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Stock Ownership and Political Behavior: Evidence from Demutualizations

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A setting in which customer-owned mutual companies converted to publicly listed firms created a plausibly exogenous increase in stock ownership. We use this shock to identify the effect of ownership of publicly listed shares on political behavior. Using instrumental variable regressions, difference-in-differences analyses, and matching methods, we find the shock changed the way people vote in the affected areas, with the demutualizations being followed by a 1.7–2.7-percentage-point increase in right-of-center vote share. Analyses of demutualizations that did not involve public listing of shares suggest that explanations based on wealth, liquidity, and tax-related incentives do not drive the results, and that the ownership of publicly listed shares was instrumental in generating the increase in conservative voting.

Keywords: stock ownership; political behavior; salience; attention; identity
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1. Introduction

A voluminous literature studies what makes individuals own and trade different types of financial instruments (for reviews, see Guiso et al. 2003, Campbell 2006, Curcuru et al. 2010, Barber and Odean 2013). Less studied, yet plausible, is the idea that the asset holdings in turn may influence individuals' behavior. In this paper, we ask whether "finance matters" in this way, in a domain with potentially far-reaching consequences on society. Specifically, we analyze how people's ownership of publicly listed shares affects their voting decisions in parliamentary elections.

The idea that ownership of capital assets can influence individuals' behavior is not new: Madison (1787) proposed that "from the influence of these [different degrees and kinds of property] on the sentiments and views of the respective proprietors, ensues a division of the society into different interests and parties." (Engels 1873, p. 240) proclaimed that "the worker who owns a little house" is "certainly no longer a proletarian." The New York Stock Exchange's campaign to promote the stock market in the 1950s had political underpinnings (Traflet 2003), whereas President Bush's Ownership Society initiative was seen by some as a strategy for crafting an "investor class" (Nadler 2000, Barnes 2004).[1]

Owning publicly listed shares creates a natural incentive to become more informed about the stock market and the economy, which may increase the weight of economic issues in voting decisions. To the extent that ownership also promotes interest in societal issues at large, it could make stockholders more likely to vote in national elections. The shift in attention, and associated changes toward the identity of a shareholder, can also increase sympathy for right-of-center policy by causing people to interpret issues through the lens of an investor.[2] For example, stockholders may learn new aspects of how the corporate

[1] Other government initiatives targeted at increasing ownership are widespread, including programs for privatizing state ownership in 59 countries (Jones et al. 1999). These programs often involve retail investor incentives intended to attract a large stockholder base (Keloharju et al. 2008). Bortolotti et al. (2004) show right-of-center governments are more likely to privatize state-owned companies.

[2] Mullainathan (2002) and Mullainathan et al. (2008) develop models of bounded rationality, salience, and persuasion. Akerlof and Kranton (2000) investigate how identity influences economic decisions.

world operates, pay more attention to financial media, or obtain new insights from their peer investors.

Isolating the influence of stock ownership from other factors is challenging. Even a perfectly randomized setting where some individuals are assigned publicly listed shares would necessarily cause a windfall increase in wealth. The assignment would thus invoke wealth effects predicted by classical models of political economy, where wealthier individuals are more likely to vote in favor of strong property rights and low taxation of returns to capital (DiPasquale and Glaeser 1999, Biais and Perotti 2002, Pagano and Volpin 2005, Perotti and von Thadden 2006).[3]

Our empirical strategy takes advantage of conversions of mutual companies into corporations. The unique features that allow us to identify the effect of owning listed shares are the following. First, the demutualizations did not entail a transfer of property rights—the owners, as well as the underlying assets they owned, stayed the same—merely converting an existing asset into another form. Second, three of the firms converted into publicly listed corporations, whereas two others changed their corporate form without public listing. This setting allows identifying the impact of a plausibly exogenous shock to ownership of publicly listed shares while controlling for other changes associated with demutualizations.

We use detailed data from Finland,[4] where five regional telecommunications firms, covering 38% of the country's population, decoupled telephone services from ownership in the 1990s. Under the mutual form, a prospective customer would acquire a certificate of participation in one of 17 local mutual telecom firms to obtain a landline. In the demutualizations, the customer-owners obtained shares in corporations that continued the operations of the old mutuals. Three of these demutualizations made about 120,000 individuals owners of publicly listed stock for the first time. Our identification strategy uncovers the effect on voting behavior on the extensive margin, i.e., coming from adding a new owner of publicly listed shares to the group of existing stock market participants in each zip code. The two remaining demutualizations, by definition, did not create new stock market participants, and so allow us to rule out alternative explanations.[5]

The ownership data we use are the official records of title and are therefore of very high quality as well as free of biases associated with survey data. We merge the ownership data with similarly reliable election results at the zip code level, as well as a large set of demographic and socioeconomic variables including wealth, income, education, homeownership, age cohort shares, and population density. These rich data also allow us to rule out the possibility that the results are driven by systematic differences between the treatment and control areas, plausible violations of the exclusion restriction, or a weak instrument.

Our main analysis utilizes the publicly listed demutualizations in instrumental variable (IV) regressions in which right-of-center vote share is explained with instrumented share ownership.[6] The instrument, an indicator for a zip code having experienced a demutualization, does not suffer from the weak instrument problem as the demutualizations cause a substantial increase in stock market participation. Shareownership rates in the treatment towns are over 80% higher over the long term (relative to other towns, and conditional on a large set of control variables, as well as zip code and election fixed effects).

The results show that instrumented ownership of publicly listed shares is positively and significantly associated with right-of-center voting. The point estimate suggests that the shock on stock market participation caused an average increase of 1.9 percentage points in the vote share of the main right-of-center party (t-value, clustered at the province level, equals 2.1). Different definitions of the dependent variable, subsamples, and control variables produce similar estimates. The data also allow us to investigate where the new right-of-center votes come from. Our analysis suggests that changes in voter turnout patterns cannot explain the shift to the right and that shifts in party choice are the major driver.

We put together several pieces of evidence that collectively speak against violations of the exclusion restriction—the assumption that the demutualizations did not have an impact on voting through

[3] Bagues and Esteve-Volart (2015) study a random wealth shock provided by the Spanish Christmas lottery. They find that the incumbent politicians receive more votes in areas with lottery winners.

[4] Demutualizations are not exceedingly rare. For example, one third of the U.S. mutual life insurance companies demutualized between 1995 and 2004 (Erhemjamts and Leverty 2010). Unfortunately, detailed ownership data are not available in those cases.

[5] The demutualization decisions of the firms in our sample were related to their expansion into new businesses, such as providing cellular network services. This made them consider a public listing in order to tap the equity market for capital and to use listed equity as an acquisition currency. It would be challenging to control for such factors if we studied the impact of demutualizations on any aspects of firm behavior. For the purpose of analyzing stock ownership's impact on political behavior, however, the resulting changes in salience of ownership resemble randomized, and plausibly exogenous, shocks from the viewpoint of the individuals affected. See §3 for a detailed discussion.

[6] In defining right-of-center voting we make use of a 1–10 left-right scale of Finnish political parties by Hix and Lord (1997). In the main analysis the left-hand side variable is defined as the vote share of the main right-wing party, National Coalition (7.4 on the left-right scale). As an example of a generally stock market friendly policy, the party's 2003 election manifesto included a pledge on low dividend taxes. See §2 for more details.

any channels other than stock market participation. We first note that our fixed-effects panel regressions control for many observable characteristics and any time-invariant unobservables. In addition, we employ difference-in-differences regressions and matching estimators using the same set of observable covariates and additionally including the pretreatment right-of-center vote level and vote trend as matching variables. This analysis compares demutualizing towns to areas that did not demutualize but had similar characteristics, as well as similar pretreatment voting levels and trends. These analyses do little to change the conjecture that the demutualization events were plausibly exogenous conditional on observables. To address any remaining omitted variable concerns, we follow Altonji et al. (2005) in determining the minimum bounds for the degree of selection on unobservables required to generate our results. We find that selection on unobservables should have been two to four times greater than selection on observables to explain the patterns in the data.

The two nonlisted demutualizations allow a direct test of the conjecture that ownership in publicly listed shares is instrumental in generating the increase in conservative voting. Recall that the process of demutualization does not entail a transfer of property rights—the owners, as well as the assets they own, stay the same. Nevertheless, the demutualizations might have changed wealth because of the benefits or disadvantages of mutuality and the increases in liquidity and tax burden following the decoupling of ownership from phone services. These features are identical in the listed and nonlisted demutualizations. When we analyze the voting patterns following the nonlisted demutualizations, we find no changes in right-of-center voting in the affected areas. These results rule out wealth, liquidity, and taxes as the driving mechanism. They also are reassuring regarding the validity of the exclusion restriction: the conversions of local telecom firms do not seem to be driven by unobservable determinants of right-of-center voting. In sum, increased participation in the public stock market appears to be the key factor.

Why does ownership of publicly listed shares matter? One narrative is that ownership of publicly listed shares directs the newly minted shareholders' attention toward new information sources, for example, financial media, which in turn increases sympathy for right-of-center policy. The ideal test of this hypothesis would measure the use of financial media at the zip code level and relate it to the demutualizations. Because such data do not exist, we turn to a nationally representative survey that records detailed information on stock ownership, the use of financial media, and political views. We find strong positive associations between ownership of publicly listed shares and the use of financial media, controlling for age, gender, education, wealth, income, and other potential factors. An equally strong positive association obtains between the use of financial media and right-of-center voting. Although these patterns should not be taken as conclusive evidence of causality exactly due to the reasons that motivate our main identification strategy, they do give credence to the argument that shifts in attention, particularly toward financial media, may drive the relation between ownership and voting.

Our paper speaks to the literature on the origins of political behavior. Political views and other fundamental beliefs have been shown to be influenced by genetic and other prebirth factors (Alford et al. 2005, Hatemi et al. 2010), early life experiences (Giuliano and Spilimbergo 2014), and exposure to a given political environment (Alesina and Fuchs-Schündeln 2007). But they also change as a result of more transient influences, such as wealth shocks, changes in property rights, and economic conditions (Di Tella et al. 2007, Brunner et al. 2011, Bagues and Esteve-Volart 2015), becoming a father to a female child (Washington 2008, Oswald and Powdthavee 2010), previous acts of voting (Mullainathan and Washington 2009), and media exposure (DellaVigna and Kaplan 2007, Gerber et al. 2009, Enikolopov et al. 2011). That individual's political views can change over time is also the main premise of a large related literature in political science that studies how the electorate can be affected by the media, various interest groups, and politicians themselves. Political scientists refer to this mechanism as "agenda setting" (see Weaver et al. 2004 for a review of the academic literature and Nickerson and Rogers 2014 for recent applications in campaign analytics). Similar to these studies, we are concerned about those voters who do in fact change their minds. Our paper is the first to analyze the role ownership of publicly listed shares plays in shaping political views, and, more generally, one of the few papers that identify a causal link between voting and different facets of wealth.

Our paper also relates to work on salience and identity in economic decisions. Priming of a particular identity (such as gender, religion, ethnicity, or race) has a strong effect on decisions in many domains, including risk taking, cooperative behavior, time preferences, and voting (Chen and Li 2009, Benjamin et al. 2010, Klor and Shayo 2010). Our study provides field evidence on the impact of the salience of stock ownership on the way people vote in elections.

The outline of the remainder of the paper is as follows. Section 2 introduces data sources and discusses the institutional details of the demutualizations. Section 3 develops the identification strategy. Section 4 presents the main results from panel regressions, and

§5 includes extensions and robustness checks. Section 6 discusses mechanisms and alternative explanations, and §7 concludes.

2. Data and Institutions

Our data set includes comprehensive information aggregated at the zip code level on (a) results in Finnish parliamentary elections, (b) ownership of shares in the area, and (c) a large number of demographic and socioeconomic characteristics. The number of zip codes is about 2,500, and we have data from the 1991, 1995, 1999, and 2003 elections.

2.1. Elections

Elections for the 200-seat Finnish Parliament are held every four years. No other elections that could introduce biases, such as referenda, take place on the election day. Finland uses the d'Hondt constituency list system, typical of multiparty proportional representation systems. The number of parliamentary seats each party receives is proportionally determined by party-level vote totals in each of 15 regional constituencies. For further details of the electoral process and results, see Nurmi and Nurmi (2004).

This multiparty voting system presents advantages for our analysis in comparison with a two-party, district-based majority voting system. First, as results count toward total party representation, voting behavior is less likely to be biased by strategic voting for a non-first-choice candidate seen as more electorally viable. Second, as each constituency offers a choice between hundreds of candidates, party-level election results contain less candidate-specific noise generated by variation in the vote-getting ability of individual candidates (Levitt 1994), such as their personalities and views on specific issues. Third, parties that get more votes overall are more likely to make it to the coalition government and have power to make their preferred policy. This suggests that it makes little sense for an individual to vote for a candidate whose views on issues important to the individual contradict with the party position. Taken together, these factors mean that we can more cleanly link party politics and voting choice, allowing for a general inference on our research question: does stock ownership marginally tilt political preferences toward the right?

Our measurement of political preferences follows a long tradition of studies that use the left-right scale as a representation of the political space (see Lo et al. 2014 for a review). We make use of the 1–10 left-right scale of Finnish political parties by Hix and Lord (1997) and calculate right-of-center voting as the vote share of the main right-wing party, National Coalition (7.4 on the left-right scale). This party is often seen as stock market friendly. For example, the party's 2003 election manifesto included a pledge on low dividend taxes. As of 2014, this party opposes a proposed European capital markets transaction tax, whereas the left-of-center parties in Finland are in favor of it. In a comparison of 47 countries, Benoit and Laver (2007) report that the National Coalition is considered a typical right-of-center party by political experts, and textual analysis of party positions puts it approximately halfway between Democrats and Republicans in the United States. According to expert views, no other party in Finland leans more to the right than the National Coalition.

The election data come from Statistics Finland, the official governmental statistics agency. We obtain the number and distribution of ballots cast on election day for each voting precinct. The data also include the number of voting-aged (18 on or before the election date) inhabitants in each voting precinct as of January 1 of the election year.

The voting data for 1991 through 1999 include the votes cast on election day (a Sunday). The voters also have the alternative to vote through a postal ballot system before the election. The 2003 postal votes are allocated to the precinct level, which allows us to work with the regional distribution of all votes. For 1991 through 1999, we are able to analyze only the election-day votes. However, the correlation between party vote shares in the postal votes and election-day votes is very high.

There is no one-to-one correspondence between voting precincts and zip codes, but the vast majority of the population in a voting precinct usually resides in a single zip code. When a voting precinct stretches over adjacent zip codes we prorate the election results in the ratio of voting aged population at the intersection of the precinct and those zip codes. Kaustia and Torstila (2011) provide further details of the procedure.

2.2. Share Ownership

The Finnish Central Securities Depository (FCSD) maintains records of stock directly held by all individuals in practically all listed companies in Finland. Our ownership data cover the period from January 1995 to November 2002. For a detailed description of the FCSD data, see, for example, Grinblatt and Keloharju (2000).

Based on these data, we calculate the number of stock market investors in each zip code at the time of the parliamentary elections. During the study period, elections took place in 1995 and 1999. The 2003 elections took place four months after the coverage of our stock ownership data ends. The ownership data from November 2002 serve as a proxy for the ownership at the time of the elections in March 2003.

In determining the local number of stock market participants, we count all those individuals who have common stock in their account at the time of the election, and divide that number by the total number of inhabitants in the zip code. Although the reason for this definition is ultimately imposed by data considerations, we note the direct share holdings are by far the most important means of participating in the stock market in Finland. During the sample period, no personal retirement accounts existed in Finland and mutual funds were a relatively new phenomenon. Kaustia and Knüpfer (2012) provide a more detailed discussion of these issues.

2.3. Demographic and Socioeconomic Controls

Statistics Finland provides a number of demographic and socioeconomic zip code level variables for the years 1991 through 2003. These data originate from registers maintained by the government and are practically free from the usual measurement error problems present in survey-based data sets (such as the U.S. Census). For 1991, we only have the total number of inhabitants by zip code. For 1995 through 2003, we have the proportion of people in various age cohorts, median income, mean wealth, level of education, unemployment rate, proportion of owner occupied dwellings, population density, and proportion of females.

We have 2,753 total zip codes before excluding 22 located in the autonomous island province of Åland, where the major parties do not set candidates. We further eliminate 121 zip codes with undefined geographical boundaries, boundary changes between the elections, or no information on the number of inhabitants available. After merging these zip codes with the ownership data, we have 2,610 zip codes. In the regressions, we further require that each of the three major parties have received at least one vote, that the zip code has all the control variables available in all the years in the sample, and that the dependent variable does not fall outside its 1st and 99th percentiles (the results are robust to relaxing this last requirement). This procedure gives a final sample size of 2,143 zip codes each year.

2.4. Demutualizations

Telecom services in Finland are provided in a decentralized manner by a number of local telecom firms, as well as one partially government-owned company. In the early 1990s, none of these firms were publicly listed.

To obtain a landline telephone, a prospective customer would acquire a certificate of participation in the local mutual company. Although 82% of households had a landline telephone as of the end of 1997, the proportion of certificate owners in the population was lower, because most households had only one certificate and some individuals rented a certificate. The owners of the certificates received a discount on their phone bill. The mutuals issued the certificates and acted as market makers when an individual needed to sell the certificate because of, for example, relocating outside the service area. The certificates also were transferable, so an individual could sell one to another. One could commonly find certificates in estate auctions.

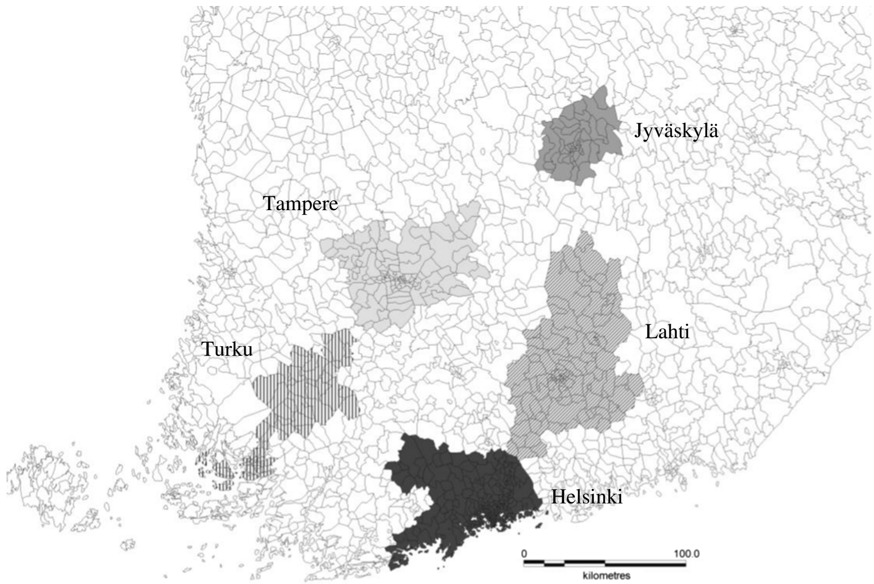
In the late 1990s, some of the local telecom companies had expanded into new businesses, such as providing wireless services. Expansion made them consider a public listing in order to tap the equity market for capital and to use listed equity as an acquisition currency. Three firms, which operated in areas that cover about 350 zip codes, chose to convert into publicly listed firms. At the same time, two firms, covering about 160 zip codes, decided to decouple the provision of phone services from ownership without the public listing.[7]

Figure 1 shows a map that indicates the main treatment areas (Helsinki, Tampere, and Jyväskylä), along with the two areas (Lahti and Turku) where the nonlisted demutualizations took place. The main features of these conversions are as follows:

- The listed demutualizations involved the firms Tampereen Puhelin (TP), Keski-Suomen Puhelin (KSP), and Helsingin Puhelin (HP). The shares were listed on March 17, 1998, September 28, 1998, and July 1, 1999, and the areas had 0.3, 0.1, and 1.1 million inhabitants in 1995, respectively.
- The nonlisted demutualizations were Loimaan Seudun Puhelin (LSP) and Päijät-Hämeen Puhelin (PHP). These firms decoupled ownership from phone services on May 31, 1999 and on April 7, 1999, respectively. Each area had 0.2 million inhabitants in 1995.
- The 1999 parliamentary elections were held on March 21, so two of the listed conversions took place six and 12 months before the 1999 election day, whereas all the other conversions happened after the 1999 election.
- The number of shares obtained with one mutual certificate was fixed and did not depend on customer type or telephone usage. Some individuals and companies had several certificates.
- The market values of shares obtained from the conversion of one certificate were 3,088 euros, 2,145 euros, and 262 euros for HP, TP, and KSP, respectively, at the close of the listing day. The certificates in the nonlisted conversions of LSP and PHP were valued at more than 1,000 euros in the active grey market following the decoupling. The differences in the values reflect

[7] To gain understanding of the institutional background of the demutualizations, we interviewed all five CEOs responsible. They confirmed that the most important driver of the demutualization decision was the need to raise capital to finance investments in new technologies. The two nonlisting CEOs cited the formation of a strategic partnership with a nationwide operator and the manageability of the shareholder base in the private market as reasons for ultimately not listing the shares.

Figure 1 Publicly Listed and Nonlisted Demutualizations



Notes. The map shows southern and central Finland by zip code. Helsinki, Tampere, and Jyväskylä areas demutualized their local telecom firms and listed the shares on the public stock market. Lahti and Turku areas decoupled the phone services from ownership, but did not publicly list the shares.

differences in both the market values of the firms and the number of certificate holders that divide the market value. Any sale of the certificate or a subsequent sale of the stock was subject to capital gains taxation at a rate of 25% until 2000 after which the rate rose to 29% up until 2006.

• The vast majority of the recipients did not sell the shares. Selling rate estimates imply that about 75%–85% of the recipients of publicly listed shares still held the stock after three years of the demutualizations.

3. How the Demutualizations Solve the Identification Problem

Our identification strategy relies on the idea that the demutualizations generated ownership changes that in many respects resemble randomized, and exogenous, shocks from the viewpoint of the individuals receiving the shares. Individuals in the demutualizing towns serve as the treatment group, whereas all other individuals belong to the control group. The benefit of this research design is that it can, under certain assumptions, address the causality of ownership on voting behavior. Below we present our identification strategy, and then address potential endogeneity concerns.

3.1. Regressions

Before presenting the models we use, it is instructive to consider a simple panel regression that attempts to document the influence of share ownership on voting behavior. The model explains the right-of-center vote share with share ownership:

$$c_{it} = \alpha p_{it} + e_{it}, \qquad (1)$$

where i and t index zip codes and elections, c_{it} is the right-of-center vote share, p_{it} is the share-ownership rate, and e_{it} is the error term (the intercept is omitted from the formulas but is included in all the models). Unobserved heterogeneity makes the estimation of α challenging. The omission of observable and unobservable characteristics that jointly determine ownership and voting behavior is likely to bias the estimate in a way that is not known a priori.

The first step to control for omitted variables is to include all the relevant observable variables and control for zip code and election fixed effects. Such a model is

$$c_{it} = \alpha p_{it} + \beta \mathbf{X}_{it} + u_i + v_t + e_{it}, \qquad (2)$$

where c_{it} is the right-of-center vote share, p_{it} is the share-ownership rate, $\mathbf{X}_{it}$ is a vector of observable characteristics, u_i and v_t are zip code and election fixed effects, and e_{it} is the error term. Natural candidates for observables $\mathbf{X}_{it}$ are wealth, income, education, home ownership, age composition, and population density (see Guiso et al. 2003 and Curcuru et al. 2010 for reviews on the determinants of stock market participation). Zip code and election fixed effects control for time-invariant zip code and election characteristics.

Although model (2) is a significant improvement over model (1), its interpretation is still hampered by time-varying shocks that *jointly* influence ownership and voting. Here we take advantage of the ownership shocks generated by the demutualizations. We estimate the following IV regression:

$$p_{it} = \delta \mathbf{X}_{it} + \pi I_{it} + u_i + v_t + e_{it}, \qquad (3)$$

$$c_{it} = \alpha \hat{p}_{it} + \beta \mathbf{X}_{it} + \gamma I_{it} + g_i + h_t + f_{it}. \qquad (4)$$

The first-stage regression explains share ownership by observables $\mathbf{X}_{it}$ and an indicator I_{it} taking the value of one if a zip code i has experienced a demutualization in time t and zero otherwise, and zip code and election fixed effects. The second-stage model explains the right-of-center vote share with the instrumented ownership rate from regression (3) and it includes the same set of observables, the zip code and election fixed effects g_i and h_t, and an error term f_{it}.

The inclusion of zip code fixed effects identifies the impact on voting from ownership *changes* that were caused by the demutualizations. The first-stage regression isolates changes in ownership in the demutualization areas from changes that took place simultaneously in other areas. The second-stage regression then estimates how much right-of-center voting changed per a unit increase in the rate of share ownership. Thus, the model allows us to estimate the change in voting that follows from adding new stock market participants to a zip code.

The exclusion restriction of the model is the assumption that demutualizations have no direct effect on voting in addition to their impact on share ownership, conditional on the control variables. In effect, we restrict the coefficient $\gamma = 0$ to capture this assumption. If the exclusion restriction holds, the IV regressions successfully identify the influence of share ownership on voting behavior.

3.2. Potential Endogeneity Concerns and Alternative Methods

The identification rests on the assumption that the demutualization treatment did not coincide with any other change in determinants of the right-of-center vote share, conditional on the control variables of the model. The controls we include capture differences between the treatment and control areas in many plausible determinants of voting, including population density, wealth, income, education, and age composition. The variables also control for the economic motives to carry out a demutualization in particular types of areas. Therefore, the regression effectively compares a treatment zip code to a control zip code that is similar to the treatment area along observable characteristics. We believe this identification strategy makes significant progress in aiding a causal interpretation.

In addition to making sure the conclusions are not confounded by zip code characteristics, we also take into account the possibility that the outcome itself, i.e., right-of-center voting, might have changed in response to aspects other than stock ownership. We do this by using methods that make it possible to include the predemutualization vote trends and vote levels in the treatment and control areas as additional control variables. Specifically, we collapse the data into differences between the elections and run difference-in-differences regressions. We also implement a covariate matching analysis that compares treatment zip codes to nondemutualizing areas that are similar along observable characteristics, including the pretreatment vote share level and trend. These analyses are a good remedy against three remaining endogeneity concerns.

First, if the demutualizations took place in areas that were independently becoming more right-of-center, the estimate of the impact of the demutualization would be upward biased. Relatedly, the underlying political sentiment might have made it easier to carry out a demutualization in an area with a greater right-of-center voter base.

Second, decisions to demutualize may have been influenced by politicians observing that the practice is becoming favorable with voters in certain areas. More than a decade after the events, however, no evidence or even speculation regarding political motives to demutualize has emerged, suggesting the political effects we document are largely an unintended consequence.[8] One might still wonder if politicians influenced the demutualization decisions. The formal decision to demutualize was made by the company general meeting that had no political motives and the demutualization did not need government approval. Nevertheless, political influence would not violate the exclusion restriction as long as the effect on voting came through increased share ownership. But if right-of-center politicians perceive an underlying favorable political trend in an area, and attempt to boost this further by increasing share ownership, then the estimates would overstate the effect of share ownership. Controlling for political trends, as described above, alleviates this concern. Institutional details also make us think this mechanism is not plausible. Connections between firms and politicians are weak in Finland. Faccio (2006) documents that in the end of 1990s, firms with political connections represented 0.14% of market capitalization in Finland, compared to 7.7% around the world. Transparency International consistently ranks Finland as the least (or second least) corrupt country in the world.

Third, areas may differ in terms of their sensitivity to national patterns in the vote.[9] Since the

[8] We have verified this by searching the article databases of two major newspapers (Taloussanomat and Kauppalehti) using different expressions referring to the main right-wing party (National Coalition) together with any of the names of the demutualized companies during the 1995–2014 period. The nine articles resulting from this search do not suggest any differences in the acceptability of demutualizations along the party lines.

[9] As an example, political analyst Nate Silver refers to this tendency as elasticity and provides values for state elasticities in U.S. presidential elections (Silver 2013).

demutualizations coincide with a national increase in the right-wing vote, our estimates may be upward biased if the treatment areas have a propensity to produce larger electoral swings than the control areas.

Finally, it may be possible that provision of telecom services was influenced by the demutualizations, which in turn had an impact on access to information in the affected areas. Little evidence supports the premise that telecom access was affected by demutualizations. Landlines were available in the whole country and two digital cellular networks with nationwide coverage had been built in the early 1990s. Thus, cellular services were available to practically all inhabitants regardless of where they lived. Furthermore, there is no obvious reason to expect that any increased provision of telecom services would promote right-of-center voting in particular.

Taken together, all of the identification concerns are alleviated by the difference-in-differences regressions and covariate matching methods, as they use the right-of-center vote share in the 1995 election as well as the 1991–1995 vote trend as control variables. These analyses thus identify the treatment effect from control areas that have the same predemutualization vote level, the same predemutualization vote trend, and the same control variables as the treatment areas.

4. Results from Panel Regressions

4.1. Descriptive Statistics

Table 1 reports descriptive statistics of the treatment and control areas pooling data over the three elections in the sample period (1995, 1999, and 2003). Panels A and B document differences between the treatment and control towns along many dimensions. The residents in treatment towns have higher income, better education, and the areas are more densely populated. For example, the median annual income in the treatment towns equals 16,360 euros, whereas it is only 12,030 euros in the control towns. Panel C reports logit regressions that explain an indicator for a zip code experiencing a demutualization with zip code characteristics measured in 1995, prior to any demutualizations had taken place. The logit marginal effects confirm many of the univariate results: wealth, income, home ownership, and population density are significantly correlated with demutualizations (but education, unemployment, and the share of female inhabitants are not). There is also no relation between share ownership in 1995 and the propensity to demutualize. The change in right-of-center vote share from 1991 to 1995 is systematically different in treatment and control areas, which indicates that the right-of-center party lost votes between the 1991 and 1995 elections in

Table 1 Descriptive Statistics

	Mean	SD	Min	p25	Median	p75	Max
			Panel A: Treated towns (n of zip codes = 340)				
Right-of-center vote share	21.23	7.14	1.79	15.89	21.09	26.70	39.55
Left-of-center vote share	34.64	9.74	8.05	28.22	34.69	41.14	67.86
Center vote share	15.53	12.85	1.79	6.39	10.07	21.37	63.27
Largest parties vote share	88.24	3.45	67.01	85.99	88.40	90.72	98.29
Voter turnout	44.66	6.68	23.80	40.58	44.99	49.12	66.47
Share-ownership rate	18.56	9.00	0.00	11.32	18.04	25.15	47.43
Wealth	52.48	33.23	8.93	35.31	45.71	60.13	412.96
Income	16.36	3.66	7.04	13.62	16.25	19.19	26.39
College degrees	9.47	5.93	0.34	5.22	8.34	12.24	33.32
Unemployment	6.63	3.16	1.05	4.04	6.31	8.77	18.03
Home ownership	27.89	6.33	0.27	25.08	29.01	32.13	44.58
Population density	1.30	2.26	0.00	0.03	0.35	1.67	23.50
Females	50.89	2.93	39.02	49.03	50.64	52.33	62.00
			Panel B: Control towns (n of zip codes = 1,803)				
Right-of-center vote share	12.99	8.55	0.23	5.88	11.36	18.92	39.56
Left-of-center vote share	29.89	14.84	0.00	18.71	29.18	39.81	89.21
Center vote share	40.40	21.54	0.00	23.02	41.94	57.14	92.81
Largest parties vote share	90.51	6.05	37.91	88.23	91.60	94.27	103.57
Voter turnout	39.16	9.79	1.08	32.47	38.86	45.19	155.80
Share-ownership rate	7.77	4.07	0.00	4.97	7.22	9.89	36.28
Wealth	44.30	16.52	10.41	32.32	41.07	52.60	133.13
Income	12.03	2.96	6.27	9.76	11.62	13.96	21.35
College degrees	3.97	2.80	0.16	2.05	3.36	5.14	23.67
Unemployment	8.24	3.33	0.53	5.88	7.85	10.17	40.00
Home ownership	32.04	5.00	4.04	29.16	32.29	35.12	63.17
Population density	0.15	0.47	0.00	0.00	0.01	0.04	5.95
Females	48.86	2.80	35.96	47.08	48.99	50.62	60.80

Table 1 (Continued)

Panel C: Regressions of demutualization indicator		
Dependent variable	*Demutualization indicator*	
Reporting	Logit marginal effects	
Specification	1	2
Wealth	0.198	0.171
	(3.64)	(3.94)
Income	0.160	0.185
	(1.43)	(2.04)
College degrees	1.362	1.310
	(1.38)	(1.26)
Unemployment	0.154	0.388
	(0.21)	(0.63)
Home ownership	−0.659	−0.445
	(−1.99)	(−1.79)
Population density	0.020	0.012
	(2.62)	(2.25)
Females	−0.203	−0.183
	(−0.49)	(−0.44)
Lagged change in right-of-center vote share		−1.017
		(−3.58)
Lagged level of right-of-center vote share		−0.031
		(−0.12)
Lagged share ownership		0.230
		(0.58)
Age controls	Yes	Yes
Pseudo-R^2	0.386	0.430
Number of observations	2,143	2,143

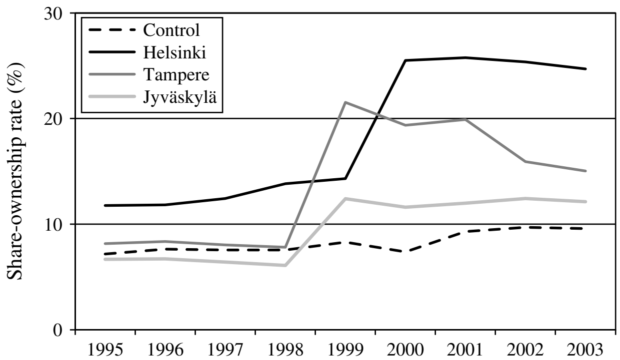
Notes. The data include vote shares, share-ownership rates, and control variables at the zip code level. Statistics are calculated over all the parliamentary elections in 1995, 1999, and 2003. The right-of-center, left-of-center, and center vote shares are defined as the share of votes cast for the National Coalition Party, the Social Democrat Party plus the Left Coalition Party, and the Center Party, respectively. In addition to these four parties, the largest parties' vote share includes the Green League and the Swedish People's Party. Voter turnout is the number of voters divided by the voting-aged (over 18 years) population. Share ownership is the number of shareholders divided by the number of inhabitants. Treated towns are municipalities that belong to the operating areas of the three demutualized telecom companies. Wealth and income are calculated as averages across households in a zip code and are measured in thousand euros. College degrees equals the share of inhabitants with a college degree. Unemployment is the share of inhabitants who are unemployed at the end of the year. Home owners is the share of owner-occupied dwellings in the area. Population density is the number of inhabitants divided by the area of the zip code and is measured in thousand inhabitants per km^2. Females is the share of the female population. Panel C reports regressions of the demutualization indicator against zip code characteristics where the unit of observation is a zip code in the year 1995 (prior to any demutualizations). The two columns report logit marginal effects evaluated at the means of all the explanatory variables. The lagged level is for 1991 and the lagged change is the 1991–1995 difference. The *t*-statistics in parentheses are adjusted for heteroskedasticity and clustering at the province level.

the demutualizing towns. Our regressions and matching estimators will control for all of these differences.

Our main variable of interest, the vote share of the main right-of-center party, equals 21.2% and 13.0% for the treatment and control towns, respectively. The six largest parties account for 88.2% and 90.5% of the votes cast in these areas. Share-ownership rates equal 18.6% and 7.8% of the population. Because these numbers are averaged over the three elections, they include any impact from the demutualizations.

Figure 2 plots the share-ownership rates in treatment and control areas in each year of the sample period. Before the conversion, only one of the treatment areas (Helsinki metropolitan and surrounding areas) has a higher level of share ownership than the control towns. It is clearly visible that the largest increase in this area, from 14.3% to 25.5%, coincides with the demutualization. In the other two areas, whose demutualizations took place in 1998, share ownership jumps between 1998 and 1999. For the Tampere area, share ownership goes from 7.8% to 21.5%, whereas the Jyväskylä area shows an increase from 6.1% to 10.4%. Averaged across the treatment areas, the share ownership rate is 9.4 percentage points higher in 2000 than what it was in 1997. In the control areas, the 1997–2000 change equals −0.2 percentage points with no sizeable jumps in any of the years in the sample period.

Figure 2 Ownership of Publicly Listed Shares Around Publicly Listed Demutualizations



Notes. This figure plots the proportion of share owners in March of each year in the sample. November 2002, the end point of the share ownership data, is used in lieu of March 2003. The rates are plotted separately for the three areas in which the mutually owned local telecom company was converted into a publicly listed firm (Helsinki, Tampere, and Jyväskylä areas) and for all the other areas. The Helsinki-area firm was demutualized in 1999, whereas the firms based in the Tampere and Jyväskylä areas converted in 1998.

4.2. First-Stage Estimates

The success of our identification strategy depends on the validity of the first-stage regression. A strong instrument should produce a large, significant, and persistent positive impact on share ownership in the treatment towns, controlling for simultaneous changes in town characteristics. Panel A in Table 2 presents results of the first-stage regressions. In these regressions, we collapse the data into two periods, 1995 and 2003, and estimate the impact of the demutualization as the change in the share-ownership rate from 1995 to 2003 between the treatment and control towns. The reasons for dropping the year 1999 from the baseline analysis are twofold. First, some of our control variables are off sync from the timing of the 1999 elections, because they are only available at the end of that year. Second, and more importantly, the first two demutualizations took place six and 12 months prior to the March 1999 elections, generating little time for the demutualizations to have an impact on voting. It likely takes time for the mechanism we believe is responsible for the effects to change political preferences (salience and its associated effects on attention).[10]

The main independent variable in the regression is the demutualization dummy that equals one for the 2003 election in all the towns in which the demutualization took place and zero for all the other observations. We include town and year fixed effects that control for confounding time-invariant influences.

[10] Consistent with this conjecture, right-of-center vote regressions that only include the first two demutualizations return small and insignificant effects for the 1995 and 1999 elections, but positive and highly significant effects for the 1995 and 2003 elections.

Table 2 Baseline Regressions

Panel A: First stage					
Dependent variable	*Share ownership*				
Specification	1	2	3	4	5
Demutualization dummy	0.080	0.084	0.086	0.076	0.077
	[7.83]	[8.43]	[8.45]	[9.08]	[9.39]
	(3.85)	(4.03)	(4.06)	(4.75)	(4.87)
F-statistic for instrument	20.5	23.7	23.8	34.9	37.2
Adjusted R^2	0.843	0.852	0.858	0.863	0.866
Number of observations	4,286	4,286	4,286	4,286	4,286

Panel B: IV					
Dependent variable	*Right-of-center vote share*				
Specification	1	2	3	4	5
Share ownership	0.337	0.292	0.283	0.256	0.249
	[3.64]	[3.26]	[3.27]	[2.64]	[2.59]
	(4.55)	(2.77)	(2.73)	(2.07)	(2.06)
Adjusted R^2	0.836	0.841	0.842	0.847	0.849
Number of observations	4,286	4,286	4,286	4,286	4,286

Panel C: OLS					
Dependent variable	*Right-of-center vote share*				
Specification	1	2	3	4	5
Share ownership	0.211	0.173	0.176	0.116	0.114
	[3.82]	[3.01]	[3.04]	[2.15]	[2.11]
	(4.13)	(2.75)	(2.77)	(1.61)	(1.56)
Adjusted R^2	0.665	0.672	0.672	0.679	0.680
Number of observations	4,286	4,286	4,286	4,286	4,286

Notes. The regressions in this table are based on a panel of zip codes in the years 1995 and 2003. Panel A reports the first-stage regressions where the dependent variable is the number of shareholders divided by the number of inhabitants in a zip code. The demutualization dummy takes the value of one if the town has fully undergone the conversion of its local telecom firm to a publicly listed company at the time of the election. Panel B reports the second stage of the IV regressions that explains the vote share of the right-of-center National Coalition party. Panel C reports the OLS regressions of the right-of-center vote share on the share ownership rate. Control variables in column 1 are logged wealth, logged income, college degrees, unemployment, home ownership, logged population density, and females (as defined in Table 1). Columns 2–5 also include age controls that calculate the share of inhabitants in 10-year intervals. Columns 4 and 5 break down the explanatory variables from columns 2 and 3, respectively, into 10 decile dummies (one omitted). All the regressions also control for zip code and election fixed effects. The *F*-statistic is for the null that the instrument does not add to the first-stage model. The *t*-statistics in brackets and parentheses are adjusted for heteroskedasticity and clustering at the town and province level, respectively. The 2,143 zip codes aggregate into 411 towns and 81 provinces.

We consider five variants of the model that include different sets of the observables $\mathbf{X}_{it}$. The simplest version is one with only the demutualization dummy and zip code and election fixed effects, appearing in the first column of panel A in Table 2. The next two columns add two different sets of continuous control variables: wealth, income, education, and unemployment, and these variables supplemented with home ownership, population density, and proportion of females. The two remaining columns allow for nonlinear relations by breaking down each control variable into 10 decile dummies (excluding one for each variable). The four rightmost columns also include eight unreported age-profile variables that calculate the share of inhabitants in cohorts of 10-year intervals. Variables other than ratios are log transformed to reduce the influence of skewness. The table reports two sets of *t*-values: the brackets indicate robustness to clustering at the town level, whereas the parentheses assume clustering at the province level. Allowing for clustering in these ways addresses the possibility of inflated standard errors due to spatial clustering of the treatment areas and having several observations per zip code (see Bertrand et al. 2004). There are 411 towns and 81 provinces in the sample.

The bare-bones model in the column 1 of Table 2 produces a positive and highly significant coefficient on the demutualization dummy, which suggests the treatment areas showed a much larger increase in the share-ownership rate than the control areas. The coefficient value of 0.080 can be directly read as the differential increase in the share-ownership rate in the towns that experienced a demutualization. The *t*-value clustered at the town level equals 7.8, whereas province-clustering produces a much lower *t*-value of 3.9. These differences are explained by the spatial clustering of the treatment in neighboring zip codes, against which the province clustering is a powerful remedy.

The unconditional rate of share ownership in the sample equals 9.5%, so the 8.0% treatment effect implies a 84.4% relative increase. The richer models in columns 2–5 of Table 2 suggest effects of similar size: the relative magnitudes range from 80.6% to 90.7%. The province-clustered *t*-values are equally large in all specifications, ranging from 3.9 to 4.9. The explanatory power of the demutualization dummy is impressive: the *F*-statistic ranges from 20.5 to 37.2. These numbers strongly indicate that the null of having a weak instrument is rejected.

4.3. Second-Stage Estimates

The previous section showed that demutualizations generate a sizeable and statistically significant increase in share ownership. In this section, we implement the IV approach developed in §3 by running two-stage regressions in which the first-stage regressions come from panel A of Table 2.

Panel B reports the results of the second-stage regressions where we follow the setup in panel A in which each specification adds either new control variables or allows for more flexible functional forms. The richest specification appears in column 5 of Table 2, which includes all the control variables broken down into decile dummies, as well as variables measuring the share of each age cohort, and zip code and election fixed effects. The inclusion of fixed effects explains the models' high explanatory power, ranging from 83.6% to 84.9%.

The coefficient estimate for the instrumented share ownership starts with the value of 0.34 in column 1 of Table 2 (province-clustered *t*-value equals 4.6) and ends up taking the value of 0.25 in the fullest specification (*t*-value equals 2.1). These estimates suggest a 10-percentage-point increase in the share-ownership rate increases the right-of-center vote share by 2.5–3.4 percentage points. The economic significance of the effect is meaningful. The average vote share for the main right-of-center party equaled 13.6% in the 1995 election. The treatment predicts (panel A, specification 5) an increase of 0.077 in stock market participation. Applying this number to the low-end-estimate of the coefficient in the second stage (from panel B, specification 5) predicts a change of $0.077 \times 0.249 = 0.019$, that is, a 1.9-percentage-point increase in the right-of-center vote.

The 1.9-percentage-point increase in the right-of-center vote share appears to be a sizeable effect, as elections are always won at the margin. One point of comparison is the result in DellaVigna and Kaplan (2007), who study the effect of the introduction of Fox News on Republican voting, and find an effect of 0.4–0.7 percentage points on U.S. presidential vote shares between 1996 and 2000.

Panel C shows the results of ordinary least squares (OLS) regressions that do not use the instrument for share ownership. Across the specifications, they produce estimates that are smaller than the IV results. The full OLS specification yields a coefficient of 0.11 (*t*-value equals 1.6), whereas the same IV model produces an estimate of 0.25 (*t*-value equals 2.1). One explanation for the difference is that unobservables bias the OLS estimates downward.

The estimates imply a considerable increase in right-of-center voting following a plausibly exogenous increase in share ownership. This conclusion follows from showing our instrument has enough power to potentially have a meaningful impact on voting and that the results are robust to many different specifications—even to the strictest specification that allows for a nonlinear relation between control variables and vote shares.

5. Alternative Methods, Dependent Variables, and Samples

This section discusses alternative methods we use to analyze the robustness of the baseline results in the previous section. We then analyze alternative dependent variables and subsamples.

5.1. Alternative Methods

We use difference-in-differences and matching methods to compare the demutualizing towns to control areas that have similar characteristics and predemutualization vote patterns. The inclusion of the vote patterns—the pretreatment level and trend of the right-of-center vote share—adds an important additional layer of controls vis-à-vis the IV regressions. Specifically, the pretreatment vote variables address the potential violations of the exclusion restriction that were outlined in §3.2.

We first collapse the data into differences and estimate OLS regressions where the dependent variable is the change in the right-of-center vote share from 1995 to 2003 and the independent variable is the demutualization dummy. The controls are based on changes in the socioeconomic characteristics used in the paper thus far. Importantly, we add the pretreatment level and trend in right-of-center voting. The vote level is the right-of-center vote share in 1995 and the trend is the difference in right-of-center vote shares between the 1991 and 1995 elections.

Panel A in Table 3 shows the difference-in-differences results. The first specification is a bivariate regression where the only independent variable is the demutualization dummy and the dependent variable is the 1995–2003 change in right-of-center voting. The coefficient estimate suggests that right-of-center voting increased by 2.7 percentage points in the demutualizing towns compared to control towns. This effect is reduced to 1.7 percentage points in the full specification that controls for the pretreatment vote trend and vote level and a large number of socioeconomic and demographic characteristics. The t-values, assuming clustering at the province level, range from 2.0 to 4.9.

To understand how much the inclusion of the pretreatment political variables affects the estimates, it is useful to consider the reduced form of the IV regressions we employed in the previous section. These regressions appear in panel B of Table 3. Mechanically, the treatment effects are identical in the first specification that does not include any control variables. The similarity of the estimates in the specifications that gradually add control variables is more remarkable. At the maximum, the inclusion of the pretreatment political variables shaves off only 0.004 from the treatment effect. In particular, the differences in the estimates are small in the specifications that nonparametrically control for socioeconomic characteristics. These results suggest that the exclusion restriction we use to establish the baseline results is likely not violated through the pretreatment political environment of the treatment and control towns.

The treatment effects in panels A and B of Table 3 also allow us to ask how strong selection on unobservables would have to be to explain the effects, given the changes in the estimates following the inclusion of control variables. We use the Altonji et al. (2005) approach, developed for the linear model in Bellows and Miguel (2009), that compares the treatment effect obtained with no control variables to the estimates that come from regressions with richer sets of control variables. Specifically, the ratio of the unconditional estimate to the difference between the unconditional and conditional estimate tells us how much greater selection on unobservables would have to be to explain the treatment effect, compared to selection on observables. The ratio of $0.021/(0.027 - 0.021) = 3.8$ implied by the estimates in column 2 of panel A in Table 3 suggests that any remaining selection on unobservables would have to be almost four times as strong as selection on observables. The

Table 3 Alternative Methods

Panel A: Difference-in-differences regressions					
Dependent variable	*Change in right-of-center vote share*				
Specification	1	2	3	4	5
Demutualization dummy	0.027	0.021	0.021	0.020	0.017
	(4.88)	(2.30)	(2.25)	(2.28)	(1.95)
Adjusted R^2	0.039	0.188	0.190	0.205	0.235
Number of observations	2,143	2,143	2,143	2,143	2,143
Panel B: Reduced-form regressions					
Dependent variable	*Right-of-center vote share*				
Specification	1	2	3	4	5
Demutualization dummy	0.027	0.024	0.024	0.020	0.019
	(4.87)	(2.84)	(2.81)	(2.45)	(2.40)
Adjusted R^2	0.666	0.673	0.673	0.681	0.682
Number of observations	4,286	4,286	4,286	4,286	4,286
Panel C: Matching estimator					
Dependent variable	*Change in right-of-center vote share*				
Specification	1	2	3	4	5
Demutualization dummy	n.a.	0.026	0.023	0.021	0.020
	n.a.	(4.98)	(4.58)	(4.54)	(3.99)
Number of observations	n.a.	2,143	2,143	2,143	2,143

Notes. This table estimates the effect of share ownership on right-of-center voting using two alternative methods. Panel A collapses the data into differences between the 1995 and 2003 elections and regresses the change in right-of-center vote share on share ownership. The control variables are based on changes in the variables used in Table 2. In addition the regressions control for the change in right-of-center vote share from 1991 to 1995 and for the level of right-of-center vote share in 1995. Panel B reports the reduced-form results of the panel regressions used in Table 2. These regressions explain right-of-center vote share with the demutualization indicator and the control variables. Panel C uses an estimator that matches the treated areas to a control sample based on the nearest-neighbor method developed in Abadie and Imbens (2006) using one match per each treatment zip code. The t-statistics in panels A and B are adjusted for heteroskedasticity and clustering at the province level and the z-statistics in panel C are robust to heteroskedasticity. n.a., not applicable.

lowest value of the ratio is obtained with the richest set of controls in panel A where selection on unobservables would have to amount to 1.8 times selection on observables. Given that the full set of controls includes not only the pretreatment vote trend and level, but also a flexible structure that controls for a wide range of socioeconomic and demographic characteristics, it is unlikely that any remaining omitted variables could explain the results.

Panel C in Table 3 uses the differenced structure of the data and reports the treatment effects obtained using the covariate matching estimator discussed in Abadie and Imbens (2006). This method finds control zip codes that are the nearest neighbors for each treatment zip code based on observable characteristics and then calculates the average treatment-control difference in right-of-center voting. The choice of the nearest neighbors is governed by a match metric that measures the distance in characteristics space between the treatment observation and a potential control observation. We report results for the inverse-of-the-variances metric (Abadie and Imbens 2006), but also have experimented with other variants of the estimator.[11] The results in panel C show that the matching estimates are well in line with the results from panels A and B. The treatment effects vary from 0.026 to 0.020 and are all statistically significant.

Taken together, the difference-in-differences regressions and the matching methods show that differences in pretreatment vote trends or vote levels do not drive the strong relation between share ownership and political behavior. These results suggest that the estimates from the IV framework are not generated by the violations of the exclusion restriction outlined in §3.2. Furthermore, the alternative methods rule out *any other violations* of the exclusion restriction that relate to the differential political environment in the treatment towns prior to the demutualizations. Indeed, when we expand the set of pretreatment political variables to include the trend and level of left-of-center voting and voter turnout, the results (not reported for brevity) remain similar to the regressions that only include variables related to right-of-center voting.

[11] These include using the Mahalanobis metric (Rubin 1980, Zhao 2004), the estimator that corrects for the bias that stems from the inability to find a perfect match for each treatment town (Abadie and Imbens 2011), and the propensity score matching estimator (Heckman et al. 1997 and 1998). We have also varied the number of matches from one to four nearest neighbors. Allowing for a larger number of matches increases efficiency, but tends to increase bias because of poorer overall match quality (Imbens 2004, Imbens and Wooldridge 2009). All of these methods produce estimates that are statistically significant and comparable in magnitude to the reported results.

5.2. Alternative Dependent Variables and Subsamples

We now return to the IV regressions and report additional results that vary the definition of the dependent variable and the sample. Our baseline model calculates the right-of-center vote share using the votes cast for the National Coalition Party, the main right-of-center party. On the Hix and Lord (1997) scale that runs from 1 (left-wing) to 10 (right-wing), the National Coalition receives a score of 7.4, the Center Party is 7.0, and the main left-of-center party (Social Democrats) is 4.4. The mean among the six largest parties is 5.5.

Columns 1–4 in Table 4 report the reduced form and IV estimates for regressions that alter the definition of the dependent variable, but we include the full set of controls that enter the regressions nonparametrically in column 5 of Table 2. Column 1 of Table 4 replaces the dependent variable with the vote share of the Center Party. In many rural areas, the left-right spectrum runs from two left-of-center parties to the Center Party, whereas the National Coalition may only have a small vote share in such areas. The prominent position of the Center Party comes from its roots as the promoter of the causes of farmers and it tends to be positioned somewhat to the left of the National Coalition.[12] Perhaps because of these differences, column 1 indicates that the demutualizations had no reliable impact on votes cast for the Center Party. The reduced form estimate suggests a statistically insignificant 0.7-percentage-point increase in the Center vote share (t-value equals 0.66). An alternative interpretation is that rural areas, in which the Center Party often dominates the right-of-center space, did not see an effect of demutualizations on any right-of-center voting. However, when we run the reduced form regressions of the National Coalition share in areas with low population density, we find a 1.2-percentage-point increase in right-of-center voting (t-value equals 1.4).

Column 2 of Table 4 defines the dependent variable as the votes cast for the two left-of-center parties (the Social Democrats and the Left Coalition). The reduced form coefficient indicates that the left-of-center voting decreased by 1.5 percentage points as a result of the increase in share ownership (t-value equals −1.6). The sizeable negative coefficient is consistent with the right-of-center parties gaining voters from the left. However, perfect symmetry with the increase in right-of-center voting should not be expected, because the left-of-center share is not a linear combination of the right-of-center and center vote shares. Smaller parties

[12] For example, the Comparative Manifesto Project (Volkens et al. 2010) gives the Center Party a 1991–2003 average of +4.9 and the National Coalition +12.3 on a left-right scale of −100 to +100.

Table 4 Alternative Dependent Variables and Subsamples

Specification	Alternative dependent variables				Subsamples	
	1	2	3	4	5	6
	Center vote share	Left-of-center	Right-of-center excluding small parties	Voter turnout	Drop Helsinki	Drop towns with no mutuals
Reduced form estimates	0.007	−0.015	0.017	0.0002	0.021	0.021
	(0.66)	(−1.62)	(2.13)	(0.03)	(1.99)	(2.10)
IV estimates	0.089	−0.195	0.223	0.002	0.269	0.433
	(0.70)	(−1.51)	(1.87)	(0.03)	(1.81)	(2.12)
Mean left-hand side (LHS)	0.365	0.312	0.151	0.404	0.160	0.128
Standard deviation of LHS	0.224	0.144	0.096	0.101	0.085	0.084
Number of observations	4,286	4,286	4,286	4,286	2,339	3,885

Notes. This table reports the reduced-form and IV regressions of alternative vote-share specifications and subsamples. Columns 1–5 vary the definition of the dependent variable. Column 1 uses the Center Party vote share whereas column 2 calculates the left-of-center share as the votes cast for the Social Democrat Party and the Left Coalition Party. Column 3 scales the right-of-center vote share with the votes cast for the six parties that have repeatedly appeared on the list of the largest parties. In addition to the National Coalition, Center Party, Social Democrat, and Left Coalition parties, the largest parties' vote share includes the Green League and the Swedish People's Party. Column 5 drops observations coming from Helsinki, the capital of Finland, and column 6 discards the towns that were not served by a mutual telecom firm. The specifications are identical to column 4 in panel B of Table 2 and the t-statistics in parentheses are adjusted for heteroskedasticity and clustering at the province level.

are present that are more difficult to classify along the right-left-dimension. One such example is the Green Party, which often promotes free-market mechanisms on issues such as labor markets, yet generally advocates tough regulation and taxation of capital markets.

Column 3 of Table 4 checks that the main results also hold when the denominator in the calculation is defined as total votes cast for the three largest and the three medium-sized parties. The medium-sized parties are the Left Coalition, the Green Party, and the Swedish People's Party. This specification produces a statistically significant and positive coefficient (t-value equals 2.1) that is comparable to the main result using total votes cast as the denominator.

Column 4 of Table 4 investigates the possibility that the increase in the right-of-center vote share is due to changes in the voter turnout pattern. Stock owners may, for example, be more motivated to vote than nonowners as a result of acquiring and processing more information about the economy. We replace the dependent variable with voter turnout that is defined as the number of votes cast divided by voting-age population.

The regression of voter turnout on share ownership suggests that voter turnout does not play a role in explaining the increase in right-of-center voting. The coefficient indicates that the demutualizations increased voter turnout by 0.02 percentage points, which would amount to an identical-size increase in the right-of-center voting if all of the new voters were to favor right of center. The small economic magnitude and the statistical insignificance (t-value equals 0.03) point to the voter turnout not being the dominant channel that explains the conservative shift in voting.

Table 4 also checks that our results are not affected by the composition of the sample. Columns 5 and 6 report results from two subsamples. The first subsample drops observations coming from the telecom firm serving the Helsinki area. It is possible that the comparison of Helsinki and its surrounding areas to other areas of the country leaves uncontrolled some unobserved factors unique to that area. The Helsinki area also accounts for a large share of the zip codes in the treatment group. The second subsample drops all zip codes in which no mutual company existed in the first place. These mostly rural areas were serviced by a partially government-owned countrywide operator. This restriction leaves us with a subsample of mutuals that decided to demutualize versus mutuals that did not.

Column 5 of Table 4 reports the results for the subsample that drops the telecom servicing the Helsinki area. The regression shows that the demutualizations caused an increase in right-of-center voting even outside Helsinki. The coefficient indicates a 2.1-percentage-point increase in right-of-center voting following demutualizations and this effect is statistically significant at the 5% level (t-value equals 1.99). Column 6 of Table 4 arrives at the same point estimate (t-value equals 2.1) in a sample that drops the towns that were not serviced by a mutual telecom company. These results lead us to conclude that our findings are robust to variations in the sample used for estimation.

6. Channels

6.1. Evidence from Nonlisted Demutualizations

The results thus far show that the demutualizations had a robust positive impact on right-of-center voting in the affected areas. In this section, we disentangle the effect of becoming an owner of publicly listed stocks from other effects related to demutualizations. These analyses use the events in which two local telecom firms decoupled ownership from the provision of phone services, but did not list the shares on the public market (see §2.4 for details).

The nonlisted demutualizations are useful in understanding if direct economic incentives play a meaningful role in explaining the increase in right-of-center voting. Wealth-related incentives for voting right-of-center following demutualizations can arise from changes in wealth, liquidity, and taxes.

Although the demutualizations did not have a direct effect on property rights and the incentives for having better protection of property rights thus did not change, they may have caused a change in the value of the firm. The mutual form could be less valuable if the bundling of customer and stockholder roles leads to inefficient risk sharing (Fama and Jensen 1983), or if the absence of a threat of outside takeover makes managers more likely to extract private benefits (Lamm-Tennant and Starks 1993). On the other hand, Mayers and Smith (1986) show empirical evidence suggesting benefits to a mutual form.

In addition, the owner faced a high fixed cost of selling the certificate prior to the conversion, because of the loss of the ownership discount. But once the phone services were decoupled from ownership, owners could sell their shares without incurring this loss. In the nonlisted demutualizations, the shares could be sold in an active grey market.

Finally, the mutual form allowed customer-owners to derive untaxed benefits in the form of the customer discount. The loss of the discount in the conversion increased the phone companies' profits, leading to increased corporate taxes and an increase in the owners' annual tax burden.[13] Other aspects of the conversions were tax neutral: mutual companies and corporations faced the same flat corporate income tax rate and similar tax treatment of cash distributions to owners and realized capital gains on the assets.

The key insight that makes the nonlisted conversions useful in addressing all of these explanations is that they share the wealth-related aspects with the publicly listed demutualizations, and only differ in terms of the public listing of the shares. If the increase in right-of-center voting following the demutualizations is driven by direct economic incentives, the nonlisted conversions should show a positive and significant treatment effect. In contrast, the salience hypothesis predicts that the treatment effect should be limited to demutualizations in which shares were publicly listed.

We investigate these issues in Table 5, which follows the now familiar IV framework from Table 2. We include two independent variables in addition to the controls. The first variable is an indicator for the three mutual companies that converted to publicly listed firms, whereas the second variable indicates the two mutual companies that decoupled the phone services from ownership, but did not publicly list their shares.

Panel A in Table 5 reports the first-stage regressions where the dependent variable is the zip code level share ownership rate. As expected, the dummy for publicly listed demutualizations attracts a positive and statistically significant coefficient that is nearly identical to the baseline estimates in Table 2. The indicator variable for the nonlisted demutualizations has a negative sign, but is very small in magnitude and statistically insignificant. The largest coefficient in the full specification in column 5 implies a 0.6-percentage-point decrease in share ownership (t-value equals 1.27). Figure 3 confirms these results by plotting share ownership rates in the two nonlisted conversion areas and in other towns (excluding the publicly listed demutualizations)—the changes in share ownership in the nonlisted conversions follow the overall patterns. These results show that ownership of publicly listed shares was not affected in nonlisted conversions. The lack of an effect validates our assumption about the exclusion restriction used in the baseline analysis—the conversions are not associated with unobservable trends in share ownership.

Panel B in Table 5 presents the reduced-form estimates from regressions that explain the right-of-center vote share with the listed and nonlisted demutualization dummies. In light of the lack of a first-stage effect, it is not surprising that the coefficients for the nonlisted conversions are insignificant and small. The full specification attracts a coefficient of -0.003 with a t-value of -0.2. Also this result is reassuring for the exclusion restriction, as it suggests that the conversions are not associated with changes in the unobservable determinants of right-of-center voting.

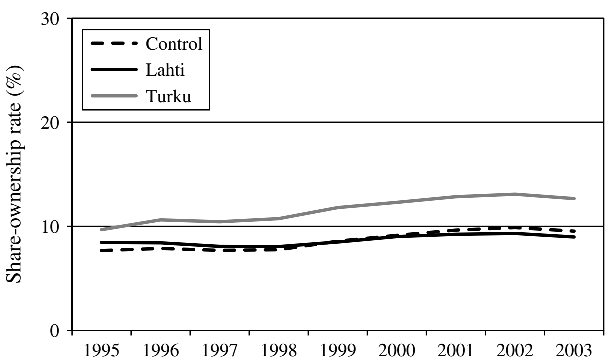
[13] In the case of the largest of the firms, Helsingin Puhelin, the customer discount had been about 60 euros per year before the conversion. Although this is not a large sum in an absolute sense, it may be relatively more important than it first appears. Adjusted for consumer price index inflation to 2014, the figure is 79 euros. A cell phone plan with data services comparable to a U.S. plan of \$50–\$70 per month currently costs around 20 euros in Finland. So, in rough purchasing power terms, the annual customer discount was equivalent to four free months in a typical smart phone plan in the U.S. today. The corporate tax rate at the time was 26%. Losing 60 euros in tax-free benefits and gaining the same amount in pretax profits thus implies an increase of 15.60 euros ($= 0.26 \times 60$ euros) in taxes paid per year, i.e., about a month's worth of a smart phone plan. For the median income earner, this amount corresponds to an increase in total annual tax burden of about 0.2%. This calculation is based on the following assumptions: the companies were operating close to breakeven profits before the demutualization effectively passing on all benefits to customer-owners; company unit sales and costs are unaffected when it starts selling at the market price; and labor supply is fixed (i.e., individuals do not increase their working hours in response to an increase in their phone bills).

Table 5 Disentangling Salience of Public Stock Ownership from Other Effects

Specification	1	2	3	4	5
		Panel A: First-stage regressions			
Publicly listed demutualization dummy	0.080	0.083	0.086	0.076	0.076
	(4.53)	(4.86)	(4.87)	(5.93)	(6.12)
Nonlisted demutualization dummy	−0.002	−0.001	−0.001	−0.004	−0.006
	(−0.37)	(−0.26)	(−0.34)	(−0.90)	(−1.27)
Adjusted R^2	0.843	0.852	0.858	0.863	0.866
Number of observations	4,286	4,286	4,286	4,286	4,286
		Panel B: Reduced-form regressions			
Publicly listed demutualization dummy	0.027	0.024	0.024	0.019	0.019
	(4.97)	(3.04)	(3.01)	(2.70)	(2.62)
Nonlisted demutualization dummy	0.001	−0.001	−0.001	−0.002	−0.003
	(0.07)	(−0.06)	(−0.06)	(−0.13)	(−0.17)
Adjusted R^2	0.839	0.842	0.842	0.846	0.846
Number of observations	4,286	4,286	4,286	4,286	4,286

Notes. This table reports results of the first-stage and reduced-form regressions with the same sets of control variables as in Table 2. The dummy for publicly listed demutualizations takes the value of one for the three areas that demutualized their local telecom firms to publicly listed companies. The nonlisted demutualization dummy takes the value of one for the two towns that decoupled the telecom services from ownership, but did not publicly list their shares. The t-statistics in parentheses are adjusted for heteroskedasticity and clustering at the province level.

Figure 3 Ownership of Publicly Listed Shares Around Nonlisted Demutualizations



Notes. This figure plots the proportion of share owners in March of each year in the sample. November 2002, the end point of the share ownership data, is used in lieu of March 2003. The rates are shown separately for the two areas in which the ownership of the local telecom was decoupled from phone services (Turku and Lahti areas) and for areas that did not convert the local telecom. The control areas exclude the areas that experienced a publicly listed demutualization. Both firms were converted in 1999.

Overall, the findings in this section validate our approach of using the demutualizations as plausibly exogenous shocks to ownership of publicly listed shares. The lack of any treatment effects from the nonlisted conversions suggests that changes in wealth, liquidity, taxes, or any other potential byproducts of demutualizing do not explain the previously observed increase in right-of-center voting.

6.2. Evidence from Survey Data

What is the channel through which ownership of publicly listed shares influences voting? New shareholders have an incentive to learn about stock markets and the economy, which may lead to giving more weight to economic issues in voting decisions. For example, new stockholders may start appreciating the workings of the corporate world, obtain new points of view from their peer investors, or pay more attention to financial media. The shift in attention, and associated changes toward the identity of a shareholder, can increase sympathy for right-of-center policy.

We assemble survey data that allow us to speak to the role of financial media in generating the conservative shift in voting. These data come from the polling organization TNS Gallup and are based on a regularly run nationally representative survey of about 6,000 individuals in Finland. The 2009 survey includes a battery of questions relating stock ownership, political views, and use of financial media, and also respondents' background characteristics (missing observations reduce the sample size to 4,384 individuals).

We use the survey data to see to what extent they are consistent with the following narrative: stock ownership affects exposure to financial media, which in turn affects political views. Table 6 reports OLS regressions of exposure to financial media on stock market participation (the first part of the narrative, columns 1–3) and regressions of political views on exposure to financial media (the second part, columns 4 and 5).

Columns 1–3 of Table 6 show that stock market participation is both economically and statistically significantly related to financial media use, controlling for age, gender, education, wealth, income, and other potential factors. Unreported univariate analyses show that the stock market participation dummy has the second highest pairwise correlation with a dummy for following financial media (the highest correlation is with the amount of financial wealth). The

Table 6 Using Survey Data to Understand Potential Channels

Dependent variable	Financial media use			Political orientation	
	Financial media composite, four-point scale	*Leading financial newspaper dummy*	*Leading financial periodical dummy*	*Right-of-center dummy*	*Free market attitude*
Specification	1	2	3	4	5
Stock market participation dummy	0.447	0.181	0.212		
	(12.37)	(8.78)	(10.32)		
Financial media, composite				0.056	0.124
				(7.65)	(10.70)
Adjusted R^2	0.207	0.102	0.159	0.109	0.096
Number of observations	4,834	4,834	4,834	4,834	4,834

Notes. This table reports OLS regressions of financial media use and political orientation. The data are from a nationally representative survey of individuals in Finland conducted in 2009. *Financial media, composite* is constructed by giving respondents one point for each affirmative answer to the following assertions: "I at least sometimes watch financial news on the television"; "I at least sometimes read a financial magazine/newspaper"; "I actively follow economic issues on TV, on the Internet, and the newspapers." *Right-of-center dummy* equals one for participants that describe their political orientation at least somewhat (or more) right of center. *Free market attitude* is constructed by giving respondents one point for each affirmative answer to the following assertions: "I accept the privatization of communal services"; "I accept growing income inequality"; "As a first principle, the state should not regulate free markets." All the regressions include an unreported constant term and controls for logged income, logged wealth, age and age squared, and dummies for females, education, urban areas, entrepreneurs, unemployed, students, and retirees. Urban areas indicate the six largest cities. Education dummies are for secondary education, college degree, and Master's degree. The t-statistics in parentheses are robust to heteroskedasticity.

correlation coefficient is 0.28 (significant at the 1% level). This pairwise correlation surpasses other variables that might plausibly affect the tendency to follow financial media, such as age, gender, education, trust, social activeness, being a homeowner, being an entrepreneur, financial literacy, and others. Van Rooij et al. (2011) also find that stock market participation correlates strongly with interest in economic issues.

Related to the second part of the narrative—that media exposure affects political views—the data include two measures of political preferences: voting right of center, and having a free market attitude. Columns 4 and 5 of Table 6 show that financial media use is both economically and statistically significantly related to right-of-center political preference and free market attitude, controlling for other relevant factors. The unreported univariate correlation between a dummy for following financial media is 0.24 with both of these measures of political preferences, and significant at the 1% level. There is no other variable with a higher correlation.

Because these results are based on a single cross section with no exogenous variation in the key variables, they cannot be taken as evidence of causality. However, the fact that these relations exist and are very strong gives added credence to the existence of a link from stock ownership to voting behavior, potentially mediated by the use of financial media.

7. Conclusion

Theory and political commentary suggest ownership matters for political behavior. It can affect both the content of political beliefs and the level of political activity. Empirical evidence on the phenomenon, however, is scarce. Using a series of demutualizations as a source of plausibly exogenous variation in the ownership of publicly listed shares, we show a positive and economically significant effect on right-of-center vote share.

The institutional features, as well as our statistical methods, give confidence in that this inference is not impaired by endogeneity issues. Panel regressions, difference-in-differences analyses, and matching methods yield positive and significant estimates that imply effect sizes of similar magnitude. The latter two analyses allow the inclusion of pretreatment vote trends and levels as additional controls, which is a particularly powerful remedy against potential violations of the exclusion restriction. In effect, it rules out *any* confounds that work through the differences in the political environment between the treatment and control towns prior to the demutualizations.

We also analyze conversions of mutuals that decoupled the phone services from ownership, but that did not distribute publicly listed shares to the owners of the mutual. These events are not associated with changes in voting patterns. Because the only major difference between the publicly listed and nonlisted conversions is the public listing of shares, the effects we document are most consistent with the ownership of publicly listed shares influencing political behavior. These results also speak to the validity of the exclusion restriction: if mutual conversions were driven by unobservable correlates of right-of-center voting, the nonlisted conversions should yield treatment effects similar to the listed demutualizations. They do not.

Do our results generalize to other settings and countries? Although our natural experiment is by its very nature local, many commentators have suggested similar influences in other contexts. (Glassman 1999, p. 1) notes that "the rapid rise in the number of U.S. families that own stocks will have important consequences for American politics. Not the least of those consequences will be broader support for free-market policies." Kudlow (1997) argues that the rise of an investor class means that voters will be more interested in nest egg protection than government social programs. Finally, *The Economist* (2001) writes that "wider share ownership is profoundly important. … [it] changes attitudes towards economic freedom." Aggregate data on secular trends in the cost of mutual fund ownership and Republican vote share in the United States (Duca and Saving 2008) suggest the causal effects we document in our natural experiment potentially extend to other settings.

That even a relatively modest change in ownership can have a decisive effect on elections has implications for public policy: sometimes changing people's frame of reference can be easier than changing their direct incentives. This relative ease helps to understand why right-of-center politicians may use ownership as a strategic tool to attract the median voter to their party, as implied by Biais and Perotti (2002), as well as political commentary about President Bush's Ownership Society initiative (Barnes 2004, Nadler 2000). The political implications of retirement savings and property-rights reforms are therefore potentially far-reaching.

Retirement savings plans that rely increasingly on defined contributions and individually managed retirement accounts may make ownership more salient to voters. Sweden and Slovakia have moved to systems with an option to place part of the pension contribution into an individual retirement account, and other countries are investigating similar reforms. Even greater changes in private ownership of assets are taking place in emerging markets. For example, more than 140 million stock trading accounts now exist on the Chinese exchanges. A poll by GlobeScan in 2011 showed that the support for free-market economy among the Chinese even surpassed that of U.S. citizens. Obviously these attitudes do not correlate with the current political systems in the United States and China, but they speak to a plausible underlying trend in the microfoundations.

That the types of financial instruments people hold can be consequential to their beliefs and attitudes can carry over to other domains as well. For example, investing in certain consumer-product manufacturers could make investors view the firms' products more positively, investing in a passively managed index fund could enhance a belief in market efficiency, and engaging in socially responsible investment could make investors more ethical.

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