otherwise better subsequent job. But if, plausibly, such a policy is not possible, we are back with the same policy options as for Liquidity Constraints. So the policy responses to the finding of severance pay effects are overall similar, nonetheless a test of whether this effect stems mostly from Liquidity Constraints or mostly from Mental Accounting can improve our understanding of what exactly is happening within the affected households.

To do so, we have stratified our sample by terciles of the wealth distribution, where the terciles are defined separately within each calendar year. We stratified in turn by last year's income, total wealth, financial wealth (total wealth net of real estate holdings), and deposits, always first at the level of the husband only and then at the level of the total household, adding the assets of the wife, if any. All measures have first been winsorized at the 99th percentile to take care of outliers. Table 4.10 shows a summary of the wealth measures by their respective terciles. Then we re-estimate our previous Cox regressions, but interact the severance pay variable with indicators for being in the second or third terciles, and also control for the main effect of being in those terciles. The interaction coefficient can then be read to inform us whether the size of the severance pay effect varies across the three terciles. The results of these analyses are displayed in Tables 4.11 and .4.12. We see that the size of the severance pay effect is monotonously decreasing in wealth, household wealth, household financial wealth, but not in individual financial wealth, deposits, household deposits or any

[7] There is also a third possible interpretation of a reduced-form effect of severance pay on job-search duration: If the payment makes households richer, and risk aversion is decreasing in wealth, then it could make the recipients more willing to reject a merely moderately good early offer in order to wait for the next one, which might either be better or worse than the one received early on. In our context, where the payment amounts only to 1 month's wages for the typical household, an effect through lifetime wealth seems less likely to be the correct interpretation though.

of these measures scaled by previous income. Also, none of the interaction coefficients is statistically significant at the conventional levels, except for that for scaled household financial wealth, where however the pattern is not monotonous. Overall, the findings do if anything provide some slight support for the Liquidity Constraints interpretation, but no conclusive conclusions on the issue are possible. The same finding is reached when we stratify into halves or quartiles instead of terciles, or when we interact our severance pay variable with continuous measures of wealth. Hence, while we have robustly shown an effect of lump-sum severance payments, further research is needed on which of the interpretations discussed here is the most appropriate.

4.6 Conclusion

We have shown a clear causal effect of lump-sum severance payments on the duration of job search in Norway. To our knowledge, this is only the second paper in the literature to find such an effect, and the first to find it in a Scandinavian-type welfare state. This makes it likely that such effects hold also in other OECD economies. We have then discussed how this can be interpreted either as evidence of liquidity constraints, as in the previous literature, or alternatively as evidence of Mental Accounting behavior. No definitive conclusion could be reached on the which of these is the more appropriate interpretation, so that this issue will require further research in the future. Nonetheless, we can infer that slightly more generous unemployment benefits are likely to solve the problems faced by the households affected, although it may not be the first-best policy response.

4.7 Appendix I: Figures and Tables

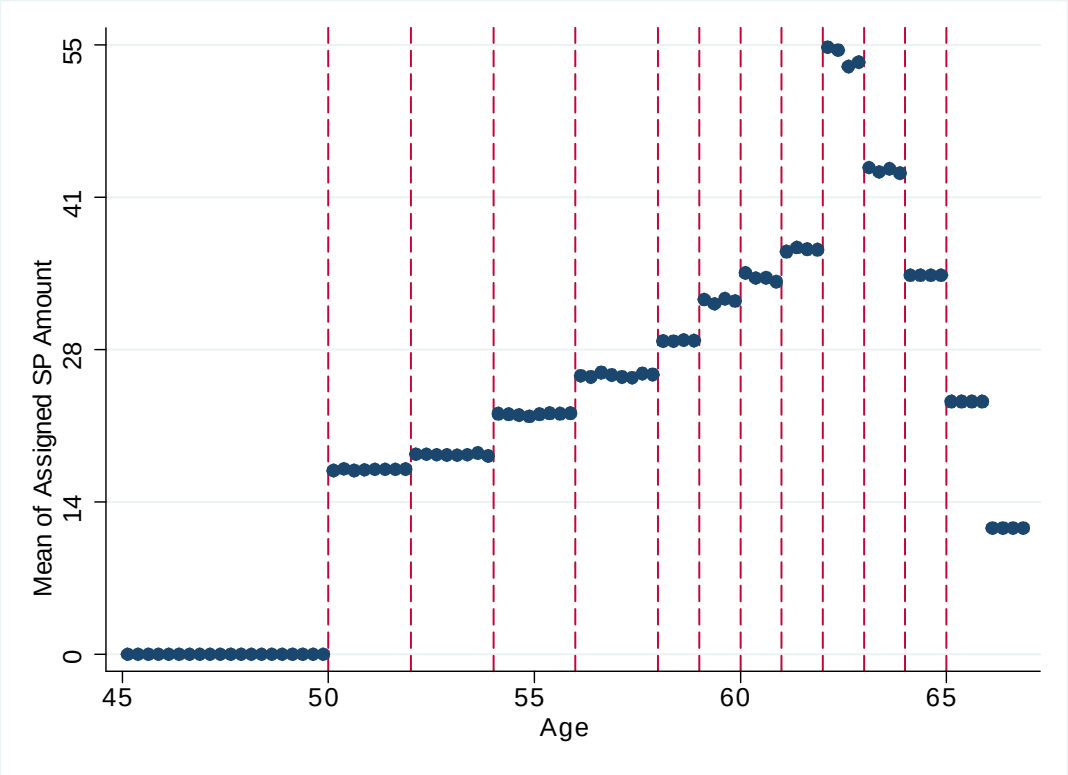


Figure 4.1: Average (across the 4 periods) amounts individuals of different ages were eligible for, following Table 4.1, in NOK 1,000.

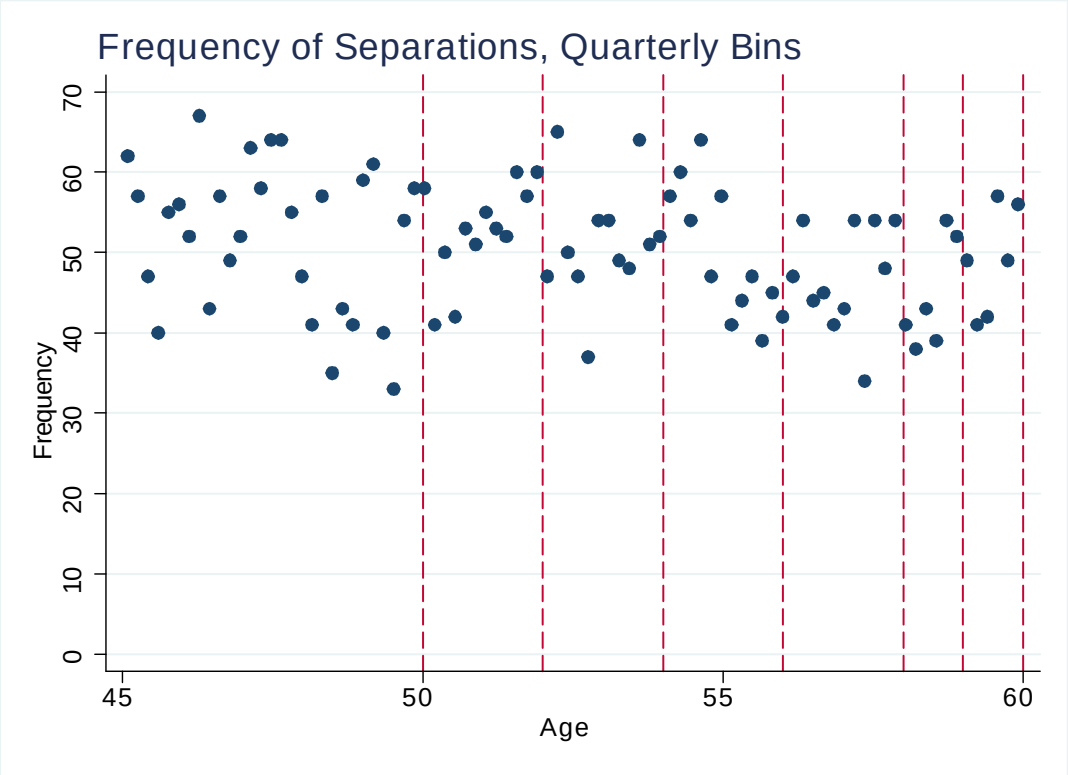


Figure 4.2: Frequency of job separations in the sample used for our main analyses, male and with at least 10 years of tenure. Bin size 3 months.

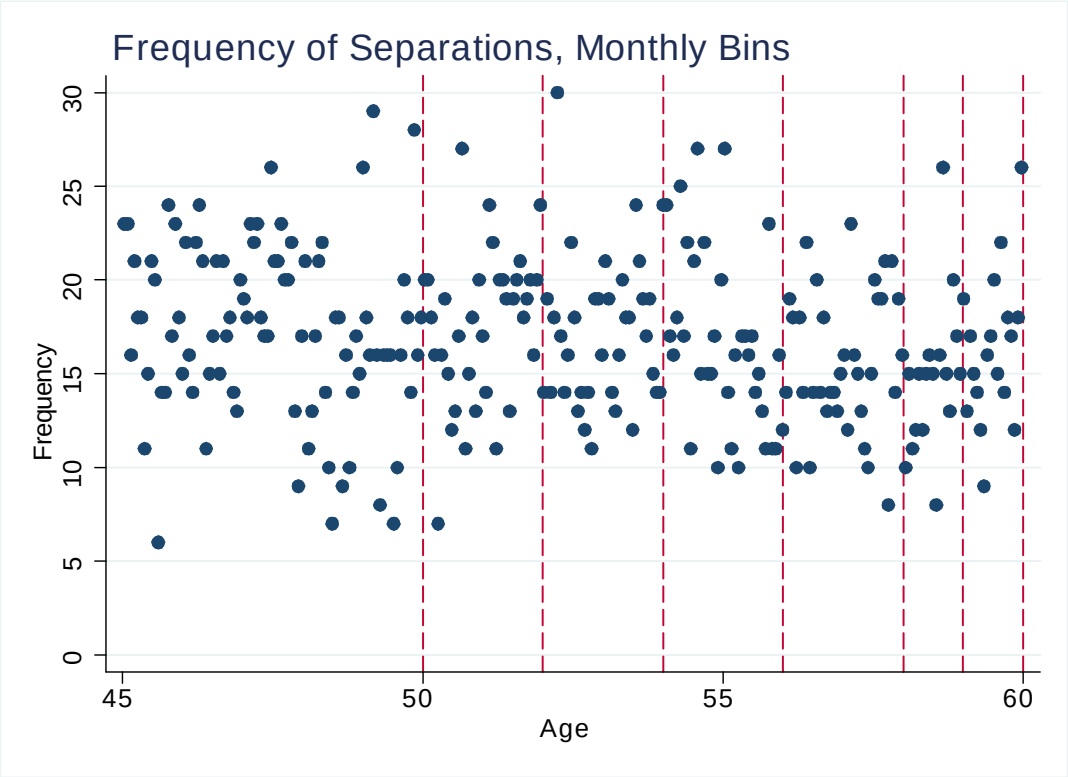


Figure 4.3: Frequency of job separations in the sample used for our main analyses, male and with at least 10 years of tenure. Bin size 1 month.

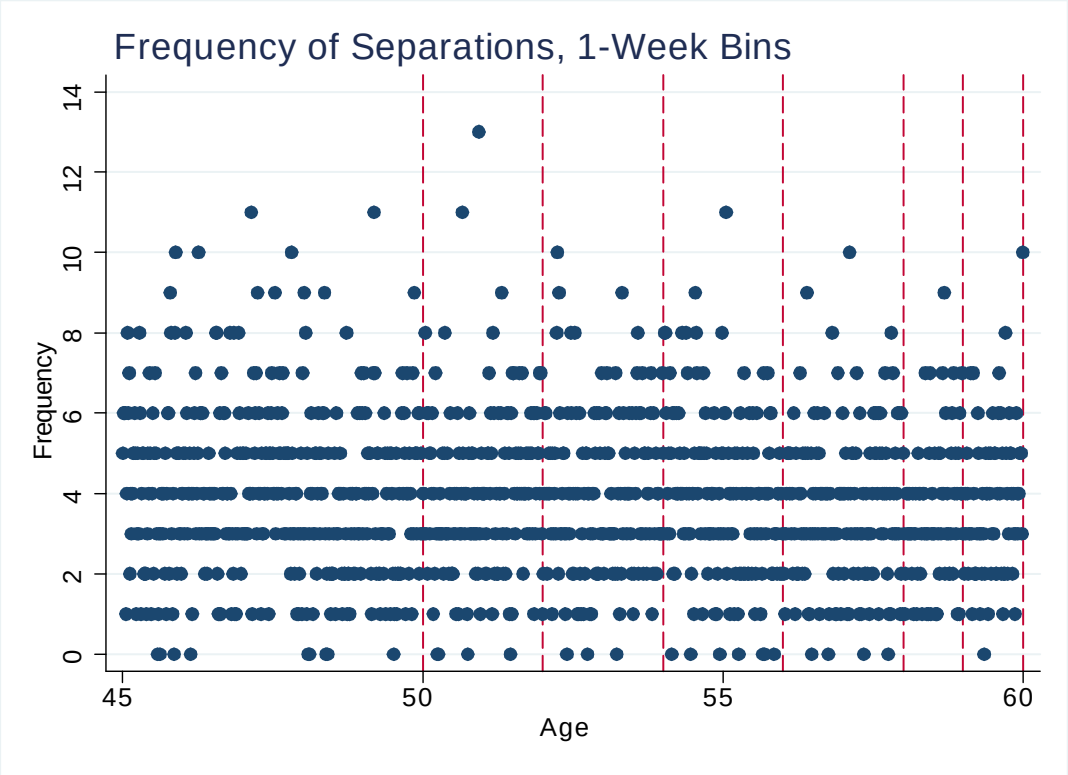


Figure 4.4: Frequency of job separations in the sample used for our main analyses, male and with at least 10 years of tenure. Bin size 1 week.

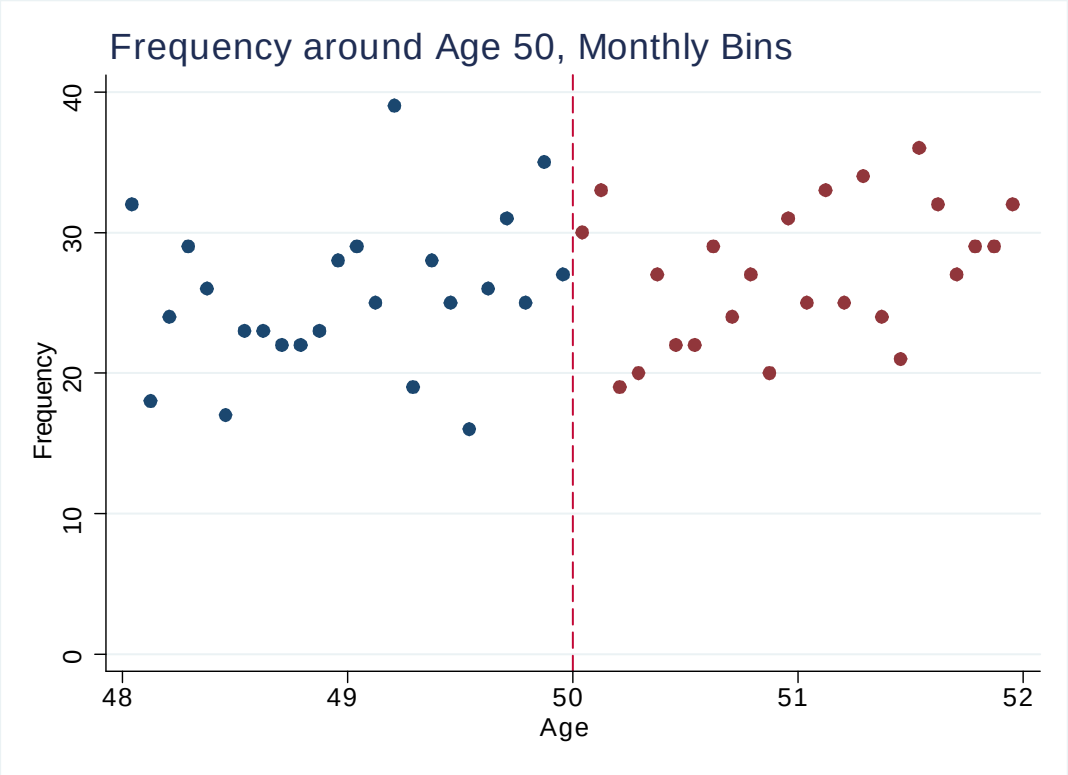


Figure 4.5: Frequency of job separations in the sample used for our complementary RD analysis around the age 50 threshold. Bin size 1 month.

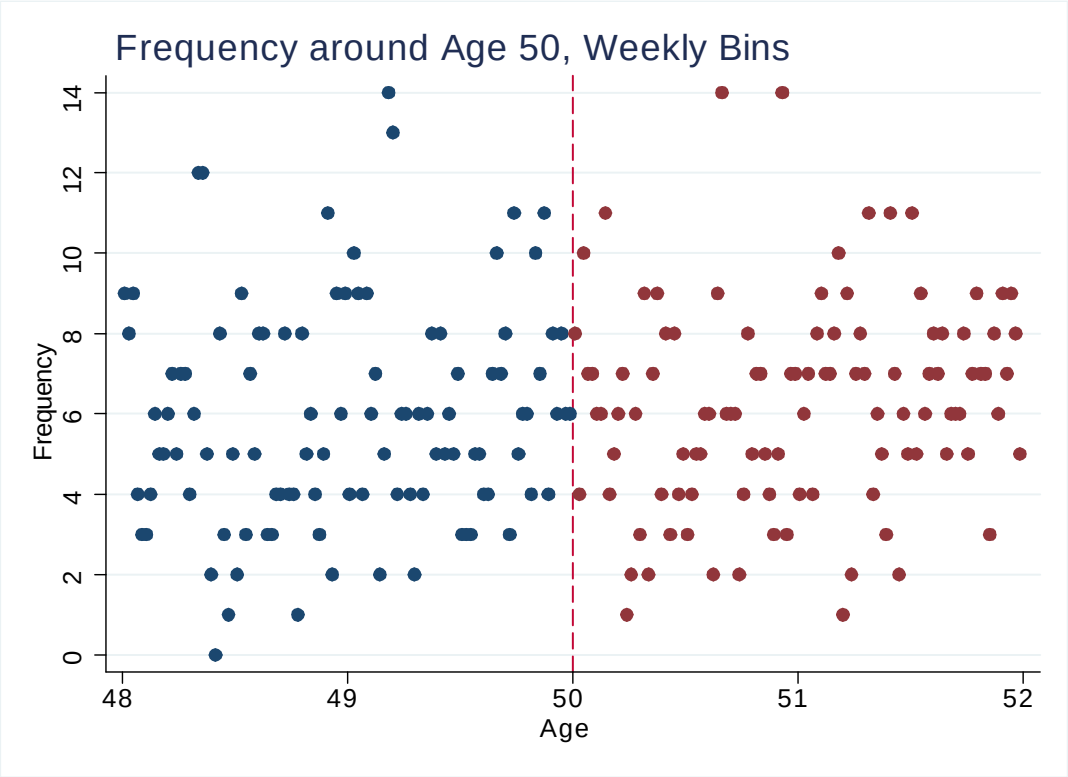


Figure 4.6: Frequency of job separations in the sample used for our complementary RD analysis around the age 50 threshold. Bin size 1 week.

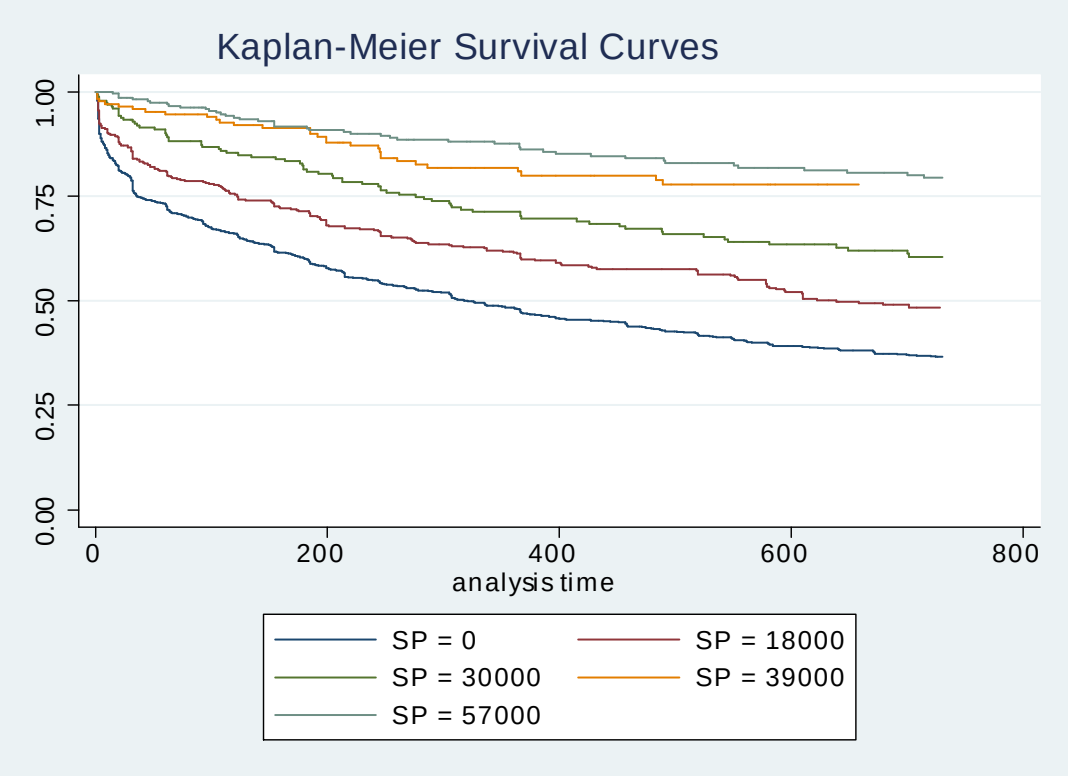


Figure 4.7: Nonparametric Kaplan-Meier Survival Curves, displaying for each day the fraction of individuals still without a job. Different curves for individuals eligible for different amounts of severance pay in NOK.

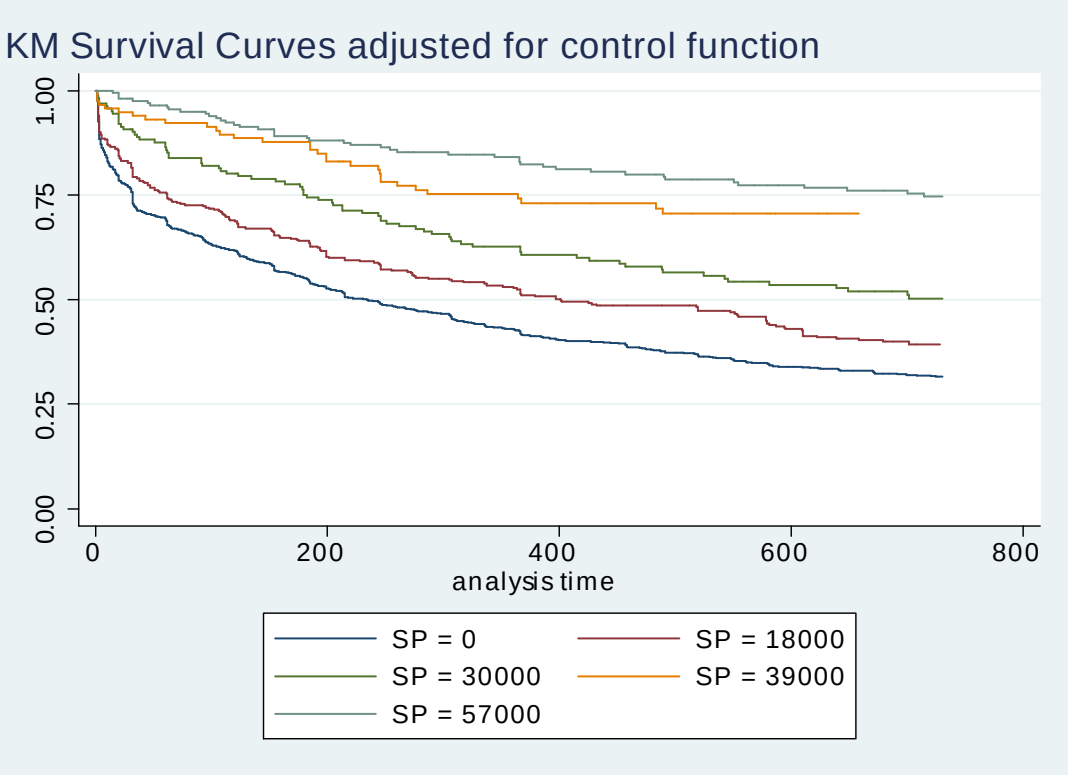


Figure 4.8: We estimated a Cox Model on a third polynomial in the deviation of age from its sample average of 52 and a set of calendar year fixed effects, with year 2000 as omitted category; Using the baseline hazard from this estimation, we then plot the non-parametric Kaplan-Meier Survival Curves.

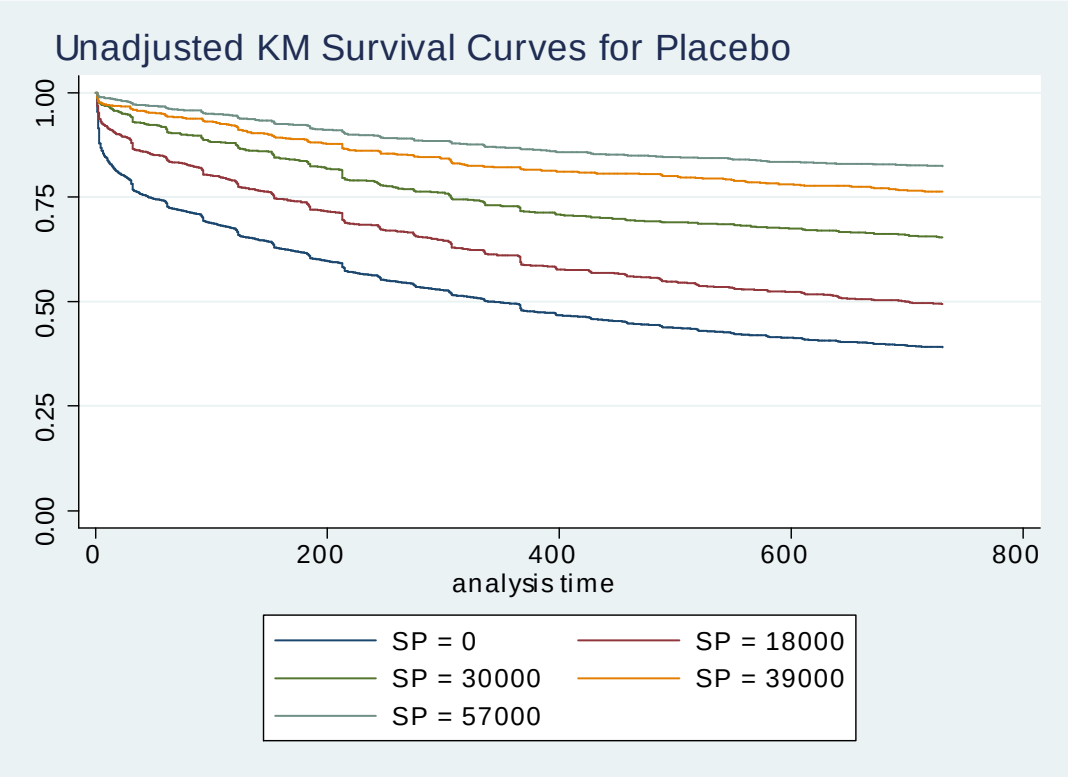


Figure 4.9: Non-parametric Kaplan-Meier Curves, as in Figure 4.7, but now for the placebo sample of individuals separated from plants that were not part of the severance pay agreement.

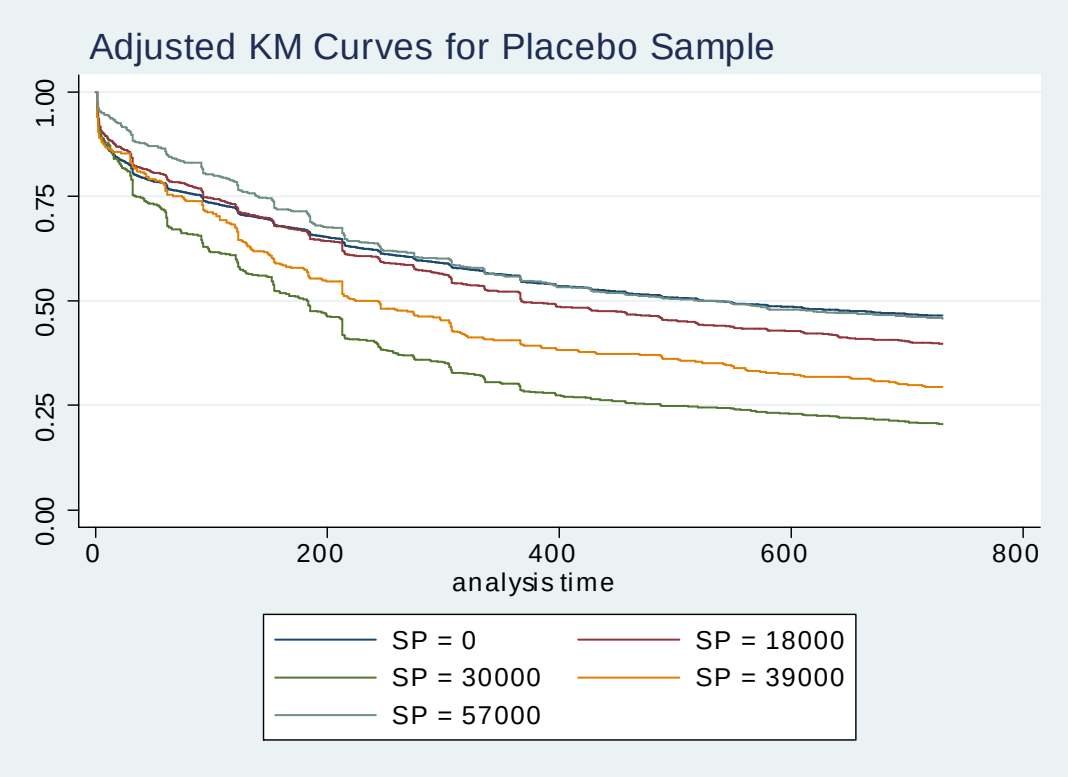


Figure 4.10: Semi-parametric Survival Curves, as in Figure 4.8, but now for the placebo sample of individuals separated from plants that were not part of the severance pay agreement.

Table 4.1: Severance Pay Amounts by Age and Period in NOK

Age	01oct1993-30sep1995	1oct1995-28feb1998	01mar1998-31jul2002	01aug2002 ff
<= 49	0	0	0	0
50	12,000	14,400	14,400	18,000
51	12,000	14,400	14,400	18,000
52	13,000	15,600	15,600	19,500
53	13,000	15,600	15,600	19,500
54	15,500	18,600	18,600	23,300
55	15,500	18,600	18,600	23,300
56	18,000	21,500	21,500	26,900
57	18,000	21,500	21,500	26,900
58	20,000	24,000	24,000	30,000
59	22,500	27,000	27,000	33,800
60	24,000	28,800	28,800	36,000
61	26,000	31,200	31,200	39,000
62	28,500	34,200	57,000	57,000
63	28,500	34,200	45,600	45,600
64	34,200	34,200	34,200	34,200
65	22,800	22,800	22,800	22,800
66	11,400	11,400	11,400	11,400

Year 1997	34,111	0.036	0.185	0	1	4,362	0.018	0.133	0	1
Year 1998	34,111	0.045	0.207	0	1	4,362	0.027	0.163	0	1
Year 1999	34,111	0.051	0.220	0	1	4,362	0.027	0.163	0	1
Year 2000	34,111	0.061	0.238	0	1	4,362	0.038	0.190	0	1
Year 2001	34,111	0.062	0.241	0	1	4,362	0.039	0.193	0	1
Year 2002	34,111	0.086	0.280	0	1	4,362	0.153	0.360	0	1
Year 2003	34,111	0.088	0.284	0	1	4,362	0.091	0.287	0	1
Year 2004	34,111	0.090	0.287	0	1	4,362	0.089	0.284	0	1
Year 2005	34,111	0.093	0.291	0	1	4,362	0.096	0.294	0	1
Year 2006	34,111	0.107	0.309	0	1	4,362	0.124	0.330	0	1
Year 2007	34,111	0.121	0.326	0	1	4,362	0.167	0.373	0	1
Year 2008	34,111	0.096	0.295	0	1	4,362	0.112	0.315	0	1
Tenure	34,111	4.660	5.424	0	36.496	4,362	15.863	5.796	10	36.496
Mean Inc	32,880	344,840	179,921	15,700	1,105,624	4,324	393,299	176,224	61,100	1,105,624
Inc	32,880	362,933	191,269	40,500	1,188,900	4,324	404,975	187,176	56,400	1,188,900
HH Inc	32,880	479,832	265,727	40,500	1,554,400	4,324	543,098	261,984	56,400	1,554,400
Wealth	32,880	480,186	634,460	0	4,073,107	4,324	662,732	805,354	0	4,073,107
HH Wealth	32,880	614,124	807,661	0	5,794,015	4,324	841,997	1,044,172	0	5,794,015
FinW	32,880	169,711	398,766	0	2,666,576	4,324	264,024	551,158	0	2,666,576
HH FinW	32,880	232,073	531,822	0	4,018,066	4,324	361,737	751,264	0	4,018,066
Deposits	32,880	104,945	225,877	0	1,448,589	4,324	145,241	276,454	0	1,448,589
HH Deposits	32,880	145,921	289,374	0	1,959,351	4,324	201,553	355,026	0	1,959,351
Edu: General	30,953	0.361	0.480	0	1	3,998	0.335	0.472	0	1
Edu: Humanities	30,953	0.016	0.125	0	1	3,998	0.012	0.108	0	1
Edu: Teaching	30,953	0.013	0.112	0	1	3,998	0.012	0.109	0	1
Edu: Social-Legal	30,953	0.009	0.094	0	1	3,998	0.010	0.101	0	1
Edu: Econ/Adm	30,953	0.086	0.281	0	1	3,998	0.103	0.304	0	1
Edu: Science/Eng	30,953	0.423	0.494	0	1	3,998	0.444	0.497	0	1
Edu: Health/Sports	30,953	0.007	0.085	0	1	3,998	0.006	0.076	0	1
Edu: Primary	30,953	0.033	0.180	0	1	3,998	0.032	0.175	0	1
Edu: Services	30,953	0.051	0.221	0	1	3,998	0.046	0.210	0	1
Edu: None	31,195	0.002	0.049	0	1	4,027	0.003	0.057	0	1
Edu: Primary	31,195	0.002	0.047	0	1	4,027	0.002	0.042	0	1
Edu: Middle School	31,195	0.270	0.444	0	1	4,027	0.231	0.422	0	1

Year 1997	4,362	0.018	0.133	0	1	31,864	0.056	0.229	0	1	
Year 1998	4,362	0.027	0.163	0	1	31,864	0.041	0.198	0	1	
Year 1999	4,362	0.027	0.163	0	1	31,864	0.046	0.210	0	1	
Year 2000	4,362	0.038	0.190	0	1	31,864	0.048	0.214	0	1	
Year 2001	4,362	0.039	0.193	0	1	31,864	0.054	0.225	0	1	
Year 2002	4,362	0.153	0.360	0	1	31,864	0.116	0.320	0	1	
Year 2003	4,362	0.091	0.287	0	1	31,864	0.093	0.291	0	1	
Year 2004	4,362	0.089	0.284	0	1	31,864	0.096	0.295	0	1	
Year 2005	4,362	0.096	0.294	0	1	31,864	0.081	0.272	0	1	
Year 2006	4,362	0.124	0.330	0	1	31,864	0.095	0.293	0	1	
Year 2007	4,362	0.167	0.373	0	1	31,864	0.119	0.324	0	1	
Year 2008	4,362	0.112	0.315	0	1	31,864	0.080	0.271	0	1	
Tenure	4,362	15.863	5.796	10	36.496	31,864	16.986	6.137	10	40.80835	-11.91
Mean Inc	4,324	393,299	176,224	61,100	1,105,624	30,778	390,163	184,722	20,000	1,024,734	1.09
Inc	4,324	404,975	187,176	56,400	1,188,900	30,778	401,644	195,349	40,700	1,082,606	1.09
HH Inc	4,324	543,098	261,984	56,400	1,554,400	30,778	543,985	279,042	40,700	1,603,606	-0.21
Wealth	4,324	662,732	805,354	0	4,073,107	30,778	684,249	808,408	0	4,145,446	-1.64
HH Wealth	4,324	841,997	1,044,172	0	5,794,015	30,778	895,870	1,152,863	0	7,082,675	-3.13
FinW	4,324	264,024	551,158	0	2,666,576	30,778	287,524	563,482	0	2,800,465	-2.62
HH FinW	4,324	361,737	751,264	0	4,018,066	30,778	402,139	812,366	0	4,876,388	-3.28
Deposits	4,324	145,241	276,454	0	1,448,589	30,778	154,045	285,149	0	1,451,702	-1.95
HH Deposits	4,324	201,553	355,026	0	1,959,351	30,778	217,435	376,121	0	2,108,490	-2.73
Edu: General	3,998	0.335	0.472	0	1	28,240	0.291	0.454	0	1	
Edu: Humanities	3,998	0.012	0.108	0	1	28,240	0.033	0.179	0	1	
Edu: Teaching	3,998	0.012	0.109	0	1	28,240	0.047	0.211	0	1	
Edu: Social-Legal	3,998	0.010	0.101	0	1	28,240	0.019	0.137	0	1	
Edu: Econ/Adm	3,998	0.103	0.304	0	1	28,240	0.116	0.320	0	1	
Edu: Science/Eng	3,998	0.444	0.497	0	1	28,240	0.365	0.481	0	1	
Edu: Health/Sports	3,998	0.006	0.076	0	1	28,240	0.028	0.166	0	1	
Edu: Primary	3,998	0.032	0.175	0	1	28,240	0.019	0.136	0	1	
Edu: Services	3,998	0.046	0.210	0	1	28,240	0.083	0.275	0	1	
Edu: None	4,027	0.003	0.057	0	1	28,401	0.001	0.038	0	1	
Edu: Primary	4,027	0.002	0.042	0	1	28,401	0.002	0.041	0	1	
Edu: Middle School	4,027	0.231	0.422	0	1	28,401	0.182	0.386	0	1	

Table 4.4: Cox Proportional Hazards Duration Model

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1 Polyn.	No Return	By Age	4 Polyn.	LS	RCS	4 LS	4 RCS
SP	-1.44	-1.3	-1.3	-1.82	-1.44	-1.25	-1.41	-1.2
	(0.018)**	(0.041)**	(0.245)	(0.004)***	(0.041)**	(0.031)**	(0.065)*	(0.049)
Age	-485.3	-397.6	-781.7					
	(0.142)	(0.258)	(0.488)					
Age2	9.63	7.96	15.9					
	(0.131)	(0.241)	-(0.477)					
Age3	-0.0636	-0.0532	-0.108					
	(0.119)	(0.221)	(0.465)					
SP*Aged			0.181					
			(0.524)					
SP*Aged2			0.034					
			(0.719)					
SP*Aged3			-0.00206					
			(0.789)					
N	4,362	4,078	4,362	4,362	4,362	4,362	4,362	4,362

Outcome: Effect of each NOK1,000 of SP on propensity to start new job in 24 months after job loss, in %

All columns control for year effects; (4) has a separate polynomial in age for each of the 4 period displayed in Table 4.

(5) - (8) control for a Linear Spline, a Restricted Cubic Spline, 4 Linear Splines, 4 RCS (for details, see text).

P-values in parentheses, based on robust and person-clustered SEs; *P<0.10, **P<0.05, ***P<0.01

Table 4.5: Baseline regression, censoring at different ages and durations

	67		62		60	
	2y	6m	2y	6m	2y	6m
SP	-1.03	-1.39	-1.22	-1.43	-1.44	-1.57
	(0.015)**	(0.012)**	(0.033)**	(0.046)**	(0.018)**	(0.038)**
Age	-209.8	-207.3	-419.6	-496.9	-485.3	-497.6
	(0.117)	(0.229)	(0.064)*	(0.073)*	(0.142)	(0.209)
Age2	4.23	4.2	8.3	9.77	9.63	9.83
	(0.089)*	(0.192)	(0.054)*	(0.064)*	(0.131)	(0.198)
Age3	-0.0287	-0.0285	-0.0548	-0.0641	-0.0636	-0.0647
	(0.062)*	(0.150)	(0.044)**	(0.054)*	(0.119)	(0.185)
N	5,452	5,452	4,876	4,876	4,362	4,362

Estimation of baseline Equation 4.4; Duration censored at ages 67/62/60 and after 2 or 0.5 years as indicated
Outcome: Effect of each NOK 1,000 of SP on propensity to start new job in %; All columns control
for year FEs; P-values in parentheses, based on robust and person-clustered SEs; *P<0.10, **P<0.05, ***P<0.01

Table 4.6: Cox Proportional Hazards Duration Model for the Placebo Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Basic	No Return	By Age	4 Polys	LS	RCS	4 LS	4 RCS
SP	0.539	0.66	1.41	-0.0179	0.335	0.294	-0.0269	0.091
	(0.017)**	(0.007)***	(0.003)***	(0.940)	(0.197)	(0.189)	(0.926)	(0.702)
Age	8.41	66.9	-17.7					
	(0.946)	(0.619)	(0.967)					
Age2	0.0937	-1.03	1.23					
	(0.969)	(0.693)	(0.884)					
Age3	-0.00318	0.00382	-0.0152					
	(0.836)	(0.819)	(0.784)					
SP*Aged	0.39							
	(0.000)***							
SP*Aged2	-0.0343							
	(0.303)							
SP*Aged3	0.00438							
	(0.123)							
N	31,864	29,135	31,864	31,864	31,864	31,864	31,864	31,864

Same regressions as in Table 4.4, but for the Placebo Sample of individuals separated from planss that were not a member of the severance pay scheme.

Table 4.7: Completed Durations

	(1)	(2)	(3)	(4)	(5)	(6)
	Basic	No Returners	By Age	4 Polys	LS	RCS
SP	4.20	3.98	4.69	5.35	5.62	4.76
	(0.051)*	(0.091)*	(0.242)	(0.016)**	(0.027)**	(0.022)**
Age	-83.19	-609.30	2680.30			
	(0.939)	(0.614)	(0.487)			
Age2	1.58	11.72	-54.55			
	(0.940)	(0.615)	(0.475)			
Age3	-0.01	-0.07	0.37			
	(0.947)	(0.621)	(0.461)			
SP*Age			-0.57			
			(0.572)			
SP*Age2			0.00			
			(0.993)			
SP*Age3			-0.02			
			(0.443)			
Cons	1760.20	10778.10	-43501.50	364.50	44.89	180.50
	(0.925)	(0.604)	(0.500)	(0.000)***	(0.910)	(0.639)
Sig Cons	453.30	477.90	452.80	451.50	452.70	452.80
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
N	4,362	4,078	4,362	4,362	4,362	4,362

Outcome: Effect of each NOK 1,000 of severance pay on duration until new job in days

Censored Normal Regressions, using the same control functions as in Table 4.4.

P-values in parentheses, based on robust and person-clustered SEs; *P<0.10, **P<0.05, ***P<0.01

Table 4.8: Completed Durations Regressions with Different Censorings

	67		62		60	
	2y	6m	2y	6m	2y	6m
slvt	4.30	1.49	4.22	1.75	4.43	1.71
	(0.003)***	(0.008)***	(0.034)**	(0.026)**	(0.032)**	(0.038)**
Age	519.10	128.60	444.80	382.50	-127.30	136.80
	(0.256)	(0.460)	(0.556)	(0.194)	(0.903)	(0.745)
Age2	-10.73	-2.74	-9.07	-7.62	2.43	-2.84
	(0.205)	(0.396)	(0.527)	(0.174)	(0.904)	(0.726)
Age3	0.07	0.02	0.06	0.05	-0.01	0.02
	(0.151)	(0.321)	(0.489)	(0.150)	(0.910)	(0.702)
Cons	-7,946.20	-1,831.50	-6,978.80	-6,262.80	2,530.40	-2,029.60
	(0.330)	(0.555)	(0.596)	(0.224)	(0.888)	(0.779)
Sig Cons	478.40	167.00	463.90	165.20	448.50	163.00
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
N	5,870	5,870	5,192	5,192	4,666	4,666

Table 4.9: Proportional Hazard Analyses around the Age 50 Discontinuity

	Main Sample			Age 49 Placebo			Non-EP Placebo		
	No Controls	1 Poly	2 Polys	NC	1 Poly	2 Polys	NC	1 Poly	2 Polys
T	-25.3	-46	-46.6	-0.12	16.3	16.3	-6.81	-1.15	2.02
	(0.001)***	(0.031)**	(0.160)	(0.990)	(0.446)	(0.446)	(0.023)**	(0.888)	(0.863)
Aged		25.8	-23.8		-14	-14		-3.38	59.9
		(0.124)	(0.813)		(0.456)	(0.456)		(0.609)	(0.113)
Aged2		2.48	-55.2		2.39	2.39		-0.762	67.8
		(0.452)	(0.636)		(0.873)	(0.873)		(0.555)	(0.122)
Aged3		-7.75	-26		3.85	3.85		0.236	21
		(0.066)*	(0.491)		(0.675)	(0.675)		(0.889)	(0.145)
T*Aged			100.1						-149
			(0.486)						(0.004)***
T*Aged2			-0.0884						29.2
			(1.000)						(0.634)
T*Aged3			36.4						-51.5
			-(0.500)						(0.011)**
N	1,160	1,160	1,160	884	884	884	8,243	8,243	8,243

Effect of being aged >=50 (1st placebo: 49) on propensity to start new job in 2 yrs after job loss, in %; Bandwidth ages 48-52 (placebo: 47-50); "Aged" is deviation of Age from 50 (49); Besides the main sample analyses have been repeated for the placebo sample aged between 47 and 50 and who are hence all too young to get SP, and for a placebo sample of individuals from plants not participating in the SP agreement
For each we have one specification without controls, one with the 3rd-order polynomial in age plus year effects, and one with separate control functions for left and right of the threshold; P-values in parentheses, based on robust and person-clustered SEs; *P<0.10, **P<0.05, ***P<0.01

Table 4.10: Males' Income and Wealth by Tercile

		N	Mean	SD	Min	Max
Income	All	4,324	404,975	187,176	56,400	1,188,900
	1	1,558	261,452	41,950	56,400	315,800
	2	1,437	364,206	30,965	315,900	424,400
	3	1,329	617,309	199,660	425,000	1,188,900
Wealth	All	4,324	662,732	805,354	0	4,073,107
	1	1,571	172,596	99,010	0	323,394
	2	1,445	456,248	86,432	323,570	628,129
	3	1,308	1,479,531	1,059,094	628,523	4,073,107
HH Wealth	All	4,324	841,997	1,044,172	0	5,794,015
	1	1,605	235,719	127,340	0	422,153
	2	1,419	586,690	110,497	422,304	803,986
	3	1,300	1,869,195	1,419,078	804,742	5,794,015
Financial W.	All	4,324	264,024	551,158	0	2,666,576
	1	1,581	9,725	8,498	0	28,554
	2	1,453	76,019	37,593	28,696	163,204
	3	1,290	787,448	789,786	163,411	2,666,576
HH Fin.W.	All	4,324	361,737	751,264	0	4,018,066
	1	1,604	18,187	14,893	0	51,033
	2	1,455	127,431	57,962	51,186	250,259
	3	1,265	1,066,851	1,102,543	250,552	4,018,066
Deposits	All	4,324	145,241	276,454	0	1,448,589
	1	1,575	6,756	5,977	0	19,578
	2	1,471	51,474	24,157	19,587	106,854
	3	1,278	423,836	382,821	106,876	1,448,589
HH Dep.	All	4,324	201,553	355,026	0	1,959,351
	1	1,598	13,215	10,605	0	36,164
	2	1,483	89,484	40,120	36,219	178,194
	3	1,243	577,388	484,408	178,856	1,959,351
Wealth/Inc	All	4,324	0.710	1.638	0.000	9.903
	1	1,574	0.028	0.024	0.000	0.078
	2	1,455	0.203	0.094	0.078	0.410
	3	1,295	2.107	2.479	0.410	9.903
HH W / Inc	All	4,324	0.989	2.441	0.000	19.422
	1	1,594	0.052	0.041	0.000	0.141
	2	1,443	0.335	0.144	0.141	0.652
	3	1,287	2.884	3.853	0.654	19.422
FinW / Inc	All	4,324	1.833	2.560	0.000	18.030
	1	1,532	0.477	0.266	0.000	0.879
	2	1,434	1.243	0.228	0.879	1.688
	3	1,358	3.988	3.694	1.689	18.030
HH FinW / Inc	All	4,324	2.345	2.725	0.000	27.256

Table 4.11: Proportional Hazard Analyses stratified by Terciles of Wealth

	Inc	Wealth	HH Wealth	FinW	HH FinW	Deposits	HH Dep.
SP	-1.620	-1.760	-1.730	-1.510	-1.860	-1.420	-1.600
	(0.020)**	(0.009)***	(0.010)***	(0.025)**	(0.005)***	(0.035)**	(0.017)**
SP*T2	0.172	0.234	0.303	-0.247	0.180	-0.610	-0.190
	(0.725)	(0.621)	(0.518)	(0.600)	(0.703)	(0.197)	(0.683)
SP*T3	0.190	0.565	0.510	0.305	0.656	0.417	0.419
	(0.699)	(0.265)	(0.309)	(0.544)	(0.193)	(0.403)	(0.409)
Age	-0.033	-0.075	-0.296	-0.170	0.189	-0.029	0.003
	(0.989)	(0.976)	(0.903)	(0.944)	(0.938)	(0.991)	(0.999)
Age2	-0.343	-0.372	-0.354	-0.357	-0.377	-0.355	-0.363
	(0.023)**	(0.014)**	(0.019)**	(0.018)**	(0.013)**	(0.019)**	(0.016)**
Age3	-0.074	-0.074	-0.071	-0.069	-0.074	-0.072	-0.072
	(0.075)*	(0.074)*	(0.084)*	(0.094)*	(0.073)*	(0.084)*	(0.083)*
T2	14.600	18.400	23.500	25.100	18.800	25.200	22.500
	(0.078)*	(0.016)**	(0.002)***	(0.001)***	(0.013)**	(0.001)***	(0.003)***
T3	25.300	-0.829	4.150	2.890	-0.397	-2.050	0.066
	(0.002)***	(0.923)	(0.630)	(0.738)	(0.963)	(0.813)	(0.994)
N	4,324	4,324	4,324	4,324	4,324	4,324	4,324

We repeat the regressions from Table 4.4, (1), but interact the assigned SP amount with indicators for the 3 terciles (T1-3) of different measures of income and wealth: Annual income, total wealth, financial wealth (stocks, bonds, cash, but not real estate) and deposits, each at the individual and the household level. Aged is the deviation of age from the sample mean. P-values in parentheses, based on robust and person-clustered SEs; *P<0.10, **P<0.05, ***P<0.01

Table 4.12: Proportional Hazard Analyses stratified by Terciles of Wealth over Income

	Wealth/Inc	HHW/Inc	Finw/Inc	HHFinw/Inc	Dep/Inc	HHDep/Inc
SP	-1.570	-1.990	-1.970	-2.170	-1.580	-1.750
	(0.018)**	(0.003)***	(0.003)***	(0.001)***	(0.018)**	(0.008)***
SP*T2	-0.250	0.646	0.767	1.290	-0.336	0.327
	(0.593)	(0.170)	(0.111)	(0.007)***	(0.475)	(0.485)
SP*T3	0.287	0.546	0.562	0.522	0.578	0.322
	(0.567)	(0.279)	(0.255)	(0.291)	(0.250)	(0.520)
Age	0.132	0.373	-0.045	0.122	0.048	0.113
	(0.957)	(0.878)	(0.985)	(0.960)	(0.984)	(0.963)
Age2	-0.364	-0.380	-0.361	-0.379	-0.367	-0.352
	(0.016)**	(0.012)**	(0.017)**	(0.012)**	(0.015)**	(0.019)**
Age3	-0.070	-0.078	-0.072	-0.077	-0.069	-0.073
	(0.088)*	(0.057)*	(0.082)*	(0.062)*	(0.093)*	(0.077)*
T2	26.400	12.800	1.620	4.660	21.900	9.330
	(0.001)***	(0.094)*	(0.836)	(0.555)	(0.004)***	(0.221)
T3	-0.605	-2.510	-2.370	1.540	-7.640	-2.380
	(0.944)	(0.770)	(0.774)	(0.850)	(0.385)	(0.782)
	4,324	4,324	4,324	4,324	4,324	4,324

As Table 4.12, but interacting with the terciles of different wealth measures scaled by annual income. P-values in parentheses, based on robust and person-clustered SEs; *P<0.10, **P<0.05, ***P<0.01

4.8 Appendix II: Computation of the Spline Control Functions

The Linear Spline consists of elements $V_{i,}$ for $i = 1, ..., n$ which – given the underlying continuous variable V (i.e. age) and knots $k_i, for\ i = 1, ..., n-1$ at each age threshold with a change in the severance pay amount – are computed as follows:

$V_1 = \min(V, k_1)$

$V_i = \max\{\min(V, k_1), k_{i-1}\} - k_{i-1}$, i=2,...,n

The Restricted Cubic Spline consists of elements $V_{i,}$ for $i = 1, ..., n-1$ which – given the underlying continuous variable V (i.e. age) and knots $k_i, for\ i = 1, ..., n$ at each age threshold with a change in the severance pay amount – are computed as follows:

$V_1 = V$

$V_{i+1} = \frac{(V-k_i)_+^3 - (k_n-k_{n-1})^{-1}\{(V-k_{n-1})_+^3(k_n-k_i) - (V-k_n)_+^3(k_{n-1}-k_i)\}}{(k_n-k_1)^2}$ for i=1,...,n-2

where $(u)_+ = u$ if u>0 and $(u)_+ = 0$ if u$\leq$ 0.

See Stata [2009], pp. 1053-1058.

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