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Institute for Evaluation of Labour
Market and Education Policy

Essays on Politics and Health Economics

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Abstract

Aggeborn, L. 2016. Essays on Politics and Health Economics. *Economic studies* 162. 203 pp. Uppsala: Department of Economics, Uppsala University. ISBN 978-91-85519-69-9.

Essay I (with Mattias Öhman): Fluoridation of the drinking water is a public policy whose aim is to improve dental health. Although the evidence is clear that fluoride is good for dental health, concerns have been raised regarding potential negative effects on cognitive development. We study the effects of fluoride exposure through the drinking water in early life on cognitive and non-cognitive ability, education and labor market outcomes in a large-scale setting. We use a rich Swedish register dataset for the cohorts born 1985-1992, together with drinking water fluoride data. To estimate the effect we exploit intra-municipality variation of fluoride, stemming from an exogenous variation in the bedrock. First, we investigate and confirm the long-established positive relationship between fluoride and dental health. Second, we find precisely estimated zero effects on cognitive ability, non-cognitive ability and education. We do not find any evidence that fluoride levels below 1.5 mg/l have negative effects. Third, we find evidence that fluoride improves labor market outcome later in life, which indicates that good dental health is a positive factor on the labor market.

Essay II: Motivated by the intense public debate in the United States regarding politicians' backgrounds, I investigate the effects of electing a candidate with earlier experience from elective office to the House of Representatives. The U.S. two-party-system with single-member election districts enables me to estimate the causal effect in a RD design where the outcomes are measured at the election district level. I find some indications that candidates with earlier elective experience are more likely to be members of important congressional committees. I also find some indications that directed federal spending (pork barrel spending) is higher in those districts were the elected representative had earlier elective experience prior of being elected to the House, but the effect manifests itself some years after the election. In contrast, I find no robust or statistically significant effects for personal income per capita or unemployment rate in the home district.

Essay III: This paper uses Swedish and Finnish municipal data to investigate the effect of changes in voter turnout on the tax rate, public spending and vote-shares. A reform in Sweden in 1970, which overall lowered the cost of voting, is applied as an instrument for voter turnout in local elections. The reform increased voter turnout in Sweden. The higher voter turnout resulted in higher municipal taxes and greater per capita local public spending. There are also indications that higher turnout decreased the vote share for right-wing parties. I use an individual survey data set to conclude that it was in particular low income earners that began to vote to a greater extent after the reform.

Essay IV (with Lovisa Persson): In a theoretical model where voters and politicians have different preferences for how much to spend on basic welfare services contra reception services for asylum seekers, we conclude that established politicians that are challenged by right-wing populists will implement a policy with no spending on asylum seekers if the cost is high enough. Additionally, adjustment to right-wing populist policy is more likely when the economy is in a recession. Voters differ in their level of private consumption in such a way that lower private consumption implies higher demand for basic welfare services at the expense of reception of asylum seekers, and thus stronger disposition to support right-wing populist policies. We propose that this within-budget-distributional conflict can arise as an electorally decisive conflict dimension if parties have converged to the median voter on the size-of-government issue.

Keywords: Fluoride, Cognitive ability, Non-cognitive ability, Income, Education, Employment, Dental health, Political Leadership, American politics, Regression discontinuity, Voter turnout, Local public finance, Sweden, Finland, Right-wing populism, welfare chauvinism

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Dedicated to Susanna

List of Papers

The following papers are included in the thesis:

1. Aggeborn, L., Öhman, M., (2016), The Effects of Fluoride In The Drinking Water. (In manuscript)

2. Aggeborn, L., (2016), The Effects of Earlier Elective Experience: Evidences From The U.S. House of Representatives. (In manuscript)

3. Aggeborn, L., (2016), Voter turnout and the size of government. *European Journal of Political Economy*, 43: 29–40

4. Aggeborn, L., Persson, L., (2016), Public Finance and Right-Wing Populism. (In manuscript)

Contents

Acknowledgments

Being a PhD Student is nothing less than a privilege. You are allowed to do research in an intellectual environment on topics that you care about. The privilege is even greater if you are fortunate to be a PhD student surrounded by brilliant and friendly coworkers as I have been.

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Uppsala, June 2016

Introduction

"Economics is what economists do" [1]

— Jacob Viner

This thesis consists of four separate essays where the first is on health economics and the last three within the field of political economics. Three of the essays are empirical whereas the last essay is theoretical. Although all of the chapters are founded in the microeconomic tradition, it would be inadequate to say that the thesis has a unifying theme. The goal with this introduction is to put the four essays into context and give some background of the different topics that are studied in the thesis. A recurrent question I often face from people not familiar with the economic field is how my work is research in economics in particular. Common claims are that the research questions are more related to public health or political science. This introduction addresses these inquiries where I argue that the four essays are clearly within the field of economics, both in terms of methodology and study objects. I do this by summarizing the four essays and then very briefly discussing the current focus of economic research. Since the thesis is eclectic, the introduction also includes a short overview of economists' focus on issues related to health and politics. It would be impossible to review the complete history of both political economics and health economics in this introduction. The aim with the second-part overview is therefore simply to introduce the reader to the research topics and to give some historical background to the questions in focus.

1 Summary and discussion of the four essays

The first three chapters in the dissertation are empirical where data is used to test hypotheses. Common for the empirical chapters is the focus on causal questions and the use econometrical methods. In essay 1, me and my coauthor investigate the effect of fluoride exposure on cognitive

[1] Many economists believe that Jacob Viner is the person this quote originates from. Backhouse and Medema (2009) is a paper on how economics should be defined where the authors have not been able to trace the quote to a specific publication. They refer to a publication of one of Viner's students, (Boulding, 1966, p.1), who in turns claims that it was a spoken quote.

development and later labor market outcomes. Fluoride has for a long time been applied to teeth since fluoride improves dental health, but recent evidence suggests that fluoride may have negative side effects on the central nervous system. Fluoride is added to dental products, but fluoride also exists in the drinking water. Since some countries artificially fluoridate their drinking water to improve dental health, the question has policy implications. We investigate whether fluoride has negative effects on cognitive development by using Swedish register data. Our paper is to our knowledge the first that address this question in a large scale empirical set-up with individual register data. In essay 2, politicians' background before being elected to office and its impact on how efficient they are in representing their constituents are in focus. The data originates from the U.S. House of Representatives where I explicitly study the effect of having earlier experience from elective office before being elected to the House on outcomes on the election district level. In essay 3, the effect of voter turnout on policy outcomes and vote shares for political parties in local elections is studied, where Swedish and Finnish municipal data is applied. In the last chapter me and my coauthor develop a theoretical model of politics that can explains how established politicians react when they face right-wing populist challengers.

Applied econometric methods with a specific emphasis on causal issues have been cornerstones in applied empirical economic research during the last decades. Economists have been increasingly concerned with how to *identify* the effects of a certain variable using exogenous variation to mimic an experimental situation. Although descriptive evidences and correlations are interesting, causal relationships are often the primary focus when testing economic hypotheses. Angrist and Pischke (2010) discuss the development of better empirical tools within economics and the *casual revolution* that has transformed the field. All three of the empirical essays in the thesis are founded in this tradition and causal interpretation of the estimated effects stands in the center of the analysis.

Different methods and techniques are applied to study the causal effects of interest in the first three empirical papers. When estimating the effect of fluoride is essay 1, we exploit the fact that the bedrock varies exogenously which yields different levels of fluoride. In essay 2, the aim is to study the effect of electing a person with earlier elective experience to the House of Representatives. To estimate the causal effect, I exploit the fact that the United States has a two party system and that certain elections are close. This creates a situation who is elected in a district near the 50 percent threshold that is as good as randomly assigned. In paper 3, the focus is on voter turnout and its effects on policy outcomes. The introduction of a common election day in Sweden for both parliament and municipal elections is used as an instrument for voter turnout in municipal elections. Finnish municipalities constitute the control group where Finland kept an election system similar to the old Swedish election system.

The data materials for the three empirical essays are also different, especially in terms in number of observations. In essay 1, we use Swedish register data with several hundred thousand observations on the individual level. Register data is particularly suitable to the questions in focus in essay 1 where we can follow individuals in the data material to look at different outcomes from different years to study the effects of fluoride. In essays 2 and essay 3, smaller data sets in terms of number of observations are applied. American data is generally not as detailed as Swedish data and the outcomes in the second essays are measured at the election district level. In essay 3, Swedish data on the municipal level constitute the data material. Because there are only a certain number of municipalities, the number of observations becomes fewer in this case. To work with individual register data is undoubtedly a luxury because of its rich structure. There are however many important research questions that cannot be answered with Swedish individual register data, where one often has to rely on smaller data sets. The research questions in essay 2 and essay 3 are examples of this.

The fourth essay is theoretical. Economic theory has become increasingly mathematized with a special emphasis on equilibrium analysis. In comparison to other social sciences, economists conduct their theoretical work with fewer words and with more math. The primary reason for explicitly stating assumptions and using formalized notation in theoretical work is to verify that the theory is logically coherent and that the conclusions follow from the initial basic fundaments. In that sense, essay 4 is in accordance with modern economic research. It is however important to remember why theoretical models are created. In light of the positivistic approach to science that dominates economics, the ultimate goal is empirical testing. The reality is *out there* and it can be measured and investigated.[2] The goal in the future is to empirically test the predictions in essay 4 that established politicians mimic right-wing populists when there is a high relative cost of immigration and that this mimicking behavior is expected to be more common when the economy is in a recession.

In conclusion, the three empirical essays and the theoretical essay are methodologically in line with contemporary economic research in its fo-

[2] Milton Friedman argued in Friedman (1953) that a theory can only be falsified and never confirmed – an idea related to Karl Popper's view on falsificationism (Boumans and Davis, 2010, chapter 3). According to Friedman's view on science, assumptions may be unrealistic as long as the theory explain empirical phenomenon accurately. This view is somewhat contrasted in Gilboa et al. (2014) who see economic theories as analogies that can explain certain economic cases, but not general phenomena.

cus on causal effects (the empirical essays) and its formalized approach (the theoretical essay). However, the questions studied may at a first glance not be considered as economic. There has been a gradual movement where economists study questions that are not clearly related to classic economic hypothesis. Some people have taken this very far arguing that the methods and the research tools in themselves constitute what economics is really about. To quote Jacob Viner: *''Economics is what economists do"*, (Boulding, 1966, p.1). This argument has some merits and the field has undoubtedly broadened. Martén (2016) shows in the introduction to her thesis that the classifications codes for economics research (JEL-codes) have been widened. Fourcade et al. (2015) discuss economists' meddling into other social sciences and how economics differs from other fields. Law and economics, behavioral economics and neuroeconomics are just a few examples of new dynamic subfields that have emerged in recent decades. This development is often referred to as *economic imperialism* and might intellectually be traced back to the idea of a unifying science proposed by the logical positivists. Although the idea of economic imperialism is illustrative for recent development in the social sciences, I do not think my thesis is particularly imperialistic. At a first gaze, the thesis is connected to questions normally posed within political science, public health and medicine, but my take on these questions are profoundly based on economic queries.

Classic labor and public economic issues concern taxation, public expenditures, intergovernmental grants, economic policy, wage structures, human capital accumulation and unemployment. Economists have for a very long time studied these issues and this thesis does not deviate from these core outcomes. My take on these study objects are admittedly a bit more peripheral. As economists we must however tackle these issues from various angles. To put it in econometric lingo: The left hand side of the regression equations in the thesis is clearly economic, but the right-hand side deals with matters that have not always been considered as economic variables. In the first essay, we study the effect of fluoride congestion on income, employment status, education, cognitive ability and non-cognitive ability. All these outcomes are fundamental variables in the labor economic literature. In essay 2, the outcome variables in the main analysis are personal income per capita, unemployment and directed federal spending. Committee placement of the elected representative is also studied. The first three variables are without doubt outcomes that have been in focus in a number of public and labor economic papers. Tax rates and public expenditures together with vote shares for political parties are the outcomes of interests in essay 3. In the theoretical essay we create a model where macroeconomic shocks and relative prices are key ingredients. Hence, I do not consider my thesis as an attempt to invade other research areas, but rather as an aim to broader the economic field.

In conclusion, the thesis is a typical thesis in economics both in terms of methodology and outcome variables. In the next part of the introduction I give a brief overview why economists became interested in issues related health and politics and how we can motivate to study these topics from an economic angle. The overview is not a complete review of the history of political and health economics, since such an analysis would be a research projects in its own right, but rather a broadened background to the research questions in the thesis.

2 A short introduction to health and political economics

Economists' interest in health was from the beginning primarily twofold. The health care market was on its own right an interesting market to study, see for example Arrow (1963) for seminal work. The theoretical literature on moral hazard is also much connected to the study of the health care market. Concepts related to the health care market such as medical insurance have therefore also been studied within the field of health economics where Pauly (1968) is one example of an influential paper. Throughout the history of health economics, cost-benefit analyses of various health measures have also been a significant part of the literature.

Parallel to the study of the health care market and cost-benefit analyses, there was also a focus on individual health status where Grossman (1972) theorized that health should be seen as an investment by the individual. The author also included a notion that an individual has an inherited health factor and that health depreciates over time. Cunha and Heckman (2007) create a model of cognitive and non-cognitive ability and how different factors throughout life affect a person's skill level. Although the focus is not on inherited factors in essay 1, fluoride exposure is potentially one aspect that can determine a person's health capital in terms of cognitive and non-cognitive ability before the person in question has any means of privately investing in his or her own health.

Another important reason for why economists became interested in health is because of its clear connection to individual labor market outcomes. The effect may go both ways, where health affects productivity and that a person's labor market situation in the same time affects the individual's health. There is also a literature that has specifically focused on health in early life. Case et al. (2005) for example demonstrated that childhood health is a determinant for both socioeconomic status and health later in life. Essay 1 is much in line with this literature. Health adds to the explanations of why certain people are more successful than others on the labor market.

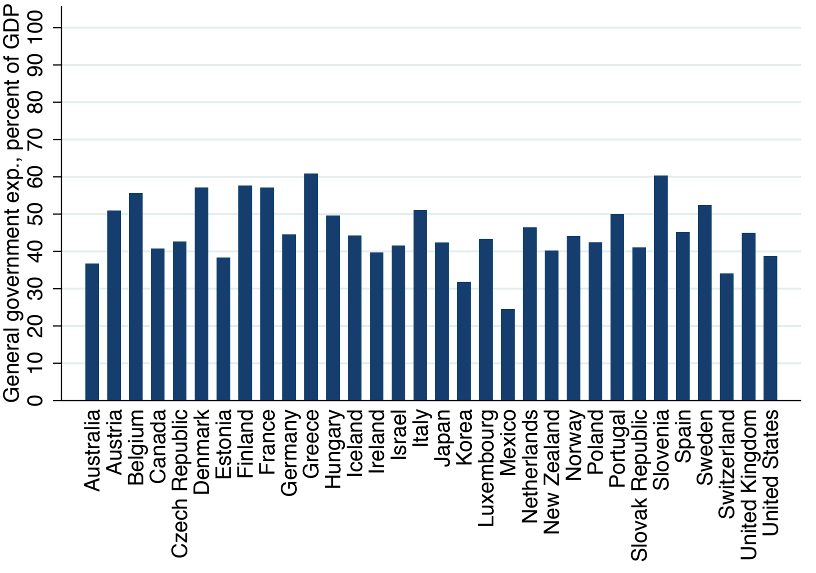
What is interesting is that health economics in itself is not that well-founded in a theory-tradition of its own right. Health status affects a person's length of life and Grossman (1972) pointed out that a person can choose his or her own health status by investment in health. Other than this notation of seeing health as a personal investment choice, there are not that many theoretical health models except for the theoretical foundations behind cost-benefit analysis and the discussion about moral hazard. The *theory* in this case mostly comes from outside of economics where economists have formalized and tested medical hypothesis with econometric methods. One reason for why health economists have not developed theoretical models of health in a higher degree is potentially because economists do not have a comparative advantage in medical knowledge. If we return to the idea of *economic imperialism*, it is clearly so that economists have entered into the field of epidemiology, but economists seldom add insights about basic mechanisms in biology and medicine. Certain parts of epidemiology are however connected to the social sciences and economists' interest in individual health may offer additional insights to areas where economic outcomes and factors are involved.

In contrast to health economics, political economics rests on an immense theoretical tradition with separate theoretical models explicitly developed for political economics issues. Several textbooks about political economic theory have been written and it is not my aim to fully review this huge literature. Instead I would like to briefly comment on certain papers that have been important for this thesis. Whereas health economists borrow hypothesis from medicine, political economists have been productive in creating models about politics. This work has undoubtedly been inspired by and influenced by research in political science. One way to make a distinction between political science and political economics is that political scientists put larger focus on the political sphere in itself, whereas political economists' primary interests is on public (economic) policy and how the political sphere affects policy outcomes.

It is not straightforward to pinpoint an exact year when political economics emerged as a separate subfield, but it is fair to say that political economics has its roots in the 1950's when economists realized that they needed to incorporate the actors within the public sector – primarily politicians – to explain economic phenomena. The size of the public sector had steadily grown to comprise a larger share of GDP and contemporary economists did not have the proper models to understand the mechanisms behind the growth of government and how actors within the public sector behave. Economists knew how to explain price mechanisms on different markets and the incentives that firms face, but the political sphere was considered as something essentially different. Politicians, in contrast to consumers and firms, were assumed to be driven by ideology and for the common good.[3] The public choice school challenged this narrative by modelling political actors as self-interested and utility maximizing. The notion of market failures was as a consequence complemented by the idea of political failures, where the classic public economic idea that a benevolent social planner can offer an efficient outcome was challenged by the conclusion from political economics that politicians themselves can give rise to inefficient outcomes. Essay 3 is much connected to the literature about the size and growth of government where essay 3 focus on voter turnout and how a variation in voter turnout affects tax rates and public expenditures.

Figure 1 displays general government's expenditures as share of GDP in various developed economies. The fact that the government's expenditures constitute such a large share of total GDP is motivation in itself why economist should keep studying political decision-making.

Figure 1. General government's total expenditures as percent of GDP. Data from 2013



Data Source: OECD.Stat.

[3]See the discussion in the introduction (chapter 1) in Persson and Tabellini (2000) and Downs (1957b)

Persson and Tabellini (2000) argue in their first chapter that modern political economics can be traced back to three separate traditions: the public choice school within economics, the macroeconomic tradition of analyzing the incentives of policy makers and the rational choice school within political science. The first concrete signs of political economics reasoning can be found in Hotelling (1929). This is an article about competition in a market with few actors, where Hotelling introduces distance as a factor. He concludes that stores have incentives to move closer to each other to maximize profit. In a passus in the end of the article, he writes that the same reasoning could be applied to the Democratic Party and the Republican Party in the United States. They had, according to Hotelling, moved closer and closer in terms of policy positions.

Downs (1957b) and Downs (1957a) bring economic theory to the political area by describing the incentives of political parties in a two-party system to converge towards the median position. This idea constitutes the median voter theorem, or the Hotelling-Downs model, which is still a workhorse model within political economics. Downs was also one of the first to note that economists cannot treat politicians' actions as exogenous. Instead, political outcomes are according to Downs the results of utility maximizing politicians serving their private interest, where Downs modeled them as office-motivated.

Later political economic models introduced policy preferences for politicians while Osborne and Slivinski (1996) and Besley and Coate (1997) modeled the choice of becoming a politicians in a Citizen-Candidate framework. Citizens decide whether to run for office in the Citizen-Candidate model, where they implement their preferred policy if elected to office. The Citizen-Candidate model has given rise to a new strand of models that have focused on the policy preferences and characteristics of elected politicians. Besley (2005) emphasized that economists should study the characteristics of elected politician to a greater extent and the new literature on *political leadership* follows in that tradition. Essay 2 is a part of this political leadership literature where the focus is on earlier elective experience.

Special emphasis has also been put on analyzing rent seeking behavior among incumbent politicians in the political economic literature where Barro (1973) and Ferejohn (1986) are seminal papers. The political agency model in Besley and Smart (2007) is a development within this theory tradition which we follow suit in essay 4, although our focus is on established politicians' behavior when they face right-wing populists challengers.

3 Concluding remarks

Hopefully I have at this point convinced the reader that political and health economics are important subfield within economics and that my focus on voter turnout, the background of elected politicians, the incentives of established politicians and fluoride are important aspects to consider when analyzing economic outcomes. The choice of research topics has nonetheless not only been driven by a will to contribute to the discussion within academia. Research should not take place in a vacuum and an important inspiration for studying these topics comes from the public debate. The potential dangers associated with fluoride is a highly debated topic; both by scientists, but also among the public. If one listen to the political discussion in the United States one cannot fail to hear the constant return to the issue of politicians' background. Voter turnout is also debated, where there is a discussion in Sweden whether the constitution should be changed back to having separate election days for parliamentary and local elections. The issue of how established politicians are challenged by right-wing populist is a well-debated topic during the last years. Hopefully, the conclusions presented in the thesis can add substance to the public discussion.

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I. The Effects of Fluoride In The Drinking Water

Co-authored with Mattias Öhman

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1 Introduction

It is well-established that fluoride strengthens the tooth enamel and that application of fluoride on the surface of the teeth prevents caries, tooth decay and cavities. The use of fluoride in a wide range of dental products is therefore considered as an important mean to improve dental health. Because there is such a well-defined link between fluoride and healthy teeth, some countries artificially fluoridate the drinking water so that people are continuously exposed to higher levels than the natural level. Australia, Brazil, Canada, Chile, Malaysia, the United Kingdom and the United States are a few examples of countries that apply such a public policy (Mullen, 2005). Other countries, such as Sweden, do not fluoridate the water, but the authorities choose not to reduce the fluoride level in the water cleaning process as long as it is below a certain limit. These public policies are, however, debated. Fluoride is deadly at high levels, and there is an emerging and much discussed epidemiological literature of potential negative side effects of long-term fluoride exposure for lower levels on the central nervous system. The hypothesis is that fluoride might function as a neurotoxin.

In comparison to dental products, drinking water containing fluoride is ingested, meaning that everyone drinking water is exposed to fluoride continuously for a long period of time. In this paper we investigate the causal effect of fluoride exposure in early life through the drinking water on cognitive and non-cognitive development, education and later labor market outcomes. We also study the long-established link between fluoride and dental health. We use a unique register data set from Sweden together with drinking water fluoride data, where we exploit intra-municipality variation in fluoride to estimate the effect.

Earlier epidemiological studies have found evidence of negative side effects of fluoride, and the results have sparked a public debate regarding the potential dangers associated with fluoride in the water (e.g. Johnston, 2014 in The Telegraph; Mercola, 2013 in The Huffington Post).[1] A meta-study by Choi et al. (2012) from Harvard School of Public Health reviewed 27 papers and concluded that exposure to high dosages of fluoride is associated with reduced cognitive ability among children (almost

[1] One indication that people tend to be very concerned with fluoridation is found in Lamberg et al. (1997). The local authorities in Finland decided that water fluoridation should stop at a given date, and this decision was communicated to the inhabitants. However, water fluoridation ceased one month earlier without notification to the public, but people still reported various symptoms in a survey.

half of a standard deviation in IQ).[2] The studies reviewed originated from China and Iran. Several of these papers considered very high levels of fluoride which surpasses the recommendation from the World Health Organization (WHO) that fluoride should not exceed 1.5 mg/l in the drinking water (WHO, 2011, p.42). However, some of the studies reported negative effects on cognitive development for levels below the recommended level. This is a cause for concern because these levels are present naturally in the drinking water in many parts of the world. Countries that fluoridate the drinking water also have fluoride within this range. Common problems with the studies reviewed by Choi et al. (2012) are that the analyses were based on small samples with poor data quality, and without clear identification strategies.[3]

Our paper is to our knowledge the first to study the effects of fluoride in a large-scale set-up with individual register data. We have access to a rich panel of Swedish register data which enables us to investigate the effect of fluoride in a more credible way and with a much larger sample than earlier studies. Sweden has a natural variation of fluoride in the drinking water which stems mainly from the bedrock under the water sources. The fluoride level in the Swedish drinking water range between 0 and 4 mg/l in our data set, and there is often variation within municipalities which we exploit to estimate the casual effect. In comparison to China and Iran, where the studies reviewed in Choi et al. (2012) originates from, Sweden has a well-supervised water supply system, meaning that other drinking water hazards that can affect cognitive development are not likely to be present. Fluoride in Sweden is generally not considered to be a problem unless the level exceeds 1.5 mg/l.[4] Since our data include a variation in fluoride in the lower spectra, our results are more policy relevant for countries that artificially fluoridate the drinking water, because water authorities seldom add fluoride so that the level

[2] See Tang et al. (2008) for an earlier meta-study, which also show a negative relation between fluoride and IQ. Papers published after or around Choi et al. (2012) include Ding et al. (2011), Saxena et al. (2012), Seraj et al. (2012), Nagarajappa et al. (2013), Ramesh et al. (2014), Khan et al. (2015), Sebastian and Sunitha (2015), Kundu et al. (2015), Choi et al. (2015), Das and Mondal (2016) and Dey and Giri (2016) who all found or discussed negative effects of fluoride on IQ. Additionally, Malin and Till (2015) found a positive association between fluoridated water and the prevalence of ADHD in the U.S.. See also Li et al. (2016) for a study on fluorosis and cognitive impairment.

[3] There are, however, studies that points in the other direction. Broadbent et al. (2015) follows approximately 1,000 individuals in an observational study from New Zeeland. The authors find no negative effect on IQ from living in an area in the city of Dunedin with artificial fluoridation. The main critique against this study is that artificial water fluoridation may be an endogenous policy variable.

[4] The absolute majority of the Swedish water plants have fluoride levels below 1.5 mg/l.

exceeds 1.5 mg/l. There are no evidence for any differences between artificially fluoridated drinking water and water with a natural occurrence of fluoride (Harrison, 2005; John, 2002), meaning that our results should be valid for countries with comparable artificial fluoride levels.

As economists, we are interested in the connection between fluoride, cognitive ability, education and labor market outcomes for at least two reasons. First, artificial fluoridation of the drinking water is a common public health program, and it is important that the effectiveness of such a policy is evaluated. Second, economists have in an increasing degree become interested in early determinants of health and human capital, and its long-run effects on labor market outcomes. Our paper is connected to this literature on environmental determinants for cognitive development where we study a treatment that millions of people are affected by all over the world: fluoride in the drinking water.

Our results confirm the positive link between fluoride and dental health. However, in contrast to earlier studies, we find a zero effect of fluoride on cognitive ability, non-cognitive ability and education (measured by test scores on a national math test). Our point estimates with regard to cognitive ability are much more precisely estimated compared with earlier studies and always close to zero. We find evidence that fluoride is a positive factor for later labor market outcomes, which indicates that better dental health is a positive factor on the labor market.

The rest of the paper is organized as follows. In the next section we review related papers, followed by a short medical background for why fluoride might have an effect on the central nervous system. Next, we provide a simple conceptual framework on how we should think about fluoride in the drinking water as a public health policy. Our identification strategy is mainly based upon the variation in fluoride which stems from an exogenous variation in the bedrock, so in section 5, we present the necessary geological background and information on how we have mapped drinking water data to the individuals. In section 6, we describe our data material. Our identification strategy and econometric set-up is discussed in section 7 followed by descriptive statistics in the same section. The empirical results are then presented, next robustness checks and lastly our conclusions. Additional results and figures are presented in the appendix.

2 Earlier literature

In this section we review the literature regarding early determinants for health and its effects on labor market outcomes. We explicitly focus on papers that have studied drinking water.

Currie (2011) provides an excellent overview of this research field with a special emphasis on determinants at birth and in utero. Economists acknowledge that health during childhood is an important determinant for success on the labor market (Currie, 2009). Case et al. (2002) and Currie and Stabile (2003) provide evidence for the connection between health and socioeconomic status. Case et al. (2005) present the conclusion that health during one's early years seems to be connected to (among others) socioeconomic status and one's education once becoming an adult. Smith (2009) has also demonstrated this link empirically, and found that poor health before age 16 is negatively associated with future income, wealth and labor supply.

Cognitive development is part of individuals' health, and earlier research have shown that cognitive ability and non-cognitive ability are very adequate explanatory variables for basically everything that we consider as positive individual labor market outcomes (e.g. Heckman et al., 2006, Lindqvist and Vestman, 2011). Cunha and Heckman (2007) create a theoretical model concerning cognitive and non-cognitive ability and Cunha and Heckman (2009) emphasize that there are "critical" and "sensitive" windows when cognitive and non-cognitive abilities are more affected by environmental factors. See also Cunha et al. (2010). According to the authors both cognitive and non-cognitive ability are very important factors for later achievements in life. This view is confirmed in Öhman (2015) and Lindqvist and Vestman (2011), who use the results from the Swedish draft tests for cognitive and non-cognitive ability and show that they are very good predictors for mortality, education and income. If fluoride has negative effects on cognitive development, this adds a piece to the puzzle why some individuals are more successful than others on the labor market.[5]

We are not aware of any other paper that has employed large individual register datasets to estimate the effect of fluoride on cognitive development specifically. In a recent manuscript, Heck (2016), studies the effects of water fluoridation on health and education with U.S. survey data. He finds that fluoridated water prevents caries in deciduous teeth, but no effects on education and general health. A limitation in this study is that education is measured only at the county level. The main critique is that water fluoridation is a result of a policy choice, making the identification less clear.

Some earlier papers in economics have focused on other potential hazards and their effects on health and cognitive ability. Currie et al. (2013) study the effect of mothers' consumption of polluted drinking wa-

[5]A seminal paper by Grossman (1972) presents a framework for individual health investment. Fluoride may affect an individual's health before he or she can make an active investment choice.

ter (broadly defined) during pregnancy on birthweight of the offspring with data from New Jersey. They find that the birthweight is negatively affected by contaminated water for mothers with a low education. Zhang (2012) uses Chinese data and study the effect of providing monitored and safe drinking water from a water plant to the population. The author finds a positive effect on the ratio of weight and height for both children and adults and some evidence of less illness among adults.[6] Galiani et al. (2005) study whether privatization of water supply in Argentina improved water quality, and find that children mortality decreased if an area was provided with drinking water from a private provider. Feigenbaum and Muller (2016) study lead and explicitly how people were treated with lead originating from the drinking water pipes. The authors study homicide incidence, where they find a positive effect of lead.

Lead has also been studied with regards to air pollution. Nilsson (2009) study the long-term effects of lead on labor market outcomes. The author uses time variation from the time period when lead in gasoline was reduced together with Swedish geographical data on lead levels in the environment, and concludes that a reduction in lead exposure in early life has positive effects on cognitive ability, education and labor market outcomes. In a similar paper, Grönqvist et al. (2014) conclude that the reduction in lead exposure also reduce criminal behavior. Schlenker and Walker (2016) study pollution from airports in California and find that prevalence of respiratory deceases, heart diseases and asthma increase among the inhabitants, especially among children and older people, if carbon monoxide emission increases. In Jans et al. (2014) the authors study air pollutants' effect on child health. Periods of inversions seems to affects children from high-income families 40 percent less than children from low-income families.

It might be that fluoride in the drinking water has negative side effects on cognitive ability, but the net effect on income still is positive because the effect on dental health is so large. Glied and Neidell (2010) found that women living in areas whose water was fluoridated had higher incomes, where the effect seems to be stronger according to the authors for those with a poor socioeconomic status.

3 Medical background

In this section we shortly review the medical discussion about fluoride and its effects on health.

[6] The author briefly discuss fluoride in the Chinese drinking water but do not study this explicitly.

Sodium fluoride (NaF), from now on called fluoride, is a toxic compound which exists naturally in the environment. WHO acknowledge a deadly dose of fluoride to about 5-10 grams depending on the body weight (Liteplo et al., 2002, p. 100). Fluoride intake from the drinking water is absorbed and transmitted throughout the blood system (Fawell et al., 2006, p.29-30). When large amounts of fluoride are ingested it has a number of toxic effects on the body. For example, approximately 100,000 individuals in the Assam region in India have been taken ill with kidney failure stiff joints and anemia and as a result of very high natural levels of fluoride in the water (WHO, 2015). Gessner et al. (1994) discuss a case in Alaska where individuals in a small village accidently were exposed to extremely high levels of fluoride (up to 150 mg/l) due to a malfunctioning water pump. One individual died and many became very ill as a result of fluoride poisoning.

Lower dosages of fluoride have, on the other hand, beneficial effects on dental health (see Griffin et al. (2007) and Twetman et al. (2003) for reviews). For that reason, fluoride is added to toothpaste and in some countries to the drinking water. Fluoride is also present naturally in tea leaves and in low concentration in the food (Liteplo et al., 2002, p. 5).

Water fluoridation is a highly debated issue (Derek, 2002; EBD, 2002; Peckham and Awofeso, 2014). Researchers have called for more research on the subject, where Grandjean and Landrigan (2014) argue for a global initiative for more research on potential neurotoxins, including fluoride. Mullenix et al. (1995) was one of the first papers testing the hypothesis that fluoride exposure has effects on the central nervous system. The researchers exposed randomly selected rats to different fluoride treatments (including fluoridation of the drinking water), and concluded that the rats' brain tissue can store fluoride and that fluoride can pass through the blood-brain barrier. They found that a higher concentration of fluoride in the brain tissue induced behavioral changes meaning that fluoride functions as a neurotoxin in rats. Chioca et al. (2008) also conducted laboratory rat experiments and concluded that high exposure of fluoride through the drinking water induced impaired memory and learning. Whether fluoride can pass the blood-brain barrier in humans is debated. Chioca et al. (2008) state that a one-time high consumption of fluoride does not seem to pass the blood-brain barrier. Hu and Wu (1988) found fluoride present in the cerebrospinal fluid, which surrounds the brain among humans. The question is whether a long-term consumption of fluoride passes the barrier. Consuming water with fluoride is an example of a long-term consumption.

Given that fluoride is both a lethal and dangerous compound at higher dosages, and improves dental health at lower dosages, it is important to find the optimal level. There has been a consensus that fluoride only has adverse effects above the threshold level of 1.5 mg/l (WHO, 2004).

In light of recent epidemiological findings reviewed in Choi et al. (2012) this threshold could be questioned.

4 Conceptual framework

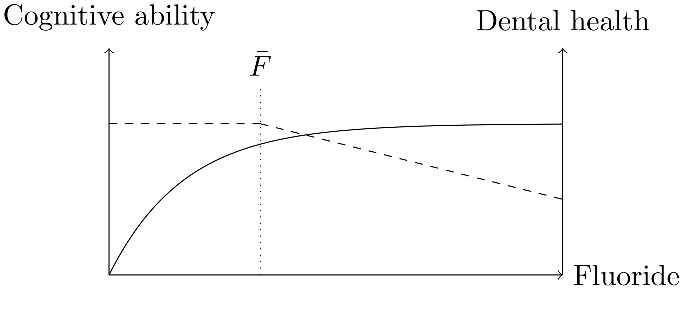
We present a simple and short conceptual framework in this section on how we can think about water fluoridation as a public policy.

Fluoride is a potential neurotoxin that may have a negative effect on cognitive ability, but is known to have a positive effect on dental health. The policy maker must decide on the cost-benefit of fluoridation in comparison to other alternatives. For example, fluoridation of the water can be less expensive than publicly subsidized dental checkups and teeth repairs, thus making it an effective public policy.

It is on the one hand unlikely that the general public would accept fluoridation if it is dangerous for the health in any known way. On the other hand, for economists, the optimal level of fluoride is where the marginal costs equal the marginal benefits. If the positive effect on dental health is very large with only a very small negative effect on cognitive ability, the net effect could still be positive. That would be positive for the individuals given that better dental health is probably a positive factor on the labor market.

Figure 1 illustrates the policy makers problem in a single figure.

Figure 1. The effects of fluoride on dental health (solid line) and cognitive ability (dashed line)



We investigate whether $\bar{F}$ exists in the Swedish drinking water. Based on this, it is possible to do a cost-benefit analysis of the optimal fluoride level.

5 Exogenous variation in fluoride: geological background

In this part of the paper we discuss how fluoride varies exogenously in Sweden. We also discuss how we map the drinking water data to individuals' place of residence.

The natural level of fluoride in the drinking water depends on geological characteristics, especially the type of bedrock under a water source (Sveriges Geologiska Undersökning, 2013, p. 81). Fluoride is both tasteless, without odor and without any color for the levels we consider in this paper, implying that individuals cannot know whether they are drinking water with lower or higher levels of fluoride (WHO, 2001).

There are different types of bedrock, providing different levels of fluoride to the water. Soil bedrock is associated with lower levels of fluoride in comparison to granite and greywacke bedrock which yields higher levels. Especially water from drilled bedrock wells usually contains higher levels of fluoride (Sveriges Geologiska Undersökning, 2013, p. 81, 84). Rainfall usually contains low levels of fluoride (Edmunds and Smedley, 2013, p. 313).[7] In Sweden, water sources are situated on different types of bedrock, thus yielding different fluoride levels. For a detailed description about fluoride and its natural geological occurrence, see Edmunds and Smedley (2013) and Sveriges Geologiska Undersökning (2013).

The fluoride level is, from our perspective, an exogenous variable that is constant for a very long time because the bedrock is constant. Hence, the water authorities have no possibility to manipulate the natural levels of fluoride in raw water. The water authorities may reduce the fluoride levels in the water cleaning process, but this is not done in Sweden unless the level exceeds 1.5 mg/l.[8]

Each municipality in Sweden is divided into several SAMS (Small Areas for Market Statistics) by Statistics Sweden. A SAMS consists of approximately 750 individuals in the year 2011, with median 520, and is a smaller geographical unit than the municipality. There are almost 9,300 SAMS in Sweden in comparison to 290 municipalities.[9] Each municipality in Sweden is responsible for the public drinking water. The large majority in Sweden drinks water from the municipal water plants. However, some individuals have private wells for which we do not have

[7] One of the main sources of fluoride in rain is volcanic emissions (Edmunds and Smedley, 2013, p. 314), but there are no active volcanos in Sweden.

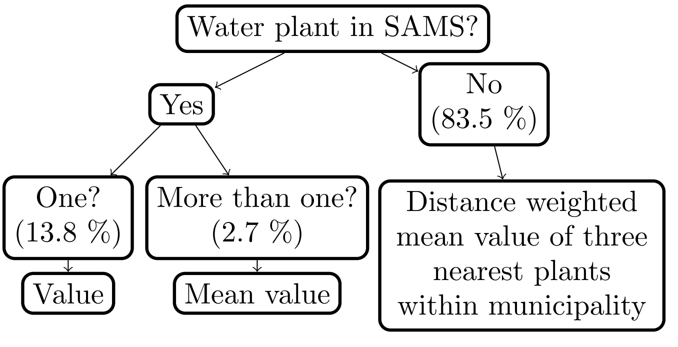
[8] In our data collecting process from the Swedish municipalities, nothing indicates that water authorities lowered the fluoride if was below 1.5 mg/l.

[9] The reader should note that SAMS areas are not something that the public in general is aware of. Municipalities, however, are administrative areas that exist in the public's mind.

data. Approximately 1.2 million people of Sweden's total population of approximately 10 million drink water from private wells (Livsmedelsverket, 2015). Because municipalities often have different water sources situated on different types of bedrock, there is a within-municipality variation in fluoride on the SAMS level.[10]

We have information on fluoride levels for the outgoing drinking water from the water plants supervised by the municipalities. There are 1,726 water plants in our data where we have manually designated a coordinate for the water plant based on the supplementary information we have from SGU and from the municipalities. Some municipalities do not have a water plant within its borders. These municipalities have been dropped from the analysis together with those municipalities where we do not have any information regarding fluoride. In total, data from 261 municipalities are included. We know in which SAMS an individual lived for a given year, but we cannot observe the exact geographical coordinate for the location where the individual lived within a SAMS.[11] Thus, we need a mapping protocol for how to assign fluoride data for each SAMS.[12] We map the fluoride level to SAMS using the mapping protocol illustrated in Figure 2. We indicate the share of SAMS in each category in parenthesis.

Figure 2. Water plants mapping. Percentage of SAMS in parenthesis



For SAMS that have a water plant within the borders we assign the fluoride level of that water plant to all the individuals that lived in the area. If there is more than one water plant within the SAMS border, we take the mean fluoride level. For SAMS without a water plant within

[10] Augustsson and Berger (2014) show that there is a variation in the fluoride level in private wells in Kalmar county in Sweden.

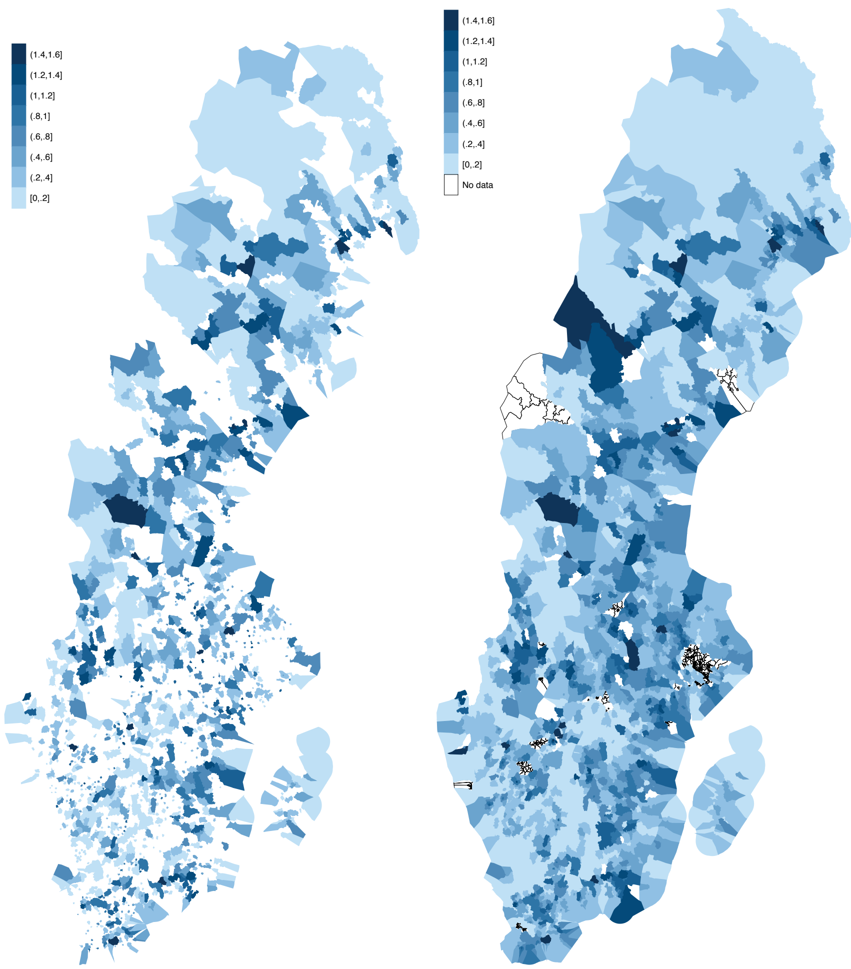
[11] Such data would abolish the anonymous structure of the Swedish individual register data, since population address registers are public information in Sweden.

[12] Since we cannot observe the exact location within a SAMS, we cannot distinguish on the household level who drinks the water from the municipal water plants and the private wells. We return to this issue in the robustness analysis.

the borders, we calculate the geographical center point of the SAMS, and assign a mean of the fluoride level for the three closest water plants (triangular polygon) using the inverse distance as a weight. We assess this mapping protocol by first looking at the effect of fluoride on dental outcomes for which we expect to see an effect of fluoride. By looking at dental health measures, we also address whether the variation in fluoride in our data is enough to estimate effects.

Figure 3a displays the raw variation in fluoride for those SAMS with a least one water plant. White areas are thus SAMS without a water plant. Figure 3b shows the variation in fluoride between SAMS after our mapping.

Figure 3. Mapping of fluoride data



(a) SAMS with at least one water plant

(b) Final mapping

6 Data

In this section we present the data material.

In short, we have register data at the individual level for all outcomes and covariates except dental health. The dental health data is only available on the SAMS level for each cohort from age 20. We observe place of residence for all individuals of age 16 and older on the SAMS level.[13] In order to track individual's place of residence before age 16 we link them to their parents, and use the mother's place of residence as a proxy. Our treatment period for fluoride consumption spans between birth and age 16. We chose this treatment period because the brain should be more sensitive to potential neurotoxins in early life in comparison to adulthood. The articles reviewed in Choi et al. (2012) never consider individuals older than 16 in their analysis.[14] We include cohorts born between 1985 and 1992 in our data.

6.1 Fluoride data

Fluoride data is measured for each water plant, and there are in total 1,726 water plants supervised by the municipalities in our data set. This data comes from two sources: Drinking water data from Swedish Geological Survey (SGU) and drinking water data from the municipalities. We use the SGU data or the municipal data depending on which data set that has the earliest available drinking water data for a given municipality. The SGU data starts in 1998. For some municipalities data is only available for later years.[15] We have contacted each of Sweden 290 municipalities to complement the SGU data set. We asked the municipalities to provide us with additional data from 1985. If data were not available, we asked them whether they have changed any of their water sources since 1985.[16]

It should be noted that the fluoride level is constant back in time because the bedrock has not changed. The fluoride level should only be

[13] For some individuals and years, SAMS codes are missing. We have imputed SAMS codes from $t-1$ or $t+1$ in these cases if municipal code is the same.

[14] There are some inconsistencies in the register data. For example, we have dropped all individuals with multiple birth years, duplicate observations, individuals not in both the LOUISE database and the multigenerational database. We also drop individuals that have immigrated to Sweden during childhood since we need to track their fluoride level from birth. Their parents may, however, have immigrated before the individual's birth.

[15] We only use the observations from the SGU data regarding drinking water and not the observations for "raw water".

[16] Not all municipalities have kept their statistics from 1985 and some have not been able to answer our questions. In the robustness analysis, we rerun all specifications but only include municipalities where we are sure that they use the same water source since 1985.

different if (1) the municipality has changed the water source (which is rare), or, (2) installed any purification for fluoride (which they do not do unless the level exceeds 1.5 mg/l). We collapse the fluoride data into a single measure for each water plant, meaning that we take the average when we have data from several years for a water plant. Variation between the years should be due to variation in the measurement validity for individual data points, meaning that an average measure is more accurate. The reader should note this means that for the very few cases where purification has been installed, we take the average for *all* years available.[17] We drop all individuals who have ever lived in a municipality between birth and age 16 for which we do not have fluoride data.

6.2 Individual level data

The data for the individuals originates from several sources which we briefly discuss in this section.

The cognitive and non-cognitive ability measures come from the Swedish military enlistment. For more detailed information about the enlistment, see Öhman (2015). Conscription was obligatory for men between 18-20 years old in Sweden until its abolishment in 2009. Those who declined their call to conscription were punished; however, this practice was not enforced in the end years of the Swedish draft. Conscription involved testing of cognitive and non-cognitive ability and the individual's physical health. Cognitive ability was measured by a test where the purpose was to measure the underlying intelligence, often called the g factor. This was done by using four sub-tests: verbal, spatial, logical and technical knowledge. The overall test score was then standardized into a single measure on a scale between 1 and 9, according to a Stanine scale. The non-cognitive ability was assessed by a psychologist during a half-hour interview with the conscript. The psychologist's goal was to evaluate the person's ability to function in a war scenario. Those who were keen to take initiative and who were well-balanced emotionally ended up with a high score. The psychologist also considered the individual's ability to deal with stressful situations. The overall assessment was a score according to the Stanine scale. Öhman (2015) shows that

[17] In 2003, the Swedish Food Agency abolished the possibilities to give exceptions for fluoride levels above 1.5 mg/l to 6 mg/l. There were fewer than 100 water plants before 2003 with a median level higher than 1.5 mg/l (Persson and Billqvist, 2004). Those plants provided water to approximately 0.26 % of the Swedish population (Svenskt Vatten, 2016). After 2003, there is a single limit set to 1.5 mg/l (Sveriges Geologiska Undersökning, 2013, p. 82). 1.3 mg/l to 1.5 mg/l yielded a note prior of 2003, but was considered safe and did not result in general purification of the water. Children below half a year old was recommended to drink such water with moderation.

both these measures are good predictors for individual outcomes later in life. We only include men born before 1988 when estimating these outcomes since we only have access to this data for those years.

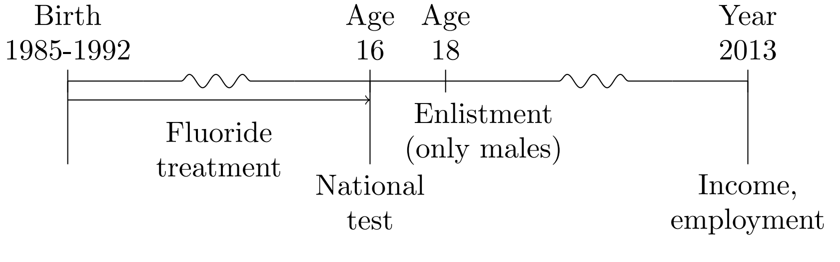
In the end years of the Swedish enlistment, there was a theoretical possibility of strategic manipulation of test results. Individuals who scored low on the tests was not always forced to do military service meaning that the incentives to perform well were less clear for later cohorts. However, the Stanine distribution is relative to others enlisting in the same cohort, so we should still be able to capture meaningful differences in cognitive ability and non-cognitive ability within a cohort (see Figure A2 in the appendix). We can also test this by looking at the correlation between this test score and the test score for the same individual on the national math test. In the latter case, the individual has clear incentives to perform well since final grade in math from junior high school depends on this test result. The correlation between these two tests is 0.43. We conclude that strategic manipulation on the military enlistment test does not seem to be a big concern.

As an outcome for education we use results from the national test taken at age 16. We focus on the basic points result on the math test. This is due to two reasons. First, this is the variable where we have the most detailed statistics, and, second, it should be a fairly good proxy variable for cognitive ability, which our correlation above confirms. The data comes from Statistics Sweden (SCB). We have results for those born in 1987 and later.

Income is measured in 2013 (the last year available), and the data comes from the Swedish tax agency through Statistics Sweden. The variable is defined as gross income for all individuals that have earned any income throughout a year. We exclude all individuals that have earned less than 1,000 Swedish kronor (about $120 in 2016) during a year for this outcome. Employment status is measured in November the year 2013. An individual is coded as employed if he or she has worked at least one hour during a week.

Figure 4 illustrates the timing of the outcome variables and the fluoride treatment.

Figure 4. Timeline for treatment and outcomes



7 Empirical strategy

This section contains a presentation of our identification strategy and a discussion about potential threats to identification. The section also includes a presentation of the econometric set-up and descriptive statistics.

We estimate the causal effect of fluoride exposure through the drinking water on cognitive ability, non-cognitive ability, education, employment status and income. The ideal experiment with maximal internal validity would be to randomly assign fluoride to individuals. Due to randomization, the fluoride levels would be independent of individual characteristics, which enable a causal interpretation of the results. Since it is not possible to randomly assign fluoride intake from birth to age 16, we need to rely on a quasiexperimental design.

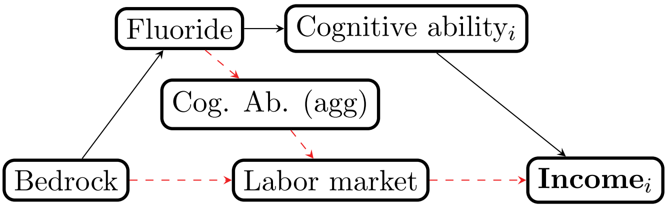
We use exogenous variation in fluoride within municipalities in Sweden to estimate the effect. This enables us to control for unobservable characteristics on the municipal level which could also be determinants for the outcomes we study. We estimate the reduced form effect of fluoride on our outcome variables. Hence, our main identifying variation in fluoride stems from an exogenous geographical variation in the bedrock within municipalities.

In addition to using within-municipality variation in fluoride, we also exploit a second source of variation stemming from individuals' moving patterns. To move or not between birth and 16 is undoubtedly an endogenous outcome, but as long as the choice of moving and the moving location is not dependent on fluoride or other variables correlated with fluoride, this yield an exogenous variation in the intensity of fluoride treatment which depends on the number of years in different SAMS. It is very unlikely that people self-select into SAMS based on the fluoride level. It is difficult to obtain information about the fluoride level since there is no comprehensive open data set in Sweden. People cannot be aware of fluoride in the drinking water because fluoride is tasteless. We confirm that the choice to move is not dependent on the fluoride level in various tests in Table A2 presented in section 12.3 in the appendix.

7.1 Threats to identification

The first threat concerns our use of geological variation in fluoride. Because the bedrock is constant, the fluoride level in the drinking water is also constant. Assume that fluoride is negative for cognitive ability. Given that people are living in the same place, fluoride might have an effect on the regional labor market because people on average have a lower cognitive ability. The individual income will depend on the average wage level. Since the labor market has adjusted to a lower cognitive ability pool, the individual wage level will on average be lower. It can also be the case that the bedrock in itself can affect the labor market. For example, specific bedrock might be more suitable for mining, which could affect the structure of the regional labor market and, hence, the labor market outcome for a specific individual. If we would consider large geographical areas and use the variation between these areas, fluoride might not be independent of the outcome variables. Figure 5 illustrates the main identification problem in this setting using the long-run outcome *income* as an example.

Figure 5. Relationships between the bedrock, fluoride level, cognitive ability and income



If our identification strategy relied on between-municipality variation, this would have been a concern. The key to identifying the causal effect of fluoride exposure is to have small geographical units between which there is a variation. We argue that Sweden's approximately 9,000 SAMS are sufficiently small and that fluoride is independent of the outcome between these small areas. A labor market region is larger than a SAMS. Given the use of SAMS level data, the red dashed lines in Figure 5 are blocked.

A second threat to identification would be that municipalities deliberately provide certain SAMS with fluoridated water because municipalities have some inside information about the dangers of fluoride. We demonstrate in Table A3, A4 and A6 in the appendix that this is not the case. There is no evidence that the provided drinking water fluoride level is dependent on predetermined characteristics in any clear way.

A third threat concerns self-selection for the outcome variables. There are missing values for the cognitive and non-cognitive test taken during conscription. There are also some missing values for individuals that have wrote the math test on the national test in ninth grade. Imagine that fluoride is negative for cognitive ability and that some individuals as a result of being exposed to lower levels of fluoride have a possibility to avoid conscription or the math test because they are more intelligent. We would then have self-selection into who is taking the conscription test or the math test. In Table A7 in the appendix, we demonstrate that this is not the case. Whether or not we have a result from the cognitive or non-cognitive ability test or the math test does not depend on the individual fluoride treatment level.

The fourth threat is about biological inheritance of cognitive ability. Assume that fluoride is negative for cognitive ability and that cognitive ability affected by fluoride can be passed on to the offspring. The effect of fluoride on the cognitive ability of the offspring is then an inherited factor, resulting in an overestimation of the effect of fluoride exposure in the present generation. This line of thought requires that environmental cognitive factors can be transmitted. The field of epigenetics concerns environmental factors that can switch genes on and off, and then be transgenerationally transmitted. Fluoride can be stored within the body which may *potentially* switch genes on or off that are related to cognitive ability. We test if such a transmission effect is present by also running all of our specifications for adoptees only. Adoptees have not inherited genes from their adoptive parents, so the effect of fluoride in this case purely stems from variation in fluoride exposure between birth and age 16 in the present generation. We discuss this in the robustness analysis.

The fifth threat to identification is related to nurture. Assume that parents exposed to high levels of fluoride develop lower cognitive ability resulting in bad parenting skills, which in turn affects our measure of cognitive ability in the present generation. Luckily, we have a rich set of generational covariates where we can control for fathers' cognitive and non-cognitive ability measured in the same way during their enlistment. We also have covariates for parents' income and education. We can thus control for nurture effects.

7.2 Econometric set-up

The fluoride level for each individual is a weighted average for the number of years a person lived within a specific SAMS. For non-movers, their fluoride level is simply the fluoride level for their SAMS between birth and age 16. People may thus have lived in the same SAMS, moved between SAMS within a municipality, or moved between municipalities. We include municipality fixed effects for where the person was born since there are several differences between municipalities that may also be determinants for our outcomes. To control for age effects we include cohort fixed effects. In addition, we add municipality fixed effects for place of residence in 2013 when we measure income and employment status, since the wage structure and the possibility of employment differs throughout Sweden. We also run two subsample specifications. Those who move could experience multiple treatments; for example, a person moving to a different municipality changes school. In the first sub-sample specification, we analyze the effect of fluoride for the non-movers only, i.e., individuals who have lived in the same SAMS from birth up until age 16. In the second specification, we analyze only those who move within a municipality but between different SAMS at least once between birth to age 16.

We estimate the following regression equation:

$$Y_i = \beta_0 + \beta_1 X_i + \beta_2 W_i + \beta_3 W_s + \beta_4 W_p + \tau_m + \gamma_m + \lambda_c + u_i \qquad (1)$$

where Y_i is the outcome variable measured at the individual level (except for dental outcomes where it is measured for each SAMS and cohort). X_i is the amount of individual fluoride exposure, taking into account moving, for each individual. W_i is a vector of covariates on the individual level. We also include aggregated covariates on SAMS level, W_s to control for peer effects. W_p designates parental covariates. τ_m designates birth municipal fixed effects, γ_m equals municipal fixed effects in 2013 and λ_c designates cohort fixed effects. u_i is the error term. β_1 is the treatment effect of interest. The reader should note that we run several specifications where we add covariates and fixed effects sequentially. For cognitive ability, non-cognitive ability and math points, we never include municipal fixed effects in 2013 since these outcomes are measured at an earlier age.

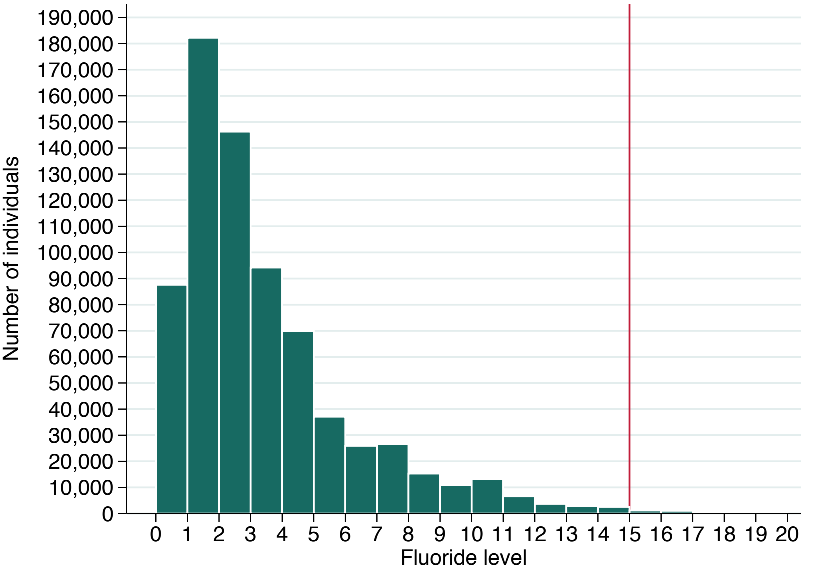
Most SAMS do not have a water plant within the borders, meaning that the fluoride level that we assign to a SAMS is not independent on the fluoride level of the other SAMS within the same municipality. Therefore, we choose to cluster the standard errors on the birth municipal level because municipalities are responsible for the drinking water. In addition, we also calculate standard errors clustered at the local labor market region in accordance with the definitions from Statistics Sweden.[18] In a third standard error specification, we calculate spatial adjusted standard errors in line with Conley (1999) and use 10 kilometers as a spatial cut-off. These standard errors are based upon Euclidian distance, and the clustering structure is specified to last up until 10 kilometers from the center point of each SAMS. It can be argued that geographical distance is a more natural clustering level since individuals living far from each other are less dependent than those who live close, in comparison to municipalities and labor market regions who are administrative constructed entities.

[18] There are 73 local labor market regions in Sweden which are statistical areas for commuting regions. These standard errors are based upon place of residence in 2013 and we only estimate them when we look at personal income and employment status in 2013.

7.3 Descriptive statistics

In this subsection we present descriptive statistics. Figure 6 presents a histogram of the frequency of individuals who are treated with the corresponding level of fluoride, expressed in 0.1 mg/l. The level displayed in the histogram is the actual individual treatment level taken into account moving patterns between different SAMS and municipalities. The WHO recommendation of maximum 1.5 mg/l in the drinking water is marked with a red line.[19]

Figure 6. Histogram of fluoride levels below 2 mg/l (in 0.1 mg/l)



In Table 1, we present some detailed descriptive statistics of the standard deviation in fluoride levels within and between municipalities.

[19] Those few cases above 1.5 mg/l originates from the earlier exceptions for higher levels mentioned in the data section. We cut the histogram at 2 mg/l because there are so few observations above 2 mg/l.

Table 1. *Standard deviation decomposition of fluoride*

	Mean	SD
Fluoride (0.1 mg/l)	3.53	
Overall		3.25
Between		2.95
Within		1.89
Observations	8,597	

Notes: Between and within variation on municipal level.

Table 2 presents the mean and standard deviations for our five main outcomes of interest. The equivalent Table A1 for dental outcomes can be found in the appendix. Cognitive and non-cognitive ability are only measured for men and are centered on 5 with a standard deviation of about 2, which follows the Stanine definition. 70 percent of the individuals in our sample are employed, which is close to the population share of employed. The maximum number of points on the math test is 45, and the mean is about 26 points.

Table 2. *Descriptive statistics of main outcome variables*

	Mean	SD
Annual income in SEK	164,173	134,308
Employment status	0.70	0.46
Cognitive ability	5.02	1.93
Non-cognitive ability	4.75	1.82
Number of basic points math test	26.19	8.57

Table 3 presents descriptive statistics of the covariates. The sample is balanced on gender (49 percent women). More than 90 percent have at least high school education in 2013. Only 5 percent is married, which is not surprising given that the individuals in the sample are relatively young. We also include covariates for parents' level of education and income (mean real wage between 1985-2013) for the parents, and whether they are immigrants. Income for the parents are specified as log income in the regressions, but displayed as real income in Table 3.[20] We are also able to include cognitive and non-cognitive ability from the enlistment

[20] Böhlmark and Lindquist (2005) find that current income is not as good measure of lifetime income as the widespread use would imply. See also the discussion in Engström and Hagen (2015). To minimize bias we use all available years of income for the parents.

for the father as covariates. However, the enlistment data starts 1969 so older fathers are not included. To capture peer-effects, we measure the mean education among individuals included in the data for each cohort and SAMS for three time points. We measure the individuals education as grown-ups in 2013 and then aggregate for each cohort and SAMS for where the individuals were born, where they started school (at 7 years of age) and where they lived at age 16. We include a dummy for whether an individual has graduated from high school when we estimate the effect on income and employment, but not when measuring cognitive ability, non-cognitive and the number of math points since these are measured before graduation.[21] We have grouped our covariates into two groups: Small set and Large set. Table 3 therefore also indicates which covariate is included in each group.

Table 3. *Descriptive statistics of covariates*

	Mean	SD	Outcomes	Set
Gender	0.49	0.50	All	Small
Individual at least high school	0.91	0.28	Income, employment	Small
Marital status	0.05	0.22	All	Large
Father at least high school	0.82	0.39	All	Large
Father's income	240,961	149,723	All	Large
Father's cognitive ability	5.07	1.90	Not non-cog. ability	Large
Father's non-cognitive ability	5.15	1.75	Not cog. ability	Large
Father immigrant	0.09	0.29	All	Large
Mother at least high school	0.89	0.32	All	Large
Mother's income	155,566	85,238	All	Large
Mother immigrant	0.10	0.30	All	Large
Both parents immigrants	0.05	0.21	All	Large
Cohort education (birth)	11.89	0.59	All	Large
Cohort education (school start)	11.89	0.60	All	Large
Cohort education (age 16)	11.89	0.60	All	Large
Observations	729,850			

Notes: Explanatory variables used in the estimations. Small set covariates are also included in the large set covariates. Cohort education variables (last three in the table) are means for cohorts and SAMS.

[21] Whether to graduate or not from high school could be a bad control. However, whether an individual graduates from high school is influenced by several other factors than cognitive ability and at the same time, graduation from high school is important for later labor market status. Therefore, we choose to include it when studying income and employment status.

8 Results

In this section we present the results. We start by looking at the effects on dental health, and then present the results for our main outcomes. We have both analyzed linear effects and non-linear effects of fluoride for our main outcomes. This section is ended with a comparison with earlier studies on fluoride.

8.1 Effects of fluoride on dental health

If our strategy of mapping statistics from water plants to individual register data on the SAMS level has worked, we expect to see a positive effect of fluoride on dental health. We have dental outcomes for each cohort for each SAMS. The average number of individuals in a SAMS per included cohorts in our dental data set is approximately 16.

We have a set of variables that measure various dental outcomes. We present the results for a subset of these variables below that we judged was closely related to fluoride. The results for all additional outcomes are presented in section 12.4 in the appendix. The variables we focus on here are visits to a dental clinic, tooth repairs, disease evaluation, prevention and treatment and root canal. Given that fluoride is good for dental health, we expect to find negative estimates for these variables. All these variables are expressed as share in percentage points; for example the share of 20 years old in in a given SAMS that had a tooth repaired during a year. For a more detailed description about the variable abbreviations we use for the outcome variables in this section, see Table A1 in the appendix.

We divide our regression results into two separate tables. In Table 4 we run unweightened regressions where we look at the connection between fluoride and the aggregated measure of these six variables on the SAMS level. We have two data years available and the analysis for both years are presented in Table 4. For this analysis, we focus on the 20 years olds which is the earliest cohort available. We can be more sure that the 20 years olds have not moved from a given SAMS in comparison to later cohorts. In Table 5 we run weightened regressions where we work with our full data set. In this case, each individual has a unique fluoride treatment depending on moving patterns (age 0 to age 16) and the aggregated fluoride level on the SAMS level thus corresponds to those living in a SAMS.[22] For this analysis, individuals from cohorts in the data analysis for the main outcomes are included. We present the results for 2013 in Table 5. The reader should consider the results presented in Table 4 as a test whether our strategy mapping water plant

[22]SAMS is not yet available for 2013 LOUISE data set. We have used SAMS for the individual in 2011 in this case.

statistics to residence data on the SAMS level has worked. The results in Table 5 should be considered as a test whether the combined geographical water-plant-mapping and the variation stemming from individuals' moving patterns captures what we want to measure, namely fluoride treatment.

Table 4. *Dental outcomes*

F.	Visit	Repair	RiskEvaluation	DiseasePrevention	DiseaseTreatment	RootCanal
2013	-0.656**	-0.337***	-0.689**	-0.845*	-0.350**	-0.0292*
	(0.299)	(0.110)	(0.302)	(0.431)	(0.139)	(0.0173)
2008	-0.637**	-0.229***	-0.678**	-0.435*	0.110	-0.0300
	(0.293)	(0.0684)	(0.320)	(0.224)	(0.106)	(0.0198)

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. First row is for 2013, and the second row for 2008. The number of observations for the year 2013 is 7,622. The number of observations for the year 2008 is 7,606. Fluoride expressed in 0.1 mg/l. The dependent variable is displayed at the top of each column.

Table 4 clearly displays a negative effect of fluoride level for these outcomes except for one point estimate. The point estimates are large and often statistically significant. If we take the first estimate in Table 4 as an example, the share of visits is decreased by approximately 6.6 percentage points if fluoride is increased by 1 mg/l. This should be considered as a large effect. The outcome that should be closest related to fluoride is tooth repair, which is display in column 2. If fluoride would increase with 1 mg/l, the share of 20 years old that had a tooth repaired would be decreased approximately 3.4 percentage points. Again, this effect is large, especially considering this cohort. 20 years old should on average have healthy teeth, but we still find these effects of fluoride. Root canal treatment is generally a treatment for more serious conditions caused by caries. We find a negative point estimate for this outcome (which is expected), but only one coefficient is statistically significant on the 10 percent level. This is again expected given that root canal treatment should be generally rare among those who are 20 years old. DiseaseTreatment is non-significant and positive for 2008, but is statistically significant, negative and large for the 2013 sample. It is important to note that comparisons across the years should not be done with this data, since definitions of treatments and diagnostics have somewhat altered across the years.

Table 5. *Dental outcomes*

F.	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Visit	-0.2592*	-0.0639**	-0.0198	-0.0038	0.0059	-0.0071	-0.0023
	(0.1472)	(0.0314)	(0.0309)	(0.0299)	(0.0239)	(0.0258)	(0.0264)
Repair	-0.0666	-0.0522***	-0.0490**	-0.0516**	-0.0575***	-0.0506**	-0.0562***
	(0.0536)	(0.0188)	(0.0249)	(0.0245)	(0.0207)	(0.0222)	(0.0204)
RiskEvaluation	-0.2709*	-0.0645**	-0.0202	-0.0036	0.0068	-0.0058	-0.0006
	(0.1546)	(0.0327)	(0.0314)	(0.0304)	(0.0239)	(0.0259)	(0.0267)
DiseasePrevention	-0.4409*	-0.0965**	-0.0742*	-0.0651*	-0.0406**	-0.0312	-0.0305
	(0.2514)	(0.0410)	(0.0380)	(0.0365)	(0.0204)	(0.0242)	(0.0243)
DiseaseTreatment	-0.0611	-0.0196	-0.0106	-0.0120	-0.0200	-0.0271	-0.0285
	(0.0886)	(0.0276)	(0.0237)	(0.0237)	(0.0203)	(0.0223)	(0.0224)
RootCanal	-0.0031	-0.0085**	-0.0108*	-0.0139**	-0.0152***	-0.0108*	-0.0131**
	(0.0110)	(0.0041)	(0.0060)	(0.0059)	(0.0054)	(0.0057)	(0.0058)
Small set covariates	No	No	No	No	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	No	Yes
Fe. birth muni.	No	No	Yes	Yes	Yes	Yes	Yes
Fe. cohort	No	No	No	Yes	Yes	Yes	Yes
Fe. muni. 2013	No	Yes	No	No	Yes	Yes	Yes
Sample	All	All	All	All	All	Col 7	All

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Outcomes on each row. The number of observations ranges between 473,624 (col 6 and 7) and 727,543.

The results presented in Table 5 point in the same direction as the ones in Table 4, but the point estimates are generally smaller in size. The reason for this is probably because people have moved between age 16 and 2013 when we measure dental health, meaning that these results are a bit noisier. The fluoride treatment we consider only take place between birth and age 16 since we want to mimic the specifications for dental health with our main results in the next section. The share of repairs is the most well-defined variable where we really expect to find an effect, and the results for this variable are stable across different specifications and points in the expected direction. If we consider column 7 where all covariates and fixed effects are included, the share of individuals that had a tooth repaired would decrease by approximately 0.6 percentage points if their individual fluoride treatment level between birth and age 16 increased by 1 mg/l. This effects is smaller than the one found in Table 4, but still large considering that fluoride needs to be applied continuously to the teeth. What our results indicate – which is interesting in itself – is that early fluoride treatment has long run positive effects on dental health even if we do not consider fluoride treatment after age 16. Root canal treatment is now often statistically significant, which is expected since we have included older cohorts. The reader should note that some of the specifications are very demanding. When we include all fixed effects we compare individuals that are born in the same municipality, in the same cohort and that lives in the same municipality in 2013. Although the point estimates are not always statistically significant, they almost always points in the expected negative direction. In the appendix, the reader may find results for additional outcomes and the equivalent results for the 2008 sample in Tables A9, A10 and A11. Note that for 2008, we cannot include all cohorts since the later cohorts were not above 20 in 2008.

We can conclude that the coefficients for the 2008 specification are generally smaller in size and less precisely estimated, where we have fewer negative (statistically significant) results. We can only speculate why this is the case, but a reform was implemented in July 2008 that gave 20-29 years old a special dental care benefits. In comparison to other health care services in Sweden, the patient pays a much larger share for him/herself for dental care. Given that people in their 20's usually have lower incomes, the benefit probably allowed people between 20 and 29 to visit the dentist regularly, which could potentially explain that the results are less clear for the 2008 sample simply because individuals were refrained in economic terms to seek dental care before the reform. The specifications for 2008 are also based on fewer observations.

The overall conclusion after considering the results in Table 4, 5 and the additional results presented in the appendix is that out mapping strategy seems to have worked. Generally, we find negative and often statistically significant results for fluoride on these outcomes; especially if we consider the 2013 sample.[23]

8.2 Main results

In this subsection we present our main results. We begin by looking at cognitive ability, non-cognitive ability and points at the math test taken in ninth grade. Then we move on and investigate the effect of fluoride on more long-term outcomes where we look at income and employment status. We begin by estimating linear effects of fluoride. There are, however, reason to believe that the effect may be non-linear, and that fluoride becomes dangerous above a certain level.[24] We estimate the non-linear effects in the next subsection.

[23] For two of the variables, we find results that point in the opposite directed that we expected for some of the specifications. These variables are median of intact teeth and median of remaining teeth. See the results in the appendix. We expect to find positive point estimates for these variables. After further consideration, we conclude that these outcomes are not suitable for this age group. Wisdom teeth are developed in this age, meaning that the median of remaining and intact teeth are mostly influenced by another factor than fluoride, namely wisdom teeth incidence.

[24] This is why WHO has a recommendation of max 1.5 mg/l fluoride in the drinking water.

Let us begin with cognitive ability, measured in a Stanine scale. In this case we only include males in our specifications. In the table below we present the point estimates for fluoride and two types of standard errors. The first standard error in parenthesis is clustered on the birth municipality. The standard errors in curly brackets are spatial adjusted standard errors in line with Conley (1999). The first column does not include any covariates and or fixed effects. In the following two columns we add fixed effects. When we include covariates for fathers' cognitive ability our sample is reduced since we only have data on fathers' cognitive ability from 1969. To make the samples comparable with and without the covariates we run column 4 with the same sample as if we had included covariates which we do in column 5. We run two subsample analyses where we only focus on those individuals that have not moved from a municipality between birth and age 16. In column 6, we run an analysis for those who have lived in the same SAMS area in a municipality for the entire period 0-16. In column 7 we restrict our sample to those who have moved, but only within a municipality.

Looking at the point estimates, they are all very small and often not statistically significant different from 0. Sometimes the point estimates are negative and sometimes they are positive, but always very close to 0. Fluoride is expressed in 0.1 mg/l. If we take the point estimate from column 5, which is equal to 0.0073, this means that cognitive ability is increased by 0.073 Stanine points if fluoride is increased by 1 mg/l (a large increase in fluoride). This should be considered as a zero effect on cognitive ability. A Stanine point roughly equals 6-8 IQ points.[25]

Table 6. *Cognitive ability*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fluoride (0.1 mg/l)	-0.0083	-0.0024	-0.0024	-0.0015	0.0073	0.0035	0.0231
	(0.0081)	(0.0051)	(0.0051)	(0.0052)	(0.0039)*	(0.0050)	(0.0081)***
	{0.0085}	{0.0045}	{0.0045}	{0.0051}	{0.0041}*	{0.0050}	{0.0092}**
Mean	5.0088	5.0088	5.0088	5.0240	5.0240	5.0851	4.9224
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	Col 5	All	SAMS stayers	SAMS movers
R^2	0.0002	0.0215	0.0238	0.0281	0.1786	0.1672	0.1906
Observations	82,010	82,010	82,010	51,322	51,322	21,348	18,848

Notes: Standard errors in parenthesis are clustered at the municipal of birth. Standard errors in curley brackets are Conley standard errors with a cut-off of 10 km, centered on each SAMS. *** p < 0.01, ** p < 0.05, * p < 0.1.

Let us move on to non-cognitive ability. The point estimates are once again very close to 0 and often not statistically significant. If we do the same calculation as before with an increase in fluoride by 1

[25] IQ measure with population mean of 100 and a standard deviation of 15. See Öhman (2015)

mg/l, the non-cognitive score would increase by 0.171 Stanine points according to column number 5. In this column, the point estimate is actually statistically significant, but the result should be interpreted as a negligible effect because of the very small estimated coefficient. In economic terms, the effect is zero.

Table 7. *Non-cognitive ability*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fluoride (0.1 mg/l)	0.0026	0.0059	0.0059	0.0110	0.0171	0.0087	0.0347
	(0.0057)	(0.0047)	(0.0047)	(0.0052)**	(0.0054)***	(0.0067)	(0.0150)**
	{0.0053}	{0.0044}	{0.0044}	{0.0052}**	{0.0050}***	{0.0064}	{0.0129}***
Mean	4.7341	4.7341	4.7341	4.7751	4.7751	4.9133	4.6873
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	Col 5	All	SAMS stayers	SAMS movers
R^2	0.0000	0.0173	0.0175	0.0211	0.0791	0.0761	0.0998
Observations	66,561	66,561	66,561	41,730	41,730	17,408	15,159

Notes: Standard errors in parenthesis are clustered at the municipal of birth. Standard errors in curley brackets are Conley standard errors with a cut-off of 10 km, centered on each SAMS. *** p < 0.01, ** p < 0.05, * p < 0.1.

For the next outcome variable – the number of points at the math test taken in the ninth grade – we have data for both males and females. In this case we also have data for additional cohorts in comparison to the first two outcomes. The average score was approximately 26. All of the point estimates are negative in this case and some of the estimated coefficients are statistically different from zero. The size of the point estimates are, however, very small. In the first four columns we have more than 500,000 observations so it is not surprising that some of our results are statistically significant. The important part is economic significance. Let us focus on column 6 where we have included all covariates and all fixed effects. If fluoride is increased by 1 mg/l (again, this is a large increase), the number of points on the math test should decrease by approximately 0.1 points. This decrease is less than 0.5 percent of the average number of points on the test which was 26 points. In economic terms, this effect should be considered as a zero effect.

Table 8. *Math points*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fluoride (0.1 mg/l)	-0.1029	-0.0295	-0.0268	-0.0268	-0.0437	-0.0110	-0.0208	-0.0180
	(0.0354)***	(0.0126)**	(0.0125)**	(0.0125)**	(0.0144)***	(0.0136)	(0.0132)	(0.0237)
	{0.0355}***	{0.0116}**	{0.0115}**	{0.0115}**	{0.0128}***	{0.0102}	{0.0118}*	{0.0189}
Mean	26.2100	26.2100	26.2100	26.2100	26.4943	26.4943	27.2265	26.0476
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	All	Col 6	All	SAMS stayers	SAMS movers
R^2	0.0013	0.0230	0.0404	0.0404	0.0431	0.1709	0.1487	0.1826
Observations	500,995	500,995	500,995	500,995	337,404	337,404	139,276	127,334

Notes: Standard errors in parenthesis are clustered at the municipal of birth. Standard errors in curley brackets are Conley standard errors with a cut-off of 10 km, centered on each SAMS. *** p < 0.01, ** p < 0.05, * p < 0.1.

We may thus conclude that fluoride does not have a negative effect on cognitive development. The last two tables include outcomes which are more long-term: Log annual income and employment status in 2013. These are the outcome variables for which we have the largest number of observations. Given the zero results for the three variables above, we do not expect to find a negative effect on these long-term outcomes. It is, however, possible that fluoride has a positive effect, because of better dental health for the individuals. In the two tables below we add an additional standard error calculation where the standard errors in brackets are clustered at the local labor market area in 2013. We also add an additional set of municipal fixed effects for where the individual lives in 2013.

Looking at log income, we have often statistically significant point estimates and the coefficients are always positive. If we look at column 6, the point estimate equals 0.004, meaning that income increases by 4 percent if fluoride increases by 1 mg/l. This is not a negligible effect and the estimate should be considered as economically significant. This indicates that fluoride improves labor market outcomes through better dental health. One interpretation could be that better looking teeth is a positive factor on the labor market. Our estimate is in line with Glied and Neidell (2010), who find that women who drinks fluoridated water on average earn 4 percent more.

Table 9. *Annual log income in SEK*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fluoride (0.1 mg/l)	0.0068	0.0064	0.0051	0.0052	0.0042	0.0040	0.0026	0.0029
	(0.0032)**	(0.0015)***	(0.0014)***	(0.0013)***	(0.0012)***	(0.0010)***	(0.0016)	(0.0031)
	[0.0018]***	[0.0017]***	[0.0016]***	[0.0009]***	[0.0010]***	[0.0010]***	[0.0016]	[0.0029]
	{0.0031}**	{0.0010}***	{0.0010}***	{0.0010}***	{0.0010}***	{0.0009}***	{0.0014}*	{0.0024}
Mean	11.7798	11.7798	11.7798	11.7798	11.7942	11.7942	11.8449	11.7835
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE, year 2013	No	No	No	Yes	Yes	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	All	Col 6	All	SAMS stayers	SAMS movers
R^2	0.0004	0.0078	0.0594	0.1004	0.1028	0.1121	0.1277	0.1096
Observations	628,732	628,732	628,732	628,732	415,341	415,341	172,669	155,980

Notes: Individuals with a yearly income below 1,000 SEK are excluded. Standard errors in parenthesis are clustered at the municipal of birth. Standard errors in brackets are clustered at the local labor market area defined by Statistics Sweden (SCB). Standard errors in curley brackets are Conley standard errors with a cut-off of 10 km, centered on each SAMS. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Let us continue to the last outcome. Employment status is a dummy variable taking the value 1 if the individual is defined as employed in 2013. In column 6, the point estimate for fluoride is 0.0012 and statistically significant. If fluoride is increased by 1 mg/l, then the probability that the person is employed is increased by 1.2 percentage points. This result thus point in the same direction as the results for log income where both these results are significant in economic terms.

Table 10. *Employment status*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fluoride (0.1 mg/l)	0.0026	0.0025	0.0022	0.0022	0.0013	0.0012	0.0005	0.0000
	(0.0012)**	(0.0007)***	(0.0007)***	(0.0006)***	(0.0006)**	(0.0005)**	(0.0007)	(0.0013)
	[0.0007]***	[0.0007]***	[0.0007]***	[0.0004]***	[0.0004]***	[0.0005]***	[0.0005]	[0.0011]
	{0.0012}**	{0.0005}***	{0.0005}***	{0.0004}***	{0.0005}***	{0.0004}***	{0.0006}	{0.0010}
Mean	0.7019	0.7019	0.7019	0.7019	0.7147	0.7147	0.7420	0.7109
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE, year 2013	No	No	No	Yes	Yes	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	All	Col 6	All	SAMS stayers	SAMS movers
R^2	0.0003	0.0073	0.0326	0.0688	0.0689	0.0794	0.0830	0.0789
Observations	729,850	729,850	729,850	729,850	475,414	475,414	192,740	179,374

Notes: Standard errors in parenthesis are clustered at the municipal of birth. Standard errors in brackets are clustered at the local labor market area defined by Statistics Sweden (SCB). Standard errors in curley brackets are Conley standard errors with a cut-off of 10 km, centered on each SAMS. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

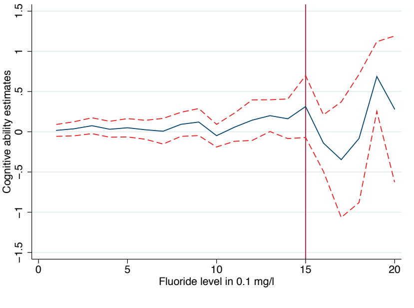
In conclusion, we find zero effects on cognitive and non-cognitive ability. We also find zero effects for the number of math points. These results indicate that fluoride does not have adverse negative effect on cognitive development for the fluoride levels we consider. We discuss our zero results in a separate subsection below. We also find that fluoride has positive effects on log income and employment status which could indicate that better dental health is a positive factor on the labor market. These results are in line with our previous results for dental health where we found that fluoride seems to results in better dental health in this age group.

8.3 Non-linear effects

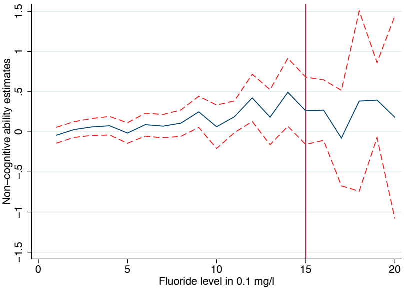
There are reasons to believe that a potential neurotoxic effect of fluoride on the central nervous system is not linear. As with many toxic compounds, small amounts do not yield any dramatic damage, but the effects manifest itself above a certain threshold. We therefore continue our analysis and look for non-linear effects.

In Figures 7-9 the effect for each fluoride level is displayed. We have created dummy variables taking the value 1 for each 0.1 fluoride level and then included these in a regression. When we run the regressions, all fixed effects and all covariates are included just as in column 6 in the tables above. We then plot the effect for each 0.1 mg/l in a figure. Fluoride in our data is between 0 and 4 mg/l, but we have very few observations above the threshold level of 1.5 mg/l, meaning that the estimated effect is very noisy. In the figures below, we have therefore cut the individual fluoride treatment level at 2 mg/l. The blue lines in the figures are the plotted point estimates and the red dashed lines are 95 % confidence intervals. The conclusion is that the effect up until 1.5 mg/l is always close to zero. In line with the earlier results for log income and employment status, the line in the figures below seems to increase when closing on 1.5 mg/l, which indicate a positive effect of fluoride through dental health for higher levels. In line with the main analysis, the point estimates for the number of math points are sometimes statistically significant. The size of the point estimates are small, and the effect does not seem to be significant when considering fluoride levels close to 1.5 mg/l, which we would expect if fluoride had a negative effect on cognitive development.

Figure 7. Non-linear effects for ability measures



(a) Cognitive ability estimates



(b) Non-Cognitive ability estimates

Figure 8. Non-linear math points estimates

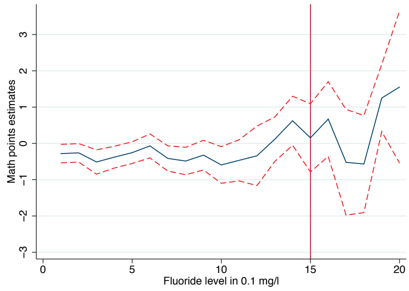
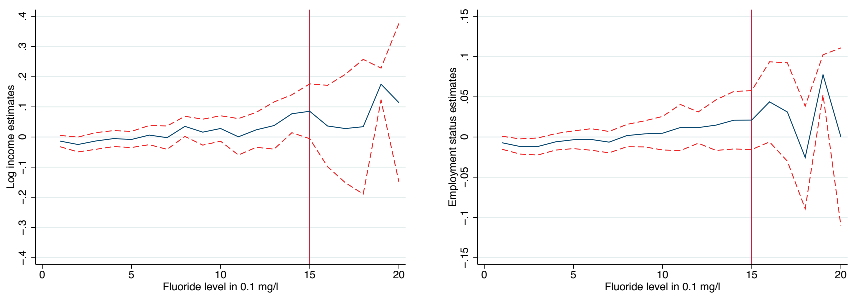


Figure 9. Non-linear effects labor market outcomes



(a) Log income estimates

(b) Employment estimates

In section 12.5 in the appendix, we also present regression tables where we run the regressions with dummy variables for each quartile value in the fluoride distribution. In the tables, we run the exact same specifications for each outcome variable as in the tables in the last section when we looked at linear effects. The conclusion is, again, that there are no indications that fluoride has an effect other than zero for cognitive ability, non-cognitive ability and math points. For cognitive ability and math points, we have some statistically significant, negative point estimates for the third quartile dummy. For the fourth quartile however, the point estimates are insignificant for all specifications which we expect if fluoride does not have a negative effect on these outcomes. With regard to log income and employment status, we find positive and statistically significant results for the fourth quartile, which again points towards the explanation that fluoride has a positive effect thorough dental health – especially for higher levels of fluoride.

8.4 Comparison with earlier studies

Are our estimated results for cognitive ability really zero? One way to evaluate a zero-result is to look at earlier studies which have found statistically significant results and compare the precision of the estimates. In the table below, we have summarized the results for the reviewed papers in Choi et al. (2012). We have only included the papers which study fluoride levels that are roughly equal to the levels we consider. Because earlier papers only have considered cognitive ability, we can only compare this outcome variable. To make our results comparable to the other papers, we have normalized cognitive ability around 0. The reader should note that we have not read the original articles since most of them are printed in Chinese or Persian. Instead, the comparison below is based on Choi et al. (2012).[26]

Table 11. *Comparison with earlier studies*

Study	Obs.	*F.*	CI 95 %
Our study: No cov. or f.e.	82,010	0.05-4.10	-0.1262, 0.0399
Our study: Cov. and f.e.	51,322	0.05-4.10	-0.0023, 0.0781
Chen et al. (1991)	640	0.89-4.55	-0.41, -0.10
Lin et al. (1991)	119	0.34-0.88	-1.01, -0.28
Xu et al. (1994)	129	0.80-1.80	-1.35, -0.52
Yang et al. (1994)	60	0.50-2.97	-1.01, 0.02
Li et al. (1995)	907	1.02-2.69	-0.70, -0.39
Zhao et al. (1996)	320	0.91-4.12	-0.76, -0.31
Yao et al. (1997)	502	0.40-2.00	-0.61, -0.25
Lu et al. (2000)	118	0.37-3.15	-0.98, -0.25
Hong et al. (2001)	117	0.75-2.90	-0.85, -0.03
Wang et al. (2001)	60	0.50-2.97	-1.01, 0.02
Xiang et al. (2003)	512	0.18-4.50	-0.82, -0.46
Seraj et al. (2006)	126	0.40-2.50	-1.28, -0.50
Li et al. (2009)	80	0.96-2.34	-0.94, 0.08
Poureslami et al. (2011)	119	0.41-2.38	-0.77, -0.04

Notes: F is fluoride level in mg/l. This table consists of the results of comparable studies presented in Table 1 and Figure 2 on page 1364-1366 in Choi et al. (2012). Note that these studies have not considered a continuous measure of fluoride.

In contrast to earlier papers, our study is based on a much larger data sample and our point estimates are much more precise. Earlier papers

[26] Since we have not read the original research articles, we do not cite them in the reference list. See Choi et al. (2012) for details about these papers.

have found negative and statistically significant effects in many cases, but our results are always very close to 0. Our 95 % confidence intervals includes the zero both with or without fixed effects and covariates.

Broadbent et al. (2015) also claim to find a zero-result. Their confidence intervals are, however, much broader than ours. They estimate a 95 % confidence interval for the effect of living in a high fluoride (0.7-1 mg/l) area in comparison to those living in a low fluoride area (0-0.3 mg/l) on cognitive ability (with covariates) to be (-3.49, 3.20) for those between 7 and 13 years old and between (0.02, 5.98) for those at age 38. In this case, cognitive ability is measured in IQ points with a mean of 100. If we translate our estimates to IQ points, roughly by replacing the Stanine scores with the corresponding IQ[27], our confidence intervals are (-1.8084, 0.5735) for the specifications without covariates or fixed effects and (-0.0362, 1.1131) for the specifications with all covariates and fixed effects.

Based on the assessment of the earlier literature, we are confident to claim that we have estimated a zero effect on cognitive ability.

9 Robustness analysis

In this section we discuss the results from various robustness checks.

First we address the potential threat to our identification strategy that fluoride as an environmental factor can switch certain genes on and off in accordance with the idea in epigenetics. To test if this is a problem, we rerun all our specifications only including individuals that were adopted in section 12.6. Their place of residence and fluoride treatment between birth and age 16 is thus that of their adoptive parents who they do not share an inherited factor with. The regression tables are presented in the appendix. The estimates point somewhat in the same direction as the ones in the main analysis, but the point estimates are not statistically significant. The reader should note that we have a much smaller sample for this robustness check.

We use a mapping protocol to assign water plant data on fluoride in the drinking water to SAMS. Since we cannot observe the exact coordinate where an individual lives, we will have some measurement error with regard to those who drink water from a private well. All we know is if an individual live in a specific SAMS for a given year.[28] The probability that an individual consume the drinking water provided by the municipality should increase when the SAMS is small and/or when the distance from the water plant to the center of the SAMS is small.

[27]See Table 1 in Öhman (2015).

[28]In a theoretical scenario where we have severe measurement error, we would have bias in our estimates towards 0. This is not likely given our results for dental health.

Smaller SAMS equals more densely populated areas. We have run all of our specifications in section 12.7 and 12.8 in the appendix where we look at subsamples in our data for various sizes of SAMS and various distances between the nearest water plant and the center point of the SAMS. We have plotted these estimates in graphs presented in the appendix. In conclusion, the point estimates does not seem to differ in a systematic way when just considering smaller SAMS and shorter distances.

We do not have water statistics for each year from 1985 for all municipalities. We have therefore contacted all municipalities and asked them if they have changed their water sources after 1985. Because the bedrock is constant, they level of fluoride should also be constant from 1985 if the water source is the same. All municipalities do not have exact information regarding their water sources and we have not received confirmation from all of them. In section 12.9 in the appendix, we also run a specification including only those municipalities where we have data from 1985 or where we have received a clear confirmation that the municipality has not changed their water sources after 1985. The results for cognitive and non-cognitive ability are in economic terms still zero. The estimated coefficients for math points are negative and sometimes statistically significant (as in the main analysis), but very small in size. For log income and employment status, we find positive and sometimes statistically significant results as in the main analysis, but the estimated coefficients are generally smaller in magnitude in this specification.

We include cohorts born between 1985 and 1992 in our main analysis. This could be problematic when we estimate the effect of fluoride on employment status and income, because those who are born in 1992 are only 21 years old in 2013 (the year we measure these two outcomes). Younger cohorts have not established themselves on the labor market to that high extent. We therefore also run specific analysis only for those born in 1985 in section 12.10. The results point in the same direction as in the main analysis, but the point estimates are not always statistically significant anymore. The size of the point estimates for log income is sometimes larger. This is what we expect given that there is a positive effect of fluoride on log income. The results for employment status are similar to the ones discussed in the main analysis.

We also run a specification where we only look at those SAMS which had one and only one water plant and where we have full information from 1985 from the municipalities in section 12.11. In this specification we only include those who have not moved between birth and age 16. In this case we are left with much fewer observations. For cognitive ability, non-cognitive ability and math points, there is still no evidence of any negative effects. For log income and employment status, the point estimates varies between different specifications and we no longer have statistically significant results. This is probably a result of having fewer observations and thus lower statistical power.

We have also run an analysis for an alternative income measure in section 12.12 in the appendix. In the main analysis we look at a measure for income from employment. In the alternative specification, we run the same analysis for a measure for income from employment and business income (förvärvsinkomst). These results point in the same direction as the ones in the main analysis.

10 Conclusions

We have investigated the effects of fluoride on outcomes related to the central nervous system and more long-term labor market outcomes. We find a zero effect of fluoride on cognitive ability, non-cognitive ability and points on the national test in math. For income and employment status we found evidence of a positive effect of fluoride, which would be in line with the explanation that better dental health is a positive factor on the labor market. We began our analysis by first investigating the dental health effects of fluoride, and could confirm the long well-established positive relationship.

Our paper is to our knowledge the first large scale empirical study with individual register data to assess the effects of drinking water fluoride. Earlier studies, which have found a negative effect of fluoride on cognitive ability, rely on much smaller samples originating from countries with poorer data quality. In addition, these papers have usually not applied credible identification strategies. That said, earlier studies have sometimes focused on higher levels of fluoride than the levels we consider in this paper. It may be that higher levels of fluoride in the drinking water have negative effects on cognitive ability. However, in comparison our paper is more policy relevant for developed countries, because water authorities seldom consider fluoridating the drinking water above 1.5 mg/l. Based on the results we find, the policy implications are that fluoride exposure through the drinking water either in the form of natural levels or artificial fluoridation is a good mean of improving dental health without risking negative side effects on cognitive development.

Future studies should try to establish where the dangerous level of fluoride begins. Since we know that fluoride is lethal and dangerous in very high dosages, it is crucial to find the safe limit for fluoride in the drinking water. Our results indicate that the dangerous level is not below 1.5 mg/l.

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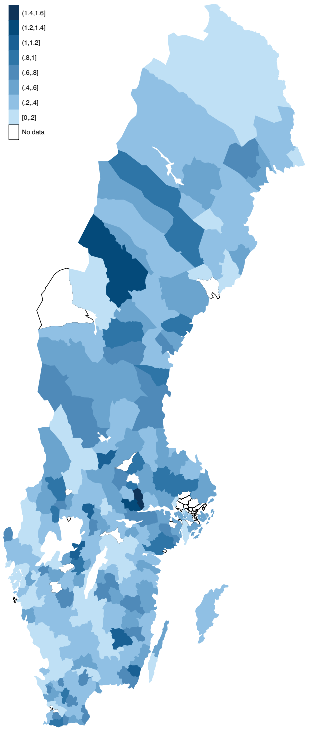
12 Appendix

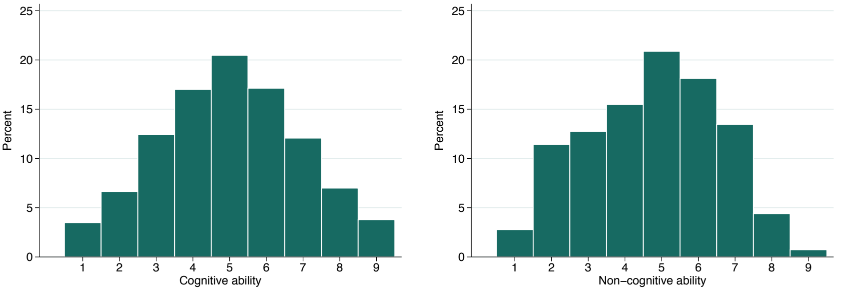
12.1 Exogenous variation in fluoride: geological background

Figure A1. Fluoride levels in Sweden: Variation between municipalities after mapping



12.2 Data: Individual level data

Figure A2. Distribution of cognitive and non-cognitive ability



(a) .

(b) .

Data: Descriptive statistics

Table A1. *Descriptive statistics of dental outcomes*

	Mean	SD	Max	Min
Visits dental clinic	66.31	24.31	100.00	0.00
Basic check-ups	59.42	25.92	100.00	0.00
Risk evaluation, health improvement measures	64.78	24.64	100.00	0.00
Disease prevention	12.82	18.97	100.00	0.00
Disease treatment	31.31	23.21	100.00	0.00
Dental surgical measures	6.33	11.66	100.00	0.00
Root canal treatment	2.75	7.67	100.00	0.00
Orthognathic treatment	1.37	5.50	100.00	0.00
Dental repair	18.85	19.22	100.00	0.00
Prosthesis treatment	0.72	4.04	100.00	0.00
Orthodontics and replacement measures	0.18	2.06	100.00	0.00
Diagnosis: Check-ups and evalutions	64.77	24.64	100.00	0.00
Diagnosis: Dental health improvement measures	9.44	15.31	100.00	0.00
Diagnosis: Treatment of illness and pain	34.93	24.00	100.00	0.00
Diagnosis: Dental repair	22.86	20.67	100.00	0.00
Diagnosis: Habilitation and rehabilitation	0.76	4.05	100.00	0.00
Median remaining teeth	29.52	1.36	32.00	1.00
Median intact teeth	25.87	2.89	32.00	0.00

12.3 Empirical framework: Balance tests

Our identifying variation stems from a geological variation in fluoride and from individuals' moving patterns between birth and age 16. It is important that we verify that people are not moving from and to different SAMS because of the fluoride level. If people were, we would have self-selection into the intensity of treatment meaning that we cannot separate treatment from the outcomes.

Table A2 displays balance tests for moving patterns where each row is a separate regression. Overall, the moving pattern is on average not depending on the individual fluoride treatment level. We run specific balance test for dummy variables taking the value 1 if an individual has moved within a municipality but between SAMS, if the individual has moved between municipalities and if the individual has moved between counties. We also run balance test for the number of moves between SAMS, municipalities and counties and the average number of years within a SAMS, municipality or county. The point estimates are always small and statistically insignificant. If the individual fluoride treatment increases by 0.1 mg/l, the probability that the individual has moved between SAMS within a municipality is 0.49 percentage points lower according to row 1 in Table A2. We have also conducted a comparison in difference in means for first time movers. The mean fluoride level prior of moving was approximately 0.33 mg/l and after moving the mean was 0.34 mg/l. Hence, there is no evidence that people move from high fluoride areas.

Table A2. *Balance test. Moving pattern, individual fluoride treatment level.*

	F. (0.1 mg/l)
Move within municipality	-0.00487 (0.00408)
Municipal Move	0.0000963 (0.00263)
County Move	0.00138 (0.00158)
# moves within municipality	-0.00373 (0.00809)
# moves between municipalities	0.00135 (0.00429)
# moves between counties	0.00239 (0.00224)
Average years SAMS	0.0279 (0.0273)
Average years municipality	-0.000902 (0.0158)
Average year county	-0.00792 (0.00880)
Observations	733,683

Notes: Standard errors clustered at the birth municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Each row is a separate regression, where the dependent variable is displayed on the row. The number of observations refers to the maximum number of observation. For row 1 and 4, we restrict the sample to those who have moved within a municipality, but between SAMS. The number of observations are thus smaller for these two specification.

In Table A3 we investigate whether the municipality provided water is endogenously rerouted to specific groups. We investigate this by running balance test for predetermined characteristics on the SAMS level for where the individual was born.[29] Municipalities may potentially know that fluoride is dangerous, and therefore give such water to groups with lower socioeconomic status. We also investigate whether

[29]We cannot run this for income and education since these are outcomes that we are interested in.

other characteristics are dependent on the fluoride level, such as the size of SAMS or the distance to the water plant. These balance tests address the question whether fluoride is correlated with population density, since less populated areas have larger SAMS. We have also run a test for those municipalities for which we do not have full information about their drinking water from 1985. Table A4 and A5 displays a similar analysis for the years of immigration for the parents. This variable is also predetermined, where we run the balance test for various dummy variables for mothers and fathers respectively. We focus on where the individual was born and calculate the share of immigrants that arrived for each year. All shares are then included into a single regression.

We do not find support of the concerns discussed above. We have statistically significant results on the 10 percent level for the share (expressed between 0 and 1) of immigrants outside the Nordic countries (although not outside Europe), but the estimates are negatively related to the fluoride level. This means that our concern that municipalities give high fluoride water does not have any support. We have one statistically significant result for the number of water plants within a SAMS. Those SAMS without a water plants have on average lower fluoride. This is because the three largest cities in Sweden has few and large water plants and generally low fluoride levels. These areas also consist of many SAMS because of large populations. The point estimate is however very small. If the fluoride level within a SAMS increased by 0.1 mg/l, the number of water plants would increase by 0.02 water plants. In practice, this is a zero effect. With regards to Table A4 and Table A5, there is no evidence that municipalities reroute fluoride to certain immigration cohorts. The share in this case is expressed between 0 and 100. Some results are statistically significant, but all point estimates are small in magnitude (below 0.1 mg/l), with the exception of one coefficient. Let us take the first row in Table A5 as an example. If the share of immigrant fathers that arrived to Sweden in 1945 increases by 1 percentage point of the SAMS population (a large increase), the fluoride level to that SAMS would be 0.09 mg/l lower. The reader should not when interpreting statistically significant results that the precision of fluoride measurement is 0.1 mg/l. The reader should also note that some of these immigration cohorts consist of very few people.

Table A3. *Balance test. Predetermined characteristics. Fluoride for each SAMS*

	F. (0.1 mg/l)
SAMS area	3.552 (2.525)
Distance WP	0.0804 (0.182)
Not full info	0.000563 (0.0115)
Number WP, SAMS	0.0203*** (0.00711)
Father immigrant	-0.00163 (0.00176)
Mother immigrant	-0.00218 (0.00171)
Both parents immigrants	-0.00122 (0.000999)
Father immigrant outside Nordic	-0.00244* (0.00146)
Mother immigrant outside Nordic	-0.00239* (0.00131)
Both parents immigrant outside Nordic	-0.00139* (0.000829)
Father immigrant outside Europe	-0.00132 (0.000914)
Mother immigrant outside Europe	-0.00120 (0.000833)
Both parent immigrant outside Europe	-0.000771 (0.000556)
Mother's age at birth	-0.0317 (0.0317)
Father's age at birth	-0.0270 (0.0250)
Gender	0.000275 (0.000303)
Adopted	0.000102 (0.000108)

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Each row is a separate regression, where the dependent variable is displayed on the row. The number of observation ranges between 8,023 and 8,597

Table A4. *Fathers*

	Fluoride (0.1 mg/l)
1945	-0.8690***
1946	-0.3151***
1947	-0.7601**
1948	0.2093
1949	0.0096
1950	0.4872
1951	0.5415***
1952	0.1027
1953	-0.4337***
1954	0.0103
1955	0.3470**
1956	0.1211
1957	0.1381*
1958	-0.0201
1959	0.0946
1960	0.0463
1961	0.0505
1962	-0.0319
1963	0.0374
1964	0.0246
1965	0.1001
1966	0.0659
1967	-0.0097
1968	-0.0238
1969	0.0024
1970	0.0054
1971	-0.1031**
1972	-0.0198**
1973	-0.0445**
1974	-0.0096
1975	-0.0136
1976	-0.0296
1977	-0.0480
1978	-0.0129
1979	-0.0254
1980	-0.0154
1981	-0.0282
1982	-0.0231
1983	-0.0292
1984	-0.0453*
1985	-0.0374
1986	-0.0745**
1987	-0.0352**
1988	-0.0154
1989	0.0154
1990	-0.0704*
1991	-0.0368***
1992	0.0612

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The number of observation is 8,018. Fluoride is dependent variable.

Table A5. *Mothers*

	Fluoride (0.1 mg/l)
1944	-1.1401***
1945	-2.3599
1946	-0.0863
1947	-0.9317***
1948	-0.1115
1949	0.6072
1950	-0.0143
1951	0.2971
1952	-0.0561
1953	0.1288
1954	0.2730*
1955	0.0028
1956	-0.0081
1957	0.0390*
1958	-0.1378*
1959	-0.0430
1960	0.0187
1961	0.0077
1962	-0.0360
1963	0.0567
1964	0.0438
1965	0.0940
1966	0.0057
1967	-0.0408
1968	-0.0195
1969	0.0546
1970	-0.0096
1971	0.0341
1972	-0.0556
1973	-0.0390
1974	0.0178
1975	-0.0722***
1976	-0.0400*
1977	-0.0338***
1978	-0.0570***
1979	-0.0716*
1980	-0.0112
1981	-0.0140
1982	-0.0136
1983	-0.0585**
1984	0.0033
1985	-0.0293*
1986	-0.0250
1987	-0.0256
1988	-0.0114
1989	-0.0176
1990	-0.0679**
1991	-0.0770**
1992	-0.0365

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The number of observation is 8,029. Fluoride is dependent variable.

A third category of predetermined characteristics concerns cohorts. Assume that people suddenly become very concerned about fluoride, and moves from high fluoride areas. If that is the case, later cohorts would have a lower fluoride level than older cohorts. We test this in Table A6, with cohort 1985 as benchmark. We also include sibling order for those with at least one sibling (twins removed). We have three statistically significant results, but the point estimates are very small. Those born in 1992 received on average 0.007 mg/l lower fluoride than those born in 1985. In terms of economic significance, this is a zero effect and below the measurable precision level of fluoride.

Table A6. *Balance test. Cohorts and sibling order*

	F. (0.1 mg/l)
Cohort 1986	0.00665
	(0.0118)
Cohort 1987	-0.00843
	(0.0147)
Cohort 1988	0.00451
	(0.0163)
Cohort 1989	-0.00623
	(0.0156)
Cohort 1990	-0.0357**
	(0.0164)
Cohort 1991	-0.0195
	(0.0182)
Cohort 1992	-0.0743***
	(0.0203)
Sibling order	0.0423*
	(0.0215)

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The number of observation is 733, 683 for the cohorts and 421,241 for the sibling order regression. Fluoride is dependent variable.

A third concern would be that high cognitive ability individuals, who were exposed to lower dosages of fluoride, were able to avoid enlistment, meaning that when we run the analysis we only estimate the effect for a biased sample. Therefore we run balance tests to see if the fluoride treatment level for men without cognitive and non-cognitive ability scores differs from those who enlisted. We also run the test for taking the math test in ninth grade (for both males and females). In conclusion, there is no evidence of such sorting.

Table A7. *Balance test. Missing test scores*

	F. (0.1 mg/l)
No Cog. ab.	0.000758 (0.000799)
No Non-Cog. ab.	-0.000149 (0.000301)
No math test	-0.000149 (0.000301)

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Each row is a separate regression, where the dependent variable is displayed at the row. The number of observations for the two first outcomes are 377,360 and for the last outcome 570,954.

12.4 Result: Effects of fluoride on dental health

Table A8. *Unweightened regressions dental outcomes*

F.	CheckUps	DentalSurgery	Orthognathic	Prosthesis	OrthodontReplace	DiCheckUpsEval	DiDentHealth	DiDiseasePain	DiRepairs	DiRehabHab	MedianRemaining	MedianIntact
2013	-0.745**	0.0216	-0.0508*	-0.00807	-0.00639	-0.688**	-0.371*	-0.614**	-0.531***	-0.0208	-0.0127	0.0135
	(0.330)	(0.0452)	(0.0293)	(0.00902)	(0.0280)	(0.302)	(0.205)	(0.262)	(0.193)	(0.0290)	(0.0101)	(0.0194)
2008	-0.715**	-0.0863***	-0.0322*	0.0141	-0.00386	-0.678**	-0.230	-0.119	-0.278***	-0.0119	-0.0718**	-0.0187
	(0.345)	(0.0307)	(0.0169)	(0.0167)	(0.00313)	(0.320)	(0.195)	(0.118)	(0.0723)	(0.0154)	(0.0329)	(0.0450)

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The number of observation ranges between 7,386 and 7,622 for 2013 and between 7,352 and 7,606 for 2008.

Table A9. *Dental outcomes 2013. Additional specifications*

F.	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CheckUps	-0.3242*	-0.0614*	-0.0158	0.0032	0.0221	0.0111	0.0189
	(0.1847)	(0.0368)	(0.0360)	(0.0346)	(0.0265)	(0.0282)	(0.0296)
DentalSurgery	0.0095	-0.0127	-0.0053	-0.0073	-0.0178	-0.0184	-0.0220*
	(0.0270)	(0.0088)	(0.0130)	(0.0130)	(0.0115)	(0.0124)	(0.0113)
Orthognathic	-0.0218**	-0.0052*	-0.0046	-0.0040	-0.0011	-0.0007	-0.0007
	(0.0089)	(0.0028)	(0.0038)	(0.0037)	(0.0035)	(0.0045)	(0.0046)
Prosthesis	-0.0143***	-0.0061***	-0.0092***	-0.0098***	-0.0063***	-0.0047**	-0.0048**
	(0.0038)	(0.0017)	(0.0023)	(0.0023)	(0.0021)	(0.0023)	(0.0023)
OrthodontReplace	-0.0043*	-0.0011	-0.0017	-0.0015	-0.0004	0.0003	0.0003
	(0.0022)	(0.0008)	(0.0012)	(0.0012)	(0.0012)	(0.0014)	(0.0014)
DiCheckUpsEval	-0.2709*	-0.0644**	-0.0202	-0.0036	0.0068	-0.0058	-0.0006
	(0.1546)	(0.0327)	(0.0314)	(0.0304)	(0.0239)	(0.0259)	(0.0267)
DiDentHealth	-0.1734	-0.0266	-0.0039	0.0026	-0.0013	0.0034	0.0040
	(0.1216)	(0.0202)	(0.0206)	(0.0206)	(0.0159)	(0.0182)	(0.0182)
DiDiseasePain	-0.2159*	-0.0598*	-0.0435	-0.0445	-0.0407*	-0.0449*	-0.0470*
	(0.1270)	(0.0309)	(0.0284)	(0.0280)	(0.0240)	(0.0260)	(0.0262)
DiRepairs	-0.1508*	-0.0757***	-0.0772**	-0.0792**	-0.0730***	-0.0634**	-0.0700***
	(0.0840)	(0.0268)	(0.0345)	(0.0339)	(0.0262)	(0.0286)	(0.0265)
DiRehabHab	-0.0097**	-0.0059***	-0.0074***	-0.0072***	-0.0052**	-0.0042	-0.0043
	(0.0044)	(0.0020)	(0.0028)	(0.0027)	(0.0025)	(0.0027)	(0.0027)
MedianRemaining	-0.0138**	-0.0040***	-0.0065***	-0.0080***	-0.0044***	-0.0036***	-0.0036***
	(0.0063)	(0.0014)	(0.0018)	(0.0020)	(0.0012)	(0.0013)	(0.0013)
MedianIntact	-0.0143	-0.0009	-0.0066	-0.0058	0.0003	-0.0004	0.0010
	(0.0174)	(0.0046)	(0.0056)	(0.0055)	(0.0039)	(0.0040)	(0.0035)
Small set covariates	No	No	No	No	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	No	Yes
Fe. birth muni.	No	No	Yes	Yes	Yes	Yes	Yes
Fe. cohort	No	No	No	Yes	Yes	Yes	Yes
Fe. muni. 2013	No	Yes	No	No	Yes	Yes	Yes
Sample	All	All	All	All	All	Col 7	All

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Outcomes on each row. The number of observations ranges between 470,528 and 727,543.

Table A10. *Dental outcomes 2008*

F.	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Visit	-0.2242**	-0.0130	-0.0096	-0.0095	0.0195	0.0095	0.0229
	(0.0949)	(0.0210)	(0.0346)	(0.0338)	(0.0286)	(0.0299)	(0.0304)
Repair	-0.0409	-0.0334*	-0.0277	-0.0269	-0.0316	-0.0416	-0.0458*
	(0.0424)	(0.0170)	(0.0291)	(0.0291)	(0.0259)	(0.0280)	(0.0266)
RiskEvaluation	-0.2430**	-0.0139	-0.0123	-0.0121	0.0176	0.0072	0.0214
	(0.1030)	(0.0218)	(0.0356)	(0.0348)	(0.0293)	(0.0309)	(0.0321)
DiseasePrevention	-0.2651*	0.0147	0.0093	0.0094	0.0092	0.0103	0.0181
	(0.1458)	(0.0222)	(0.0470)	(0.0469)	(0.0309)	(0.0329)	(0.0320)
DiseaseTreatment	0.0730	0.0050	-0.0223	-0.0215	-0.0241	-0.0389	-0.0403*
	(0.0632)	(0.0139)	(0.0213)	(0.0212)	(0.0204)	(0.0242)	(0.0241)
RootCanal	-0.0140	-0.0065	-0.0094	-0.0092	-0.0054	-0.0031	-0.0051
	(0.0093)	(0.0040)	(0.0069)	(0.0069)	(0.0062)	(0.0071)	(0.0071)
Small set covariates	No	No	No	No	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	No	Yes
Fe. birth muni.	No	No	Yes	Yes	Yes	Yes	Yes
Fe. cohort	No	No	No	Yes	Yes	Yes	Yes
Fe. muni. 2013	No	Yes	No	No	Yes	Yes	Yes
Sample	All	All	All	All	All	Col 7	All

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Outcomes on each row. The number of observations ranges between 209,914 and 336,637

Table A11. *Dental outcomes 2008. Additional specifications*

F.	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CheckUps	-0.2692**	0.0022	-0.0010	-0.0010	0.0346	0.0224	0.0400
	(0.1161)	(0.0241)	(0.0395)	(0.0385)	(0.0324)	(0.0341)	(0.0358)
DentalSurgery	-0.0223	-0.0168**	-0.0272***	-0.0268***	-0.0276***	-0.0319***	-0.0345***
	(0.0164)	(0.0068)	(0.0097)	(0.0097)	(0.0093)	(0.0107)	(0.0106)
Orthognathic	-0.0121**	0.0017	-0.0051	-0.0051	-0.0001	-0.0016	-0.0012
	(0.0052)	(0.0027)	(0.0038)	(0.0038)	(0.0038)	(0.0045)	(0.0045)
Prosthesis	-0.0019	0.0008	-0.0017	-0.0016	0.0001	0.0027	0.0026
	(0.0036)	(0.0022)	(0.0032)	(0.0032)	(0.0030)	(0.0037)	(0.0037)
OrthodontReplace	-0.0027*	-0.0025***	-0.0034***	-0.0034***	-0.0031***	-0.0033**	-0.0033**
	(0.0015)	(0.0007)	(0.0010)	(0.0010)	(0.0010)	(0.0013)	(0.0013)
DiCheckUpsEval	-0.2430**	-0.0139	-0.0123	-0.0121	0.0176	0.0072	0.0214
	(0.1030)	(0.0218)	(0.0356)	(0.0348)	(0.0293)	(0.0309)	(0.0321)
DiDentHealth	-0.1369	0.0288	0.0354	0.0356	0.0195	0.0225	0.0301
	(0.1185)	(0.0197)	(0.0418)	(0.0417)	(0.0265)	(0.0293)	(0.0286)
DiDiseasePain	-0.0726	-0.0119	-0.0500**	-0.0491**	-0.0311	-0.0474**	-0.0485**
	(0.0542)	(0.0152)	(0.0247)	(0.0247)	(0.0214)	(0.0239)	(0.0239)
DiRepairs	-0.0491	-0.0414**	-0.0386	-0.0377	-0.0440	-0.0534*	-0.0586**
	(0.0440)	(0.0179)	(0.0309)	(0.0309)	(0.0275)	(0.0300)	(0.0284)
DiRehabHab	-0.0058	-0.0022	-0.0036	-0.0035	-0.0024	-0.0007	-0.0008
	(0.0045)	(0.0023)	(0.0034)	(0.0034)	(0.0033)	(0.0042)	(0.0042)
MedianRemaining	-0.0381***	-0.0043***	-0.0153***	-0.0152***	-0.0032**	-0.0023	-0.0024
	(0.0138)	(0.0013)	(0.0049)	(0.0049)	(0.0016)	(0.0018)	(0.0018)
MedianIntact	-0.0075	0.0124***	0.0062	0.0060	0.0093**	0.0104**	0.0122**
	(0.0204)	(0.0031)	(0.0057)	(0.0057)	(0.0045)	(0.0048)	(0.0047)
Small set covariates	No	No	No	No	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	No	Yes
Fe. birth muni.	No	No	Yes	Yes	Yes	Yes	Yes
Fe. cohort	No	No	No	Yes	Yes	Yes	Yes
Fe. muni. 2013	No	Yes	No	No	Yes	Yes	Yes
Sample	All	All	All	All	All	Col 7	All

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Outcomes on each row. The number of observations ranges between 208,691 and 336,637

12.5 Results: Non-linear effects, regression tables

Table A12. *Cognitive ability*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fluoride 2nd quartile	0.1444**	0.0654	0.0633	0.0184	0.0628**	0.0323	0.0776*
	(0.0656)	(0.0412)	(0.0416)	(0.0436)	(0.0261)	(0.0508)	(0.0468)
Fluoride 3nd quartile	-0.1729**	-0.0669**	-0.0654**	-0.0524	-0.0033	-0.0230	-0.0598
	(0.0696)	(0.0325)	(0.0324)	(0.0336)	(0.0253)	(0.0453)	(0.0575)
Fluoride 4nd quartile	0.0136	0.0263	0.0257	0.0006	0.0117	0.0549	0.1395**
	(0.0518)	(0.0258)	(0.0258)	(0.0326)	(0.0262)	(0.0417)	(0.0628)
Mean	5.008816	5.008816	5.008816	5.024005	5.024005	5.085113	4.922379
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	Col 5	All	SAMS stayers	SAMS movers
Observations	82,010	82,010	82,010	51,322	51,322	21,348	18,848

Notes: Standard errors clustered at the municipal level. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A13. *Non-cognitive ability*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fluoride 2nd quartile	-0.0196	-0.0559	-0.0561	-0.0679*	-0.0324	-0.0342	-0.0082
	(0.0655)	(0.0347)	(0.0346)	(0.0409)	(0.0358)	(0.0636)	(0.0638)
Fluoride 3nd quartile	-0.0652	0.0260	0.0262	0.0312	0.0651**	0.1049**	0.1144
	(0.0665)	(0.0311)	(0.0310)	(0.0346)	(0.0305)	(0.0531)	(0.0782)
Fluoride 4nd quartile	0.0528	0.0131	0.0135	0.0180	0.0283	0.0273	0.1293*
	(0.0430)	(0.0261)	(0.0260)	(0.0355)	(0.0343)	(0.0557)	(0.0701)
Mean	4.734139	4.734139	4.734139	4.775078	4.775078	4.913258	4.687314
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	Col 5	All	SAMS stayers	SAMS movers
Observations	66,561	66,561	66,561	41,730	41,730	17,408	15,159

Notes: Standard errors clustered at the municipal level. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A14. *Math points*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fluoride 2nd quartile	-0.0316	-0.2773**	-0.2649*	-0.2649*	-0.3439***	-0.1956**	-0.1076	-0.1887
	(0.2741)	(0.1341)	(0.1367)	(0.1367)	(0.1322)	(0.0970)	(0.1450)	(0.1469)
Fluoride 3nd quartile	-0.9186***	-0.3013**	-0.2990**	-0.2989**	-0.2884**	-0.0797	0.0919	-0.1268
	(0.3272)	(0.1205)	(0.1190)	(0.1189)	(0.1313)	(0.1026)	(0.1319)	(0.1174)
Fluoride 4nd quartile	0.0765	0.1050	0.1123	0.1122	-0.0045	0.1096	-0.0415	0.1695
	(0.2538)	(0.0941)	(0.0959)	(0.0959)	(0.0924)	(0.0988)	(0.1036)	(0.1293)
Mean	26.20998	26.20998	26.20998	26.20998	26.4943	26.4943	27.22649	26.04757
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	All	Col 6	All	SAMS stayers	SAMS movers
Observations	500,995	500,995	500,995	500,995	337,404	337,404	139,276	127,334

Notes: Standard errors clustered at the municipal level. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A15. *Annual log income in SEK*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fluoride 2nd quartile	-0.0407	-0.0009	-0.0023	-0.0040	-0.0045	-0.0050	0.0086	0.0216
	(0.0319)	(0.0109)	(0.0111)	(0.0104)	(0.0111)	(0.0094)	(0.0132)	(0.0190)
Fluoride 3nd quartile	0.0466*	0.0149*	0.0128*	0.0133	0.0067	0.0076	0.0131	0.0093
	(0.0265)	(0.0078)	(0.0073)	(0.0105)	(0.0104)	(0.0098)	(0.0109)	(0.0133)
Fluoride 4nd quartile	0.0301*	0.0264***	0.0209***	0.0199***	0.0196***	0.0183***	0.0140	-0.0002
	(0.0172)	(0.0068)	(0.0066)	(0.0057)	(0.0060)	(0.0057)	(0.0095)	(0.0119)
Mean	11.77979	11.77979	11.77979	11.77979	11.79419	11.79419	11.84486	11.78348
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE, year 2013	No	No	No	Yes	Yes	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	All	Col 6	All	SAMS stayers	SAMS movers
Observations	628,732	628,732	628,732	628,732	415,341	415,341	172,669	155,980

Notes: Standard errors clustered at the municipal level. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A16. *Employment status*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fluoride 2nd quartile	-0.0150	-0.0047	-0.0050	-0.0043	-0.0049	-0.0044	0.0028	0.0081
	(0.0114)	(0.0047)	(0.0048)	(0.0045)	(0.0045)	(0.0038)	(0.0058)	(0.0072)
Fluoride 3nd quartile	0.0115	0.0002	-0.0005	0.0003	-0.0006	0.0005	0.0014	0.0007
	(0.0098)	(0.0032)	(0.0031)	(0.0042)	(0.0041)	(0.0039)	(0.0046)	(0.0055)
Fluoride 4nd quartile	0.0173**	0.0152***	0.0136***	0.0125***	0.0088***	0.0079***	0.0020	0.0062
	(0.0078)	(0.0031)	(0.0030)	(0.0027)	(0.0028)	(0.0025)	(0.0043)	(0.0052)
Mean	.7018826	.7018826	.7018826	.7018826	.7147307	.7147307	.7419529	.7108723
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE, year 2013	No	No	No	Yes	Yes	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	All	Col 6	All	SAMS stayers	SAMS movers
Observations	729,850	729,850	729,850	729,850	475,414	475,414	192,740	179,374

Notes: Standard errors clustered at the municipal level. *** p < 0.01, ** p < 0.05, * p < 0.1.

12.6 Robustness analysis: Analysis with adoptees only

Table A17. *Cognitive ability, adopted*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fluoride (0.1 mg/l)	-0.0220	-0.0478	-0.0498	0.0286	0.0369	-0.1619	-0.1423
	(0.0222)	(0.0650)	(0.0654)	(0.0700)	(0.0782)	(0.3014)	(0.2493)
Mean	4.30303	4.30303	4.30303	4.34375	4.34375	4.163793	4.533333
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	Col 5	All	SAMS stayers	SAMS movers
Observations	528	528	528	288	288	116	90

Notes: Standard errors clustered at the municipal level. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A18. *Non-cognitive ability, adopted*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fluoride (0.1 mg/l)	-0.0276	0.0254	0.0182	-0.0416	-0.0421	0.0665	-0.1832
	(0.0209)	(0.0658)	(0.0654)	(0.0874)	(0.0864)	(0.2175)	(0.1958)
Mean	4.491443	4.491443	4.491443	4.669683	4.669683	4.635294	4.617647
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	Col 5	All	SAMS stayers	SAMS movers
Observations	409	409	409	221	221	85	68

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A19. *Math points, adopted*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fluoride (0.1 mg/l)	-0.0386	-0.1278	-0.1354	-0.1379	-0.0833	-0.0816	-0.1532	-0.0036
	(0.0933)	(0.1305)	(0.1288)	(0.1290)	(0.1603)	(0.1523)	(0.2526)	(0.3897)
Mean	23.73546	23.73546	23.73546	23.73546	24.06825	24.06825	24.69838	23.54964
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	All	Col 6	All	SAMS stayers	SAMS movers
Observations	2,098	2,098	2,098	2,098	1,260	1,260	557	413

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A20. *Annual log income, adopted*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fluoride (0.1 mg/l)	0.0105*	0.0153	0.0120	0.0132	0.0206	0.0189	0.0024	0.0088
	(0.0059)	(0.0105)	(0.0107)	(0.0123)	(0.0195)	(0.0197)	(0.0306)	(0.0474)
Mean	11.75914	11.75914	11.75914	11.75914	11.7436	11.7436	11.72156	11.783
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE, year 2013	No	No	No	Yes	Yes	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	All	Col 6	All	SAMS stayers	SAMS movers
Observations	3,167	3,167	3,167	3,167	1,712	1,712	735	551

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

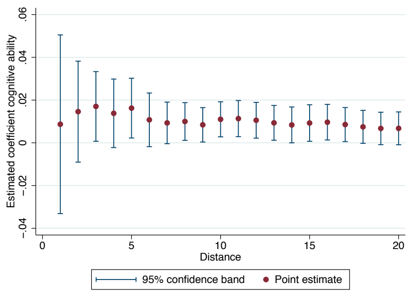
Table A21. *Employment status, adopted*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fluoride (0.1 mg/l)	0.0030	0.0022	0.0008	0.0033	0.0036	0.0033	0.0031	0.0322
	(0.0024)	(0.0038)	(0.0038)	(0.0040)	(0.0066)	(0.0067)	(0.0113)	(0.0326)
Mean	.6738847	.6738847	.6738847	.6738847	.6661836	.6661836	.6575964	.6910198
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE, year 2013	No	No	No	Yes	Yes	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	All	Col 6	All	SAMS stayers	SAMS movers
Observations	3,833	3,833	3,833	3,833	2,070	2,070	882	657

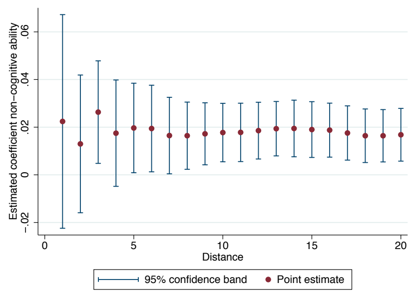
Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

12.7 Robustness analysis: Distance of SAMS

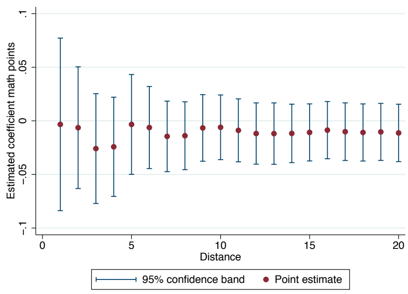
Figure A3. Estimates for different geographical distances from water plant. The X-axis corresponds to distances in kilometers between water plant and the center point of the SAMS.



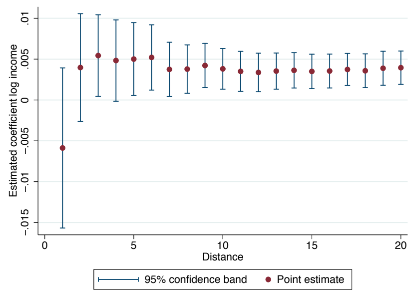
(a) Cognitive ability



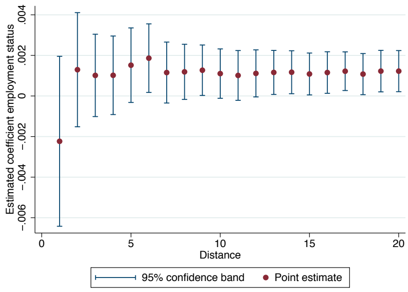
(b) Non-Cognitive ability



(c) Math points



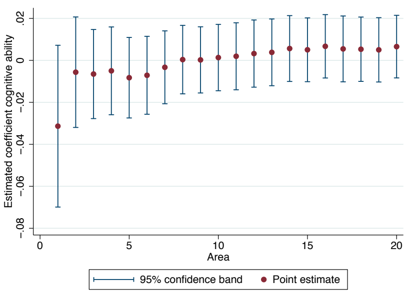
(d) Annual log income



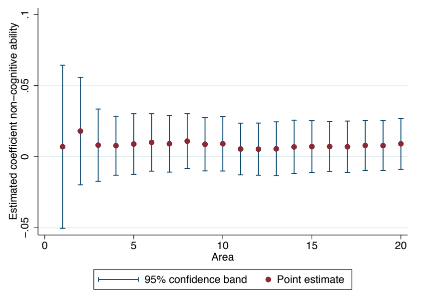
(e) Employment status

12.8 Robustness analysis: Area of SAMS

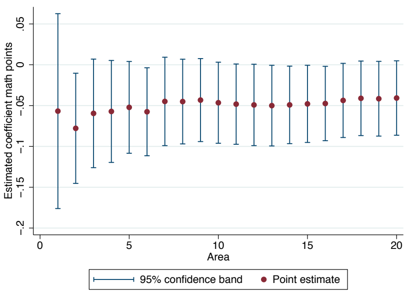
Figure A4. Estimates for different geographical areas SAMS. The X-axis corresponds to areas in square kilometers.



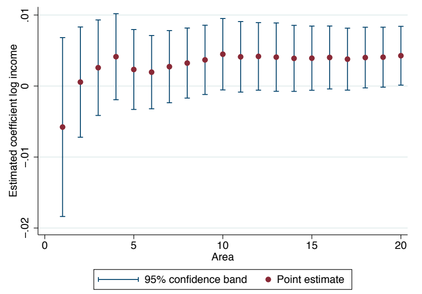
(a) Cognitive ability



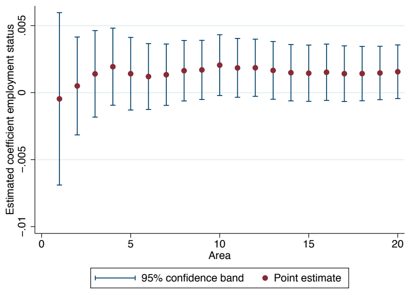
(b) Non-Cognitive ability



(c) Math points



(d) Annual log income



(e) Employment status

12.9 Robustness analysis: Confirmed water source

Table A22. *Cognitive ability, confirmed water source since 1985*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fluoride (0.1 mg/l)	-0.0185*	0.0079	0.0076	0.0113	0.0190**	0.0044	0.0430**
	(0.0105)	(0.0078)	(0.0077)	(0.0075)	(0.0083)	(0.0088)	(0.0181)
Mean	4.976069	4.976069	4.976069	4.975969	4.975969	5.072222	4.869272
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	Col 5	All	SAMS stayers	SAMS movers
Observations	18,971	18,971	18,971	12,234	12,234	6,300	5,194

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A23. *Non-cognitive ability, confirmed water source since 1985*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fluoride (0.1 mg/l)	-0.0028	0.0075	0.0076	0.0158	0.0251	0.0256*	0.0181
	(0.0095)	(0.0122)	(0.0123)	(0.0147)	(0.0154)	(0.0132)	(0.0265)
Mean	4.776578	4.776578	4.776578	4.819875	4.819875	4.939883	4.674413
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	Col 5	All	SAMS stayers	SAMS movers
Observations	15,285	15,285	15,285	9,882	9,882	5,140	4,174

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A24. *Math points, confirmed water source since 1985*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fluoride (0.1 mg/l)	-0.2399***	-0.0404	-0.0416	-0.0417	-0.0614**	-0.0119	0.0022	-0.0386
	(0.0560)	(0.0290)	(0.0273)	(0.0273)	(0.0284)	(0.0274)	(0.0248)	(0.0391)
Mean	26.36135	26.36135	26.36135	26.36135	26.54011	26.54011	27.26725	25.83771
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	All	Col 6	All	SAMS stayers	SAMS movers
Observations	113,568	113,568	113,568	113,568	79,597	79,597	40,430	34,685

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A25. *Annual log income, confirmed water source since 1985*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fluoride (0.1 mg/l)	0.0081**	0.0064***	0.0056***	0.0039***	0.0023	0.0016	0.0013	0.0058*
	(0.0039)	(0.0014)	(0.0013)	(0.0013)	(0.0016)	(0.0015)	(0.0021)	(0.0032)
Mean	11.81459	11.81459	11.81459	11.81459	11.82211	11.82211	11.85328	11.79489
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE, year 2013	No	No	No	Yes	Yes	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	All	Col 6	All	SAMS stayers	SAMS movers
Observations	144,066	144,066	144,066	144,066	98,690	98,690	50,298	42,853

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A26. *Employment status, confirmed water source since 1985*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fluoride (0.1 mg/l)	0.0030*	0.0025***	0.0023**	0.0017*	0.0006	0.0003	0.0002	0.0010
	(0.0018)	(0.0009)	(0.0009)	(0.0009)	(0.0011)	(0.0011)	(0.0011)	(0.0017)
Mean	.7197483	.7197483	.7197483	.7197483	.7289151	.7289151	.7457007	.7160464
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE, year 2013	No	No	No	Yes	Yes	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	All	Col 6	All	SAMS stayers	SAMS movers
Observations	164,966	164,966	164,966	164,966	111,810	111,810	56,056	49,170

Notes: Standard errors clustered at the municipal level. *** p < 0.01, ** p < 0.05, * p < 0.1.

12.10 Robustness analysis: Only those born in 1985

Table A27. *Annual log income, cohort 1985*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fluoride (0.1 mg/l)	0.0004	0.0018	0.0018	0.0034**	0.0042**	0.0051**	-0.0020	0.0154***
	(0.0014)	(0.0016)	(0.0016)	(0.0017)	(0.0020)	(0.0021)	(0.0038)	(0.0049)
Mean	12.14913	12.14913	12.14913	12.14913	12.16456	12.16456	12.22664	12.14301
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE, year 2013	No	No	No	Yes	Yes	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	All	Col 6	All	SAMS stayers	SAMS movers
Observations	69,909	69,909	69,909	69,909	41,393	41,393	16,978	15,271

Notes: Standard errors clustered at the municipal level. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A28. *Employment status, cohort 1985*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fluoride (0.1 mg/l)	0.0014*	0.0009	0.0009	0.0012	0.0007	0.0011	0.0001	0.0029*
	(0.0008)	(0.0007)	(0.0007)	(0.0008)	(0.0008)	(0.0008)	(0.0010)	(0.0017)
Mean	.8132965	.8132965	.8132965	.8132965	.8294167	.8294167	.8648925	.820007
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE, year 2013	No	No	No	Yes	Yes	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	All	Col 6	All	SAMS stayers	SAMS movers
Observations	79,329	79,329	79,329	79,329	46,341	46,341	18,563	17,234

Notes: Standard errors clustered at the municipal level. *** p < 0.01, ** p < 0.05, * p < 0.1.

12.11 Robustness analysis: Confirmed water source and only one water plant within SAMS, non-movers

Table A29. *Cognitive ability*

	(1)	(2)	(3)	(4)	(5)
Fluoride (0.1 mg/l)	-0.0208	0.0119	0.0112	0.0065	0.0065
	(0.0098)**	(0.0143)	(0.0141)	(0.0150)	(0.0150)
Mean	4.9742	4.9742	4.9742	4.9064	4.9064
Birth cohort FE	No	No	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	Yes	Yes
Sample	All	All	All	Col 5	All
Observations	2,051	2,051	2,051	1,325	1,325

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A30. *Non-cognitive ability*

	(1)	(2)	(3)	(4)	(5)
Fluoride (0.1 mg/l)	-0.0153	0.0035	0.0037	0.0145	0.0145
	(0.0136)	(0.0131)	(0.0131)	(0.0182)	(0.0182)
Mean	4.8273	4.8273	4.8273	4.8612	4.8612
Birth cohort FE	No	No	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	Yes	Yes
Sample	All	All	All	Col 5	All
Observations	1,668	1,668	1,668	1,081	1,081

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A31. *Math points*

	(1)	(2)	(3)	(4)	(5)	(6)
Fluoride (0.1 mg/l)	-0.0457	0.0470	0.0420	0.0416	0.0107	0.0032
	(0.0192)**	(0.0270)*	(0.0268)	(0.0268)	(0.0297)	(0.0242)
Mean	26.6662	26.6662	26.6662	26.6662	26.8046	26.8046
Birth cohort FE	No	No	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	Yes	Yes
Sample	All	All	All	All	Col 6	All
Observations	12,671	12,671	12,671	12,671	9,174	9,174

Notes: Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A32. *Annual log income*

	(1)	(2)	(3)	(4)	(5)	(6)
Fluoride (0.1 mg/l)	0.0019	0.0010	0.0007	-0.0001	-0.0033	-0.0031
	(0.0022)	(0.0030)	(0.0030)	(0.0028)	(0.0038)	(0.0035)
Mean	11.9054	11.9054	11.9054	11.9054	11.9060	11.9060
Birth cohort FE	No	No	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes
Municipal FE, year 2013	No	No	No	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes
Large set covariates	No	No	No	No	Yes	Yes
Sample	All	All	All	All	Col 6	All
Observations	16,401	16,401	16,401	16,401	11,439	11,439

Notes: Standard errors clustered at the municipal level. *** p < 0.01, ** p < 0.05, * p < 0.1.

Table A33. *Employment status*

	(1)	(2)	(3)	(4)	(5)	(6)
Fluoride (0.1 mg/l)	0.0006	0.0010	0.0009	0.0005	-0.0015	-0.0014
	(0.0011)	(0.0012)	(0.0013)	(0.0013)	(0.0017)	(0.0016)
Mean	0.7685	0.7685	0.7685	0.7685	0.7724	0.7724
Birth cohort FE	No	No	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes
Municipal FE, year 2013	No	No	No	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes
Large set covariates	No	No	No	No	Yes	Yes
Sample	All	All	All	All	Col 6	All
Observations	18,185	18,185	18,185	18,185	12,651	12,651

Notes: Standard errors clustered at the municipal level. *** p < 0.01, ** p < 0.05, * p < 0.1.

12.12 Robustness analysis: Alternative income measure

Table A34. *Log income, "förvärvsinkomst"*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fluoride (0.1 mg/l)	0.0078**	0.0069***	0.0054***	0.0047***	0.0035***	0.0031***	0.0027	0.0013
	(0.0035)	(0.0017)	(0.0016)	(0.0014)	(0.0013)	(0.0011)	(0.0017)	(0.0033)
Mean	11.8526	11.8526	11.8526	11.8526	11.86668	11.86668	11.9128	11.85671
Birth cohort FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth municipal FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE, year 2013	No	No	No	Yes	Yes	Yes	Yes	Yes
Small set covariates	No	No	No	Yes	Yes	Yes	Yes	Yes
Large set covariates	No	No	No	No	No	Yes	Yes	Yes
Sample	All	All	All	All	Col 6	All	SAMS stayers	SAMS movers
Observations	634,842	634,842	634,842	634,842	419,164	419,164	174,362	157,356

Notes: Standard errors clustered at the municipal level. *** p < 0.01, ** p < 0.05, * p < 0.1.

II. The Effects of Earlier Elective Experience: Evidences From The U.S. House of Representatives

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1 Introduction

Candidates running for office in the U.S. House of Representatives frequently promote themselves as persons who have never been close to politics or as candidates that already hold office at a lower level. The public discussion in the U.S. about the merits and disadvantages of these two types of backgrounds has become intense in recent years.[1] The campaigns for president of the United States in the spring of 2016 may serve as an illustrating example of the phenomenon. The public debate was much concentrated on the question whether America would be better off with a political insider or with an outsider with no experience of holding elective office. Hillary Clinton and Donald Trump were the two frontrunners where Hillary Clinton ran a campaign where she portrayed herself as someone who had a lot of political experience. She has been secretary of state and a senator for New York. Donald Trump on the other hand promoted himself as a political outsider with no earlier experience of holding elective office. In this case, the Democratic candidate had experience from elective office and the Republican candidate did not. In elections to the House of Representatives, both politician-types can be found in both of the two major parties.

What are the effects for the election district if the voters elect a candidate to the U.S. House of Representatives with earlier experience from elective office over a candidate with no such experience? Investigating this addresses the larger question of how well different politician types cater to their constituents. Politics in the U.S. is very locally oriented where representatives work to secure influence for their district. The question I want to answer in this paper is whether earlier elective experience is a positive factor in the game of securing influence for the voters on the federal level. This paper is to my knowledge the first to focus on this issue explicitly. Other papers have been concerned with the length of politicians' tenure, the socioeconomic background and the gender of elected politicians as well as the incumbency advantage of elected officials. The shortage of empirical research on having experience from elective office on policy outcomes is surprising given the current intense discussion about the backgrounds of politicians in the U.S..

I estimate the effect using a sharp regression discontinuity design (RD). The treatment group consists of those districts that elected a candidate to the U.S. House with prior experience from elective office. I include all types of earlier elective experience in my treatment group such as being mayor at the local level, state legislator or a politician at

[1] This has become a more important distinction after the Tea Party Movement became a prominent factor in American politics, where the candidates endorsed by the Tea Party often aggressively promote themselves as anti-Washington and emphasize that they have no elective experience.

the county level.[2] I investigate the effect on outcomes on the election district level where each district has one representative in the House. The main outcome variables are personal income per capita and unemployment in the home district, directed federal spending to the home district (informally known as pork barrel spending) and committee placement of the elected representative.

Having earlier elective experience is likely to improve a politician's political network on the local political level, with the political party leadership in Washington DC and with other elected representatives in the House. These contacts are likely to be important for all of the outcome variables. For example, it is the political leadership that decide who will sit on which committee in the House and I expect that having earlier experience from elective office increases the probability of being a member of a powerful committee. In the political game of influencing spending and legislation so that it benefits the district, a better connected politician should be more successful. Unemployment and personal income per capita in the home district measure such overall successfulness. With regard to directed federal spending, a politician with experience from elective office should equivalently have a better possibility to secure such spending. Such a representative has on the other hand probably better connections on the local political level, meaning that he or she has a better possibility to secure economic and management support from the local party organization. As a result, a representative with prior experience from elective office does perhaps not need to chase directed federal spending in order to *buy* votes to get reelected. The theoretical prediction with regard to directed federal spending is hence more ambiguous.[3]

I find indications that having prior experience from elective office increases the probability of being a member of a powerful congressional committee once being elected to the House of Representatives. I find indications of a positive effect on directed federal spending some years after a candidate with earlier elective experience was elected to the House. I find however no statistical significant effects on personal income per capita or unemployment rate in the home district. The results should be interpreted with caution given that the analysis is not based on a large number of observations.

The rest of the paper is organized as follows: The next section reviews the earlier papers that have investigated other treatments and other out-

[2] A candidate is coded as having earlier elective experience if he or she had held an elective position during the last 20 years before being elected to the House. See the empirical framework section for more details.

[3] Dal Bó et al. (2009) and Primo and Snyder (2010) are the foundation for these theoretical priors. See the theoretical framework section for a more extensive theoretical review.

come variables related to the background of elected politicians. I continue with a description of the American political system followed by a more comprehensive discussion about the theoretical framework and the theoretical priors briefly discussed here in the introduction. Follow suit is the data section, which also include descriptive statistics and next the empirical framework section where I discuss the identification strategy, the econometric specification and potential threats to identification. The results are presented in the following section and then the robustness analysis. The paper is ended with some concluding remarks. Additional regression tables and figures can be found in the appendix.

2 Earlier literature

The literature on elected politicians' background has emerged as a new subfield within political economics. Besley (2005) was one of the first papers highlighting the importance of political leadership and political selection. The research field is novel and earlier papers are empirically oriented and not always connected to a specific theory. These papers have studied various outcome variables, different treatments and used data from many different countries. In short, the earlier literature can be divided into three broad categories: papers that have used cross-country data, papers that have used disaggregated data from specific countries and papers that have focused on a particular aspect of politicians' background, namely their gender. In this section I review these earlier papers in short.

Jones and Olken (2005) was the first paper investigating the effects of political leadership in a cross-country study where the authors apply the sudden deaths of political leaders as an instrument for change in political leadership. They find that GDP growth rate and inflation are affected by a change in political leadership. Besley et al. (2011) explore whether education attainment of political leaders has an effect on GDP and find a positive effect, where higher education is associated with higher economic growth. The exogenous variation is a time effect based on when a political leader resigns and the data originates from a long time period where both democratically elected politicians as well as dictators are included.[4] Dreher et al. (2009) conclude that political leaders who were entrepreneurs or scientists before entering politics are more prone to implement market reforms, measured by the Fraser Reform index. Moessinger (2014) studies the background of the finance ministers and conclude that the education level is not related to public debt, but that finance ministers that have served for a longer period of time seem

[4] Besley and Reynal-Querol (2011) is based on the same data set and the authors conclude that democracies have more educated leaders

to be more efficient in lowering debt as a share of GDP. Hayo and Neumeier (2016) focus on heads of government and heads of state and reach the conclusion that those leaders that are from a less advantage socioeconomic background tend to increase public dept.

The second category of papers has applied more disaggregated data. Hayo and Neumeier (2014) investigate the effect of state governors' socioeconomic background in Germany and find that a governor who comes from a poor background tend to spend more public resources and increase public debt. Jochimsen and Thomasius (2014) study the background of the finance ministers in the German states. They conclude that educational background does not seem to be related to the size of public deficit, but that the previous professional experience does. Several papers have focused on politicians' seniority and the length of their political tenure. Freier and Thomasius (2015) study earlier political tenure and education among German mayors and estimate the effect in a RD-setting. They find indications that reelected mayors lower taxes, reduce debt-levels and decrease spending.[5] Fowler and Hall (2015) also focus on seniority and investigate its effect on pork barrel spending in American elections. They do not find any evidence of the popular belief that more senior representatives bring home more pork.[6] Economic growth is the outcome of interest in Levitt and Poterba (1999) where they consider federal spending as the transitional variables through which seniority may affect economic growth. They find that states benefit in terms of economic growth from having more senior members in the House, but they find no evidence that this relation can be explained by increased federal spending. States with higher economic growth were also those states where the elected representative had a better committee placement. Moore and Hibbing (1996) use directed federal spending to the entire state and to a specific district as outcomes. Their main result is that seniority does not influence spending to a specific district, but the overall spending to a state decreases if its congressional delegation as a whole has spent less time in the Congress.

[5] See also Freier (2015) that concludes that there is a significant incumbency advantage in municipal elections in Germany. See also Ade et al. (2014) for a study on incumbency advantage.

[6] One might think that committee membership is a mechanism that can explain directed federal spending to a district. Berry and Fowler (2015) find results that point in the other direction. The exception seems to be positions as chairman of one of the subcommittees of the Appropriations committee. These representatives are able to reroute federal money to their districts. Berry et al. (2010) neither find any evidence that committee membership increases pork spending. Alvarez and Savings (1997a) have found results that somewhat points in the other direction where they demonstrate a relation between membership in certain committees and pork barrel spending.

The third group of papers has focused on gender representation and its effect on policy, for example Chattopadhyay and Duflo (2004) (Indian data), Ferreira and Gyourko (2014) (American municipal data), Svaleryd (2009) (Swedish municipal data) and Schild (2013) (German municipal data). The results are somewhat mixed, where Ferreira and Gyourko (2014) and Schild (2013) do not find any effect of a woman being elected mayor in contrast to the results in the other papers where an increase in the representation of women changed policy and public spending.

3 Institutional framework

This section contains a description of the institutional framework. I describe the American election system to motivate the choice of using a RD-design. To give a background to the chosen outcome variables, I discuss briefly the incentives that representatives face and how members of the House of Representatives work in different committees.

Elections to the House of Representatives are held every second year. Elections take place in single-member districts and elections are of the sort "first-pass-the-post". Each state in the United States is divided into election districts where one representative in each district is elected to serve in the House. More populous states have more districts than less populous states. In almost all cases, the general election in a district stands between a candidate from the Republican Party and one candidate from the Democratic Party. Since the United States has a two party system where two candidates compete for a single seat, I can use a sharp regression discontinuity design.[7] Incumbent politicians often run for reelection and in the large majority of the elections, an incumbent is facing a challenger. If the incumbent resigns or do not choose to run for reelection, two challengers are contesting the seat in an *open election*. I focus on these open elections in the empirical analysis, where my focus is on those open elections where one candidate with prior experience from elective office competed against a candidate without.

The United States Congress is divided into two chambers: The House of Representatives and the Senate. I focus my analysis on the House of Representatives because the House has more members than the Senate and I want to restrict the analysis to a single institutional body. Representatives have a clear local connection since only one representative is elected in each district. Representatives have thus incentives to work for

[7] There are several earlier papers that have used a RD strategy together with data from the U.S. House of Representatives, see for example Lee et al. (2004). See also Pettersson-Lidbom (2008) for seminal work on RD where the author investigates party representation effects with Swedish municipal data.

the influence of their particular districts since it is the voters of their districts that decide whether the representative is going to be reelected and not voters in other districts. Three of the outcomes variables measure the representative's successfulness in this dimension. Directed federal spending might serve a role in the legislative bargaining game where directed federal spending benefits the voters in the district. Representatives might also try to adapt legislative bills and overall spending with the aim to benefit their districts economically. The outcome variables personal income per capita and unemployment in the home district intend to measure representatives' successfulness in this regard.

Much power of the House of Representatives is vested in the committees and certain committees are usually considered to be more powerful than others. I choose to focus on four committees that sometimes are called super-committees, or big4 committees, which are the: 1) Ways and Means committee which is responsible for taxation decisions, 2) Appropriations Committee which overseas almost all federal spending, 3) Armed Service Committee which is responsible for the Department of Defense and 4) Committee on Foreign Affairs.[8] When I investigate the effect of having earlier political experience on committee membership, I consider membership of these big4 committees. It is the Democratic Party and the Republican Party that decides who sits on which congressional committee. Newly elected members of the House submit requests to belong to a certain committee before a new mandate period begins. The political party leadership and other senior party members constitute a so called *steering committee* and decide on a recommendation for committee assignment. Their recommendation is transferred to the Democratic Caucus and the Republican Conference where the formal decision takes place. The House of Representatives confirms the committee placement by voting for the committee assignment proposal (Schneider, 2014c, p.53-55). For a detailed description of the committee assignment process, see Schneider (2014c).

4 Theoretical framework

In this part of the paper I review and explain in more detail the theoretical priors briefly discussed in the introduction.

If a candidate has experience from elective office before being elected to the House of Representatives, he or she has had a longer career within the political or public sphere. Such a person should all else equal be

[8]The term big4 committee has mostly been used to describe the corresponding committees in the Senate, see Schneider (2014a). I choose to focus on the same group of committees in the House. See Schneider (2014b) for a description on the committee system.

better connected to other politicians. For instance, he or she should have better personal connections to local politicians in his or her home district. It is also likely that the candidate has better connections with the party leadership and with other politicians in the House.

Dal Bó et al. (2009) has analyzed political dynasties. Explicitly, the authors study members of the U.S. Congress and their family connections to other elected members. They argue that politicians whose relatives were also members of Congress are better in name recognition and that they have more political human capital. The same idea of political human capital can be applied to the treatment of interest in this paper. Someone who has held elective office should on average have more political human capital because of longer political training. Political human capital in terms of longer political tenures is also analyzed in Miquel and Snyder (2006).[9]

Since representatives with prior experience from elective should have on average better political contacts and more elaborated political networks, my prior is that the probability that they receive a seat on a big4 committee is larger. They should also be more successful in the game of securing federal influence for their constituents by streamlining legislation and overall spending in a way they think benefits their home district economically. In order to measure representatives' successfulness in this regard I investigate the effect on personal income per capita and unemployment.

Having better political connections should also be beneficial when a representative tries to secure directed federal spending. Primo and Snyder (2010) argue on the other hand that both economic support from the local party organization and directed federal spending are positive factors for an incumbent running for reelection. If we believe that having experience from elective office results in having a better political network on the local level, a candidate with such experience has a higher probability of receiving local organizational and economic support when running for reelection. Hence, he or she does not need as much directed

[9]See also Caselli and Morelli (2004), Messner and Polborn (2004), Poutvaara and Takalo (2007), Mattozzi and Merlo (2007), Mattozzi and Merlo (2008) and Carrillo and Mariotti (2001) for a discussion about political careers, the quality of politicians and politicians' effectiveness. Kotakorpi and Poutvaara (2011) present empirical evidence that higher salary increases the education level among females members of the Finnish parliament. All the theoretical models on the quality of politicians are based on the Citizen-Candidate framework that was originally presented in Osborne and Slivinski (1996) and Besley and Coate (1997)

federal spending to convince the voters to reelect him or her.[10] In the end, it is an empirical question whether the potential negative effect of having a larger local political network on directed federal spending dominates the other possible positive effect of having better connections with other members of the House.[11]

5 Data and descriptive statistics

In this part of the paper I present the data material. This section also contains summary descriptive statistics and a description of sample restrictions.

Professor Gary Jacobson has provided me with data regarding earlier experience from elective office for candidates for the years 1945 to 2012 which is the data set I use for the treatment variable. Jacobson's data set also contains information on vote shares for the running candidates.

Information on committee assignment comes from Charles Stewart III and Jonathan Woon's Congressional Committee Assignments dataset and from Garrison Nelson's Committees in the U.S. Congress data set. Altogether, I have data on committee membership from 1979 to 2013 in the main analysis.[12]

Data regarding directed federal spending, so called pork spending, is the same data that is used in Berry et al. (2010) and covers the years 1984-2007. The variable of interest contains federal spending to a specific district minus defense spending. The variable is defined to only include spending that policy makers may influence, meaning that more

[10] Levitt and Snyder (1997) and Stratmann (2013) find empirical support for the hypothesis that voters reward pork barrel spending in elections to the U.S. House of Representatives. See also Alvarez and Savings (1997b), who found that the positive effect were present for Democrats, but not for Republican politicians. See also Weingast et al. (1981) for seminal theoretical work on pork barrel spending and Stein and Bickers (1994) for a discussion and analysis on pork barrel spending in the U.S..

[11] At an earlier stage I investigated the effect on number of times a representative stands witness in front of congressional committees in the main analysis. The theoretical prior for this outcome was after further consideration unclear. First, representatives must be invited to witness to give their view on a subject, but they still have a choice. Not only representatives can witness, but also members of the public. Second, I wanted to focus on outcomes where voters have some common ground. It is not clear that all voters want their representative to witness. For example, they might prefer that a local politician witness instead. Since I have run the analysis for witness appearances, I present all the results in the appendix.

[12] When putting these data materials together, some few observations have been dropped because they do not match between the data sets. In the robustness analysis, I also run a similar analysis for an older part of the Garrison Nelson's data set.

fixed spending posts are excluded. For a more detailed data description, see Berry et al. (2010).

Data for personal income per capita from 1968 to 2012 is downloaded from the Bureau of Economic Analysis. Data on unemployment rate is downloaded from the Bureau of Labor Statistics and covers the years 1990 to 2014.[13]

I run balance tests in the end of the empirical framework section to investigate in more detail the composition of the treatment group and the control group. Explicitly, I investigate candidates' earlier professional experience. There are no comprehensive data sets on political candidates' profession, so I have collected the data from different sources. For the winners I use data from CQ Press Library and for the losers I have gathered information from the candidates' campaign websites. When running the covariate balance tests regarding demographic characteristics on the district level, I use data from Snyder and Strömberg (2010)[14]

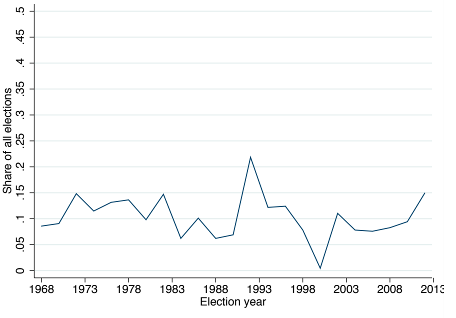
I choose to focus my empirical analysis on *open elections* where no incumbent politician was running for reelection in order to avoid estimating the incumbency advantage effect. Hence, I only consider elections where two challengers that have never been members of the House are competing against each other for a district's seat. The share of such

[13]These data sets contains information for each county which I aggregate to the congressional district level by using information from Missouri Census Data Center MABLE/GeoCorr which have relationship files from 1990. I take the weighted mean for the number of parts a county have been slit into to form a congressional district (for the large majority of the cases, a congressional district is formed by including several non-split counties). For personal income per capita I have used the 1990 relationship files for years 1968-1990. I hence have some measurement errors for this dependent variable. The reader should note that there are some counties that I have not been able to match to the information in MABLE and hence to the Gary Jacobsson data set, meaning that these observations have been dropped. I have dropped a county if it has some missing value in the panel. There are also some differences between data sources in the county statistics for Virginia and Hawaii (independent cities and counties are sometimes reported together in an aggregated measure). I have focused on the larger part of such aggregated measure when merging data sets together.

[14]I have used the relevant demographic covariates with clear definitions. Some of this data is also on the county level and it has been aggregated to the congressional district level. As with personal income per capita, I have used the 1990 data file for years prior of 1990. The data regarding the number of times a representative witness in front of congressional committees also comes from Snyder and Strömberg (2010), where the results are presented in the appendix.

open elections are presented in Figure 1. As indicated in the figure, the share of open elections is around 10 percent over the years. [15]

Figure 1. Share of open elections for each election year



(a) Share of open elections

To give the reader an overview of the data sample, I present a Table 1 indicating how I have restricted the data set. In the column *Total observations*, the number of observations available for the outcome variable for the entire measurement period is displayed. The number of observation corresponds to the number of election districts. When I only keep the open elections, the sample is substantially reduced, which is indicated in column 4, *Open elections*. When I estimate the sharp RD, only election districts that had one candidate with prior experience from elective office running against a candidate without such experience contribute to the estimation. The final data sample used in the data analysis is thus presented in the last column *Open elections and different experience.*

Table 1. *The data sample for each outcome variable*

Outcome	Meas. per.	Tot.	Op. elec.	Op. elect. & diff exp.
Member big4 committee dummy	1979-2013	7364	678	362
Direct fed. spend in million USD	1984-2007	5653	510	282
Personal income cap. in USD	1968-2012	9866	953	499
Unemployment rate in %	1990-2014	5219	492	265

[15] I drop unopposed elections since they cannot contribute to the estimation. I also drop elections when a former representative runs again for office. I also drop some elections indicated as ‘’odd” in the Gary Jacobsen data set. I also drop third party winners (very few). When I run the analysis for committee membership, I drop the observation if the elected member in an open election resigned during the mandate period after the open election. The reader should note that there are some missing values in the data sets with regard to vote shares and experience from elective office.

Table 2 presents summary statistics for the outcome variables. All variables in Table 2 are measured as a mean for the coming two years after an election (a mandate period in the House of Representatives) on the district level. In the first column, *Total*, the descriptive statistics for the entire data panel is displayed (column 3 in Table 1). In the second column, *DataAnalysisSample*, summary statistics is displayed for districts that had open elections with one challenger with prior experience from elective office running against a challenger without (the last column in Table 1).

Table 2. *Descriptive summary statistics of the outcome variables*

	Total		DataAnalysisSample	
	mean	sd	mean	sd
Member big4 committee dummy variable	0.45	0.50	0.25	0.43
Directed federal spending in million USD	580.23	1072.18	531.48	952.39
Personal income per capita in USD	33892.27	9774.30	32972.70	7985.81
Unemployment rate in percentage points	6.51	2.44	6.60	2.48

Looking at personal income per capita, directed federal spending and unemployment rate, they seem to be fairly balanced between these two samples, meaning that there are no notable differences between districts that held open elections with challengers with different experience in comparison to the entire data sample where all elections are included. Representatives elected for the first time in open elections are however less likely to end up in a big4 committee in comparison to the full sample where incumbents that won reelection are included. This can probably be explained by the fact that these committees are powerful and that reelected members sometimes request to change to these committees.

6 Empirical framework

This section presents the empirical framework. I discuss the identification problem, how the treatment and control groups are defined and how I implement the RD-design. An important assumption behind RD is that the treatment status of districts cannot be manipulated. I discuss this assumption in more detail in the second part of this section.

The goal is to estimate the causal effect of electing a candidate with earlier experience from elective office to the U.S. House on outcomes on the election district level. There might be a selection of candidates with earlier elective experience to certain election districts. For instance, experienced candidates might be more common in districts that have a well-organized public system with good local governance. Those districts have probably better possibilities to prosper economically. Without exogenous variation in the background of elected representatives, I will not be able to separate selection of treatment status from the outcomes. The first-best empirical strategy would be to randomize previous experience from elective office among elected representatives. The background of a district's representative is then random, meaning that I can estimate the casual effect. However, it is not possible to randomly assign earlier elective background to representatives and therefore, I need to rely on a quasiexperimental method.

The American two-party system with single-member election districts provides a way to exploit randomness close to the 50 % cut-off in a sharp RD design. By focusing only on open elections I can estimate the effect where incumbency is not an issue. The treatment group then consists of those districts that just elected a representative which had held elective office before being elected to the U.S. House of Representatives. Candidates are coded as having elective experience if they have held any elective positions during the last 20 years, such as being mayor at the local level, member of the city council, member of the school board or member of the state legislature. In some states, the voters elect persons serving in the branch of law enforcement, for example attorney general, sheriff and judges. These positions are also included in the treatment group. The control group includes districts where the elected representative had not held any elected office during the last 20 years prior of being elected to the House.

There are several potential differences between candidates with and without experience for elective office. A candidate without elective experience may have acquired other types of experiences when not serving in an elective position. He or she may for example have worked in the private sector. The average age or the gender composition between the two groups may also be different. Hence, the control group incorporates districts with elected politicians of various backgrounds. In my empirical analysis, I estimate the *average effect* of electing a candidate with experience from elective office over a candidate with some of these other backgrounds.[16] To investigate in more detail the candidates' background, I have collected data on their earlier profession for some later years. I specify a balance test between challengers with and without experience from elective office with regards to their previous professions. The balance test is presented in the end of this section.

There is a discussion in the econometric literature how to implement a regression discontinuity design. In short, there are two traditions: the global parametric approach and the local non-parametric approach. The former uses all available observations and includes various polynomials

[16] My paper is not unique in the regard that I consider a broad treatment. Lee et al. (2004) for instance estimate the effect of electing a Democrat over a Republican on voting behavior in the House. There are several potential factors that may differ between elected Democrats and Republicans other than their party labels.

in the regression. The latter applies a narrower sample close to the cut-off, where the standard application is to use local linear regression in accordance with Hahn et al. (2001) since the functional form is probably linear within this discontinuity sample.[17]The main challenge when applying a non-parametric approach is to choose the bandwidth which determines the discontinuity sample.[18] Gelman and Imbens (2014) are critical against the inclusion of higher order polynomials in regression discontinuity design because inference based on higher order polynomials is problematic and the regression results are often sensitive to the choice of order of the polynomial. They argue that polynomials higher than quadratic ones should be avoided. In line with the critique of including higher order polynomials in a global parametric approach, I choose to work with a non-parametric approach. In a recent paper, Calonico et al. (2014) have introduced the RD-robust procedure which is a non-parametric approach for constructing robust confidence intervals for various choices of the bandwidth. In the main specification I use the RD-robust[19] set-up with a triangular kernel in line with Calonico et al. (2014).[20] I run all specifications for various bandwidths including two optimal bandwidths. An optimal bandwidth is a way of finding a balance between precision and bias when estimating the effect on a subsample of datapoint close to the cut-off. I present results for the optimal bandwidth suggested in Calonico et al. (2014) and the optimal bandwidth suggested in Imbens and Kalyanaraman (2012).

In its simplest general form, the RD regression equation looks like this:

$$Y_{i,t} - Y_{i,t-1} = \beta_0 + \beta_1 T_i + \beta_2 X_i + \beta_3 T_i * X_i + u_i \tag{1}$$

Y is the outcome variable on the election district level, i, collapsed over a mandate period, t after the open election.[21] I focus on the difference in directed federal spending, personal income per capita and unemployment rate between the mandate period after the open election, t, and the mandate period before the open election, $t-1$. Because changes in directed federal spending, personal income per capita and unemployment may take some time to manifest, I also run specifications where I use a lead difference for later mandate periods (time period $t+1$ and time period $t+2$) and the mandate period before the open elec-

[17]Local polynomial regression for the discontinuity sample is discussed in Porter (2003)

[18]See Jacob et al. (2012) for an overview and a discussion about RD estimation.

[19]November 2014 version

[20]Hyytinen et al. (2014) shows that the RdRobust method proposed in Calonico et al. (2014) works well when replicating real experimental data.

[21]For some outcome variables, I only have one year of a congress in the beginning or the end of the panel. In this case, the collapse is only over one year.

tion, $t-1$. For committee membership I run a specification where the dependent variable takes the value 1 if the newly elected representative was member of a big4 committee at any time in the mandate period after the open election, t, and 0 otherwise. In this case I do not specify a difference. β_0 is the constant, T is the treatment dummy. $T=1$ if the elected representative in district i had experience from elective office prior of being elected and 0 otherwise. X is the running variable which is the vote share in percentage points for the candidate with experience from elective office in election district i. In the analysis, I have normalized the running variable around 0, which is the cut-off. A negative value of the running variable indicates that the candidate with elective experience lost the open election and vice versa.

6.1 Discussion on identifying assumptions

The sharp regression discontinuity design requires that there is random assignment of the treatment around the cut-off. In this subsection I discuss this identifying assumption and the concerns that have been raised against using regression discontinuity design together with U.S. House data.

There are two concerns that may threaten the validity of the RD: 1) the possibility of election fraud and 2) systematic but legal sorting of certain politicians into winning. I start by discussing these two concerns one at the time by reviewing earlier papers that have addressed these issues. These earlier papers have mostly been concerned with incumbency advantage in the U.S. House and whether incumbents are better able to conduct election fraud or sort legally into winning. Incumbency is however somewhat connected to having earlier experience from elective office since both are about having connections within the political system.

Large scale election fraud is the most direct threat that would invalidate the RD feature of randomness who wins close to the cut-off. The most obvious sign of election fraud would be a higher mass of observation very close to the cut-off for one of the candidate types since election fraud is more likely in close elections.[22] Snyder (2005) shows that in close elections to the U.S. House of Representatives, incumbents running for reelection have a higher probability of winning. We would expect that incumbents win half of the elections when the election is close. Snyder (2005) interpret this finding as indicative evidence of manipulation in close elections to the House. If this were true, the RD

[22]There are many different kinds of manipulation that in various degree can be defined as election fraud. Legal battles after the election in close races are for example not outright election fraud, but sometimes attempts to manipulate the election outcome. See the discussion and examples in Grimmer et al. (2011)

design would not be valid together with U.S. House data. Snyder et al. (2015) (another Snyder than the first one) disagree however and argue that exactly around the cut-off, it should be random who wins and who loses, but further away, but still close to the cut-off, there should be a higher mass of observations to the right of the cut-off, given that incumbents often comes from the party that most voters prefer. This would explain the observed sorting pattern found in Snyder (2005).

The second concern is about systematic but legal sorting into winning. Imagine a scenario where certain candidates have better information about their prospects of winning so that they can work harder and spend more money if they were about to lose the election.[23] In contrast to election fraud which should probably yield a higher mass of observation directly around the cut-off, legal sorting into winning might be displayed as a higher mass a bit away from the cut-off if politicians cannot perfectly control their final vote share. If there is systematic sorting into winning, the bare winners and the bare losers would be different because the bare winners have self-selected into winning. There are some empirical evidences that the winners and the losers in U.S. House elections are in fact different. Grimmer et al. (2011) show that candidates that win a close election to the House of Representatives are more often from the party with a stronger power base on the state level.[24] Caughey and Sekhon (2011) argue that candidates winning in close elections spend more money than those who lose with a small margin and that the winners are also more frequently those who were expected to win in ratings before the election. The authors also demonstrate that the winners and the losers in close elections are not balanced in terms of tenure, incumbency status and experience.[25] Eggers et al. (2015) argue that the observed covariate sorting pattern for in Caughey and Sekhon (2011) essentially boils down to incumbency, where the imbalance in other covariates disappear when controlling for incumbency status. They assess this by also examining other electoral setting and earlier time periods for U.S. House elections. Eggers et al. (2015) con-

[23]See the discussion in (Eggers et al., 2015, p. 267-269). Lee (2008) points out that this relates to a more general case where agents are aware of the cut-off. He gives an another example with admittance to higher education based on an entrance exam, where individuals are aware of the entrance threshold are hence work to get above the threshold.

[24]Grimmer et al. (2011) also point out that when considering larger bandwidths and not just observations exactly at the cut-off, winners and losers are likely to differ. Snyder et al. (2015) argue that the imbalance found in Grimmer et al. (2011) is due to political party imbalance.

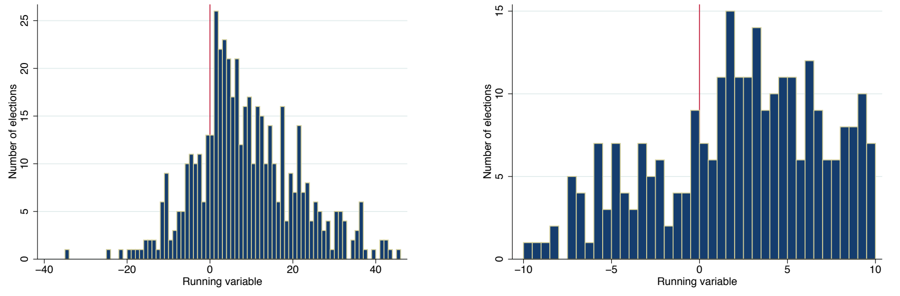
[25]Caughey and Sekhon (2011) conclude that important assumptions behind RD do not seem to be met when using U.S. House data. In spite of these concerns, the RD design is probably still better than the empirical alternatives according to the authors.

clude that the observed sorting pattern for incumbency in U.S. House elections after 1946 may be a result from statistical chance. Eggers et al. (2015) also discuss and dismiss potential mechanisms that can explain the sorting in U.S. House election given that they were not a result from statistical chance. The authors point out that even if certain candidates are more successful in winning elections *ex post* it does not mean that they can perfectly self-select into winning before the election.

An important condition for systematic legal sorting of certain candidates is that they are better in predicting election outcomes and hence able to adjust their campaign effort. Enos and Hersh (2015) provide survey evidence with data from American election campaigns in 2012. They investigate how good campaign staffers are in interpreting polling data and predicting election outcomes. The authors both analyses the presidential election, elections to Congress and state elections. In general, campaign staffers seem to be overconfident about winning and they provide relatively inaccurate predictions about the election outcome. Staffers on the Obama presidential campaign for instance mispredicted the election outcome by approximately 8 percentage points on average in their respective state. Inaccuracy in predicting election results seems to be in general worse in open elections (the elections I consider) according to the authors. Klarner (2008) applies statistical forecasting – sometimes used by political campaigns - to elections to the U.S. Congress. It seems that these predictions miss the actual election outcome by several percentage points, see (Enos and Hersh, 2015, p.269). My reading of this literature is that even if we observe a higher mass of observation on one side of the cut-off, it does not mean that politicians have perfectly sorted into winning. There is still a random component left that we can use to estimate the effect in a RD as a result of the lack of ability to predict election outcomes.

Let us now assess the data sample of this paper where only open elections are included. The imbalance that is related to incumbency that was analyzed in earlier papers is of less concern when considering open elections since no incumbent is running for reelection. In Figure 2 my running variable is displayed in histograms. The histogram to the left displays the entire distribution. In the histogram to the right, I have zoomed in on the distribution around the cut-off.

Figure 2. Histograms running variable, time period 1968-2013, data analysis sample

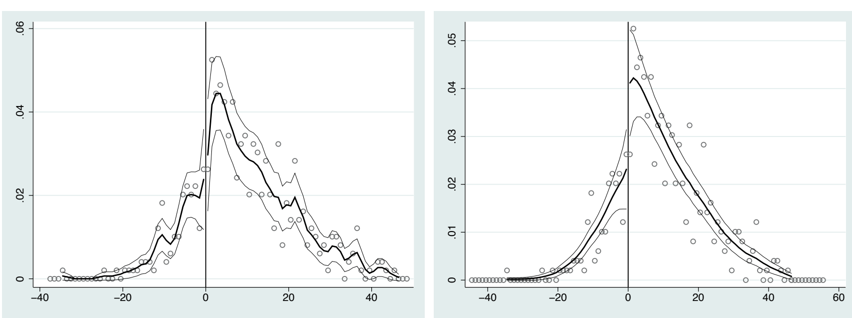


(a) 1 percentage point width for each bar in the histogram

(b) 0.5 percentage point width for each bar in the histogram

There does not seem to be any clear bunching directly around the cut-off and hence no evidences of election fraud or manipulation in close elections. There is almost the exact same number of elections to the left and to the right of the cut-off. It is however clear in the histograms that there is a larger mass of observations to the right of the cut-off. Candidates with earlier experience from elective office win more often and they win with larger margins. I investigate this further by looking at the results from the McCrary density tests discussed in McCrary (2008) in Figure 3.[26]

Figure 3. McCrary tests. Time period 1968-2013. Data analysis sample



(a) Bandwidth: 3. Bin: 1

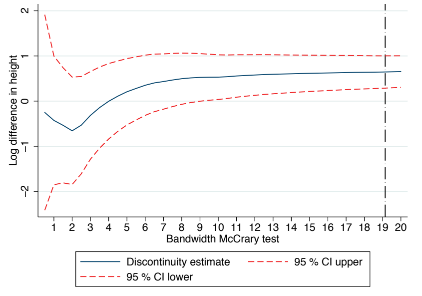
(b) Bandwidth: 10. Bin:1

There are no evidences of manipulation in close elections. There is however a higher mass of observations further away on the right-hand side of the cut-off, which is picked up in the McCrary test with the larger bandwidth. In Figure 4 I have plotted the discontinuity estimate (log

[26]I use the DCdensity command to create the figures below.

difference in height) for various bandwidth specifications in the McCrary test.

Figure 4. McCrary test. Discontinuity estimates. Bin: 0.5



Note: The dashed vertical line is the automatic bandwidth in McCrary (2008)

There are no statistically significant evidence of a discontinuity up until a bandwidth of 9. One has to remember that the power of the test decreases when more narrow bandwidths are applied, but that the bias is larger when the bandwidth is increased. It is however telling that the point estimates seems to move more towards zero when the bandwidth is decreased below 9. In light of the survey evidences presented in Enos and Hersh (2015) it does not seem likely that candidates can perfectly predict election outcomes and hence adjust their campaign effort before the election. The observed pattern of more observations to the right of the cut-off is actually expected if we believe that having earlier experience from elective office is a positive factor for running a successful political campaign. The most probable explanation is that candidates with earlier experience from elective office are better campaigners and hence win more often with larger margins, but that they cannot sort into winning.

In conclusion, there are no evidence of election fraud where politicians manipulate treatment status close to the cut-off. There are indications that candidates with earlier experience from elective office win more often with larger margins, but as long as they cannot perfectly predict the election outcome and select into winning, there is still exogenous variation in treatment. In line with the conclusions in Lee (2008), I argue that there is a random component who wins the election that I can use to estimate the effect of electing a candidate with prior experience from elective office. I should however focus on narrower bandwidth where I can be surer to have no legal self-selection into winning.

6.2 Test of identifying assumptions

To investigate in more detail the candidates' background, I have collected data on their earlier profession for the years 2006-2010. This analysis addresses whether challengers with and without earlier elective experience are similar in terms of professional experience.[27] It would not be informative to run a RD analysis for these three elections when I only look at open elections, since they would yield very few observations. Table 3 is instead a balance test for all challengers. The variable ''ExpChallenger" takes the value 1 if the challenger has earlier experience from elective office prior of being elected to the House of Representatives. Each row in the Table 3 is a simple regression where the variables in the vertical column are dummy variables. Note that a candidate may be coded as having multiple earlier professions. I have included all challengers running against an incumbent in the balance test.[28]

Table 3. *Balance test, professional background, challengers*

	ExpChallenger
Democrat	-0.0267 (0.0409)
Republican	0.00263 (0.0408)
Public Service	0.0563* (0.0307)
Bussiness/Banking	-0.0640 (0.0397)
Congressional Aide	-0.0259 (0.0167)
Military Service	-0.0510 (0.0333)
Medicine	0.0146 (0.0211)
Clergy	-0.0277** (0.0136)
Agriculture	0.0214 (0.0145)
Education	-0.0146 (0.0293)
Construction	-0.0274 (0.0188)

Notes: Standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Each row is a separate regression, where the dependent variable is displayed on the row. The number of observations is 1,053.

[27] At an earlier stage I ran a separate analysis for the effects of having earlier experience from policy close workplaces on my outcome variables. After further consideration, I choose to focus on earlier elective experience and use my profession data as a balance test, because these professions and elective experience is overlapping. The reader should note that the data set regarding earlier profession for the losers are is based on self-described measures.

[28] This is not a RD estimation and I do not need to restrict it to only open elections to investigate the difference. If I would restrict the sample to only open elections, I would end up with very few observations in each group since I only have data from 2006-2010. Such a test is presented Table A1 in the appendix.

It seems that there are slightly fewer with clergy background in the treatment group (approximately 3 percentage point less). There are also some evidence that there are slightly more people with a previous career in public service, but this point estimate is only statistically significant at the 10 percent level.[29] We should however expect to see some differences between the two groups out of pure chance when running several individual tests. What is interesting is that there is no statistical difference for challengers between the mean of Democrats and the mean of Republicans with prior elective experience. Gender and age are two additional variables that I would like to run balance tests on, but unfortunately I do not have such data. The conclusion from this balance test is that earlier elective experience is evenly distributed between different professions and party affiliations for these later elections.

The regression discontinuity design hinges on random assignment around to the cut-off. Characteristics associated with the congressional districts should therefore be evenly distributed around the threshold. I investigate this by analyzing two set of characteristics. In the first test I create dummy variables for whether a district belongs to a specific geographical region the U.S. and use these dummy variables as outcomes. In total four regional dummies are generated: New England, which is a cultural and historical area in the northeast corner, the Northern Tier consisting of all states that border Canada, the South which includes all states that formed the Confederation in the 19th century and the West incorporating the pacific states together with the mountain states. For this robustness check I have data for the years 1968 to 2012. The outcome variables in the second test are shares of different socioeconomic groups in each election district. I consider the share of military population, share of blue collar workers, share of farmers, share of people that are foreign born, the number of cities within the election district, share of women, the share of people older than 65, the share of people under 20, the share of African Americans, the share of high school graduates, share of inhabitants living in urban areas,[30] log population density and lastly log population. For this robustness check I have data for the years 1984 to 2004.

Because I run 17 different specifications, it is important to assess the overall results since it is expected that some results are significant due to chance. The results are presented in Figures A1 – A5 and Tables A3 – A19 in the appendix. According to the RD-plots, there are no

[29] I also find that there are more lawyers and attorneys in the treatment group, which is expected since elective legal positions such as attorney general are included in the treatment group. See also Table A2 in the appendix for a separate analysis where I drop observations where I am less sure how to classify the candidate's profession.

[30] The share of people living in urban areas takes a value smaller than 0 or larger than 1 for some counties in the raw data file. I replace these values as missing.

clear discontinuities except for belonging to the geographical category *West*. Looking at the regression tables and the bandwidth graphs, the overall picture is that the point estimates are small and they are seldom significantly different from 0. I find large and statistical significant results for the share of people living in urban areas for two bandwidth specifications.[31] The point estimates sometimes vary in size for smaller bandwidth specifications, but these specifications are based on few observations and they should be interpreted with caution. The conclusion is that there seems to be balance in observable covariates on the election district level implying that the RD randomization in this sample with open elections seems to have worked.

7 Results

In this section the main results are presented. I start by presenting the results for the OLS analysis, followed by RD-plots and RD-regression results. Two specifications are included in the RD analysis: one specification where the outcomes are measured in the mandate period following the open elections and one specification for more long-term effects. The section is ended with a summary of the empirical findings. Additional regression tables and specifications mentioned in this section can be found in the appendix.[32]

7.1 OLS Analysis

Table 4 displays the OLS results. In the first column, the outcome is a dummy variable indicating whether a newly elected representative was a member of a big4 committee during the mandate period after the open election. For the other three dependent variables, the outcome is specified as the difference between the mandate period after the election, t and the mandate period before, $t-1$. The difference in directed federal spending (in millions) and personal income per capita is expressed in real values in USD.[33] The difference in unemployment rate is expressed in percentage points between 0 and 100. The variable of interest, *Experiencewinner*, takes the value 1 if the candidate that won the open

[31] Some point estimates are statistically significant on the 10 percent for some bandwidth specifications. See the tables in the appendix.

[32] In the appendix, I usually just present the bandwidth graphs for the RD analysis. A 30 pages supplementary material including RD-plots and regression tables is available by the author upon request.

[33] Directed federal spending is expressed in 2004 years value and personal income per capita in expressed in 2010 years value.

election had earlier experience from elective office. The mean of the outcome variables are presented at the bottom of the table.

Table 4. *OLS results*

VARIABLES	(1) MemberBig4	(2) SpendingDif	(3) IncomeCapitaDif	(4) UnemploymentDif
Experiencewinner	0.108***	32.73	334.4	0.305
	(0.0380)	(40.59)	(271.9)	(0.202)
Constant	0.175***	28.27	410.1*	-0.501***
	(0.0326)	(35.05)	(229.4)	(0.172)
Observations	668	425	861	432
R-squared	0.012	0.002	0.002	0.005
Mean value outcome variable	0.453	36.18	770.6	-0.0437

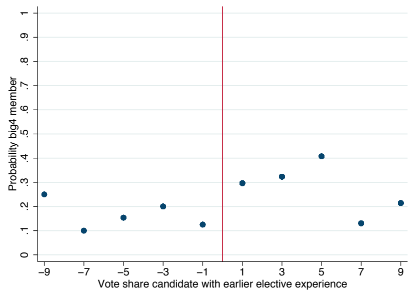
Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

There is a positive relation between having earlier elective experience and the probability of ending up in a big4 committee after the election (an increase by 11 percentage points). For the other three variables, the point estimates are positive, but not statistically different from 0.

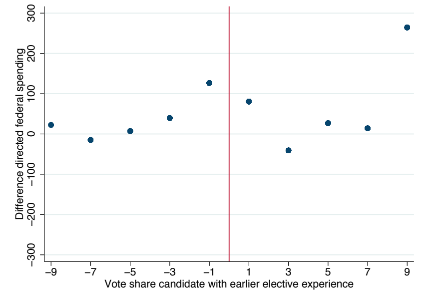
7.2 RD-plots

I start by visually inspecting the data for various bins on both sides of the cut-off in RD-plots in Figure 5. In order to be as transparent as possible, the raw RD-plots are displayed. The running variable is vote share in percentage points for the candidate with earlier elective experience where the cut-off is set to 0. Hence, a positive value implies that the candidate with earlier elective experience won the election and vice-versa. In all of the RD-plots presented in the paper, a bin size of 2 is used and the RD-plots are displayed for values of the running variable between -9 percentage points and 9 percentage points from the cut-off. I choose this range of the running variable in order to remain within the window of random assignment previously discussed in the section where the McCrary tests were presented.

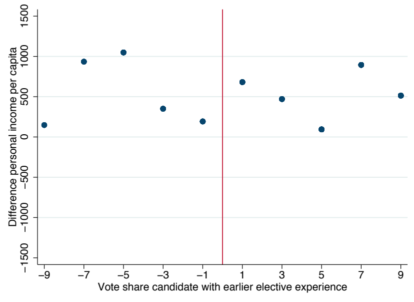
Figure 5. RD plots



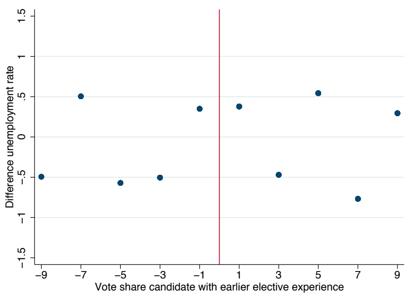
(a) Membership big4 committee



(b) Difference directed federal spending



(c) Difference personal income per capita



(d) Difference unemployment rate

Let us start by focusing on the probability of being a big4 committee member. Just to the left of the cut-off, the probability that the elected representative had a seat on a big4 committee is just above 10 percent. To the right of the cut-off, the probability is instead around 30 percent, meaning that the probability is increased by approximately 20 percentage points if the newly elected representative had earlier elective experience. This effect should be considered large and it is in line with the theoretical predictions. Notably, the probability of being a member of a big4 committee is substantially higher just to the right of the cut-off, but then it falls further to the right when the candidate with earlier elective experience won the election with a safer winning margin. This could suggest that the party leadership is rewarding those with earlier elective experience winning in swing districts with a better committee placement. One potential reason would be that the political leadership wants to protect representatives with elective experience that sit on more vulnerable seats with a better committee placement which could increase their reelection possibilities.

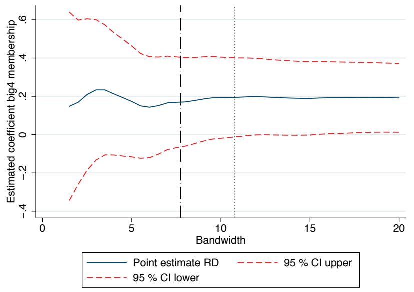
There seems also to be a positive effect for the difference in personal income per capita, which is also in line with the theoretical prior. To the left of the cutoff, the difference from the previous mandate period is just above 0, but to the right of the cutoff, the difference is about 600 USD. This could be compared to the average personal income per capita which was approximately 34 000 USD. The difference is thus not very large and it should also be noted that the plotted bins are scattered further away from the cutoff. For directed federal spending, the effect is not as easily interpreted. There seem to be a small decrease just to the right of the cutoff, but what is interesting is the overall picture where there is a plateau around the cutoff. Districts where the election was close – regardless whether the candidate had or did not had earlier elective experience – receive more directed federal spending in comparison to the mandate period before the open elections. For unemployment rate, the effect seems to be 0 and the observations are spread out, meaning that there are no clear evidences of an effect.

To assess these results in more detail I continue to the regressions. A local linear specification for various bandwidths is applied in the main analysis below. The reader may find local polynomial specifications as a robustness check in Figures A8 and A9 in the appendix. The most transparent way of displaying RD estimates is to run the RD for various bandwidth specifications and plot the RD estimates and the confidence intervals in a figure. These figures display whether the point estimates are stable for different bandwidth specifications. Each bandwidth graph is ran on bandwidths between 1.5 and 20 with 0.5 increase in each step (37 regressions in total).[34] The estimations are carried out by RdRobust presented in Calonico et al. (2014) where observations closer to the cut-off are weighted more heavily by a triangular kernel. The confidence intervals in the figures are the standard confidence intervals. Two optimal bandwidth specifications are presented: The optimal bandwidth in Imbens and Kalyanaraman (2012) and the one in Calonico et al. (2014). The dotted vertical line in the figures corresponds to the optimal bandwidth suggested by Imbens and Kalyanaraman (2012). The dotted dashed vertical line corresponds to the bandwidth suggested by Calonico et al. (2014). The same principle is applied for all the bandwidth graphs in the paper (including those in the appendix). The corresponding regression tables are presented in Tables A20, A21, A22 and A23 the appendix. In the regression tables, I also report the robust p-value which is based on the robust confidence intervals derived in Calonico et al. (2014).

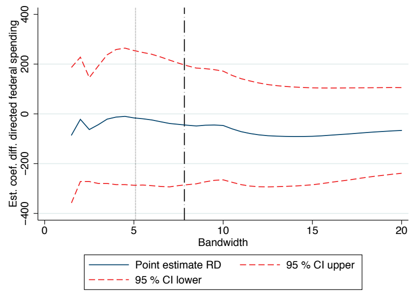
[34]Because I am often left with less observations than 30 when running the 1.5 bandwidth I do not go further below. 1.5 is also more the half of the optimal bandwidths.

7.3 RD Estimation results

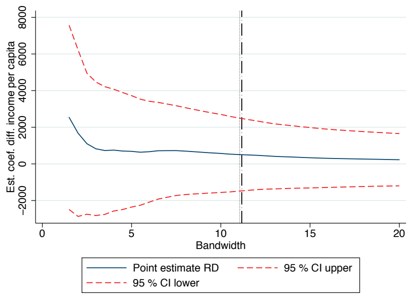
Figure 6. Bandwidth graphs



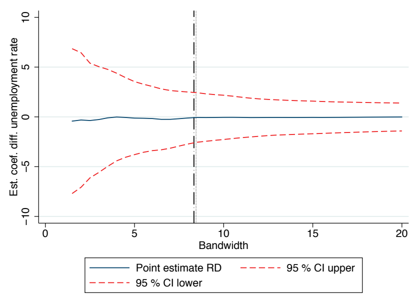
(a) Membership big4 committee



(b) Difference directed federal spending



(c) Difference personal income per capita



(d) Difference unemployment rate

Note: The dotted vertical line corresponds to the optimal bandwidth suggested by Imbens and Kalyanaraman (2012). The dotted dashed vertical line corresponds to the bandwidth suggested by Calonico et al. (2014). All specifications are local linear specifications

If we consider both these bandwidth graphs and the regression tables presented in Tables A20, A21, A22 and A23 in the appendix some pattern emerges. For committee membership, the effect seems stable and positive across different specifications of the bandwidth, but seldom not statistically significant. Considering the point estimates in Table A20 they are between 0.15 and 0.21, implying that the probability that the elected representative has a seat on a big4 committee increases by 15-21 percentage points if the candidate had earlier experience from elective office. This is a large increase. The reader should note that I am only left with very few observations when the bandwidth is narrowed, which could explain the loss of statistical significance due to low statistical power. In comparison to the OLS result, the point estimates are larger in the RD for committee membership.

For personal income per capita, the point estimates are positive as in the OLS analysis and they grow in magnitude closer to the cut-off. In Table A22, we can see that the coefficient for the specification with the narrowest bandwidth is above 2000 USD, meaning that the difference in comparison to the last mandate period is about 5.9 percent of the total mean income per capita which was approximately 34 000 USD. These estimations are however only based on very few observations and should be interpreted with caution. The point estimates are however never statistically significant.

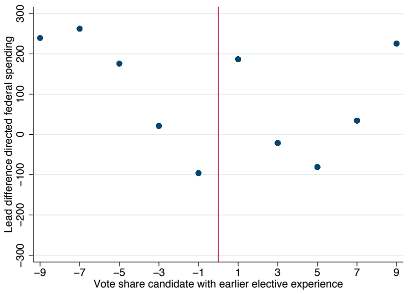
The point estimates are all negative for directed federal spending (in comparison to the OLS results which were positive), but never statistically significant. The size of the point estimates is also sensitive to the choice of the bandwidth (see Table A21 in the appendix). Considering the smallest bandwidth specification, the point estimates equals approximately -86, which is fairly large if we compare it to the average difference in directed federal spending which is 56 million USD. The distribution of this variable is however very skewed, where the difference is much more than 56 million USD for some districts. The maximum difference for a district was approximately 4675 million USD for the years included.

For unemployment rate, the coefficients are negative, which is in line with the point estimates for personal income per capita since personal income per capita and unemployment rate should be positively correlated. I thus find an opposite sign of the point estimate in comparison to the OLS result. The point estimates are however very sensitive to the bandwidth specification and the coefficients are small (see Table A23 in the appendix). As with personal income per capita, the magnitude of the point estimates grows when a narrower bandwidth is applied. Average unemployment rate for the entire period was approximately 6.5 percentage points. Looking at the robust p-values in Tables A20, A21, A22 and A23, they never indicate any statistically significant effects for any of the outcome variables.

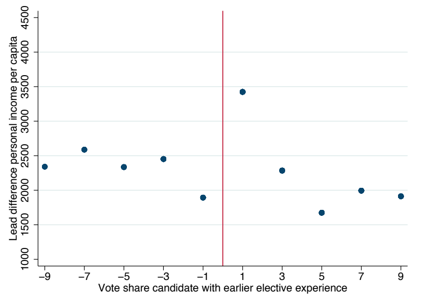
7.4 Long term effects

The effect on directed federal spending, personal income per capita and unemployment rate could manifest itself after some years. A newly elected member could experience a learning period and general outcome variables as personal income per capita and unemployment take time to change. Berry et al. (2010) for example found that representatives that are newly elected are less able to secure directed federal spending. I have therefore run specifications where I look at the difference between time period $t + 2$ after the open election and time period $t - 1$ before the election.

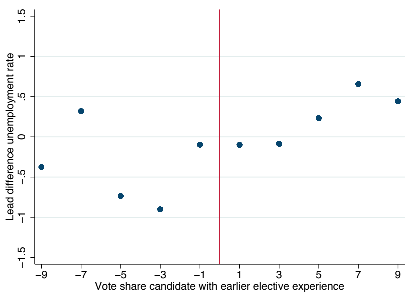
Figure 7. RD plots. Results for lead difference $t + 2$ and time period $t - 1$



(a) Difference directed federal spending



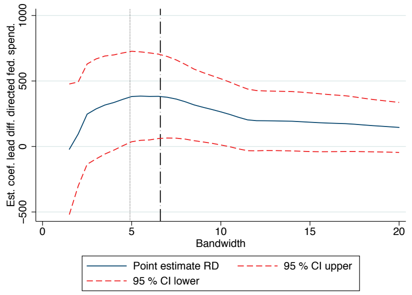
(b) Difference personal income per capita



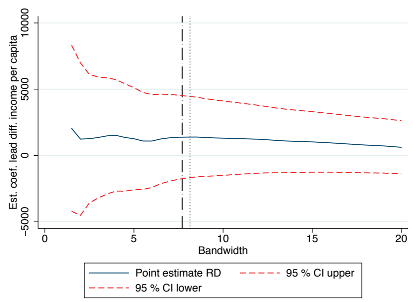
(c) Difference unemployment rate

In Figure 7 the variables directed federal spending and personal income per capita increase sharply to the right of the cut-off. To the right of the cut-off, candidates with earlier elective experience that just won an open election receive more directed federal spending to their districts than those who won with a safer margin. The fact that the plotted bins are spread out further away from the cut-off, means that the effects should be interpreted with caution. This could indicate that the party leadership targets representatives with elective experience from vulnerable districts. For personal income per capita, there are indications that districts that just elected a candidate with earlier elective experience prosper more in comparison to districts that just elected a candidate without earlier elective experience. The reader should note that the plotted outcomes are positive and larger in magnitude in comparison to the ones in Figure 5. This should be interpreted as a general economic development since I take the difference from time period $t - 1$. The difference is however larger for those cases where a representative with elective experience just won the election, which could again indicate that that those representatives are targeted by the political leadership. For unemployment rate, the effect seems to be zero.

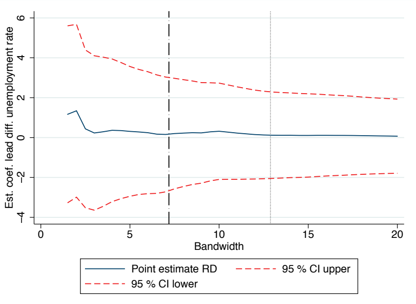
Figure 8. Bandwidth graphs. Results for lead difference $t + 2$ and time period $t - 1$



(a) Difference directed federal spending



(b) Difference personal income per capita



(c) Difference unemployment rate

Note: The dotted vertical line corresponds to the optimal bandwidth suggested by Imbens and Kalyanaraman (2012). The dotted dashed vertical line corresponds to the bandwidth suggested by Calonico et al. (2014). All specifications are local linear specifications

The corresponding regression Tables A24, A25 and A26 are found in the appendix. The point estimates for personal income per capita are stable and positive across different bandwidth specifications, but never statistically significant. With regard to directed federal spending, the point estimates varies across different bandwidth specifications, but are significant and large for the two optimal bandwidths.

The reader may find additional specifications for the difference between time period $t + 1$ and $t - 1$ in Figure A6 in the appendix. These results points in the same direction as the ones discussed above, but no of the results are statistically significant. I have also run the entire analysis for higher order polynomials and the results presented in Figures A8, A9 and A10 point somewhat fairly in a similar direction as the results presented above.[35]

In conclusion, there are some indicative evidence that having earlier experience from elective office increases the probability that the elected representative receives a seat on a big4 committee. The results for directed federal spending are multifold. There seems to a positive effect of electing a candidate with earlier elective experience on the amount of directed federal spending to the district, but the effect manifests itself some years after the open election. Both these results should be interpreted with caution since the results are not always statistically significant. Considering the RD-plots, the plotted bins are spread out further away from the cut-off, which indicates that there are something else than just electing a candidate with earlier elective experience around the cut-off that affects these outcome variables. I find no robust or statistically significant effects on personal income per capita or unemployment rate.[36]

8 Robustness analysis

The robustness analysis is presented in this section. I focus the discussion on committee membership and directed federal spending where I found indications of effects in the main analysis. Results for all outcomes are however presented in the appendix.

The first robustness check is a specification with lagged dependent variables. I do not expect to find any effect in time period $t-1$, since this was before the open election took place. The results are presented in Figure A11 in the appendix. For directed federal spending, the difference in now specified between time period $t-1$ and time period $t-2$ before the open elections. I find no statistical significant results for committee membership or directed federal spending. This indicates that nothing

[35] I have run the analysis for both a local quadratic specification and a third order local polynomial specification. The reader should however direct the attention to the local quadratic specification given the critique against higher order polynomials in Gelman and Imbens (2014). The results in my case are however somewhat fairly similar.

[36] As mentioned earlier, I have also run the analysis for the number of times a candidate stands witness in front of a congressional committee. I find no statistically significant effect for this outcome (see Figure A7 Table A27) and the point estimates are sensitive to the choice of bandwidth.

prior of the open election influences the results, which yields credibility to my main findings.[37]

I analyze committee membership for the years 1979-2013 in the main analysis. There is an additional data set available with older data, but the definitions of committees may have changed somewhat over the years and may not be entirely comparable. A separate analysis with this older data is presented in Figure A12. The results indicate no effect of having elective experience on big4 committee membership in comparison to the results with more recent data, where I found indications of an effect. The most likely explanation for these different effects is probably changes in the organizational structure in the House of Representatives.

Assume that the very close elections are special. They might receive much more media attention and involvement from political pundits outside of the congressional district. This does not per se invalidate the RD design, but to see whether the main results are stable, I run donut estimations where observations of the running variable ± 0.5 percentage points from the cut-off are dropped. The results are presented in Figures A13 and A14.[38] The donut estimation results are more sensitive to the choice bandwidth, which is probably a results of having fewer number of observations overall. The estimated coefficients for committee placement are slightly smaller and I have one negative coefficient significant on the 10 percent level for the 1.5 bandwidth. For directed federal spending, the estimated effect is now stable around 0 for the main specification, but positive – as in the main analysis – and close to statistically significant when looking at the lead difference between time period $t + 2$ and $t - 1$.

In the main analysis, the treatment group includes those districts that elected a candidate with any sort of prior elective experience. I have also run a separate analysis for state legislators.[39] The control group includes those districts were the elected representative had no prior political experience from the state legislature. The results are presented in Figures A15 and A16. The estimated coefficient for the probability of receiving a seat on a big4 committee is slightly larger than the estimated coefficients in the main analysis and some of the point estimates are now statistically significant. I have however one negative and statis-

[37] Another standard robustness test is to use a lagged running variable. Since I only include open elections, many districts are only included once in the panel, meaning that I would end up with mostly missing observations where the lagged running variables may originate from an election several years ago for the rest of the observations.

[38] I do not have enough observation to run the donut estimation with RDRobust for the 1.5 bandwidth for directed federal spending or unemployment rate.

[39] I do not have enough observation to run the estimation with RDRobust for the 1.5 bandwidth for directed federal spending or unemployment rate.

tically significant coefficients for committee membership for the smallest bandwidth. For directed federal spending, the results points in a similar direction as those in the main analysis but the point estimates for the lead specification are no longer statistically significant.

I have also run an analysis where all incumbents are coded as having earlier elective experience together with the challengers that have earlier elective experience. All elections are included in this specification.[40] Former representatives that run again are now included and the results are presented in Figure A17. I find no statistically significant effects for committee placement or directed federal spending. The coefficient for committee membership is always positive and between 10 and 30, meaning that the probability that a candidate with earlier elective experience has a higher probability of securing a seat on an important committee. For directed federal spending, the estimated coefficient is sensitive to the choice of bandwidth, but seems to converge towards a zero effect.

8.1 Additional specifications

I have run a separate analysis for partisan effects on the same outcomes to contrast the findings in the main analysis. Elections to the House almost always stand between two candidates from the two major political parties. To connect to the literature on partisan effects, I have also estimated the effect of electing a Democrat over a Republican. Both an analysis for open elections and an analysis for all elections has been conducted. The results are presented in Figures A18 and A19. One point estimate for unemployment is statistically significant and very large (in both specifications), but these results should be interpreted with caution since it is based on very few observations.[41] I have also run an analysis for vote share in the next elections (Figure A20) for the candidate that won the open election. Having earlier elective experience when running in the open election does not seem to benefit a candidate when running in an upcoming election.

9 Concluding remarks

The main motivation to start writing this paper was the intense public debate in the United States regarding politicians' backgrounds. I have investigated the effect of electing a candidate with earlier elective

[40] This means that I can consider smaller bandwidths since I have more observations to work with.

[41] I also find a positive and large effect on electing a Democrat on the number of times a representative stand witness in front of a congressional committee when considering all elections.

experience to the U.S. House of Representatives, which is one of the most debated differences between candidates running for federal office. Hopefully, my results may contribute with some substance to this heated debate.

Overall, I do not find many statistically significant results meaning that my results should be interpreted with caution. There are some indications that having earlier elective experience increases the probability of getting a seat on an important big4 committee (at least when looking at more recent data). There are also some evidences that candidates with earlier elective experience are more successful in securing directed federal spending to the home district, but this effect seems to manifest itself some years after the election. Since I restrict my analysis to open elections, my data sample is relatively small. The non-significant point estimates might be a result of low statistical power. For personal income per capita and unemployment rate, I find no statistically significant or robust results.

Although the empirical results provide a somewhat unclear picture about the effect of having earlier elective experience, the fact that I find some indications of effects for two of the variables is interesting. The results provide some indications that the earlier elective background of a politician is a factor to consider when explaining committee membership and directed federal spending.

Future papers should focus more on the intermediate step between electing a candidate with earlier elective experience and outcomes such as personal income per capita and unemployment. Is it so that these politicians with earlier experience from elective office get more things done in the House? One way to investigate this would be to look at the legislative bills these people sponsor in the House in more detail. Another interesting aspect to investigate is whether elective experienced representatives cater better to the ideological position of the median voter in the home district. It would also be interesting to look at other characteristics than earlier elective experience. Future paper could gather data on the gender of elected representatives, more data on their professional experience, and the age of representatives. In this paper, I have only been able to estimate an average effect, where these variables are included.

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11 Appendix

11.1 Empirical framework section: Balance test, additional specifications

Because of the structure of the data set, I cannot run a balance test on Democrats and Republicans when considering open elections since all are challengers and all are Democrats or Republicans.

Table A1. *Balance test, background challengers, only open elections*

	ExpChallenger
Public Service	0.0943 (0.124)
Bussiness/Banking	-0.0541 (0.133)
Congressional Aide	-0.156* (0.0805)
Military Service	-0.152 (0.126)
Medicine	0.182* (0.0944)
Clergy	-0.0247 (0.0511)
Agriculture	-0.00201 (0.0621)
Education	0.0167 (0.105)
Construction	0.0455 (0.0510)

Notes: Standard errors in parenthesis. *** p < 0.01, ** p < 0.05, * p < 0.1. Each row is a separate regression, where the dependent variable is displayed on the row. The number of observations is 105 in total.

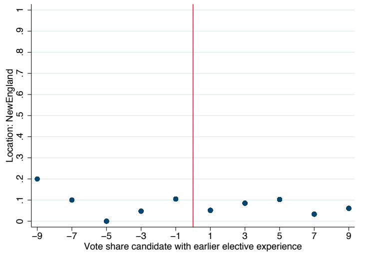
Table A2. *Balance test, background challengers, unclear cases dropped*

	ExpChallenger
Democrat	-0.0177 (0.0421)
Republican	-0.00602 (0.0420)
Public Service	0.0541* (0.0306)
Bussiness/Banking	-0.0648 (0.0406)
Congressional Aide	-0.0226 (0.0170)
Military Service	-0.0372 (0.0343)
Medicine	0.0211 (0.0217)
Clergy	-0.0237* (0.0133)
Agriculture	0.0212 (0.0143)
Education	-0.00864 (0.0301)
Construction	-0.0203 (0.0188)

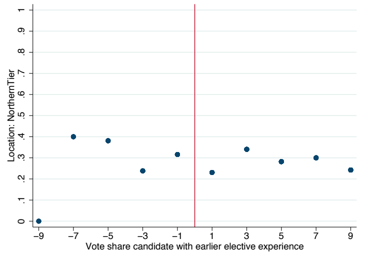
Notes: Standard errors in parenthesis. *** p < 0.01, ** p < 0.05, * p < 0.1. Each row is a separate regression, where the dependent variable is displayed on the row. The number of observation is 1,012.

11.2 Empirical framework section: Covariates balance

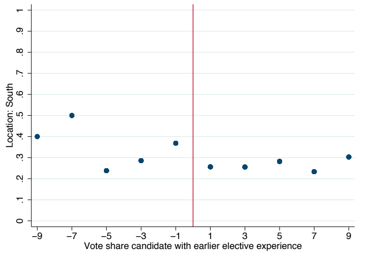
Figure A1. RD plots and bandwidth graphs. Different geographical regions



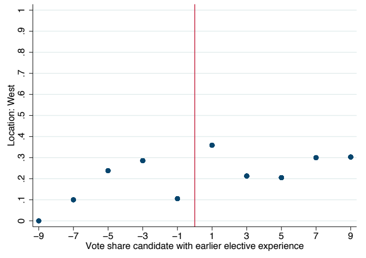
(a) New England



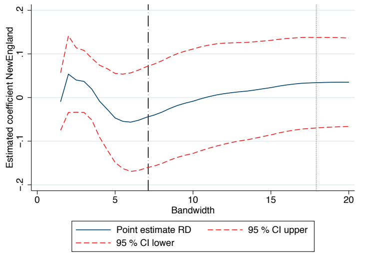
(b) Northern Tier



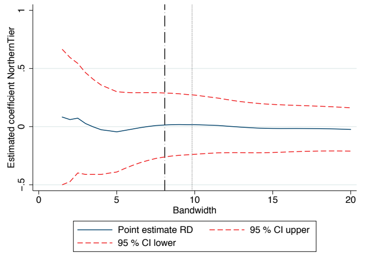
(c) South



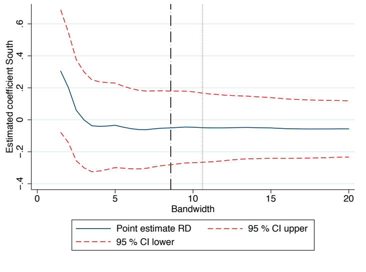
(d) West



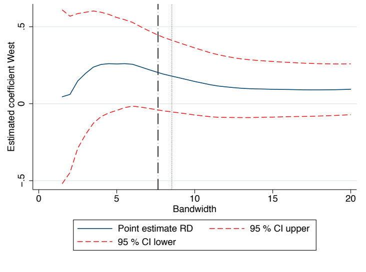
(e) New England



(f) Northern Tier



(g) South



(h) West

Table A3. *Geographical area: New England*

VARIABLES	(1) NewEngland	(2) NewEngland	(3) NewEngland	(4) NewEngland	(5) NewEngland	(6) NewEngland
RD_Estimate	-0.00842	-0.0468	0.0401	-0.00916	-0.0446	0.0341
	(0.0610)	(0.0521)	(0.0377)	(0.0336)	(0.0592)	(0.0529)
Observations	264	154	75	41	211	385
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.264	0.115	0.803	0.274	0.274	0.190
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A4. *Geographical area: Northern Tier*

VARIABLES	(1) NorthernTier	(2) NorthernTier	(3) NorthernTier	(4) NorthernTier	(5) NorthernTier	(6) NorthernTier
RD_Estimate	0.0169	-0.0449	0.0727	0.0826	0.0141	0.0173
	(0.130)	(0.176)	(0.240)	(0.297)	(0.140)	(0.131)
Observations	264	154	75	41	228	262
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.904	0.863	0.738	0.460	0.901	0.793
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A5. *Geographical area: South*

VARIABLES	(1) South	(2) South	(3) South	(4) South	(5) South	(6) South
RD_Estimate	-0.0462	-0.0343	0.0604	0.305	-0.0504	-0.0496
	(0.113)	(0.135)	(0.161)	(0.195)	(0.118)	(0.110)
Observations	264	154	75	41	237	276
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.725	0.921	0.0660	0.0240	0.709	0.834
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

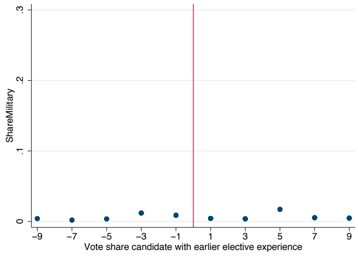
Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A6. *Geographical area: West*

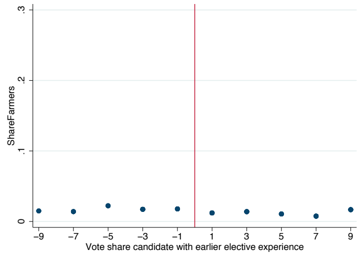
VARIABLES	(1) West	(2) West	(3) West	(4) West	(5) West	(6) West
RD_Estimate	0.144	0.258*	0.148	0.0450	0.203	0.180
	(0.111)	(0.154)	(0.223)	(0.288)	(0.124)	(0.119)
Observations	264	154	75	41	222	237
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.0704	0.408	0.875	0.497	0.0929	0.184
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

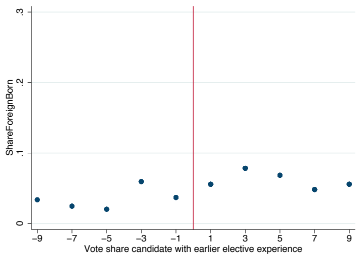
Figure A2. RD-plot: Demographic characteristics 1



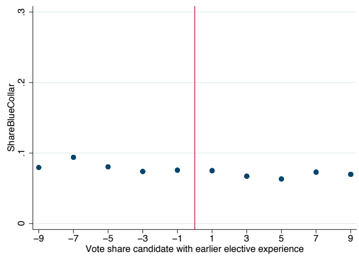
(a) Share Military



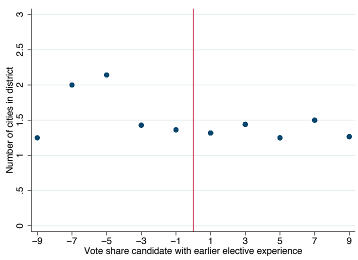
(b) Share Farmers



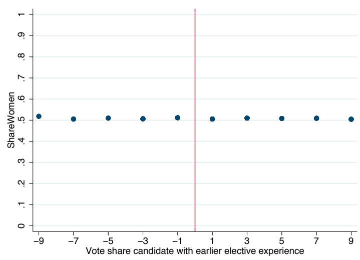
(c) Share Foreign Born



(d) Share Blue Collar

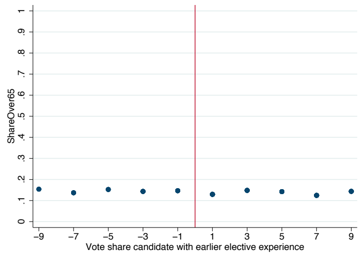


(e) Number cities in election district

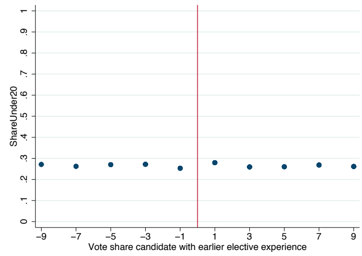


(f) Share women

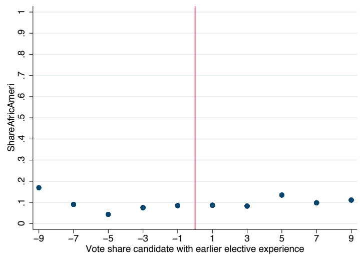
Figure A3. RD-plot: Demographic characteristics 2



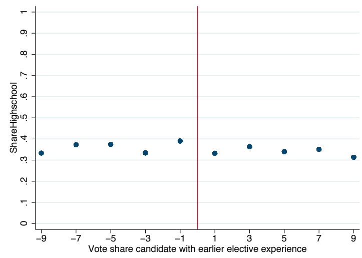
(a) Share people over 65



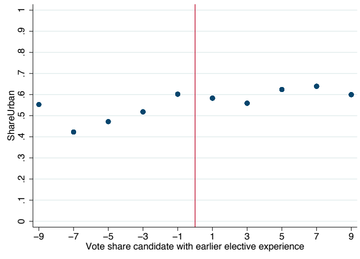
(b) Share people under 20



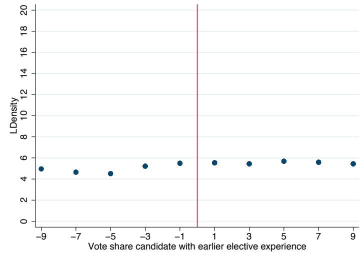
(c) Share African Americans



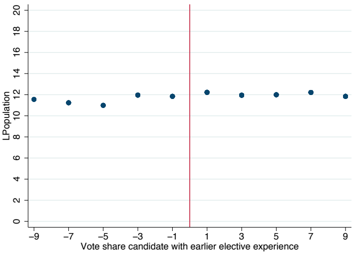
(d) Share high school education



(e) Share people in urban areas

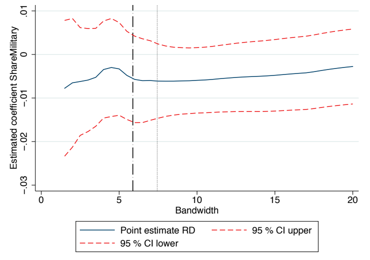


(f) Log population density

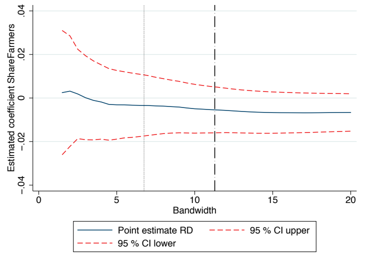


(g) Log population

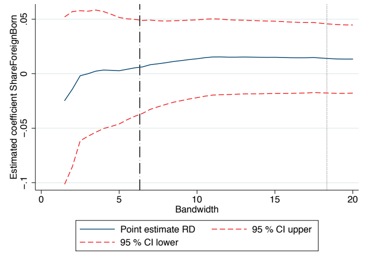
Figure A4. Bandwidth graphs: Demographic characteristics 1



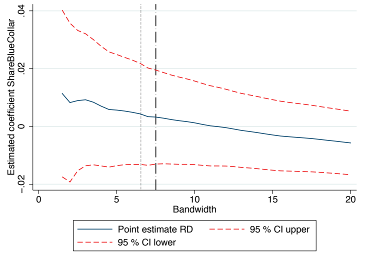
(a) Share Military



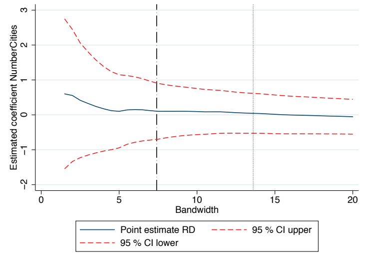
(b) Share Farmers



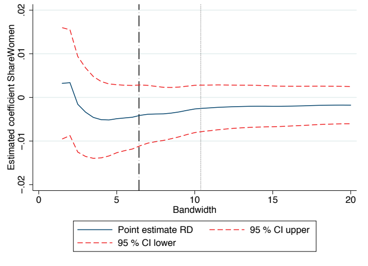
(c) Share Foreign Born



(d) Share Blue Collar

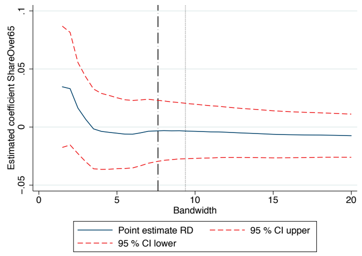


(e) Number cities in election district

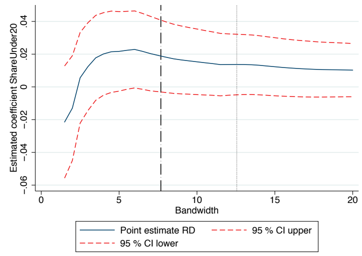


(f) Share women

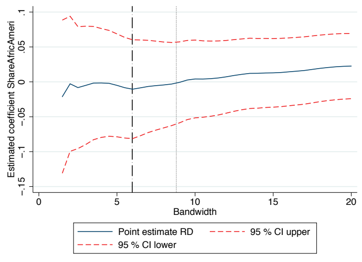
Figure A5. Bandwidth graphs: Demographic characteristics 2



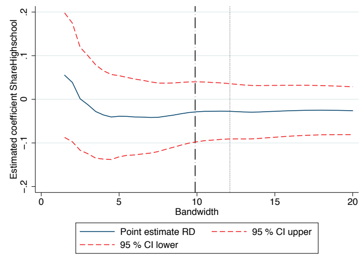
(a) Share people over 65



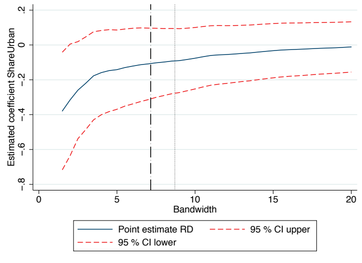
(b) Share people under 20



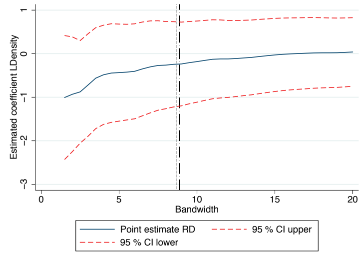
(c) Share African Americans



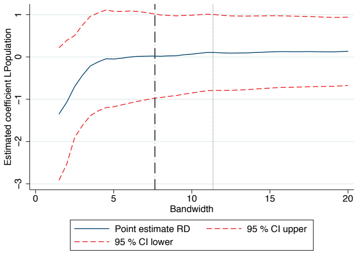
(d) Share high school education



(e) Share of people living in urban areas



(f) Log population density



(g) Log population

Table A7. *Share Military*

VARIABLES	(1) ShareMilitary	(2) ShareMilitary	(3) ShareMilitary	(4) ShareMilitary	(5) ShareMilitary	(6) ShareMilitary
RD_Estimate	-0.00594	-0.00331	-0.00621	-0.00777	-0.00557	-0.00611
	(0.00383)	(0.00543)	(0.00631)	(0.00795)	(0.00509)	(0.00438)
Observations	135	85	46	23	90	110
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.338	0.385	0.380	0.526	0.310	0.360
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A8. *Share Farmers*

VARIABLES	(1) ShareFarmers	(2) ShareFarmers	(3) ShareFarmers	(4) ShareFarmers	(5) ShareFarmers	(6) ShareFarmers
RD_Estimate	-0.00493	-0.00312	0.00186	0.00248	-0.00543	-0.00340
	(0.00572)	(0.00804)	(0.0105)	(0.0146)	(0.00537)	(0.00716)
Observations	135	85	46	23	147	102
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.845	0.939	0.902	0.563	0.424	0.806
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A9. *Share Foreign Born*

VARIABLES	(1) ShareForeignBorn	(2) ShareForeignBorn	(3) ShareForeignBorn	(4) ShareForeignBorn	(5) ShareForeignBorn	(6) ShareForeignBorn
RD_Estimate	0.0137	0.00273	-0.00186	-0.0246	0.00561	0.0140
	(0.0182)	(0.0249)	(0.0304)	(0.0391)	(0.0221)	(0.0162)
Observations	135	85	46	23	97	196
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.936	0.997	0.463	0.281	0.959	0.652
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A10. *Share blue collar workers*

VARIABLES	(1) ShareBlueCollar	(2) ShareBlueCollar	(3) ShareBlueCollar	(4) ShareBlueCollar	(5) ShareBlueCollar	(6) ShareBlueCollar
RD_Estimate	0.00124	0.00566	0.00895	0.0114	0.00324	0.00430
	(0.00738)	(0.00981)	(0.0124)	(0.0147)	(0.00829)	(0.00889)
Observations	135	85	46	23	112	98
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.523	0.445	0.481	0.0605	0.552	0.474
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A11. *Number of cities within election district*

VARIABLES	(1) NumberCities	(2) NumberCities	(3) NumberCities	(4) NumberCities	(5) NumberCities	(6) NumberCities
RD_Estimate	0.0948	0.102	0.417	0.602	0.105	0.0444
	(0.337)	(0.534)	(0.838)	(1.095)	(0.412)	(0.292)
Observations	135	85	46	23	110	164
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.791	0.649	0.507	0.379	0.771	0.853
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A12. *Share women*

VARIABLES	(1) ShareWomen	(2) ShareWomen	(3) ShareWomen	(4) ShareWomen	(5) ShareWomen	(6) ShareWomen
RD_Estimate	-0.00264	-0.00488	-0.00157	0.00323	-0.00419	-0.00253
	(0.00277)	(0.00398)	(0.00559)	(0.00651)	(0.00358)	(0.00272)
Observations	130	82	44	24	93	133
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.204	0.569	0.309	0.230	0.230	0.460
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A13. *Share people older than 65*

VARIABLES	(1) ShareOver65	(2) ShareOver65	(3) ShareOver65	(4) ShareOver65	(5) ShareOver65	(6) ShareOver65
RD_Estimate	-0.00365	-0.00530	0.0164	0.0347	-0.00328	-0.00342
	(0.0119)	(0.0156)	(0.0200)	(0.0266)	(0.0134)	(0.0122)
Observations	130	82	44	24	108	127
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.858	0.664	0.146	0.621	0.879	0.724
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A14. *Share people under 20*

VARIABLES	(1) ShareUnder20	(2) ShareUnder20	(3) ShareUnder20	(4) ShareUnder20	(5) ShareUnder20	(6) ShareUnder20
RD_Estimate	0.0153	0.0217*	0.00557	-0.0215	0.0189*	0.0137
	(0.0102)	(0.0124)	(0.0141)	(0.0175)	(0.0112)	(0.00943)
Observations	130	82	44	24	108	154
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.0788	0.512	0.110	0.0725	0.0869	0.597
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A15. *Share African Americans*

VARIABLES	(1) ShareAfricAmeri	(2) ShareAfricAmeri	(3) ShareAfricAmeri	(4) ShareAfricAmeri	(5) ShareAfricAmeri	(6) ShareAfricAmeri
RD_Estimate	0.00414	-0.00501	-0.00821	-0.0212	-0.0104	-0.00166
	(0.0284)	(0.0376)	(0.0446)	(0.0560)	(0.0361)	(0.0299)
Observations	130	82	44	24	86	119
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.706	0.974	0.808	0.598	0.701	0.765
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A16. *Share high school graduates*

VARIABLES	(1) ShareHighschool	(2) ShareHighschool	(3) ShareHighschool	(4) ShareHighschool	(5) ShareHighschool	(6) ShareHighschool
RD_Estimate	-0.0286	-0.0388	0.00120	0.0554	-0.0289	-0.0274
	(0.0350)	(0.0474)	(0.0602)	(0.0728)	(0.0352)	(0.0323)
Observations	133	83	45	24	133	152
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.333	0.852	0.342	0.326	0.413	0.965
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A17. *Share inhabitants living in urban areas*

VARIABLES	(1) ShareUrban	(2) ShareUrban	(3) ShareUrban	(4) ShareUrban	(5) ShareUrban	(6) ShareUrban
RD_Estimate	-0.0761	-0.142	-0.259*	-0.379**	-0.107	-0.0910
	(0.0900)	(0.116)	(0.142)	(0.172)	(0.104)	(0.0947)
Observations	133	83	45	24	102	121
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.218	0.115	0.0332	0.0179	0.265	0.219
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A18. *Log population density*

VARIABLES	(1) LDensity	(2) LDensity	(3) LDensity	(4) LDensity	(5) LDensity	(6) LDensity
RD_Estimate	-0.182	-0.434	-0.877	-1.007	-0.238	-0.240
	(0.475)	(0.569)	(0.601)	(0.726)	(0.492)	(0.496)
Observations	133	83	45	24	122	121
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.441	0.259	0.257	0.571	0.536	0.445
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A19. *Log population*

VARIABLES	(1) LPopulation	(2) LPopulation	(3) LPopulation	(4) LPopulation	(5) LPopulation	(6) LPopulation
RD_Estimate	0.0666	-0.0497	-0.700	-1.348*	0.0192	0.107
	(0.469)	(0.576)	(0.615)	(0.799)	(0.507)	(0.459)
Observations	132	83	45	24	110	144
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.879	0.467	0.102	0.145	0.990	0.821
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

11.3 Result section: Regression tables section 7.3

Table A20. *Membership of a big4 committee*

VARIABLES	(1) MemberBig4	(2) MemberBig4	(3) MemberBig4	(4) MemberBig4	(5) MemberBig4	(6) MemberBig4
RD_Estimate	0.193*	0.173	0.210	0.148	0.170	0.195*
	(0.108)	(0.148)	(0.202)	(0.251)	(0.119)	(0.105)
Observations	197	116	59	29	162	209
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Mean Memberbig4	0.250	0.250	0.250	0.250	0.250	0.250
Robust p-value	0.308	0.222	0.619	0.202	0.237	0.459
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A21. *Difference directed federal spending*

VARIABLES	(1) SpendingDif	(2) SpendingDif	(3) SpendingDif	(4) SpendingDif	(5) SpendingDif	(6) SpendingDif
RD_Estimate	-46.66	-16.20	-63.04	-85.53	-44.37	-16.78
	(111.4)	(138.1)	(106.5)	(138.9)	(122.9)	(137.6)
Observations	133	85	46	22	111	85
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Mean SpendingDif	56.43	56.43	56.43	56.43	56.43	56.43
Robust p-value	0.856	0.773	0.701	0.166	0.941	0.818
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A22. *Difference personal income per capita*

VARIABLES	(1) IncomeCapitaDif	(2) IncomeCapitaDif	(3) IncomeCapitaDif	(4) IncomeCapitaDif	(5) IncomeCapitaDif	(6) IncomeCapitaDif
RD_Estimate	571.1	684.7	1,102	2,541	504.0	508.5
	(1,087)	(1,551)	(1,963)	(2,565)	(1,007)	(1,014)
Observations	246	145	72	39	267	266
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Mean IncomeCapitaDif	747.7	747.7	747.7	747.7	747.7	747.7
Robust p-value	0.596	0.619	0.309	0.126	0.581	0.577
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A23. *Difference unemployment rate*

VARIABLES	(1) UnemploymentDif	(2) UnemploymentDif	(3) UnemploymentDif	(4) UnemploymentDif	(5) UnemploymentDif	(6) UnemploymentDif
RD_Estimate	-0.0497	-0.124	-0.367	-0.425	-0.0749	-0.0638
	(1.134)	(1.871)	(2.940)	(3.712)	(1.292)	(1.276)
Observations	122	70	37	17	106	107
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Mean UnemploymentDif	-0.290	-0.290	-0.290	-0.290	-0.290	-0.290
Robust p-value	0.883	0.978	0.929	0.557	0.970	0.871
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

11.4 Result section: Regression tables section 7.4

Table A24. *Dir. fed. spending: Lead difference time period $t + 2$ and $t - 1$*

VARIABLES	(1) SpendingDif2	(2) SpendingDif2	(3) SpendingDif2	(4) SpendingDif2	(5) SpendingDif2	(6) SpendingDif2
RD_Estimate	264.3**	381.7**	246.9	-21.37	381.5**	379.9**
	(129.0)	(176.2)	(195.3)	(254.4)	(163.1)	(177.3)
Observations	111	74	40	19	84	73
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Mean SpendingDif	90.69	90.69	90.69	90.69	90.69	90.69
Robust p-value	0.0208	0.264	0.811	0.421	0.0209	0.0279
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A25. *Pers. inc. capita: Lead difference time period $t + 2$ and $t - 1$*

VARIABLES	(1) IncomeCapitaDif2	(2) IncomeCapitaDif2	(3) IncomeCapitaDif2	(4) IncomeCapitaDif2	(5) IncomeCapitaDif2	(6) IncomeCapitaDif2
RD_Estimate	1,304	1,264	1,267	2,050	1,380	1,388
	(1,432)	(1,965)	(2,473)	(3,196)	(1,597)	(1,563)
Observations	233	142	71	38	199	205
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Mean IncomeCapitaDif	2477	2477	2477	2477	2477	2477
Robust p-value	0.505	0.529	0.643	0.442	0.438	0.450
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

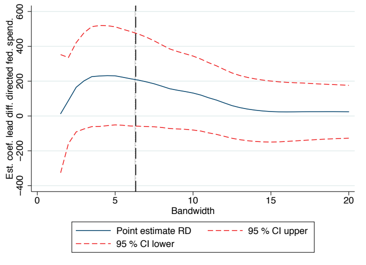
Table A26. *Unemployment: Lead difference time period $t + 2$ and $t - 1$*

VARIABLES	(1) UnemploymentDif2	(2) UnemploymentDif2	(3) UnemploymentDif2	(4) UnemploymentDif2	(5) UnemploymentDif2	(6) UnemploymentDif2
RD_Estimate	0.317	0.310	0.438	1.166	0.158	0.120
	(1.232)	(1.660)	(2.019)	(2.264)	(1.451)	(1.108)
Observations	110	67	36	16	83	127
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Mean UnemploymentDif	0.173	0.173	0.173	0.173	0.173	0.173
Robust p-value	0.947	0.844	0.533	0.542	0.911	0.935
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

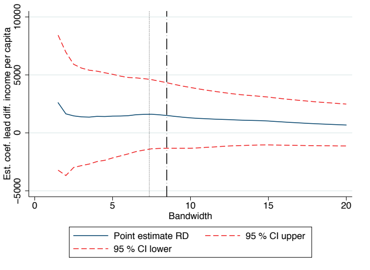
Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

11.5 Result section: Lead difference dependent variable

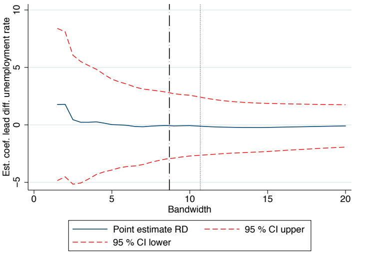
Figure A6. Bandwidth graphs. Lead difference time period $t+1$ and $t-1$



(a) Directed federal spending per capita



(b) Personal income per capita

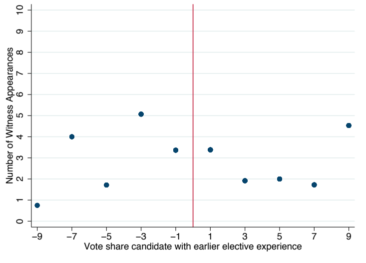


(c) Unemployment rate

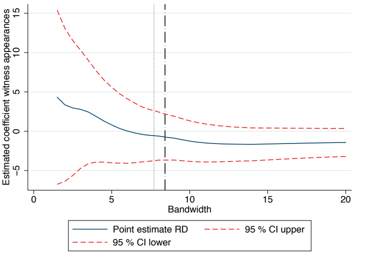
11.6 Result section: Additional outcome

Number of witness appearances was included in the main analysis as an outcome in earlier versions of the paper. In the appendix, I run all additional specifications including this outcome.[42]

Figure A7. RD plot and bandwidth graph. Number of witness appearances



(a) RD-plot



(b) Bandwidth graph

Table A27. *Number of witness appearances*

VARIABLES	(1) Appearances	(2) Appearances	(3) Appearances	(4) Appearances	(5) Appearances	(6) Appearances
RD_Estimate	-1.253	0.799	2.955	4.330	-0.736	-0.567
	(1.312)	(2.453)	(4.376)	(5.649)	(1.494)	(1.617)
Observations	133	83	45	22	119	111
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Robust p-value	0.681	0.383	0.476	0.425	0.824	0.277
BW	10	5	2.500	1.500	CCT	IK
Order local polynomial estimator	1	1	1	1	1	1

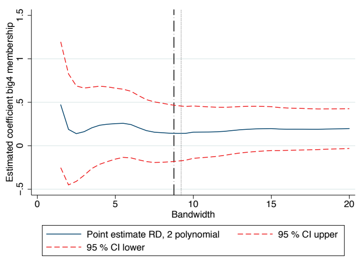
Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[42] I have also run the analysis for number of times a representative stand witness in front of a committee responsible for the budget. These results points in the same direction as the results presented here in the appendix, i.e. no significant results. These additional results can be provided by the author upon request.

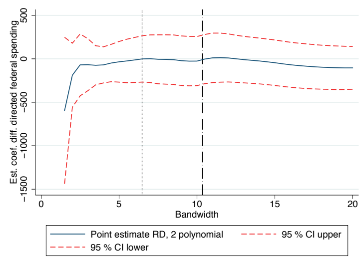
11.7 Result section: Higher order polynomial local regressions

Note in the figure that one of the optimal bandwidths for unemployment is beyond a bandwidth of 20 when considering the third order polynomial.

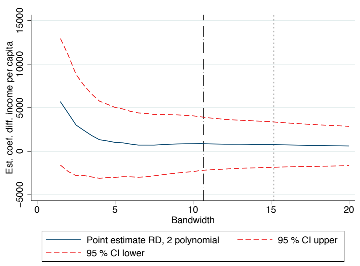
Figure A8. Bandwidth graphs. Second order polynomial, main specification



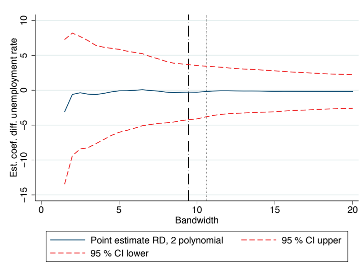
(a) Membership big4 committee



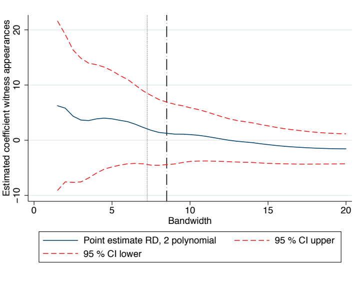
(b) Difference directed federal spending



(c) Difference personal income per capita

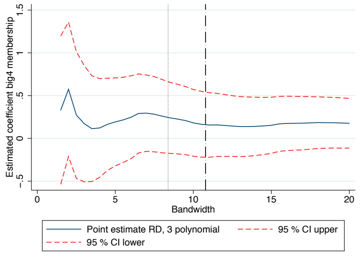


(d) Difference unemployment rate

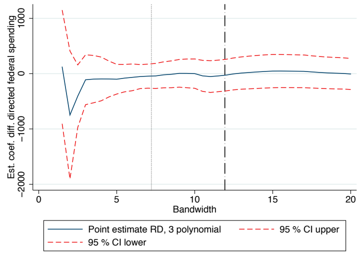


(e) Number of witness appearances

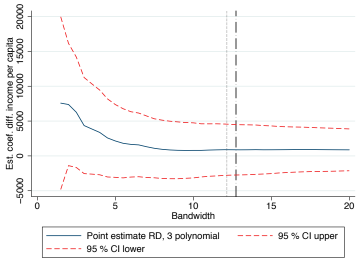
Figure A9. Bandwidth graphs. Third order polynomial, main specification



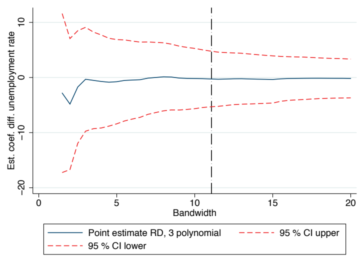
(a) Membership big4 committee



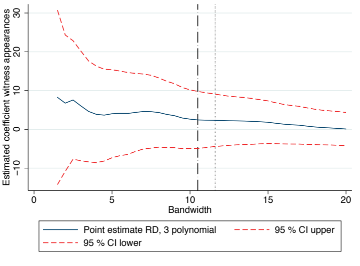
(b) Difference directed federal spending



(c) Difference personal income per capita



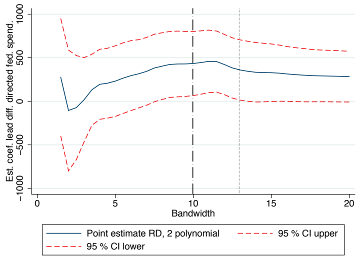
(d) Difference unemployment rate



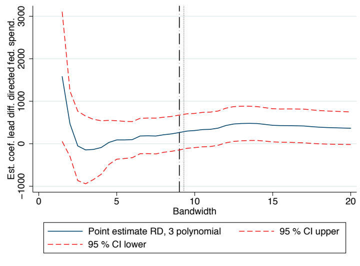
(e) Number of witness appearances

11.8 Result section: Higher order polynomial local regressions, lead specifications

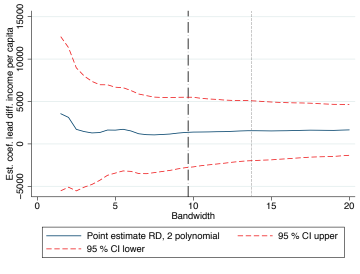
Figure A10. Bandwidth graphs. Results for lead difference time period $t+2$ and $t-1$



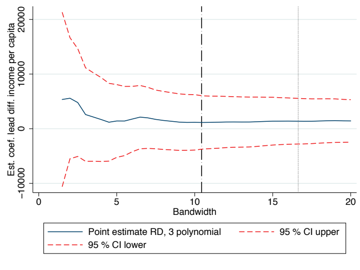
(a) Diff. directed fed. spending. 2 polynomial



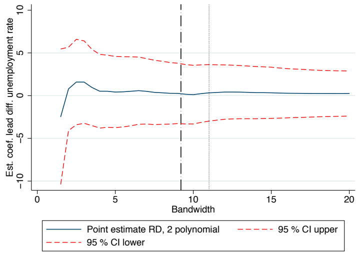
(b) Diff. directed fed. spending. 3 polynomial



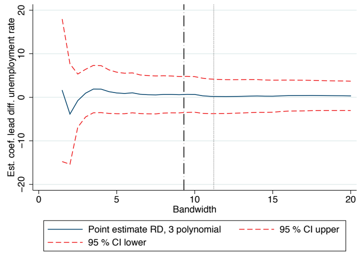
(c) Diff. personal income capita. 2 polynomial



(d) Diff. personal income capita. 3 polynomial



(e) Diff unemployment rate. 2 polynomial

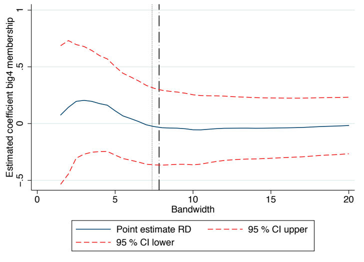


(f) Diff unemployment rate. 3 polynomial

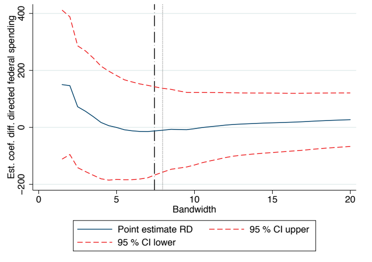
11.9 Robustness: Lagged dependent variable

For unemployment rate, I only have enough observations left to run the RD with 2.5 as the smallest bandwidth.

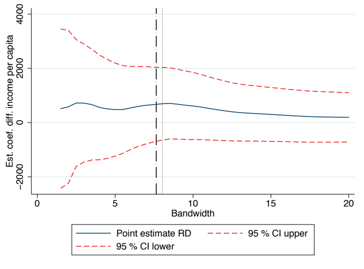
Figure A11. Bandwidth graphs. Lagged dependent variable



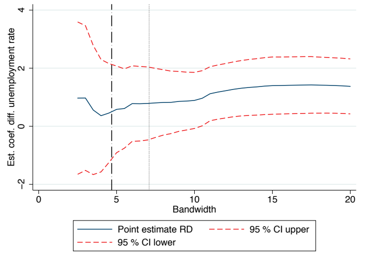
(a) Membership big4 committee



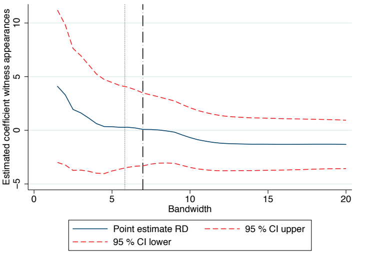
(b) Difference directed federal spending



(c) Difference personal income per capita



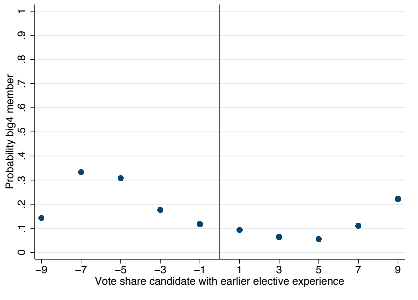
(d) Difference unemployment rate



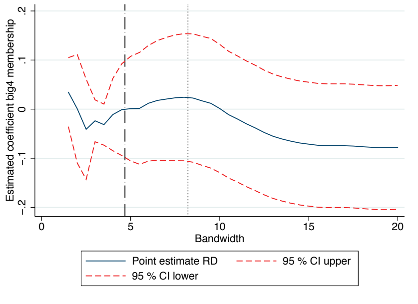
(e) Number of witness appearances

11.10 Robustness analysis: Committee membership, older time series

Figure A12. Earlier elective experience on committee membership



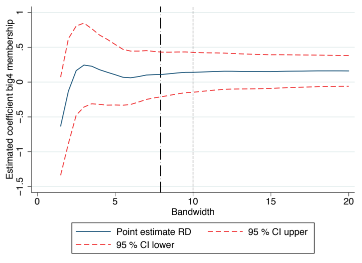
(a) RD-plot



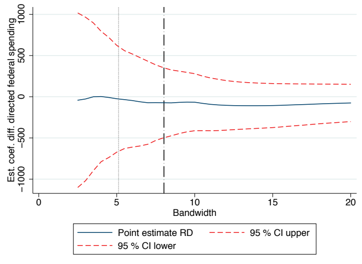
(b) Bandwidth graph

11.11 Robustness analysis: Donut estimation

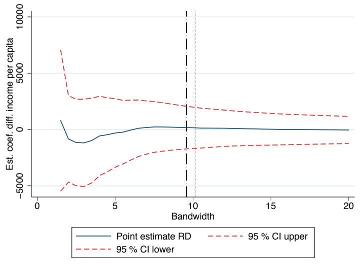
Figure A13. Bandwidth graphs. Donut estimations



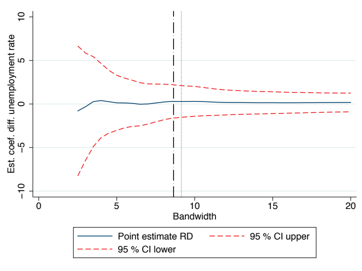
(a) Membership big4 committee



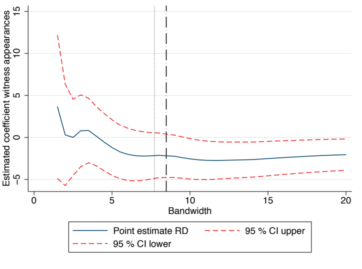
(b) Difference directed federal spending



(c) Difference personal income per capita



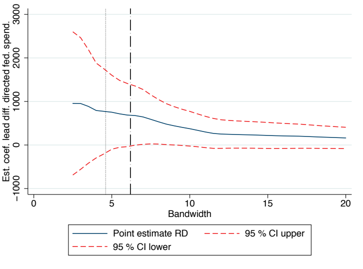
(d) Difference unemployment rate



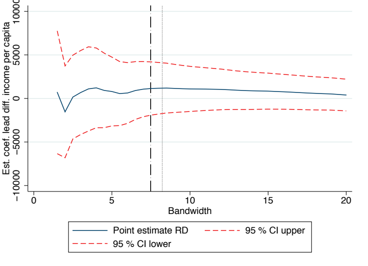
(e) Number of witness appearances

11.12 Robustness analysis: Donut estimation, lead specifications

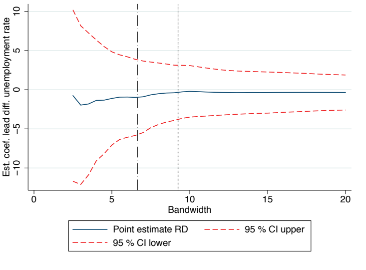
Figure A14. Bandwidth graphs. Donut estimations. Lead difference time period $t + 2$ and $t - 1$



(a) Difference directed federal spending



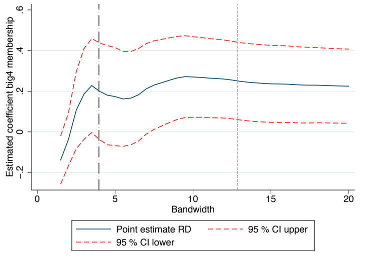
(b) Difference personal income per capita



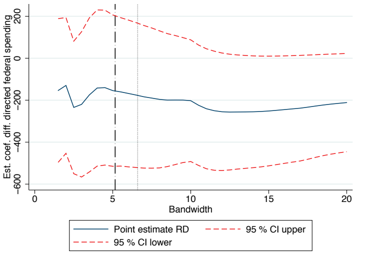
(c) Difference unemployment rate

11.13 Robustness analysis: State legislators

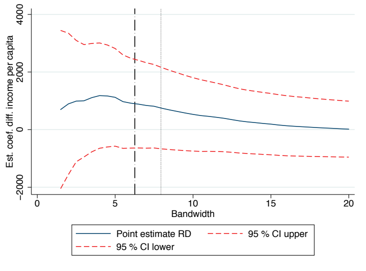
Figure A15. Bandwidth graphs. State legislators



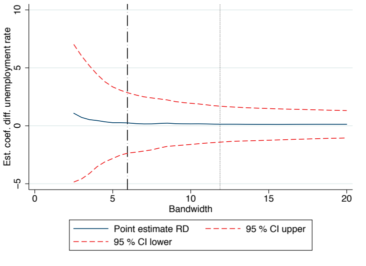
(a) Membership big4 committee



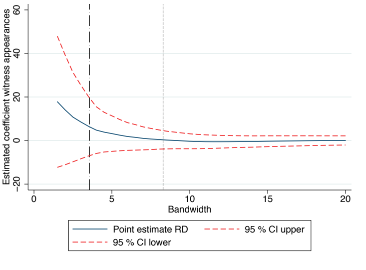
(b) Difference directed federal spending



(c) Difference personal income per capita



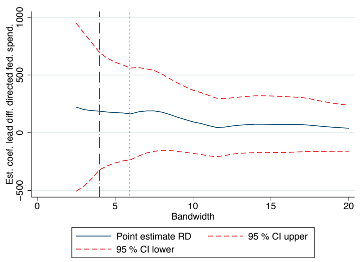
(d) Difference unemployment rate



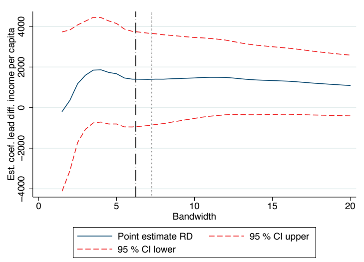
(e) Number of witness appearances

11.14 Robustness analysis: State legislators, lead specification

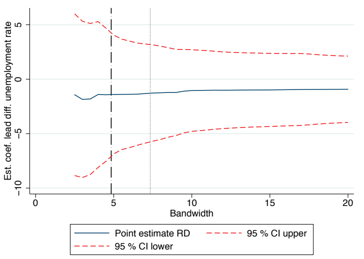
Figure A16. Bandwidth graphs. State legislators, lead difference time period $t+2$ and $t-1$



(a) Difference directed federal spending



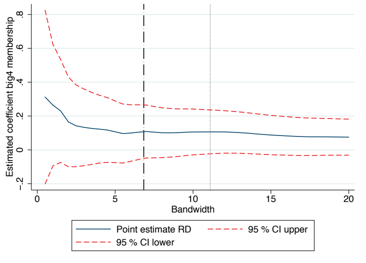
(b) Difference personal income per capita



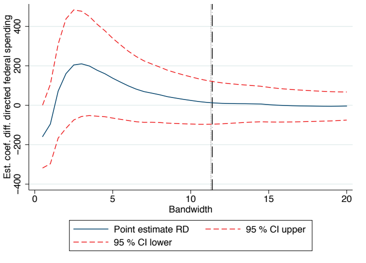
(c) Difference unemployment rate

11.15 Robustness analysis: Analysis with incumbents

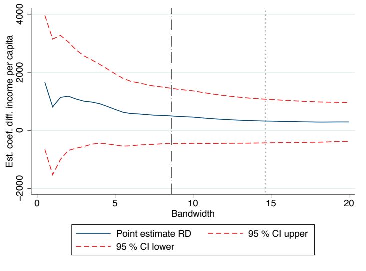
Figure A17. Bandwidth graphs. Incumbents included



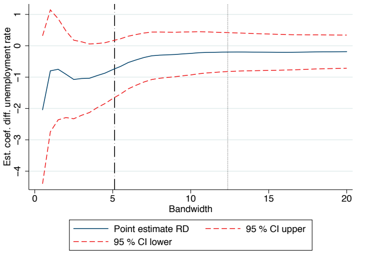
(a) Membership big4 committee



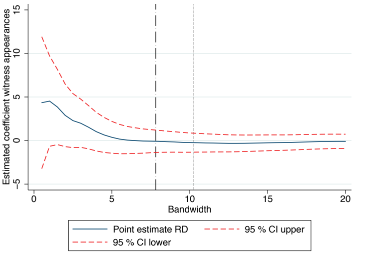
(b) Difference directed federal spending



(c) Difference personal income per capita



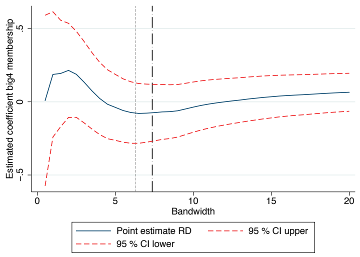
(d) Difference unemployment rate



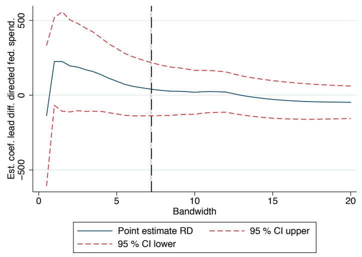
(e) Number of witness appearances

11.16 Robustness analysis: Political party effect, open elections

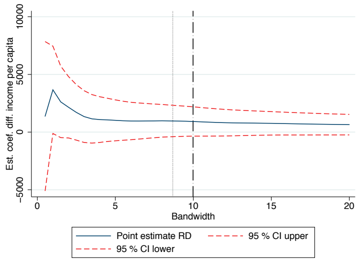
Figure A18. Bandwidth graphs. The effect of electing a Democrat over a Republican



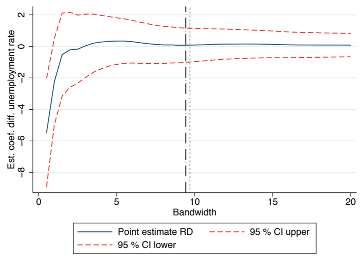
(a) Membership big4 committee



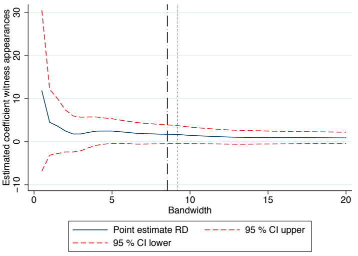
(b) Difference directed federal spending



(c) Difference personal income per capita



(d) Difference unemployment rate

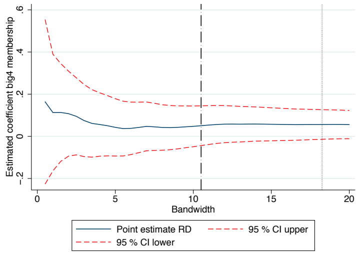


(e) Number of witness appearances

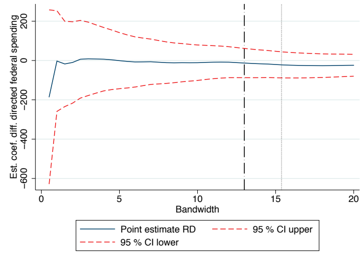
11.17 Robustness analysis: Political party effect, all elections

Note that one of the optimal bandwidths is beyond a bandwidth of 20 for some of the outcomes.

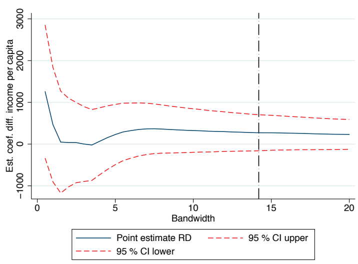
Figure A19. Bandwidth graphs. The effect of electing a Democrat over a Republican



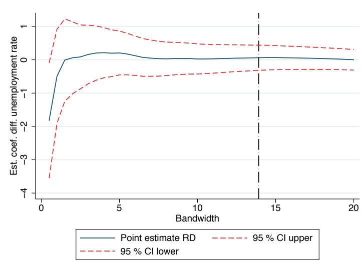
(a) Membership big4 committee



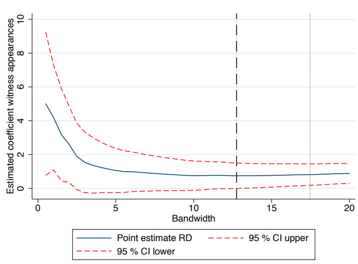
(b) Difference directed federal spending



(c) Difference personal income per capita



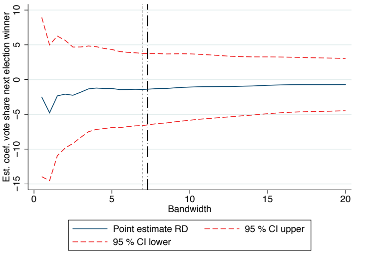
(d) Difference unemployment rate



(e) Number of witness appearances

11.18 Robustness analysis: Vote share next election

Figure A20. The effect on having earlier elective experience on vote share next election for winner in time period t.



(a) Bandwidth graph

III. Voter turnout and the size of government

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Voter turnout and the size of government

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ABSTRACT

This paper uses Swedish and Finnish municipal data to investigate the effect of changes in voter turnout on the tax rate, public spending and vote-shares. A reform in Sweden in 1970, which overall lowered the cost of voting, is applied as an instrument for voter turnout in local elections. The reform increased voter turnout in Sweden. The higher voter turnout resulted in higher municipal taxes and greater per capita local public spending. There are also indications that higher turnout decreased the vote share for right-wing parties. I use an individual survey data set to conclude that it was in particular low income earners that began to vote to a greater extent after the reform.

1. Introduction

Voting is not only an individual political action linked to the issue of democratic legitimacy (Lijphart, 1997) but is also a mean of aggregating preferences for the formation of public policy. The purpose of this paper is to investigate the causal effect of a variation in voter turnout on tax rates, public expenditures and vote shares for political parties. I use municipal data from Sweden and Finland. A reform in 1970 that decreased the cost of voting in local elections in Sweden is applied as an instrument for voter turnout.

Who votes and who abstains can have two effects on policy, one through political party support and one that affects policy directly. The first case is more in line with the Citizen-Candidate model presented in Osborne and Slivinski (1996) and Besley and Coate (1997), in which political candidates implement their preferred policy if they win an election. Another scenario is that political parties position themselves in accordance with the voting median voter, meaning that policy is affected directly.[1]

Questions relevant for this study are why non-decisive people vote and what are the attributes that determine who votes? The standard framework for the individual choice of voting is the rational voter hypothesis presented in Downs (1957) and Tullock (1967).[2] This hypothesis states that the cost of voting, the benefit of having a particular policy implemented and the probability of being the decisive voter are the key parameters that voters consider. The classic model predicts a voter turnout rate of zero, because the probability of being decisive is extremely small and there are always some costs associated with voting. A number

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[1] Meltzer and Richard (1981) argue that the person with the median income is decisive in a democracy. The median *voting individual* may however be different from the median *entitled citizen*.

[2] See also Mueller (2003, chapter 14) and Riker and Ordeshook (1968).

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of papers have suggested modifications to the rational voter hypothesis to present a theory that can explain voting even if voters individually are non-decisive. See Mueller (2003); Geys (2006b), and Harder and Krosnick (2008) for reviews of this literature.

A particular explanation for non-decisive voting is the *expressive voting hypothesis*, according to which voters, in addition to receiving utility from having a particular policy implemented, also receive expressive utility from the act of voting.[3] The idea is present in Fiorina (1976). Brennan and Buchanan (1984) view voting as expression of emotion through personal support. They make an analogy to football, saying that people cannot possibly believe that their cheering in front of the television or at the stadium will affect the outcome of the game—but they do it anyway. In a model by Schuessler (2000), inspired by the anthropological literature, voting is about "being" and not "doing", meaning that voting is not an action taken to obtain something from the action in itself, but is about being a type of person supporting a political alternative. Voters vote to "become" what they want to be and this is independent of the probability of being the decisive voter.[4] Hillman (2010) proposes that the source of expressive utility is affirmation of identity and describes a scenario in which non-decisive voters vote expressively for a candidate whose policy platform is different from the voter's materially utility maximizing policy. An "expressive policy trap" arises when a majority has voted expressively for a policy that each voter in the majority would oppose if decisive. For a survey of expressive voting, see Brennan and Brooks (2013).

Given that voters are expressive and not decisive, which political alternative do they prefer voting for? In a standard model of public finance (see for example Persson and Tabellini, 2002, p. 48), rich individuals demand less redistribution and lower taxes. If they vote according to their true preferences or self-interest, they would choose to vote for a low-tax low-spending party. However, they could vote expressively to display generosity or a social conscience. Tullock (1971) proposed that they might not actually want to be in the majority but obtain utility only from voting to be generous. Low-income people might be expected to vote expressively for the party that would give them material benefit on the grounds that the income distribution is unfair.

What determines voter turnout? It is an empirical fact that voter turnout is never 100% in democracies, even when voting is associated with expressive utility.[5] The cost of voting for some people exceeds the total utility (material utility and expressive utility). An established finding is that people with a higher education exhibit higher voter turnout than those with low education.[6] The idea is that education fosters political participation. Frey (1971) argues that high-income earners vote to a greater extent because their ability to evaluate political alternatives is higher. Filer et al. (1993) hypothesize that people with relative higher incomes vote in a higher extent because they have more to lose. They also argue that we should expect a drop in voter turnout if a country experiences a rise in overall income. High-income earners might receive more expressive utility from voting than low-income earners because poorer people are more preoccupied with material matters in their everyday lives (Hillman, 2010). Poor people might therefore increase their voter turnout if the cost of voting falls.

A reform in Sweden lowered the cost of voting by introducing a common election day for local and national elections. My prior is that high-income people were voting before the reform in a higher degree than low-income people, for whom the pre-reform cost of voting exceeds the total utility (material utility and expressive utility) from voting. I predict that, after the reform lowered costs of voting, low-income earners began to vote. This had two potential effects: (1) An increase in voter turnout increases the vote share for the left-wing parties if new voters vote expressively in line with their preferences, which would result in higher taxes and more redistribution. (2) Political party support is not affected because the political parties reposition themselves on the policy spectrum. In either way, voter turnout has an effect on policy.[7]

Earlier papers have applied different strategies to identify the causal mechanism underlying voting. Horiuchi and Saito (2009) use Japanese municipal data and election day rainfall as an instrument for turnout to address the problem of endogeneity and find that a higher voter turnout rate in a municipality results in greater intergovernmental transfers. Fujiwara (2015) examines a voting reform in Brazil through the introduction of electronic voting. Voting is mandatory in Brazil so the reform did not increase the turnout rate per se; instead it augmented the share of valid votes from people that were illiterate. The result was that left-wing parties increased their vote share and that policy changed; for example, public expenditures on healthcare increased. Fowler (2013) focuses on an actual increase in voter turnout: when voting became mandatory in Australia, working-class citizens increased their share in the electorate, resulting in more votes for the Labor Party. Furthermore, the implementation of mandatory voting laws also translated in a change in public policy whereby pension spending increased in Australia in comparison to other OECD countries. Martins and Veiga (2014) show that increases in voter turnout in municipal elections in Portugal disadvantages incumbent governments, but that this effect is not uniform in the sense that right-wing majorities lose from increases in voter turnout but left-wing majorities do not. Fumagalli and Narciso (2012) use the same data set as Persson and Tabellini (2003) and Persson and Tabellini (2004) (which is on the economic effects of constitutions) in their cross-country study but argue that voter turnout is the transitional variable between the constitution and the economic outcomes. Mueller and Stratmann

[3] De Matos and Barros (2004) suggest that voting is a social game, which can explain voting even if individuals are non-decisive.

[4] Ashenfelter and Kelley (1975) argue that voting should be viewed as "consumption" and not as an "investment" in policy. Hortala-Vallve and Esteve-Volart (2011) analyze the cost and benefits and conclude that voter turnout should be zero among voters that emphasize smaller number of issues. Brennan and Hamlin (1998) point out that the political position of the candidates becomes very important in the expressive voting framework.

[5] Hillman et al. (2015) note an exception of complete or near complete voter turnout in minority local-government jurisdictions in Israel, as a consequence of voting based on the extended family and group identity.

[6] See for example the discussion in Glaeser et al. (2007) and Sigelman et al. (1985) on the link between education, income and voter turnout. Also see Crain and Deaton (1977).

[7] Geys (2006a) concludes in a meta-analysis that grouping elections together seems to be one factor that increases voter turnout. See also SOU 2001:65. Hillman et al. (2015) point out that expressive behavior among high income earners might be expected to be less pronounced in local elections. In my case however, the national parties run in the local elections and the municipalities are important economic entities.

(2003) find support for what the authors call a "*class bias*" whereby a lower turnout rate leads to more unequal income distribution. Also, Lott and Kenny (1999) and Husted and Kenny (1997) studied the extension of the voting franchise. Aidt et al. (2006) use historical European data and focus on the democratization of the continent and the extension of the franchise and its effect on spending. They also study the voting rule. Aidt and Eterovic (2011) also analyzes the growth of government by using Latin America data. They consider political competition and political participation in particular, which they find have differing effect on the growth of government.

As mentioned, the effect of voter turnout on policy may go through a change in the vote share of different political parties. Pettersson-Lidbom (2008) shows that party representation on the municipal level affects both policy and economic outcomes by applying a regression discontinuity design to Swedish municipal data.[8] Lee et al. (2004) also find evidence in favor of the Citizen-Candidate model. The focus of Folke (2014) is on the representation effect of smaller political parties in a proportional voting system.[9]

2. Empirical design

The Swedish reform in 1970 comprised different parts. The most important feature for my identification strategy was the introduction of a common election day for parliamentary, county and municipal elections. Before the reform, Sweden held elections every second year (as in Finland), with county and municipal elections held together in one year and a parliamentary election held separately two years later. The mandate period was four years for all three levels of government, but after the reform the mandate period was changed to three years (SOU 2001:65, p.31). Additionally, the bicameral parliamentary system was abolished and Sweden introduced a unicameral parliamentary system. Before 1970, direct elections were held for the second chamber and indirect elections to the first chamber (SOU 2001:65, p. 21, 28–30).

There is a likely two-way causality problem between voter turnout and policy outcomes. The reform instrument is therefore used for exogenous variation in the cost for voting. I estimate the effect of voter turnout in an IV-regression setup where the reform is the instrument for voter turnout in local elections.

Parallel to the change regarding the election system, a municipal merger reform took place. In 1966, Sweden had approximately 900 municipalities and in 1974, after the merger reform was completed, fewer than 300 municipalities remained. The foremost reason for reducing the number of municipalities was that many municipalities were small in terms of population. Demands on municipal ability to provide a variety of services and a need for each municipality to be functionally independent and able to manage itself within the municipal borders were also important arguments (Erlingsson et al., 2010, p.15). I calculate weighted means for all of my variables using population as a weight so that the number of municipalities is constant each year throughout my panel.[10] Sporadic municipal mergers in Finland are handled in the same manner.

The different parts of the reform and their expected effects on voter turnout are summarized in the table below.

Reform	Expected sign on voter turnout
Common election day	++
3 year mandate period	+/−
Unicameral parliamentary system	+
Municipal merger	−

2.1. The econometric model

The first stage in the IV-analysis for the reform instrument consists of a difference-in-difference regression with a binary treatment variable taking the value 1 for the Swedish municipalities and a treatment period after the reform. Both Sweden and Finland apply the same election schedule whereby elections are held in the fall of each election year and the newly-elected councils meet in the beginning of the following year. Therefore, data regarding voter turnout will be merged with municipal finance statistics for the following year.[11] The regression equations are:

$$Y_{i,t} = \beta_o + \beta_1 Turout_{i,t} + \beta_2 W_{i,t} + \tau_t + f_i + u_{i,t} \tag{1}$$

[8] Tyrefors Hinnerich (2008) applies a similar RD strategy to Swedish municipal data from 1959 to 1966.

[9] Fiva et al. (2015) use Norwegian data and conclude that property taxation and spending on child care increases, but expenditures on elderly care decreases when there is a an increase in the size of a left-wing party. Another application with German municipal data is found in Freir and Odendahl (2012) where the authors also find evidence that political parties actually matter for policy outcome. Angelopoulos et al. (2012) have studied the relationship between political parties in the cabinet and tax policy.

[10] Sometimes a municipality splits and its parts are merged together with more than one municipality. I have included the total municipality to each of the municipalities that were extended.

[11] This will be important in my case because municipal mergers took place 1963–1974 in Sweden. People vote for the municipal councils that were legally in place in the upcoming year.

$$Turnout_{i,t} = \pi_0 + \pi_1 Z_{i,t} + \pi_2 W_{i,t} + \tau_t + f_i + e_{i,t} \tag{2}$$

Y denotes the dependent variable of interest in the second stage. In total, I have three dependent variables: the municipal tax rate[12] total public expenditures per capita[13] and vote share for the right-wing block. β_1 is the parameter of interest which estimates the effect of the instrumented variable voter turnout on the dependent variables. The fixed effects are denoted as τ_t and f_i respectively. u and e are the error terms. W is a vector of control variables. In the first stage Eq. (2) Z is the binary reform instrument.

The covariates used in the analysis are population,[14] state grants[15] and tax base.[16] I also include lagged variables for tax rate and public expenditures per capita because these two variables tend to be persistent over time. To control for general economic performance in each country I include GDP per capita on the national level. I use the share of entitled voters which serves as a proxy variable for the age structure.[17] I also include a variable for how close the political election was in a given municipality in a given year. Dummy variables for municipal mergers are also included together with interaction variables.

I do not have data regarding the share of entitled voters in Finland for the years 1963 and 1964. For this last variable, I have used the same values as in 1965. I deflate all relevant variables and express them in Swedish kronor. The exception is GDP per Capita on the national level that comes from the Maddison project which is measured in 1990 Int GK$. The municipalities of Stockholm, Malmö and Göteborg have been excluded from the analysis, together with the municipalities of Åland and Gotland because these particular municipalities have had different responsibilities than the rest of the municipalities.

Voter turnout only varies for each election year in contrast to municipal data that varies on a yearly basis. In the main specification I therefore only look at the election years.[18] This means that the treatment period is election 3 to 5 in my panel, which is thus different years for Sweden and Finland because they hold elections in different years.

2.2. Municipalities in Sweden and Finland: background

This paper is based on a similar empirical strategy to that in Dahlberg and Mörk (2011). Sweden and Finland have a long common history, and their political institutions display a high degree of similarity. Finland was a part of Sweden from the early middle ages up until 1809. See the online appendix for more descriptive statistics on Sweden and Finland.

Swedish and Finnish municipalities are important economic entities which have the right to collect taxes and they are free to choose their own tax rate. Direct political elections are conducted to fill municipal council.[19] Both countries conduct elections through a PR voting system.[20] In Sweden and in Finland, political parties may be divided into a right-wing and a left-wing block and the vote share for one entire block will act as dependent variable in my analysis.[21] The important assumption is that the trend in voter turnout for municipal elections in Sweden and Finland is parallel and that I have a change in the voter turnout rate in Sweden in 1970 when the reform was implemented. I present Fig. 1 illustrating the average voter turnout rate in local elections for Swedish and Finnish municipalities. Note that voter turnout is displayed as constant during a mandate period in the figure. As you can see, the average voter turnout rate is higher in Sweden for the entire time period, but there is an increase in 1970. There seems however to be an increase in voter turnout in Finland for the earlier years. Note that the reform had a long-

[12] Denoted "*utdebitering per skattekrona*" in the Swedish printed statistics and "*skatteörets värde*" in the Finnish statistics.

[13] Denoted "*summa utgifter*" in the Finnish printed statistics 1967–1972 and "*egentliga utgifter*" between 1972 and 1977 and "*utgifter total*" in the Swedish printed statistics. This variable is reported with a 2 year lag for Sweden. The sample is thus somewhat reduced because some municipalities has over the mentioned two years merged with other municipalities. The calculated weighed means are affected by the fact that I have some missing values for some of the merged municipalities.

[14] Before 1972, population in Finland was measured yearly on the first of January each year ("*mantalsskriven befolkning*"), but in the new publication after 1972 the population is measured yearly on December 31st.

[15] Denoted as "*skatteutjämningsbidrag*" and "*statsbidrag*" in the Swedish statistics and "*statsbidrag och ersättning*" and "*summa inkomster av. staten*" in the Finnish statistics. For the years 1965 and 1966 I do not have access to this covariate for the Swedish subsample. I have chosen to linearly interpolate the values for this variable for the Swedish subsample. For Sweden for the years 1963–1964 there seems to be a slight change in the definition of state grants. For the earlier years, grants is defined as state grants and after 1966 it is defined as "*skatteutjämningsbidrag*".

[16] "*Antal skattekronor*" in the Swedish statistics and "*Antal skatteören*" in the Finnish statistics. I do not have data for tax base for the Swedish subsample for 1965. This variable is somewhat differently reported for Sweden for 1963 and 1964.

[17] For some municipalities, there are some odd discrepancies between the population variable and the share of entitled voters. I have chosen to replace this variable with missing values for all municipalities with a share of entitled voters below 20 and above or equal to 100. Foreign citizens receive the right to vote in municipal elections in 1976 in Sweden.

[18] The Finnish elections were held in 1960, 1964, 1968, 1972 and 1976. The Swedish elections were held in 1962, 1966, 1970, 1973 and 1976. I use 1963 as the year 1 for both Finland and Sweden when I run the regressions without covariates. When I include covariates, I use 1964 as year 1. Otherwise I cannot include lagged dependent variables. There are no statistics regarding vote share for the different political parties in the Finnish election in 1960. Therefore, I only use 4 election years when I analyze vote share for the right-wing block or when I include the winning marginal covariate. I use the share of entitled voters in the 1964 years election for Finland, although the municipal councils were first in place in 1965.

[19] For the years in my panel, the election day in Sweden is on a Sunday in September. In Finland, elections are held in October and people can vote on a Sunday and the following Monday.

[20] See Pettersson-Lidbom (2012) and Dahlberg and Mörk (2011) for a description of Swedish and Finnish local governments.

[21] The right-wing block consists of the Conservative party, the Christian Democrats, the Center party and the Liberal Peoples Party in the Swedish subsample. The left wing block incorporates the vote shares for the Social Democrats and the Left party. For Finland, the statistics is presented only on the block-level for some years and for others it is presented for parties separately. For the Finnish subsample, the Conservative party, the Christian Democrats, the Swedish Peoples Party, the Liberal Party, the Finnish Rural Party and the Center Party constitute the right-wing block together with minor right-wing parties. The Finnish left-wing block is the Social Democrats, the Social Democratic Union of Workers and Small Farmers and the Democratic League of the People of Finland together with other minor left wing parties. Note that this means that there are some small discrepancies between the definitions of the right-wing block between the years.

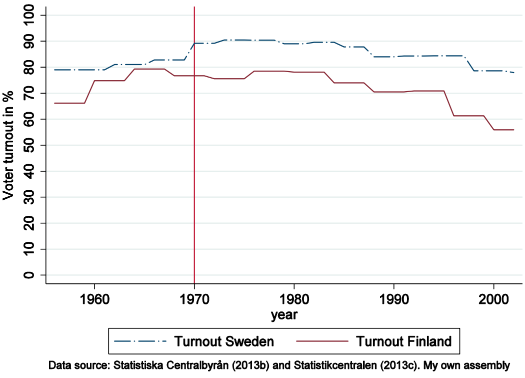

Fig. 1. Voter turnout local elections. Aggregated data.

lasting effect on voter turnout in Sweden. It is interesting to note that voter turnout in Finland and Sweden seems to be rather parallel even after 1970. I will come back to parallel trend assumption in the result section after I have analyzed the first stage IV.

2.3. The exclusion restriction

Is there reason to believe that the reform affected determinants of policy outcomes directly and hence violate the exclusion restriction?

In the years prior to the reform, a public inquiry had taken place. When this inquiry was presented, none of the political parties in the Swedish parliament were in favor of the idea of a common election day. In fact, it was the issue of the single chamber parliamentary system that divided the political parties. The Social Democrats wanted to keep the bi-cameral system and the right-wing parties supported a unicameral parliament. The upper chamber had a local connection since its members were elected indirectly through the county councils and the Social Democrats argued that the local connection in national politics would be lost if the upper house was abolished. As a compromise, a common election day was introduced and the two-house parliament was replaced by a single chamber parliament. Because all elections were grouped together, there was still some local connection in the national election in accordance with the compromise (SOU 2001:65, p.29–31).

It is difficult to imagine why the reform would have affected policies such as tax rates and local public spending directly since the reform was the result of political logrolling on the national level. There is no particular reason that the reform should have affected the policy outcomes in the Swedish municipalities directly.

2.4. Data

The data used in the main analysis were collected from Statistics Sweden and Statistics Finland, from the publication series *Årsbok för Sveriges kommuner*, *Kommunal Finansstatistik*, *Årsbok för Finland*, *Statistisk Rapport*, *Allmänna valen* and *Kommunalvalen*.[22] GDP per capita data comes from the Maddison Project. The data that I use to investigate voter turnout in different groups in Sweden before and after the reform comes from Svensk valundersökning (SND).

Large parts of data have been converted into a digital format using Optical Character Recognition (OCR). The OCR-process is not without flaws and misinterpretations occur. Some variables for some municipalities become for example missing observations. These errors are sometimes easily spotted and may be corrected directly. I have also performed a sample check in order examine the prevalence of OCR-error.[23] The reader should however be aware that there are some remaining random scanning errors and random missing values and in the final data set. See the online appendix for an analysis in which I drop random part of the data and show that the point estimates and the statistical significance are relatively unaffected. Table 1 displays summary descriptive statistics for my variables for Sweden and Finland respectively.

3. Empirical results

The main results are presented here and additional regression tables are presented in the online appendix.

[22] The Government Institute of Economic Research (VATT) and Statistics Sweden has provided data regarding municipal mergers. Data of CPI, GDP and aggregated measures for taxation as share of GDP and other data that was used to create the graphs in the Institutional Setting section in the Online Appendix come from OECD.Stat. Exchange rate data comes from Riksbanken.

[23] This sample check was performed before the data was recalculated with weighed means to create a balanced panel and before some revisions of the paper.

Table 1
Descriptive statistics. Means and standard deviations for the years 1963–1977. For some variables data for all the years is not available.

	Finland		Sweden	
	Mean	sd	Mean	sd
Municipal tax rate in percent	14.09	1.98	12.67	1.93
Voter turnout in percent	78.80	5.00	85.56	5.90
Population in thousands	10.42	27.99	24.11	22.32
State grants in thousands	21,795.59	46,226.81	22,678.38	50,829.57
Taxbase in thousands	416,244.79	2,005,147.38	1,250,882.95	1,501,943.66
Municipal merge during the year	0.01	0.10	0.07	0.26
Public expenditures Capita	8538.88	3772.88	18,221.68	14,120.34
Vote share right wing-block	60.69	15.30	47.86	13.34
Vote share left wing-block	37.64	14.77	48.34	13.64
Share entitled voters	64.63	8.63	67.52	7.44
GDP/capita, country level	9406.78	1623.09	12,355.46	1410.53
Winning marginal municipal election	30.07	21.61	19.98	15.63
Observations	6720		4110	

3.1. First stage

Let me start by discussing the first stage regression results in Table 2. Voter turnout is defined between 0 and 100. Tax base and state grants are expressed in thousands Swedish kronor in real values (year 2005). Standard errors are always clustered on the municipal level. In total, I run four different specifications (corresponding to different specifications of the second stage): one without covariates, one including the winning margin covariates (this specification will not be used in the second stage and the estimation only includes 4 election years), one where I include tax rate as a lagged variable and one where public expenditures per capita is included as a lagged variable.

It is clear from Table 2 that the reform in Sweden had an effect on voter turnout. The coefficient for the variable of interest is statistically significant and the F-value is clearly above the rule-of-thumb value of 10. Considering the point estimate, the introduction of the reform increased voter turnout by approximately 6–7 percentage points. A single voter is unlikely to be decisive

Table 2
First stage IV. Election years.

	(1)	(2)	(3)	(4)
Variables	Turnout	Turnout	Turnout	Turnout
Constitutional change Sweden	6.726***	7.535***	6.291***	6.456***
	(0.271)	(0.434)	(0.459)	(0.537)
Population in thousands		−0.086***	−0.095***	−0.097***
		(0.020)	(0.022)	(0.022)
State grants in thousands		0.000	0.000*	0.000
		(0.000)	(0.000)	(0.000)
Taxbase in thousands		0.000	−0.000	−0.000
		(0.000)	(0.000)	(0.000)
Municipal merge during the year		−7.167**	−4.221*	−4.948*
		(2.947)	(2.429)	(2.618)
Population × Merge dummy		0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)
Tax base × Merge dummy		−0.000	0.000**	0.000**
		(0.000)	(0.000)	(0.000)
GDP/capita, country level		−0.001***	−0.002***	−0.002***
		(0.000)	(0.000)	(0.000)
Share entitled voters		−0.063	0.012	0.008
		(0.046)	(0.058)	(0.058)
Winning marginal municipal election		0.046***		
		(0.011)		
Tax rate lagged			0.133	
			(0.094)	
Pub Exp capita lagged				−0.000
				(0.000)
Observations	3573	2829	3522	3517
R-squared	0.582	0.555	0.609	0.609
Number of municipalities	722	722	722	722
Municipal fixed effects?	Yes	Yes	Yes	Yes
Election year fixed effects?	Yes	Yes	Yes	Yes
F-value	614.8	301.3	187.6	144.5

Clustered standard errors in parentheses.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

Table 3
Placebo test first stage.

	(1)
Variables	Turnout
Placebo instrument	−2.736***
	(0.346)
Treatment group	5.346***
	(0.445)
Placebo period	5.001***
	(0.170)
Constant	75.175***
	(0.292)
Observations	1416
R-squared	0.216

Clustered standard errors in parentheses.
*** p < 0.01.
** p < 0.05.
* p < 0.1.

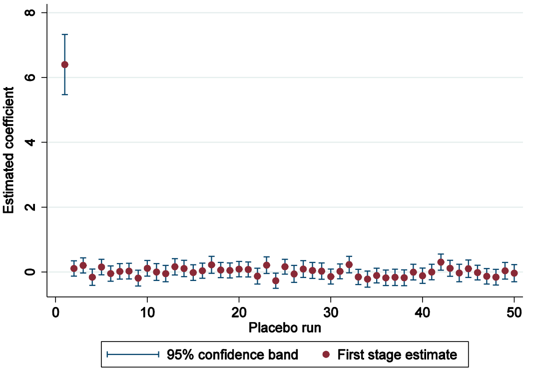

Fig. 2. Placebo test, first stage IV.

in a municipal election, but voters in both Sweden and Finland vote apparently in local elections, which indicates that they receive expressive utility from the act of voting. The results presented in Table 2 indicate however that the voters are still affected by the cost of voting. When the cost decreases, a larger share of the entitled population have a total utility which is higher than the cost of voting so they begin to vote expressively. In a few sections below I investigate who these new voters are.

When just running the regression without covariates the point estimates equals 6.7 and when including additional controls in equivalent specifications the estimated effect varies between 6.3 and 6.5 (the third specification is with fewer election years). The estimated effect hence seems insensitive to the inclusion of covariates.

Because I only have two pre-reform elections I have a very limited possibly to run a pretreatment placebo test. In essence, I can run this as a two time period regression for the years 1963 and 1966, which I do in Table 3. Unfortunately, this includes the years where I do not have access to a full range of covariates. I have however specified such a test without covariates, but its results should be interpreted with caution.

We see in Table 3 that Sweden has a higher voter turnout rate than Finland and that turnout increases between election year 1 and election year 2. However, there seems like voter turnout rate rises faster in Finland which is picked up by the placebo instrument, which is negative. This could indicate that the estimated effect of the instrument in the first stage is somewhat underestimated (the coefficient in Table 3 is however much smaller than the estimated coefficients in Table 2). If the reduced form is also underestimated, it would not make a difference in the second stage IV. There are no reasons to believe that the implementation of the reform is endogenous to voter turnout between Sweden and Finland.

Another critical argument might be that the estimated effect in the first stage is due to some other factors other than the implementation of the reform which is picked up because these factors are more prevalent in the Swedish subsample. I therefore run 50 regressions with a random treatment group (equal in size to the control group).[24] If the estimated effect in the first stage is due to some other factors, this effect should be picked up in some of these 50 runs, otherwise the estimated effect should always be around 0. As we can see in Fig. 2, the coefficients from the placeboruns are always around 0. The first run is my true estimated first stage.

[24] In this case I include the standard covariates, but I do not include any of the additional covariates in column 2–4 in Table 2.

Table 4
Reduced form results.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Spec 1			Spec 1	Spec 2
Variables	Taxrate	PubExpCapita	RWVoteShare	Taxrate	PubExpCapita	RWVoteShare	RWVoteShare
Constitutional change Sweden	0.713***	17,101.88***	−1.954***	0.088**	10,706.40***	−2.714***	−6.389***
	(0.067)	(356.55)	(0.668)	(0.041)	(1098.22)	(0.696)	(0.389)
Population in thousands				−0.005*	−119.19**	0.070	0.047
				(0.003)	(54.26)	(0.059)	(0.035)
State grants in thousands				−0.000	0.04***	0.000**	0.000***
				(0.000)	(0.01)	(0.000)	(0.000)
Taxbase in thousands				0.000	0.00**	−0.000	0.000
				(0.000)	(0.00)	(0.000)	(0.000)
Municipal merge during the year				−0.158	22,180.90*	−6.363***	−8.071
				(0.124)	(11,746.24)	(2.341)	(6.086)
Population × Merge dummy				0.000**	−0.28**	0.000***	0.000
				(0.000)	(0.12)	(0.000)	(0.000)
Tax base × Merge dummy				−0.000**	0.00**	−0.000***	−0.000*
				(0.000)	(0.00)	(0.000)	(0.000)
GDP/capita, country level				0.000***	−4.53***	−0.003***	−0.003***
				(0.000)	(0.82)	(0.001)	(0.000)
Share entitled voters				−0.001	112.50***	0.104**	0.128***
				(0.002)	(40.09)	(0.046)	(0.035)
Tax rate lagged				0.734***			
				(0.019)			
Pub Exp capita lagged					0.33***		
					(0.09)		
Observations	3599	3596	2869	3536	3531	2842	2550
R-squared	0.873	0.715	0.087	0.953	0.784	0.106	0.451
Number of municipalities	722	722	722	722	722	722	711
Municipal fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

3.2. Reduced form and second stage IV results

Let us now have a look at the reduced form results which are presented in Table 4. When it comes to the results for the vote share of the right-wing block, I use two different specifications. In the first specification, all observations are included. In Fig. 3 I show the distribution of the vote share for the right-wing block. I also run a second specification dropping those observations

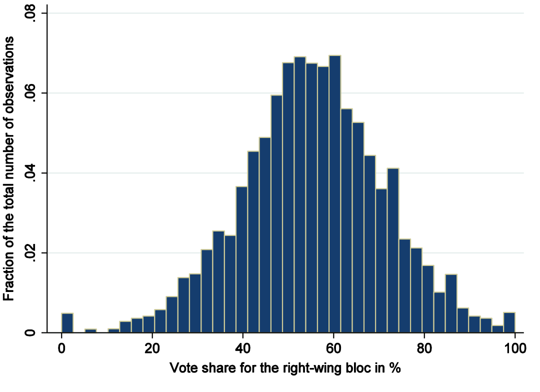

Fig. 3. Distribution, vote share for the right-wing block.

Table 5
Second stage IV.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Spec 1			Spec 1	Spec 2
Variables	Taxrate	PubExpCapita	RWVoteShare	Taxrate	PubExpCapita	RWVoteShare	RWVoteShare
Voter turnout in percent	0.104***	2534.98***	−0.231***	0.016**	1641.65***	−0.373***	−0.855***
	(0.011)	(111.64)	(0.083)	(0.006)	(231.90)	(0.101)	(0.096)
Population in thousands				−0.003	34.02	0.036	−0.026
				(0.003)	(78.96)	(0.062)	(0.047)
State grants in thousands				−0.000	0.03***	0.000**	0.000*
				(0.000)	(0.01)	(0.000)	(0.000)
Taxbase in thousands				0.000	0.00**	−0.000	0.000
				(0.000)	(0.00)	(0.000)	(0.000)
Municipal merge during the year				−0.085	30,739.17***	−8.946***	−15.717
				(0.133)	(10,428.80)	(3.381)	(10.039)
Population × Merge dummy				0.000*	−0.32***	0.000**	0.000
				(0.000)	(0.11)	(0.000)	(0.000)
Tax base × Merge dummy				−0.000**	0.00	−0.000**	−0.000
				(0.000)	(0.00)	(0.000)	(0.000)
GDP/capita, country level				0.000***	−1.29	−0.004***	−0.004***
				(0.000)	(0.79)	(0.001)	(0.001)
Share entitled voters				−0.001	96.53	0.083	0.080
				(0.002)	(101.02)	(0.054)	(0.078)
Tax rate lagged				0.731***			
				(0.019)			
Pub Exp capita lagged					0.34***		
					(0.10)		
Observations	3563	3559	2861	3516	3511	2837	2530
Number of municipalities	721	721	720	721	721	720	694
Municipal fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

where the right-wing block received less than 1% of the votes or over 99% of the votes in an election together with observations were local parties received more than 5% of the votes. Local parties received a large share of the votes in some municipalities for some elections and these parties are not always easy to categorize as either right-wing or left-wing.

According to the reduced form, presented in Table 4, there is a positive relation between the reform and tax rates and public expenditures per capita and a negative relation between the reform and the vote share for the right-wing block. The second stage IV is presented below in Table 5.

Voter turnout has a positive effect on municipal tax rates and on public expenditures per capita and a negative effect on the vote share for the right-wing parties. When all covariates are included in Table 5, the point estimate for tax rates equal 0.016, which should be interpreted as an increase of 0.016 percentage points in municipal tax rate when voter turnout increases one percentage point. The estimated effect is not enormous, although municipals seldom make drastic changes to municipal tax rates. This effect is probably downward biased because municipalities change their tax rates several times during a mandate period. I can compare this result to the result in Table A5 in the online appendix, where the point estimate is equal to 0.036 instead. In this case, all the years 1966–1977 are included in the analysis. With regard to public expenditures per capita, a one percentage point increase in voter turnout increases public expenditures per capita by 1642 Swedish kronor, which could be compared by the average public spending per capita in Sweden, which was 18,200 during this time period. Lastly, the estimated coefficient for vote share for right-wing parties is negative and statistically significant for both specifications. When all municipalities are included in the analysis, the point estimated equals −0.37 in the first specification meaning that the vote share for the right-wing block is reduced by 0.37 percentage points when voter turnout is increased by one percentage point.

3.3. The effect of the reform on voter turnout among different groups of voters

The question remains who the new voters are in relation to the reform in Sweden. I display Fig. 4 showing voter turnout in different subgroups in Sweden. The data is very suitable because the primary researchers[25] have checked whether the surveyed individuals in the data set voted or not. Data regarding income also comes from official registers. For the years up until 1962–

[25] See Särlvik (1984, 1986a, 1986b); Petersson and Särlvik (1984); Petersson (1984) and Holmberg (1986) in the *Electronic Data Sources-section.*

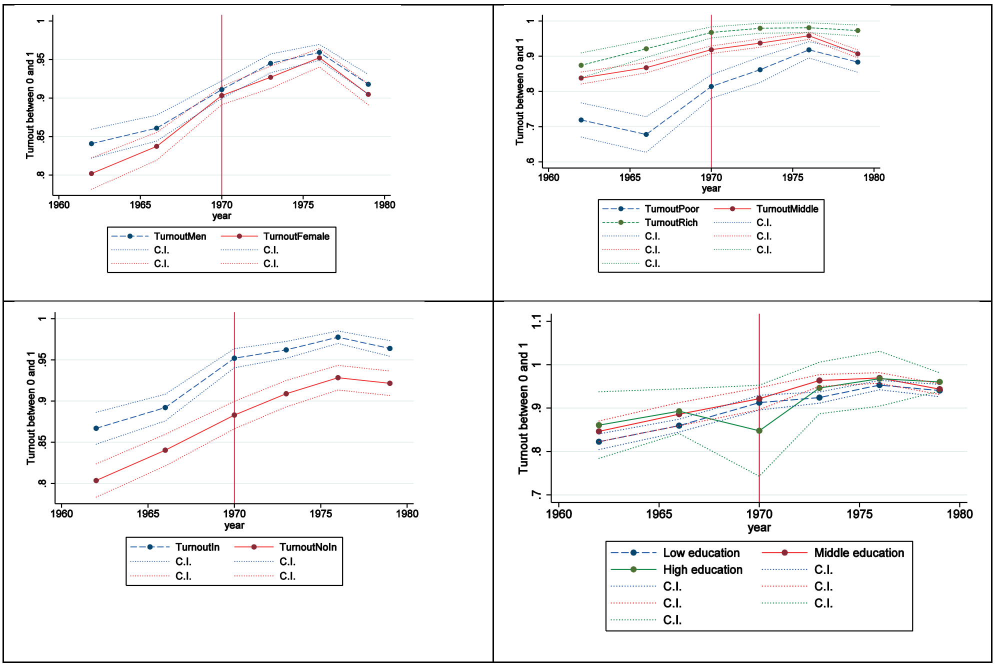

Fig. 4. Voter turnout in different groups.

1970, the survey was a three step panel where individuals were interviewed three times. After 1970 the survey is a rolling two-step panel where half of the participants in the survey in 1973 were re-interviewed in 1976 and so forth.

The first subfigure in Fig. 4 displays voter turnout among males and females respectively. The second subfigure displays voter turnout by income.[26] I have defined "Poor" as the bottom 10 percentile of the individuals in the sample in a given year. "Middle" is defined as the income group including the 25th percentiles and up until and including the 75th percentile. "Rich" is the top 90 percentile. I have used an income measure that is the sum of the family income.[27] The education levels in the fourth subfigure are the following. Low educated: Only basic education corresponding to the Swedish Folkskola (6–8 years). Middle educated: If the individual have some education above "Folkskola" including upper secondary education corresponding to the Swedish "gymnasieutbildning". High educated corresponds to education on the university level (both unfinished and completed).[28] The third subfigure displays voter turnout by those who have stated in the survey that they are interested in politics (defined as point 3 and 4 on a four point scale) and those who have stated that they are not (point 1 and 2).

According to Fig. 4, it seems that especially low income earners in Sweden enter the voting population in 1970 and the following elections in comparison to the municipal election in 1966. Voter turnout rate converges after 1970 between the three income groups. There does not seem to be any dramatic change in the share of those not interested in politics, but this variable should be interpreted with caution because it is a self-described measure. We also see a small increase in the share of women who vote and the participation between men and women seems to converge somewhat in the election of 1970. Interestingly, high educated voters might have participated in a lower degree in the 1970 election, but this result should also be interpreted with caution because of the large confidence intervals.

[26] The income groups are defined in different ways depending on the years of the survey. Sometimes, the survey includes 11 income groups and sometimes only 6. For one year it is a continuous variable. Note that income does not have to be measured in the exact same year as the municipal election took place.

[27] For some years, this is a proper variable in the data set and for some years I have to create it by summing the income of the respondent and his or her spouse (if any). There might be some slight differences between the definitions in the survey. Data regarding income for 1973 is not part of the original data file from SND, but the researchers at University of Gothenburg gratefully shared this data with me.

[28] The middle education group was particular difficult to create because of the variety of education levels stated in the survey. The reader should be aware that the definitions in the survey vary somewhat between the years. There are also surveyed individuals with missing values for this variable and the political interest variable, especially for 1970.

4. Robustness analysis

In the online appendix I run the analysis for all years 1966–1977 and not just the election years.

I also run a specification where I have excluded the municipalities having a voter turnout rate, taxrate and public expenditures per capita over the 95 percentile in dataset and below the 5 percentile. The main results are robust to these procedures.

Tax rates and vote share for the right-wing block are measured in percentage points and I have some observations close to 0 and 100 especially for the vote share for the right-wing block. I therefore also take the log of these two dependent variables. The results for tax rates are still statistically significant but with vote share for the right-wing block the statistical significance disappear. In the robustness section I also estimate the standard errors with the Donald and Lang two step procedure suggested in Donald and Lang (2007).

After examining the effect of my instruments on voter turnout for different groups of municipalities, I may conclude that there are no indications that the monotonicity assumption is violated in the first stage. The sign of the point estimate remains the same when I analyze different subgroups of the municipalities and I have statistically significant and positive point estimates in all cases. Please see the online appendix for details.

5. Conclusions

In this paper, the causal link between voter turnout and the size of government has been investigated with the overall conclusion that there is a link between an increase in voter turnout and higher tax rates and larger public spending per capita on the local level. Higher voter turnout is also associated with a lower vote share for right-wing parties. I found that the reform, which decreased the cost of voting, implemented in 1970 in Sweden, increased voter turnout rate in local elections. I also found that especially low income earners began to vote in municipality elections in Sweden after the reform was implemented.

These empirical results are in line with the Citizen–Candidate model in the sense that a change in political party support changes the policy outcome, where I find that voter turnout also changes the vote share for political parties. My results are also related to the discussion regarding the individual choice of voting. Voters in my data set are not decisive and seem to expressively vote in accordance with their preferences, with new voters with lower incomes supporting the right-wing parties in a lower degree. Although voters are not decisive and vote expressively, they still react to the cost of voting, with utility from voting lower than the cost for some voters.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.ejpoleco.2016.01.003.

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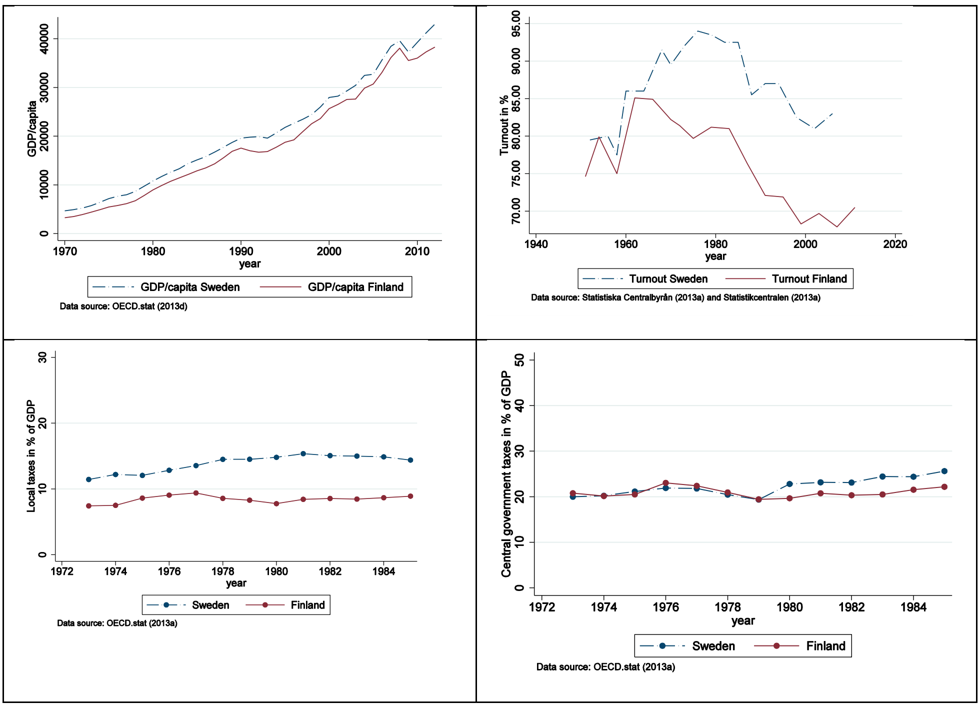
Online Appendix

A1. Institutional setting

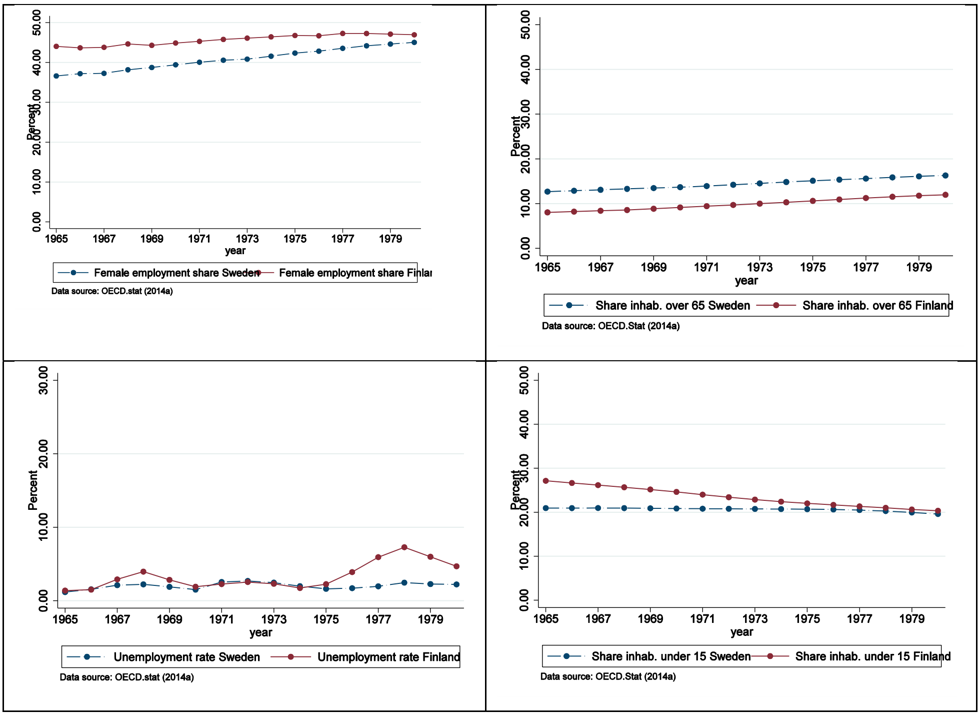
One important assumption in this paper is that Finland and Sweden are similar countries. I show some descriptive figures below regarding GDP, taxation and voter turnout in parliamentary elections. Overall, Sweden and Finland seems to be alike with regard to these variables. Also note that voter turnout in parliamentary elections in Finland and Sweden follows a similar path, but that Sweden has a higher voter turnout rate in absolute numbers.

Figure A1: Descriptive figures Sweden and Finland



Let us also have a look at some figures displaying various measures related to the labor market and also some population statistics. Last we have a figure displaying the share of inhabitants under the age of 15. Second, a corresponding figure for the share of inhabitants over 65 is displayed. It is also interesting to look as some more general labor market measures. Unemployment rate is presented in figure 3 and women's share of civilian employment is showed in figure 1. Again, Sweden and Finland display high degree of similarity. Unemployment seems to rise somewhat in Finland in the end, but is fairly similar to the unemployment rate in Sweden for the years I consider.

Figure A2: Descriptive figures Sweden and Finland



A2: OLS estimates

Table A1: OLS Estimates, Election years.

VARIABLES	(1) Taxrate	(2) PubExpCapita	(3) RWVoteShare	(4) Taxrate	(5) PubExpCapita	(6) RWVoteShare
Voter turnout in percent	0.028***	780.16***	0.010	0.003	256.59***	-0.003
	(0.005)	(62.44)	(0.062)	(0.003)	(38.77)	(0.065)
Population in thousands				-0.005*	-164.29**	0.101
				(0.003)	(80.75)	(0.062)
State grants in thousands				-0.000	0.03***	0.000**
				(0.000)	(0.01)	(0.000)
Taxbase in thousands				0.000	0.00**	-0.000
				(0.000)	(0.00)	(0.000)
Municipal merge during the year				-0.161	14,545.03	-5.737**
				(0.123)	(9,190.52)	(2.437)
Population*Merge dummy				0.000**	-0.15	0.000***
				(0.000)	(0.10)	(0.000)
Tax base*Merge dummy				-0.000***	0.00	-0.000***
				(0.000)	(0.00)	(0.000)
GDP/capita, country level				0.000***	-2.62***	-0.003***
				(0.000)	(0.91)	(0.001)
Share entitled voters				-0.002	-53.48	0.153***
				(0.002)	(49.78)	(0.050)
Tax rate lagged				0.739***		
				(0.019)		
Pub Exp capita lagged					0.56***	
					(0.10)	
Observations	3,564	3,560	2,863	3,517	3,512	2,839
R-squared	0.867	0.509	0.086	0.953	0.737	0.098
Number of Municipalities	722	722	722	722	722	722
Municipal fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Election year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses, *** $p<0.01$ ** $p<0.05$ * $p<0.1$

Table A2: OLS Estimates. All years 1966-1977

VARIABLES	(1) Taxrate	(2) PubExpCapita	(3) Taxrate	(4) PubExpCapita
Voter turnout in percent	0.034***	835.86***	0.011***	360.10***
	(0.004)	(55.88)	(0.002)	(44.50)
Population in thousands			-0.013***	-294.84***
			(0.004)	(85.26)
State grants in thousands			0.000	0.02**
			(0.000)	(0.01)
Taxbase in thousands			0.000	0.00***
			(0.000)	(0.00)
Municipal merge during the year			-0.127***	-3,322.21***
			(0.035)	(551.27)
Population*Merge dummy			0.000	0.07*
			(0.000)	(0.04)
Tax base*Merge dummy			0.000	-0.00
			(0.000)	(0.00)
GDP/capita, country level			-0.000***	-8.51***
			(0.000)	(0.77)
Share entitled voters			-0.003***	-114.33***
			(0.001)	(20.06)
Tax rate lagged			0.599***	
			(0.020)	
Pub Exp capita lagged				0.10
				(0.07)
Observations	8,566	8,549	8,474	8,446
R-squared	0.840	0.319	0.901	0.393
Number of Municipalities	722	722	722	722
Municipal fixed effects?	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses, *** p<0.01 ** p<0.05 * p<0.1

A3: IV-estimates, All years 1966-1977

Table A3: First stage IV. All years 1966-1977

VARIABLES	(1) Turnout	(2) Turnout	(3) Turnout	(4) Turnout
Constitutional change 1970 Sweden	8.200***	8.592***	8.453***	8.341***
	(0.238)	(0.269)	(0.266)	(0.267)
Population in thousands		-0.061***	-0.069***	-0.066***
		(0.017)	(0.018)	(0.018)
State grants in thousands		-0.000***	-0.000***	-0.000***
		(0.000)	(0.000)	(0.000)
Taxbase in thousands		0.000**	0.000**	0.000**
		(0.000)	(0.000)	(0.000)
Municipal merge during the year		-0.247	-0.214	-0.176
		(0.236)	(0.238)	(0.243)
Population*Merge dummy		0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)
Tax base*Merge dummy		-0.000	-0.000	-0.000
		(0.000)	(0.000)	(0.000)
Share entitled voters		-0.037	-0.035	-0.029
		(0.025)	(0.026)	(0.026)
GDP/capita, country level		0.001***	0.001***	0.001***
		(0.000)	(0.000)	(0.000)
Winning marginal municipal election		0.038***		
		(0.011)		
Tax rate lagged			0.125	
			(0.079)	
Pub Exp capita lagged				0.000**
				(0.000)
Observations	8,588	8,474	8,492	8,465
R-squared	0.550	0.564	0.556	0.558
Number of Municipalities	722	722	722	722
Municipal fixed effects?	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes
F-value	1191	1023	1012	978.3

Clustered standard errors in parentheses, *** p<0.01 ** p<0.05 * p<0.1

Table A4: Reduced form IV. All years 1966-1977

VARIABLES	(1) Taxrate	(2) PubExpCapita	(3) Taxrate	(4) PubExpCapita
Constitutional change 1970 Sweden	0.503***	13,021.18***	0.299***	9,995.80***
	(0.055)	(453.13)	(0.045)	(652.73)
Population in thousands			-0.014***	-312.11***
			(0.004)	(80.93)
State grants in thousands			0.000	0.02**
			(0.000)	(0.01)
Taxbase in thousands			0.000	0.00***
			(0.000)	(0.00)
Municipal merge during the year			-0.099***	-2,459.10***
			(0.036)	(539.09)
Population*Merge dummy			0.000	0.05
			(0.000)	(0.03)
Tax base*Merge dummy			0.000	-0.00
			(0.000)	(0.00)
GDP/capita, country level			0.000*	0.19
			(0.000)	(0.64)
Share entitled voters			-0.003***	-112.54***
			(0.001)	(19.07)
Tax rate lagged			0.594***	
			(0.021)	
Pub Exp capita lagged				0.09
				(0.07)
Observations	8,636	8,619	8,507	8,479
R-squared	0.841	0.378	0.901	0.405
Number of Municipalities	722	722	722	722
Municipal fixed effects?	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes

Standard errors clustered on municipality
*** p<0.01 ** p<0.05 * p<0.1

Table A5: Second stage IV. All years 1966-1977

VARIABLES	(1) Taxrate	(2) PubExpCapita	(3) Taxrate	(4) PubExpCapita
Voter turnout in percent	0.060***	1,583.25***	0.036***	1,196.19***
	(0.007)	(69.47)	(0.005)	(79.74)
Population in thousands			-0.011***	-234.99***
			(0.004)	(80.63)
State grants in thousands			0.000	0.03***
			(0.000)	(0.01)
Taxbase in thousands			-0.000	0.00***
			(0.000)	(0.00)
Municipal merge during the year			-0.091**	-2,227.54***
			(0.036)	(569.84)
Population*Merge dummy			0.000	0.05*
			(0.000)	(0.03)
Tax base*Merge dummy			0.000	-0.00
			(0.000)	(0.00)
GDP/capita, country level			0.000	-1.34**
			(0.000)	(0.66)
Share entitled voters			-0.002	-77.55**
			(0.001)	(33.78)
Tax rate lagged			0.594***	
			(0.021)	
Pub Exp capita lagged				0.06
				(0.06)
Observations	8,565	8,548	8,473	8,445
Number of Municipalities	721	721	721	721
Municipal fixed effects?	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses, *** p<0.01 ** p<0.05 * p<0.1

A4: Additional robustness analysis

Table A6: Subsample analysis tax rates. Random 80 percent of sample. Election years

VARIABLES	(1) SubSample 80 Taxrate	(2) SubSample 80 Taxrate	(3) SubSample 80 Taxrate	(4) SubSample 80 Taxrate	(5) SubSample 80 Taxrate	(6) SubSample 80 Taxrate	(7) SubSample 80 Taxrate	(8) SubSample 80 Taxrate
Voter turnout in percent	0.019***	0.015**	0.017**	0.020***	0.015**	0.013*	0.015**	0.011
	(0.007)	(0.008)	(0.007)	(0.008)	(0.007)	(0.008)	(0.007)	(0.007)
Observations	2,810	2,812	2,822	2,809	2,809	2,819	2,822	2,818
Number of Municipalities	577	577	578	577	577	577	578	578
Additional covariates?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses, *** p<0.01 ** p<0.05 * p<0.1

Table A7: Subsample analysis public expenditures per capita. Random 80 percent of sample. Election years

VARIABLES	(1) SubSample 80 PubExpCapita	(2) SubSample 80 PubExpCapita	(3) SubSample 80 PubExpCapita	(4) SubSample 80 PubExpCapita	(5) SubSample 80 PubExpCapita	(6) SubSample 80 PubExpCapita	(7) SubSample 80 PubExpCapita	(8) SubSample 80 PubExpCapita
Voter turnout in percent	1,642.66***	1,409***	1,583***	1,696***	1,479***	1,600***	1,537***	1,686***
	(257.65)	(190)	(249)	(259)	(212)	(248)	(241)	(279)
Observations	2,813	2,816	2,812	2,809	2,810	2,805	2,809	2,814
Number of Municipalities	578	578	577	578	577	577	577	577
Additional covariates?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses, *** p<0.01 ** p<0.05 * p<0.1

Table A8: Outliers dropped. Taxrate. Election years

VARIABLES	(1) OLS Taxrate	(2) IV-First stage Turnout	(3) IV-Second stage Taxrate	(4) Reduced form Taxrate
Voter turnout in percent	0.004		0.018**	
	(0.005)		(0.009)	
Constitutional change Sweden		6.884***		0.126**
		(0.285)		(0.059)
Observations	1,843	1,843	1,843	1,843
R-squared	0.948	0.717		0.948
Number of Municipalities	373	373	373	373
Additional covariates?	Yes	Yes	Yes	Yes
Municipal fixed effects?	Yes	Yes	Yes	Yes
Election year fixed effects?	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses, *** p<0.01 ** p<0.05 * p<0.1

Table A9: Outliers dropped. Public expenditures per capita. Election years

VARIABLES	(1) OLS PubExpCapita	(2) IV-First stage Turnout	(3) IV-Second stage PubExpCapita	(4) Reduced form PubExpCapita
Voter turnout in percent	341***		1,979***	
	(48)		(180)	
Constitutional change Sweden		6.544***		12,952***
		(0.436)		(747)
Observations	1,713	1,713	1,713	1,713
R-squared	0.683	0.572		0.829
Number of Municipalities	345	345	345	345
Additional covariates?	Yes	Yes	Yes	Yes
Municipal fixed effects?	Yes	Yes	Yes	Yes
Election year fixed effects?	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses, *** p<0.01 ** p<0.05 * p<0.1

Table A10: Logged tax rates. Election years

VARIABLES	(1) OLS Taxratelogged	(2) IV-Second stage Taxratelogged	(3) Reduced form Taxratelogged
Voter turnout in percent	0.000*	0.001**	
	(0.000)	(0.001)	
Constitutional change Sweden			0.007**
			(0.004)
Observations	3,517	3,516	3,536
R-squared	0.950		0.950
Number of Municipalities	722	721	722
Additional covariates?	Yes	Yes	Yes
Municipal fixed effects?	Yes	Yes	Yes
Election year fixed effects?	Yes	Yes	Yes

Clustered standard errors in parentheses, *** p<0.01 ** p<0.05 * p<0.1

Table A11: Logged vote share for right-wing parties. Election years

VARIABLES	(1) OLS RWVoteSharelogged	(2) IV-Second stage RWVoteSharelogged	(3) Reduced form RWVoteSharelogged
Voter turnout in percent	0.00	0.00	
	(0.00)	(0.00)	
Constitutional change Sweden			0.00
			(0.03)
Observations	2,828	2,825	2,831
R-squared	0.089		0.089
Number of Municipalities	722	719	722
Additional covariates?	Yes	Yes	Yes
Municipal fixed effects?	Yes	Yes	Yes
Election year fixed effects?	Yes	Yes	Yes

Clustered standard errors in parentheses, *** p<0.01 ** p<0.05 * p<0.1

A5: Test of the monotonicity assumption

In the following three tables, I run separate first-stage analysis for those municipalities with a population above the mean in the data set (Large pop.) and those municipalities with a population below the mean (small pop) in table A12. In table A13 I run a similar analysis with regard to tax base, where the first column display the estimation for those municipalities below the mean tax base and the second for those municipalities above the mean tax base. In the last table A14, I run a separate analysis for those municipalities that have merged and those municipalities that have not. Note that I run this last analysis with all years between 1966 and 1977.

Table A12: Test of the monotonicity assumption. Population.

VARIABLES	(1) Small pop. Turnout	(2) Large pop. Turnout
Constitutional change Sweden	5.459***	11.229***
	(0.647)	(0.911)
Observations	2,461	804
R-squared	0.567	0.790
Number of Municipalities	500	164
Additional covariates?	Yes	Yes
Municipal fixed effects?	Yes	Yes
Election year fixed effects?	Yes	Yes

Clustered standard errors in parentheses, *** p<0.01 ** p<0.05 * p<0.1

Table A13: Test of the monotonicity assumption. Tax base.

VARIABLES	(1) Below mean Turnout	(2) Above mean Turnout
Constitutional change Sweden	5.676***	10.039***
	(0.845)	(0.984)
Observations	2,179	377
R-squared	0.519	0.797
Number of Municipalities	444	77
Additional covariates?	Yes	Yes
Municipal fixed effects?	Yes	Yes
Election year fixed effects?	Yes	Yes

Clustered standard errors in parentheses, *** p<0.01 ** p<0.05 * p<0.1

Table A14: Test of the monotonicity assumption. Merged and non-merged, All years 1966-1977

VARIABLES	(1) No merge Turnout	(2) Merge Turnout
Constitutional change 1970 Sweden	9.059***	8.246***
	(0.363)	(0.366)
Observations	3,321	5,196
R-squared	0.679	0.379
Number of Municipalities	281	441
Additional covariates?	Yes	Yes
Municipal fixed effects?	Yes	Yes
Year fixed effects?	Yes	Yes

Clustered standard errors in parentheses, *** p<0.01 ** p<0.05 * p<0.1

A6: Donald and Lang specification

In the baseline specification I cluster the standard errors at the municipal level. Swedish and Finnish municipal data are probably correlated within groups where each municipality cannot be considered a random observation independent of other observations. This concern was first addressed by Moulton (1986) who concludes that that if there is some within-group correlation the estimated standard errors will be down-ward biased as a result of a correlation in the error terms.

I cannot cluster on the country level when considering only the reform because I only have one treatment group and one control group for several years. [29] To address the standard errors issue, I also estimate standard errors using the approach suggested in Donald and Lang (2007). Briefly, this is a two-step procedure by which data is aggregated for each different group and time combination, thus reducing the number of observations by collapsing the data. This Donald and Lang specification will be used for the first stage IV and the reduced form specifications when the reform is used alone as an instrument, which are the estimations where the binary instrument is directly applied. Formally:

$$Y_{i,t} = \beta_0 + \beta_1 W_{i,t} + \gamma_2 Sweden_i * year_t + \gamma_3 Finland_i * year_t + \tau_t + f_i + u_{i,t} \quad (3)$$
$$\hat{\gamma_{i,t}} = \beta_0 + \beta_1 X_{i,t} + \gamma_2 Sweden_i + \gamma_3 Finland_i + e_{i,t} \quad (4)$$

$\hat{\gamma_{i,t}}$ constitutes the predicted values from the first step (covariate adjusted group means) in the Donald and Lang procedure. I use the number of observation in each group and year as weights and estimate equation (4) by weighted least squares (WLS). W is the vector of covariates. β_1 is the parameter of interest and *X* is the binary instrument taking the value 1 if the observation belongs to Sweden and any year after 1970. $\tau_t + f_i$ are municipal and year fixed effects. In equation (3), *year* and *Sweden* and *Finland* country dummies are interacted with each other resulting in one binary variable for each time and group combination. By collapsing the data, we end up with two observations from each year – one for the Swedish subsample and one for the Finnish. In the second step (4) I use the saved predicted values to run a regression where I include the variable of interest together with dummy variables for Sweden and Finland and dummy variables for each of the years in my panel. Note that I end up with very few observations when I only consider the election years.

[29] Angrist and Pischke (2008) suggest that you should at least have 42 clusters.

Table A15: Donald and Lang. Election years

	(1)	(2)	(3)	(4)	(5)
				Spec 1	Spec 2
	Turnout	Taxrate	PubExp	RW.vote.share	RW.vote.share
VARIABLES	DonaldLang	DonaldLang	DonaldLang	DonaldLang	DonaldLang
Constitutional change	8.706	0.099	12,935**	-6.243	-3.039
	(4.541)	(0.107)	(3,492)	(15.936)	(13.576)
Observations	10	10	10	6	6
R-squared	0.997	0.9998	0.98631	0.9019	0.7687
Additional covariates?	Yes	Yes	Yes	Yes	Yes
Municipal fixed effects?	Yes	Yes	Yes	Yes	Yes
Election year fixed effects?	Yes	Yes	Yes	Yes	Yes
F-value	3.676				

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A16: Donald and Lang. All years 1966-1977

	(1)	(2)	(3)
	Turnout	Taxrate	PubExp
VARIABLES	DonaldLang	DonaldLang	DonaldLang
Constitutional change 1970	11.865***	0.468***	13,495***
	(1.058)	(0.099)	(2,235)
Observations	24	24	24
R-squared	0.999	0.9998	0.99880
Additional covariates?	Yes	Yes	Yes
Municipal fixed effects?	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes
F-value	125.7		

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

A7: More detailed descriptive statistics

To evaluate the accuracy of the data for all years and for all variables, some tables with more detailed descriptive statistics are presented in this section.

	(1) Sweden	(2) Finland
Voter turnout in percent		
1963	80.51	75.09
1964	80.52	75.17
1965	80.55	80.18
1966	80.56	80.18
1967	82.79	80.18
1968	82.81	80.17
1969	82.86	78.91
1970	82.85	78.94
1971	88.43	78.93
1972	88.43	78.93
1973	88.44	78.63
1974	91.00	78.63
1975	91.00	78.63
1976	91.00	78.63
1977	91.00	80.74
Observations	4070	6629

	(1) Sweden	(2) Finland
Municipal tax rate in percent		
1963	10.04	12.15
1964	10.39	12.24
1965	10.84	12.39
1966	11.04	12.58
1967	11.44	12.91
1968	11.70	13.32
1969	12.07	13.85
1970	12.57	14.16
1971	13.33	14.60
1972	13.94	14.86
1973	13.99	15.08
1974	14.11	15.39
1975	14.57	15.64
1976	14.89	15.99
1977	15.13	16.08
Observations	4083	6708

	(1) Sweden	(2) Finland
Taxbase in thousands		
1964	654522.5	348234.1
1966	623746.8	407421.8
1967	763665.0	412112.8
1968	870218.8	330391.7
1969	915585.4	362862.9
1970	1026622.2	397479.6
1971	1428769.5	407726.2
1972	1476530.5	407185.1
1973	1418250.2	440025.0
1974	1722509.2	463716.1
1975	1714463.7	477038.9
1976	1741132.5	519005.9
1977	1886796.5	534718.8
1963		350263.8
1965		385149.6
Observations	3538	6707

	(1) Sweden	(2) Finland
Public expenditures Capita		
1963	7057.0	6938.4
1964	9174.4	6822.5
1965	7478.0	7416.4
1966	7861.0	7963.8
1967	9502.3	8165.3
1968	12280.6	6898.9
1969	15908.5	7514.3
1970	15291.7	8769.4
1971	20492.4	9299.1
1972	23345.6	9418.9
1973	23359.1	7863.5
1974	27121.4	9022.5
1975	29805.9	10139.4
1976	30838.6	10521.1
1977	33603.4	11286.3
Observations	4068	6687

	(1) Sweden	(2) Finland
Population in thousands		
1963	23.03	10.14
1964	23.16	10.21
1965	23.41	10.29
1966	23.66	10.34
1967	23.79	10.39
1968	24.26	10.43
1969	23.88	10.46
1970	24.39	10.47
1971	24.13	10.45
1972	24.26	10.54
1973	24.47	10.53
1974	24.54	10.51
1975	24.73	10.52
1976	24.71	10.54
1977	25.19	10.55
Observations	4094	6704

	(1) Sweden	(2) Finland
State grants in thousands		
1963	49936.3	15397.7
1964	39450.6	15416.8
1965	30905.5	16085.0
1966	18311.5	17326.7
1967	8185.0	17863.6
1968	9383.2	15544.1
1969	10791.8	16479.5
1970	11068.0	17806.6
1971	19577.9	18871.0
1972	19506.1	20193.7
1973	19466.9	22614.6
1974	28082.8	27687.6
1975	26540.0	32570.9
1976	23640.6	35591.9
1977	25189.0	37491.8
Observations	4107	6703

Note that stats grants for Sweden have been linearly interpolated for the years with missing values; especially the years 1965 and 1966.

	(1) Sweden	(2) Finland
Vote share right wing-block		
1963	44.41	
1964	44.35	
1965	44.31	57.49
1966	44.27	57.48
1967	47.63	57.52
1968	47.69	57.54
1969	47.21	62.72
1970	47.16	62.72
1971	48.97	62.67
1972	48.96	62.67
1973	48.97	61.52
1974	50.85	61.53
1975	50.85	61.55
1976	50.85	61.52
1977	51.14	61.94
Observations	4083	5756

	(1) Sweden	(2) Finland
Vote share left wing-block		
1963	51.01	
1964	51.08	
1965	51.14	41.76
1966	51.21	41.77
1967	46.54	41.74
1968	46.59	41.71
1969	46.94	35.30
1970	47.00	35.34
1971	48.34	35.39
1972	48.35	35.39
1973	48.33	36.26
1974	47.33	36.27
1975	47.33	36.24
1976	47.33	36.27
1977	46.73	35.99
Observations	4083	5756

	(1) Sweden	(2) Finland
Share entitled voters		
1963	65.63	58.41
1964	65.49	58.41
1965	64.65	58.41
1966	64.29	58.80
1967	67.36	59.11
1968	66.91	59.44
1969	64.98	62.41
1970	64.69	62.97
1971	69.74	63.98
1972	69.36	64.97
1973	68.98	71.78
1974	69.12	72.20
1975	68.99	72.66
1976	68.66	73.14
1977	73.76	72.80
Observations	4066	6644

Note that the share of entitled voters has been extrapolated backwards for Finland for the years 1963 and 1964

	(1) Sweden	(2) Finland
Winning marginal municipal election		
1963	21.16	
1964	21.16	
1965	21.10	25.97
1966	21.13	25.98
1967	20.52	25.92
1968	20.48	25.96
1969	20.34	32.26
1970	20.30	32.28
1971	19.82	32.23
1972	19.82	32.24
1973	19.81	31.74
1974	18.62	31.74
1975	18.62	31.79
1976	18.62	31.76
1977	18.33	31.01
Observations	4080	5727

A8: Sample evaluation
In order to evaluate the OCR process, random sample analyses have been conducted. The samples were analyzed before the municipal blocks with weighted means were created to facilitate the check in the scanned statistics and before some minor revisions of the paper (the sample check was mostly performed before the first working paper version of this paper). Since the data comes from different printed sources, a sample analysis has been conducted for all sources for relevant variables. Only data which have been part of the OCR process are included in this evaluation.

From the publication series *Kommunal Finanstatistik - Finland* for the years 1967-1972 a random sample was drawn in STATA (without replacement). In total 25 observations were sampled (0.8 percent of total dataset). I analyzed the variables population, state grants, tax rate and tax base. I detected no errors and all the sampled observations corresponded to the scanned material.[30] Second, for the publication series *Årsbok för Sveriges kommuner* for the years 1967-1977 a random sample of 35 observations was drawn (0.6 % of total data set). I analyzed the variables population, state grants, tax rate and tax base. In total, this corresponds to 140 cell values. 3 cell values were spotted to include a false value. The error rate is 2.14 %. [31]

Third, for the publication series *Statistisk Rapport - Finland* for the years 1973-1977 a random sample of 21 observations were drawn (0.9 % of total data set). I analyzed the variables population, state grants, tax rate, total public expenditures and tax base. One cell value contained an error (population); however, in total 3 cell values were infected since state grants and public expenditures are expressed in per capita and I use the population variable to transform these variables. This corresponds to an errors rate of 2.86 % (in total 105 cell values). One “2” had been interpreted as a “7”. This is a result of bad must and faint printing especially for the number 2 in this publication series.

Fourth, for the publication series *Kommunala valen - Finland*, a random sample of 16 observation were drawn (1 % of total data set – but I excluded the observations from year

[30] For the population variable, no sample analysis was performed if the observation belonged to a city since these variable values has been manually inputted from a different data source.
[31] One cell value had taken the value 0 for state grants – but it should have a positive value. This is most likely due to the fact that I replace all missing values with 0 since an empty cell in the printed statistics corresponds to 0 state grants according to the definitions in *Årsbok för Sveriges kommuner* and the coding in my dofile. For the other two errors, one number in each cell was wrong.

1977). I analyzed voter turnout and vote share for the right wing block[32]. In total, one cell value did not correspond to the scanned statistics. I only analyzed the years, 1967, 1969 and 1973 since these are the years for which I have used OCR conversion. This corresponds to an error rate of 3.33 % (in total 30 cell values in the sample)

Fifth, for the publication series *Kommunala valen - Sweden*, a random sample of 14 observations was drawn (1 % of total dataset – but observations belonging to any year after 1973 were excluded from the analysis). I analyzed voter turnout and vote share for the right wing block[33]. No cell values contained a wrong cell value.

The variable Public expenditure was analyzed separately, since it is defined with a two years lead in the Swedish data. For the public expenditure variable in *Årsbok för Sveriges kommuner* a random sample of 29 observations was drawn. Only the years 1967-1975 are included in the analysis since I have manually inputted the information for 1976 and 1977. Two cell values contained inaccurate information. This equals an error rate of 6.9 %. For the Finnish part, 23 observations were randomly drawn for the years 1967-1972 and no cell value contained inaccurate information.

I extended my panel to include the years 1963-1977 at a later stage. I therefore conduct a separate sample evaluation for the years 1963-1966. A random sample of 6 observations and 50 cell values was drawn for these three years including one observation for Finland and Sweden for each year. I analyzed taxrate, population, public expenditures, tax base, state grants for both Finland and Sweden and also voter turnout, number of entitled voters and vote share for the political blocks for the Swedish subsample. I analyzed voter turnout for Finland for the years 1963 and 1964. One cell value was wrong (it was blanc when it should not have been) which corresponds to an error rate of 2 percent.

In conclusion, there are some remaining scanning errors in the final data set that the reader should be aware of. The overall conclusion is however that the OCR process has worked rather satisfactory.

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[32] This was done by some manual calculations since the vote share for the right-wing block is a variable I create by using information from other variables.

[33] Again, this was done by some manual calculations since the vote share for the right-wing block is a variable I create by using information from other variables.

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IV. Public Finance and Right-Wing Populism

Co-authored with Lovisa Persson

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1 Introduction

In the wake of economic crisis and increased immigration flows, established parties in Europe are being challenged by right-wing populists. Incumbent politicians have responded to this challenge in different ways. Consider for instance Prime Minister David Cameron of the Conservative Party (UK), who in 2013 promised to hold a referendum on Britain's EU membership, and pledged to reduce immigration as a reaction to rising voter support for the UK Independence Party. In the 2012 presidential campaign, the incumbent French President, Nicolas Sarkozy from the conservative party UMP, proposed a reduction in the number of immigrants and tighter controls on access to welfare benefits for immigrants in order to counterbalance the growing popularity of the presidential candidate of the National Front, Marine Le Pen. On the other hand, parties in Sweden for long refrained from imitating the policies proposed by the right-wing populist party the Sweden Democrats. Former Swedish Prime Minister Fredrik Reinfeldt of (conservative) the Moderates famously asked of the Swedish people in the general election campaign of 2014 to "open their hearts" to the increased immigration flows. What can explain the different reactions of the established politicians to the challenge of right-wing populist politicians? When do we expect to see an adjustment to the policies proposed by right-wing populists, and when do we expect to see established politicians defending a liberal stance on immigration? In this paper we set up a theoretical model aiming at answering these questions. Our model makes two central predictions. The first is that established politicians will adopt right-wing populist policies if the costs of immigration are high enough. The second is that this adaptation is more likely to happen in a recession compared to when resources are plenty.

The right-wing populist message is that the European welfare states need to be protected from the influx of immigrants. We primarily have in mind asylum seekers that are offered housing, financial contributions and integration programs during and after the asylum process when we refer to immigration in the paper, and not labor immigration. Right-wing populists argue that one additional euro spent on this kind of immigration implies one euro less to be spent on welfare services such as elderly care or child care. Dynamic (or economic) aspects of immigration, that immigrants are a potential future source of revenue, are downplayed in favor of a short-term focus on (accounting) costs. The preference for excluding immigrants from social protections in favour of native citizens is often referred to as *welfare chauvinism*.[1] In a re-

[1] Although we primarily have in mind costs for maintenance of an asylum system, the model can also be applied to the notion of wanting to exclude Schengen migrants from national welfare systems.

cent paper, Eger and Valdez (2015) describe the ideological transformation that right-wing populist parties have undergone in recent decades.[2] Whereas the right-wing populists of the 70's and 80's can be described as neo-liberal and strongly opposed to the welfare state, modern right-wing populists support the welfare system on the condition that the recipients belong to the ethnic majority or current residents. However, modern right-wing populists should still be referred to as right-wing in the sociocultural dimension, i.e. they are conservative rather than progressive, and authoritarian rather than libertarian.[3]

To make the displacement in right-wing populist platforms more explicit, consider the analysis in Kitchelt (1995), where electoral success of right-wing populists is argued to depend on their ability to appeal to free-market solutions, and compare it with a more recent description in Akkerman (2015), where electoral competition between right-wing populists and established parties takes place exclusively in the anti-immigration and nationalistic conflict dimension. The Danish Progress Party (DPP) is a concrete example; the platforms of the 70's and 80's were clearly libertarian and included calls for abolition of income taxes as well as reduced immigration. In the 90's however, the party split and the new Danish People's Party was founded with a platform that indeed kept the anti-immigration proposals but was in favor of the Danish welfare state. Another example is Donald Trump who in 2015 and 2016 campaigned on a less libertarian and a more fierce anti-immigration platform than many of the other candidates in the Republican primary elections.

In line with the discussion above, we present a model where not only voters – but also politicians – have different preferences for how much to spend on two types of public goods: basic welfare services and a generous asylum system. We propose that this within-budget-distributional conflict can arise as an electorally decisive conflict dimension in a situation where parties – including right-wing populists – have converged to the median voter position in the size-of-government conflict dimension. We model voter preferences for spending on different public goods

[2]Parties such as Jobbik in Hungary and Golden Dawn in Greece should be referred to as extremist parties with openly anti-semitic, neo-nazi agendas with violent images, and are not the focus of our paper.

[3]Eger and Valdez (2015) prefer to label the right-wing populist parties as "neo-nationalistic", but in this paper we stick to the term "right-wing populist", since they still have a right-wing flavor in terms of their national conservatism and they are populists in the sense that they strongly advocate one (or few) solution(s) – such as reduced immigration – to a wide range of complex social issues. Admittedly, the concept of "populism" has previously been used to describe a variety of political phenomenon; we simply choose to use as our starting point the "popular" or journalistic definition, i.e. by including those parties that are referred to as right-wing populist in the public debate.

as being generated by differences in private consumption. Private consumption and basic welfare services are modeled as perfect substitutes and as such, *poor* voters always want a higher level of basic welfare services than *rich* voters. If public resources are scarce, preferences of the poor voters are perfectly aligned with the welfare chauvinistic populist option which is to spend everything on basic welfare services and nothing on immigration.

We set up a two period model where the politician in office in each period decides how to divide funds between the two public goods. Due to the absence of electoral incentives, politicians implement their preferred policy in the second period. The problem for voters is to maximize second period utility by voting for the incumbent or the challenger depending on who is most likely their preferred type. If poor voters are in a majority, incumbent populists can implement populist policies and be reelected, since the preferences of the incumbent and the poor voters are aligned. On the other hand, if the incumbent is an established politician, he will only deviate from his own preferred policy, which includes spending on immigration, if the costs of immigration are high enough.

Basic welfare services is a public good that satisfies basic needs. A generous asylum system, on the other hand, is only demanded if either public resources are plentiful or if individual private consumption is high enough. In this sense, asylum system generosity is a "luxury" public good, which could be said to satisfy a need to express social preferences. Although we currently focus on spending on immigration in this paper, it is possible to imagine other public goods that fit into a category of "luxury" public goods, such as environmental-friendly policies. We denote basic welfare services as primary goods in the model and goods such as immigration are denoted as secondary goods to emphasize the general nature of these two types of public goods. Although our model is not concerned with the classic right-left conflict dimension, the primary good can be interpreted to include goods that are typically preferred by the left, such as generosity in welfare programs, and as well as goods that are typically preferred by the right, such as defense.[4] Because of the connection between the level of resources and

[4]One inspiration for modeling the relationship between these two goods in such a way comes from the "hierarchy of needs"-model by Maslow (1943). According to this theory, an individual's moral needs – as part of a wider self-fulfillment strive – can first be fulfilled as long as the individual is satisfied in terms of basic needs. As an example, Maslow (1943) puts food and water at the bottom of the hierarchy, and morality and lack of prejudice at the top. One can also make a connection with the literature on human altruism, the preference for the secondary good can be described as an expression of strong reciprocity. Strong reciprocity, or pure altruism, in contrast to reciprocal altruism, implies punishment of bad behavior and rewarding of good behavior even in non-repeated interactions, i.e. even when individuals do not expect any "return" on their behavior, see Fehr and Fischbacher (2003).

support for right-wing populist policies we model the poor voters as being "materialistically" motivated. This is in line with the socioeconomic explanation of support for right-wing populist policies, see Malchow-Møller et al. (2008). However, in our model we focus not on potential labor market competition between immigrants and blue-collar workers but rather on the perceived competition for public resources. The right-wing populists themselves, on the other hand, are modeled as being "ideologically" motivated (nationalistic or xenophobic) since they want to reduce expenditures on a generous asylum system independent of the level of public resources and private consumption.

Let us now return to the dilemma of David Cameron, and not the least, the dilemma of the British voters, who have to decide what candidate to vote for. Even if David Cameron, who belongs to a political party that can be considered to be an established party, chooses to implement anti-immigration policies today or make use of such rhetoric when running for reelection, voters cannot be sure that he will stick to this policy path after the election. Since voters can never be certain whether the anti-immigrant policies implemented by incumbents are manifestations of true preferences or whether they are short-term strategic maneuvers to win votes, we adopt a framework with hidden politician types as in the agency model of Besley and Smart (2007). We thus combine the large literature on political agency with the literature on policy-motivated politicians.[5] A second information asymmetry in our model regards the relative cost of public goods, which is only observed by the incumbents and not by the voters. This assumption does not imply that the voters are ignorant about world-events; a war in another country for example with the result of an increase of refugees in the world can be observed by the voters, but they do not observe the relative cost of the asylum system in comparison to the cost of basic infrastructure for instance. This assumption is in line with the conclusions presented in Caplan (2008) where American voters were demonstrated to have poor knowledge about the relative cost of different public goods. The realization of the relative cost variable is the driving mechanism in our model and determines the action that the established politician takes. Since voters neither know the politician type nor the realized value of the relative cost variable, voters update their expectations on the incumbent's type by mapping implemented policies and expected incentives of the politicians. Voters elect the incumbent over the challenger if the posterior probability that the incumbent is their preferred politician type is higher than the prior probability.

[5] See Besley and Coate (1997), Osborne and Slivinski (1996) and Alesina (1988) for seminal papers on policy-motivated politicians. The first papers to focus on agency and incumbent behavior was Barro (1973) and Ferejohn (1986).

There are very few earlier papers in economics which explicitly describe a right-wing populist policy conflict. In a paper by Acemoglu et al. (2013) the starting point is to describe left-wing populism in the context of South American politics. In a model extension, the authors explore the possibility of right-wing populism, which they describe as when an incumbent politician adopts policies that are situated to the right of the median voter in order to signal that she is not "captured by the left-wing lobby such as trade unions". As such, the Acemoglu et al. (2013) model of populism focuses on the role of vested interests in politics against the interest of the wider population. The analysis of right-wing populism in Acemoglu et al. (2013) is constrained by the fact that it has to conceptually fit into a model which primarily has the purpose of analyzing left-wing populism. Although there are certainly factors where the concepts of left-wing and right-wing populism relate to each other, there could also be interesting variations in voter concerns and politician trade-offs.

In the popular debate, right-wing populist parties are often referred to as "protest parties". Political scientists and economists generally have differing views of the widely used concept of "protest voting". Whereas protest voters are depicted as strategically sophisticated in economics models, they are emotively expressive in political science theory, see Van der Brug et al. (2000) for a political science perspective. To summarize the economics literature on protest voting would be to say that protest voting is an act of not choosing sincerely among the present political alternatives, but instead choosing a less preferred alternative so as to signal exact preferences. In Piketty (2000), voters might deviate from their most preferred policy in the first round of voting, in order to get more people to support a third even more preferred option in the second round of voting.[6] In Myatt (2016), voters would like their most preferred candidate to win, but just barely, such that the candidate might still reevaluate its policies and present an even better proposal in the future. In the Downsian inspired model of Kselman and Niou (2011), some voters are discontent with the position of their most preferred party in a one dimensional policy space. After an election, parties will respond to vote losses due to protests and adjust policy in the direction in which they gain most votes. In Castanheira (2003), voters support extremist parties (even if they are expected to lose) in order to send signals of where the median voter is placed, and thus induce a policy change from the mainstream parties. A common feature of the models described above is the uncertainty of preferences and/or sizes of

[6]While Piketty (2000) refers the model as describing the phenomenon of "communicative voting" and not "protest voting", the mechanism has a lot in common with what is usually referred to as the protest mechanism.

different groups of voters, in which case repeated elections must be held in order for voters to signal their preferred policies. Another unifying theme in this literature is that the policy conflict is highly abstract in order to focus attention on the strategic mechanisms.[7]

Our contribution to the literature above is to specify the public finance conflict that is at the center of right-wing populist platforms. Instead of abstracting from the policy conflict we specify it as a within-budget distribution conflict, and we relate it to the concept of welfare chauvinism. We describe voters as being materialistically motivated in their electoral support for right-wing populist policies. However, this description is only one among many other motivations found in the literature on support for right-wing populism such as: strategic protest behavior or xenophobic attitudes, among which empirical studies will have to decide which plays the greater role. To our knowledge we are the first to model welfare chauvinism as a political preference that can be decisive for an election. More importantly, with these preferences as given we can describe a possible mechanism in the form of relative costs between two public goods as a factor that drives established politician responses to right-wing populist challengers. Our model makes two intuitive predictions: (1) If the relative costs of immigration are high enough, we expect more incumbent politicians to adopt right-wing populist policies; and (2) incentives to adopt populist policies are stronger when economy is in a recession.

2 The Model

Our model is inspired by political agency models, primarily Besley and Smart (2007), where the decision making problem of an incumbent policy-motivated politician is at the center of the analysis. There are two time periods, and in each period the incumbent politician makes decisions concerning government spending on two different types of public goods. In between the two time periods there is one election where two types of voters decide to vote for either the incumbent or the challenger. Since voters do not directly observe the true type of the incumbent politician, they have to infer the type based on the implemented policies. Voters then choose to vote for the candidate that has the highest probability of being their most preferred type of politician. The incumbent politician might deviate from his most preferred policy in the first period if that gets him reelected to a second period, where he is free to

[7] Earlier literature also highlights the difference between protest and strategic voting, where strategic voting emerges when the most preferred party is unlikely to win, and protest voting, however, takes place even though the preferred party is expected to win, see Kselman and Niou (2011).

implement his most preferred policy without worrying about reelection incentives. We return to the timing of the model in section 2.3.

2.1 Voters

Voters receive utility from two public goods; a primary public good g and a secondary public good h. The primary public good g_t represents basic welfare services, while the secondary public good h_t is to be interpreted as the generosity of the asylum system in terms of housing, financial contributions, measures of integration and the possibility of relatives to asylum seekers to receive residence permits. Voters derive utility from this generosity because of social preferences for people in need. Besides public goods, voters also get utility from private consumption c_v, which enters the model as a perfect substitute to the primary public good. We assume two groups of voters: p**o**or voters o and r**i**ch voters i, who differ in their levels of private consumption such that $c_i > c_o$. Voters receive utility in each time period t according to the utility function in Equation 1.

$$U_{vt} = h_t + G(g_t + c_v) \tag{1}$$

Voters have linear preferences in the secondary public good h_t and strictly concave preferences in the primary public good g_t such that $G'(\bullet) > 0$ and $G''(\bullet) < 0$.

We assume the following public budget constraint.

$$T_t = g_t + \theta_t h_t \tag{2}$$

In light of the discussion concerning policy convergence in the size-of-government dimension in the introduction, the tax rate is determined exogenously. However, the resource level T_t can be either high (boom) or low (recession) depending on the realization of a macroeconomic shock. The level of resources T_t is i.i.d. in each period with $T \in \{H, L\}$ and $Pr(T = H) = \phi$ and $H > L$. The relative cost of the secondary public good θ_t is independently drawn from a uniform distribution $\theta \in [\underline{\theta}, \overline{\theta}]$ with expected value θ_E. Voters do not observe the realized value of θ_t but they observe the parameters of the probability distribution. In order to simplify notation, the time subscript t is dropped when characterizing voters' static maximization problem below.

$$\max_{g,h} U_v = h + G(g + c_v) \quad \text{s.t.} \quad T = g + \theta h \tag{3}$$

The voters' optimal bundles g_v^* and h_v^* can be represented by the following equations, where $G_g^{-1}(\bullet)$ is the inverse of the first derivative of the function $G(\bullet)$.

$$g_v^* = G_g^{-1}\left(\frac{1}{\theta}\right) - c_v \qquad h_v^* = \frac{T + c_v - G_g^{-1}\left(\frac{1}{\theta}\right)}{\theta} \tag{4}$$

From the first order conditions it is clear that demand for h increases in public resources T, while demand for g is entirely determined by the relative cost variable and private consumption. Consider our two different voter groups; poor voters demand more of the primary good g for all levels of public resources than rich voters since poor voters have less private consumption. We make the following assumptions regarding poor voters' optimal policies:

Assumption 1.

$$G_g^{-1}\left(\frac{1}{\underline{\theta}}\right) - c_o \geq L \qquad G_g^{-1}\left(\frac{1}{\overline{\theta}}\right) - c_o < H$$

Assumption 1 states that poor voters' optimal policy in a recession ($T = L$) is a corner solution such that $g_o^* = L$. In other words, the value of the relative cost shock θ can never be so low so that poor voters prefer an interior solution in a recession. However, in a boom ($T = H$) poor voters' optimal policy is an interior solution with positive levels of both public goods. While poor voters strictly prioritize the primary public good when resources are low, we additionally assume that rich voters' consumption is such that they prefer an interior solution in both a boom and a recession. The relationship between resource level and the demanded level of the two public goods illustrates how voters prioritize between two different needs. Only if public or private resources are high enough the preferences allow for spending on the "luxury" public good h_t.

2.2 Politicians

Politicians j can be one of two types: **e**stablished e or **p**opulist p. The established politician has policy preferences that are identical to rich voters, whereas the populist politician never receives utility from the secondary public good.[8] The utility functions of the politicians are the following

[8] After deciding by a flip of a coin, the established politician will be denoted *he* and the populist politician will be denoted *she* in the text that follows.

$$U_{et} = h_t + G(g_t + c_e) \tag{5}$$
$$U_{pt} = g_t + c_p \tag{6}$$

We make the following assumptions concerning private consumption.

Assumption 2. $c_e = c_p = c_i > c_o$

In other words, both politicians have the same level of private consumption as the rich voters. The populist politician has no care at all for the secondary public good, as opposed to poor voters who want to consume the secondary public good as long as the resource level is high enough.[9] The populist politician thus prefers welfare chauvinistic policies on ideological or xenophobic grounds, while poor voters support welfare chauvinistic policies on financial (or material) grounds. In the main analysis we assume that politicians are exclusively policy-motivated. However, in section 4 we add office-motivation as a politician incentive.

2.3 Timing and Information

In the first time period an incumbent is drawn from a pool of politicians. The incumbent is populist with $Pr(j = p) = \mu$ and established with $Pr(j = e) = (1 - \mu)$. The incumbent type is known to the incumbent but not to the voters. Thereafter, the state of the world is realized and can either be a boom or a recession. The probability of a boom is $Pr(T = H) = \phi$ and the probability of a recession is $Pr(T = L) = (1 - \phi)$. There is perfect information in the model with regards to ϕ. The first period cost shock θ_1 is realized and observed by the incumbent but not by the voters. The state of the world and cost shock realizations are independent from each other. The incumbent maximizes the sum of utility over the two time periods by choosing policy (g, h) in the first period. The incumbent knows which voter group that is in majority in the electorate. Voters observe the implemented policy, they get utility from the policy, and they update their beliefs

[9]In line Besley and Smart (2007), we do not model the decision to run for office, which becomes especially clear since the preferences of the populist do not exactly represent any voter group. We acknowledge that for there to be populist politicians, there must also be some voters that share their utility function, in other words, that are also motivated by xenophobia rather than their low private consumption level. The fact that poor voters in our models are "misrepresented" agrees with earlier analyses of protest voting where voters are also intermediately misrepresented, see the introduction. In the case with preferences that we are studying, misrepresentation is an important ingredient.

about the incumbent's type according to Bayes rule. Voters elect the incumbent or the challenger depending on which is their preferred type with highest probability. The candidate receiving the most votes wins. Second period state of the world and relative cost shock θ_2 are realized. The elected politician implements a second period policy, voters and politicians get utility from the policy, and after that the world ends.

3 Equilibrium

In this section we show the existence of pooling and separating equilibria in our two period model. We solve the model using backwards induction, i.e. we start by investigating the incentives of voters and politicians in the last period. Voters realize that politicians implement their own preferred policy in the last period since there are no reelection incentives. Therefore we start by analyzing what type of politician voters prefer. In the first period, however, incumbent politicians can either implement their preferred policy or mimic the behavior of the other type in order to get reelected. Voters update their beliefs about the true type of the incumbent after observing the implemented policy. We analyze when first period state of the world in terms of public resources is low and high respectively. For each resource state there are two interesting cases to analyze; when the incumbent is established and poor voters are in a majority, and when the incumbent is populist and rich voters are in a majority. However, we only analyze the pooling dilemma of first period established incumbents since this is the most interesting case.

3.1 Which Politician Type Do Poor Voters Prefer in the Last Period?

Clearly, the rich voters' preferred politician is the established politician since they have the same level of private consumption according to Assumption 2, and thus they have the exact same policy preferences. The poor voters' preferred type is not as easily determined. All we know hitherto is that poor voters have the same demand for public goods as the populist politician if the world is in a recession. However, when poor voters decide what candidate to vote for they have to take into account the possibility that the second period will be a boom.

Consider W_H^p that represents poor voters' indirect expected utility (in a boom) evaluated at the preferred policy of the populist, and W_H^e represents poor voters' indirect expected utility (in a boom) evaluated at the preferred policy of the established politician. The poor voters strictly prefer the populist policy over the established politician's policy if $W_H^p > W_H^e$. From Assumption 2 we know that $c_o < c_e$, from which it follows that W_H^e is decreasing in c_e. As the established politician's private consumption level grows, the more dissimilar his preferences become in relation to poor voters. Poor voters thus prefer the populist policy in a boom if the difference between c_e and c_o is large enough. Consider the following proposition

Proposition 1. *There exists a $\hat{c}_e$ such that poor voters prefer the populist politician in a boom if $c_e \geq \hat{c}_e$.*

Proof. See Appendix for proof of Proposition 1. □

In an uncertain world, where public resources can be either high or low, the objective for voters is to choose the candidate that is most likely the type of politician that the gives the highest *expected* utility in the second period. Even if Proposition 1 does not hold, it could still be the case that poor voters prefer the populist type in expectation. Poor voters prefer the populist politician in expectation if

$$\phi W_H^p + (1-\phi) W_L^p \geq \phi W_H^e + (1-\phi) W_L^e \tag{7}$$

After inserting utility functions, rearranging and simplifying, we get the following expression, where θ_E is the expected value of θ

$$W_L^p + \phi(W_H^p - W_L^p) \geq \phi \frac{H-L}{\theta_E} + W_L^e \tag{8}$$

Whether or not Equation 8 holds partly depends on the size of the difference $(W_L^p - W_L^e)$, which is positive and growing in $c_e - c_o$. Additionally, as H grows, the benefits from consuming the established politician's preferred policy (RHS) is increasing faster than if consuming the populist politician's preferred policy, since the populist politician spends the extra resources only on the primary good in which marginal utility is decreasing. Thus, poor voters prefer the populist politician in expectation if $c_e - c_o$ is large enough or if H is small enough. For the equilibrium analysis that we conduct, the conditions in Equations 7 and 8 must hold. In any other case, all voters will always prefer the established politician, and there will never be a pooling equilibrium where the established politician mimics the populist type.

Up until now, we have only briefly mentioned xenophobic attitudes among the voters and instead focused on economic reasons for why poor voters would vote for a populist. Our model may however be applied to a scenario where a majority of the voters are motivated by xenophobic attitudes. The populist politician would be preferred by such voters in both time periods regardless if the economy is in a boom or in a recession. In that case we do not need the conditions in equation 7 and 8 for the equilibrium analysis.

3.2 Recession

The economy is assumed to be in a recession (resources are low $T = L$), the support of the poor voters is needed to win the election, and poor voters prefer the populist politician in expectation, see section 3.1. If the incumbent were a populist, the equilibrium solution is trivial, since all she has to do is implement her preferred policy and get reelected. The interesting situation thus arises when the incumbent is an established politician. The established incumbent observes the first period realized value of θ_1 and chooses the levels of the primary public good g_1 and the secondary public good h_1 that maximizes expected utility taking into account voter strategies. The established politician can decide to pool with the populist type by implementing $g_p^* = L$ in the first time period, in which case the incumbent type is indistinguishable in the eyes of the voters. If the established politician instead decides to separate and implement his preferred policy, poor voters will know for sure that the incumbent is the established type and they will then prefer to take their chances on the challenger. Poor voters assign probability zero to the populist type at any other policy than $g_1 = g_p^* = L$, because there are no incentives for a populist incumbent to implement anything else when poor voters are in a majority.

If voters observe levels of both goods and also the resource shock, they can make use of the public budget constraint in Equation 2 to back out the value of θ_1. However, since there is no spending at all on h, if the established politician decides to pool, voters are not able to figure out the value of θ_1. This is very important, since if voters know θ_1 they will have additional information about the true type of the incumbent. What voters *do* know however, is the probability distribution for θ_t and therefore they evaluate their options according to the Bayes' rule expression below.

$$\frac{\mu}{\mu + (1-\mu)\rho} \geq \mu \tag{9}$$

With probability ρ, the draw of θ_t is such that pooling is beneficial for the established politician. According to Bayes' rule, poor voters are better off reelecting an incumbent that has implemented a populist policy, than selecting a random challenger. The left-hand side expression in Equation 9 is the probability that the incumbent is populist conditional on having observed populist policies being implemented in the first period. As long as there is some possibility that an established politician reveals his type in a separating equilibrium, $\rho < 1$, the posterior probability is strictly larger than the prior probability μ. According to Bayes rule, all incumbents will be reelected if they implement $g_1 = g_p^* = L$, which include all populists and some of the established politicians.

The established incumbent faces the following short-run and long-run trade-offs: if he pools with the populist type, he experiences a short-run decrease in utility compared to if he would implement his preferred policy. However, since he gets reelected for acting like a populist (according to Bayes rule) he can implement his preferred policy in the last period without worrying about getting reelected. In order for him to find it beneficial to pool, the long run benefits must outweigh the short run cost. The established politician decides to pool if the following condition holds

$$\begin{aligned} & W_L + \phi W_H^* + (1-\phi) W_L^* \geq \\ & \geq W_L^*(\theta_1) + \mu[\phi W_H + (1-\phi) W_L] + (1-\mu)[\phi W_H^* + (1-\phi) W_L^*] \end{aligned} \quad (10)$$

where W_L represents indirect utility of the established politician evaluated at the populist policy in state L, whereas W_L^* (simplified from $W_L^*(\theta_E)$) represents indirect utility from the established politician's optimal policy in state L. W_H and W_H^* have corresponding interpretations. Indirect utility from implementing the populist policy W_L does not depend on θ_1 since the populist policy implies that all resources are spent on g independent of the relative cost. However, indirect utility from the established politician's preferred policy $W_L^*(\theta_1)$ depends on the relative cost in a strictly negative way. Also note that as the relative cost increases, the preferred policies of the established politician and the populist becomes more alike since the established politician prefers more of the primary good g when the secondary good h becomes relatively expensive. Both these mechanisms work in the same direction, namely that incentives to pool with a populist politician in period 1 is increasing in θ_1. We summarize this result in the following proposition.

Proposition 2. *There exists a $\hat{\theta}$ such that it is optimal for the established politician to pool in a recession when $\theta_1 \geq \hat{\theta}$.*

Proof. See Appendix for proof of Proposition 2 □

We prove the above proposition by first examining the incentives when the realized relative cost in period 1 equals the expected relative cost, $\theta_1 = \theta_E$. It turns out that in this case, the established politician will never pool if $\mu = 0$, and always pool when $\mu = 1$. The first result is particularly easy to understand. With politicians that are exclusively policy-motivated, implementing the preferred policy yourself is equivalent to having someone else doing it. So if the challenger is populist with zero probability there is no longer any point for the established politician to try to win the election. However, if the challenger is populist with certainty the established incumbent always pools. The reason is that he does not want to miss out on the chance of implementing his preferred policy in a boom, a possibility he would completely miss out on if he separates in a recession and has to live with populist policies in the second period. If the established politician always pools for the extreme value $\mu = 1$ and always separates for the other extreme $\mu = 0$, there exists a cutoff point of $0 < \mu < 1$ when pooling and separating are equally good and $\theta_1 = \theta_E$.

Extreme cases of μ are used in the section above in order to simplify intuition. However, the model is complete only when $\mu \in (0, 1)$. So let us solve for μ in Equation 10 and let $\theta_1 \neq \theta_E$. The established politician pools if

$$\mu \geq \frac{W_L^*(\theta_1) - W_L}{EW^* - EW} \tag{11}$$

In Equation 11 $EW = \phi W_H + (1 - \phi) W_L$ and $EW^* = \phi W_H^* + (1 - \phi) W_L^*$. The nominator in Equation 11 is the first period utility *loss* from pooling, and the denominator is the second period expected *gain* from pooling. For high values of θ_1, the nominator is small which implies that the share of populist politicians μ can be smaller for the established politician to find it beneficial to pool. We can also reverse the argument and say that for higher values of μ the cut-off value $\hat{\theta}$ can be smaller, and smaller cost shocks will therefore induce a pooling response.

Comparative statics

We have already established how changes in μ affect equilibrium behavior; increases in μ makes pooling more attractive. What about the other parameters? If the probability of having a boom ϕ increases, incentives to pool increases. The intuition behind this result is the following: Having a boom in the second period is most beneficial if the politician can implement the optimal policy. This tips the scales towards pooling since the established politician can be sure that the realization of a boom is capitalized by himself and not the populist.

The effect of an increase in the established politician's private consumption level c_e is more difficult to characterize. The established politician prefers more secondary public goods h and less primary public goods g when c_e increases, meaning that his preferred policy diverges more from the populist policy. This process serves to increase both the cost of pooling in the first period, while at the same time also increase the cost of separating. The effect of c_e on the pooling decision of the established politician is thus ambiguous, and depends on the parameters of the model and the realization of θ_1.

We summarize the comparative statics discussed above in the proposition below.

Proposition 3. *The pooling decision of the established politician in a recession depends on the parameters of the model in the following way:*

i An increase in μ increases pooling incentives and decreases the cut-off value $\hat{\theta}$.
ii An increase in ϕ increases pooling incentives and decreases the cut-off value $\hat{\theta}$.
iii An increase in c_e has an ambiguous effect on pooling incentives

Proof. See Appendix for proof of Proposition 3 □

3.3 Boom

As in the case with a recession, if poor voters are in majority and the incumbent is a populist politician, she will always separate in the first period, get reelected and again implement her preferred policy in the second time period. If the incumbent is an established politician he has to consider the option of pooling with a populist politician. As was previously stated in section 3.1, poor voters might prefer the established politician's policy during a boom. However, if they prefer the populist politician in expectation (for which we have derived a condition in 8) they will still not reelect the established politician for implementing their preferred policy, but rather the politician which is most likely populist. The intuition is that second period utility is the only thing voters can actually influence by voting. The established politician therefore has to implement the populist policy if he wants to be reelected. Since voters again cannot observe the true value of θ_1 we have the same Bayes' rules as in Equation 9.

The established politician pools in a boom if

$$\begin{aligned} & W_H + \phi W_H^* + (1-\phi)W_L^* \geq \\ & \geq W_H^*(\theta_1) + \mu[\phi W_H + (1-\phi)W_L] + (1-\mu)[\phi W_H^* + (1-\phi)W_L^*] \end{aligned} \quad (12)$$

Since $W_H^*(\theta_1)$ is strictly decreasing in θ_1, higher values of θ_1 again implies stronger incentives for choosing the pooling option. Consider the proposition below.

Proposition 4. *There exists a $\tilde{\theta} > \theta_E$ such that it is optimal for the established politician to pool in a boom state when $\theta_1 > \tilde{\theta} > \theta_E$*

Proof. See Appendix for proof of Proposition 4 □

In the proof of Proposition 4 we show that when $\theta_1 \leq \theta_E$, the established politician will never pool, not even when the probability of having a populist in office in the second period is certain ($\mu = 1$). This result is different to what we previously saw for the case with a recession, where the established incumbent always pooled if the challenger is the populist type with probability one and $\theta_1 = \theta_E$. The intuition for $T = H$ result is that the incumbent has the opportunity to implement the preferred policy in a situation where resources are plentiful. If the established incumbent chooses to pool, there is a downward risk of ending up with a recession in the second period and as such, the incumbent might miss out on implementing the most preferred policy in a boom. In a recession, on the other hand, the incumbent only faces an upward risk after pooling. A necessary condition for a pooling equilibrium in a boom is therefore that $\theta_1 > \theta_E$. Let us once again solve for μ in the pooling condition where $\theta_1 \neq \theta_E$:

$$\mu \geq \frac{W_H^*(\theta_1) - W_H}{EW^* - EW} \tag{13}$$

The nominator is the loss of pooling and the denominator is the expected gain of pooling. If we compare with the recession pooling condition, Equation 11, we see that the loss of pooling in a recession is lower than the loss of pooling in a boom. That $W_L^*(\theta_1) - W_L < W_H^*(\theta_1) - W_H$ is proved in Lemma 1 in the Appendix. Our model thus clearly predicts that pooling will be more common when first period resources are low than when first period resources are high.

Comparative statics

The intuition behind the comparative statics is similar to what we presented in section 3.2. A difference lies in the effect of c_e which now increases the incentives to separate if $\theta_1 \geq \theta_E$, but for other values of θ_1 the effect is ambiguous.

Proposition 5. *The pooling decision of the established politician in a boom depends on the parameters of the model in the following way:*

- *i An increase in μ increases pooling incentives and decreases the cut-off value $\tilde{\theta}$*
- *ii An increase in ϕ increases pooling incentives and decreases the cut-off value $\tilde{\theta}$.*

iii An increase in c_e increases incentives to separate if $\theta_1 \leq \theta_E$ but has an ambiguous effect on pooling incentives for $\theta_1 > \theta_E$.

Proof. See Appendix for proof of Proposition 5. □

4 Office-Motivated Politicians

We now introduce office-motivation in the form of ego-rents, by inserting $R > 0$ in the established politicians utility function in Equation 5. Since the politician receives ego-rents in the first period no matter what policy is implemented, the ego-rent only affects the pooling option. The pooling condition for the established politician now looks as follows

$$\begin{aligned} & W_L + R + \phi W_H^* + (1-\phi) W_L^* + R \geq \\ & \geq W_L^*(\theta_1) + R + \mu[\phi W_H + (1-\phi) W_L] + (1-\mu)[\phi W_H^* + (1-\phi) W_L^*] \end{aligned} \tag{14}$$

Clearly, introducing ego-rents from being in office makes it more beneficial to pool. In a hypothetical scenario where the ego-rents from holding office goes to infinity, the established politician becomes strictly office-motivated and will pool for all values of θ_1.

Proposition 6. *An increase in R increases pooling incentives and lowers the cut-off values $\hat{\theta} \wedge \tilde{\theta}$.*

5 Discussion and Conclusion

As right-wing populists have moved more and more to the political center in the traditional left-right economic conflict dimension, a within-budget distributional conflict has become a salient public finance issue of right-wing populism. In other words, the policy proposal of today's right-wing populists is to decrease public resources that are spent on the asylum system so that basic welfare services are prioritized instead; an attitude towards public finances that we have referred to as "welfare chauvinistic".

In this paper we have analyzed the reelection behavior of incumbent politicians in the presence of right-wing populist challengers who promote welfare chauvinistic policies. Even though the model is methodologically inspired by political agency models, the driving mechanism of the model is the policy-motivation of politicians – in terms of preferences for welfare chauvinism. Our model includes two types of politicians, two types of voters, two types of public goods, and two time periods. We model poor voters are being the main supporters of welfare chauvinistic policies, promoted by the populist politicians. The interesting situations in our model appear when support of poor voters is needed to win an election and the incumbent politician is an established politician who has preferences against welfare chauvinism. The established politician then has to decide whether to implement (populist) welfare chauvinistic policies in order to win reelection, or to implement his preferred policy and leave office altogether.

We concluded that when the incumbent is an established politician, he adopts the policies of a populist type and get reelected if the relative cost of secondary public goods is high enough. As the cost of immigration increases, the preferred policy bundle of the established politician becomes more similar to the preferred bundle of the populist politician, and thus the pooling option becomes more tempting. When the first period is a booming state, the established politician pools less often since the utility gain of implementing the preferred policy in the first period when resources are plentiful is particularly high.

Let us return to our initial examples of Sweden and the United Kingdom. Our model predicts that an adaptation to the populist policy will be more common if the economy is a recession. After the financial crisis in 2008, all countries in Europe experienced high unemployment, shrinking economies and a dissolving banking sector. The UK, with its large financial sector was particularly afflicted and the Conservative-Liberal government rolled-out austerity policies after their election win in 2010 in order to put a stop to the growing budget deficit. This difference in experience after the financial crisis between the UK and Sweden may explain why the Tory lead government adapted rhetoric and policy proposals previously held by UKIP while the Moderate Party in Sweden kept their liberal immigration policy in the 2014 election. On the other hand, the number of refugees arriving to Sweden increased dramatically in 2015, meaning that the relative cost of a generous asylum system in comparison to other public goods has increased. The fact that political parties in Sweden dramatically changed their immigration policy in the fall of 2015 is thus consistent with our model.

Future papers should attempt to incorporate both the cost and the revenue side of secondary goods. Immigration is the primary focus in our model, but also environmental protection can be counted within this class of "luxury" public goods. Clearly, these goods can be see as investments that makes future revenues possible. Future models should incorporate these dynamic effects, possibly with subjective expectations on how large these dynamic effects are.

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7 Appendix

Proof of Proposition 1. In this proof we show that poor voters strictly prefer the established politician when they have the same income ($c_e = c_o$), but that utility from the established politician's policy decreases as c_e increases. Poor voters prefer the populist policy in the high state if $W_H^p \geq W_H^e$. Using the indirect utility functions we can write this as

$$G(H + c_o) \geq G\left[G_g^{-1}\left(\frac{1}{\theta_E}\right) - c_e + c_o\right] + \frac{H + c_e - G_g^{-1}\left(\frac{1}{\theta_E}\right)}{\theta_E} \tag{15}$$

The above never holds if $c_e = c_o$, since the poor voters would then have the same preferences as the established politician. Let us take the derivative of W_H^e (RHS) with respect to c_e to see how poor voter utility, evaluated at the policy of the established politician, changes as the private consumption of the established politician increases.

$$\frac{\partial W_H^e}{\partial c_e} = -G'\left[G_g^{-1}\left(\frac{1}{\theta_E}\right) - c_e + c_o\right] + \frac{1}{\theta_E} \tag{16}$$

From the poor voter first order condition for an interior solution we know that the following must hold in optimum.

$$G'(g_o^* + c_o) = \frac{1}{\theta_E} \tag{17}$$

Inserting the solution for optimal policy into Equation 17 we get the following

$$G'\left[G_g^{-1}\left(\frac{1}{\theta_E}\right)\right] = \frac{1}{\theta_E} \tag{18}$$

Since $c_e > c_o$ we know that $G_g^{-1}\left(\frac{1}{\theta_E}\right) - c_e + c_o < G_g^{-1}\left(\frac{1}{\theta_E}\right)$. Since $G''(\bullet) < 0$ we know that the marginal utility evaluated at the established politicians preferred policy is higher than the marginal utility evaluated at the preferred policy of the poor voters. It follows that the expression in Equation 16 is negative when $c_e > c_o$, and that poor voter utility evaluated at the preferred policy of established politicians is decreasing in c_e. Since utility from the populist politicians preferred policy is a constant there will be a cut-off value of $\hat{c}_e$ where poor voters switches from preferring the established politician to preferring the populist politician.

□

Proof of Proposition 2. We begin by showing that there exists a pooling equilibrium when $\theta_1 = \theta_E$. Consider also that $\mu = 0$. When inserting the above assumptions into Equation 10 we get the following contradiction $W_L \geq W_L^*$. The condition for pooling will thus never be satisfied if $\theta_1 = \theta_E$ and $\mu = 0$.

Consider now the case where $\mu = 1$. The established politician will pool if

$$W_H^* - W_L^* \geq W_H - W_L \tag{19}$$

The inequality in Equation 19 is a recurrent feature in our analysis, and therefore we prove that this inequality always holds in Lemma 1.

Lemma 1. *The utility gain from $T = H$ compared with $T = L$ is higher when evaluated at the optimal policy.*

$$W_H^* - W_L^* > W_H - W_L \tag{20}$$

Proof of Lemma 1. We prove Lemma 1 by taking the derivative of W^* and W respectively with respect to T. We get the following results.

$$\frac{\partial W^*}{\partial T} = \frac{1}{\theta} \qquad \text{and} \qquad \frac{\partial W}{\partial T} = G\prime(T + c_e) \tag{21}$$

In an interior optimum we know that $G'(g_e^* + c_e) = \frac{1}{\theta}$. We also know that $g_e^* < T$ for an interior solution. Since $G'(\bullet) > 0$ and $G''(\bullet) < 0$ it follows that a utility increase from higher T is larger if the established politician consumes his optimal policy. Since $G'(T + c_e) < \frac{1}{\theta}$ we know that $W_H^* - W_L^* > W_H - W_L$ will always hold.

□

Proof of Proposition 2, cont. Let us rewrite the pooling condition as follows under the assumption that $\theta_1 = \theta_E$.

$$W_L + EW^* \leq W_L^*(\theta_E) + \mu EW + (1 - \mu)EW^* \tag{22}$$

So far we have shown that the established politician will never pool if $\mu = 0$, and always pool if $\mu = 1$. It follows that there is some linear combination where $0 < \mu < 1$ of the two constants EW^* and EW

which makes the right-hand side and the left-hand side in Equation 22 equal to each other. Consider now that we keep μ fixed at this interior value and instead we let θ_1 vary. The realized value of θ_1 will determine whether the established politician will pool or not since $W_L^*(\theta_1)$ is strictly decreasing in θ.

In the calculation below we show that the established politician's utility from the preferred policy is decreasing in θ.

$$W_L^*(\theta) = \frac{L - g^*(\theta)}{\theta} + G[g^*(\theta) + c_e] \tag{23}$$

$$\begin{aligned}
\frac{\partial W_L^*(\theta)}{\partial \theta} &= -\frac{L - g^*(\theta)}{\theta^2} - \frac{g^{*\prime}(\theta)}{\theta} + G'[g^*(\theta) + c_e]g^{*\prime}(\theta) = \\
&\text{substituting in first order condition} \\
&= -\frac{L - g^*(\theta)}{\theta^2} - \frac{g^{*\prime}(\theta)}{\theta} + \frac{1}{\theta}g^{*\prime}(\theta) = \\
&= -\frac{L - g^*(\theta)}{\theta^2} < 0
\end{aligned} \tag{24}$$

□

Proof of Proposition 3.

i A higher μ implies that the draw of θ_1 needed for the established incumbent to want to pool can be lower. This is explained thoroughly in the text and in the proof of proposition 2.

ii We take the derivative w.r.t. to ϕ on both sides of Equation 10 and end up with the following condition.

$$\begin{aligned}
W_H^* - W_L^* &\geq \mu[W_H - W_L] + (1 - \mu)[W_H^* - W_L^*] \\
W_H^* - W_L^* &\geq W_H - W_L
\end{aligned} \tag{25}$$

The above holds according to Lemma 1. An increase in the probability that the next state is a boom therefore increases utility from pooling compared to separating.

iii Since private consumption and the primary good are perfect substitutes, more private consumption c_e for the established politician implies that he prefers to redistribute more from the primary good to the secondary good. We insert the optimal policies of the populist and the established politician into the pooling condition of the established politician and take the derivative w.r.t. to c_e to get the following expression.

$$G'(L+c_e)+\frac{1}{\theta_E}\geq$$
$$\geq\frac{1}{\theta_1}+\mu[\phi G'(H+c_e)+(1-\phi)G'(L+c_e)]+(1-\mu)\frac{1}{\theta_E} \quad (26)$$

From the expression above we can only say that the effect of an increase in c_e is ambiguous. It makes separating more beneficial in the first period, and pooling more beneficial in the second period, but we cannot analytically say which effect is larger since it depends on the realization of θ_1, the difference between H and L, and how close the established politician is to preferring a corner solution.

Proof of Proposition 4. Consider $\theta_1 = \theta_E$. If $\mu = 0$, the established politician never pools since $W_H \geq W_H^*$ never holds. If $\mu = 1$, the established politician never pools since $W_H - W_L \geq W_H^* - W_L^*$ never holds according to Lemma 1.

Now consider $\theta_1 > \theta_E$. Let us rewrite the pooling condition as $W_H - W_H^*(\theta_1) \geq \mu(EW - EW^*)$. Let us once again consider $\mu = 0$. In this case, the established politician will never pool, see above. Now consider $\mu = 1$. The sufficient and necessary condition for the existence of a pooling equilibrium in this case is

$$EW^* + (1-\phi)(W_H - W_L) \geq W_H^*(\theta_1) \quad (27)$$

For interior values of $0 < \mu < 1$, θ_1 must be even larger for there to be a pooling equilibrium.

□

Proof of Proposition 5.

i See proof of proposition 3

ii See proof of proposition 3

iii The only difference to the analysis in proposition 3 is that the first period is now a booming state.

$$G'(H+c_e)+\frac{1}{\theta_E}\geq$$
$$\geq\frac{1}{\theta_1}+\mu[\phi G'(H+c_e)+(1-\phi)G'(L+c_e)]+(1-\mu)\frac{1}{\theta_E} \quad (28)$$

Consider that $\theta_1 = \theta_E$, which gives us the following expression

$$G'(H+c_e)\geq\mu[\phi G'(H+c_e)+(1-\phi)G'(L+c_e)]+(1-\mu)\frac{1}{\theta_E} \quad (29)$$

We know that the equation above never holds since $G'(H + c_e) < G'(L + c_e) < \frac{1}{\theta_E}$, which we know from concavity and the first order condition for an interior solution. Therefore an increase in c_e increases incentives to separate if $\theta_1 \leq \theta_E$. If $\theta_1 > \theta_E$, the effect of c_e on the pooling decision is ambiguous.

$\square$