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On the Economics and Politics of Mobility

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Abstract

Exit, Voice and Political Change: Evidence from Swedish Mass Migration to the United States. During the Age of Mass Migration, 30 million Europeans immigrated to the United States. We study the long-term political effects of this large-scale migration episode on origin communities using detailed historical data from Sweden. To instrument for emigration, we exploit severe local frost shocks that sparked an initial wave of emigration, interacted with within-country travel costs. Because Swedish emigration was highly path dependent, the initial shocks strongly predict total emigration over 50 years. Our estimates show that emigration substantially increased membership in local labor organizations, the strongest political opposition groups at the time. Furthermore, emigration caused greater strike participation, and mobilized voter turnout and support for left-wing parties in national elections. Emigration also had effects on formal political change, as measured by welfare expenditures and adoption of inclusive political institutions. Together, our findings indicate that large-scale emigration can achieve long-lasting effects on the political equilibrium in origin communities.

Mass Migration and Technological Innovation at the Origin. This essay studies the effects of migration on technological innovations in origin communities. Using historical data from Sweden, we find that migration caused a long-run increase in patent innovations in origin municipalities. The same instrumental variable design as in the previous essay is employed to establish causality. Our IV estimate shows that a ten percent increase in emigration entails a 7 percent increase in a muncipality's number of patents. Weighting patents by a measure of their economic value, the positive effects are further increased. Discussing possible mechanisms, we suggest that low skilled labor scarcity may be an explanation for these results.

Richer (and Holier) Than Thou? The Impact of Relative Income Improvements on Demand for Redistribution. We use a tailor-made survey on a Swedish sample to investigate how individuals' relative income affects their demand for redistribution. We first document that a majority misperceive their position in the income distribution and believe that they are poorer, relative to others, than they actually are. We then inform a subsample about their true relative income, and find that individuals who are richer than they initially thought demand less redistribution. This result is driven by individuals with prior right-of-center political preferences who view taxes as distortive and believe that effort, rather than luck, drives individual economic success.

Wealth, home ownership and mobility. Rent controls on housing have long been thought to reduce labor mobility and allocative efficiency. We study a policy that allowed renters to purchase their rent-controlled apartments at below market prices, and examine the effects of home ownership and wealth on mobility. Treated individuals have a substantially higher likelihood of moving to a new home in a given year. The effect corresponds to a 30 percent increase from the control group mean. The size of the wealth shock predicts *lower* mobility, while the positive average effect on mobility can be explained by tenants switching from the previous rent-controlled system to market-priced condominiums. By contrast, we do not find that the increase in residential mobility leads to a greater probability of moving to a new place of work.

Till Lina

Acknowledgments

I want to begin by thanking my main supervisor, David Strömberg, who has supported me since the first years of the PhD. When I first asked him to be my supervisor, he looked at me with a very excited look on his face, and asked what research proposals I had. I offered a couple of early and probably misguided ideas, which he casually ignored, and instead he said: “Think about what you believe are the major forces shaping the world around us. What can you say about them?” I’m paraphrasing, of course, but the message was clear: the bar was set high. Throughout the years, that conversation stayed with me and always encouraged me to aim a little higher.

I came back to Stockholm after my Master’s in Paris because of my relationship with Lina. It did not hurt, however, when it became clear to me that Stockholm University and the IIES in particular were outstanding places to study political economics. The first time I met Torsten Persson, my second supervisor, I felt my voice shaking from being so nervous. He became my supervisor quite late in the game, but was a marvelous influence and motivator from day one. My only regret is that I didn’t ask him earlier.

Stockholm’s graduate program has a very friendly and supportive environment among students. I have been very lucky to meet fantastic colleagues during the program. First and foremost, Erik Prawitz, who has been my longest running office mate, good friend and partner in crime for coming up with interesting research ideas. I owe a lot of this dissertation to our collaboration. I also want to thank Audinga Baltrunaite, Shuhei Kitamura, Nathan Lane and Andrea Guariso for sharing their creativity, encouragement and fun throughout these years.

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Most of all I know that my mother is very proud of me for getting a PhD. As with so many other things, she most likely knew long before me that I would go down this path, even as I loudly complained that doctorates are "practically useless and only for people who want to become a professor!" I think of her late cousin in Algeria, also a PhD economist, who was very dear to her heart and whom I would have loved to get to know better.

And lastly, thank you Lina, for everything.

Stockholm, September 2016
Mounir Karadja

Contents

Chapter 1

Introduction

This thesis consists of four self-contained essays. The essays span a wide variety of topics, use different empirical methods and have samples sizes ranging from a few hundred observations to several million. They also have some things in common. For instance, they are all empirical in nature. Although economic theory has been an important source of inspiration for my research, I have strived to use empirical methods to not only prove that an idea is interesting in theory, but that it has demonstrable importance in the world around us.

The four essays in this thesis also only use Swedish data. According to no preconceived plan, my proposed projects that were set in Sweden have turned out to be the ones that were both feasible and sufficiently interesting to pursue. This is partly by coincidence, but also due to the preponderance of high-quality data in Sweden. I still find it remarkable that we were able to amass such an amount of detailed data from 19^{th} century Sweden, used in Chapters 2 and 3. Many years of effort from local governments and academics to unearth and digitize historical archives were instrumental for my co-authors and I to build our own data set and carry out those studies.

Mobility is the main thread pulling the four essays in this dissertation together. In 2016, the concept of mobility seems ever more important. As citizens of war-torn and dictatorial countries struggle to find safe harbor in richer and more peaceful countries, many worry about the effects on those people who cannot move, and are left behind. Chapters 2 and 3 offer historical and perhaps hopeful lessons about the effects of international mobility on origin countries. In the 19^{th} century, when Sweden was one of the poorest countries in Europe, more than one million Swedes left their homes and emigrated, mostly to the United States. Although there were great worries about Sweden's economic future because of this, my co-authors and I find that emigration brought both political and economic development to the communities that sent the most emigrants. Sweden's well-known labor unions and left-wing parties both became stronger in locations with more emigration, as shown in Chapter 2. Local redistribution policy and political institutions also tended to change in line with the preferences of the labor movement.

The main idea underlying these findings is that people who have better outside options (for example, who can threaten to emigrate to a country with high wages) have a greater bargaining position and hence are more able to advance their own interests, even without migrating themselves. Because it was a rather risky activity to engage in labor organization in the late 19^{th} century, this outside option is likely to have encouraged more workers to organize, even in the face of punishment from anti-union employers.

In addition, economic development followed similar patterns. In Chapter 3, we show that there was an increase in patent innovations occurring in the areas that had more emigrants. Looking at a period of several decades, we find that municipalities that had ten percent more emigration also saw an average of 7 percent more patents. This effect is likely due to the fact that emigration lowers the supply of labor, hence increasing the price of hiring workers. As a result, there is an incentive to develop innovations that may save on labor. Because the period of Swedish mass migration occurred during Sweden's relatively late industrialization phase, an interesting question raised by this research is if Sweden's strong subsequent economic development may in part be explained by emigration to the United States.

Mobility is also important within countries and across social groups. As income inequality increases in developed countries, the question of how people perceive their economic status compared to others garners new interest. In Chapter 4, my co-authors and I use a tailor-made survey carried out in 2011 to ask how well individuals in Sweden can locate themselves in the income distribution. We find that a majority of Swedes in fact underestimate their relative income position by a large amount. The median respondent believes that they are poorer by almost two deciles, than they in fact are. This misinformation has real implications when corrected. When half of our study participants are randomly told their true position in the income distribution, those who get relatively richer report lower demand for redistribution. Interestingly, this effect is entirely accounted for by individuals who already had right-leaning preferences to begin with. Our evidence suggests that this is due to the fact that individuals who believe in the role of effort rather than luck, and that taxation is distortive, are more likely to be right-wing.

In the final essay, Chapter 5, we study how residential mobility is affected by home ownership and wealth. Using data on residents of Stockholm, we look at a policy that converted more than 100,000 rental apartments to condominiums between 1998 and 2012. Tenants who converted their apartments received substantial discounts compared to market prices, which we estimate to at least twice the median yearly wage in 2005. We find that individuals who converted their apartments to condominiums display much higher residential mobility after treatment, compared to those who were not treated. Treated individuals display an increase in the probability of moving of about 3 percentage points, which is large relative to the control group's average mobility of 9.5 percent. Interestingly, those who received larger wealth shocks display lower residential mobility, while the direct effect of converting one's apartment from rent control to market-priced condominium drives the positive average effect. As a result, this study shows that the forms of rent control used by many cities across the globe can substantially hamper the residential mobility and likely causes large allocative inefficiencies.

Chapter 2

Exit, Voice and Political Change: Evidence from Swedish Mass Migration to the United States*

2.1 Introduction

Institutions are widely regarded as important determinants of long-run development. Yet, less is known about what causes them to change over time. This paper proposes and empirically verifies that large-scale emigration can be a mechanism leading to political change in origin countries. Using one of the largest migration episodes in human history, the Age of Mass Migration, we estimate the long-run effects of emigration on local political outcomes.

Starting in the mid 19^{th} century, the Age of Mass Migration saw

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30 million Europeans leave their home countries for the United States. Through social and family ties, early movers spurred additional emigrants over time, leading to a long-lasting pattern of chain migration (Hatton and Williamson, 1998). What were the political repercussions at the origin of this shock to ordinary citizens' mobility? Consider Figure 2.1, which plots the relationship between a measure of the bargaining strength of labor, the share of workers in 2000 who were unionized, against the size of the US emigrant stock in 1910. Across 29 OECD countries, the figure reveals a clear, positive relationship between modern unionization and historical migration. Among the countries that have well-known US populations, such as Italy, Germany, Ireland, and Sweden, unionization rates are considerably higher than low-emigration countries such as Spain, France, and Poland. The correlation suggests that the United States policy of open borders during the 19^{th} century may have had considerable consequences on labor relations in the Old World. This paper is devoted to understanding if this relationship may be causal.

We focus our attention on Sweden, which had one of the highest exit rates in the period. A quarter of its population, or about 1.3 million citizens, emigrated in the course of sixty years, mainly to the United States. Swedish economic and political elites were highly concerned about the newfound mobility of ordinary citizens. As a result, proposals to restrict emigration were continually made, but were never put in place. Instead, the Age of Mass Migration coincided with a period of political development in Sweden. The dominant force in Swedish 20^{th} century politics, the Social Democratic Party, as well as the powerful labor union movement, were founded during the period and became key actors in reforming Swedish policy and political institutions.

It is unclear how emigration related to this development, however. Theoretically, the effect of emigration on an autocratic origin country is ambiguous. If political dissidents choose to exit the country rather than to push for reforms, the result may be a lower level of voice for political development (Hirschman, 1970). On the other hand, social ties to past migrants increase the mobility of citizens who stay behind, improving stayers' outside option and potential for bargaining with local elites. For example, Hovde (1934) argues that the threat of emigration put labor in a strong bargaining position and enabled the formation of highly effective organizations.[1] Moreover, Hirschman (1978) discusses the potential for exit to complement voice during the Age of Mass Migration, sparked by the observation that US emigration coincided with a wave of European democratization.

We deploy a wide range of data sources spanning the mass migration period to study the long-term political effects of emigration across Swedish municipalities. The empirical analysis is organized in two sections, broadly examining citizens' demand for political change and elites' response through policy making. On the demand side, we start by investigating the relationship between emigration and the local political organization of citizens. Our main outcome variable is membership in the labor movement, defined as labor unions and the Social Democratic Party. To further probe the role of the labor movement, the membership data are supplemented with measures of the movement's strength and influence. First, participation in the 1909 general strike involving 300,000 workers is used as a measure of direct, individually costly engagement. Second, we study voter turnout and vote shares for left-wing parties in national elections 1911–1921, allowing us to identify political mobilization as well as labor-related political preferences.

If emigration induces citizens to demand political change, a natural question is if this is reflected in local government actions. In the second part of our analysis, we therefore turn to estimating the effect of emigration on local policy and political institutions. We use data on welfare expenditures to test if emigration resulted in changed patterns of redistribution across municipalities. Using data on the form of democracy

[1] Similarly, regarding the out-migration of blacks from the US south, Myrdal (1944) writes that "the experience [suggests] that emigration of a significant number of Negroes is one of the surest ways of stimulating the Southern whites to give more consideration to the Negroes that remain in the South".

chosen by local governments, we then test the hypothesis that emigration leads municipalities to adopt more inclusive formal institutions.

To establish causality, we exploit the fact that Sweden's mass emigration was sparked by a series of severe agricultural shocks in the 1860s, caused by unusually cold temperatures (Sundbärg, 1913; Barton, 1994; Beijbom, 1995). Using daily temperature data from this period, we measure the incidence of growing-season frost shocks 1864–1867, just prior to the onset of early mass migration. We then construct an instrument which only captures variation in the intensity of emigration push factors: the interaction between frost shocks 1864–1867 and the proximity to one of the two major emigration ports.[2] Using only the interaction term as our instrument allows us to control for both proximity to port and frost shocks themselves, which avoids picking up any confounding direct effects of severe economic shocks on political outcomes.[3] Importantly, because Swedish emigration was highly path dependent, which we show, the instrument strongly predicts cross-sectional variation in total emigration across the 50-year sample period.[4] The instrument passes several exogeneity tests, including a balance test on pre-determined covariates and placebo treatments using shocks in other periods. Shocks occurring in the *non-growing* seasons 1864–1867 have no effect on emigration or second-stage outcomes.

Our results show that municipalities that experienced more emigration during the Age of Mass Migration exhibit significantly increased demand for political change. Membership in local labor organizations is significantly higher starting in 1900, which provides a link between

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[2]See e.g. Quigley (1972) and Morten and Oliveira (2014) regarding the importance of travel costs for migration decisions.

[3]For example, it is possible that municipalities which were more affected by frost shocks in this period developed more extensive social insurance systems as a result. However, even if such effects are persistent, they would be taken into account by controlling for the direct effect of frost shocks.

[4]High degrees of path dependence in migration patterns is a canonical finding in the migration literature and has been found in numerous settings, see e.g. Massey et al. (1993), Hatton and Williamson (2002), McKenzie and Rapoport (2007), Bryan et al. (2014) and Giulietti et al. (2014).

Sweden's mass emigration and the growth of its influential labor movement. This relationship is also reflected in our measures of organizational strength, as emigration leads to higher participation in the major general strike of 1909. Furthermore, we find increased voter turnout in national elections 1911 to 1921, as well as higher vote shares for left-wing parties in those same elections. Rather than inhibiting their use of voice, higher emigration led to more political coordination and left-wing preferences among ordinary citizens, and arguably resulted in a greater bargaining power vis-à-vis local elites.

Emigration also had an impact on policy and political institutions, in line with the preferences of the labor movement. Welfare expenditures per capita are significantly higher in municipalities with more emigration, both before and after the introduction of democracy in 1919.[5] In 1918, a weighted voting system in local elections gave wealthy individuals up to 40 votes, biasing decision making power towards economic elites. The observed increase in expenditures is therefore unlikely to have been caused by changes in the preferences of ordinary citizens. Rather, it is consistent with concessions being made by elites in favor of citizens.

During this period, local governments were organized either as direct or representative democracies. Recent evidence has shown that municipalities under representative democracy provided higher welfare expenditures, likely due to direct democracies being more easily captured by local elites (Hinnerich and Pettersson-Lidbom, 2014). We find that municipalities with greater emigration are more likely to adopt the more inclusive political institution between 1919 and 1938. As such transitions were required to last at least five years, and often lasted longer in practice, this finding is in line with the theory of institutional change as a commitment device (Acemoglu and Robinson, 2000, 2006).

Lastly, we test for long-run persistence of the effect of emigration. A number of studies in economics and political science have found political preferences to be persistent within individuals as well as correlated across

[5]These results are not explained by decreased population, as results hold in expenditure levels as well.

generations.[6] We find that left-wing party preferences persist in present day elections, over a century after the start of Swedish mass migration, using data at both the municipal and national level between 1998 and 2014.

Emigration may have affected political outcomes through other channels than labor organization. Using the data sources available to us, including censuses, we evaluate the plausibility of a number of additional mechanisms. We start by assessing different types of selection into migration. First, we find that labor movement membership and strike participation are not driven by changes in the employment composition of municipalities towards manufacturing. Second, we find no effects on the share of voting eligible citizens 1911–1921 nor on sex ratios, marriage rates, household size and in-migration from other municipalities. Third, a bounding exercise additionally shows that even if emigration were highly skewed in terms of ideology, the effect of emigration of left-wing voting remains sizable and significant. We continue by evaluating the potential transfer of American attitudes as a result of transatlantic migration, finding indirect evidence against this mechanism in our setting. There are no positive effects on membership in two types of organizations that were highly influenced by the US: non-state free churches and temperance lodges.

This study relates to a nascent empirical literature on the political effects of emigration in origin countries[7]. One set of papers studies the effects of migration to democratic countries, finding a positive effect of such migration on democratization (Spilimbergo, 2009) and voting for an opposition party (Pfutze, 2012; Omar Mahmoud et al., 2015). Batista and Vicente (2011) find that households in Cap Verde with more migration experience exhibit a higher demand for good governance. Docquier et al. (2014) and Preotu (2016) use cross-country data for developing countries to measure the effect of migration flows on origin country in-

[6] See e.g. Alford et al. (2005), Jennings et al. (2009) and Madestam et al. (2011).

[7] There is also theoretical literature linking political and economic repression to migration (Docquier and Rapoport, 2003; Mariani, 2007; Wilson, 2011).

stitutions and conflict, respectively.

Our contribution to this literature is three-fold. First, we exploit plausibly exogenous variation in order to identify the causal effects of emigration on political outcomes. Although current studies are aware of the challenges in identifying a causal effect, there is still a lack of well-identified estimates. Second, while the existing literature emphasize the transfer of attitudes from host to origin countries, we focus on the mechanism of emigration improving the outside option of citizens. Third, we contribute by not only showing how demand for political change responded to emigration but also tracking its effects on actual political change in terms of local policies and institutions.

We also relate to the literature on institutions in economic development, and political change in particular. There is a large literature investigating transitions into and from democracy at the national level.[8] Besley et al. (2015) study the effect of incumbents' probability of staying in power on institutional reform. A growing set of papers investigates the effects of trade on institutional change (Acemoglu et al., 2005; Puga and Trefler, 2014; Dippel et al., 2015; Sánchez de la Sierra, 2015). The importance of factor mobility for institutional change has been studied theoretically, though mainly focusing of the mobility of capital rather than labor (Boix, 2003). Our proposed mechanism links emigration to citizens' outside option. Relatedly, Acemoglu and Wolitzky (2011) show that greater outside options for workers improves their equilibrium outcomes under coercive institutions. This paper also relates to the literature on groups and voter turnout, in finding a co-occurrence of labor movement size and voting (Morton, 1991).

The remainder of the paper proceeds as follows. Section 5.2 provides an overview of Swedish mass emigration and describes the historical evidence regarding the cause of its onset in the 1860s. The labor movement and its relationship to emigration are also described. Section 5.3 describes our data. Section 5.4 introduces the econometric framework and

[8] See Persson and Tabellini (2009) and studies cited therein.

our identification strategy. Sections 2.5 to 2.8 discusses the first-stage relationship as well as the effects of emigration on our political outcomes. Section 5.6 performs a series of robustness checks on our main specifications. Section 2.10 tests for longer term persistence in labor movement membership and left-wing voting. Lastly, Section 5.7 discusses our results and concludes.

2.2 Background

2.2.1 Swedish mass emigration to the United States

Starting in 1850, the Age of Mass Migration saw 30 million Europeans settle in the United States. Under its policy of free immigration, individuals from all over the world were allowed permanent residency in the United States. Sweden was one of the biggest sending countries in per capita terms, along with Ireland, Italy and Norway (Taylor and Williamson, 1997). A total of 1.3 million Swedes emigrated from 1860 to 1920, corresponding to one quarter of the average population over the period.

Swedish emigration took off abruptly at the end of the 1860s. In the peak year of 1869 alone, nearly 1 percent of the population emigrated and in the years between 1867 and 1879, 200,000 Swedes left their home country. We refer to the sharp increase in emigration 1867–1879 as the *first wave of mass emigration*.[9] The spike of the first wave is evident in Figure 2.2, which displays per capita emigration rates over the period. The causes and timing of the Swedish mass emigration episode have been widely discussed by historians. Central to the existing accounts is the series of bad harvests in the 1860s, caused by unusually poor weather conditions, which led to widespread poverty and served as a catalyst

[9] Earlier emigration was uncommon – in 1865 the Swedish American population was estimated at 25,000 (Barton, 1994). Poor communications may have held back potential emigrants, as crossing the Atlantic was expensive and time-consuming. Sailing was the predominant means of transport and traveling from Sweden to North America took up to two months.

for emigration on a large scale (see e.g. Sundbärg, 1913; Barton, 1994; Beijbom, 1995).[10] In particular, cold weather led to a high incidence of frost as nighttime temperatures fell below zero degrees Celsius, even during the regular growing season. The spring of 1867 saw the most extreme weather, in some cases lasting well into the summer months.[11]

The famine years were particularly harshly felt because agriculture was the main source of food and income for most citizens: in 1865, 83 percent of the population lived in rural areas and only 11 percent of the labor force worked in manufacturing (Edvinsson, 2005). Cities and towns were affected indirectly, however, as the supply of food and the demand for goods and services dropped (Beijbom, 1995). In our data set, 28.3 percent of emigrants 1867–1920 are from urban areas. Figure 2.3 displays detrended Swedish real GDP per capital 1850–1900. There is a visible trough during the years 1864–1867.[12]

Later emigration waves occurred during the 1880s and at the turn of the century, as seen in Figure 2.2. This pattern was common throughout Europe and has been linked to inversely developing business cycles across the Atlantic during this period (Hatton, 1995). For Sweden, differences in growth rates between the United States and Sweden have been shown to predict aggregate emigration flows between 1870 and 1910 (Bohlin and Eurenius, 2010). Social networks were also crucial drivers of emigration in the later waves. First-hand accounts of Swedes in the US reveal that many would not have emigrated if it were not for having

[10]Sweden's case is similar to that of Ireland, whose first emigration wave was caused by a famine (Hatton and Williamson, 1993).

[11]The month of May 1867 is the coldest known May in Swedish history and the meteorological summer (five days in a row with temperatures above 10 C) started only in mid-June in many parts of Sweden (SMHI, 2013). In Finland, the temperatures observed during the spring of 1867 have a 1 in 500 probability of occurring (Jantunen and Ruosteenoja, 2000).

[12]Several factors are likely to have interacted with the poor harvests in sparking the first wave of mass emigration to the United States. The introduction of steam ship technology led to a shift away from sailships in the late 1860s and the cost of migration fell considerably. The US Homestead Act of 1862, which offered free land to immigrants, together with the end of the US Civil War in 1865 are also considered to have contributed to the large number of emigrants observed (Barton, 1994).

family members overseas (Sundbärg, 1913). Having emigrants in one's network reduced uncertainty and lowered the costs associated with traveling to the United States and finding an occupation once there (Runblom and Norman, 1976). Postal communication was well-developed and emigrants frequently sent home pre-paid tickets for family members to join them in America.[13] Pre-paid tickets accounted for up to half of all travelers.[14]

The mass emigration of Swedes did not go unnoticed among elites. Policies to reduce emigration were proposed throughout the period, and applied a mix of carrot and stick strategies: proposals to restrict emigration were common, as were calls for improving living standards so as to induce citizens to stay. In 1869, at the height of the first emigration wave, several motions were raised in parliament by MPs from high-emigration counties. Even at this very early stage, the awareness of and concern about emigration was high (Kälvemark, 1972). The central government later surveyed governors about their counties' experiences with emigration. A majority of governors then agreed that emigration was a net bad for the country (Kälvemark, 1972). When asked for policy proposals to reduce emigration, governors suggested measures to both make emigration more difficult and to improve the conditions in Sweden, for example by facilitating the procurement of small land plots by landless farmers.[15] However, emigration remained essentially unrestricted throughout the mass migration period.

The return of high emigration rates in Sweden in the early 20th century brought the strongest political reactions yet. Landowners and agrar-

[13]Data from Denmark, which had a much smaller number of emigrants than Sweden, has shown that up to 1.8 million letters were sent yearly to Denmark from the US (Beijbom, 1995).

[14]Studies of the archives of the Larsson Brothers emigration agency in Gothenburg have shown that around half of their clients traveled using pre-paid tickets (Runblom and Norman, 1976). Beijbom (1995) also reports that half of the Swedish emigrants traveled on pre-paid tickets at the beginning of the 1880s, and around 40 percent by the end of that decade. Pre-paid tickets also accounted for 40 percent of Norwegian travelers (Hvidt, 1975).

[15]The survey was carried out in 1882. Governors also identify family ties to emigrants as a chief determinant of emigration.

ian interest groups worried about labor scarcity and identified emigration as the main culprit.[16] Others were concerned about the emigration of young men who would otherwise perform military service, and worried about a deterioration in national defense (Kälvemark, 1972). These concerns eventually lead to the appointment of a large public commission, assigned the task of finding measures to end the mass emigration. When its 21 volume report was published in 1913, it recommended political reforms to improve the conditions of ordinary citizens to induce them to stay, rather than suggesting emigration restrictions. The large-scale emigration of Swedes ended in the 1920s, as the United States introduced quotas on immigration.

2.2.2 The labor movement and emigration

The Social Democratic Party was a dominant actor in Swedish politics during the 20th century and long garnered a near majority of votes in national elections. Founded in 1889, it entered government for the first time in 1917 and remained in government for most of the 20th century.[17] The Social Democrats were closely linked to the Swedish Trade Union Confederation (LO), founded in 1898 as a central organization for the many smaller unions that existed at the time. Both organizations championed the right to organize, the 8 hour workday and universal suffrage (Lundkvist, 1977).[18]

The labor movement was regularly in conflict with employers and

[16]Noting that landowners were less likely to emigrate, the state was encouraged to sell smaller plots of land and provide a transfer to enable poor farmers to acquire land. The plot size was a key parameter, however, as transfers were not intended to allow farmers to become self-sufficient but to remain attached to major landowners' farms. In a parliamentary debate in 1904, the Minister of Agriculture openly discussed the central point of contention: should the subsidy be so large that it allowed a farmer to be self-sufficient or should it be smaller, so that "owners would invariably need to seek employment with others in order to earn a living (Kälvemark, 1972).

[17]The party was in government between 1932 and 1976 without interruption.

[18]The 8 hour workday and universal suffrage were adopted in 1919 by a Liberal-Social Democratic coalition government. In the 1938, LO become a key player in the Swedish Labor Market Model, representing employees in collective bargaining over wages and benefits without intervention of the national government.

was known to use emigration as a tool. In Stockholm, labor unions held English courses and helped colleagues emigrate. The Social Democrats' main newspaper updated readers about prospects in the US labor market (Tedebrand, 1983). After the general strike in 1909, which was considered to be a defeat for the labor movement, a socialist newspaper called upon workers to emigrate (Beijbom, 1995). Many emigrated labor activists continued their work overseas, founding labor organizations in the United States (Nordahl, 1994; Bengston and Brook, 1999).

Emigration might have been useful for the labor movement because of the high risks involved in labor activism. Workers could be fired, evicted and blacklisted for being union members. Until 1885, an anti-loitering law made striking illegal and punishable by forced labor (Westerståhl, 1945).

A case study of the town of Ljusne elucidates the conflicted interactions between labor, elites and emigration. In 1906, more than a hundred workers emigrated from Ljusne, following a clash between the local Social Democratic club and the main employer, who owned all buildings in town and disallowed political and union organizing among workers. After the Social Democrats sent an incendiary telegram to the Swedish King, leading figures were fired while others were intimidated via the local police to stop their activities or be evicted. Rather than complying, many opted to emigrate. The option of emigrating was facilitated by the town's history of US migration – it had experienced large participation in the emigration waves of the 1860s and 1880s. Regarding the choice of emigrating rather than relocating within the country, one of the central activists later commented that "strangely enough, there were only two places for us in the world then, Ljusne or America". The news of Ljusne's "mass emigration" became widely spread in national media at the time and severely hurt the reputation of the owner and first chamber parliamentarian Count Walther von Hallwyl (Rondahl, 1985). When the plant shut down in 1907, the company announced that it would be paying pensions to older workers in gratitude for their service. The Ljusne case illustrates the use of emigration among labor activists, and indicates that the outside option of exit could serve as an insurance mechanism, seeming to empower citizens who would otherwise not dare to object to employers' demands.

2.3 Data

Emigration Data We compile local emigration histories using two distinct, individual level data sets encompassing the universe of registered emigrants during the Age of Mass Migration. The final data set contains 1.1 million emigrants from 1867 to 1920. To our knowledge, this is the first study to make use of any of these two data sources for disaggregated statistical analysis. They are described in detail below.

The State Church in Sweden was historically tasked with tracking demographic statistics in their parishes. Births, deaths, marriages as well as migration information were recorded year by year at the individual level and stored in parish records. These were later incorporated by the central statistical agency. We obtain emigration data from these parish records that were digitized by family researchers and through various municipal and county efforts.[19] Individual migrants are matched to an origin municipality and year using information on the date of exit and home parish available in the data set.

The second source of individual level emigration data is from archived passenger lists kept by shipping companies. Starting in 1869, at the peak of the first emigration wave, ships with foreign destinations were required by law to compile lists of all their passengers (Clemensson, 1996). The lists were controlled for authenticity by the police who checked off travelers as they boarded their ships. The passenger manifests were later stored in various city archives and were digitized by the Gothenburg Provincial Archives. The same matching procedure as the parish level data is used to match emigrants to origin municipalities. The data provide a less precise "home town" location rather than the exact parish, leading to lower match rates.

[19]The data are obtained from The Swedish Migration Center in Karlstad, Sweden.

Since our two data sets are independently collected and record emigrants at different points in time, they afford us a rare opportunity to measure the accuracy of our data. Appendix Section 2.11 shows that there is a high degree of within-year similarity between the data sources. This indicates a high reliability of the emigration numbers and that there is no important lag between leaving the home parish and boarding a ship to the United States.

In the remainder of the paper, we use a single emigration variable defined as the maximum of either the church book or passenger list data each year. The primary concern is in undercounting emigrants, and since the passenger list data are imperfectly matched, using the maximum value each year yields our best estimate of emigration.[20]

Election and Labor Movement Data Municipal level voting data for all national elections between 1911 and 1921 are taken from Berglund (1988).[21] The data set includes the number of eligible voters and votes cast as well as the distribution of votes across political parties.[22] Precinct-level data from municipal and national elections 1998 to 2014 are taken from the Swedish Election Authority and are geographically matched to 1865 municipality borders.

Local organization membership 1881-1945 comes from the Social Movement Archive.[23] The Social Movement Archive lists the number of members by municipality as of December 31 each year, for the following organizations: free churches, temperance lodges, labor unions and the Social Democratic Party. We group labor unions and the Social Demo-

[20]Note that after 1895, all data are necessarily from passenger lists since church books have not been digitized after that year.

[21]Provided through the Swedish National Data Service (SND).

[22]The data begin in 1911 as it was the first year when party denominations were formally required of all members of parliament. Before then, the parliament consisted of a mix of partisans and independents and partisanship was not systematically recorded. In the absence of roll-call data from the period, this makes it hard to determine the political identification of MPs before 1911. Roll call data from the Parliament were not recorded until 1927.

[23]The data were collected by historians at Uppsala University (Andrae and Lundqvist, 1998). Provided through the Swedish National Data Service (SND).

cratic Party into one variable that we label *labor movement membership.*

Participation numbers for the 1909 general strike, divided by union and non-union members, are digitized from the original government report following the strike (Kommerskollegii, 1910).

Weather Data Daily temperature data are obtained from the historical records of the Swedish Meterological and Hydrological Institute. We complement this with daily data for Norwegian weather stations near the Swedish border, provided by the Norwegian Meteorological Institute. The Swedish data contain temperature readings three times per day: 6 am, 12 pm and 8 pm. In addition, most observations have daily minimum and maximum temperatures. The Norwegian data contain daily average temperatures only. Appendix 2.11 describes how daily minimum temperatures are predicted from existing data in cases when the minimum temperature is not available.

In total, the data contain 32 unique temperature stations between 1864 and 1867, with a median distance from municipality centroids to the nearest station of 36 kilometers.[24] The relatively small number of stations could be a problem for our ability to find enough variation in weather conditions to precisely predict emigration. However, temperature is known to be evenly distributed over large areas, especially in the northern hemisphere. Rain is, by comparison, more idiosyncratic (Dell et al., 2014). Climatologists have also established that temperature deviations from long-run means are more similar over large distances as compared to levels (Hansen and Lebedeff, 1987). Intuitively, the reason for this is that even if two neighboring locations have different temperature levels, e.g. due to differences in altitude, they are likely to experience similar deviations from their long-run means within a given window of time due to common weather shocks. As our identification strategy relies on estimating shocks to weather, we are precisely interested in using deviations, allowing us to exploit this feature of the data. Section 5.4 describes how we define frost shocks in detail.

[24]The mean distance is 39 kilometers.

Additional Data In the final data set, all variables are aggregated to the municipality level using 1865 boundaries. Georeferenced data on administrative borders in 1865 are taken from the National Archives of Sweden. Proximity to an emigration port is defined as minus the log distance to either Gothenburg or Malmö, whichever is closest. The two cities were the main emigration ports during the Age of Mass Migration.[25] Population data were kindly shared by Lennart Palm (Palm, 2000). Soil suitability data (for barley, oats, wheat, livestock and forestry), used as control variables, are from the FAO GAEZ database. County-level harvest grades 1860 to 1870 are from Hellstenius (1871). The data set grades harvests yearly on a scale from 0 to 6, with higher values indicating larger yields.

Municipal level welfare expenditures and type of political institutions (direct or representative democracy) are taken from Hinnerich and Pettersson-Lidbom (2014). Mortality data for infants, children and mothers, averaged over the 1850–1859 period, are from the The Demographic Data Base, CEDAR, Umeå University. Complete decennial censuses for 1880–1920 were obtained from the National Archives of Sweden and the North Atlantic Population Project. The census gives population-wide data on demographic variables including gender, civil status, family structure, and occupation. Summary statistics are presented in Table 2.1.

2.4 Empirical Framework

Our goal is to estimate the effect of emigration over the course of the Age of Mass Migration on long-run political outcomes in origin municipalities. The cross-sectional equation of interest is

$$y_{mct} = \beta Emigration_{mct} + \phi_c + \mathbf{X}'_{mc}\beta_X + \varepsilon_{mct}, \tag{2.1}$$

[25] All distances are calculated using the great circle haversine formula. The results are robust to excluding lakes and waterways between municipalities and Gothenburg or Malmö. Figure 2.6 shows that the proximity to Gothenburg and Malmö is well approximated by a straight line for most locations in Sweden.

where y_{mct} is a political outcome in municipality m, county c and year t, $Emigration_{mct}$ is the log of cumulated emigration from 1867 to year t, ϕ_c is a fixed effect for the 24 counties and $\mathbf{X}_{mc}$ is a vector of municipality characteristics determined before the start of mass emigration. The specification focuses on the stock of emigrants as a determinant of political outcomes, capturing the extent of overseas social networks present in a municipality and, hence, the ease of future migration for current citizens. Throughout the paper, we estimate (2.1) by OLS as a baseline and reference for comparing other estimates, always including the log of population in 1865 in $\mathbf{X}_{mc}$ in order to scale the level of emigrants to the initial municipality size.

For several reasons, long-run emigration histories can be expected to correlate with important characteristics of the origin municipality, either observable or unobservable, that can have a direct impact on the outcomes of interest. A strong concern in estimating (2.1) by OLS is hence that it may yield biased estimates of the effects of emigration. In particular, the risk of picking up reverse causation is high. Locations with favorable initial institutions may induce more emigration because of better access to information or higher incomes. By contrast, places with more repressive leaders might actively inhibit emigration, thus leading to a positive bias in the OLS estimate of β. In the abstract, the reverse situation is, however, equally likely: fewer people may want to leave locations with good institutions and bad institutions could act as a push factor for emigrating. Without the ability to quantify the relative importance of these effects, OLS estimates yield limited information about the causal effect of emigration on local politics.

To overcome the issues related to omitted variables and to consistently estimate parameters, we propose an identification strategy exploiting only migration-related push factors prior to the first wave of mass emigration: the interaction between growing-season frost shocks 1864–1867 and the proximity from a municipality to the nearest of the two main emigration ports. The remainder of the section describes how we construct frost shocks and presents the instrumental variables strategy in more detail.

Frost shocks The empirical economics literature often uses rainfall as source of exogenous variation in income for developing countries, motivated by the idea that rainfall has a direct effect on crop yields. Somewhat less attention has been given to the importance of temperature variation. However, low temperatures and frost in particular are closely linked to agricultural outcomes in non-tropical climates (Snyder and Melo-Abreu, 2005). Frost has severe effects on crop growth and the likelihood of plant death. In the United States, more economic losses are caused by freezing of crops than by any other weather hazard (White and Haas, 1975). The perniciousness of frost is linked to its non-linear effects once temperatures fall below zero degrees Celsius. One night of freezing temperatures can lead to a complete crop loss (Snyder and Melo-Abreu, 2005). As mentioned in Section 5.2, the poor harvests in Sweden in the 1860s occurred during years with unusually cold temperatures in the growing season. Throughout Sweden, frost was observed as late as in June, in the middle of the growing season for most municipalities in our data. Estimating the incidence of frost is difficult, however, as it does not only require daily data but also estimates of the *minimum* temperature at a daily resolution.

Our measure of frost shocks follows the approach of Harari and La Ferrara (2013). It defines a binary shock indicator by month, and expresses shocks relative to the local long-run weather in that particular month. Shocks are constructed as follows. For each month r, we calculate the total number of frost days, defined as days with a minimum daily temperature below zero degrees Celsius. At the weather station level, we compute a series of monthly deviations from the mean,

$$deviation(Frost\ Days)_{srt} = Frost\ Days_{srt} - \overline{Frost\ Days}_{sr},$$

where $\overline{Frost\ Days}_{sr}$ is the long-term mean of frost days per calendar month r at station s. Each municipality is then matched with the nearest station available in each month.[26] This is used to compute the municipality's long-term standard deviation of frost days in each month, $sd(Frost\ Days)_{mr}$. A monthly frost shock at the municipality level is then defined as a binary variable:

$$Shock_{msrt} \equiv I[deviation(Frost\ Days)_{srt} > sd(Frost\ Days_{mr})], \quad (2.2)$$

where $Shock_{msrt}$ is an indicator equal to one if municipality m, whose nearest station is s, experienced a positive frost shock in month r of year t. Note that we compute the deviation from the long-term mean at the *weather station* level rather than the municipality level. This exploits the fact that weather variables are more precisely interpolated in deviations from long-term means than in levels, as discussed in Section 5.3 (Hansen and Lebedeff, 1987). Given that we are exactly interested in anomalous temperature variation, this feature increases accuracy of our measures. Finally, we sum the number of shocks over the growing season for each municipality over the 1864-1867 period. A growing season month is defined as a month with a long-term mean temperature above 3 degrees Celsius, following guidelines of the Swedish Meteorological and Hydrological Institute. The frequency distribution of frost shocks 1864–1867 is displayed in Figure 3.3. As evidenced by the figure, this period saw a high incidence of cold temperatures in the growing season, with the median municipality experiencing three frost shocks. Figure 2.6 displays the spatial distribution of growing season frost shocks 1864–1867, indicating considerable variation in shocks across Sweden.

Identification strategy In order to consistently estimate β in (2.1), we instrument for emigration using the number of growing season frost

[26] Enough variation is captured by the nearest station that using more weather stations (e.g. the second and third nearest ones) does not contribute any additional information. In our data, the adjusted R^2 from regressing monthly frost days at weather station s on frost in the nearest neighboring weather station is slightly lower when we add the frost of the second nearest weather station.

shocks 1864–1867 interacted with the proximity to the nearest emigration port. We only exploit shocks occurring during this four-year period as it was bookended by a particularly high incidence of cold temperatures, with shocks rarely occurring in other years of the decade. The direct effects of frost shocks and port proximity are used as controls. The first-stage equation is

$$\begin{aligned} Emigration_{mct} &= \gamma_1 Shocks_{mc} + \gamma_2 Port_{mc} \\ &\quad + \gamma_3 Shock_{mc} \times Port_{mc} + \theta_c + \mathbf{X}'_{mc}\gamma_X + v_{mct}, \end{aligned}$$

where $Emigration_{mct}$ is the log of cumulated emigration from 1867 to year t in municipality m, $Shocks_{mc}$ is the number of frost shocks experienced prior to the first wave of emigration, $Port_{mc}$ is the proximity to the nearest emigration port and θ_c is a county fixed effect. Because frost shocks are constructed to capture random variation with respect to fixed municipality characteristics, the coefficient of interest, γ_3, is estimated without bias.[27]

Proximity to emigration port is defined as minus the log of the shortest distance to either Gothenburg or Malmö, the two main emigration ports.[28] Likely due to economies of scale, the points of exit were very concentrated, and between them the cities handled more than 95 percent of all emigration before 1920. Their importance is confirmed by comparing yearly emigration shares across ports.[29] Figure 2.4 displays the share of emigrants exiting through four ports over the period 1869 to 1920. Gothenburg was the biggest port by far throughout the period, with 79 percent of all traffic on average and about 82 percent during the first wave of emigration. Malmö was the second largest emigration port with 18 percent of emigrants on average and 14 percent during the first wave.[30] Stockholm, the capital and Sweden's largest city by far, was

[27]This implication is tested below.

[28]All results are robust to using levels of distance instead of logs, see Appendix Table B.8.

[29]Shares are computed using the passenger list data, which includes the port of exit for all emigrants.

[30]The data distinguish between emigrants from Malmö and Copenhagen. Due to

less suited for emigration because of its location on the eastern coast of Sweden. Its port averaged 2 percent of total emigrants. Similarly, Norrköping, the third largest city and an important trade port, was minor in terms of emigration.[31] In our data set, 75 percent of municipalities have Gothenburg as their closest emigration port, while the rest are closer to Malmö.

Exclusion restriction The identification strategy only relies on the *interaction term* of frost shocks and port proximity. This has two main advantages. First, a basic cost-benefit analysis would suggest that potential migrants let the cost of traveling to the emigration port factor into their decision. By implication, including it in the empirical model should improve its explanatory value.[32] Second, and perhaps more importantly, it allows us to control for the direct effects of proximity to the port as well as the frost shocks themselves. A typical complication in studies that use weather shocks as instruments for some endogenous variable is that weather may simultaneously affect many variables, including citizens' values and attitudes (Giuliano and Spilimbergo, 2014).[33] Hence, there are potential direct effects of the shocks on our variables of interest, which would violate the exclusion restriction and invalidate the use of the shocks as an instrument. By using only the interaction term, we are able to isolate exogenous variation in shocks that is solely related to migration push factors.

For the identifying assumptions to hold, it is nevertheless required that no variables other than emigration correlate with the instrument. We test this by performing balance tests of the instrument on a number

their geographical proximity and because most emigrants likely transited via Malmö before being registered in Copenhagen, we count the two exit ports as one unit.

[31] Gothenburg and Malmö were the second and fourth largest cities in 1865, respectively.

[32] Beijbom (1995) highlights the importance of travel possibilities, noting that the northern regions of Sweden were hit hard by the bad weather in the famine years, while most emigrants came from southern Sweden.

[33] For example, Sarsons (2015) shows that rainfall might have effects on conflict through other channels than agricultural yields, invalidating its use as an instrument for income.

of observable characteristics of municipalities. Table 2.2 displays the outcome of these tests. The instrument is uncorrelated with all variables but one, log population in 1865. By random chance, we should expect some variable to be correlated with the instrument. Yet, it is reassuring that the correlation predicts that high-emigration municipalities have *lower* baseline population levels, while it is expected that larger municipalities are more politically organized.[34] Nevertheless, the 1865 population is included in all regressions as a control. We include the following additional control variables: log area, latitude, longitude, the share of arable land in 1810, an urban indicator, as well as indicators for high soil suitability for the production of barley, oats, wheat, dairy and lumber. We also include the following proximity measures, all in logarithms: to the nearest weather station, to the nearest railway, to Stockholm, to the nearest town and to the nearest of the ten most important trade ports in 1890[35]. The three mortality variables at the bottom of Table 2.2 are not included as control variables in our regressions due to a lower number of observations. They provide relevant tests of the instrument, however, as they directly relate to municipal policy and wellbeing.[36] Reassuringly, the instrument is not correlated with any measure of mortality, for infants, children or mothers.

2.5 Frost shocks, travel cost and emigration

Frost and agricultural outcomes Before investigating the link between the instrument and emigration, we verify the effect of frost shocks on agriculture using a panel of county-level harvest grades from 1860 to 1870. Column 1 of Table 2.3 shows that frost shocks in the growing season indeed cause worse harvests in the same year. A standard

[34]Indeed, OLS regressions show that the population in 1865 is weakly positively correlated with labor organization rates and welfare spending, while it is unrelated to support for left-wing parties.

[35]As before, we define proximity as minus one times the log of the distance.

[36]Maternal mortality was partially a function of local policies, as midwives were employed by parishes (Pettersson-Lidbom, 2009)

deviation increase in frost shocks causes a 17 percentage point higher probability of crop failure, an increase of about 0.8 standard deviations. The result is robust to the inclusion of fixed effects for counties as well as county-specific linear trends. Column 2 provides evidence that the distinction between growing and non-growing seasons is crucial, as shocks that occur in the non-growing season have a near-zero and insignificant effect on harvests. If emigration was indeed caused by poor agricultural yields, we should expect to find the same pattern when emigration is the dependent variable. Columns 3 and 4 re-estimate the specifications in the first two columns using the full 0–6 scale of harvest grades, with results displaying the same pattern.

First stage Path dependency in migration patterns has been well established in the migration literature.[37] Historical accounts of the Swedish experience indicate similar patterns of chain migration. Figure 2.7 uses our emigration data set to graphically evaluate this pattern. Panel A plots the spatial distribution of emigration rates during the first wave of emigration 1867–1879, while Panel B displays emigration in the whole 1867–1920 period. Comparing the raw data between the two maps reveals a substantial correlation in the propensity to emigrate over time. This is consistent with the fact that up 50 percent of emigrants traveled on pre-paid tickets sent by network members in the US (Runblom and Norman, 1976; Beijbom, 1995). Figure 2.8 also displays the relationship between early and later emigration in a scatter plot, which displays a strong positive correlation.[38]

With this in mind, Table 2.4 estimates how emigration over the full sample period is related to growing season frost shocks 1864–1867, proximity to the nearest emigration port and our instrument: the interaction between the two. The results in Column 1 are in line with our expectations — over the 1867–1920 period, municipalities that are closer to a

[37]See e.g. Massey et al. (1993); Hatton and Williamson (2002); Munshi (2003); McKenzie and Rapoport (2007); Bryan et al. (2014); Giulietti et al. (2014).

[38]The next subsection also tests for path dependency causally.

port emigrate more in response to an additional frost shock. As proximity and shock variables are demeaned, the estimates show that municipalities that are one standard deviation closer to ports emigrate by an additional 6.3 percent given a frost shock, while the effect at the mean proximity is weakly positive but insignificant.[39] This result is robust to the inclusion of pre-determined control variables in Column 2.

If the proximity to an emigration port proxies for the market access of a municipality, a potential concern could be that the instrument is associated with the differential impact of experiencing shocks in more or less connected areas. This could lead to violations of the exclusion restriction in instrumental variables estimations below. To control for this possibility, Column 3 includes our two measures of market access, the proximity to nearest town and the proximity to nearest major trade port, interacted with frost shocks. The coefficient on the instrument is not sensitive to this inclusion. The interaction terms themselves are also not significantly different from zero. Frost shocks therefore only affect emigration when interacted with travel costs, indicating that the instrument captures only migration-related push factors at the onset of mass emigration.

To provide support for the claim that frost shocks affect emigration through their impact on the agricultural sector, Column 4 additionally includes non-growing season frost shocks and their interaction with port proximity.[40] The coefficient of the interaction term is substantially smaller and statistically indistinguishable from zero, thus mirroring the null effect found for agricultural outcomes in Table 2.3. The variation picked up by the growing season shocks therefore identifies economically meaningful events and not spurious correlations with underlying variables at the municipality, as captured by the proximity to emigration

[39]This is consistent with the theory that individuals take the internal migration cost into account in their decision to emigrate. E.g. Morten and Oliveira (2014) find that individuals with a shorter road distance to the new city of Brasilia were more likely to migrate and take advantage of the comparatively high wages offered there.

[40]Non-growing season frost shocks over the period are defined analogously to growing season frost shocks.

ports.

Figure 2.9 displays the first-stage relationship non-parametrically. In Panel A, residuals of log emigration 1867–1920 and the instrument are plotted after controlling for the full set of covariates. Municipalities are collected in 50 groups of equal size, with dots representing the mean value in each group. The figure shows that across the whole range of the instrument, observations are clustered near the regression line.[41] The even distribution of group means indicates that there is compliance with the instrument at all values and that the linear specification is an appropriate model. In Panel B, we display the effect of the placebo instrument on migration using the specification in Column 4 of Table 2.4. As expected, the figure shows that emigration has no apparent relationship with the placebo instrument, whether linear or non-linear.

Early migration and future mobility Having established the importance of the initial frost shocks for emigration over the whole mass migration period, we next investigate two different ways in which early emigration affected future mobility and migration patterns. First, we divide the data into first and later waves and estimate the elasticity of later emigration with respect to early migration. Panel A of Table 2.5 estimates the effect of the instrument on first-wave emigration, 1867–1879. The results in Columns 1 to 3 indicate the same pattern as that found in Table 2.4: locations that experienced frost shocks closer to a port had more emigration. In Panel B of Table 2.5, we use the relationship in Panel A as the first stage for estimating the causal effect of early emigration on later waves. The coefficients in Columns 1 to 3 show that there is a strong, causal pattern of path dependency, with an intertemporal elasticity of emigration near unity. Thus, these results confirm the canonical finding in the migration literature of strong path dependence in migration patterns referred to earlier. Interestingly, the IV coefficients

[41] The slope of the regression line corresponds to the estimate in Column 3 of Table 2.4.

are greater in magnitude than their OLS counterparts.[42] This may be due to measurement error in emigration levels, since unregistered emigration was more common before 1884, when a new law made it harder to emigrate without proper documentation. The larger coefficients may also reflect the estimation of a different parameter between OLS and IV, if the instrument causes different types of individuals to emigrate.[43]

As a second test of mobility, we examine if municipalities with more early emigration were differentially likely to take advantage of favorable economic conditions in the United States. In Table 2.6, we use panel variation in emigration 1880–1920 and interact the instrument with a measure of the relative prosperity of the United States compared to Sweden. Exploiting panel variation, we are able to include municipality and year fixed effects. Column 1 shows that locations with more early emigration (high values of the instrument) are more likely to emigrate when the difference between US and Swedish GDP is larger. Columns 2 and 3 include additional controls for linear trends in the three major regions of Sweden as well as for a number of baseline municipal characteristics. The estimates remain significant and show the same effect. Table 2.6 then highlights a different channel through which early emigration lead to higher future mobility. The results suggest that overseas networks, as proxied by early emigration from the municipality, provided an option to emigrate and that this option was specifically exercised when the conditions were most favorable to do so. It also suggests that living standards were an important concern for potential emigrants, which would have to be taken into account by local elites.

To get a fuller picture of the relationship between emigration, frost shocks and within-country travel costs, Appendix Table A.2 presents additional estimates using the panel variation in emigration and frost shocks. Again, with municipality and year fixed effects we cancel out

[42] This is similar to estimates in McKenzie and Rapoport (2007).

[43] For example, liquidity constrained individuals should be more likely to emigrate as a response to the reduced migration cost of having a relative already in the US. If our instrument causes a higher fraction of poor people to emigrate than would otherwise have been the case, the incidence of chain migration could also be higher.

any potential biases related to fixed municipality characteristics that could potentially influence the cross-sectional relationship that we observe. The results follow the same pattern as in the cross-section models, with yearly growing season frost shocks leading to more emigration and a larger marginal effect of shocks as the proximity to an emigration port increases. This holds true only during the first wave of emigration, however. For the later period of emigration, 1880 to 1920, neither frost shocks nor the interaction with proximity to emigration port matter. The importance of both frost shocks and port proximity hence diminishes over time, perhaps as infrastructure improves and the economy shifts away from agriculture towards manufacturing. Both variables that compose our instrument can thus be thought of as only capturing variation that was relevant during the first wave of mass emigration.[44]

2.6 Emigration and citizens' demand for political change

This section estimates the effect of emigration on citizens' demand for political change across Swedish municipalities. The main variable of interest is membership in the labor movement, given that unions and the Social Democratic Party were the strongest proponents of political change during our period of study and were directly involved in conflicts with economic and political elites at local and national levels.

Labor movement membership The Social Democratic Party was founded in 1889. In the preceding decade, modern labor unions became more widespread, ultimately leading to the formation of the Swedish Confederation of Trade Unions in 1898. In Figure 2.10, we trace out the impact of emigration on municipal labor movement membership rates starting in this period and ranging until 1920. The figure displays IV coefficients from separate regressions in five-year intervals 1890–1920,

[44]In Section 5.6, we test the interaction between the proximity to emigration ports and frost shocks occurring during all four-year periods other than 1864-1867.

including the full set of controls, with bars representing 95 percent confidence intervals. In the earliest years of the labor movement, 1890 to 1895, the IV estimates are insignificant and close to zero, albeit with a positive sign. Starting in 1900, however, emigration has a clear positive and statistically significant effect on labor organization rates. The effect sizes show an increasing trend, which mirrors the general positive trend in membership rates in the period. The figure provides the first evidence of a positive causal effect of emigration on local labor organization.

To get an aggregate picture of the relationship, Table 2.7 reports regression results using the average labor movement membership rate between 1900 and 1920 as the dependent variable. Panel A shows first-stage and reduced-form estimates, while Panel B displays OLS and IV results. The first-stage results are similar to those explored in the previous section, indicating a positive relationship between the instrument and emigration in the period 1867–1900. In the two specifications in Columns 4 and 5 of Panel B, the estimated IV coefficients are strongly significant and stable at approximately 0.02, including when we control for pre-determined municipal characteristics. Column 6 includes the two market access interactions, using proximity to the nearest trade port and town to control for potential violations of the exclusion restriction. The point estimate is robust to this inclusion and remains significant at the 1 percent level, indicating that frost shocks did not have any important differential effects between locations that were more or less connected to markets.

The point estimates are large. The preferred estimate in Column 6 suggests that a municipality which doubles its emigration over a 30-year period increases local labor movement membership by 2.3 percentage points. The effect size corresponds to moving a municipality from the mean to the 90^{th} percentile of the distribution of membership rates, or approximately 0.6 standard deviations. The IV coefficients are also just over twice as large as the corresponding OLS estimates. The difference implies a downward bias in OLS and that, if anything, OLS estimates provide a lower bound on the effect of emigration on labor movement size. A possible reason for this is that emigration was more common in regions that were also less likely to develop labor organizations, perhaps where landlords and employers were particularly powerful. This would be consistent with bad institutions acting as a push factor for emigrants. Measurement error in emigration may in addition be contributing to the difference in estimates.

These results provide evidence of a strong positive effect of emigration on membership in the Swedish labor movement. The fact that the effect is present in its initial growth phase, during which it established itself as an important political player, provides a novel explanation for the well-known strong position of labor unions in Sweden.[45] These findings thus contrast with the hypothesis that vocal political dissidents would emigrate and decrease the level of activism in origin communities. It instead lends support to the hypothesis that emigration increased the pool of activists over time. A possible explanation for this is that established migrant networks improved stayers' outside options, acting as an insurance for them to organize despite the risk of repression by elites. Another, complementary, interpretation is that higher labor mobility increased the responsiveness of elites to citizens' demands, which raised the incentives for collective action and by consequence also organization rates.

To verify the robustness of our results, we also graphically display nonparametric estimates of the first stage and reduced-form relationships. Figure 2.11 plots the instrument against emigration 1867-1900 (the first stage) and labor movement membership 1900-1920 (the reduced-form). All variables are residualized using the full set of covariates. We see that both outcomes are positively correlated with the instrument across the entire range of its values. Taken together, these results imply a positive relationship between emigration and labor movement membership, summarizing the main result of this section.

To further probe the effects on demand for political change, we study

[45] In 2000, Sweden had the second highest trade union density among OECD countries, behind Iceland.

the effect of emigration on a direct, costly action directed towards employers. In response to a downturn in the business cycle in 1909, the Swedish Employers Association sought to lower workers' wages. Anticipating opposition by labor organizations, it enacted a lockout of thousands of workers in order to force acceptance from the unions. The Swedish Confederation of Trade Unions instead responded by calling a general strike, affecting 300,000 laborers who halted work for three months. Using data on strike participation by municipality, we estimate the effect of emigration on mobilization of workers in Table 2.8. If our estimated effect on labor movement size indeed captures a greater ability to organize and mobilize citizens, we should expect high-emigration municipalities to display greater participation in the strike. This is confirmed by the IV result in Column 1, which shows a positive and significant effect. Similar to the case with labor movement membership, the estimated effect is large and indicates that a doubling of emigration increases strike participation by 0.7 standard deviations. Membership in the labor movement was not only ceremonial then, but also resulted in effective collective action.

Separating strikers by union membership, we can define the share of *unionized* strikers as a more direct indicator for the extent to which the labor movement was the mechanism behind strike participation. This variable is constructed to equal zero for locations with no strikers, while it takes on negative values where non-unionized strikers were more common and positive values where union members were a larger fraction of strikers. As a result, a statistically significant estimate indicates that emigration causes more strike participation, while the sign of the coefficient shows which group that was most common. Column 2 of Table 2.8 indicates that emigration indeed causes a greater share of union members among strikers. Approximately 9 percent of municipalities that participated in the strike had more non-unionized than unionized strikers. As the so-called "striking weapon" was the most common tool available for the political and economic protest, this finding suggests that emigration developed a stronger bargaining position of citizens, through its effect on the labor movement.[46]

The labor unions that we observe in our data were almost exclusively organized in non-agricultural sectors. If emigration was concentrated among agricultural workers, who were more directly affected by the shocks used in constructing the instrument, a worry is that the effects on labor movement membership and strike participation in Tables 2.7 and 2.8 could potentially be a mechanical result of agriculture-skewed emigration. To test for this possibility, we use employment data from the 1910 census and rerun regressions for the effect of emigration on labor movement and strike participation expressed *per industrial worker* rather than per capita. This specification will net out any changes in the sectoral composition of employment. Columns 3 and 4 of Table 2.8 show the results of this test. Both variables are positive and significant, indicating that the main conclusions are robust to this variation. Hence, holding the number of industrial workers constant, labor organization in 1910 as well as strike participation in 1909 were still more intense in high-emigration areas.

Electoral effects The Social Democratic Party had strong ties with labor unions, and the central Confederation of Trade Unions in particular, each side making up one leg of the Swedish labor movement. The greater local membership of labor unions made them interesting for the Social Democrats, who saw a way of expanding the local penetration of socialist ideas. Unions indeed participated in election campaigns for the Social Democrats and a large fraction of voters for the left are thought to have come from labor union members (Westerståhl, 1945). Having established that emigration increased labor organization and striking, we therefore proceed to test if the relationship also extends to electoral mobilization. For this purpose, we look at turnout rates and support

[46] While the 1909 general strike was not considered a victory for the labor movement, strikes often resulted in favorable outcomes for workers. Summary evidence on 748 strikes 1863-1902 found that strikes resulted in concessions to workers' demands in 47 percent of the cases, while 32 percent of the cases ended in a compromise and only 20 percent sided with the employers (Kommerskollegii Arbetsstatistik E:1, 1909).

for left-wing parties in national elections between 1911 and 1921. This period ranges from the first election with mandatory party affiliations to the first election with universal suffrage.[47]

Figure 2.12 displays the IV coefficients of emigration on the vote share of the Social Democrats and Socialists across these elections. Emigration lead to significantly greater support for left-wing parties. The effect is strongest in the earliest elections, possibly indicating catch up among low-emigration municipalities over time as suffrage was gradually expanded. Aggregating the two left-wing parties, Table 2.9 reports regression results for the effect of emigration on the average vote share of the Social Democratic and Socialist parties between 1911 and 1921. The IV estimates in Columns 2 to 4 range from 0.115 to 0.128, implying that an increase in emigration by 10 percent increased the vote share of the left by approximately 1.2–1.3 percentage points. Similar to the case of labor movement membership, the IV estimates are larger than OLS. Even after adding controls for baseline characteristics, the OLS estimates are smaller, implying the presence of unobserved factors that yield a downward bias in OLS.

Until 1921, voting eligibility was reserved for men who had payed their taxes, who were not in poverty care or bankruptcy and who had performed their military service. These restrictions disenfranchised one fifth of otherwise voting eligible men (Grenholm et al., 1985). Nevertheless, even during a time when only relatively well-off men could vote, there is a shift in party preferences toward left-wing parties.[48]

It is also relevant to note that these results take into account any

[47] In addition, data on municipal elections would have been informative, because the weighted voting scheme present in local elections until 1919 would have given an indication of how elites' preferences were affected by emigration. It would also have been directly relevant for municipal policy. Unfortunately, such data are unavailable to us.

[48] It is difficult to distinguish how much of this change is due to an increased popularity among working-class voters and how much is due to elites shifting their voting towards parties that would be more popular among average citizens. Given that one-man, one-vote was used in national elections, however, the effects are unlikely to be driven mostly by the voting of elites.

potential changes in voter turnout due to emigration, as the left-wing vote share is computed using the total number of votes as the denominator. The increase in the vote share of left-wing parties is therefore not simply explained by an increased mobilization of poor voters, but is due to a differential voting behavior among voters.

Finally, Columns 5 to 8 of Table 2.9 display regression results for the effect of emigration on average voter turnout during the period. We find positive effects on turnout, with the IV results in Panel B ranging from 0.074 to 0.082 in the preferred specification with market access interactions. The effect sizes indicate that a doubling of emigration increases voter turnout by approximately 7–8 percentage points, from an average of 60 percent during the period. This result suggests a complementary role of labor organization and voting, in line with the goal of the Social Democratic Party of using local organizations to mobilize citizens for larger, national-wide political change.

2.7 Alternative mechanisms

We have emphasized the role of improving outside options in high-emigration municipalities leading to a stronger labor movement and political change. This section considers additional alternative explanations for our results.

Selection into migration Selection effects are a first-order concern when studying migration. If those who choose to emigrate are very different from those who stay behind, migration may change the composition of the origin community population substantially over time. This could itself have direct, more or less mechanical effects on our outcomes of interest. It thus constitutes a competing explanation for our results, one which does not imply any changes in behavior. While we have already seen that labor movement membership was not solely driven by agriculture-skewed emigration, we here consider several additional selection mechanisms.

Research on Norwegian migrants during the Age of Mass Migration has found that migrant self-selection in terms of earnings potential was negative from urban areas, but ambiguous from rural areas (Abramitzky et al., 2012). Given that most of the variation used in this study is from rural areas, as well as the fact that the majority of emigrants were rural, our results should not be substantially affected by this form of self-selection. To verify this, we replicate the effect of emigration on labor movement participation only using the rural sample. Appendix Table B.2 shows that our findings on labor movement membership, striking and voting are robust to the exclusion of urban areas.[49] All point estimates remain significant, and point estimates are roughly similar, with two being higher and two being lower than the main estimates.

Data on the share of eligible voter allow us to test for a certain type of selection effect, that could explain our results, in a more direct way. Given that voting eligibility was based on economic status and gender, it can serve as an indicator of changes to the composition of the population that has direct bearing on electoral outcomes. However, Columns 9 to 12 of Table 2.9 indicates that emigration had no significant effect on the share of eligible voters. Moreover, the sign of the estimated changes from positive without additional control variables, to negative when we include controls. As a result, the effects on voting patterns are thus cannot be explained by this form of selection.

Selection may also be active along other dimensions than income or voting eligibility. Using 1910 census data, we test for a wider range of demographic differences across high and low emigration municipalities. Table 2.10 tests the effect of emigration on a number of indicators of demographic change. Column 1 shows that there is no differential in-migration from other regions of Sweden in municipalities with more emigration, ruling out e.g. welfare migration and selective in-migration of more leftist individuals. Columns 2 and 3 test for evidence of a fertility transition related to emigration. However, we find no effect on the

[49] Note that regressions on welfare expenditures and representative democracy are already restricted to the rural sample.

average family size, nor on the incidence of unmarried adults. Finally, Column 4 shows that the ratio of women to men is not affected by emigration. Estimated coefficients are all relatively small compared to their mean. Hence, low power should not be the reason for failing to reject the null hypothesis.

Lastly, we consider the possibility that emigrants were ideologically selected. If more right-leaning individuals chose to emigrate, for example because of the pull factors of more freedom or because of a more risk-taking or entrepreneurial preferences, the pool of voters would mechanically change in favor of the left. To deal with this concern, we perform a simple bounding exercise. First, we count the number of emigrants 1867–1910 and assume that they would have voted in all elections 1911–1921. We then consider three scenarios for the ideological selection of emigrants. Appendix Table B.3 displays the sensitivity of our estimates for the left's vote share when assuming that 75 percent, 90 percent or 100 percent of emigrants would have voted for a non-left party if they had stayed in Sweden. As to be expected, point estimates becomes successively smaller as we assume a more skewed ideological selection, reducing the baseline result by up to about half. Nevertheless, all results remain sizable and statistically significant, indicating that such selection cannot explain the entire effect of emigration on left-wing voting.

Overall, these results are consistent with the view represented in Runblom and Norman (1976) that the mass migration became "general and popular", and hence that individuals who chose to emigrate to the US were not substantially different to the general population.

Exposure to American attitudes Existing studies linking migration and political outcomes have emphasized the potential of host country attitudes being transmitted to origin countries, and thereby potentially affecting political outcomes. This raises the question of whether American attitudes could have inspired the Swedish labor movement, whether it be via return migration or information transmission through social networks. We provide an indirect test of the hypothesis of such a cultural transmission effect by estimating the impact of emigration on two other voluntary associations that we observe in the data: non-state free churches and temperance lodges. Both types of organizations had strong influences in the United States. Methodists were common among the free churches and the temperance movement did largely consist of Swedish chapters of an American organization, the International Order of Good Templars (IOGT).

Table 2.11 displays our results for per capita membership in both types of organizations. Similar to the specification for the labor movement, we consider the average membership between 1900 and 1920. If there was transmission of information or attitudes to Swedes through their overseas networks, one would expect to see increased participation in these types of organizations. The results in Table 2.11 show no positive effect, however. Free churches do not see any significant change in membership with more emigration and temperance lodges experience a negative effect. These results do not rule out that the labor movement was in some way influenced by the United States, but they nevertheless suggest that cultural transmission effect through migrants was not a major factor.

2.8 Emigration and local government policy

The results in the previous sections show that emigration increased the political organization, mobilization and, arguably, improved the outside option of citizens during the Age of Mass Migration. In this section, we turn to analyzing whether these changes were also reflected in the local government policy making, by looking at welfare expenditures and local political institutions.

Welfare expenditures We use welfare expenditures as a measure of redistributive, pro-citizen actions taken by local governments. The choice of expenditures can also be seen as an equilibrium outcome of bargaining between elites, who hold political power, and citizens. We study per capita expenditures on welfare in 1918, one year before democratization, and in 1919, immediately afterwards. In 1918, municipal voting was restricted by wealth, income and property ownership. Votes were also weighted by a factor of up to 40 in favor of richer voters (Nilsson, 2008).[50] As a result, formal authority over spending levels was heavily biased in favor of economic elites in 1918. Changes in policy at this time are thus reflective of their choices rather than those of common citizens. This can be quantified by comparing average welfare spending before and after democracy: in 1918 it was 2.42 SEK per capita while it rose by 13 percent to 2.74 SEK in 1919 as ordinary citizens could vote.[51] Nevertheless, it is possible that citizens could wield some influence on the welfare spending decisions of elites before democracy.

Table 2.12 displays our results. Column 2 shows that emigration leads to significantly higher per capita expenditure in 1918, one year before democracy was introduced. The estimate remains stable as we include baseline controls (Column 3) as well as the market access interactions to control for potential violations of the exclusion restriction (Column 4).

How could welfare expenditures rise even before ordinary citizens could vote? A potential mechanism is that Social Democrats and labor representatives were allowed positions in municipal governance. By 1917, several municipalities had representatives from the labor movement present in local administration (Östberg, 1995). Social Democrats could also be voted into formal political power by being given a place on the election lists of other, more popular parties which sought to increase their representativeness (Lundkvist, 1977).

The effects on welfare spending remain in 1919, as voting rights were extended on an equal basis. The estimates are higher than in 1918, and are also robust to the inclusion of controls in Columns 7 and 8.

[50] In 1905, 1 percent of the rural population held as many votes as the remaining 99 percent (Nilsson, 2008). The cap on votes was 1000 at that time, however, rather than 40.

[51] Expenditure data are deflated by CPI.

The estimate in Column 8 indicates that a doubling of emigration leads to approximately 1.1 SEK higher expenditures per capita in 1919, an increase of 40 percent over the mean. Overall, both before and after citizens had the formal power of affecting welfare policy in municipalities, emigration thus lead to higher levels of redistribution.

Form of democracy in local governments During this period, rural local governments could adopt two different institutions for decision making, direct or representative democracy. In direct democratic municipalities, public town meetings would be held at least three times a year to decide on economic matters. Deliberations were open, as well as many votes. By contrast, in municipalities of the representative type, eligible citizens voted for their party of choice in closed elections. Starting in 1919, there was an assignment rule dictating that municipalities with more than 1500 inhabitants adopt the representative form of government, whereas those below the threshold were free to choose between the two. In practice, however, only a small fraction of municipalities chose the representative form voluntarily. Hinnerich and Pettersson-Lidbom (2014) study the effects of these institutions in detail. They find that direct democracies implement substantially lower levels of welfare spending per capita, potentially due to direct democracies being more easily captured by elites. This is partly seen by the low attendance rate at town meetings, 12 percent, whereas voter turnout in national elections was routinely above 50 percent. The choice of institution was then to a large extent a choice about its inclusiveness, the relative power of elites and the amount of redistribution. This may, in turn, explain the low rate of voluntary transitions from direct democracy, which was the default, to representative democracy.

We use data on the local form of democracy to test for the effect of emigration on institutional change. This is done by coding a dummy variable taking the value of 1 if the municipality was a representative democracy by 1919 or 1938, and had a population of 1500 or less in the preceding year. The last condition is included to take into account only voluntary transitions from direct to representative government. We take this measure to be an indicator of the inclusiveness of local political institutions. In addition, we include indicators for a municipality having ever crossed the population threshold in the preceding years.

Panel B of Table 2.13 shows that high emigration municipalities were indeed more likely to adopt the more inclusive form of democracy in their local governments. The effects are statistically significant in 1919, the first year of the new assignment rule, with the coefficient for our preferred specification implying a 4.7 percentage point increase in the likelihood of a representative democracy from a doubling of emigration. In 1938, when a larger share of municipalities had transitioned voluntarily, the effect is larger. Transitions were hence more common in the longer run, possibly reflecting that organized citizens gained more influence over time.

How should we interpret these effects? An important institutional feature was that municipalities that switched to representative democracy were required to keep the institution for at least five years. Reversions back to direct democracy were rare, however. An interesting question for interpreting these results is to what extent these institutional changes represent elites' concessions to citizens, versus citizens' own enforcement of their preferences. While we only observe transitions between political regimes after the introduction of one man, one vote, it is not necessarily the case that ordinary citizens held complete *de facto* political power in rural municipalities. Some elites were able to maintain important positions of power even after 1919. Moreover, electoral competition was generally limited, with 30 percent of the elections only having one party in 1919 (see Hinnerich and Pettersson-Lidbom, 2014, and references therein). While the preferences of citizens should more directly affect outcomes after the introduction of local democracy in 1919, our results may therefore still reflect the outcome of bargaining between elites and citizens. This may especially be the case as the default institution was direct democracy, which had been restricted to wealthy citizens for decades. Observing that emigration leads to the adoption of persistently more inclusive institutions may reflect a strategy of elites to commit to more pro-citizen policies by reforming the basic rules of the game, as suggested by Acemoglu and Robinson (2000, 2006).

2.9 Placebo and robustness tests

The available time-series data suggest a natural placebo test for our identification assumption. Since we only rely on frost shocks occurring in the 1864–1867 period, we run placebo reduced-form regressions for the main outcomes (including emigration) using frost shocks during all other four-year periods from 1859 to 1900, interacted with port proximity.[52] As the variation in frost shocks is random, placebo coefficients are expected to be distributed around zero. A potential worry with this prediction is that frost shocks in other periods could also have affected emigration and yield large point estimates. Nevertheless, we believe that placebo treatments provide a meaningful test, as no weather events other than those associated with the 1860s famine have been identified by historians as causes of emigration.

We should therefore expect coefficients associated with the treatment period to be in the extremes of the distribution. To make frost shocks comparable across periods with very few or very many shocks, and avoid the influence of outliers, they are categorized in quintiles of the shock distribution over the period. Appendix Figure B.1 displays probability density functions of all placebo point estimates. The black bars represent the reduced-form effect associated with the treatment period (1864–1867), while white bars represent placebo periods. As expected, placebo estimates are scattered across the range of values while the treatment coefficients are consistently at the ends of the distribution for all outcomes.

In Table 2.4, we found that constructing the instrument using *non-growing season* frost shocks could not predict emigration. This was the expected result, given that frost shocks have no effect on agricultural

[52]Shocks 1864–1869 are excluded to avoid the treatment period.

outcomes outside of the growing season. Appendix Table B.4 further shows that such shocks do not have any reduced form effects on our outcomes either. The estimates in all columns are insignificant and close to zero. Hence, the main effects that we find are do not appear to be driven by unobserved fixed characteristics of municipalities.

Different cutoffs for defining frost shocks are examined in Appendix Table B.5. Panel A displays the reduced-form estimates of our main outcome variables using shocks defined at the baseline of 1 standard deviation, while Panels B and C display estimates from letting shocks count at 0.75 or 1.25 standard deviations. Finally, in Panel D, we define growing season using months with a mean temperature of above 5 degrees Celsius, as this is the upper bound for counting a month as being in the growing season following the recommendations of the Swedish Meteorological and Hydrological Institute. The baseline case uses 3 degrees. The signs, magnitudes and statistical significance of these results are similar to the main results.

We next evaluate the robustness of our analysis to large (absolute) values of our key variables. In particular, we want to control for the possibility of certain locations that are very distant from ports driving our results. To do so, we censor variables at the 5^{th} and 95^{th} percentiles, assigning observations outside of that interval the variable value at the nearest bound. This compresses the range of values that variables take on and reduces the potential for a small number of observations with extreme values to affect estimates. We also display the results after tightening variable distributions further, by censoring at the 10^{th} and 90^{th} percentiles. Panels A and B of Appendix Table B.6 do this for two variables: growing-season frost shocks 1864–1867 and proximity to the nearest emigration port. The resulting variables are then used to redefine the instrument, i.e. the interaction between shocks and port proximity. All results are robust to this change. Panels C and D then extend this procedure to *all* non-binary variables that are included in our models. Our results are robust to this modification as well.

To test for the robustness of our inference, Appendix Table B.7 provides estimates of the reduced-form regressions using two different types of standard errors. In Panel A, we cluster standard errors at the county level rather than at the weather station, as political organization and policy may be more correlated within counties, which are established political boundaries. Panels B and C instead estimate spatial-correlation robust standard errors which allow linearly declining correlations across municipalities of up to 100 or 200 kilometers, using the method of Conley (1999). This method has the advantage of not relying on a fixed number of clusters and allows residuals to be correlated within a given radius of each unit of observation. Panel D generates standard errors using the wild cluster-t bootstrap method, which may improve tests when there are few clusters (Cameron et al., 2008). The estimates in Panels A to D display the same pattern as our main regressions, with few changes to significance levels. The estimates on transitioning to representative democracy lose precision with the wild cluster-t bootstrap method but are nevertheless robust to both levels of spatial dependence using Conley-type standard errors.

We also verify the robustness of our results to using logs of our main outcome variables, rather than per capita values. Appendix Table B.9 displays our results for labor organizations and welfare spending.[53]

2.10 Are the effects persistent?

Political preferences have been found to exhibit path-dependence within individuals after being shaped by pivotal events (Kaplan and Mukand, 2011; Madestam et al., 2011) and to be correlated between parents and children (Alford et al., 2005; Jennings et al., 2009). Moreover, institutions may have long-lasting effects on individual beliefs and values (Nunn and Wantchekon, 2011). There is thus reason to believe that the effects found in previous sections may persist in the long run. In this section,

[53]To avoid putting high weight on near-zero values on welfare spending, we use the transformation $log(spending + 100)$, where 100 SEK is less than the first percentile value of the distribution. Without this transformation, results are weaker, with p-values of 0.124 and 0.014 in 1918 and 1919, respectively.

we test for the long-run persistence of the effect of emigration on the support for left-wing parties.

Using data on both national and municipal elections from the five most recent election rounds, 1998 to 2014, we estimate the persistence of emigration on left-wing voting. Table 2.14 displays reduced-form effects of the instrument as well as IV estimates using emigration from 1867 until 1945, after which emigration was uncommon. Strikingly, the results show that the frost shocks occurring 1864–1867 have significant effects on voting up to the five latest Swedish elections. The results are stronger in municipal elections than at the national level, possibly due to issues at the national level having a stronger sway over voters as compared to tradition. The estimate in Column 6 of 0.078 is roughly half as large as the corresponding estimate for the 1911–1921 elections. The mean vote share of the left is also higher in the later period, i.e. 38 percent rather than 24 percent.

2.11 Discussion and conclusion

During the Age of Mass Migration, 30 million Europeans left their home countries for the United States. Among them were more than one million Swedish citizens, making Sweden one of the major origin countries in per capita terms. This paper uses detailed Swedish data from the period 1860–1920 to shed light on the question of whether large-scale emigration can lead to political development in undemocratic origin countries. Our results indicate that it may indeed be the case. Using an instrument based on travel costs and the severe agricultural shocks that sparked the initial wave of migration to the United States, we predict total emigration flows over 50 years. We show that emigration caused significantly higher rates of labor organization, strike participation, voter turnout and left-wing voting in the long run. The findings are consistent with the hypothesis that the improved outside options generated by migrant networks bolstered potential labor activists, who faced repression from local elites for organizing. Since the labor movement had strong ties to the political left, our findings on turnout and political party preferences are likely driven by that mechanism.

Emigration also lead to formal political change. Welfare expenditures per capita rose in high emigration municipalities, as did the likelihood of adopting more inclusive institutions by transitioning from direct to representative democracy. These results are consistent with the mechanism proposed by Acemoglu and Robinson (2000, 2006), in which elites implement institutional change in order to commit to better outcomes for citizens.

Overall, the Age of Mass Migration improved connected citizens' outside options and brought positive effects on support for redistribution and actual redistribution during a time when Sweden was still undemocratic. Migration arguably played a role in the country's transition to a full democracy in the early 20th century.

How externally valid are the results presented in this study? For example, do our findings generalize to other countries that had high emigration rates during the Age of Mass Migration? Figure 2.1, discussed in the introduction, shows that there is a clear positive correlation between historical emigration from 29 OECD countries and contemporary trade union density. Table 2.15 displays output from regressing trade union density on US emigration in two years, 1960 and 2000. The estimated relationship is stable across the two periods, and imply a 7 percentage point increase in trade union density for a 10 percent increase in historical US emigration. The positive association is robust to the inclusion of basic control variables that may be correlated with both emigration and trade union density, including GDP per capita, life expectancy, share of urban population and length of schooling.

We also make the (admittedly stark) assumption that our estimate of the causal effect in Table 2.7 is generalizable across countries to predict each OECD country's trade union density.[54] Using the main IV specification to generate predictions in Columns 3 and 6 of Table 2.15

[54]Predicted values are *log(US emigration by 1910)* $\times$ *IV coefficient.*

shows that a 1 point increase in the predicted union density corresponds to a 3 point increase in actual trade union density. This model hence yields estimates that are approximately one third of the actual value. In addition, the results indicate that the simplest models in Columns 1 and 4, which only include emigration and initial population size, can account for 45 to 53 percent of the variation in the outcome variable.

Extrapolating from our main setting to a cross-country analysis with a small number of observations is certainly very speculative. Nevertheless, the robust correlation raises the possibility that the free immigration policy maintained by the United States in the 19th century and until World War I may have had significant unintended consequences for political development in the rest of the world.

Another concern is if our findings have any bearing on current emigration waves and record global refugee stocks. The mechanism that we propose, that improved outside options may encourage risky activism, is general and potentially applies to many other settings, including contemporary ones. However, the question of how responsive political elites will be to such activism is less straightforward. Agricultural and early industrial economies, such as Sweden in our period of study, are heavily reliant on labor for production. This may explain the urgent political response of Swedish elites are emigration took on greater proportions. In modern autocracies, where leaders often rely on natural resource rents, the economic incentive for elites to respond to popular movements may be lower. Nevertheless, to the extent that activists are able to reach a significant mass, institutional change may occur as economies experiences critical junctures (Acemoglu and Robinson, 2006).

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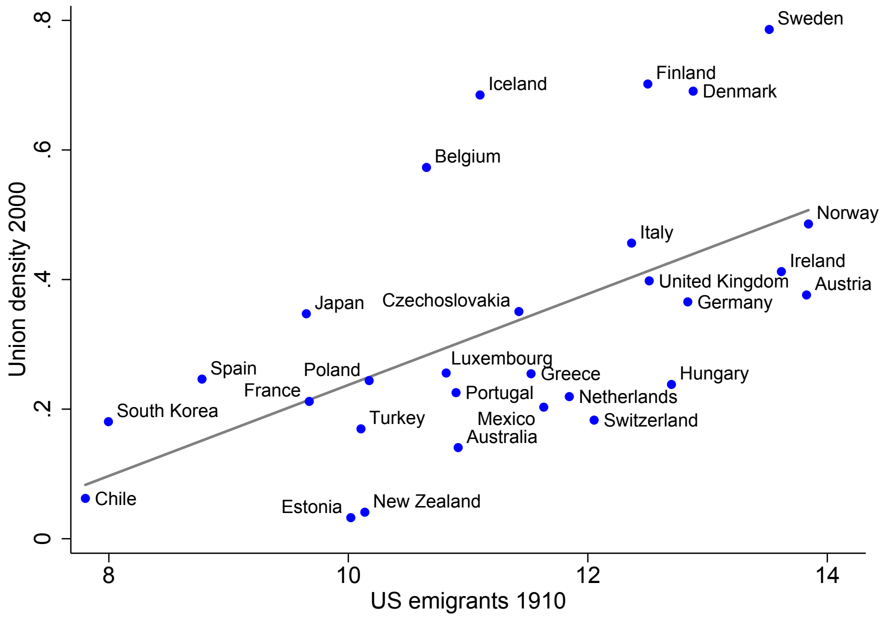
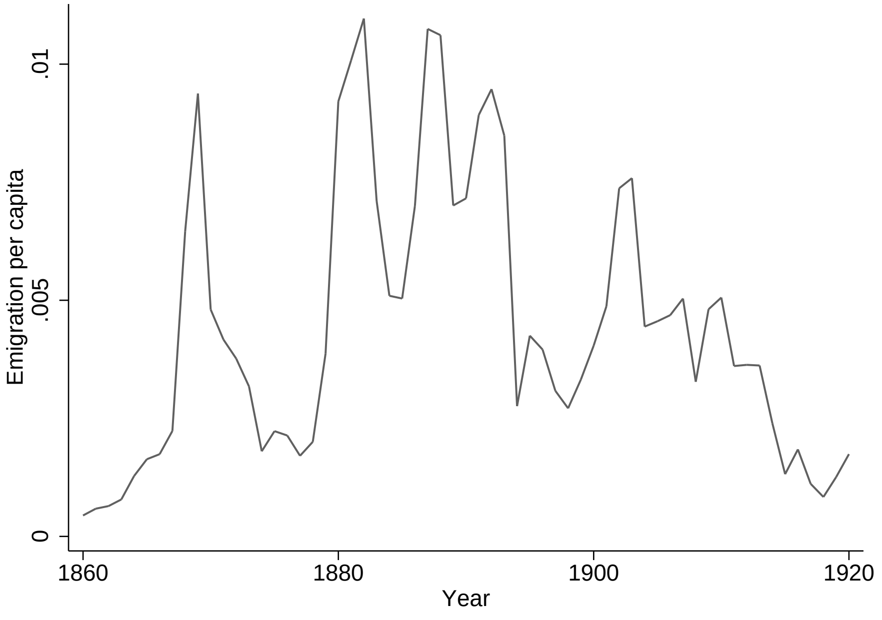
Figures and Tables

Figure 2.1: Emigration and trade union density across OECD countries



Notes: This figure displays the cross-country relationship between trade union density in the year 2000 and the log number of US emigrants as of 1910. Both variables are the residuals after being regressed on log population 1820. Means of the unadjusted variables have been added for scale. A regression line based on the underlying data is displayed, also controlling for the 1820 population. Trade union density is defined as the share of wage and salary earners that are members of a trade union.

Figure 2.2: Emigration flows 1860–1920

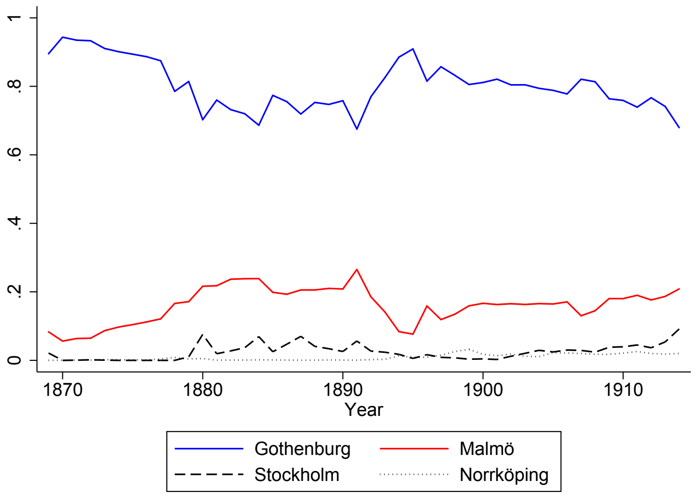


Notes: This figure displays aggregate emigration flows per year between 1860 and 1920. We label emigration during the 1867–1879 period *the first wave of mass emigration*. Later waves, during the 1880s and early 1900s, are also visible. Mass migration from Sweden ended in the 1920s, as the United States enacted immigration quotas.

Figure 2.3: Detrended real GDP per capita 1850–1900

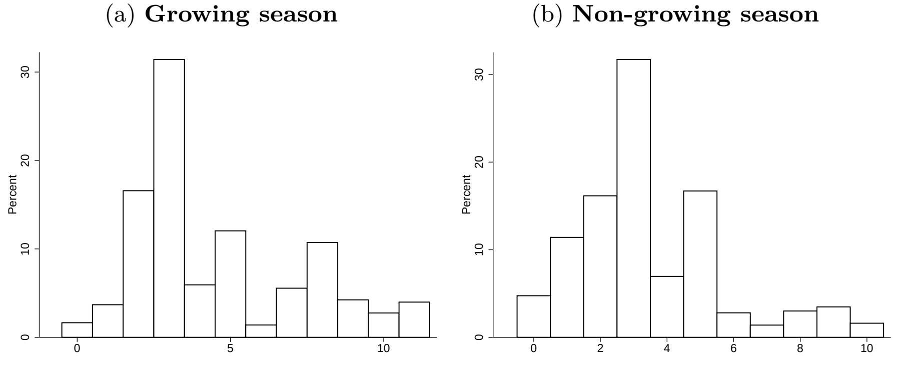
Notes: This figure displays the cyclical component of Swedish real GDP per capita (using a Hodrick Prescott-filter with smoothing parameter set to 100). The shaded area highlights the years used when defining our measure of frost shocks, 1864–67. Source: Edvinsson (2013).

Figure 2.4: Main emigration ports 1869–1920: Gothenburg and Malmö



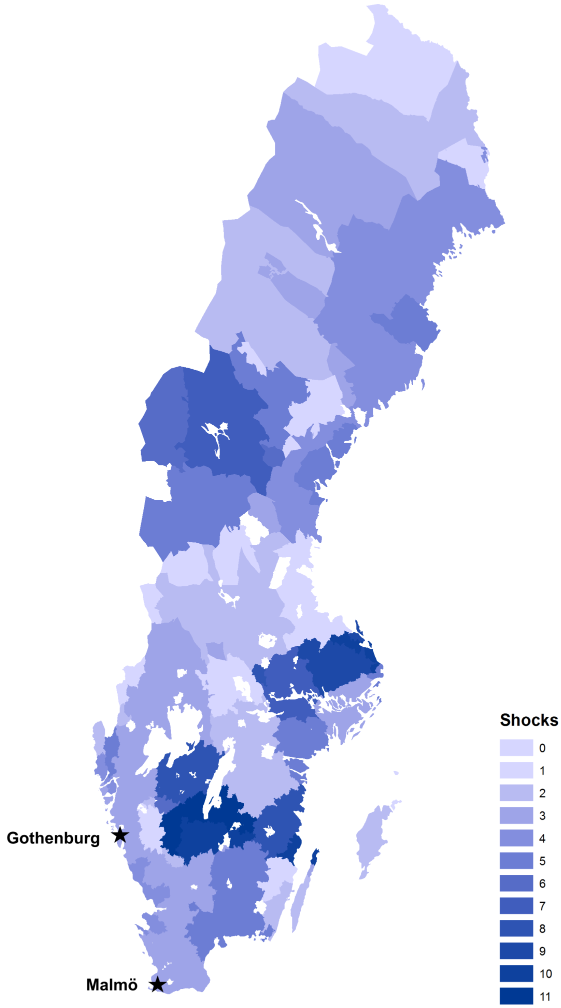
Notes: Share of total emigrants per year 1869–1914 by port of emigration. The figure shows that Gothenburg and Malmö were the main emigration ports in the mass migration period. This motivates our use of proximity to these cities to define the instrumental variable. Stockholm and Norrköping, the first and third largest cities at the time, had minor shares of emigration. Source: passenger list data set.

Figure 2.5: Frequency distribution of frost shocks 1864–1867



Notes: Distribution of frost shocks during 1864-1867 by growing and non-growing season. Shocks are defined at a monthly resolution. For example, a value of 5 in Panel A indicates that a municipality experienced 5 growing-season months with above-average frost between 1864 and 1867.

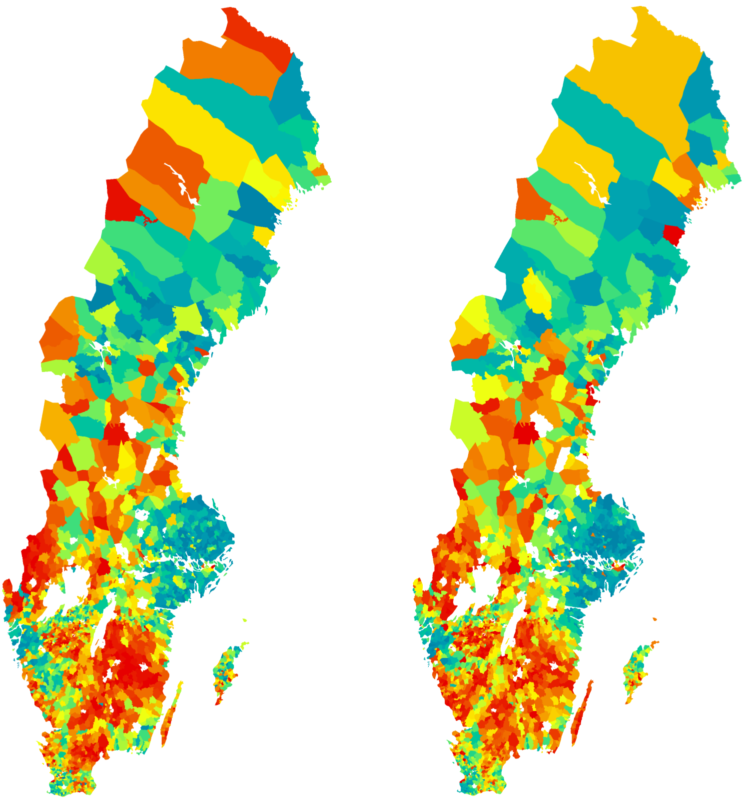
Figure 2.6: Spatial distribution of growing-season frost shocks 1864–1867



Notes: This figure displays the spatial distribution of growing-season frost shocks 1864–1867, used to define the instrumental variable. Darker areas indicate a higher number of shocks. Frost shocks are defined by month, relative to the local long-term mean and standard deviation of frost in that month. Gothenburg and Malmö are the two main emigration ports. In our data, 75 percent of municipalities are closest to Gothenburg, while 25 percent are closer to Malmö.

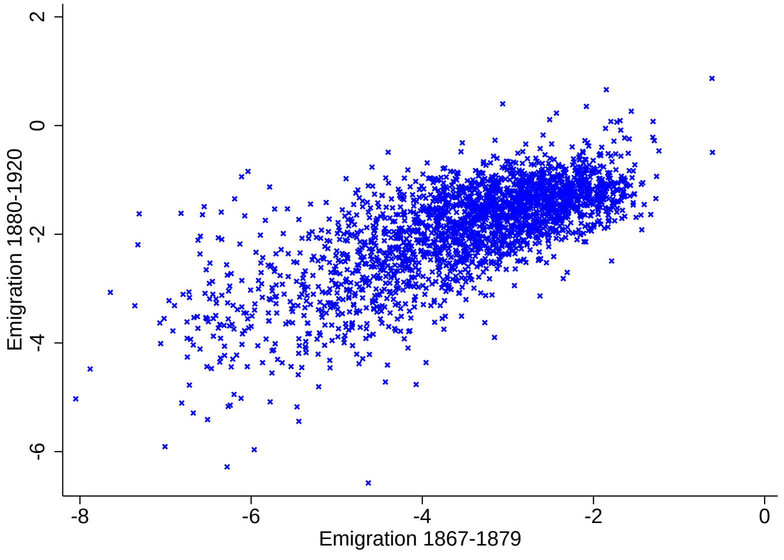
Figure 2.7: Spatial distribution of emigration

(a) **First wave, 1867–1879** (b) **Total, 1867–1920**



Notes: This figure displays the spatial distribution of emigration during the first wave of emigration (1867–1879) and in total (1867–1920). Each geographical unit represents one municipality. Emigration values are divided by the population in 1865. More red values indicate that a larger fraction of the 1865 population emigrated. Color scales are relative to the distribution in the period in question, hence color comparisons between Panels A and B indicate difference in relative importance across periods.

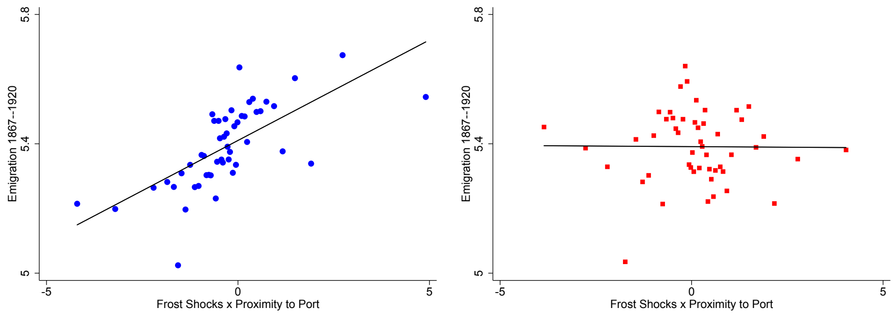
Figure 2.8: Correlation between early and late emigration



Notes: This figure displays a scatter plot of total emigration during the first wave of emigration (1867–1879) against later emigration (1880-1920). Each dot represents one municipality. Emigration values are in logarithms and divided by the population in 1865.

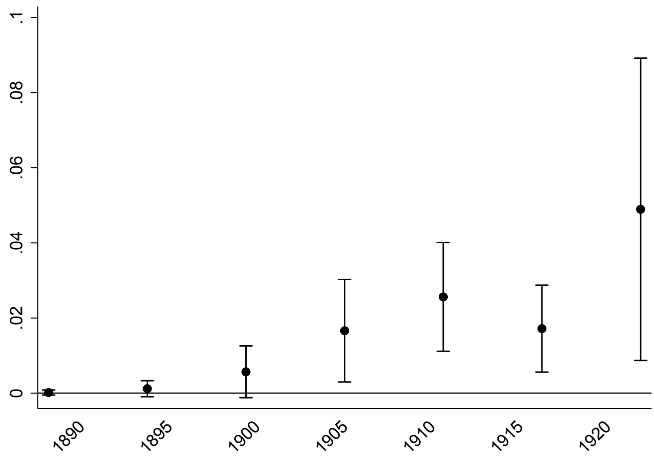
Figure 2.9: First stage: relation between emigration and the instrument

(a) **Growing-season shocks** (b) **Non growing-season shocks**



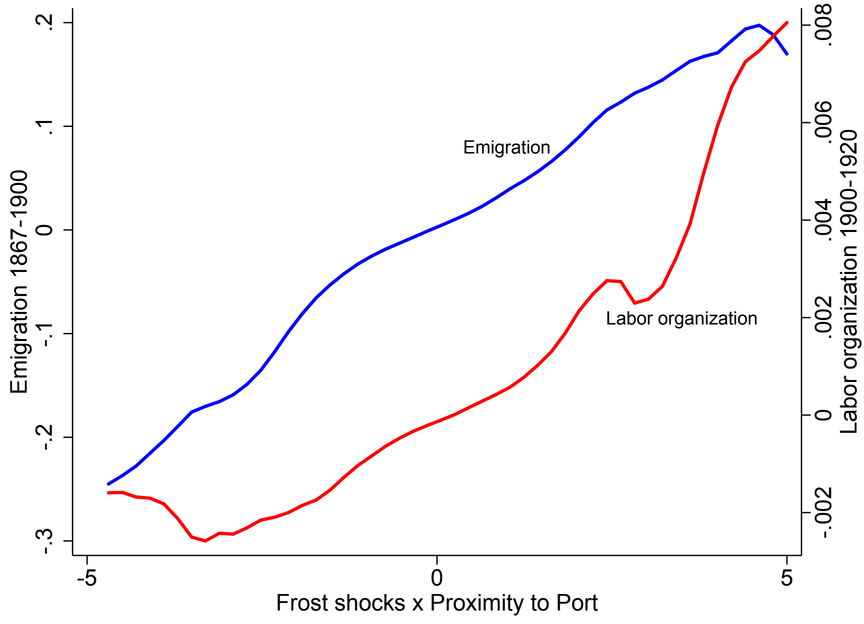
Notes: This figure shows the first stage relationship non-parametrically. Panel A plots log total emigration 1867–1920 against the instrument, defined as the interaction between the number of growing-season frost shocks 1864–1867 and proximity to the nearest emigration port. Proximity is defined as minus the log of distance. Panel B instead the effect of non growing-season shocks occurring in the same period. Municipalities are sorted into 50 groups of equal size. Dots indicate the mean value in each group. A linear regression line based on the underlying (ungrouped) data is also shown. Included controls are county fixed effects, frost shocks 1864–1867, proximity to nearest emigration port, nearest trade port, nearest weather station, nearest town and Stockholm, log population in 1865, log area, latitude, longitude, arable land share in 1810 and indicators for urban municipalities and high soil suitability for the production of barley, oats, wheat, dairy and timber. Panel B additionally controls for growing-season frost shocks 1864–1967 and the instrument. The number of observations is 2358.

Figure 2.10: Emigration and labor organization rates 1890–1920



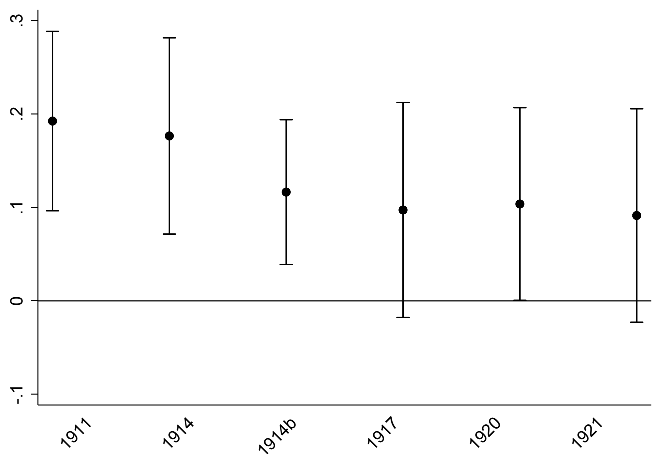
Notes: This figure displays the IV coefficients on the log of total emigration from 1867 to year t on the labor organization rate, defined as the number of members of labor unions and the Social Democratic Party over total population. The instrument is the interaction between the number of growing season frost shocks 1864–1867 and proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects, the log of the population at baseline, log area, latitude, longitude, proximity to nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, the number of growing season frost shocks in 1864–1867, the interaction between growing season frost shocks and proximity to the nearest town and trade port, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. Bars represent 95 percent confidence intervals using standard errors clustered at the weather station level.

Figure 2.11: Nonparametric effect of the instrument on labor movement and emigration



Notes: Local mean smooth. Bandwidth: 1. This figure nonparametrically displays the first stage relationship, as well as the reduced-form effect of the instrument on the average labor organization rate 1900–1920. The instrument is the interaction between the number of growing season frost shocks 1864–1867 and proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All variables have been residualized using the following covariates: county fixed effects, the log of the population at baseline, log area, latitude, longitude, proximity to the nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, the number of growing season frost shocks in 1864–1867, the interaction between growing season frost shocks and proximity to the nearest town and trade port, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. 16 observations that have residuals values above 5 have been top coded at 5 to reduce noise.

Figure 2.12: Emigration and left-wing vote share in national elections 1911-1921



Notes: This figure displays the IV coefficients on the log of total emigration from 1867 to year t on vote shares for the Social Democratic and Socialist parties. The excluded instrument is the interaction between the number of growing season frost shocks 1864–1867 and proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects, the log of the population at the baseline, log area, latitude, longitude, proximity to the nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, the number of growing season frost shocks in 1864–1867, the interaction between growing season frost shocks and proximity to the nearest town and trade port, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. Standard errors are clustered at the weather station level. Bars around point estimates represent 95 percent confidence intervals.

Table 2.1: Summary statistics

	Mean	SD	P10	P50	P90
Frost shocks 1864–1867	4.578	2.764	2.000	3.000	9.000
Proximity to emigration port	-5.024	0.989	-6.084	-5.135	-3.739
Emigration 1867–1920	5.391	1.271	3.664	5.485	6.922
Emigration 1867–1879	3.590	1.482	1.609	3.714	5.394
Emigration 1880–1920	5.172	1.259	3.526	5.247	6.682
Labor organization 1900–1920	0.012	0.037	0.000	0.001	0.031
Strike participants 1909	0.011	0.041	0.000	0.000	0.027
Left vote share 1911–1921	0.246	0.188	0.030	0.210	0.519
Turnout 1911–1921	0.603	0.099	0.476	0.606	0.725
Welfare exp. per capita 1918	2.413	2.116	0.841	2.064	4.183
Welfare exp. per capita 1919	2.756	1.942	0.972	2.381	4.794
Direct democracy 1919	0.634	0.482	0.000	1.000	1.000
Population 1865	7.079	0.782	6.094	7.047	8.076
Urban	0.048	0.214	0.000	0.000	0.000
Area	8.633	1.263	7.209	8.482	10.145
Arable land share	0.702	0.220	0.500	0.667	1.000
Proximity to trade port	-4.383	0.925	-5.294	-4.517	-3.210
Proximity to town	-2.872	0.871	-3.833	-2.920	-1.933
Proximity to Stockholm	-5.534	0.725	-6.193	-5.746	-4.452
Proximity to railway	-3.152	1.429	-4.907	-3.264	-1.317
Proximity to station	-3.482	0.681	-4.174	-3.594	-2.620
Latitude	58.336	2.022	55.881	58.170	60.417
Longitude	14.823	2.064	12.594	14.217	17.859
Barley	0.239	0.426	0.000	0.000	1.000
Oat	0.136	0.343	0.000	0.000	1.000
Wheat	0.177	0.382	0.000	0.000	1.000
Livestock	0.223	0.417	0.000	0.000	1.000
Forest	0.179	0.384	0.000	0.000	1.000

Notes: This table provides summary statistics for emigration as well as outcome and control variables. Emigration, population, area and proximity variables are in logs. Proximity is defined as minus the log of distance.

Table 2.2: Balance tests

Dependent variable:	(1)	(2)
Population 1865	-0.048**	(0.017)
Urban	-0.004	(0.002)
Area	-0.012	(0.026)
Arable land share	0.003	(0.005)
Proximity to trade port	0.016	(0.022)
Proximity to town	-0.034	(0.023)
Proximity to Sthlm	0.024	(0.026)
Proximity to railway	0.015	(0.040)
Proximity to station	0.001	(0.020)
Latitude	-0.026	(0.023)
Longitude	-0.018	(0.020)
Barley	0.008	(0.009)
Oat	-0.002	(0.006)
Wheat	0.003	(0.003)
Livestock	0.006	(0.009)
Forest	-0.012	(0.008)
Infant Mortality	-1.387	(1.004)
Child Mortality	-0.817	(1.067)
Maternal Mortality	-0.089	(0.502)

Notes: OLS regressions. Each row represents a separate regression of the dependent variable on growing season frost shocks 1864-1867, proximity to the nearest emigration port, and their interaction, which is our instrument. Column 1 displays the coefficient related to the instrument, while Column 2 displays standard errors. Proximity variables are defined as minus the log of the distance. Population and area variables are in logs. All regressions include county fixed effects. The number of observations is 2358, except for the final three variables, which have 1784, 1778 and 1268 observations, respectively. Standard errors clustered at the weather station level in parentheses. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 2.3: Frost shocks and agricultural outcomes in a panel 1860–1870

Dependent variable:	Crop failure		Harvest Grade	
	(1)	(2)	(3)	(4)
Frost Shocks	0.166**	0.167**	-1.025**	-1.022**
	(0.060)	(0.062)	(0.424)	(0.423)
Frost Shocks NGS		-0.010		-0.044
		(0.042)		(0.095)
County fixed effects	Yes	Yes	Yes	Yes
County linear trend	Yes	Yes	Yes	Yes
Observations	264	264	264	264

Notes: Columns 1-2: OLS regressions. Columns 3-4: Ordered probit regressions. This table displays the effect of frost shocks on county level agricultural outcomes in a panel 1860–1870. The dependent variable in Columns 1 and 2 is a yearly indicator of crop failure, defined as a harvest grade below 3 on a scale from 0 to 6. The dependent variable in Columns 3 and 4 is the full harvest grade index. *Frost Shocks* is the mean number of growing season frost shocks among a county's municipalities. *Frost Shocks NGS* is defined analogously but for the non-growing season. Both variables are normalized by their standard deviations. Regressions are weighted by arable land area. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 2.4: First stage: Frost shocks and emigration 1867-1920

Dependent variable:	Emigration 1867–1920			
	(1)	(2)	(3)	(4)
Shocks×Proximity to port	0.063***	0.061***	0.062***	0.061***
	(0.016)	(0.013)	(0.014)	(0.015)
Shocks	0.004	0.013**	0.011	0.008
	(0.006)	(0.006)	(0.009)	(0.011)
Shocks×Proximity to trade port			-0.013	-0.009
			(0.022)	(0.021)
Shocks×Proximity to town			0.003	0.004
			(0.008)	(0.008)
Shocks NGS×Proximity to port				-0.001
				(0.016)
Shocks NGS				0.010
				(0.013)
Controls	No	Yes	Yes	Yes
Observations	2358	2358	2358	2358

Notes: OLS regressions. This table displays the effect on log emigration 1867–1920 of frost shocks 1864-1867 interacted with proximity to the nearest emigration port. Proximity is defined as minus the log of distance. *Shocks NGS* indicate frost shocks occurring in the non-growing season. All regressions include county fixed effects and control for the log of the population at baseline. Additional control variables are log area, latitude, longitude, proximity to the nearest town, nearest trade port, nearest weather station and Stockholm, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. $Shocks \times Market\ Access$ includes the interaction between growing season frost shocks and proximity to the nearest town and trade port, respectively. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 2.5: Intertemporal Elasticity of Emigration

A. Dependent variable:	Emigration 1867–1879				
	(1) OLS	(2) OLS	(3) OLS		
Shocks×Proximity to port	0.066*	0.061**	0.062**		
	(0.034)	(0.029)	(0.030)		
Shocks	0.036**	0.050***	0.047**		
	(0.014)	(0.014)	(0.019)		
B. Dependent variable:	**Emigration 1880–1920**				
	(1) IV	(2) IV	(3) IV	(4) OLS	(5) OLS
Emigration 1867–1879	0.955**	1.015**	1.001**	0.391***	0.382***
	(0.385)	(0.413)	(0.414)	(0.022)	(0.022)
County fixed effects	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes
Shocks×Market Access	No	No	Yes	No	No
Observations	2358	2358	2358	2358	2358
F-statistic	3.77	4.28	4.16		

Notes: OLS and IV regressions. Panel A displays the effects of frost shocks, proximity to the nearest emigration port and their interaction on log emigration 1867-1879. Proximity is defined as minus the log of distance. Panel B displays the relationship between early and late emigration. The excluded instrument in Columns 1 to 3 of Panel B is the number of frost shocks interacted with the proximity to the nearest emigration port. Controls for main effects of shocks and proximity to emigration port are included. All regressions include county fixed effects and control for the log of the population at baseline. Additional control variables are log area, latitude, longitude, proximity distance to the nearest town, nearest trade port, nearest weather station and Stockholm, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. *Shocks* $\times$ *Market Access* includes the interaction between growing season frost shocks and proximity to the nearest town and trade port, respectively. The F-statistic refers to the excluded instrument. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 2.6: Elasticity of emigration with respect to US and Swedish business cycles

Dependent variable:	Yearly emigration 1880–1920		
	(1)	(2)	(3)
US-SWE GDP gap × Shocks × Prox. to port	0.209***	0.168***	0.165***
	(0.053)	(0.054)	(0.046)
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Regional trends	No	Yes	Yes
Covariate trends	No	No	Yes
Observations	96637	96637	96637

Notes: OLS regressions. This table displays how locations with different initial early emigration – as measured by the *Shock* × *Prox. to port* interaction – emigrate as the US business cycle is improved relative to the Swedish one. Proximity is defined as minus the log of distance. Yearly emigration is measured as the log of emigrants over population one year prior. $US - SWE\ GDP$ is defined as the difference in log real GDP per capita from The Maddison Project (2013), where the two series have been detrended using a linear trend and controls for three lags before differencing. All specifications control for interaction terms between $US - SWE\ GDP$, $Shock$ and $Prox.\ to\ port$. *Regional trends* indicates additional controls for linear trends across three major regions of Sweden. *Covariate trends* indicates additional controls for linear trends interacted with the following baseline control variables: log population in 1865, an urban dummy, proximity to the nearest emigration port and latitude. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 2.7: The Effect of Emigration on Labor Organization

A. Dependent variable:	Emigration 1867–1900			Labor org. 1900–1920		
	First stage			Reduced form		
	(1)	(2)	(3)	(4)	(5)	(6)
Shocks×Proximity to port	0.066***	0.063***	0.063***	0.001***	0.001***	0.001***
	(0.018)	(0.015)	(0.016)	(0.000)	(0.000)	(0.000)
B. Dependent variable:	**Labor organization 1900–1920**					
	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Emigration 1867–1900	0.009***	0.009***	0.009***	0.022***	0.021***	0.023***
	(0.002)	(0.002)	(0.002)	(0.008)	(0.008)	(0.007)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes
Shocks×Market Access	No	No	Yes	No	No	Yes
Observations	2357	2357	2357	2357	2357	2357
F-statistic				12.95	17.83	16.42

Notes: OLS and IV regressions. This table displays the effects of log emigration 1967-1900 on the average labor organization rate 1900-1920. The excluded instrument is the interaction between the number of growing season frost shocks 1864-1867 and proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects and control for the log of the population at baseline. Additional control variables are growing season frost shocks 1864-1867, proximity to the nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, log area, latitude, longitude, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. *Shocks* × *Market Access* includes the interaction between growing season frost shocks and proximity to the nearest town and trade port, respectively. The F-statistic refers to the excluded instrument. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 2.8: Mobilization of workers

Dependent variable:	Strikers 1909 per capita	Share unionized strikers 1909	Labor org. 1910 per industrial worker	Strikers 1909 per ind. worker
	(1)	(2)	(3)	(4)
Emigration 1867–	0.029^{**}	0.043^{**}	0.215^{***}	0.189^{*}
	(0.013)	(0.019)	(0.073)	(0.105)
County fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Shocks×Market Access	Yes	Yes	Yes	Yes
Observations	2357	2358	2298	2300
F-statistic	18.77	18.73	20.73	19.93

Notes: IV regressions. Column 1 reports the effects of log emigration 1967-1908 on participation in the 1909 general strike. The dependent variable in Column 2 is the share of strikers who were union members. It is normalized so that zero indicates equal fractions of union and nonunion strikers. Municipalities without any strikes are also assigned value zero. Columns 5 and 6 report labor organization and strike participation per number of industrial worker in 1910, rather than per capita, to account for changes in employment structure over time. The excluded instrument is the interaction between the number of growing season frost shocks 1864-1867 and proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects and control for the log of the population at the baseline. Additional control variables are growing season frost shocks 1864-1867, proximity to the nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, log area, latitude, longitude, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. $Shocks \times Market\ Access$ includes the interaction between growing season frost shocks and proximity to the nearest town and trade port, respectively. The F-statistic refers to the excluded instrument. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 2.9: Electoral Effects, National Elections 1911-1921

Dependent variable:	Left-wing vote share				Voter turnout				Eligible voter share			
	OLS	IV			OLS	IV			OLS	IV		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Emigration 1867–1910	0.031***	0.123**	0.115**	0.128**	0.016***	0.074*	0.075**	0.082**	0.037	0.050	-0.088	-0.088
	(0.009)	(0.063)	(0.054)	(0.053)	(0.003)	(0.040)	(0.037)	(0.035)	(0.054)	(0.080)	(0.076)	(0.076)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes
Shocks×Market Access	No	No	No	Yes	No	No	No	Yes	No	No	Yes	Yes
Observations	2358	2358	2358	2358	2358	2358	2358	2358	2357	2357	2357	2357
F-statistic		15.28	20.63	19.36		15.28	20.63	19.36		15.27	19.38	19.38

Notes: OLS and IV regressions. This table displays the effects of log emigration 1967-1910 on three outcomes in national elections between 1911 and 1921. The dependent variable in Columns 1 to 4 is the average vote share of the Social Democratic and Socialist parties. The dependent variable in Columns 5 to 8 is voter turnout, defined as the number of cast votes over total eligible voters. The dependent variable in Columns 9 to 12 is the share of eligible voters, defined as eligible voters per capita. The excluded instrument is the interaction between the number of growing season frost shocks 1864-1867 and proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects and control for the log of the population at baseline. Additional control variables are growing season frost shocks 1864-1867, proximity to nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, log area, latitude, longitude, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. $Shocks \times Market\ Access$ includes the interaction between growing season frost shocks and proximity to the nearest town and trade port, respectively. The F-statistic refers to the excluded instrument. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 2.10: Emigration and census outcomes 1910

Dependent variable:	In-migration	Family size	Umarried	Female ratio
	(1)	(2)	(3)	(4)
Emigration 1867–1910	1.096	-0.079	0.596	0.148
	(3.978)	(0.277)	(1.272)	(0.895)
County fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Shocks×Market Access	Yes	Yes	Yes	Yes
Observations	2300	2285	2300	2300
F-statistic	20.75	20.44	20.75	20.75
Mean dep. var.	44.84	6.53	35.80	51.25

Notes: IV regressions. This table displays the effects of log emigration 1967-1910 on demographic variables in 1910. *In-migration* is the share of inhabitants born in another municipality. *Family size* is the average number of individuals per family. *Unmarried* is the number of unmarried individuals, in percent. *Female ratio* is the share of women, in percent. The excluded instrument is the interaction between the number of growing season frost shocks 1864-1867 and proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects and control for the log of the population at baseline. Additional control variables are growing season frost shocks 1864-1867, proximity to nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, log area, latitude, longitude, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. $Shocks \times Market\ Access$ includes the interaction between growing season frost shocks and proximity to the nearest town and trade port, respectively. The F-statistic refers to the excluded instrument. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 2.11: Emigration and membership in non-labor organizations

Dependent variable:	Free church members			Temperance lodge members		
	OLS	IV		OLS	IV	
	(1)	(2)	(3)	(4)	(5)	(6)
Emigration 1867–1900	0.005***	-0.007	-0.008	0.007***	-0.032**	-0.030*
	(0.001)	(0.008)	(0.008)	(0.002)	(0.015)	(0.016)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Shocks×Market Access	No	No	Yes	No	No	Yes
Observations	2357	2357	2357	2357	2357	2357
F-statistic		18.49	17.89		18.49	17.89

Notes: OLS and IV regressions. This table displays the effects of log emigration 1967-1900 on average per capita membership in non-labor organizations 1900-1920. The excluded instrument is the interaction between the number of growing season frost shocks 1864-1867 and proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects and control for the log of the population at baseline. Additional control variables are growing season frost shocks 1864-1867, proximity to nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, log area, latitude, longitude, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. *Shocks* × *Market Access* includes the interaction between growing season frost shocks and proximity to the nearest town and trade port, respectively. The F-statistic refers to the excluded instrument. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 2.12: Welfare expenditures per capita in 1918 and 1919

Dependent variable:	Expenditures per capita 1918				Expenditures per capita 1919			
	OLS	IV			OLS	IV		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Emigration 1867–	0.227**	1.049**	1.070**	1.038***	0.121	1.108**	1.121***	1.126***
	(0.098)	(0.506)	(0.456)	(0.389)	(0.077)	(0.526)	(0.353)	(0.339)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	No	Yes	Yes	Yes	No	Yes	Yes
Shocks×Market Access	No	No	No	Yes	No	No	No	Yes
Observations	2218	2218	2218	2218	2202	2202	2202	2202
F-statistic		13.57	15.02	15.76		12.93	14.79	15.08

Notes: OLS and IV regressions. This table displays the effects of log emigration 1967-1918 on per capita welfare expenditures in 1918 and 1919. The excluded instrument is the interaction between the number of growing season frost shocks 1864-1867 and proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects and control for the log of the population at baseline. Additional control variables are growing season frost shocks 1864-1867, proximity to nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, log area, latitude, longitude, as well as a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. *Shocks* × *Market Access* includes the interaction between growing season frost shocks and proximity to the nearest town and trade port, respectively. The F-statistic refers to the excluded instrument. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 2.13: Effect of Emigration on Choice of Political Institutions

A. Dependent variable:	Emigration 1867–1918		Repr. democracy	
	First stage		Reduced form	
	(1) 1919	(2) 1938	(3) 1919	(4) 1938
Shocks×Proximity to port	0.060***	0.060***	0.003*	0.009**
	(0.016)	(0.015)	(0.001)	(0.004)
B. Dependent variable:	Voluntary transition to representative democracy			
	OLS		IV	
	(1) 1919	(2) 1938	(3) 1919	(4) 1938
Emigration 1867–1918	0.010**	0.025**	0.048**	0.153**
	(0.004)	(0.011)	(0.022)	(0.068)
Threshold indicators	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Shocks×Market Access	Yes	Yes	Yes	Yes
Observations	2220	2207	2220	2207
F-statistic			15.05	15.24

Notes: OLS and IV regressions. This table displays the effects of log emigration 1867–1918 on choice of local political institutions. The dependent variable is an indicator for having adopted representative (rather than direct) democracy by 1919 or by 1938. The excluded instrument is the interaction between the number of growing season frost shocks 1864-1867 and proximity to the nearest emigration port. Proximity is defined as minus the log of distance. *Threshold indicators* is a set of dummy variables taking value one if the municipal population was 1500 or higher in 1918 (Columns 1 and 3), or in each year 1918–1937 (Columns 2 and 4). All regressions include county fixed effects and control for the log of the population at baseline. Additional control variables are growing season frost shocks 1864-1867, proximity to nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, log area, latitude, longitude, as well as a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. *Shocks×Market Access* includes the interaction between growing season frost shocks and proximity to the nearest town and trade port, respectively. The F-statistic refers to the excluded instrument. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 2.14: The Effect of Emigration on Contemporary Left-wing Voting

Dependent variable:	Left vote share 1998–2014					
	Reduced-form					
	Municipal elections			National elections		
	(1)	(2)	(3)	(4)	(5)	(6)
Shocks×Proximity to port	0.003	0.005**	0.005***	0.003	0.004**	0.005**
	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
Dependent variable:	Left vote share 1998–2014					
	IV					
	Municipal elections			National elections		
	(1)	(2)	(3)	(4)	(5)	(6)
Emigration 1867–1945	0.050	0.080**	0.087**	0.042	0.071**	0.078**
	(0.047)	(0.039)	(0.037)	(0.049)	(0.036)	(0.034)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes
Shocks×Market Access	No	No	Yes	No	No	Yes
Observations	2357	2357	2357	2353	2353	2353
F-statistic	18.22	24.03	23.29	18.23	24.05	23.37

Notes: OLS and IV regressions. This table displays the effects of log emigration 1967-1945 on the average vote share of the Social Democratic and Socialist parties 1998-2014. The excluded instrument is the interaction between the number of growing season frost shocks 1864-1867 and proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects and control for the log of the population at baseline. Additional control variables are growing season frost shocks 1864-1867, proximity to nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, log area, latitude, longitude, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. *Shocks* $\times$ *Market Access* includes the interaction between growing season frost shocks and proximity to the nearest town and trade port, respectively. The F-statistic refers to the excluded instrument. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 2.15: Emigration and trade union density across OECD countries

Dependent variable:	Union density 1960			Union density 2000		
	(1)	(2)	(3)	(4)	(5)	(6)
Log US emigrants 1910	0.054**	0.070*		0.070***	0.070***	
	(0.020)	(0.032)		(0.016)	(0.020)	
Predicted Union Density			3.074*			3.050***
			(1.378)			(0.859)
Log population 1820	-0.091***	-0.076**	-0.076**	-0.116***	-0.086***	-0.086***
	(0.023)	(0.025)	(0.025)	(0.026)	(0.029)	(0.029)
Controls	No	Yes	Yes	No	Yes	Yes
Observations	15	15	15	29	27	27
R-squared	0.53	0.75	0.75	0.45	0.54	0.54

Notes: OLS regressions. This table displays the relationship between US emigration and trade union density across OECD countries. Predicted Union Density is computed using the estimate from Column 6, Panel B of Table 2.7 and the log population in the US 1910. Controls include: real GDP per capita in 1960, rural share of the population in 1960, life expectancy at birth in 1960 and length of primary and secondary schooling in 1970. Robust standard errors in parentheses. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Appendix A: Data and supporting evidence

Estimating minimum daily temperatures

In order to fill in the missing values on minimum temperatures, we use the remaining variables to predict daily minimum temperatures. Observations containing minimum daily temperatures are used to fit a model relating minimum recorded daily temperature to the minimum of temperatures at 6 am, 12 pm and 8 pm, respectively, as follows:

$$min(Temp)_{sdmt} = \alpha_0 + \alpha_m min(Temp^{6am}, Temp^{12pm}, Temp^{8pm})_{sdmt} + \nu_{sdmt},$$

where $min(Temp)_{sdmt}$ is the minimum temperature on day d at station s, month m and year t and $min(Temp^{6am}, Temp^{12pm}, Temp^{8pm})_{sdmt}$ is the minimum of the three daily readings. The coefficients are allowed to vary by month to capture seasonal variation in the relationship. We then use this model to predict daily minimum temperatures for observations with missing values.

Correlation between emigration data sets

Appendix Table A.1 quantifies the correlation by regressing the passenger list data on the parish data using the 1869-1895 period when both data sets are available. As in the remainder of the analysis, we aggregate all data to the municipality level using 1865 borders. Using county fixed effects, the estimated relationship is 1.3 passenger-data emigrants for every church-data emigrant. This reflects the fact that some parishes are not fully matched in the passenger data, leading to underestimation in the latter. Controlling for municipality fixed effects, however, the point estimate becomes statistically indistinguishable from one. This indicates that for those parishes from the passenger data that we are able to match, the two data sets report the same number of emigrants on average. The R-squared value of 0.84 indicates a high degree of similarity. For comparison, in Columns 3 and 4, we use one-year lagged values of emigration from both data sets to predict parish emigration. Both models return lower and similar point estimates of 0.70 and 0.63. Taken together, these results suggest a high reliability of the emigrant data sets and that there is no important lag between leaving the home parish and boarding a ship to the United States.

Table A.1: Comparison between emigration data sets

	Church book emigrants			
	(1)	(2)	(3)	(4)
Passenger emigrants	1.288***	0.987***		
	(0.068)	(0.062)		
Lag church book emigrants			0.703***	
			(0.041)	
Lag passenger emigrants				0.639***
				(0.081)
County FE	Yes	No	No	No
Municipality FE	No	Yes	Yes	Yes
Observations	64530	64557	64557	64557
R-squared	0.66	0.84	0.83	0.73

Notes: OLS regressions. This table displays the relationship between the church book and passenger list data 1869-1895. Lag variables are lagged one year.

Frost shocks and emigration in the panel

Table A.2: Frost shocks and emigration in the panel 1867-1920

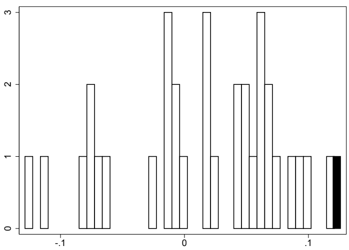
Dependent variable:	Yearly emigration per capita			
	1867–1879		1880–1920	
	(1)	(2)	(3)	(4)
Shocks×Proximity to port	0.009**	0.024***	-0.000	-0.004
	(0.004)	(0.005)	(0.004)	(0.004)
Shocks	0.064***	0.059***	0.004	-0.008*
	(0.006)	(0.007)	(0.005)	(0.005)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Year × Region FE	No	Yes	No	Yes
Observations	30641	30641	73067	73067

Notes: OLS regressions. This table displays the contemporaneous relationship between growing-season frost shocks, proximity to emigration port and their interaction using panel data. Columns 2 and 4 additionally control for yearly fixed effects that vary by the three main regions of Sweden (South, Central, and North). Standard errors clustered at the municipality level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

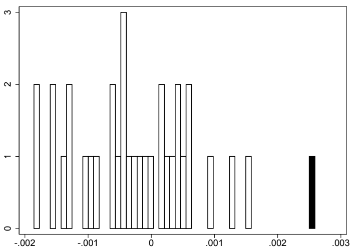
Appendix B: Robustness and placebo tests

Figure B.1: Treatment and Placebo shocks 1859-1900

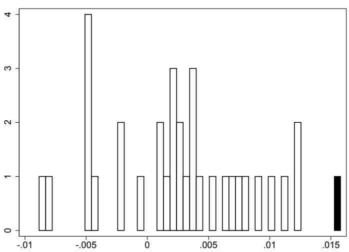
(a) **Emigration 1867-1920**



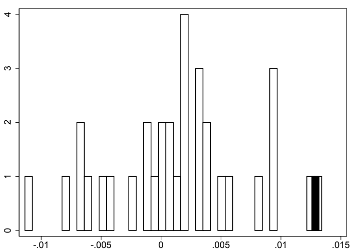
(b) **Labor org. 1900-1920**



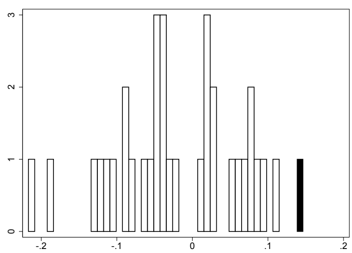
(c) **Left vote share 1911-1921**



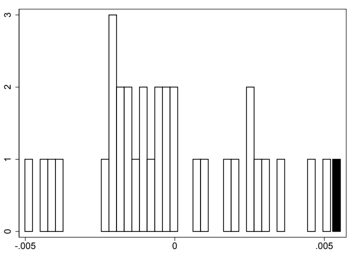
(d) **Turnout 1911-1921**



(e) **Welfare expenditures 1919**



(f) **Repr. democracy 1919**



Notes: Probability density functions of reduced form coefficients from regressing the outcome variables on the interaction between growing season frost shocks and proximity to emigration port during all consecutive four-year periods between 1859 and 1900. Proximity is defined as minus the log of distance. The coefficient associated with the treatment period of 1864–1867 is highlighted in black. Frost shocks are categorized into quintiles of the distribution before interacting with port proximity. All regressions include county fixed effects and control for the log of the population at baseline. All regressions control for growing season frost shocks in the relevant four-year period, proximity to the nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, log area, latitude, longitude, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. Regressions also include the interaction between growing season frost shocks and proximity to the nearest town and trade port, respectively.

Table B.2: Excluding urban municipalities

Dependent variable:	Labor org.		Striking		Left vote		Turnout	
	(1) All	(2) Rural	(3) All	(4) Rural	(5) All	(6) Rural	(7) All	(8) Rural
Emigration 1867–	0.023***	0.018***	0.029**	0.025**	0.128**	0.132**	0.082**	0.092***
	(0.007)	(0.007)	(0.013)	(0.010)	(0.053)	(0.059)	(0.035)	(0.035)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shocks×Market Access	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2357	2244	2357	2244	2358	2245	2358	2245
F-statistic	16.42	13.49	18.77	15.53	19.36	16.16	19.36	16.16

Notes: IV regressions. This table displays the effects of log emigration 1867 to year t on four outcome variables, with and without urban municipalities. *Labor org.* denotes the average per capita membership in labor unions and the Social Democratic Party 1900–1920. *Striking* denotes per capita strike participation in the 1909 general strike. *Left Vote* denotes the average vote share of the Social Democratic and Socialist parties in national elections 1911-1921. *Turnout* denotes the average turnout rate in those same election. The excluded instrument is the interaction between the number of growing season frost shocks 1864-1867 and proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects and control for the log of the population at baseline. Additional control variables are growing season frost shocks 1864-1867, proximity to nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, log area, latitude, longitude, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. *Shocks × Market Access* includes the interaction between growing season frost shocks and proximity to the nearest town and trade port, respectively. The F-statistic refers to the excluded instrument. Standard errors are clustered at the weather station level *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table B.3: Bounding ideological selection of emigrants

Dependent variable:	Left-wing vote share 1911–1921			
	Main result	75 percent	90 percent	100 percent
	(1)	(2)	(3)	(4)
Emigration 1867–1910	0.128**	0.106***	0.085**	0.071*
	(0.053)	(0.036)	(0.037)	(0.039)
County fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Shocks×Market Access	Yes	Yes	Yes	Yes
Observations	2358	2358	2358	2358
F-statistic	19.36	19.36	19.36	19.36

Notes: IV regressions. This table puts lower bounds on the size of our estimated effect of emigration on left-wing party voting, taking into account the possibility of ideological selection of emigrants. The basic assumption is that all emigrants would have been eligible to vote and would have voted in all elections 1911-1921. Columns 2 to 4 then consider 3 different scenarios for how emigrants would have voted if they had stayed. Column 2 assumes that 75 percent of all emigrants would have voted for the non-left. Columns 3 and 4 make this assumption 90 and 100 percent, respectively. All regressions include county fixed effects and control for the log of the population at baseline. Additional control variables are growing season frost shocks 1864-1867, proximity to the nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, log area, latitude, longitude, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. *Shocks* $\times$ *Market Access* includes the interaction between growing season frost shocks and proximity to the nearest town and trade port, respectively. Proximity is defined as minus the log of distance. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table B.4: Non-growing season shocks as placebo instrument

	Placebo test using non-growing season shocks 1864–1867							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Emi. 1867–1920	Labor	Strike	Left	Turnout	Welfare 1918	Welf. 1919	Repr. dem. 1919
Shocks NGS×Proximity to port	0.003	0.000	-0.000	0.001	0.001	0.055	0.012	0.001
	(0.019)	(0.000)	(0.001)	(0.003)	(0.002)	(0.041)	(0.030)	(0.002)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shocks×Market Access	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2358	2357	2357	2358	2358	2218	2206	2220

Notes: OLS regressions. This table displays the effects of non-growing season frost shocks 1864–1867 interacted with proximity to the nearest emigration port on our outcome variables. Proximity is defined as minus the log of distance. *Emi. 1867-1920* denotes log total emigration 1867–1920 in a municipality. *Labor* denotes average per capita membership in the labor movement 1900–1920. *Strike* denotes per capita participation in the 1909 general strike. *Left* and *Turnout* denote average vote share of left-wing parties and average turnout in national elections 1911–1921. *Welfare 1918* and *Welf. 1919* denote per capita expenditures on welfare in 1918 and 1919, respectively. *Repr. Dem. 1919* denotes whether a municipality had voluntarily adopted representative rather than direct democracy in 1919. All regressions control for the log of the population at baseline. Additional control variables are non-growing season frost shocks 1864-1867, proximity to the nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, log area, latitude, longitude, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. $Shocks \times Market\ Access$ includes the interaction between frost shocks and proximity to the nearest town and trade port, respectively. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table B.5: Different cutoffs for frost shocks

	(1) Emi. 1867–1920	(2) Labor	(3) Strike	(4) Left	(5) Turnout	(6) Welfare 1918	(7) Welf. 1919	(8) Repr. dem. 1919
A.	**Shocks defined with 1 standard deviations as threshold**							
Shocks×Proximity to port	0.062***	0.001***	0.002**	0.008***	0.005**	0.065***	0.067***	0.003*
	(0.014)	(0.000)	(0.001)	(0.003)	(0.002)	(0.023)	(0.022)	(0.001)
B.	**Shocks defined with 0.75 standard deviations as threshold**							
Shocks×Proximity to port	0.045***	0.002***	0.002**	0.009***	0.006***	0.068**	0.054*	0.004**
	(0.013)	(0.000)	(0.001)	(0.002)	(0.002)	(0.027)	(0.029)	(0.002)
C.	**Shocks defined with 0.125 standard deviations as threshold**							
Shocks×Proximity to port	0.057***	0.002***	0.002***	0.007**	0.003	0.067**	0.066***	0.003*
	(0.011)	(0.000)	(0.001)	(0.003)	(0.002)	(0.025)	(0.023)	(0.002)
D.	**Growing season cutoff at 5 degrees C instead of 3**							
Shocks×Proximity to port	0.057***	0.001***	0.001*	0.006**	0.004	0.062**	0.062***	0.003**
	(0.013)	(0.000)	(0.001)	(0.003)	(0.002)	(0.023)	(0.021)	(0.001)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shocks×Market Access	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2358	2357	2357	2358	2358	2218	2206	2220

Notes: OLS regressions. This table displays the sensitivity of the main results to changing the definition of frost shocks. Panel A displays the baseline reduced-form results. Panels B and C displays results from counting frost shocks with a 0.75 or 1.25 deviation cutoff. Panel D maintains the baseline cutoff of 1 standard deviation but counts as growing season months with a long-run mean temperature above 5 degrees Celsius, rather than 3 as in the baseline. See Table B.4 for details on the outcome and control variables used. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table B.6: Sensitivity to large values: Winsorizing variables

	(1) Emi. 1867–1920	(2) Labor	(3) Strike	(4) Left	(5) Turnout	(6) Welfare 1918	(7) Welfare 1919	(8) Repr. dem. 1919
A.	**Censor shocks and port proximity at 5^{th} and 95^{th} percentiles**							
Shocks × Proximity to Port	0.066***	0.001***	0.002***	0.007**	0.007***	0.058**	0.064***	0.003*
	(0.016)	(0.000)	(0.001)	(0.003)	(0.002)	(0.024)	(0.023)	(0.002)
B.	**Censor shocks and port proximity at 10^{th} and 90^{th} percentiles**							
Shocks × Proximity to Port	0.086***	0.002***	0.002***	0.007**	0.009***	0.059**	0.066***	0.003*
	(0.020)	(0.000)	(0.001)	(0.003)	(0.003)	(0.028)	(0.024)	(0.002)
C.	**Censor all variables at 5^{th} and 95^{th} percentiles**							
Shocks × Proximity to Port	0.058***	0.001***	0.002**	0.008***	0.006**	0.068**	0.075***	0.003**
	(0.016)	(0.000)	(0.001)	(0.002)	(0.002)	(0.025)	(0.025)	(0.002)
D.	**Censor all variables at 10^{th} and 90^{th} percentiles**							
Shocks × Proximity to Port	0.081***	0.001***	0.002**	0.010***	0.008***	0.059**	0.065***	0.003*
	(0.018)	(0.000)	(0.001)	(0.003)	(0.003)	(0.023)	(0.020)	(0.002)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shocks×Market Access	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2358	2357	2357	2358	2358	2218	2206	2220

Notes: OLS regressions. This table displays the sensitivity of the main results to large values in the model's variables. It does so by censoring variables at the bottom and top 5^{th} (10^{th}) percentile of the distribution. That is, observations below the 5^{th} percentile of values are assigned the value at the 5^{th} percentile, and so on. The first two panels censor two variables: growing-season frost shocks 1864–1867 and proximity to port, and re-define the instrument using these new variables. The bottom two panels repeat the exercise for *all* non-binary variables that are in the model. See Table B.4 for details on the outcome and control variables used. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table B.7: Varying methods for estimating standard errors

	(1) Emi. 1867–1920	(2) Labor	(3) Strike	(4) Left	(5) Turnout	(6) Welfare 1918	(7) Welf. 1919	(8) Repr. dem. 1919
A.	**Standard errors clustered at county level**							
Shocks×Proximity to port	0.062***	0.001***	0.002**	0.008***	0.005***	0.065**	0.067**	0.003*
	(0.014)	(0.000)	(0.001)	(0.002)	(0.002)	(0.026)	(0.025)	(0.002)
B.	**Standard errors robust to spatial correlation up to 100 kilometers**							
Shocks×Proximity to port	0.062***	0.001***	0.002**	0.008***	0.005***	0.065***	0.067***	0.003*
	(0.011)	(0.000)	(0.001)	(0.002)	(0.002)	(0.024)	(0.025)	(0.002)
C.	**Standard errors robust to spatial correlation up to 200 kilometers**							
Shocks×Proximity to port	0.062***	0.001***	0.002*	0.008***	0.005***	0.065***	0.067***	0.003**
	(0.015)	(0.000)	(0.001)	(0.001)	(0.001)	(0.021)	(0.020)	(0.001)
D.	**Wild cluster-t bootstraped errors at weather station level**							
Shocks×Proximity to port	0.062***	0.001***	0.002*	0.008***	0.005**	0.065***	0.067***	0.003
	(0.000)	(0.000)	(0.001)	(0.003)	(0.002)	(0.024)	(0.024)	(0.002)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shocks×Market Access	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2358	2357	2357	2358	2358	2218	2206	2220

Notes: OLS regressions. This table estimates reduced-form regressions on the main outcomes, including emigration, using different standard errors. Panel A uses cluster-robust standard errors at the county-level (24 clusters). Panel B and C estimate spatial correlation-robust standard errors (Conley, 1999), with spatial dependencies allowed up to 100 and 200 kilometers from the center of a municipality, respectively. Panel D estimates wild cluster-t bootstrapped standard errors (Cameron et al., 2008). See Table B.4 for details on the outcome and control variables used. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table B.8: Proximity in levels instead of logarithms

	Proximity in levels instead of logs							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Emi. 1867–1920	Labor	Strike	Turnout	Left	Welfare 1918	Welfare 1919	Repr. dem. 1919
Shocks × Proximity to Port	0.268***	0.007***	0.008**	0.014	0.045***	0.276**	0.363***	0.018**
	(0.066)	(0.002)	(0.004)	(0.012)	(0.012)	(0.106)	(0.109)	(0.008)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shocks×Market Access	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2358	2357	2357	2358	2358	2218	2206	2220

Notes: OLS regressions. This table estimates reduced form regression on the main outcomes, including emigration, using the proximity to nearest port in levels rather than in logarithms. Proximity is defined as minus the log of distance. Due to small point estimates, all outcomes have been multiplied by 1000. *Emi. 1867-1920* denotes log total emigration 1867–1920 in a municipality. *Labor* denotes average per capita membership in the labor movement 1900–1920. *Strike* denotes per capita participation in the 1909 general strike. *Left* and *Turnout* denote average vote share of left-wing parties and average turnout in national elections 1911–1921. *Welfare 1918* and *Welf. 1919* denote per capita expenditures on welfare in 1918 and 1919, respectively. *Repr. Dem. 1919* denotes whether a municipality had voluntarily adopted representative rather than direct democracy in 1919. All regressions control for the log of the population at baseline. Additional control variables are growing season frost shocks 1864-1867, proximity to the nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, log area, latitude, longitude, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. $Shocks \times Market\ Access$ includes the interaction between growing season frost shocks and proximity to the nearest town and trade port, respectively. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table B.9: Outcomes in logs instead of per capita

	(1) Labor	(2) Strike	(3) Welfare 1918	(4) Welfare 1919
Shocks×Proximity to port	0.075***	0.094***	0.024**	0.037***
	(0.027)	(0.029)	(0.012)	(0.011)
County fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Shocks×Market Access	Yes	Yes	Yes	Yes
Observations	2358	2358	2218	2206

Notes: OLS regressions. This table estimates reduced form regression on outcomes defined using logarithms rather than per capita values. Columns 1 and 2 use the $log(x+1)$ transformation, while Columns 3 and 4 use $log(x+100)$ to avoid putting high weight on near-zero outliers. *Labor* denotes log average membership in the labor movement 1900–1920. *Strike* denotes log number of participants in the 1909 general strike. *Welfare 1918* and *Welf. 1919* denote log expenditures on welfare in 1918 and 1919, respectively. All regressions control for the log of the population at baseline. Additional control variables are growing season frost shocks 1864-1867, proximity to the nearest emigration port, nearest town, nearest trade port, nearest weather station and Stockholm, log area, latitude, longitude, as well as an urban indicator and a set of indicators for high soil quality for the production of barley, oats, wheat, dairy and timber. Proximity is defined as minus the log of distance. Due to small point estimates, all outcomes have been multiplied by 1000. $Shocks \times Market\ Access$ includes the interaction between growing season frost shocks and proximity to the nearest town and trade port, respectively. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Chapter 3

Mass Migration and Technological Innovations at the Origin*

3.1 Introduction

Emigration has often been depicted as a major problem for struggling developing countries. Nations may lose both human capital and laborers important for production. In the long run, however, the effects of migration on economic development could be positive for different reasons. Labor flight may for instance affect factor endowments and prices, which could induce technological progress (Habakkuk, 1962; Allen, 2009; Acemoglu, 2010).

This paper presents an empirical analysis of the short and long term effects of migration on technological innovations in origin communities during the Age of Mass Migration, one of the largest migration episodes in human history. Between 1850 and the First World War, nearly 30 million Europeans left their home soil and crossed the Atlantic Sea for

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the United States. We focus on Sweden, which was still primarily an agrarian society with very few patented technological innovations in the mid 19th century. At the turn of the century, after about a quarter of its population had left the country, Swedish technological innovations surged.

One possible mechanism connecting these two developments is due to the effect that migration may have on labor inputs in production. In particular, emigration may lead to a decrease in the labor available to production, which in turn can increase the price of labor. Additionally, if workers can credibly threaten to emigrate, wages could increase due to a strengthening of the bargaining power of labor. In fact, Karadja and Prawitz (2016) show that the Swedish labor movement advanced in communities with relatively more emigration. They interpret their findings as a result of improved outside options for stayers, as a larger emigrant stock overseas facilitates future migration.

With an increase in labor costs, altering relative prices of factor inputs, new technological innovations may be induced, an idea going back to John Hicks. In particular, innovations that decrease the need for the endowment that has become relatively more costly should increase (Hicks, 1932). For instance, Allen (2009) argued that high wages were fundamental in explaining the Industrial Revolution in eighteen-century Britain, as it lead to labor-saving technologies, such as the spinning jenny, being invented.

To capture the effects of migration on technological innovations we use a novel yearly data set on successful patent application spanning from the mid 19th century to the First World War. Similar to Karadja and Prawitz (2016), we exploit historically severe local growing season frost shocks prior to the start of emigration, together with a measure of the travel cost to reach an emigration port, to identify an arguably causal effect. Due to tendency of migration to be highly path dependent, our instrument not only predicts early migration flows, but has strong predictive power several decades later. The main IV specification then compares long term outcomes between municipalities within the same county, but with different intensity of the migration push factor captured by our instrument. In a placebo test, we show that frost events during non growing seasons do not affect emigration, nor do they have an effect on other outcome variables.

Our results show that, in the long run, migration caused an increase in technological innovations in origin municipalities. Our IV estimates report that a ten percent increase in the number of emigrants during the main Swedish transatlantic emigration period 1867–1900 would have increased the number of patents by roughly 7 percent. Moreover, weighting patents by a measure of the economic value of patents, we find an even stronger positive effect on innovations. Our results are robust to the inclusion of several baseline controls as well as a variety of different specifications.

In order to examine the possibility that labor scarcity induced technological innovation, we consider the effect of emigration on the availability of low skilled labor. We find that both the volume and the share of low skilled labor became significantly lower after the main emigration period in municipalities with higher emigration numbers. Moreover, employing a yearly panel on low skilled wages at the county level using a fixed effects strategy, we document a positive association between real wage growth and cumulative emigration in the preceding years. This relationship is robust to the inclusion of linear county time trends as well as linear time trends in baseline county characteristics. Together this suggests that increased labor costs is a causal mechanism behind the positive effect on technological innovation.

To our knowledge, there is a lack of empirical research linking emigration to technological innovation in the origin country. Most similar to our paper is Hornbeck and Naidu (2014). They study the out-migration of low-wage labor affected by the Great Mississippi Flood of 1927 in the American South, and find that it leads the agricultural sector in flooded counties to become more mechanized. They stress that rural flight may induce economic development through a sectoral reallocation of labor and the adoption of new capital-intensive technologies. We extend on this line of work by measuring the effect on actual innovations, rather than the adoption of already existing technologies.

Through our proposed mechanism, our paper is related to the literature on directed technical change. Within this literature there are a few studies that empirically investigates the effect of a change in the supply, or price, of an input on technological innovation. Little attention has however been directed at labor inputs[1]. Hanlon (2015) find that an unexpected reduction of high quality cotton supplied to the British cotton textile industry induced the development of new technologies complementary to the use of lower quality cotton as input. Most attention has otherwise been directed towards the energy sector; e.g. Popp (2002) and Aghion et al. (2016) studies the effect of high energy prices on energy-saving technologies and find that firms or regions subject to an increase in energy prices innovate more in clean technologies.

In terms of migration and innovation, there is a small recent literature concerning immigration of high skilled migrants and innovation. Moser et al. (2014) study the effect that Jewish chemists leaving Nazi Germany have on chemical patents in the United States and find that patenting by U.S. inventors increased by about 30 to 70 percent in research fields of emigrés. Borjas and Doran (2012) study immigration of Soviet mathematicians to the United States after the Cold War and find a negative effect on the productivity of American mathematicians as measured by journal publications. While this literature focuses on high-skilled migration, we study a migration episode that predominantly took place among low skilled workers within the agricultural sector. Moreover, we emphasize out-migration rather than in-migration, although we will discuss the possibility of return migration as an explanatory mechanism.

[1]In terms of adoption of already existing technologies, Lewis (2011) studies how an increase in low-skill labor through immigration affected adoption of automation machinery, finding that investments decreased on plants in areas with relatively more low-skill immigration. Acemoglu and Finkelstein (2008) studies the effect of an increase in the relative price of labor, due to a reform, on technological adoption in the US Health Care Sector and find that it induced hospitals to adopt various new medical technologies.

Finally, through the empirical setting, our paper is also related to a growing body of empirical literature in economics concerning different aspects of the Age of Mass Migration. However, most of this literature focuses on the effects of immigration on receiving regions in the United States[2], while there is, perhaps, surprisingly little work on the effect of emigration on sending regions in Europe.

The remainder of the paper proceeds as follows. Section 5.2 provides an overview of the historical background, while Section 5.3 describes our data. Section 3.4 introduces the econometric framework as well as our identification strategy. Sections 3.5 discusses the relationship between our instrument and emigration. Section 3.6 presents the main results, while section 3.7 discusses potential mechanisms. Finally, Section 3.8 concludes.

3.2 Background

Nearly 30 million Europeans left their home countries and crossed the Atlantic Sea for the United States during the Age of Mass Migration (1850–1914), rendering it into one of the largest emigration episodes in human history. Along with Ireland, Norway and Italy, Sweden had one of the highest sending rates in per capita terms (Taylor and Williamson, 1997). Between the 1860s and the First World War, about a quarter of the Swedish population emigrated, mostly to the United States.

Sweden's transatlantic emigration episode took off in the last years of the 1860s, coinciding with severe famine in large parts of the country. It is well known that the famine and resulting poverty followed after a series of bad harvests due to bad weather conditions in the late 1860s. Especially 1867 saw record breaking cold weather during growing season months. While the cold weather was most harshly felt in the north of Sweden, also the rest of the country experienced frost during regular growing season months (SMHI, 2013). It is widely believed that these so called *famine years* were crucial as a push factor behind the onset of the

[2]See Abramitzky and Boustan (2015) for a review of this literature.

Swedish transatlantic mass migration (see *e.g.* Sundbärg (1913), Barton (1994) and Beijbom (1995)).

Figure 3.1 depicts yearly flows of emigrants in Panel A. The initial rapid increase in emigration starting in 1867 is clearly visible in the figure. In the five years following 1867, as many as 150,000 Swedes or four percent of the population emigrated. This first wave of emigration was followed by a period of comparatively low emigration numbers before it took off again during the first years of the 1880s. During the following decade about half a million Swedes left the country during the most intense period of the Swedish transatlantic migration experience.

While poor economic conditions continued to play an important part in migration decisions, especially relative to the United States[3], historians often point out the important role of social networks when describing the migration of the later part of the mass migration episode. Besides reducing migration costs when arriving to the New World, migrants already in the United States sent pre-paid travel tickets back home. As many as every second emigrant is believed to have traveled on such tickets (see Runblom and Norman (1976) and Beijbom (1995)). Moreover, migrant letters describing their experiences overseas, often in overly positive language, were common. Among migrants from Scandinavia arriving in the United States in 1908–09, 93.6 percent stated that they were joining friends or relatives who had migrated previously (Hatton, 1995).

At the time of the start of the Swedish migration episode, Sweden was predominantly an agrarian society. In 1860, before mass migration started, almost 80 percent of the labor force worked within the agricultural sector, compared to about 10 percent within the industrial and manufacturing sector (Edvinsson, 2005). In the following decades the Swedish rural population declined in relative numbers, during a time when Sweden increasingly became more industrialized. As emigration reached new peaks at the turn of the century, backlash from economic

[3] As shown in (Bohlin and Eurenius, 2010), the GDP difference between Sweden and the USA is a good predictor of aggregate Swedish migration patterns. Such patterns seem to have been common in the rest of Europe as well (Hatton, 1995).

and political elites became more severe, often based on concerns of a potentially adverse effect of labor scarcity on the Swedish economy (Kälvemark, 1972). Following the start of the first wave of emigration in the end of the 1860s, Swedish wages saw a substantial increase. Low-skilled agricultural wages in particular increased and came closer to low-skilled industrial wages Jörberg (1972a). After a downturn, Swedish wages rapidly increased again starting in the mid 1880s and continued to do so for the following decades. Swedish economic historian Lennart Jörberg noted that emigration may have played an explanatory role behind this development (Jörberg, 1972a), while others, such as Ljungberg (1997), argue more forcefully that transatlantic emigration, by draining the supply of labor, was key to this development. In neighboring Norway, the emigration commission of 1912–13, concluded that, by contributing to the increase of wages, emigration had been instrumental in promoting the process of mechanization and rationalization of production (Hovde, 1934).

Concurrent to this development, technological patents increased rapidly as seen in Panel B of Figure 3.1. Several technological innovations are believed to have reduced the need for manual labor. For example, Gustaf de Laval was granted a patent in 1878 for a centrifugal separator, making it possible to separate cream from milk faster and more easily, an invention which was improved during the following decades. And Gustaf Dalén's invention of the sun valve, which lead to his 1912 Noble prize in Physics, automated lighthouse technology and made many lighthouse keepers unemployed.

In terms of patent laws, Sweden changed from a registration system to an examination system in 1885 and signed the Paris Convention for the Protection of Industrial Property the year after. Similarly to the German patent system, grants could be given to the first person to file an application. After the reform, a rigorous novelty search was required before a patent was granted. Moreover, with the Paris Convention in place, filing a patent in one member state gave the right to file the same patent during a one year period in any other of the member states[4].

To apply and receive patent protection for an invention the applicant needed to pay both an application fee and a renewal fee. The Swedish renewal fees were increasing over the patent duration, rendering renewals relatively costly. In real prices, the cost of applying and renewing a patent for the maximum number of 15 years was similar to today's cost of keeping a patent in force for the same duration (Andersson and Tell, 2016)[5].

3.3 Data

Our data is organized at the municipal level in Sweden following the historical administrative boundaries in the 1860s, which we define using an administrative map from the National Archives of Sweden (*Riksarkivet*). To get consistent borders over time we collapse urban municipalities with their adjacent rural municipality as these borders sometimes changed due to urban expansion. In total we end up with nearly 2400 municipalities.

The patent data we use were personally compiled and digitized from the archives of the Swedish Patent and Registration Office (PRV). The data set includes all granted Swedish patents between the mid 19th century and 1914, and specifies the year of application and grant, the names of all inventors and patent holders, their professions and home location. There are in total 18,250 registered patents with an inventor residing in Sweden. Of these, about 90 percent have information on the name of the location of the inventors. We spatially link these to our administrative data using geographic information system software in two steps. First, we find the geographic coordinates of each geographic location and second, we match them to our municipalities.

[4]If the priority year had expired, the first person to file a patent application could receive the right to the grant.

[5]In 1914 nominal value, the total cost was about 745 Swedish krona (Andersson and Tell, 2016).

To get a measure of the quality or value of a particular patent, the literature typically uses either the number of citations received or the amount of patent fees paid by the owner to maintain the patent in force for a longer period of time. Unfortunately, we do not have any data on the former. While patent citations are widely considered to be good indicators of the innovative quality in a patent, renewal fees paid can be argued to be a more suitable measure of the *economic* value of patents. This is because the patentee has to make the renewal decision each year, based on their expected economic return from extending the patent right (see eg. Schankerman and Pakes (1986) and Burhop (2010)). As the patent files in our raw data were updated on a yearly basis to include information of fee payment by the patent owner, we can therefore use the number of years a patent is in force as a proxy for its economic value.

We document emigrants for each municipality, using data collected by priests at the parish level. Variables include migrant's first and last names, migration date, age, gender and occupation. We link migrants in each parish to a municipality. To complement the emigration data obtained by priests, we also use data from passenger lists compiled by shipping companies. Besides the variables available in the church records this additional data set also includes port of exit, giving us information on which routes emigrants used when migrating. Although these two data sets are independent they are highly correlated, suggesting that most emigrants migrated directly from their home parishes rather than migrating within the country before leaving Sweden. To decrease the extent of unreported migration in the parish records we aggregate both emigration data sets to the municipality-year level and choose the maximum of the two numbers for any given year.

For our first stage relationship, we use meteorological data on temperature provided by the Swedish Meteorological Institute (SMHI) as well as the Norwegian Meteorological Institute (MET). It includes the daily minimum temperature at the weather station level.

To study labor force changes across Swedish municipalities, we use complete decennial censuses between 1880 and 1910 obtained from the

National Archives of Sweden and the North Atlantic Population Project[6]. In order to investigate the relationship between emigration and low skilled wages we use a yearly county panel on daywages in agriculture from Jörberg (1972b). We construct real wages by deflating the nominal wage series with a regional foodstuff index consisting of 14 food items obtained from Jörberg (1972a).

Moreover, we use a few additional data sources to obtain several baseline control variables. Soil suitability data for different agricultural produce (barley, oats, wheat, livestock and forestry) is taken from the FAO GAEZ database. Historical trade data is from Statistics Sweden. Railway data is from Norstedts. Population data was kindly shared by Lennart Palm (Palm, 2000).

Summary statistics of the variables used in the empirical section are presented in Table 3.1.

3.4 Empirical framework

To measure the long run effects of emigration on innovation, our starting point will be a cross-sectional regression of the following form:

$$y_{ic}^{t_1-t_2} = \theta_c + \beta Emigrants_{ic}^{1867-t_1} + \mathbf{X}_{ic}'\delta + \varepsilon_{ic}, \tag{3.1}$$

where $y_{ic}^{t_1-t_2}$ is the natural logarithm of the number of successful patents between year t_1 and t_2, for a municipality i in county c. As we want to measure the effects of a period of migration over several years on innovations and scale the effects to per capita levels, $Emigrants_{ic}^{1867-t_1}$ is defined as the natural logarithm of the number of migrants between the start of migration and some year t_1. In $\mathbf{X}_{ic}$, which is a vector of municipality controls at baseline, we include the natural logarithm of the population in 1865. As several municipalities do not have any registered patent, we let $y = log(1 + \#patents)$. To control for that municipalities

[6]Unfortunately, neither the earlier censuses from 1860 and 1870, nor 1920 and 1930, are currently available in digitized format.

may differ in several dimensions, besides $\mathbf{X}_{ic}$, we additionally include θ_c, which represents a set of county fixed effects[7]. Thus we only compare municipalities within smaller regions. The error term ε_{ic} captures all omitted influences.

The OLS regression stated in (3.1) will estimate the true coefficient of interest, β, if $Cov(Emigrants_{ic}, \varepsilon_{ic}) = 0$. For different reasons, this is unlikely to hold. The main concern about using OLS is the potential for a spurious relationship between a municipality's history of migration and later outcomes driven by underlying unobserved factors at the local level. The sign of the bias is *a priori* ambiguous. For instance, emigrants may leave places that are better connected, which might be beneficial in other relevant dimensions, or they may leave municipalities that performs poorly for reasons that also affects later economic outcomes.

To identify a causal effect of emigration on our outcomes of interest we therefore make use of an IV strategy which exploits frost shocks leading up to the initial wave of emigration interacted with a measure of the travel cost to reach an emigration port. As mentioned, there are numerous historical accounts of the severe agricultural conditions in these record breaking cold years and their relation to the flood of emigration that started in the end of the 1860s. On the national level, Figure 3.2 suggests that the bad harvest years also had strongly negative effects on real GDP per capita. Although frost shocks may affect agricultural areas in particular, also urban areas may be indirectly affected as they are connected to local agricultural markets.

The important role of social networks in migration decisions and the existence of path dependency in migration patterns is well known in migration research[8]. As we will also see, the shocks to agriculture in the 1860s will affect both immediate migration and subsequent migration during a couple of decades. We will argue that this sign of path

[7]Treating the county of the city of Stockholm (*Stockholms stad*), which includes one single municipality, as a part of the county of Stockholm (*Stockholms län*), there are 24 historical counties.

[8]See *e.g.* Massey et al. (1993), Hatton and Williamson (2002), McKenzie and Rapoport (2007), Bryan et al. (2014) and Giulietti et al. (2014).

dependency is due to the importance of social networks in later rounds of emigration.

Before going into the details of the first stage we start by introducing our measure of frost shocks.

Frost shocks

Frost is generally defined as a deposit of soft white ice crystals or frozen dew drops on objects near the ground formed when the surface temperature falls below freezing point. It is particularly detrimental in agriculture, leading to frozen plants and lower harvests. Our measure of frost shocks is identical to the one used by Karadja and Prawitz (2016) and follows the approach of Harari and La Ferrara (2013). We will denote a frost day as a day with a minimum temperature below zero degrees Celsius. Moreover, in order to restrict attention to frost days that matter for agriculture, we only include temperature in growing season months in our measure of frost shocks[9].

We define a frost shock in three steps. First, for each month we calculate the mean number of frost days and calculate the deviation from the mean at the weather station level:

$$deviation(\#Frost\ days)_{smt} = \#Frost\ days_{smt} - mean(\#Frost\ days)_{sm},$$

for each station s in month m and year t. Secondly, using nearest neighbor matching, we match municipalities to a weather station and assign them the value from the weather station so that we obtain $deviation(\#Frost\ days)_{imt}$ for each municipality i. The reason for calculating deviations at the station level rather than at the municipality level is due to the fact that temperature deviations are more reliable for spatial interpolation; a well known fact from climatology (Hansen and Lebedeff, 1987). Lastly, we define a frost shock as a month when the deviation from the mean number of frost days is above one standard

[9]Following meteorological practice, we define a growing season month as a month with an long term average temperature of above 3 degrees Celsius (SMHI)

deviation of the number of frost days in that month:

$$\textit{Frost shock}_{imt} := I[\textit{deviation}(\#\textit{Frost days})_{imt} > \textit{sd}(\#\textit{Frost days})_{im}],$$

The number of yearly growing season frost shocks is then obtained by summing $\textit{Frost shock}_{imt}$ over all growing season months for each year. In Figure 3.3 we see the distribution of frost shocks in the growing season months of 1864-67 in Panel A alongside the distribution in non growing season months of the same years in Panel B. We will later use these non growing season shocks to construct placebo instruments.

As discussed above, the years leading up to the first wave of emigration saw record breaking cold weather in parts of Sweden, resulting in low harvests and famine. In Karadja and Prawitz (2016) it is already demonstrated that the frost shocks we use can be shown to have had a negative impact on the harvest as measured by harvest grades.

Instrumental variable

After defining frost shocks, we now turn to how we can exploit these shocks for our IV identification strategy.

Since agricultural shocks in a municipality may have direct effects on any outcome related to economic activity, both in the short and long term, we should not use these shocks themselves as our instrument, as this could violate the exclusion restriction. Instead we introduce an aspect of the cost of emigrating that arguably only has an effect on the emigration decision and not on our outcomes of interests, such as innovations. Our hypothesis is that frost shocks should matter differently for the likelihood of emigrating depending on the cost of emigration. It is well known in migration research that the travel cost related to migration is highly important for the migration decision (see *e.g.* Morten and Oliveira (2014) and Quigley (1972)). To proxy for traveling costs to reach an emigration port we use the proximity to the nearest major emigration port, either Gothenburg or Malmö[10]. In our data, these two

[10] We define proximity as minus the log distance to a locality.

ports make up more than 95 per cent of all emigration between the 1860s and 1920.

We can then set up the following cross-sectional first stage relationship between emigration, frost shocks and port proximity:

$$Emigrants_{ic}^{1867-t_1} = \theta_c + \beta Shocks_{ic} \times Port_{ic} + \mathbf{X}_{ic}'\delta + u_{ic}, \qquad (3.2)$$

where $Emigrants_{ic}^{1867-t_1}$ is defined as in (3.1), $Shocks_{ic}$ is the number of frost shocks a municipality i in county c experienced in 1864-67 prior to the first wave of emigration and $Port_{ic}$ is the proximity to the nearest emigration port. Importantly, in $\mathbf{X}_{ic}$ we include both $Shocks_{ic}$ and $Port_{ic}$, separately. Furthermore, we always include the log population in 1865 in order to scale the effects to per capita levels. Additionally we control for the proximity to the nearest railway, town and weather station, the log area, an indicator variable indicating if a municipality is urban, latitude and longitude, the share of arable land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. All continuous variables are de-meaned to facilitate interpretation, including the number of frost shocks and proximity to an emigration port. As before, θ_c represents a set of county fixed effects.

We can then interpret β as the effect of the instrument on emigration. The two equations, given by (3.1) and (3.2), then constitutes our system of two equations. Note that our sole excluded instrument in the second stage will be the interaction $Shocks_{ic} \times Port_{ic}$. As discussed, an attractive feature of our empirical strategy is that it allows us to explicitly control for the main effects of both travel cost and the frost shock itself, as we only rely on the interaction between the two to identify a causal effect. Thus we can rule out any potential effects, besides through migration, that these variables may have, either directly or indirectly, on our outcome. Perhaps most importantly, as agricultural shocks have a negative effect on agricultural output, which is also the reason why we argue that the instrument has an effect on emigration, it could have a variety of effects on the economic environment in a region, which in the end may affect innovations. For the same reasons we can also rule out indirect effects of our frost shocks that go through other channels than agriculture. For instance, cold ambient temperature *in utero* may have an effect on birth outcomes as shown by Bruckner et al. (2013) using Swedish data from 1915-29.

Still, one potential threat to the exclusion restrictions is that $Shock_{ir} \times Port_{ic}$ could capture the differential impact of experiencing a shock in more isolated areas relative to more connected areas. For instance, Donaldson and Burgess (2013) show that locations with better railway connections were less responsive to local productivity shocks in colonial India. This may be a concern as it could potentially have long lasting effects. Although our two emigration ports, Malmö and Gothenburg, were important trade ports, there were several other important cities in terms of market connectedness, not least the national capital, Stockholm. Nevertheless, to control for this possibility we include an interaction of the shocks with a more localized measure of market connectedness: the proximity to the nearest major trade port or nearest town[11].

As we have constructed the frost shocks to be unexpected events by including long term mean and standard deviations in our definition, we would like to see that our instrument is not systematically correlated with other variables, besides emigration, for the exclusion restrictions to hold. As already shown in Karadja and Prawitz (2016), among the rich set of baseline measures we include in $\mathbf{X}_{ic}$, only the relationship to the population level in 1865 is significantly different from zero at the 5 percent level; Population is positively associated with our instrument. However, by random chance, we should expect some variable to be correlated with the instrument. To the extent there are any differences in baseline characteristics in e.g. population, the empirical specifications will control for pre-frost shock differences in our set of available controls.

[11] We chose the ten major trade ports based on baseline trade volumes.

3.5 Frost shocks, travel costs and emigration

Figure 3.4 plots the coefficients of our instrument, the interaction between the number of frost shocks 1864–1867 and the proximity to the nearest emigration port, on cumulated emigration from 1867 up to all years between 1867–1914. The effect of the instrument is positive for all years, implying that municipalities closer to an emigration port migrate relatively more in response to an additional frost shock as compared to municipalities further away. As seen in the figure, the coefficients get more precise after 1880 when the second wave of emigration takes off and the total mass of emigrants become greater. The first stage coefficients are strikingly stable over more than 40 years. The instrument hence predicts permanent differences in migration across municipalities, with little sign of catch-up or convergence over time.

Table 3.2 documents our first stage, where we sum the number of emigrants between 1867 and 1900. Column 1 presents the simple model which controls for the main effects of frost shocks and proximity to the nearest port, without additional control variables. Corresponding to the results presented in Figure 3.4, we find a positive relationship between our instrument and the cumulated number of emigrants over this period. Again, we can interpret the result as saying that the marginal effect of frost shocks on emigration is greater the closer an emigration port that a municipality is located. Since all variables are de-meaned, the near-zero and insignificant estimate for the main effect of frost shocks by themselves indicates that *at the mean proximity to the port*, the marginal effect of additional shocks is zero as the distance is too large. In Column 2, we include all baseline covariates in the model, which has a very small effect on the estimates. Column 3 controls for the possibility of exclusion restriction violations. If it is the case that frost shocks are worse at different distances to economic hubs, our instrument might be correlated with the intensity of economic shocks rather than simply identifying migration costs. Such an effect should however be picked up if we interact frost shocks with measures of local market access, which we define as the proximity to the nearest major trade port and nearest town, respectively. Controlling for this in Column 3 changes very little to the estimate. If anything, the result becomes stronger.

3.6 Mass Emigration and Technological Innovation

Figure 3.5 plots the coefficients on the instrument from separate reduced-form OLS regressions for every year between 1867 and 1914, with the outcome variable being an indicator taking value one if a municipality had at least one patent in that year and zero otherwise. We have included the full set of controls in these regressions, including our measure of trade access. As seen in the figure, there is an increasingly strong and significant positive effect of the instrument on patents.

Interestingly, the effect on patents is generally not significantly different from zero until the turn of the century. With some exceptions, the effect becomes more pronounced and statistically significant after 1897, three decades after the famine years that are used to define the instrument. As emigration numbers responded both immediately after the bad harvest years and during the following decades, this indicates that technological innovations took several decades to respond significantly.

Table 3.3 shows the regression output for the effect of emigration on innovation. The dependent variable is the log of the cumulated emigration 1867–1900, and the outcome is the log of the total number of patents between 1900 and 1914, the last year of our patent data set. Columns 1 to 3 display the OLS regression estimates, which indicate a positive relation between emigration and patents. The estimated elasticity is 0.3 in the baseline specification, and 0.23 when including control variables.

The IV estimates are consistent with OLS. They show that there is a strong positive effect of emigration on patents in the long run. Column 1 shows the simple regression model with only county fixed effects, estimating an elasticity of 0.77. Including pre-determined control variables lowers the estimate to 0.67. Lastly, in the most demanding specification which controls for potential violations of the exclusion restriction, the estimate is slightly larger, at 0.69. The IV estimates indicate that a ten percent increase in the number of emigrants 1867–1900 would increase the number of patents in a municipality by roughly 7 to 8 percent. For a mean municipality, this effect corresponds to an increase of 0.04 standard deviations in our outcome or 0.35 patents.

While OLS and IV coefficients are consistent in terms of the predicted sign of the relationship between emigration and innovation, the latter are at least twice as large in magnitude, indicating an even stronger positive effect. A straight-forward explanation for this difference is that migrants generally left municipalities that were economically worse off and less likely to become innovative. This would lead to OLS estimates exhibiting a negative bias. The difference between the two models' output is also consistent with the instrumental variable's compliers being a subgroup of municipalities wherein migration would cause larger economic benefits. The local average treatment effect (LATE) would hence indicate that migration caused by strong push factors, such as famines and economic shocks, could have stronger effects on local origin economies than migration caused by pull factors. Additionally, measurement error in migration could imply that OLS underestimates the true effect.

The results in Table 3.3 show that Sweden's mass migration lead to increased innovation in origin communities. However, it is hard to ascertain the economic value of these innovations. While we cannot directly assess the value of the patents in our data, we can indirectly infer it by exploiting information on the number of years that patent holders paid fees to keep their patent in force. The renewal fee was annual and covered the whole patent duration period. If a patentee chose to prolong the patent licensing for a patent, this should therefore be an indication that the economic value of the patent was higher compared to a patent that was not prolonged.

In Table 3.4, we display results from regressing emigration on the total number of patents weighted by the number of years that each patent was renewed. The results display the same pattern as previously. OLS estimates indicate a positive and significant correlation with fee-weighted patents. Again, IV estimates are about three times larger than their OLS counterparts. Patent elasticities are above unity, with estimates ranging from 1.14 without controls to 1.06 with baseline controls in our IV specifications. These results thus show that a ten percent increase in emigrants 1867–1900 lead to a ten percent increase in the number of patent fee years in a municipality between 1900 and 1914. For a mean municipality, this would correspond to an increase of 0.07 standard deviations in our outcome.

For completeness, Table A.1 in the Appendix presents the reduced form effects of our instrument on the number of patents in Columns 1 to 3 and fee-weighted patents in Columns 4 to 6. As expected the displayed coefficients are all positive.

The Swedish mass migration that was started by the famine years in the 1860s thus has a statistically significant positive effect on technological innovation in origin communities, as measured by patenting. This increase is also robust to controlling for the economic value of patents, as the total number of patent-years that patentees pay for displays a similar positive relationship. In fact the results are strengthened in terms of relative magnitudes.

Placebo treatment using non-growing season frost shocks

The motivation behind our instrument rests on the relationship between frost and agriculture. When an unexpectedly severe frost shock hits a community, individuals receive a negative economic shock and as a consequence become more likely to migrate. Thus, we would expect that frost shocks during non-growing season months, which should not affect harvests to the same extent, also should not have an effect on emigration or any other outcome variables of interest. To test this, we define non-growing season shocks in the same way as our growing season frost shocks and use them to construct a placebo instrument. We saw before in Figure 3.3 the distribution of these non-growing season frost shocks in Panel B.

In Table 3.5 we display the results from the placebo test, with non-growing season frost shocks interacted with port proximity as our placebo instrument and our patent variables of interest as outcomes. Columns 1–3 and 6–8 replicate the equivalent specifications from Table 3.3 and Table 3.4, respectively, but with the placebo instrument in place of our instrument. In Column 4 and 9 we include both the placebo instrument and the instrument, while Column 5 and 10 reproduces the coefficients from our preferred specification in Table 3.3 and Table 3.4 for reference. As seen in Columns 1–4 and 6–9, neither the placebo instrument nor the non-growing season frost shocks themselves are significantly related to our outcomes. In magnitudes, the effect of our true instrument is about ten times as large as the corresponding (statistically insignificant) effect of the non-growing season interaction with the proximity to an emigration port.

Table A.2 instead shows that the placebo instrument has a near-zero and insignificant estimated effect on emigration. Columns 1 to 3 replicate the equivalent specifications from the first stage displayed in Table 3.2, but with the placebo instrument. While the interaction with the proximity to the nearest emigration port is never significant, non growing season frost shocks by themselves show a positive association with emigration in some of the specification. However, when we include growing season frost shocks in Column 4, it turns insignificant. By contrast, the coefficient of the instrumental variable is similar in magnitude to the corresponding first-stage estimate, replicated in Column 5. As in Table 3.5, the coefficients belonging to the true instrument are about ten times as large as the corresponding coefficients belonging to the placebo instrument.

Robustness

Our results are robust to a range of alternative specifications, different samples, as well as spatial-correlation robust standard errors.

We have already seen above that the positive effect of emigration on patents is robust to the use of fee-weighted patents as outcomes. If anything, the effects were even larger in relative magnitudes.

In Table A.3 we consider an alternative specification using patents per capita, expressed per thousands, as our dependent variable. Population levels are taken from the census of 1900 which marks the end of the migration period we are considering. The coefficient of emigration is significant at the 5 percent level in all specification. A ten percent increase in the number of emigrants is associated with a 0.2 increase in the number of patents per 1000 inhabitants. Since the mean number of inhabitants is about 1,700 citizens, this would imply an increase of about 0.4 patents in a mean municipality, which is similar to what we found in our main specification.

Since there is a considerable amount of municipalities with zero patents, we have defined our main outcome variable as the natural logarithm of one plus the number of patents in a municipality. During the period 1900 to 1914, two thirds of municipalities had no registered patents. To check that these municipalities do not drive our results, we exclude them in the regressions presented in Table A.4. As seen, the significant positive effect of emigration on patents is still there. In fact elasticities are about twice as large in this subsample compared to the full sample.

To see that our results are not driven by urban areas, which exhibit a larger amount of patents during our period of study, we check in Table A.5 how robust our results are to dropping towns of different sizes. In Column 1 and 4 we only drop the capital, Stockholm, in Column 2 and 5 we drop towns with more than 10,000 people in 1865, which are 12 in total, and finally in Columns 3 and 6 we drop all 117 urban municipalities[12]. Our results remain stable, although the elasticities decrease somewhat when dropping all urban areas.

In Table A.6 we check that our results are robust to the inclusion of linear or cubic splines of the proximity to an emigration port. Columns 1 and 4 display our preferred reduced form regression including the market access controls, while Columns 2 and 5 includes a cubic spline and Columns 3 and 6 includes a linear spline with four knots[13] As seen in the table, our coefficients are remarkably stable over all specifications.

Lastly, we consider alternative standard errors. In Table A.7 we show that our results are robust to spatial-correlation robust standard errors. We display results using three different distance cutoffs: 200, 100 and 50 kilometers. Our results remain significant at the 1 percent level with slightly smaller standard errors as compared to the ones clustered at the weather station level.

3.7 Possible channels of causality

We will here discuss some potential mechanisms to why we document a positive effect of emigration on innovations.

Labor-saving technological innovation

One possible mechanism connecting migration and innovative activity is due to the effect migration may have on labor inputs in production. As labor becomes more scarce or receives better outside options, increasing the cost of labor, technological innovation may be a pathway to decrease the need for labor inputs. Ultimately, for this channel to be driving our results, innovations that occur should be *(strongly) labor saving* and not *(strongly) labor complementary*, in the terminology of Acemoglu (2010).

[12]We define an urban municipality as a municipality that had either town privileges or was administered as a market town (*Köping* or *Municipalsamhälle*) in the beginning of our sample period

[13]The location of the knots are based on Harrell's (Harrell, 2001) recommended percentiles.

Unfortunately there is no commonly accepted way to implement this terminology and categorize our very large number of patents as either labor saving or labor complementary. However, while the incentive to invent new labor-saving technologies should increase, if new patents instead concern technologies that are predominantly complements to labor we would expect such patents to decrease with increased labor costs.

While it is well known that migrants were predominantly low skilled, we can directly test if municipalities with relatively more migration also faces a decline in available low skill labor. We define low skill population as the agricultural landless population. Panel A in Figure 3.6 plots the coefficient of log emigration in 1867 to 1900 on the log of the low skill population as the dependent variable in separate IV regressions, using all four available census years. The estimated elasticities are consistently negative and economically significant around -0.2, although we cannot reject the hypothesis that they are equal to zero at the 5 percent level. Panel B instead uses the same specification to plot the corresponding effect on the share of the low skilled population within a municipality that is agricultural. The estimates are increasingly negative and significantly different from zero at the 5 percent level starting in 1900.

Table 3.6 displays the effects of emigration on the landless agricultural population for the mean of the two census years of 1900 and 1910, which are both after the main migration waves had taken place. Aggregating across two census rounds should also increase power to distinguish effects with more confidence. The instrumented variable is again total emigration 1867–1900. Columns 1 and 2 show that there is a negative effect of emigration on the size of the low skill population which is significant at the 10 percent level when including all controls. The elasticity is estimated to -0.17. In Column 3 and 4, we show the result for the share of the low skill population. The share of low skill population is negatively affected by emigration. A ten percent increase in emigrants reduces the low skill share of the population with 1 percentage points from a mean share of low skill population of 57 per cent. The results are significant at the 5 percent level when including all controls.

To test if low skilled wages increased as a result of emigration we employ yearly data on low skilled wages within agriculture at the county level. To our knowledge this is the only consistent wage series for the second part of the 19th century. It covers wages on agricultural day-workers, which essentially were low skilled and landless agricultural workers. Figure 3.8 depicts the aggregate real wage growth in this series, defined as the percentage growth rate between year $t-1$ and t. The mean real wage growth is 3 per cent over this period. Table 3.7 displays the relationship between cumulative emigration in the preceding five year period and the real wage growth in each year for our county level panel with low skilled wages spanning the years 1860-1914. While all specifications include county and year fixed effects, Column 2 includes linear time trends for each county and Column 3 includes linear time trends in baseline county characteristics[14]. In Columns 4-6 we rerun the same regressions, but include same year emigration as well. A doubling of emigration during a five year period increased the real wage growth by 0.9 to 1.9 percentage points in the end of that period. Instead, emigration in the same year has a negative association with wage growth, although we cannot reject the hypothesis that it is equal to zero at the 10 percent level, suggesting that migration decreases when times are good.

The result that Swedish US-emigration reduced the size of the landless agricultural population, and their share of total population, is consistent with that emigration may have induced technological innovation through labor scarcity. Moreover, supporting this mechanism, we have displayed that more emigration is associated with higher wages for low-skilled labor.

Return migration

Almost a fifth of emigrants eventually returned to Sweden. However, many of these returned after the start of the First World War and less

[14]Controls include the log area, the log number of urban municipalities, latitude and longitude, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber.

than 10 percent had returned in 1910. Nevertheless, these returning migrants could have an effect on the knowledge production in Sweden as they may bring important human capital home when they come back from overseas. For return migration to explain our results it is however necessary that returning migrants positively affected technological innovations in the *same* municipalities that exhibited high emigration. Unfortunately it may prove difficult to fully separate emigration from return migration.

To examine the likelihood that migrants brought home new ideas of technological innovations, we can instead study the occupational distribution of emigrants and inventors. Figure 3.7 displays these alongside the total population. We follow the Historical International Standard Classification of Occupations (HISCO) when classifying occupations in seven main groups[15]. As seen in the figure, emigrants were chiefly within agriculture followed by the service sector and the industrial sectors. In terms of skill level, these sectors consist of mostly lower skill workers. By contrast, inventors in our data were mostly high skilled. The clearly most common profession of inventors is engineer with about 30 percent of patents, with the second most common being managers with about 10 percent. No other professions exceeds five percent of the share of patents.

Although it seems unlikely that a significant amount of migrants returned home and patented technological innovations themselves, it is still possible that they transmitted ideas and human capital to their surroundings. As an indirect test of the general importance of return migrants on Swedish origin communities, Karadja and Prawitz (2016) test whether locations that had also had more membership in temperance and free church movements. They argue that since the Swedish free

[15]The main categories in HISCO are the following: *Professionals* includes professional, technical and related workers. *Administrative* includes administrative and managerial workers. *Clerical* includes clerical and related workers. *Sales* includes sales workers. *Service* includes service workers. *Agricultural* includes agricultural, animal husbandry and forestry workers, fishermen and hunters. *Industrial* includes production and related workers, transport equipment operators and laborers

churches and the temperance movement in particular were influenced by their American counterparts, that this test can be used as a gauge of the cultural importance of return migration on origin municipalities. They find that emigration either reduced or had no effect on these organizations, and hence argue that return migrants seem to have had a relatively limited local influence in origin emigrant municipalities.

An alternative channel, through which return migration could affect technological innovation is by bringing home capital. Although such cases surely exist it is however unclear to what extent this took place. Failing migrants returning home may have little capital to invest. Abramitzky et al. (2014) and Ward (2015) find that on average there was negative self-selection of returning migrants to Europe, suggesting that this channel may have played a minor role.

3.8 Conclusions

This paper uses the Age of Mass Migration, when 30 million Europeans left their home countries to settle in the United States, to measure the effects of migration on technological innovations in the sending communities. We have focused on Sweden, where about a quarter of the initial population migrated.

We have shown that migration may cause an increase in innovations in the sending location. Using an instrument based on travel costs and the severe agricultural shocks that sparked the initial wave of migration to the United States, our IV estimates suggest that a ten percent increase in the number of emigrants during the main Swedish transatlantic emigration period 1867–1900 would have increased the number of patents with about 7 percent at the end of this period. Moreover, weighting number of patents by patent fee years paid, as a measure of patent quality, strengthens our positive results.

Discussing possible mechanisms, we have suggested that low skilled labor scarcity may be an explanation for these results. In favor of this channel, we find that both the volume and the relative share of the landless agricultural population decreases in high emigration municipalities after the main emigration waves had taken place. Additionally, consistent with this mechanism, we find suggestive evidence that emigration increased low skilled wages.

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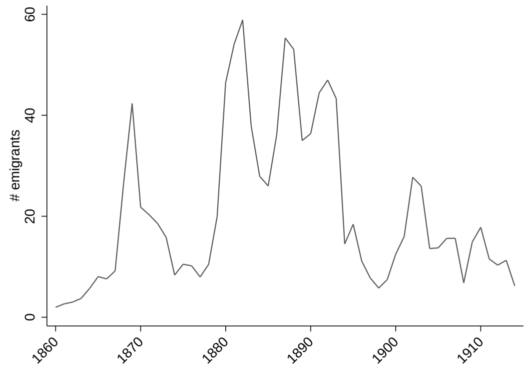
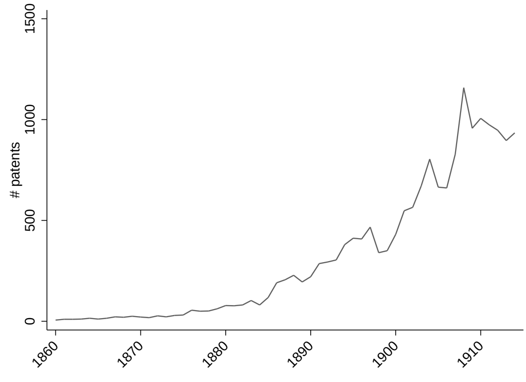
Figures and Tables

Figure 3.1: Aggregate national time series, 1860-1914

(a) Emigration

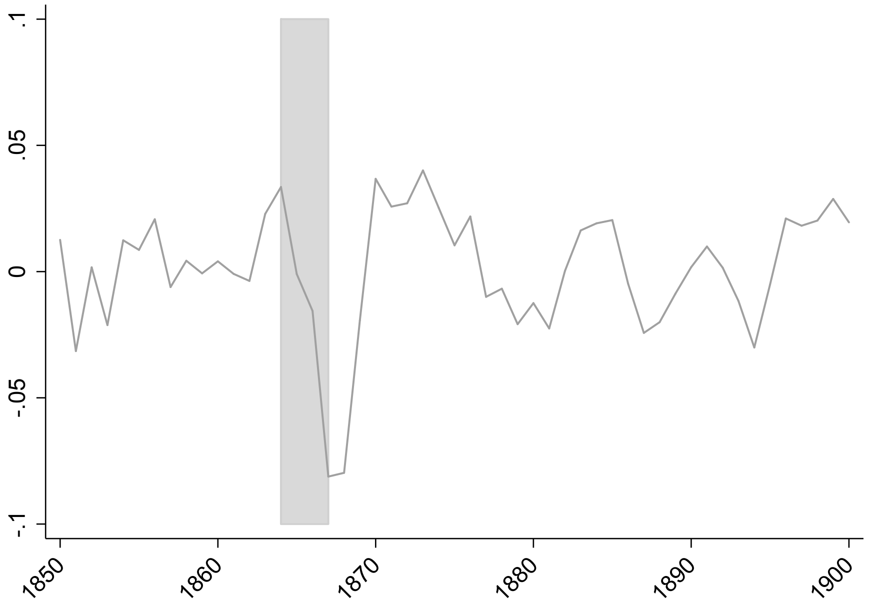


(b) Patents



Notes: This figure displays the aggregate yearly flow of emigrants in thousands (Panel A) and granted Swedish patents with an inventor residing in Sweden (Panel B).

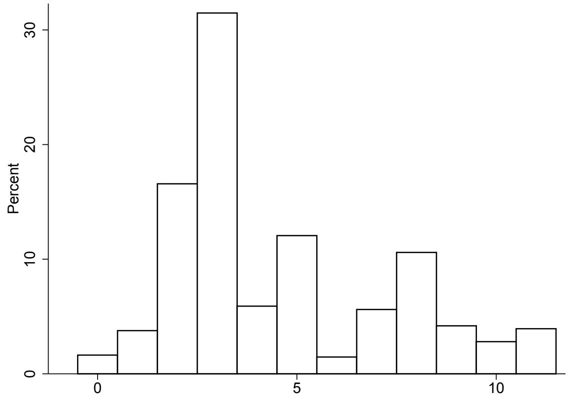
Figure 3.2: Detrended Swedish real GDP per capita 1850–1900



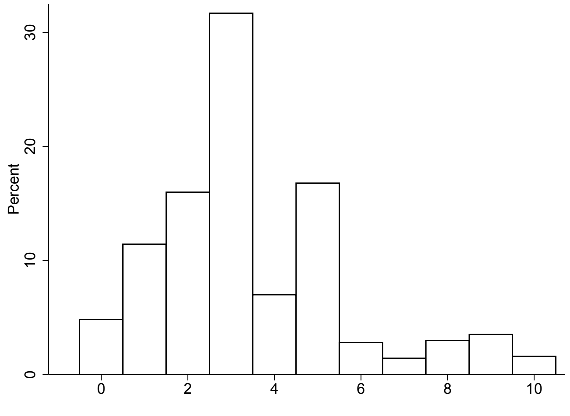
Notes: This figure displays the cyclical component of Swedish real GDP per capita (using a Hodrick Prescott-filter with smoothing parameter set to 100). The shaded area highlights the years used when defining our measure of frost shocks, 1864–67. Source: Edvinsson (2013).

Figure 3.3: Distribution of frost shocks

(a) Growing season

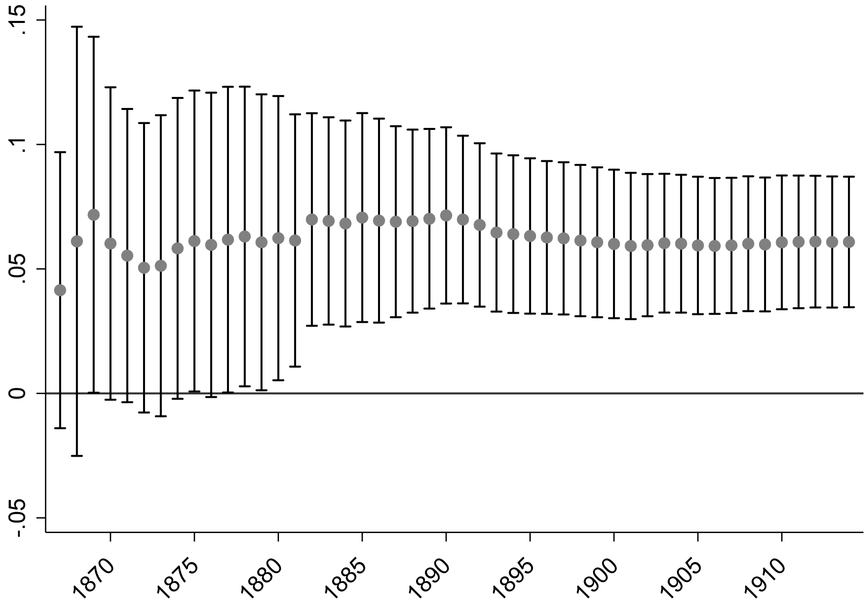


(b) Non growing season



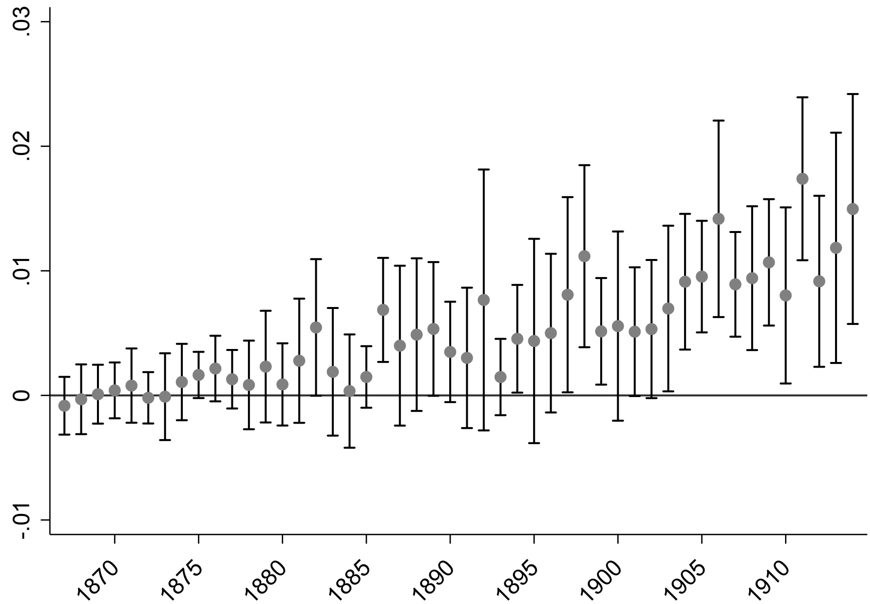
Notes: Panel A displays the distribution of the number of frost shocks during the growing season months. Panel B displays the distribution of the number of frost shocks during the non growing season months.

Figure 3.4: First stage coefficients of instrument



Notes: This figure displays the coefficients and confidence intervals at the 95 percent level of the interaction of frost shocks in 1864-67 and the proximity to the nearest emigration port with the log number of emigrants between 1867 and year t, $t = 1867, 1868, ..., 1914$ as the dependent variable. Proximity is defined as minus the log of distance. All regressions include county fixed effects, the log population in 1865, an indicator that is 1 if a municipality had at least one patent in 1856-63 and zero otherwise, the log of one plus the number of emigrants in 1856-63, the number of growing season frost shocks in 1864-1867, the proximity to the nearest emigration port, railway, town and weather station, the log area, an indicator for if a municipality is urban or not, log latitude and longitude, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. Additionally we include the interaction between growing season frost shocks and the log distance to the nearest town and trade port, respectively. Standard errors are clustered at the weather station level.

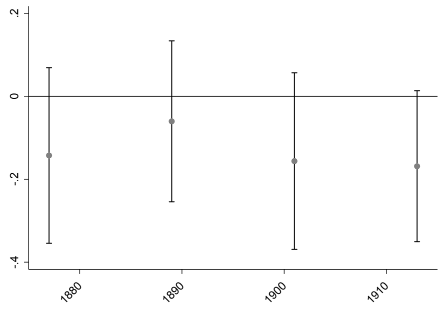
Figure 3.5: Reduced-form coefficients of instrument - yearly effects on having at least one patent



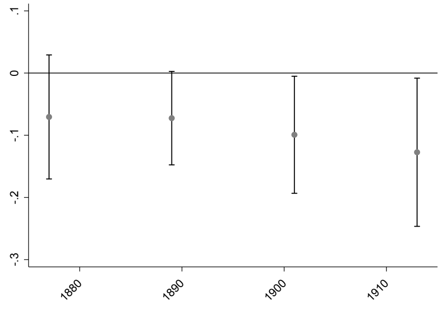
Notes: This figure displays the coefficients and confidence intervals at the 95 percent level of the interaction of frost shocks in 1864-67 and the proximity to the nearest emigration port with an indicator that is 1 if a municipality had at least one patent in year t, $t = 1861, 1861, ..., 1914$, and zero otherwise as the dependent variable. Proximity is defined as minus the log of distance. All regressions include county fixed effects, the log population in 1865, the number of growing season frost shocks in 1864-1867, the proximity to the nearest emigration port, railway, town and weather station, the log area, an indicator for if a municipality is urban or not, latitude and longitude, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. Additionally we include the interaction between growing season frost shocks and the log distance to the nearest town and trade port, respectively. Standard errors are clustered at the weather station level.

Figure 3.6: Emigration and low skilled agricultural population in 1880-1910

(a) Levels

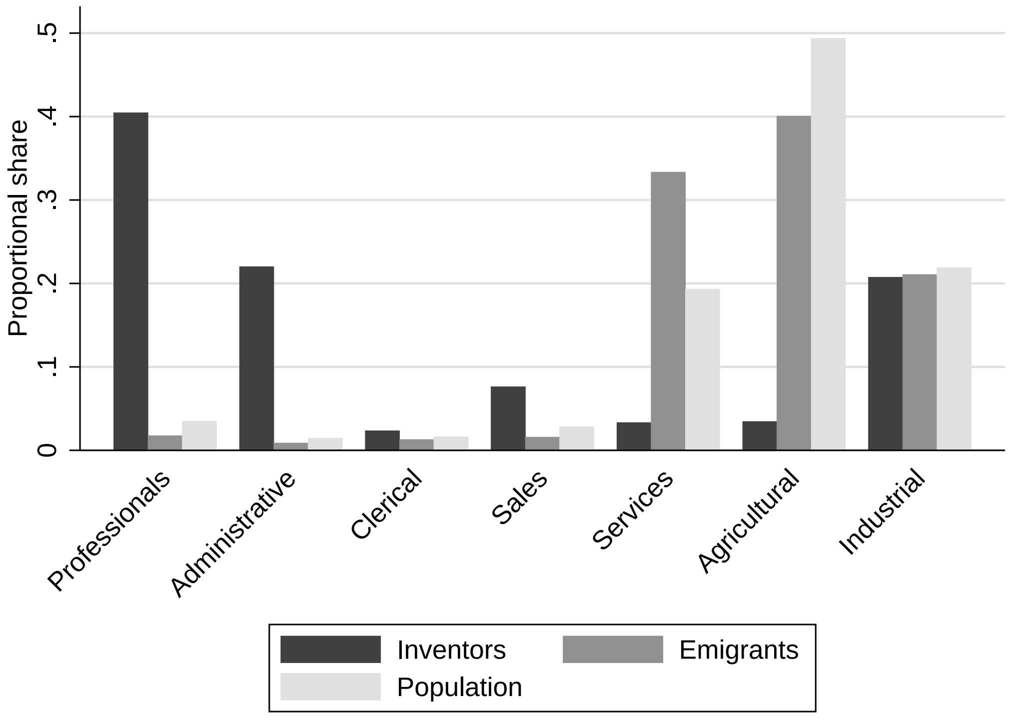


(b) Shares



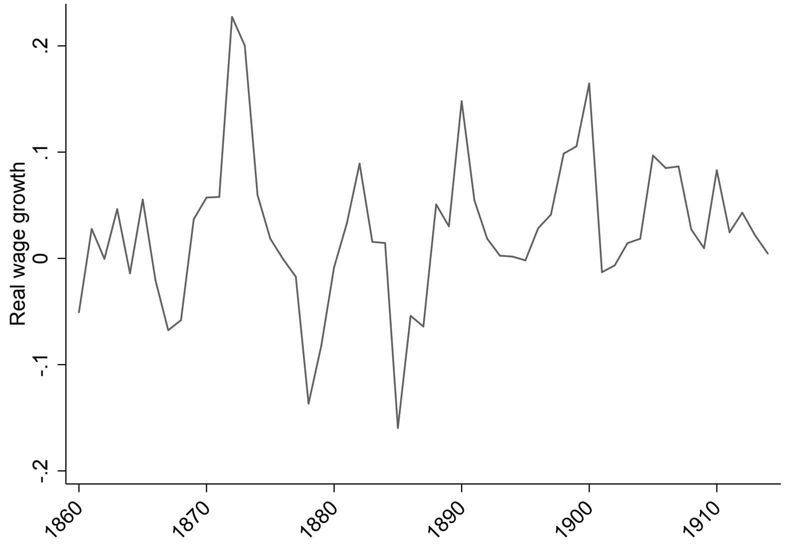
Notes: This figure displays the coefficients and confidence intervals at the 95 percent level of the log of the number of emigrants in separate IV regressions with the agricultural share of population (Panel A) or the log of the number of the agricultural population (Panel B) in 1880, 1890, 1900 and 1910 as the dependent variable. The excluded instrument is the interaction between the number of growing season frost shocks 1864-1867 and the proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects, the log population in 1865, the number of growing season frost shocks in 1864-1867, the proximity to the nearest emigration port, railway, town and weather station, the log area, an indicator for if a municipality is urban or not, latitude and longitude, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. Additionally we include the interaction between growing season frost shocks and the log distance to the nearest town and trade port, respectively. Standard errors are clustered at the weather station level.

Figure 3.7: Distribution of occupational sector among inventors and migrants 1867-1914 compared to the population 1880-1910



Notes: The figure displays the proportional share of each occupational sector among emigrants and inventors 1867-1914 alongside the mean in the population 1880-1910. Occupational sectors follow the major 7 groups of the Historical International Standard Classification of Occupations (HISCO). *Professionals* includes professional, technical and related workers. *Administrative* includes administrative and managerial workers. *Clerical* includes clerical and related workers. *Sales* includes sales workers. *Service* includes service workers. *Agricultural* includes agricultural, animal husbandry and forestry workers, fishermen and hunters. *Industrial* includes production and related workers, transport equipment operators and laborers.

Figure 3.8: Real wage growth of low skilled labor 1860-1914



Notes: The figure displays the yearly percentage real wage growth of annual agricultural day-wages 1860-1914. Nominal wages are deflated using a regional foodstuff index using 14 food items (see Jörberg (1972a) for household budget weights). Source: Jörberg (1972b,a).

Table 3.1: Summary statistics

	mean	sd	min	max	count
Log emigrants 1867-1900	5.188	1.273	0.000	10.168	2389
Log patents 1900-1914	0.528	0.957	0.000	8.350	2389
Log patent fees 1900-1914	0.855	1.455	0.000	9.926	2389
Shocks	4.570	2.760	0.000	11.000	2389
NGS Shocks	3.485	2.197	0.000	10.000	2389
Log proximity to emigration port	-5.030	0.979	-7.167	0.000	2389
Log proximity to railway	-3.148	1.445	-6.657	6.008	2389
Log proximity to weather station	-3.481	0.686	-5.312	0.504	2389
Log proximity to major town	-2.881	0.843	-5.837	0.000	2389
Log proximity to trade port	-4.390	0.894	-6.528	9.129	2389
Log proximity to capital	-5.529	0.776	-6.952	9.129	2389
Log population 1865	7.085	0.785	4.905	11.807	2389
Arable share 1810	0.702	0.221	0.000	1.000	2389
Log area	8.640	1.267	3.135	14.483	2389
Latitude	58.347	2.030	55.346	68.651	2389
Longitude	14.827	2.067	11.178	23.901	2389
Barley suitability	0.239	0.427	0.000	1.000	2389
Oats suitability	0.135	0.342	0.000	1.000	2389
Wheat suitability	0.176	0.381	0.000	1.000	2389
Livestock suitability	0.226	0.418	0.000	1.000	2389
Timber suitability	0.182	0.386	0.000	1.000	2389

Notes: This table provides summary statistics for emigration, patents and control variables. Log denotes the natural logarithm.

Table 3.2: The first stage

Dependent variable:	Log emigrants		
	(1)	(2)	(3)
Shocks×Log port	0.065***	0.058***	0.060***
	(0.019)	(0.014)	(0.015)
Shocks	0.007	0.017**	0.015
	(0.007)	(0.007)	(0.009)
Region FE	Yes	Yes	Yes
Controls	No	Yes	Yes
Market access controls	No	No	Yes
Observations	2389	2389	2389

Notes: OLS regressions. The dependent variable is the log number of emigrants in 1867-1900. All regressions include county fixed effects, the log population in 1865 as well as the number of growing season frost shocks 1864-1867 and the proximity to the nearest emigration port. Proximity is defined as minus the log of distance. *Controls* include the log area, an indicator for if a municipality is urban or not, latitude and longitude, the proximity to the nearest railway, town and weather station, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. *Market Access controls* includes the interaction between growing season frost shocks and the proximity to the nearest town and trade port, respectively. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 3.3: The effects of log number of emigrants 1867-1900 on log number of patents 1900-1914

Dependent variable:	Log patents					
	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Log emigrants	0.302***	0.233***	0.232***	0.767**	0.669***	0.686***
	(0.051)	(0.031)	(0.031)	(0.331)	(0.256)	(0.229)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes
Market access controls	No	No	Yes	No	No	Yes
Observations	2389	2389	2389	2389	2389	2389
F-statistic				11.71	17.05	16.84

Notes: OLS and 2SLS regressions. The dependent variable is the log number of patents in 1900-1914. The excluded instrument is the interaction between the number of growing season frost shocks 1864-1867 and the proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects, the log population in 1865 as well as the number of growing season frost shocks 1864-1867 and the proximity to nearest emigration port. *Controls* include the log area, an indicator for if a municipality is urban or not, latitude and longitude, the proximity to the nearest railway, town and weather station, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. *Market Access controls* includes the interaction between growing season frost shocks and the proximity to the nearest town and trade port, respectively. The F-statistic refers to the excluded instrument. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 3.4: The effects of log number of emigrants 1867-1900 on log number of patents in 1900-1914 weighted by patent fees paid

Dependent variable:	Log patent fees					
	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Log emigrants	0.428***	0.331***	0.330***	1.139**	1.063**	1.098***
	(0.067)	(0.046)	(0.046)	(0.514)	(0.429)	(0.384)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes
Market access controls	No	No	Yes	No	No	Yes
Observations	2389	2389	2389	2389	2389	2389
F-statistic				11.71	17.05	16.84

Notes: OLS and 2SLS regressions. The dependent variable is the log number of patents in 1900-1914, weighted by patent fees paid. The excluded instrument is the interaction between the number of growing season frost shocks 1864-1867 and the proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects, the log population in 1865 as well as the number of growing season frost shocks 1864-1867 and the proximity to nearest emigration port. *Controls* include the log area, an indicator for if a municipality is urban or not, latitude and longitude, the proximity to the nearest railway, town and weather station, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. *Market Access controls* includes the interaction between growing season frost shocks and the proximity to the nearest town and trade port, respectively. The F-statistic refers to the excluded instrument. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$..

Table 3.5: Placebo - Non growing season frost shocks in 1864-67

Dependent variable:	Log patents					Log patent fees				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
NGS Shocks×Log port	-0.020	-0.000	0.001	-0.005		-0.019	0.002	0.004	-0.003	
	(0.015)	(0.012)	(0.012)	(0.010)		(0.023)	(0.020)	(0.019)	(0.015)	
NGS Shocks	0.003	-0.002	-0.008	-0.012		0.008	0.004	-0.005	-0.009	
	(0.010)	(0.008)	(0.010)	(0.008)		(0.014)	(0.011)	(0.013)	(0.011)	
Shocks×Log port				0.044***	0.041***				0.068***	0.066***
				(0.009)	(0.008)				(0.014)	(0.013)
Shocks				-0.007	-0.011*				-0.017	-0.020*
				(0.007)	(0.006)				(0.011)	(0.010)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Market access controls	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Observations	2389	2389	2389	2389	2389	2389	2389	2389	2389	2389

Notes: OLS regressions. The dependent variable is the log number of patents in 1900-1914 in Columns 1-5 and log of fee-weighted patents in Columns 6-10. All regressions include county fixed effects, the log population in 1865 as well as the number of growing season frost shocks 1864-1867 and the proximity to the nearest emigration port. Proximity is defined as minus the log of distance. *Controls* include the log area, an indicator for if a municipality is urban or not, latitude and longitude, the proximity to the nearest railway, town and weather station, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. *Market Access controls* includes the interaction between growing season frost shocks and the proximity to the nearest town and trade port, respectively. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 3.6: The effects of emigration on low skilled agricultural population 1900-1910

Dependent variable:	Log low skill		Low skill share	
	(1)	(2)	(3)	(4)
Log emigrants	-0.166	-0.165*	-0.101*	-0.115**
	(0.103)	(0.096)	(0.056)	(0.055)
Region FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Market access controls	No	Yes	No	Yes
Observations	2378	2378	2378	2378
F-stat	17.16	18.26	17.16	18.26
Mean dep. var.	5.19	5.19	0.57	0.57

Notes: 2SLS regressions. The dependent variable is the mean of the share of the low skilled agricultural population in 1900 and 1910. The excluded instrument is the interaction between the number of growing season frost shocks 1864-1867 and the proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects, the log population in 1865 as well as the number of growing season frost shocks 1864-1867 and the proximity to the nearest emigration port. *Controls* include the log area, an indicator for if a municipality is urban or not, latitude and longitude, the proximity to the nearest railway, town and weather station, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. *Market Access controls* includes the interaction between growing season frost shocks and the proximity to the nearest town and trade port, respectively. The F-statistic refers to the excluded instrument. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 3.7: Emigration and low skilled real wage growth 1860-1914

Dependent variable:	Real wage growth					
	(1)	(2)	(3)	(4)	(5)	(6)
Log(emigration preceding 5 years)	0.009***	0.015***	0.015***	0.015**	0.019***	0.019***
	(0.003)	(0.003)	(0.003)	(0.006)	(0.005)	(0.005)
Log(emigration same year)				-0.009	-0.007	-0.007
				(0.006)	(0.006)	(0.006)
Year and Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Region trends	No	Yes	Yes	No	Yes	Yes
Baseline trends	No	No	Yes	No	No	Yes
Observations	1265	1265	1265	1265	1265	1265
Mean dep. var.	0.03	0.03	0.03	0.03	0.03	0.03

Notes: Fixed effects OLS regressions. The dependent variable is the annual percentage real wage growth. Nominal wages are deflated using a regional foodstuff index using 14 food items (see Jörberg (1972a) for household budget weights). All regressions include county and year fixed effects. *Region trends* include linear county time trends. *Baseline trends* include linear trends in baseline controls. Controls include the log area, the log number of urban municipalities, latitude and longitude, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. Standard errors are clustered at the county level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

A Appendix

Table A.1: Reduced form effects of instrument on patents and patent fees

Dependent variable:	Log patents			Log patent fees		
	(1)	(2)	(3)	(4)	(5)	(6)
Shocks×Log port	0.050**	0.039***	0.041***	0.074**	0.062***	0.066***
	(0.018)	(0.011)	(0.008)	(0.028)	(0.018)	(0.013)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes
Market access controls	No	No	Yes	No	No	Yes
Observations	2389	2389	2389	2389	2389	2389

Notes: OLS regressions. The dependent variable is the log number of patents in Columns 1–3 and the log of fee-weighted patents in Columns 4–6 in the years 1900-1914. All regressions include county fixed effects, the log population in 1865 as well as the number of growing season frost shocks 1864–1867 and the proximity to the nearest emigration port. Proximity is defined as minus the log of distance. *Controls* include the log area, an indicator for if a municipality is urban or not, latitude and longitude, the proximity to the nearest railway, town and weather station, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. *Market Access controls* includes the interaction between growing season frost shocks and the proximity to the nearest town and trade port, respectively. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table A.2: Placebo - Non growing season frost shocks in 1864-67 and emigration 1867-1900

Dependent variable:	Log emigrants				
	(1)	(2)	(3)	(4)	(5)
NGS Shocks×Log port	0.011	0.016	0.016	0.005	
	(0.022)	(0.017)	(0.016)	(0.018)	
NGS Shocks	0.015	0.032**	0.032**	0.018	
	(0.016)	(0.013)	(0.014)	(0.015)	
Shocks×Log port				0.056***	0.060***
				(0.017)	(0.015)
Shocks				0.009	0.015
				(0.011)	(0.009)
Region FE	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	Yes
Market access controls	No	No	Yes	Yes	Yes
Observations	2389	2389	2389	2389	2389

Notes: OLS regressions. The dependent variable is the log number of emigrants in 1867-1900. All regressions include county fixed effects, the log population in 1865 as well as the number of growing season frost shocks 1864–1867 and the proximity to the nearest emigration port. Proximity is defined as minus the log of distance. *Controls* include the log area, an indicator for if a municipality is urban or not, latitude and longitude, the proximity to the nearest railway, town and weather station, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. *Market Access controls* includes the interaction between growing season frost shocks and the proximity to the nearest town and trade port, respectively. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table A.3: Specification check - Patents per capita

Dependent variable:	Patents per capita			Patent fees per capita		
	(1)	(2)	(3)	(4)	(5)	(6)
Log emigrants	2.378**	2.088**	2.209***	13.325**	12.323**	13.103***
	(1.119)	(0.889)	(0.755)	(5.938)	(4.998)	(4.317)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes
Market access controls	No	No	Yes	No	No	Yes
Observations	2374	2374	2374	2374	2374	2374
F-stat	12.27	17.07	18.12	12.27	17.07	18.12
Mean dep. var.	0.84	0.84	0.84	3.67	3.67	3.67

Notes: 2SLS regressions. The dependent variable is the number of patents per capita in Columns 1–3 and the log of fee-weighted patents per capita in Columns 4–6 in the years 1900–1914, as measured per 1,000 municipal inhabitants in 1900. The excluded instrument is the interaction between the number of growing season frost shocks 1864–1867 and the proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects, the log population in 1865 as well as the number of growing season frost shocks 1864–1867 and the proximity to the nearest emigration port. *Controls* include the log area, an indicator for if a municipality is urban or not, latitude and longitude, the proximity to the nearest railway, town and weather station, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. *Market Access controls* includes the interaction between growing season frost shocks and the proximity to the nearest town and trade port, respectively. The F-statistic refers to the excluded instrument. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table A.4: Robustness - Excluding municipalities with zero patents 1900-1914

Dependent variable:	Log patents			Log patent fees		
	(1)	(2)	(3)	(4)	(5)	(6)
Log emigrants	1.579***	1.387***	1.382***	2.363***	2.297***	2.295***
	(0.583)	(0.486)	(0.462)	(0.904)	(0.799)	(0.778)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes
Market access controls	No	No	Yes	No	No	Yes
Observations	790	790	790	790	790	790
F-stat	5.12	5.79	5.87	5.12	5.79	5.87
Mean dep. var.	1.60	1.60	1.60	2.58	2.58	2.58

Notes: 2SLS regressions. The dependent variable is the log number of patents in Columns 1–3 and the log of fee-weighted patents in Columns 4–6 in the years 1900–1914. The excluded instrument is the interaction between the number of growing season frost shocks 1864–1867 and the proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects, the log population in 1865 as well as the number of growing season frost shocks 1864–1867 and the proximity to the nearest emigration port. *Controls* include the log area, an indicator for if a municipality is urban or not, latitude and longitude, the proximity to the nearest railway, town and weather station, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. *Market Access controls* includes the interaction between growing season frost shocks and the proximity to the nearest town and trade port, respectively. The F-statistic refers to the excluded instrument. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table A.5: Robustness - Excluding urban municipalities

Dependent variable:	Log patents			Log patent fees		
	(1) Capital	(2) Major towns	(3) All urban	(4) Capital	(5) Major towns	(6) All urban
Log emigrants	0.650***	0.626***	0.562***	1.023***	1.022***	0.968***
	(0.223)	(0.234)	(0.209)	(0.363)	(0.384)	(0.355)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Market access controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2388	2377	2272	2388	2377	2272
F-stat	14.46	13.01	12.76	14.46	13.01	12.76
Mean dep. var.	0.52	0.51	0.42	0.85	0.83	0.70

Notes: 2SLS regressions. The dependent variable is the log number of patents in Columns 1–3 and the log of fee-weighted patents in Columns 4-6 in the years 1900–1914. The excluded instrument is the interaction between the number of growing season frost shocks 1864–1867 and the proximity to the nearest emigration port. Proximity is defined as minus the log of distance. All regressions include county fixed effects, the log population in 1865 as well as the number of growing season frost shocks 1864–1867 and the proximity to the nearest emigration port. *Controls* include the log area, an indicator for if a municipality is urban or not, latitude and longitude, the proximity to the nearest railway, town and weather station, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. *Market Access controls* includes the interaction between growing season frost shocks and the proximity to the nearest town and trade port, respectively. The F-statistic refers to the excluded instrument. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table A.6: Robustness - including linear or cubic spline of distance to nearest emigration port

Dependent variable:	Log patents			Log patent fees		
	(1)	(2)	(3)	(4)	(5)	(6)
Shocks×Log port	0.041***	0.043***	0.039***	0.066***	0.069***	0.063***
	(0.008)	(0.009)	(0.009)	(0.013)	(0.014)	(0.015)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Market access controls	Yes	Yes	Yes	Yes	Yes	Yes
Cubic spline	No	Yes	No	No	Yes	No
Linear spline	No	No	Yes	No	No	Yes
Observations	2389	2389	2389	2389	2389	2389

Notes: OLS regressions. The dependent variable is the log number of patents in Columns 1–3 and the log of fee-weighted patents in Columns 4-6 in the years 1900–1914. All regressions include county fixed effects, the log population in 1865 as well as the number of growing season frost shocks 1864–1867 and the proximity to nearest emigration port. Proximity is defined as minus the log of distance. *Controls* include the log area, an indicator for if a municipality is urban or not, latitude and longitude, the proximity to the nearest railway, town and weather station, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. *Market Access controls* includes the interaction between growing season frost shocks and the proximity to the nearest town and trade port, respectively. Standard errors are clustered at the weather station level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table A.7: Robustness - Conley standard errors

Dependent variable:	Log patents			Log patent fees		
	(1)	(2)	(3)	(4)	(5)	(6)
	200 km	100 km	50 km	200 km	100 km	50 km
Shocks×Log port	0.041***	0.041***	0.041***	0.066***	0.066***	0.066***
	(0.005)	(0.007)	(0.008)	(0.009)	(0.010)	(0.011)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Market access controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2389	2389	2389	2389	2389	2389

Notes: OLS regressions. The dependent variable is the log number of patents in Columns 1–3 and the log of fee-weighted patents in Columns 4–6 in the years 1900–1914. Spatial correlation-robust standard errors (Conley, 1999) in parenthesis. Columns 1 and 4, 2 and 5, and 3 and 6 allows spatial dependencies up to 200, 100 and 50 kilometers from the center of a municipality, respectively. All regressions include county fixed effects, the log population in 1865 as well as the number of growing season frost shocks 1864–1867 and the proximity to the nearest emigration port. Proximity is defined as minus the log of distance. *Controls* include the log area, an indicator for if a municipality is urban or not, latitude and longitude, the proximity to the nearest railway, town and weather station, the arable share of land, as well as a set of indicator variables for high soil quality for the production of barley, oats, wheat, livestock and timber. *Market Access controls* includes the interaction between growing season frost shocks and the proximity to the nearest town and trade port, respectively. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Chapter 4

Richer (and Holier) than Thou? The Effect of Relative Income Improvements on Demand for Redistribution*

4.1 Introduction

Most governments redistribute economic resources between citizens, and policies with redistributive components have become increasingly impor-

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tant in recent years (Alesina et al., 2004). However, the extent to which income and wealth are redistributed varies across countries, and the academic struggle to understand individual preferences for redistribution has been ongoing for decades. As many countries witness increasing inequality, questions about how preferences for redistribution form and change are likely to remain at the core of both the public and the academic debate.

Theoretical models of how preferences for redistribution are formed often include relative income or wealth as a key element. In seminal theoretical contributions, Romer (1975) and Meltzer and Richard (1981) suggest that as a relatively richer person benefits less from redistribution in monetary terms, she should demand less of it. An implicit assumption in these, and other, models aiming to explain individual preferences for redistribution is that people hold correct information about their position in the income distribution. However, the validity of this assumption is often rejected empirically, see e.g. Cruces et al. (2013).

We conduct an experiment on a Swedish sample. Our data consist of answers from two tailor-made surveys that are linked to individual administrative records containing information on income, wealth, education, civil status, government transfers and cognitive ability. To our knowledge, papers that address this type of question have never had access to such a rich set of information.

We use the first survey to assess if Swedes perceive their position in the income distribution correctly. We find that 86 % of the respondents believe that they are poorer, relative to others, than they actually are, while only 13 % overestimate their position. The average respondent underestimates her position by 16 percentiles. In addition, we use the administrative data to investigate heterogeneities in the documented misperceptions and find that more educated, more cognitively able, people who consume more media, and individuals who recently experienced upward income mobility hold beliefs that are significantly more accurate.

The second survey was distributed three months after the first and is a randomized experiment where half of the respondents were treated with personalized information about their true relative position in the income distribution. The second survey also elicited preferences for redistribution, party preferences and opinions on taxation from both treated and untreated respondents.

We find that informing individuals that they have a higher relative income than they thought makes them demand less redistribution and express more support for the Conservative Party. The effect is large: the demand for redistribution falls by 22.8 percent relative to the control-group mean. This effect is driven by the subset of respondents who expressed right-of-center political preferences pre-treatment, i.e. in the first survey. While they respond to the positive relative income news by moving even further to the right on the political spectrum, individuals who did not express right-of-center sympathies in the first survey are not impacted at all by the information treatment. We find that two sets of beliefs about how the economy works can explain much of the heterogeneous response: those with political preferences right-of-center tend to believe (i) that effort, rather than luck, is the main determinant behind individual economic success, and (ii) that redistribution is distortive in the sense that income taxes impact labor supply. Economic and demographic differences between the right and the left cannot explain the heterogeneous treatment response, although such differences certainly exist.

Our paper relates to the vast literature that seeks to understand how individual preferences for redistribution are formed. The theoretical predictions about relatively richer individuals wanting less redistribution, originally proposed by Romer (1975) and Meltzer and Richard (1981) have found empirical support in, for instance, Alesina and Giuliano (2010), but have also been scrutinized and challenged. For example, individuals have been found to deviate from pure self-interest in caring also about the consumption of others (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). Such other-regarding preferences tend to correlate positively with the demand for redistribution (Fong, 2001; Alesina and Giuliano, 2010). Beliefs about the income-generating process have also been studied theoretically (Piketty, 1995; Benabou and Tirole, 2006) and beliefs about the extent to which individuals' economic success can be attributed to effort, rather than to luck, have been found to be a stronger empirical determinant of preferences for redistribution than income itself (Fong, 2001).

Understanding the role that income, or perceptions of income, play for political preferences is made more difficult by the fact that other underlying variables may also cause a correlation between income and political preferences. For example, Mollerstrom and Seim (2014) find that high-IQ individuals favor less redistribution, which could reflect that high-ability individuals, who more easily succeed and tend to have higher earnings, lean toward a more individualistic, right-wing view of the world. In general, existing evidence on the impact of income on political preferences is mixed. Even though many studies provide results supporting self-interested political preferences and pocket-book voting (Peltzman, 1985; Margalit, 2013; Durante et al., 2014; Powdthavee and Oswald, 2014; Elinder et al., 2015), evidence for socially motivated political preferences has also been documented, see e.g. Sears and Funk (1990). We contribute to this literature by correcting misperceptions of respondents' relative income, yielding identified estimates of causal effects with credible external validity.

Furthermore, we contribute to a nascent literature that addresses misperceptions of relative income. Using an Argentinian sample, Cruces et al. (2013) find that individuals are wrong about their own position in the income distribution, but that there is no systematic under- or over-reporting of relative income. The same has been found using data from Spain (Fernández-Albertos and Kuo, 2015) and Germany (Engelhardt and Wagener, 2016). Using US data, Chambers et al. (2014) find that individuals tend to underestimate average income. In line with the findings we present, Grigorieff and Roth (2016) find that Americans tend to underestimate their relative income.

A shortcoming of these papers is that differences between perceived and actual income ranks may be due to deviations of self-reported income from actual income rather than incorrect beliefs about the income distribution. By using administrative data on annual income, our research design disentangles those two mechanisms. We show that misreporting of actual income is small compared to misreporting of relative income, suggesting that the validity of results obtained in similar settings where the data is not as rich, may still be high.

Given the presence of biased beliefs, it is natural to ask how individuals react to receiving correct information. The existing research provides mixed results. Cruces et al. (2013) show that respondents who overestimated their relative income prior to treatment increase their demand for redistribution, while there is no statistically significant treatment effect for those who underestimated their rank. Engelhardt and Wagener (2016) find no significant effects on political preferences from revealing correct information, while Fernández-Albertos and Kuo (2015) report statistically significant effects only for the bottom of the income distribution. Kuziemko et al. (2015) find that even though providing information about taxes and the distributions of income and wealth affects views on whether inequality is an important problem, the effects on policy views and demand for redistribution are small. Similarly, Zilinsky (2014) finds that subjects who are exposed to information about US inequality are not, on average, more willing to take specific action, such as support higher taxes, to increase equality.

Our results are also related to theoretical work on the self-reinforcing relationships between personal income, beliefs and political preferences, which have been proposed by Piketty (1995), Alesina and Angeletos (2005) and Benabou and Tirole (2006). For example, in Piketty (1995), agents with different prior beliefs about the role of effort for economic success tend to diverge in terms of incomes, political preferences and posterior beliefs because of a limited updating process. Our finding that in particular individuals with right-wing preferences decrease their demand for redistribution when faced with positive news about their relative income is consistent with this framework. The self-reinforcing nature of the relationship between income, voting and beliefs is further corroborated by another of our results: right-of-center individuals are more likely to believe in the importance of effort in the first survey, and treatment strengthens these beliefs even further.

The paper proceeds as follows. The next section describes our experiment and the resulting data. In Section 4.3, we document the results from the first survey and describe the bias in beliefs about the relative position in the income distribution held by the respondents. Section 4.4 describes the second survey and the outcome of the experiment. Section 5.7 concludes.

4.2 Data

The surveys used in this study were designed by us and implemented by Statistics Sweden. Conducting the study in collaboration with Statistics Sweden, who collect and handle official data in Sweden, allowed us to link survey data to administrative records.

The First Survey and Administrative Data

The first survey was sent by postal mail to a representative sample of 4,500 Swedish citizens above 18 years of age in May 2011. Respondents were asked to report their annual income from the previous year (2010) and to state their perceived position in the national income distribution by answering the following question: *How many percent of the Swedish population (18 years or older) do you think have a total annual income which is lower than yours?* Total annual income was explicitly defined as the sum of labor and capital income before taxes, including pensions but exclusive of transfers such as unemployment insurance. In addition, respondents were asked to state what they believed the mean annual income in Sweden to be in 2010.[1]

The first survey also asked respondents to report how often (0=never, 1=every month, 2=every week and 3=every day) they use various sources

[1] All survey questions, in the order they were presented to participants, are available in the Online Appendix.

of information, with the alternatives being printed newspapers, news on radio/TV, magazines, other radio/TV programs and news online. We define the variable *Informed* as the sum of the answers pertaining to each medium, so that a higher value of this variable indicates more extensive media usage.

In addition, the first survey elicited political party preferences, and beliefs about how distortive income taxes are and how individual economic success comes about.

There are nine main political parties in Sweden. Preferences for these were elicited by asking respondents to state the party that they would vote for if there were to be an election at the time when the respondent filled out the survey.[2] We use this information to define an indicator of left-right preferences. The binary variable *Right* assumes the value 1 if the respondent stated an intention to vote for one of the four Swedish right-of-center parties and 0 otherwise.[3]

We capture respondents' beliefs about the distortive effects of redistribution gauging agreement with the following statement: *Changes in income taxes influence how much individuals choose to work.* The binary variable *No Distort* takes the value of 1 for respondents who reported an agreement to the statement of 5 or lower on a 1-10 scale (where 10 indicated complete agreement with the statement).

The following question was used to elicit beliefs about how individual economic success comes about: *Is it mostly effort or luck that matters*

[2]The respondents also had the option to state that they did not know or did not want to answer, that they would cast a blank vote or that they would abstain from voting.

[3]As discussed in Section 5.6 and showed in the Online Appendix, our specifications are robust to alternative definitions of this variable, including one where those who abstain from voting, cast blank votes, decline to answer or vote for non-traditional parties are excluded from the analysis. Details about the left-right scale in Swedish politics can be found in Petersson (1994), Pettersson-Lidbom (2008) and Oscarsson and Holmberg, 2013. See also Alesina et al. (1997) for a comparison of the Swedish left-right scale to the American setting. The parties included in *Right* are Moderata Samlingspartiet, Folkpartiet, Centerpartiet and Kristdemokraterna. The remaining parties are Socialdemokraterna, Vänsterpartiet, Miljöpartiet, Feministiskt Initiativ and Sverigedemokraterna. Our results are also robust to replacing the binary variable by a continuous measure of political views (see Section 5.6).

for how well an individual does economically in life? Respondents were asked to indicate their answer on a scale of $1 - 10$ where 1 was defined as "Only luck" and 10 as "Only effort" and we let the binary indicator *Luck* assume the value 1 for answers below 6. If economic success is realized through effort, redistribution can be argued to be more distortive (Fong, 2001) and we use these two questions to create the index *Redist-Distort*. Following Kling et al. (2007), we construct this index by standardizing the two variables *No Distort* and *Luck* and computing the equally-weighted average. A lower index-value indicates a stronger belief that redistribution creates inefficiencies and distortions.[4]

A total of 1,562 individuals responded to the first survey. This corresponds to a response rate of 36 percent, which is in line with other postal mail surveys of similar length carried out by Statistics Sweden.

To implement the randomized experiment in the second survey, reported data on annual income and perceived relative income were required. Thus, we excluded respondents that abstained from answering these questions. We also excluded respondents who stated that they were located above what they believe to be mean income but, at the same time, below the median income, as well as respondents where the difference between self-reported and annual income according to administrative registers for 2010 was so large that the respondent probably did not correctly understand the question and, for example, reported monthly instead of annual income.[5] After these exclusions, the sample is comprised of 1,242 respondents.

The survey responses were linked to national administrative records at the individual level, mainly from the longitudinal integration database for health insurance and labor market studies (LISA, by Swedish acronym).

[4]The index is computed by first subtracting the control group mean from each observation and then dividing by the control group standard deviation. Any missing values of the variables in the index are ignored when taking the mean to form the index.

[5]Our results are robust to ignoring the last two exclusion criteria. They are also robust to wide variations in the allowed divergence between stated and administratively reported income. In the specifications reported here, we allowed for a maximum difference between stated and administratively reported income of 750 percent.

LISA comprises information on age, education, civil status, number of children, home region, and government transfers such as unemployment insurance and social security benefits. In addition, the data were complemented by annual taxable income for the years 1999-2010, and by data on real estate and financial wealth for the year 2006 from the Income and Tax register.[6]

Finally, for a subset of men born after 1950 and before 1981, we retrieve test scores for cognitive ability from the Swedish Military Records.[7] Until 1999, military enlistment was mandatory for all Swedish men. The enlistment normally took place in the year a man turned 18 or 19 and encompassed a test of cognitive ability. This test consisted of four sections assessing logical ability, verbal ability, technological comprehension and metal folding, comprising 40 questions each, and is an accepted measure of intelligence (Carlstedt, 2000; Heckman et al., 2006; Lindqvist and Westman, 2012). The combined score is converted into a scale of 1 to 9 and we let the dummy variable *IQ* assume the value 1 for values of cognitive ability above the sample median.

The first survey also contained questions regarding income mobility. We asked respondents about their own mobility through questions where we asked them to state their relative position in the income distribution 10 years ago and 10 years into the future. By combining these with their perceived current position, we define the variables *Subjective Relative Income Growth* (past position minus current position) and *Subjective Future Relative Income Growth* (future position minus current position). The first survey also elicited respondents' opinions about income mobility in general through the following question: *If one is born in a certain income group, one will probably not end up in another income group in the future.* Respondents were asked to agree or disagree with this statement on a 1-10 scale, where 10 incidates complete agreement.[8]

[6]Administrative data on wealth exist because Sweden used to tax wealth. When the tax was repealed, in 2007, the Tax Agency ceased collecting these data.

[7]There are men in our sample born before 1951, but for these cohorts military enlistment data are not available in digitized form.

[8]While those who chose to respond to the survey are older, have fewer children

Design of the Experiment and the Second Survey

In August 2011, three months after the first survey, a second survey was sent to those who responded to the first. Half of the second-round recipients were randomly selected to receive a treatment revealing their actual position in the income distribution.

The income distribution of the full Swedish population was calculated using administrative data. However, we used the self-reported income from the first survey to locate each individual's percentile, to avoid the variation that would stem from informing some subjects about both their absolute and their relative income. This procedure also makes our results comparable to previous studies, such as Cruces et al. (2013), which do not have access to administrative records.

As expected, administrative and self-reported income are highly correlated. In Figure 4.1, we compute the bias in reported income as the difference between reported and administrative income, divided by administrative income. The distribution is centered around zero and indicates that our respondents do not hold biased beliefs about their own absolute income. This result is important for understanding our treatment, as it shows that bias does not stem from misreported actual incomes. While the mean perception of relative income is understimated by 16 percentiles, income levels are not significantly misreported on average (p=0.19).[9]

Information about the respondents' true relative position in the income distribution was provided to the treatment group using a scale reprinted in Figure 4.2. The explanation entails a horizontal line with numbers representing income deciles. For each decile, the actual median annual income in 2010 was stated. A marker indicated where in the

living at home, are more educated and have a higher income compared to non-respondents, we verify the robustness of our results to reweighting observations to match the overall population according to key variables. See Online Appendix Table A.1 for a comparison between the population and our sample.

[9]This finding is also informative for similar studies, which do not include data on actual incomes. The bias found in the other contexts is unlikely due to biased income reporting.

distribution the respondent's income was located. The following information was provided: *In the previous survey, you reported an annual income for 2010 of [X] SEK. In the figure below we have indicated where your income is located on the income scale.*[10] To ensure that respondents considered the information, this statement was immediately followed by a question asking individuals to categorize themselves as being in either one of the five lowest or one of the five highest deciles.[11]

The treatment is relatively subtle as we do not explicitly compare an individual's actual position on the income scale with the beliefs stated in the first survey. This, together with the time lag between the two surveys, reduces the likelihood that our results are due to the framing effect that could arise if subjects were told that they were "wrong" or "right" in the first survey. After the information treatment and the simple follow-up question, the second survey was identical for both groups.

We use three outcome variables to study the effect of treatment on individuals' preferences. The first is a question about the demand for redistribution by means of economic policies, where subjects indicate their preferred level of income redistribution. The scale comprised 10 steps, with 1 being defined as no redistribution (meaning that the government does not influence the income distribution at all) and 10 as full redistribution (everyone receives the same income after taxes and subsidies). We let the variable *Against-Redist* assume the value 1 if the individual provided an answer below 5 to this question, which corresponds to demanding less redistribution than the control-group median.

Our second outcome variable, labeled *Cons. Party*, assumes the value 1 if a respondent reported that she would vote for the Conservative Party (Moderata Samlingspartiet) if an election were to be held that

[10] This intervention is similar to Card et al. (2012) and Perez-Truglia (2015), but in those papers the subjects learn both key characteristics of the income distribution (self-image) and also how other individuals perceive them (social-image), thus receiving a double-edged treatment.

[11] At the time when our survey was conducted, there existed no easy way, for example through a website, to find information about one's relative income. To get a precise measure of relative income, a person had to formally request and purchase the relevant information from Statistics Sweden.

day. This party is the one most strongly associated with low levels of taxation and redistribution among the Swedish parties (c.f. Petersson, 1994; Pettersson-Lidbom, 2008; Oscarsson and Holmberg, 2013).

The third outcome variable gauges the response to the following question: *Would you like to change the income taxes that we have in Sweden today, and if so in what way?* Subjects who prefer to decrease taxes were assigned the value 1 for this indicator, labeled *Decrease Tax*. Individuals who wanted no change or an increase were given the value 0.

We consider these three outcomes separately but also create a summary index using *Against-Redist*, *Cons. Party* and *Decrease Tax* which we label the *Outcome Index*. The three components of the *Outcome Index* have equal weight and following Kling et al. (2007), the index is calculated in the same way as our other index, as described in footnote 4. A higher value of the *Outcome Index* indicates preferences that are more right-leaning and less in favor of redistribution.[12]

The response rate of the second survey was considerably higher than that of the first. This is not surprising as the first round selects individuals willing to fill out surveys in general. Out of the first-round sample of 1,242, 1001 individuals, or 80.5 percent, completed the second round.[13]

4.3 Bias in Perceptions of Relative Income

To what extent do respondents have a biased perception of where in the income distribution they are located? We define the bias of a respondent as the difference between her actual and perceived income percentile. Figure 4.3 displays the distribution of bias. It is substantially skewed to

[12] Our data set does not contain any variables that could be used as "real" outcome variables, such as a voter being registered as a supporter of or donor to a particular party. In Sweden this information is not part of the administrative records.

[13] Importantly, we find no impact of the treatment on the likelihood that a person responded to the second survey, conditional on having responded to the first, as shown in Online Appendix Table A.2. We also show, in the Online Appendix Table A.3 that the treatment and control groups are balanced with respect to a range of characteristics.

the left, with a median of -18 and a mean of -16.6. This indicates that a vast majority of respondents underestimate their position, i.e. believe that they are poorer – relative to other Swedes – than they actually are.

In fact, 85.8 percent report a position in the distribution that is below their actual location, while only 12.5 percent report a position above. When weighting these observations by population weights, the corresponding figures are 82.4 and 15.7 respectively, indicating that this result is not driven by a selected group of individuals with certain observable characteristics who chose to respond to our survey (we can not rule out that the sample is selected on unobservable characteristics).

If we do not allow for any error in the perceived position, only 1.7 percent of our sample have an unbiased view of their relative income. However, even if we permit some error, the pattern is similar. 68 percent of our sample underestimate their relative income by more than 10 percentage points, while only 6 percent overestimate their position by the same amount (63 and 8 percent respectively using population weights). This implies that out of those with an absolute bias of more than 10 percentage points, 92 percent exhibit a negative bias. A comparison between Figure 4.3, which shows the bias in perceived position in the income distribution, and Figure 4.1, which displays the bias in reported income, indicates that our results are not driven by biased beliefs about own annual income.

Taken together, our results show that Swedes generally believe that they are relatively poorer than they actually are. This finding differs from results found in Cruces et al. (2013), Fernández-Albertos and Kuo (2015) and Engelhardt and Wagener (2016), who document that misperceptions about relative income are balanced among Argentinian, Spanish and German citizens, respectively. However, our finding is in line with the bias documented in the US. Using an American sample, Grigorieff and Roth (2016) find that 68 % of the respondents *under*estimate their relative income, while 28 % think their position is higher than it actually is. Below, we investigate potential determinants of bias in our sample.

We start by investigating if the bias differs across the income distribution. Figure 4.4 shows the perceived position in the income distribution in relation to the actual position. The estimated slope is 0.657, which is significantly different from 1 ($p<0.01$). Figure 4.4 also shows more noise and a less pronounced negative bias on average among low-income individuals (possibly because the perceived relative income percentile has a lower bound of zero).

There are several ways to test if the bias differs across subgroups. Columns 1 and 2 of Table 4.1 present the results from regressing bias on individual characteristics. We control for indicators of actual position, so that the specification exploits variation in the perceived position only. A positive coefficient thus corresponds to a more positive value of bias (which generally implies more accurate beliefs as most individuals underestimate their position). Two subgroups may have similar average beliefs about actual position, but different variation in their beliefs. Such differences are also informative about perceptions of the income distribution. Columns 3 and 4 of Table 4.1 repeat the analysis presented in the first two columns, but replaces the dependent variable with the absolute value of the bias.

Table 4.1 paints a relatively consistent picture about differences in bias between subgroups. Subgroups that exhibit more positive bias also have less dispersed beliefs. As is evident from Columns 1 and 2 of Table 4.1, respondents with at least college education have an average bias that is 2.6 percentage points less negative than those without college education. The same holds when considering the absolute value of bias, in Columns 3 and 4. Respondents with above-average cognitive ability have more precise and less dispersed beliefs than those with below-average ability, the difference in average bias corresponds to 3.7 points. Individuals who report consuming more media exhibit less bias, but whether the respondent lives in an urban area is not related to the extent of bias, although exposure to individuals from different socio-economic groups is arguably higher in cities. No differences in bias is documented between right-of-center individuals and other respondents. Older individuals are worse at estimating their position in the income distribution but neither wealth nor income are associated with bias or the absolute value of bias.

One strand of the literature has argued that the different levels of redistribution in the US and Europe is not due to income inequality or social mobility per se, but to differing perceptions about inequality or mobility (cf. Benabou and Tirole, 2006). If individuals fail to perceive the determinants of social mobility, their perceived position in the income distribution may be imperfect as well. In Table 4.1 we test whether individuals who have experienced social mobility in the recent past are better at placing themselves in the income distribution. Those who experienced the largest relative income growth from 2000 to 2010 (top quartile of changes) have on average 2.4 percentage points less negative bias than others. This is verified in the row below, which comprares bias among those who report a positive relative income growth over the past ten years to those who report no or negative growth. Even stronger in magnitude is the relation between expecting positive relative income growth over the next ten years and bias, captured by the variable *Subjective Future Rel. Inc. Growth*. Finally, the perception of income mobility in general terms is also strongly correlated with less negative bias, as shown in the final row. Without an objectively established metric for social mobility, our different measures do suggest that both actual and expected mobility matter for beliefs about one's position in the income distribution.

Relating these results to the canonical theoretical frameworks of Romer (1975) and Meltzer and Richard (1981), we conclude that the implicit assumption of full and correct information about relative income does not hold. Moreover, the bias differs across groups.

4.4 Correcting the bias

We now investigate the impact of correcting inaccurate beliefs about relative income. We start by presenting average effects of the information treatment within bias-categories and then proceed to exploring heterogeneous responses by bias, political opinions and economic views. We follow Cruces et al. (2013) and in the main analysis, respondents who underestimate (overestimate) their relative income by more than 10 percentage points are categorized as exhibiting a negative (positive) bias. As discussed in Section 5.6 and presented in the Online Appendix, our results are robust to varying this cutoff. Section 5.6 also contains numerous other robustness. In addition, we conduct a dose-response analysis where we allow the treatment effect to vary linearly depending on the extent of bias. The results of the dose-response analysis are discussed in Section 5.6 and the full analysis is presented in the Online Appendix.

Average Effects

Table 4.2 presents the average effects of treatment on the three outcome variables *Against-Redist*, *Cons. Party* and *Decrease Tax*, as well as on the composite *Outcome Index* which, as described above, is an equally-weighted, composite measure of the three outcome variables such that a higher value indicates more right-leaning and more anti-redistribution preferences. The results suggest that the treatment leads to a significant shift in preferences towards the political right among those with a negative bias of more than 10 percentage points.[14] While the point estimate of the outcome index within the positive-bias group is almost as large as within the group with negative bias, only six percent of the sample have a positive bias of more than 10 percentage points and the coefficient for this group is thus very imprecisely estimated. Column 2 shows that the treatment increases the probability of a person with a negative bias larger than 10 percentage points demanding redistribution below the median by 8.1 percentage points from a base of 35.6 percent in the control group, i.e. a 22.8 percent increase. It also increases support for the Conservative Party (Column 3) by 8.1 percentage points, a relative increase of 32.2 percent from the control group mean. Finally, in Column 4, the point estimate for the willingness to decrease taxes is

[14] The Online Appendix provides plots visualizing the average effect by showing the distribution of responses by treatment status. The average effect seems to be driven by responses across the distribution.

positive, but not significantly different from zero.[15]

Heterogeneous Effects

As an overwhelming majority of our respondents underestimate their relative income, we continue to focus on them and explore heterogeneous responses to treatment among those who underestimate their relative income in the first survey by at least 10 percentage points. We first investigate the interaction of treatment and the support for a right-of-center party in the first survey, among those who underestimate their position.

Table 4.3 shows that within the right-of-center group, the treatment effect on the *Outcome Index* more than doubles in size as compared to the average effect, while the effect among the non-right is a precisely estimated zero.[16] The average treatment effects reported in Table 4.2 thus seem to be entirely driven by the respondents with prior political preferences right-of-center.

Columns 2 to 4 of Table 4.3 consider each outcome variable separately. The probability of demanding low levels of redistribution increases with treatment by approximately 12 percentage points among those with right-of-center preferences. Support for the Conservative Party increases by approximately 15 percentage points upon treatment, implying a reshuffling of party allegiances among those with prior party pref-

[15]These results indicate that people with a negative bias larger than 10 percentage points react to treatment, whereas others do not. Note however, that the treatment effect on the Outcome Index for those with a positive bias larger than 10 percentage point is of similar magnitude (but with very large standard errors and hence statistically insignificant). A potential interpretation of this could be that also these people are impacted by the information treatment and hence that this does not work only through correcting beliefs about relative income but also through, for example, increasing the saliency of relative income in general. We do not believe that this is a correct, given the large standard errors and the fact that the components of the Outcome Index (studied in Columns 2-4) render an ambiguous picture for both people with a positive bias larger than 10 percentage points, and for the people with an absolute bias which is smaller than 10 percentage points. This alternative interpretation of our data is discussed further in Section 4.3.

[16]Figure A.2 in the Online Appendix confirms the hetoregenous responses graphically.

erences right-of-center. In both cases, the treatment effect in the rest of the sample is close to zero and insignificant. The willingness to decrease taxes is not significantly affected in either group.[17]

Notice that this heterogeneous treatment effect contributes to a polarization of political preferences across the political spectrum, with right-wing individuals becoming less supportive of redistributive policies while the non-right maintaining their views even when confronted with the news that they are richer than they thought they were. It is interesting to consider what would happen in a setting where most individuals instead overestimate their relative position. If our results are symmetric in the sense that respondents with prior right-of-center preferences also react more strongly to negative news about their relative position, such an information intervention might reduce the dispersion of opinions.

To understand the working of the heterogeneous treatment effect in more detail, we exploit the vast information that we hold on background characteristics and find that having right-of-center preferences in the first survey is positively correlated with age, being married, length of education, cognitive ability, income, wealth and self-reported media consumption, see Online Appendix Table A.4. As several of these variables also predict a lower relative income bias, we investigate the interactions between these variables and the treatment effects but find that none of the interactions between treatment, bias and the heterogeneity of interest is statistically significant and that the heterogeneous effects of right-of-center preferences remain even when controlling simultaneously for these variables, see Online Appendix Tables A.5–A.6.

Instead, we turn to beliefs about the workings of the economy and how economic success is generated, in an attempt to understand why

[17] A potential concern is that the heterogeneity is driven exclusively by the non-conservative party variable. When dividing the sample into right-of-center and non-right-of-center, supporting the Conservative party might only be considered by individuals with prior preferences right-of-center, and the heterogeneous treatment effect would obtain automatically. We address, and alleviate, this concern in Section 5.6 and in the Online Appendix.

those with prior preferences right-of-center respond to being informed that they are richer than believed, while those with non-right preferences do not. Right-of-center preferences are positively correlated with the belief that individual economic success is the result of personal effort rather than luck. There is also a positive correlation between right-of-center preferences and the belief that income taxes are distorting and impacting labor supply. Table 4.4 reports the results from estimating separate treatment effects by prior beliefs. Columns 1 to 3 shows that treated subjects on average support less redistributive policies, but that this effect is zero for those who believe that redistribution is non-distortive and that luck is the key determinant of economic success.

These results suggest that beliefs about how the economy works play an important role in shaping the response to treatment. However, as these beliefs are correlated with right-of-center political preferences, the variation in Columns 1 to 3 in Table 4.4 may simply be a result of this correlation. In Column 4, we add a control variable for right-of-center political preferences and show that the treatment effect remains significant. Restricting the sample to the respondents who reported right-of-center preferences (Column 5), the effect of the interaction with beliefs disappears. Finally, we restrict the sample to those who reported non-right preferences in the first survey (Column 6) and even within this group, those who believe that taxation is distortive and that effort is more important than luck respond more to treatment (although the result is only marginally statistically significant for this group).

Self-serving bias could play a role for why this heterogeneity arises. Research in sociology and political science shows that individuals tend to be very persistent in their views on the determinants of economic success (Lane, 1959; Hochschild, 1986, 1996; Lamont and Lamont, 2009). If the positive news about relative position received by treated subjects who believe in the importance of effort reinforces their view that effort determines success, they might demand less redistribution. Evidence of such a reinforcement effect on beliefs regarding the respective importance of luck and effort can be found in Column 5 in Table 4.2. Moreover, as discussed in the introduction, out results supports the notion of self-reinforcing relationships between beliefs and political preferences (Piketty, 1995; Alesina and Angeletos, 2005, and Benabou and Tirole, 2006).

Taken together, we conclude that informing a person that she is relatively richer than previously believed has very different effects depending on the individual's political orientation. Individuals with prior political preferences right-of-center, who believe that effort is conducive to economic success and who think that redistribution creates distortions respond more strongly than individuals who do not share these views.

Robustness

All robustness tests below, including more detailed information about the tests, are available in the Online Appendixes unless otherwise stated.[18]

Online Appendix Tables A.7 and A.8 display our main specifications allowing for either a smaller or a larger error before defining a person as exhibiting a bias. Instead of setting the cutoff to 10 percentage points, Tables Tables A.7 and A.8 employ cutoffs of 5 and 15 percentage points respectively. The average effects as well as the heterogeneities documented are statistically significant and similar in magnitude to those obtained when using the 10 percentage point cutoff.

The recipients of the first survey were chosen as a representative sample of the Swedish adult population. However, as the response rate varies across subgroups of the population, our final sample is not representative in some respects. In Online Appendix Table A.9, we run our main specification using a weighted OLS regression applying population weights. The results are similar both in terms of magnitude and statistical significance.

Online Appendix B (Tables B.1–B.8) shows that our results are robust to adding control variables, including whether or not a person has

[18] The Online Appendix is available on https://sites.google.com/site/mounirkaradja/.

a college education, to our main specifications.

We define four of the Swedish parties as right-of-center, following previous literature. However, the political landscape is constantly changing, and the recently successful anti-immigration party the Sweden Democrats (*Sverigedemokraterna*) could also be defined as a right-of-center party. We redo the relevant analysis in Online Appendix Table A.10, classifying support for this party as having preferences for right-of-center and find that the results do not change. We also show that the analysis can be done using only the traditionally right-wing and left-wing parties without the results changing. This indicates that how we categorize individuals who answered that they would cast a blank vote, not vote at all, or vote for a non-traditional party politically is not important for our results.

Due to a lower response rate for party preferences in the second round of the survey, there is a smaller number of observations in Column 2 than in the other columns of Table 4.2. To investigate a possible attrition bias, we report results from two variations of the basic models in Online Appendix Table A.11, where we use the same specifications as in Table 4.2 but restrict the sample to the subset with non-missing values for party preferences. The results suggest a similar pattern as in the benchmark, but with somewhat stronger overall effects. To avoid basing our results only on those confident enough to indicate party preference in the second survey, we also report results under the assumption that those who did not respond would have cast blank votes. The results in Table **??** reveal that the effects now become stronger in magnitude and more precise, indicating that attrition may, if anything, attenuate our results towards zero.

As discussed in Section 4.4, the fact that we use support for the Conservative Party as one of our outcome variables can raise concerns of this being an outcome that a non-right person would never consider, thereby mechanically creating the heterogeneities that we document. In Online Appendix Table A.12 we report results from using a continuous version of the party-preferences variable, where all parties have been classified according to an election survey (Oscarsson and Holmberg, 2013), and show that our conclusions hold.[19]

We use the follow-up question that was asked immediately after the information provision in the treatment to test the possibility that individuals with different prior political preferences, different beliefs about effort determining individual economic success, or different beliefs about redistribution being distortive, vary in their understanding or acknowledgment of the information given in the treatment. Online Appendix Table A.13 shows that neither prior political preferences nor beliefs about luck or effort are related to understanding the treatment. Believing that taxation is *not* distortive, however, predicts understanding the treatment better at the 10-percent level. Column 4 includes the three variables simultaneuously and displays the F-statistic from testing that all coefficients are zero. The null hypothesis cannot be rejected (p=0.18). Taking into account the signs of the coefficients we conclude that, across these characteristics, there are no systematic differences in the understanding of the treatment that can explain the heterogeneous treatment effects that we find.

It is possible that the treatment does not only provide an information shock, but also increases the salience of relative income. For those receiving information that they are above the median, the increased salience of relative income may increase the likelihood of reporting more right-leaning views, regardless of whether the treatment provided any new information. If so, our results would not be entirely due to the information given in the treatment. We first note that this would imply that our estimates would simultaneously be biased in two different directions since the treatment group consists of individuals who are below the median as well as individuals above it. Online Appendix Table A.14 shows that there is no significant difference in the responses to treatment

[19] Oscarsson and Holmberg (2013) ask respondents to place parties on a scale from 0 to 10, where higher values indicate being more to the right. We use the following values from 2010: Vänsterpartiet: 1.3, Socialdemokraterna: 3.3, Miljöpartiet: 3.9, Centerpartiet: 6.3, Folkpartiet: 6.6, Kristdemokraterna: 6.8, Sverigedemokraterna: 7.4, Moderata Samlingspartiet: 8.3.

across these two subgroups for those who underestimated their relative income.

If our results were due to framing rather than to information, we should also expect participants exhibiting no, or just a small, bias to respond to treatment despite not receiving any new information. In Online Appendix Part C (Figures C.1–C.2 and Tables C.1–C.6) we conduct a dose-response analysis which suggests that individuals with the smallest (i.e. most negative) bias respond most strongly to treatment, even though the point estimates are not statistically significant. In Online Appendix Part C we also show that our results are qualitatively robust to replacing the outcome variables with their continuous counterparts, although the estimated coefficients decrease somewhat in magnitude.

4.5 Conclusion

We document that almost 70 % of Swedish individuals believe that they are poorer, relative to others, than they actually are and underestimate their rank by more than 10 percentiles. Only 6 percent overestimate their relative position by the same amount. Linking the survey responses to administrative records at the individual level, we find that the more educated, the cognitively able and individuals who have experienced significant upward income mobility hold more accurate beliefs. The misperceptions that we find matter for political preferences: when provided with the correct information, subjects who learn that they are relatively richer than they thought shift their preferences to the right. This effect is entirely driven by individuals who indicated right-of-center political preferences prior to treatment.

An implication of these findings is that political outcomes could be different if individuals held correct beliefs, with the underlying bias-distribution determining the direction of effects. In Sweden, the Conservative party would benefit from correcting misperceptions, while left-wing parties would gain from information provision in countries where individuals overestimate their position. Further, correcting misinformation would increase political polarization in countries prone to underestimation, while countries with overestimation would observe a closer alignment of political views. Future work should investigate the origins and the nature of misperceptions in other countries to shed further light on the effects of income information treatments on policies and electoral results.

While we find that certain characteristics are able to predict individual misperceptions about relative income, additional channels may contribute to the bias that we find. For instance, self-serving behavior may lead individuals to lower their perceived rank in order to justify subsequent selfish behavior (Di Tella et al., 2015). Another possibility is that respondents intentionally misrepresent their relative income to avoid appearing arrogant or divulging sensitive information. Misperceptions may also arise from difficulties in estimating the income distribution. For example, individuals may not sufficiently appreciate the fraction of the distribution that consists of near-zero incomes.

The exact workings of the heterogeneous effect of correct information on political preferences need further investigation. We show that beliefs about redistribution being distortive, and about individual economic success being the result of effort rather than luck, are more common among those with right-of-center preferences. This may imply that an individual who learns that she is richer than she thought and at the same time believes income to be generated by effort, interprets the information as evidence of her having worked harder, relative to others, than she previously thought. Self-serving bias may then lead this person to believe even stronger in the role of effort determining success, which, in turn, decreases her demand for redistribution.

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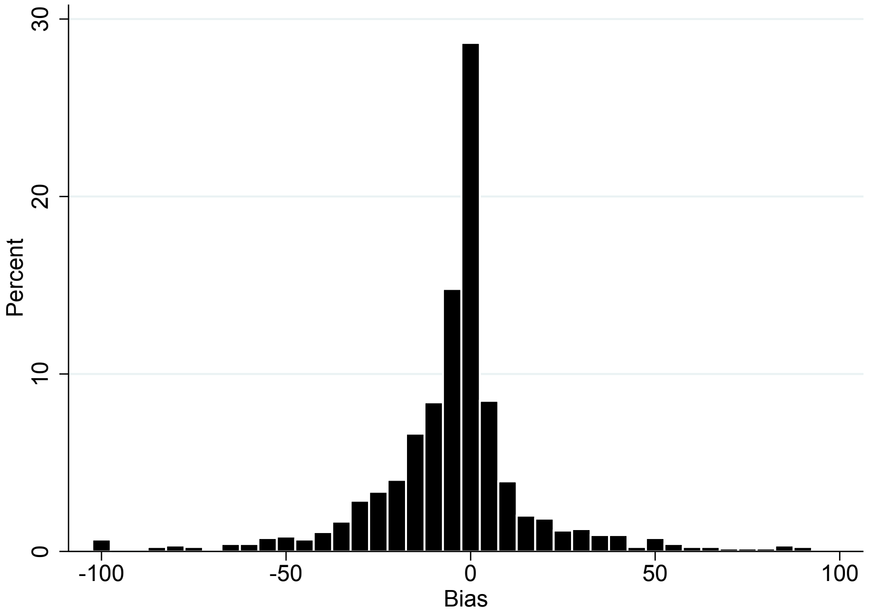
Figures and Tables

Figure 4.1: Deviation between actual and stated income



Notes: The figure displays the distribution of erroneous annual income reports. Bias is defined as the 100 $\times$ (reported income $-$ administrative-data income) /administrative-data income. We drop 20 observations with administrative-data income of zero, leaving the number of observations at 1222.

Figure 4.2: Treatment design

Before you answer the questions we want to inform you about the following:

Imagine that we group all Swedes into 10 groups of equal size such that those in group 1 had the lowest yearly income in 2010 and those in group 10 had the highest yearly income. In the figure below, the numbers 1-10 indicate the groups on the scale. Below the numbers, we have reported the yearly income of the person who was in the middle of that group.

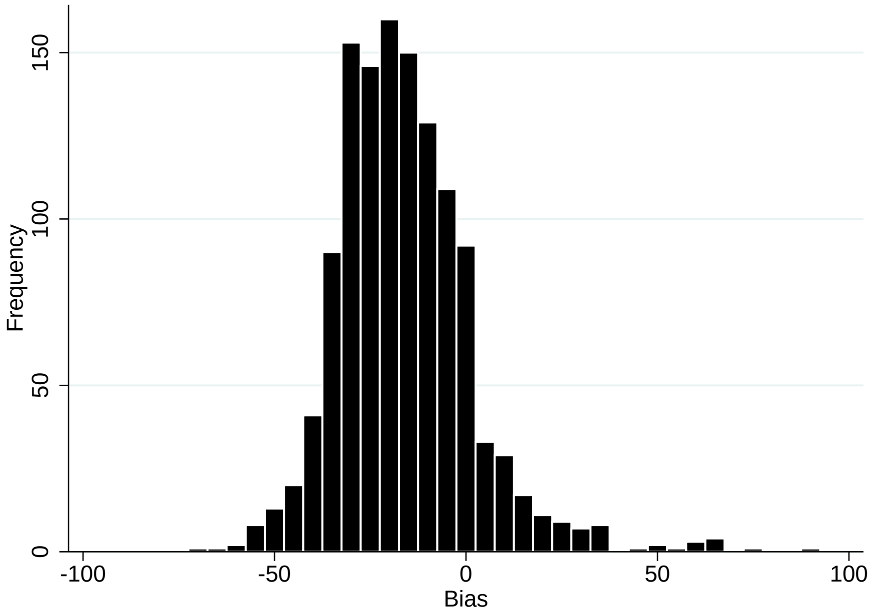
In the previous survey you reported a yearly income for 2010 of X SEK.
In the figure below we have indicated where you income is located on the scale.
.

	1	2	3	4	5	6	7	8	9	10	
Group 1 (lowest yearly income)											Group 10 (highest yearly income)
	0 SEK	67 697 SEK	123 648 SEK	168 498 SEK	207 719 SEK	246 267 SEK	283 546 SEK	326 058 SEK	390 802 SEK	580 388 SEK	

By income we mean total yearly income, defined as wage and capital income before taxes. Pensions before taxes are also included. Student stipends and other transfers such as unemployment transfers from the government are not included in total yearly income. The income statistics come from Statistics Sweden and are based on the whole Swedish population above age 18.

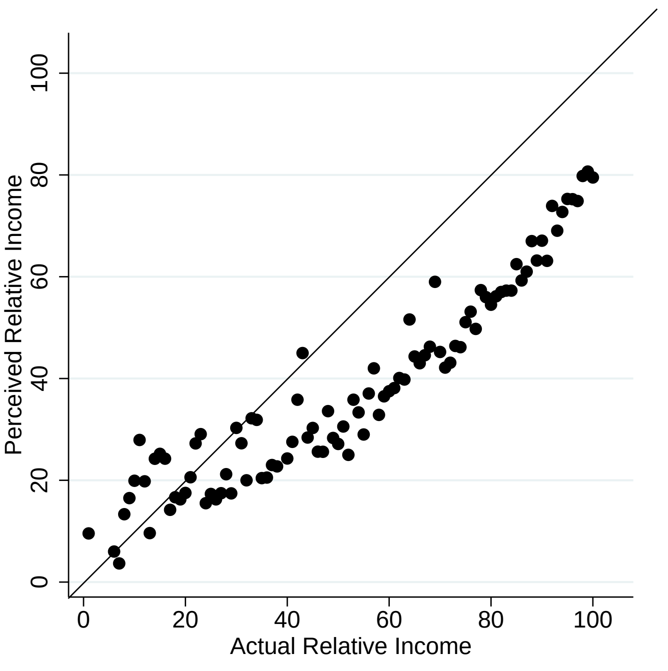
Notes: The figure displays the text presented to the treatment group at the beginning of the second survey. The exact percentile of the respondent, based on her previously reported income, was indicated with an X on the horizontal scale.

Figure 4.3: Distribution of bias in the sample



Notes: The figure displays the distribution of bias – defined as perceived minus actual percentile in the income distribution – among the 1242 respondents of the first round. Higher values indicate overestimation of relative income. The bar width is 5 percentiles.

Figure 4.4: Actual and perceived relative income over the income distribution



Notes: The figure displays the relation between perceived and actual relative income among the respondents of the first round. We construct 100 equally-sized bins of actual relative income and display mean perceived relative income in each bin. The solid 45-degree line illustrates the no-bias case. The number of observations is 1242.

Table 4.1: Determinants of Bias

Dependent variable:	Bias		Absolute Value of Bias	
	(1)	(2)	(3)	(4)
College	2.569***	2.847***	-2.377***	-2.746***
	(0.920)	(0.976)	(0.784)	(0.842)
IQ	3.716*		-4.006**	
	(2.078)		(1.880)	
Informed	1.862**	1.808**	-1.160	-1.587**
	(0.892)	(0.903)	(0.757)	(0.777)
Urban	-0.953	-1.292	0.592	0.539
	(0.905)	(0.916)	(0.790)	(0.812)
Right	0.161	-0.526	0.780	1.067
	(0.942)	(0.924)	(0.795)	(0.800)
Age	-0.106***	-0.033	0.095***	0.057
	(0.029)	(0.046)	(0.023)	(0.037)
Male	1.240	1.941**	-0.852	-1.695**
	(0.892)	(0.945)	(0.771)	(0.814)
Married	-2.004**	-0.256	1.648**	0.102
	(0.858)	(0.967)	(0.718)	(0.817)
Log Total Taxable Income	0.530	0.247	0.490	0.528
	(0.399)	(0.525)	(0.326)	(0.471)
Log Net Wealth	-0.059	0.022	0.040	-0.007
	(0.040)	(0.041)	(0.034)	(0.036)
Relative Income Growth	2.462**	-0.958	-2.318**	0.189
	(1.042)	(1.319)	(0.981)	(1.205)
Subjective Rel. Inc. Growth	2.896***	1.389	-2.213**	-1.689*
	(1.000)	(1.031)	(0.863)	(0.906)
Subjective Future Rel. Inc. Growth	4.273***	2.557**	-2.011***	0.224
	(0.856)	(1.108)	(0.726)	(0.978)
Income Mobility Belief	0.603***	0.494***	-0.311**	-0.199
	(0.155)	(0.167)	(0.131)	(0.142)
Max. Observations	1242	1099	1242	1099

See following page for table notes.

Notes: OLS regressions. Robust standard errors in parentheses. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$. Columns 1 and 3 display coefficients from separate regressions for each covariate, while Columns 2 and 4 includes all variables in the same model. All regressions include fixed effects for each percentile of the actual relative income distribution. The dependent variable in Columns 1 and 2 is bias, defined as perceived minus actual percentile in the income distribution. Higher values indicate overestimation of relative income. The dependent variable in Columns 3 and 4 is the absolute value of bias. *College* is a dummy for having more than two years of post-secondary schooling, *IQ* is a dummy for above-median cognitive ability, as determined during military enlistment, and is only available for men, *Informed* is a dummy for above-median usage of news, and *Urban* is a dummy for living in one of Sweden's four metropolitan areas (Stockholm, Gothenburg, Malmo or Uppsala). *Right* is a dummy for preferring one of the four right-of-center parties in Sweden in the first survey. *Log Total Taxable Income* and *Log Net Wealth* are log taxable income in 2010 and log net wealth in 2006, respectively, taken from the Swedish Tax Registries. Net wealth is logaritmized using the inverse sine function to incorporate negative values. *Relative Income Growth* is a dummy for being in the top 25 percentiles of growth in actual relative income between 2000 and 2010, calculated using register data. *Subjective Rel. Inc. Growth* is a dummy for answering that one's relative income is higher compared to 10 years earlier. *Subj. Future Rel. Inc. Growth* is a dummy for expecting one's future relative income to be higher in 10 years as compared to when the survey was taken. *Income Mobility Beliefs* measures disagreement with a statement about limited income mobility in society.

Table 4.2: Average effects

	(1) Outcome Index	(2) Against-Redist	(3) Cons. Party	(4) Decrease Tax
Treated×Neg. Bias	0.134**	0.081**	0.081**	0.040
	(0.058)	(0.038)	(0.037)	(0.038)
No bias	-0.010	-0.004	-0.018	0.024
	(0.073)	(0.049)	(0.050)	(0.051)
Treated×No Bias	-0.067	-0.052	-0.013	-0.023
	(0.085)	(0.059)	(0.056)	(0.062)
Pos. bias	-0.032	-0.112	0.117	0.013
	(0.162)	(0.092)	(0.114)	(0.104)
Treated×Pos. Bias	0.112	0.179	-0.068	-0.003
	(0.202)	(0.129)	(0.139)	(0.136)
Constant	0.008	0.362***	0.251***	0.404***
	(0.040)	(0.026)	(0.027)	(0.027)
Obs	1001	991	872	985

Notes: OLS regressions. Robust standard errors in parentheses. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$. The table shows estimated treatment effects by bias type. *Neg. Bias* is an indicator for underestimating relative income by more than 10 percentage points. *Pos. Bias* indicates overestimation by more than 10 percentage points. *No Bias* indicates misestimation of relative income of 10 percentage points or less. *Outcome Index* is a composite measure of the outcome variables in Columns 2-4, and a higher value indicates more right-leaning and more anti-redistribution preferences. *Against-Redist* is a binary indicator for demanding low levels of redistribution. *Cons. party* is a binary indicator for supporting the Conservative Party. *Decrease tax* is a binary indicator for wanting to decrease income taxes. See more detailed definitions in Section 5.3.

Table 4.3: Heterogeneous effects by prior party preferences

	(1) Outcome Index	(2) Against-Redist	(3) Cons. Party	(4) Decrease Tax	(5) Effort
Treated	0.020	0.029	0.012	0.026	-0.080
	(0.055)	(0.045)	(0.024)	(0.047)	(0.187)
Treated×Right	0.274***	0.117	0.147**	0.046	0.588**
	(0.103)	(0.073)	(0.066)	(0.075)	(0.268)
Right	0.710***	0.270***	0.517***	0.266***	0.585***
	(0.075)	(0.052)	(0.051)	(0.053)	(0.198)
Constant	-0.286***	0.251***	0.045***	0.291***	6.095***
	(0.039)	(0.031)	(0.017)	(0.033)	(0.131)
Obs	678	672	589	671	674

Notes: OLS regressions. Robust standard errors in parentheses. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$. The table shows estimated heterogeneous treatment effects with respect to prior party preferences. The sample consists of those who underestimated their relative income by more than 10 percentage points. *Right* is a binary indicator for supporting one of the four right-of-center political parties in Sweden in the first survey, i.e. before treatment. *Outcome Index* is a composite measure of the outcome variables in Columns 2-4, and a higher value indicates more right-leaning and more anti-redistribution preferences. *Against-Redist* is a binary indicator for demanding low levels of redistribution. *Cons. party* is a binary indicator for supporting the Conservative Party. *Decrease tax* is a binary indicator for wanting to decrease income taxes. *Effort* is a variable indicating the degree to which one believes that effort determines economic success in life. See more detailed definitions in Section 5.3.

Table 4.4: Heterogeneous effects by prior economic beliefs

	Dependent variable: Outcome Index					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.138**	0.223**	0.221***	0.137***	0.280***	0.052
	(0.055)	(0.090)	(0.070)	(0.048)	(0.091)	(0.058)
Treated×Redist-Distort	-0.159**			-0.131**	-0.034	-0.130*
	(0.073)			(0.062)	(0.120)	(0.073)
Redist-Distort	-0.194***			-0.065	-0.105	-0.067
	(0.053)			(0.047)	(0.086)	(0.054)
Treated×No Dist.		-0.160				
		(0.114)				
No Dist.		-0.317***				
		(0.079)				
Treated×Luck			-0.268**			
			(0.119)			
Luck			-0.121			
			(0.083)			
Right				0.786***		
				(0.054)		
Constant	0.008	0.182***	0.046	-0.318***	0.401***	-0.276***
	(0.039)	(0.064)	(0.048)	(0.037)	(0.066)	(0.040)
Obs	687	687	687	678	281	397

Notes: OLS regressions. Robust standard errors in parentheses. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$. The table shows estimated heterogeneous treatment effects on the outcome index by prior beliefs about how the economy works. The sample consists of those who underestimated their relative income by more than 10 percentage points. Column 5 estimates the same model as Column 1 but restricts the sample to those who expressed right-of-center preferences in survey 1, i.e. before treatment, while Column 6 only uses the sample of those who did not express such preferences. *Outcome Index* is a composite measure of the variables Against-Redist, Cons. Party and Decrease Tax, and a higher value indicates more right-leaning and more anti-redistribution preferences. *Redist-Distort* is a composite measure of the variables *No Dist.* and *Luck*, and a higher value indicates beliefs about redistribution not being distorting. *No. Dist* is a binary indicator for believing that income taxes do not distort labor supply. *Luck* is a binary indicator for believing that luck determines economic success in life. See more detailed definitions in Section 5.3.

Chapter 5

Wealth, home ownership and mobility

5.1 Introduction

Labor mobility is known to be an important source of economic gains, both at the individual and national level. Migration across countries can boost workers' wages by several hundred percent (Clemens et al., 2008). Within a given country, labor mobility may also act as a means to cope with economic shocks. Workers in depressed areas often migrate to more prosperous regions, improving wages and reducing spatial income inequality (Blanchard and Katz, 1992; Bound and Holzer, 2000). However, regional income convergence has been found to be hampered by regulations on land use, which reduce housing supply and therefore inhibit labor migration as prices increase (Ganong and Shoag, 2013). Similarly, urban areas often employ price controls on rents, which has long raised worries about reduced allocative efficiency (Olsen, 1972).

This paper studies how individual mobility is affected when tenants are allowed to buy their rent controlled apartments at subsidized prices. Between 1998 and 2012, more than 100,000 apartments in Stockholm, Sweden, were converted from rentals to tenant-occupied condominiums. The treatment introduced both a shock to wealth as apartments were sold at a discount, but also turns tenants into home owners who can more easily sell their property. We examine both residential and workplace mobility using detailed administrative data on location of residence and work.

Assignment into treatment was not random, but voluntarily decided upon by a property's residents. If a qualified majority of residents voted in favor of converting to condominiums, the whole property was sold to a cooperative formed by the tenants. Participation is hence partly exogenous to individual characteristics, introducing a level of randomness to the treatment. However, properties that are able to achieve majority support for the conversion may be different than those that did not, and it is hence important to take potential selection effects into account in order to estimate the causal effect of participation. We employ a difference in differences strategy paired with administrative panel data in order to cancel out the fixed characteristics that may otherwise drive differences in outcomes between treated and untreated individuals. Looking at extended pre-treatment data, we verify that both groups display similar trends prior to apartments being converted.

We begin by establishing the treatment had the intended direct effects. Administrative wealth data show that treated individuals are indeed more likely to be condominium owners after a property conversion has taken place. Moreover, the treatment group displays an average increase in condominium assets at least 700,000 SEK. The increase in net wealth is somewhat smaller (500,000 SEK) as many buyers take out loans to finance the purchase. To corroborate the effects on individual wealth, we carry out another test using a separate data set on condominium sales. We estimate that treated individuals who later sold their converted apartments made gross profits that were on average 710,000 SEK higher compared to sellers who had bought apartments in the same parish but at non-discounted prices. Based on these two analyses, we conclude that the treatment induces a substantial wealth shock of 2 to 3 times the median yearly income in 2005.

Our main outcome variable is whether individuals have moved to a new residence in a given year. We find that treated tenants are approximately 3 percentage points more likely to move in the years following the conversion to a condominium. This is a substantial increase in mobility, constituting a 30 percent increase over the average probability of moving among the control group. The effect is also persistent. Analyzing up to fourteen years of data post-treatment, we find that after the large initial jump in residential mobility, treated individuals remain approximately 2 percentage points more mobile until the end of our sample. As a result, the policy of transitioning residents away from the rent control housing system that dominated the market for apartments has increased mobility among directly affected residents.

Two main mechanisms can explain why residential mobility increases after treatment. First, it may be due to a *liquidity effect*. Apartments that are converted to condominiums are easily bought and sold at market prices. Sellers also reap the full value of their housing when they leave it. By contrast, rental contracts either have to be given up without compensation, or substituted for another rental contract, provided that a match can be found. Second, because treatment includes a sizable discount, there may be a *wealth effect* which allows individuals to change to different and potentially higher quality housing.

We distinguish between these two mechanisms by evaluating if there are differential effects on mobility within the treatment group, depending on the magnitude of the received wealth shock. Interestingly, using two different measures of wealth shocks, we find more wealth predicts *lower* residential mobility. However, the negative effect is relatively modest. Moving from the 25th to the 75 percentile of the wealth shock distribution decreases residential mobility by 0.3 to 0.7 percentage points. By contrast, the strong positive effect on mobility is chiefly explained by the liquidity effect, which ranges between 5 and 8 percentage points within the treatment group.

As a robustness check, we exclude those who recently moved in to buildings that were about to be converted, to avoid potential issues related to self-selection in to treatment. We also construct an alternative treatment definition that jointly controls for self-selection *out of* and *in to* treatment. The results are robust to these modifications, suggesting that we are indeed capturing the causal effect of the treatment.

Contrary to the common view that residential mobility improves mobility between jobs, we find zero or small negative effects on the probability of moving to a new workplace after treatment. The largest point estimate we find implies only a 3.5 percent decline from the control group mean, while the smallest estimate suggests a 0.5 percent reduction.

Our results clash with the received wisdom that home ownership is associated with low residential mobility. Lower mobility is generally attributed to transaction costs associated with home ownership (Dietz and Haurin, 2003). Haurin and Gill (2002) show that homeowners are less likely to move than tenants, after controlling for subjective future mobility needs. Head and Lloyd-Ellis (2012) theoretically show that home ownership induces lower mobility and employment. An important factor accounting for our different findings compared to the literature is likely that we analyze a setting with binding rent controlled housing. Given that many localities worldwide employ various forms of rent-control, these results may be applicable to many external settings.[1]

This paper is related to a long literature about the economics of rent control.[2] Olsen (1972) and Glaeser and Luttmer (2003) study the efficiency of resource utilization under rent control, finding evidence of misallocation of housing in New York City. Several studies have found that rent controls in New York city is related to longer tenancy durations (Gyourko and Linneman, 1989; Ault et al., 1994; Nagy, 1995), while Munch and Svarer (2002) present similar findings in Denmark. In a cross-section of OECD countries, Sánchez and Andrews (2011) find a correlation between the extent of rent control and low residential mobility. We differ from these studies by using quasi-experimental data where

[1]For example, about 140 jurisdiction in the United States regulate rents (Jenkins, 2009). Turner and Malpezzi (2003) provide an overview of rent control levels across OECD countries.

[2]See Turner and Malpezzi (2003) for a comprehensive review.

we observe individuals both with and without rent control. The study by Wang (2011) is the closest to ours. Wang analyses a Chinese reform in 1994 that privatized state-owned, rent-controlled housing, and shows that rent decontrol leads households to reoptimize their housing consumption. An important difference is that Wang does not consider the possibility of the wealth effect in driving mobility.

The pre-treatment rental contracts that we study afford tenants use-based property rights that require households to be officially registered in the property and to use it actively in order to keep it, an institution common in the agricultural sector of developing countries. Using a national land certification program in Mexico, De Janvry et al. (2015) show that households whose land rights no longer required active use have a higher likelihood of having a migrant in the household.

Housing is an important source of household wealth. As housing markets have become more congested, housing wealth has also become a major driver of the increased wealth inequality during recent decades (Piketty and Zucman, 2014; Rognlie, 2015). Housing wealth may also be different from other types of wealth, as it is less liquid (Case et al., 2005). As a result, research studying the economic effects of housing wealth may become increasingly relevant for understanding modern economies and designing policies.

5.2 Policy background

The Swedish system for rental apartments is mostly organized via a municipally administered and non-discriminatory waiting list. Individuals accumulate queuing days and are able to declare interest in available apartments from both public and private landlords. Rents are controlled by a system that sets prices according to the so called *utility value* of an apartment, which should reflect the overall value of the characteristics of the apartment to a generic tenant. Traditionally, the utility value has not factored in the quality of local amenities or the popularity of an area (Boverket, 2008). As a result, rent subsidies are effectively larger in central and more popular areas, where demand typically outstrips supply by a wide margin (Boverket, 2013).[3]

Motivation for converting apartments to condominiums

As with many rent control systems globally, the Swedish system has often been critiqued by economists for introducing distortions in housing decisions and inhibiting worker mobility. In part as a response to these worries, from 1982 private landlords were allowed to convert rental apartment buildings to condominiums, provided that the current tenants were given first priority for buying them. Starting in 1992, the same provision began applying to public housing companies as well. However, it was only in the late 1990s that the number of rental-to-condominium conversions began to approach very high levels. Between 1998 and 2014, more than 200,000 apartments became tenant-occupied condominiums. About one fifth of these were sold by public service housing companies. By contrast, only around 10,000 apartments were converted in the 1990-1996 period.[4] While our building-level data begin in 1998, we are still able to observe the period during which the majority of conversions occurred.

Converting a rental apartment property to condominiums can often be mutually beneficial for sellers and buyers (Lind and Lundström, 2007). Without the option of converting the property to condominiums, a property's market value is governed by the the present value of expected future returns. Since returns are driven by (controlled) rents, the property's market value is lower than the potential market value without rent controls. However, the option of converting the property raises its market value, because tenants who convert their rentals to condominiums are allowed resell them at market prices. Current tenants are hence willing to offer a higher price for the building compared to a would-be landlord who would continue renting out apartments at

[3]The average waiting time for an apartment in central Stockholm was 13 years in 2014 (Bostadsförmedlingen).

[4]Boverkets rapport ”Ombildning av hyresrätt till bostadsrätt 1990-2001” (2002)

subsidized prices. The policy may therefore cause a property's market value to exceed the present value of future (controlled) rents, explaining the popularity of conversions among landlords. Tenants on their part are willing to purchase the apartment building provided that the price is sufficiently below market price so as to cover the loss from no longer benefiting from the subsidized rents. If a middle ground can be found, both parties should be willing to make the transaction (Andersson and Söderberg, 2002). In practice, the final sale price of buildings is generally thought to lie between 60 and 70 percent of the market value.[5]

This solution was found to be attractive by many landlords and tenants. Between 1998 and 2014, 6,759 properties were sold to tenants, converting around 200,000 rental apartments to condominiums. The probability of converting a property is related to the gap between subsidized and market-based rents, as this gap creates the potential for a mutually beneficial transaction. Correspondingly, conversions were more common in the urban regions of Stockholm and Gothenburg, where price controls are most binding.[6] Almost half of all conversions occurred in Stockholm municipality alone. Stockholm stands out relative to its existing housing stock as well, as 25 percent of the 2013 stock of apartments had been converted from rentals to condominiums. The national average was 8 percent.[7]

The conversion procedure

Individual rental apartments cannot be converted, as properties may only be sold as a whole. In order for tenants to buy the property that they rent, the landlord must offer to sell and a qualified majority of 2/3 of tenants must vote in favor of a conversion. If sufficient votes are in

[5] See for example Berg (2007) and Andersson (2011).

[6] To illustrate, rents in Stockholm county, the most populous region of Sweden, are only 9.5 percent higher than the national average and only 22 percent higher than the cheapest county (Boverket, 2014). By contrast, market-based condominiums prices are 54 percent higher in Stockholm county compared to the national average (Mäklarstatistik, 2014).

[7] Own calculactions based on aggregate data from Statistics Sweden.

favor of the purchase, the landlord and tenants may negotiate about the price. If a price is agreed upon, the building is purchased by a housing cooperative formed by the tenants. The purchase is financed in part by loans taken up by the cooperative itself, and in part by tenants who purchase a share of the coop's equity. Tenants then get the right to occupy their specific apartment.

Apartments are not converted into condominiums in a strict sense, as they are formally owned by the housing cooperative. For simplicity, however, we refer to such apartments as condominiums, given that they retain important features of condominiums. This most notably includes the right to make extensive modifications and renovations to the inside the apartment, as well as the right to capture the full market value of the apartment when it is sold.

Tenants who do not want to purchase their apartment (even though 2/3 vote in favor of purchase) are allowed to continue renting under similar conditions as before, but from the new owner (the housing cooperative). Hence, voting in favor of the buildings' conversion does not not necessarily entail converting the building oneself.

The monetary value to tenants

Common estimates of the discount given to tenants when converting rental apartments to condominiums are around 30 to 40 percent of the market value. Here, we briefly discuss some potential mechanisms explaining why the discount occurs.

The main reason for providing a discount is that treated tenants forgo the right to a rent subsidy when they buy their apartment. This loss is especially high for tenants living in popular neighborhoods, where the implicit subsidy is greatest.[8] At a minimum, a forward-looking tenant would require a discount amounting to the present value of forgone future subsidies. Discounts may be larger than this in practice. There is

[8]As condominium owners, tenants instead pay utility fees to the coop as well as potential mortgage payments to the bank.

a continuum of acceptable prices between the minimum that the landlord is willing to sell for, and the maximum that tenants are willing to buy for. Except in the case of a corner solution where landlords set the highest possible price, tenants receive a discount over and above the minimum they require to break even.

One important factor driving down the price in our setting is the fact that landlords cannot engage in price discrimination with individual tenants. Instead, landlords offer a single price for the whole property. Therefore the building's price must appeal to a qualified majority of tenants in order for the transaction to go through. The willingness to pay of the marginal tenant then becomes important aspect in negotiations. Along with reimbursements for transaction costs that tenants must bear, this may be an important reason why discounts appear high.[9]

5.3 Data and treatment assignment

We combine two main, administrative data sets to perform our analysis, at the individual and property level. We start by describing the property data, before discussing the treatment definition and the individual-level data.

Property data

We use property-level data from yearly administrative tax registers covering all individual houses and apartment complexes in Sweden between 1998 and 2012. The series begins in 1998 as there was a restructuring of the data in that year. Relatively few rental properties were converted to condominiums prior to 1998, however, meaning that we observe the

[9]There are also benefits of the building conversion that would allow the landlord to offer a higher selling price. This includes the fact that conversion gives tenants a liquidity shock as condominiums can be easily sold and used as security for loans, whereas the value implicit in the subsidies of the rental contract is highly illiquid. Condominium owners are also more free to modify the interior of their apartments, and are the sole claimants on the value added generated by renovations when the apartment is sold.

most important years. Figure 5.1 displays the growth of the apartment stock by rentals and condominiums.

The building-level data set categorizes a property's owner into one of several groups. The group of interest for us is whether the owner is a housing cooperative. Using this information, we identify treated properties as those rental apartment complexes that convert to being owned by a condominium housing cooperative. The year of treatment is defined as the year during which a property switched to being owned by a housing cooperative. Using this definition, we identify 3500 properties in Stockholm county that were sold to tenants during the 1998-2012 period. The sale of these properties lead to a corresponding creation of 130,000 condominium apartments.

Treatment assignment

At the individual level, we define treatment status in the following way. First, the individual must be a registered resident of the property on December 31 the year prior to the sale year (e.g. December 31, 2000 if the building was sold during 2001). We match this condition using identifiers on all individuals' building of residence as of December 31 each year. It was also a formal requirement for treatment participants to register their current address to the building in question. Second, only heads of households are defined as treated, excluding children from treatment.[10] We match this condition using family relation identifiers. Hence, we assume that household heads are treated to equal extents, regardless of who was formally the owner of the lease.[11]

We favor this definition because of its simplicity and the fact that it errs on the side of caution, as it potentially includes more individuals in

[10]This definition also excludes parents of household heads in the relatively rare cases where three generations live together.

[11]Our data identifies families that either consist of parents and their children, as well as married individuals or couples that have children in common. Co-habiting couples without common children are hence identified as singles. Our empirical strategy does not rely on distinguishing between these relations, however, and simply use family identifiers to focus on the likely beneficiaries of treatment.

the treatment group than do in fact benefit from it. Our results should then represent a lower bound of the treatment effect. In particular, two types of residents may erroneously be defined as treated. First, spouses who live in a treated building but who do not own the lease and do not directly benefit from the treatment. Second, individuals who sub-rent their apartment and are therefore registered on the address, but who do not benefit from treatment. It is unfortunately hard to quantify the size of these two groups relative to those for whom the treatment definition is accurate.

Some individuals continue to rent their apartment even after treatment. This group should hence not experience any treatment effects. As the decision to forgo converting one's apartment is endogenous, we do not control for this behavior, nor do we attempt to remove them from the treatment definition. For the reason listed above, our estimand is therefore the intention to treat effect (ITT) of condominium conversion.

Individual data

To verify that the treatment had the hypothesized first order effects, we employ administrative wealth data collected by the Swedish Tax Agency. The data set contains information on debts, real and financial assets, as a well as a residual wealth category. Using these variables, we construct a measure of net wealth defined as the sum of real, financial and other assets minus total debt. The wealth data are available for parts of our sample period, namely 1999–2007, due to a law change that lead to wealth data no longer being collected after 2007.

Within the category of real assets, the data distinguish condominium-based wealth from other assets. Using this, we also code a binary variable indicating the presence of any condominium assets as a measure of condominium ownership. While financial assets and debts are easily valued at prevailing market rates, the market value of real assets has to be estimated in order to calculate net wealth. For condominiums, the Swedish Tax Agency estimated market value using a combination of the registered tax value and a coefficient which is estimated every year based on sales of similar apartments in the local area. For this reason, there is likely to be a relatively high level of measurement error in estimated market values. Moreover, to the extent that there is a higher turnover on smaller - and hence cheaper - apratments, estimated values will be underestimated, as the Tax Agency does not take quality or apartment sizes into account.

Additional individual level data is taken from the *Integrated database for labour market research* (LISA). LISA is a complete data set of all individuals aged 16 and above.[12] The data base contains both demographic and labor related outcomes. We use plant IDs to track individual workplace mobility each year. In addition, the data set includes age, birth year, family identifiers and educational attainment.

We also match individuals with information about their place of residence. Available at a yearly frequency, individuals are matched to the exact building corresponding to the their registered address. Addresses are self-reported. While it is a legal requirement to register one's primary address of residence to the tax authority, enforcement is low and hence there is likely to be measurement error in this variable. However, individuals who wanted to participate in a building conversion were required to be registered in the building in question. As a result, we may expect treated individuals to exhibit a higher degree of registered mobility just prior to treatment, as official addresses are updated in order to take part in the treatment.

Our sample consists of individuals who lived in rental apartments in Stockholm county in 1998. A random sample of 20 percent of the control group is used to generate a smaller final sample size, which has 119602 and 108723 individuals in the treatment and control groups, respectively.

[12]From 2010, individuals aged 15 and up are included.

5.4 Empirical strategy

Assignment into treatment is not random, as participation in the condominium conversion is decided collectively by a building's residents. If a qualified majority of residents vote in favor of a conversion, the whole building is sold to the housing cooperative and everyone is counted treated.[13] Given that unanimity is not required, some treated individuals will have voted against it, while some untreated individuals will have been in favor of a conversion. The lack of an individual selection mechanism introduces an element of randomness to individual treatment participation, as it will to a large extent be dependent on the choices of neighbors, who are not directly chosen by the individual. Nevertheless, we employ a difference-in-differences strategy which controls for various unobserved differences between treatment and control groups. The following is the baseline specification used:

$$\begin{aligned} y_{ipt} = &\beta_1 Treated_i + \beta_2 Treated_i \times Post_{it} \\ &+ \theta_{pt} Parish_i^{1998} + \mathbf{X}'_{it}\beta_X + \varepsilon_{it}, \end{aligned} \tag{5.1}$$

where y_{it} is the outcome of interest for individual i in year t. $Treated_i$ indicates whether the individual was ever part of a condominium conversion. $Post_{it}$ is a dummy taking value one in all years starting from the conversion year. The coefficient of interest is β_2, which measures the change in outcomes among the treated compared to the control group. Including $Treated_i$ in the model controls for all unobserved and time-invariant characteristics that may differ across the two groups of individuals. θ_{pt} is a set of yearly fixed effects which control for the evolution over time of individuals who lived in the same parish at the beginning of our sample period, 1998. $\mathbf{X}_{it}$ is a vector of individual control variables including birth year and age fixed effects, as well as dummies for being female and for having a college education.

[13] Nevertheless, residents have the option to remain renters even after a conversion. The newly formed coop then takes over as landlord.

In order to interpret β_2 as the causal effect of the treatment, one must fulfill the standard difference-in-differences assumption that treatment and control groups follow parallel trends in the outcome variable prior to treatment. More specifically, the assumption is that the treatment group would have had a similar evolution of their probability of moving had they not participated in the building conversion. In the results section, we study the validity of this assumption by examining the trends in outcomes before treatment across the two groups.

An advantage of our setting is that treatments occur every year throughout the 1998–2012 period. Hence, the identification strategy is robust to the possibility that treated individuals were for example more likely to benefit from a particular macroeconomic shock occurring in a particular year.

To examine the timing of the treatment effect, as well as to compare trends among treatment and control groups, we also estimate a flexible model with yearly treatment coefficients. The following model display the effect of treatment relative to the treatment year:

$$\begin{aligned} y_{ipt} = &\beta_1 Treated_i + \sum_{s=-10}^{s=10} \beta_{2,s} Treated_i \times 1[TreatmentYear + s]_{it} \\ &+ \theta_{pt} Parish_i^{1998} + \mathbf{X}_i' \beta_X + \varepsilon_{it}, \end{aligned} \tag{5.2}$$

where $1[TreatmentYear + s]_{it}$ is an indicator for whether the current year t is exactly s years before (or after) individual i is treated. For instance, if an individual is treated in 2005, the dummy $1[TreatmentYear + 5]_{i,2010}$ takes value one. These indicators are all zero for the control group. The coefficient $\beta_{2,s}$ thus describes the outcome difference between the treatment and control groups s years before (or after) the treatment. We estimate up to 10 lags and leads, where the first and last coefficients refer to 10 years *or more* before/after treatment.

5.5 Treatment effect on home ownership and wealth

Home ownership

Before turning to the main results, this section validates that the treatment has the expected effects on home ownership. The latter is defined as having any registered condominium-related wealth registered in administrative data. Administrative wealth data are only available between 1999 and 2007, and results should hence only be seen as indicative of the effect in later years.

Panel A of Figure 5.2 displays the difference in condominium ownership between treatment and control groups up to 10 years before and after treatment, based on equation (5.2). The estimates come from a regression that controls for basic individual controls, as well as home parish in the start of our sample. The left-hand side of the panel indicates that both groups had very similar rates of ownership before treatment. In the treatment year, there is a clear jump, which reaches a top of about 60 percent higher ownership rates in the year after treatment. The reason for the lag in effect is due to reporting by housing cooperatives not always occurring in the same calendar year as the purchase of a property. The difference in condominium ownership decreases after treatment. This is potentially due to the growing popularity of condominiums in general over this period, as seen in Figure 5.1.

Table 5.1, Panel A provides regression output for the average effect of treatment on condominium ownership. Column 1 displays the estimate from the simplest specification, which controls for treatment status and year fixed effects. In order to take into account the lagged effect of the treatment, seen in Figure 5.2, all regressions omit the treatment year itself and the variable of interest $Treat \times Post$ takes value one starting in the year after treatment. Using this model we find that treated individuals are on average 55.5 percent more likely to own a condominium than they otherwise would have been if it were not for their building being converted into condominiums. Column 2 adds fixed effects for birth year, age and home parish in 1998, as well as dummies for gender and college education. These control variables induce a very small change on the estimated coefficient. In Column 3, we further add yearly non-parametric trends related to the home parish in 1998, the start of our sample period. The estimate is stable at 55.1 percent. In the final two columns, we control for separate cubic time trends by treatment status and additionally control for individual fixed effects, which yields similar results.

The treatment induces a statistically significant increase in condominium ownership of over 50 percent compared to the control group. There may be several reasons for why the effect is not closer to 100 percent. Tenants in converted buildings are allowed to continue renting (from the housing cooperative rather than from the previous landlord) even after the conversion, provided a sufficiently large number of tenants actually bought their apartments. It is also possible that only one out of two adults in the family bought the apartment, whereas our definition counts all adults as treated. This can be due to only one of the adults being registered on the lease or that the household for other reasons decided that only one person should buy the apartment. Lastly, there is also a general upward trend in apartment ownership throughout the period of study. This can be seen in Panel A of Figure 5.2, which shows that the difference between t-1 and t+1 is closer to 70 percent.[14]

Wealth

Next, we estimate the wealth effect of converting one's rental apartment to a condominium. We use two approaches. The first uses the same difference-in-differences specifications as the previous section to compare the evolution of wealth among treated and untreated individuals over time. This will yield the effect on the market value of assets as estimated

[14] As an illustration, the average change in ownership status among treated individuals only. The effect is a 67 percentage point change between the year before treatment and the year after (0.04% in t-1 to 0.71% in t+1).

by the Swedish Tax Agency. The advantage of this approach is that we have information on individuals' debt as well as assets. As treated individuals are likely to increase their debt in order to purchase their apartment, it will be important to correct for such changes.

Panels B and C of Figure 5.2 display the evolution of condominium wealth and net wealth in the years before and after treatment. Similar to the case with home ownership, there is a stable trend pre-treatment, after which both measures exhibit sharp increases. Interestingly, both variables display persistent or even diverging differences in wealth, whereas the difference in condominium ownership tends to decline at a relatively rapid rate post treatment (Panel A). This indicates that while the supply of condominiums increased in general during this period, the differences in wealth created by the conversion of rental apartments resulted in persistent differences between the two groups.

Panels B and C of Table 5.1 show the average effect of treatment on wealth in regression form. Reported condominium values increase by over 700,000 SEK in all specifications in Panel B. Treatment leads to slightly lower increases in net wealth, as households may take out loans or liquidate other assets in order to finance the purchase of their apartment. The effect on net wealth ranges from 410,000 to 550,000 SEK. This effect is quite large and roughly corresponds to twice the median yearly income in 2005.

The data used in Table 5.1 only span the 1999–2007 period. They also use estimates of market values for condominiums carried out by the Swedish Tax Agency. As these estimates are likely prone to measurement error, attenuation bias may yield coefficients closer to zero. Moreover, the value of condominiums tends to be underestimated. We therefore also use a second approach to estimate the treatment effect on wealth. Namely, we study the gross profit made when selling apartments, which by definition is done at market prices. Using additional administrative data on the universe of apartment sales between 2000 and 2014 including both sale and purchase prices, we can compare the profits made by owners of treated and untreated apartments.

Column 1 of Table 5.2 shows that apartments that were bought through a condominium conversion generate a gross profit that is 773,600 SEK higher than comparable apartments that were bought *and* sold in the same years as the treated apartments. Introducing fixed effects for unique purchase and sale year pairs means that the estimated effect nets out all price increases at the market level due to the overall positive trend in Stockholm housing prices. In Column 2 we also control for parish fixed effects to take into account the possibility that more centrally located or more popular areas saw more conversions. The estimate is lowered slightly, to 701,200 SEK. Column 3 also controls for construction year fixed effects as a proxy for the quality of the apartment.[15] Controlling for the construction year increases the estimate slightly to 710,000 SEK.

A potential problem with the estimates in Table 5.2 is that not all treated apartments are sold, and that those which are sold may be specifically those in the higher end of the distribution. Nevertheless, the similarity of estimates in Columns 1–3 indicate that treated apartments that were sold did not come disproportionally from more expensive areas or from higher quality buildings.

Another drawback of this method is however that we cannot control for the investments that individuals make between the purchase and sale date. If converted apartment came from a housing stock that was generally of lower quality, it may be the case that treated sellers had to invest more money for renovations. As a result, Table 5.2 only measures gross profit, rather than net profit. However, the cost of renovating an apartment typically range from 100,000–300,000 SEK, which is not enough to offset the average gross profit. Moreover, as converted apartments are purchased at lower prices, treated individuals will have had smaller loans and therefore smaller interest payments during their post-treatment tenancy.

Taken together, the estimates provided in Tables 5.1 and 5.2 indicate that on average, treated individuals increased their wealth by between

[15] For example, apartments built before 1930 tend to command higher prices. The same is true for apartments in newer buildings, constructed since 2000.

400,000 and 700,000 SEK on average, ranging from about twice to 3 times the median yearly income in 2005.

5.6 Treatment effect on mobility

Residential mobility

What is the effect of home ownership and wealth on residential mobility? To examine this question, we compare the evolution in the probability of moving in a given year before and after treatment. Panel A of Figure 5.3 displays estimated coefficients from Equation 5.2 showing the difference in moving probability between treated and untreated individuals over time. In the years leading up to tenants purchasing their apartments, the treatment group is less geographically mobile. Up to 10 years before treatment, they are about 1 to 2 percentage points less likely to have moved in a given year. Moreover, there seems to be no clear trend in the difference between the two groups. There is a small increase in mobility one year before treatment, however. This may be problematic if there is self-selection into buildings that are about to undergo a conversion to condominiums. It may nevertheless also be due to treated individuals updating their official addresses to the treated property, as this was a requirement for being eligible to purchase one's apartment. Individuals may for example have had to correct their official address from a previous residence or a partner's home. Below, we also assess the impact of this potential self-selection by excluding individuals who moved in just prior to the treatment taking place.

Panel A of Figure 5.3 shows that there is a noticeable increase in mobility already during the treatment year. Apartments that were resold within a year of the conversion were subject to a higher capital gains tax rate. This likely explains why in the following year, there is a second, larger jump in residential mobility. By one year after treatment, individuals are more than 2 percentage points more likely to have moved compared to the control group, whereas they were previously about 2 percentage points *less* likely to move. The effect persists at a similarly high rate in the following year. Starting three years after treatment, the effect begins to subside and gradually approaching at long-run rate of mobility which is higher than before treatment. Nevertheless, later moves may be driven by those who took longer time to change housing after treatment, rather than coming from repeat movers.

Table 5.3 displays the average effect of treatment on moving probability. Columns 1 to 5 gradually introduce different controls to the specification. With only the minimum controls for yearly fixed effects, Column 1 displays an average difference in mobility between treatment and control groups of 2.6 percent. As we control for birth-year and age fixed effects, home parish in 1998 as well as gender and educational attainment in Column 2, the coefficient remains similar at 2.7 percentage points. The estimate is also robust to controlling for separate yearly fixed effects by home parish in 1998, which is the first year that we observe individuals' parish of residence. In Column 4, we add separate cubic time trend for the treatment and control groups. This controls for the possibility that treated individuals had a different evolution of residential mobility over the sample period as a whole. The coefficient is slightly larger in this specification, which indicates that there may have been a somewhat downward trend in mobility among the treatment group. Lastly, Column 5 additionally controls for fixed effects at the individual level, restricting the analysis to within-individual comparisons and picking up unobserved fixed differences in geographic mobility across treated and untreated individuals. With this specification, the effect becomes larger, showing a 4 percentage point increase in mobility.

The estimated effect on residential mobility from a rental apartment building being converted into condominium are large. The average probability of moving in the control group is 9.5 percent per year. Given the estimates in Table 5.3, this indicates that the treatment increased geographic mobility by 27 to 42 percent over the mean. The treatment also increases mobility by 0.09 to 0.13 standard deviations. In Section 5.6, we attempt to decompose to what extent this effect is due to the wealth effect of the treatment versus the affect of no longer being in the rent control system.

Given the general increase in mobility after treatment, we next ask where treated individual chose to move. The way the outcome variable was defined in the above analysis, any change in address counts as moving, even if it is within the same city block. However, we may also ask whether treatment induces relocations over greater distances than this. Table 5.4 shows that relocations are indeed not only local. The treatment induces a greater probability of moving outside of the current parish (Columns 1–2), as well as outside of the current municipality (Columns 3–4). Moreover, there is a significant positive effect on moving to a different county than Stockholm. This constitutes additional evidence consistent with the existence of mismatch between the tenants housing consumption under rent control compared to choices that would be made if there were no distortions in housing allocations.

Robustness The possibility of financial gain from participating in a condominium conversion may have encouraged some individuals to move into properties that were about to be treated. Panel A of Figure 5.3 showed a small increase in moving one year before properties were converted. It is hence possible that there is selection into treatment, complicating a causal interpretation of the results. In order to quantify such possibilities we modify the treatment definition in two different ways. The results are presented in Table 5.5. In Columns 1 and 2, we remove from the sample all individuals who moved into a treated property during the year prior to its conversion. This specification should account for selection in to treatment. The estimated treatment effects on this subsample are significantly larger the baseline results. The probability of moving is increased by 1.5 to 2 percentage points over the baseline. As such, it appears that late-movers were not selecting in to treatment in order to quickly sell their housing and realize the profits. Rather, it is consistent with recent movers being more likely to have a good match with the new apartment, hence having less need to relocate after treatment.

In Columns 4 and 5, we instead define treatment as affecting everyone who lived in the building one full year before the treatment year. For example, rather than being a resident on December 31 of 2000 for a building that was converted in 2001, we now define treatment as affecting all those who lived there on December 31, 1999. This implies that individuals who for various reasons moved out before treatment will still be counted as treated, while those who moved in will not (same as in Columns 1–2). This specification should control both for selection in to and out of the treatment. This more conservative definition yields somewhat smaller but still economically and statistically significant positive effects on the probability of moving.

We conclude that neither opportunistic selection into treatment nor selection out of treatment are likely to be substantial causes of the estimated effects that were present in the baseline specifications of Table 5.3.

Relative importance of wealth versus increased liquidity

In this section, we consider the potential causal mechanisms that can explain the observed increase in mobility after the conversion of apartments from rentals to condominiums. We distinguish between two main mechanisms. First, because the treatment turns apartments into more liquid assets that can be easily sold at market prices, there may be a *liquidity effect* that encourages mobility. Such an effect allows tenants to re-optimize their consumption of housing (Wang, 2011). Second, the willingness to move may be increased due to a *wealth effect* as a result of the treatment. The larger the discount (compared to the market value) that an individual receives when purchasing the apartment, the greater the possibility to change and potentially upgrade one's type of housing. The extent to which each of these mechanisms explain residential mobility is crucial for understanding the policy consequences of controlled rents.

We propose a simple method for disentangling the relative importance of the liquidity and wealth channels. All treated individuals should exhibit the liquidity effect by virtue of participating in a condominium conversion. By contrast, the size of the wealth effect depends on the size of the discount (relative to the market value). We therefore distinguish between these two effects by also estimating the heterogeneous treatment effect by the size of the wealth shock within the treatment group. Given that the interaction between post-treatment and wealth shock estimates the wealth effect, the post-treatment indicator by itself captures the effect of the liquidity effect (i.e. the effect on mobility of receiving a wealth shock of zero).

Two separate but complementary measures of wealth shocks are used. For the first, we use data on gross profits made when selling apartments. For each parish, we estimate the excess profit made by treated versus non-treated apartments. Individuals are then assigned a wealth shock based on the parish in which they were treated. This measure serves as an proxy for the relative size of wealth shocks that treated individuals would receive if they bought their apartment. As the measure is fixed within time and a parish, it is not affected by potential strategic behavior on the part of treated individuals. For the second measure, we use directly observed changes in wealth from administrative data between the treatment year and the following year. This provides the most direct measure, at the individual level, of how much treatment increased wealth. The drawback is that individual wealth shocks may be endogenous, and that we only observe wealth between 1999 and 2007.

Table 5.6 displays the results of disentangling the liquidity and wealth mechanisms. For comparability, we first group individuals by percentiles of each measure's distribution, before taking logs. Columns 1 and 2 interact the post-treatment indicator with the parish-based shock measure. Interestingly, the interaction has a negative effect, suggesting that individuals who became relatively more wealthy were less likely to move after the treatment. The post-treatment dummy by itself indicates a significant positive effect on residential mobility, 4.8 percent both with and without the inclusion of individual fixed effects. Since the coefficient on the post-treatment dummy can be interpreted as the effect of treatment when the wealth shock was the smallest (the 1^{st} percentile), it is an indication of the direct effect of moving away from the rent controlled system. Hence, the liquidity effect seems to be the main mechanism driving the average effect seen in Table 5.3. Moreover, the effects on mobility would have been larger if the condominium conversion did not entail substantial discount to tenants. At the median wealth shock of 3.9 log points, the coefficients imply an increase in residential mobility of 3.5 percentage points, close to the average effect estimated in Table 5.3.

Substituting the parish-based shock measure with the directly observed change in net wealth following treatment yields similar results. Individuals whose net wealth increased by more were less likely to move, while the liquidity effect is estimated to approximately 8 percentage points (Columns 3 and 4).

Using both measures for the wealth shock induced by the treatment, we conclude that increased wealth is negatively linked to residential mobility. The effect is nevertheless comparatively small. Moving from the 25th to the 75th percentile of wealth shocks (1 log point for both measures) entails less than a one percentage point decline in moving. By contrast, the liquidity effect of moving away from the previous system leads to substantial positive effects on mobility.

Workplace mobility

The literature on labor mobility has highlighted the importance of residential mobility in enabling workers to switch jobs and take on more profitable employment opportunities. We test whether this holds true in our setting using yearly administrative data on the plant that individuals are employed at. This measure captures any changes in the location of the workplace, even if the employer stays the same.

Panel B of Figure 5.3 displays the estimated difference between treatment and control groups in workplace mobility before and after treatment. Contrary to the effects on residential mobility, workplace mobility in terms of switching to a place of work tends to decrease after treatment. We display the average effects on workplace mobility in Table 5.7. All estimated effects are negative, close to zero and considerably smaller than the case for residential mobility. Columns 1 to 5 introduce the same specifications as above. With no controls, the estimated effect is a reduction in probability of moving to a new plant of 0.5 percentage points per year. The effect is also robust to including individual-level control variables, but varies in size and statistical significance when adding further controls to the model. In two out five specifications, we cannot rule out that the effect is zero with 95 percent confidence. However, even the largest estimate implies a decline in workplace mobility that is small compared to the control group average of 3.5 percentage (the mean is 19.7 percent).

5.7 Conclusion

Numerous cities world-wide employ systems of rent control in order to regulate market prices and allow accessibility of attractive housing to less affluent citizens. An unintended consequence of such policies is that mobility of tenants may be inhibited. We study a Swedish policy which allowed landlords to sell rent controlled properties to tenant cooperatives, turning more than 100,000 apartments in the Stockholm region in to condominiums that could be bought and sold on the free market. In addition, tenants who converted their apartment to a condominium received substantial discounts compared to market prices, corresponding to several times the yearly median income.

We employ a difference-in-differences strategy to estimate the effect of this treatment on mobility. We first document a strong positive effect on residential mobility. In the year following a condominium conversion, the treatment group's residential mobility increases substantially by up to 42 percent of the control group mean. The effect subsidies over several years, but remains positive up to 10 years after treatment. The change in mobility is not only local, as treatment increases the probability of moving to a new parish, municipality as well as to a new county.

We are also able to disentangle the relative importance of two mechanisms that may cause this effect. We find that the wealth shocks generated by treatment tend to decrease the probability of moving. By contrast, the effect of participation in the treatment is larger and positive, irrespective of the size of wealth shock. This finding has important implications for the evaluation of rent controlled systems, as it indicates that the treatment caused greater mobility simply by moving residents into a market-based system.

While increased the residential mobility that we document is an indication of the misallocation of resources in the housing market, tenants may still have been mismatched to even if they chose to stay after converting their apartment to a condominium. Anecdotal evidence indicates that extensive renovations were common, even for those did not move. The quality of housing matches may hence have improved more than is indicated by residential mobility alone. Exploring the extent to which this is true would be an important extension to this project. In addition, the rise in the share of market-priced condominiums in the Swedish housing market of our period of study likely also had aggregate effects on the economy, as the possibility for outsiders to move into economic hubs such as Stockholm may have improved.

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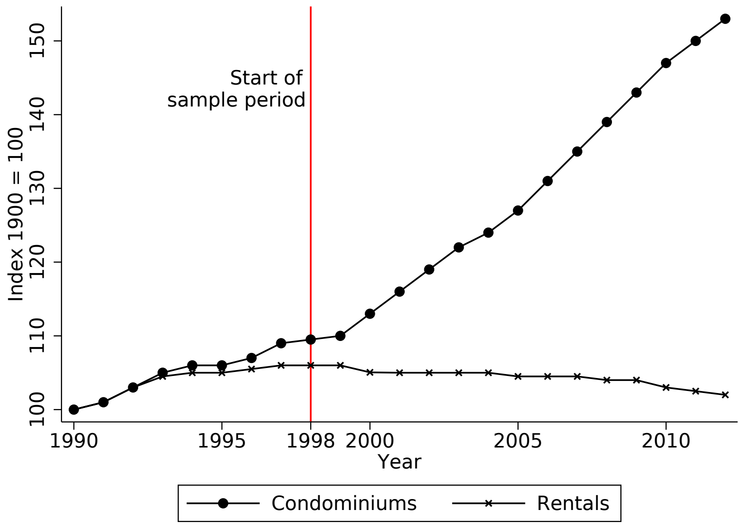
Figures and Tables

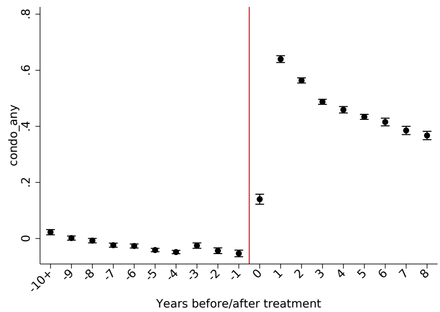
Figure 5.1: Evolution of apartments stock by type



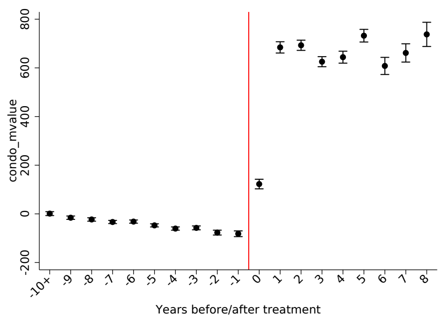
Notes: This figure displays indexes showing the evolution of the total number of condominiums and rental apartments in Sweden between 1990 and 2012. Our sample covers the period since 1998, during which most of the growth in the number of condominiums occurred. Source: Statistics Sweden.

Figure 5.2: Treatment effects on wealth and home ownership

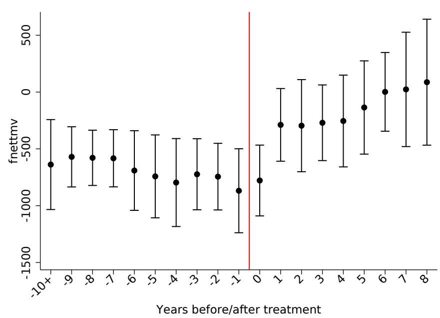
(a) Ownership of condominium



(b) Value of condominum assets



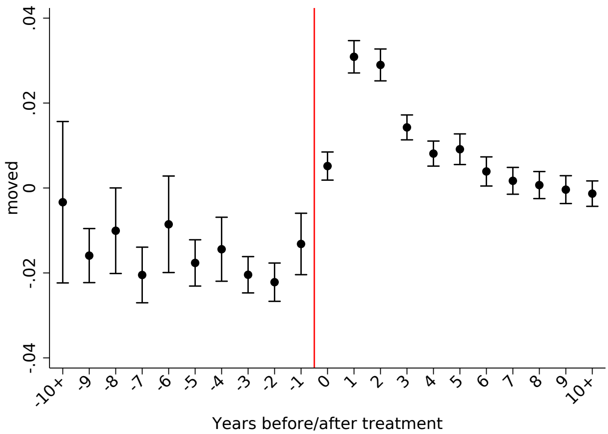
(c) Net wealth



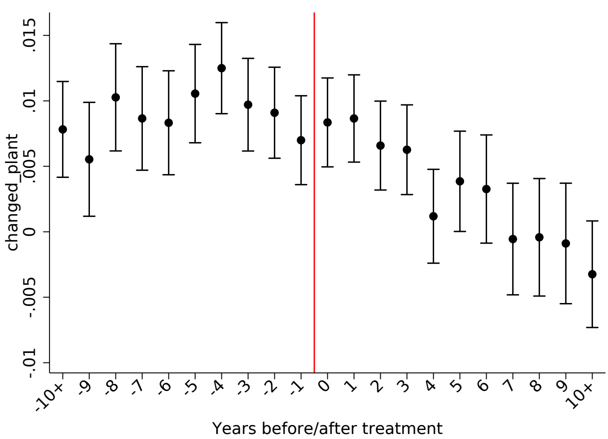
Notes: This figure displays the evolution of condominium ownership and wealth before and after treatment, with the red line indicating the treatment year. The data covers the period 1999-2007. Every point denotes the difference between the treatment and control groups at a particular year before or after treatment, as indicated by the x-axis. The label "-10+" indicates the average outcome 10 years or more before treatment. 95 percent confidence intervals are displayed, based on robust standard errors clustered at the property level. Each lead and lag is computed using data for those treatment years that are available. Hence, the further away from year zero one goes, the fewer treatment years are included.

Figure 5.3: Treatment effects on residential and workplace mobility

(a) Residential mobility



(b) Workplace mobility



Notes: This figure displays the evolution of residential and workplace mobility before and after treatment, with the red line indicating the treatment year. The data covers the period 1999-2007. Every point denotes the difference between the treatment and control groups at a particular year before or after treatment, as indicated by the x-axis. The labels "-10+" and "10+" indicate the average outcome 10 years or more before and after treatment, respectively. 95 percent confidence intervals are displayed, based on robust standard errors clustered at the property level. Each lead and lag is computed using data for those treatment years that are available. Hence, the further away from year zero one goes, the fewer treatment years are included.

Table 5.1: Treatment, home ownership and wealth

A. Dependent variable:	**Condominium ownership**				
	(1)	(2)	(3)	(4)	(5)
Treated	-0.020***	-0.027***	-0.028***		
	(0.003)	(0.003)	(0.003)		
Treated×Post	0.555***	0.546***	0.551***	0.583***	0.645***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.008)
B. Dependent variable:	**Total condominium wealth**				
	(1)	(2)	(3)	(4)	(5)
Treated	-16.3***	-45.1***	-41.7***		
	(2.6)	(3.0)	(2.8)		
Treated×Post	765.5***	722.3***	707.4***	733.3***	775.0***
	(11.4)	(9.7)	(9.5)	(9.8)	(12.4)
C. Dependent variable:	**Net wealth**				
	(1)	(2)	(3)	(4)	(5)
Treated	-766.3***	-706.4***	-699.5***		
	(151.6)	(149.5)	(147.4)		
Treated×Post	548.5***	488.3***	467.2***	551.2***	409.9***
	(85.6)	(84.2)	(105.5)	(69.6)	(59.0)
Individual controls	No	Yes	Yes	Yes	No
Parish 1998×Year FE	No	No	Yes	Yes	Yes
Treated Trend	No	No	No	Yes	Yes
Individual FE	No	No	No	No	Yes
Observations	2468409	2387928	2387928	2387928	2426122

Notes: OLS regressions. This table estimates the treatment effect on the likelihood of owning an condominium apartment (Panel A), on the value of condominium assets (Panel B) and on net wealth (Panel C). The sample covers the period 1999–2007. Wealth variables are denoted in 1,000 SEK. *Treated* indicates that an individual lived in a property that was converted to condominiums in the following year. *Post* as a dummy taking value one starting in the year of the condominium conversion. Individual controls are fixed effects for birth year, age and home parish in 1998, as well as dummies for being female and having a college education. *Treated Trend* denotes the inclusion of separate cubic time trends for the treatment and control groups. Robust standard errors clustered at the apartment building level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 5.2: Treatment effect on gross profits when selling apartments

Dependent variable:	**Gross profit**		
	(1)	(2)	(3)
Treated	773.6***	701.2***	710.9***
	(62.2)	(62.0)	(57.3)
Purchase and sale year FE	Yes	Yes	Yes
Parish FE	No	Yes	Yes
Construction year FE	No	No	Yes
Observations	190404	190404	190404

Notes: OLS regressions. This table estimates the difference in gross profits among apartments whose sellers acquired them through a condominium conversion and those that were not. Gross profits are denoted in 1,000 SEK. The sample includes all condominium sales in Stockholm county between 2000 and 2014. The observation level is one sale. Parish and construction year fixed effects refer to the apartment that was sold. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 5.3: Treatment effect on residential mobility

Dependent variable:	**Residential mobility**				
	(1)	(2)	(3)	(4)	(5)
Treated	-0.011***	-0.015***	-0.016***		
	(0.002)	(0.002)	(0.002)		
Treated×Post	0.026***	0.027***	0.028***	0.029***	0.040***
	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Individual controls	No	Yes	Yes	Yes	No
Parish 1998×Year FE	No	No	Yes	Yes	Yes
Treated Trend	No	No	No	Yes	Yes
Individual FE	No	No	No	No	Yes
Observations	3832373	3721596	3721596	3721596	3771582

Notes: OLS regressions. This table estimates the treatment effect on residential mobility, defined as the probability of reporting a new primary residence in a given year. The sample covers the period 1998–2012. *Treated* indicates that an individual lived in a property that was converted to condominiums in the following year. *Post* as a dummy taking value one starting in the year of the condominium conversion. Individual controls are fixed effects for birth year, age and home parish in 1998, as well as dummies for being female and having a college education. *Treated Trend* denotes the inclusion of separate cubic time trends for the treatment and control groups. Robust standard errors clustered at the apartment building level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 5.4: Treatment and moving destination

Dependent variable:	Residential mobility					
	New parish		**New municipality**		**New county**	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	0.028***	0.037***	0.024***	0.027***	0.008***	0.009***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Individual controls	Yes	No	Yes	No	Yes	No
Parish 1998×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Treated Trend	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	No	Yes	No	Yes	No	Yes
Observations	3673758	3721168	3721596	3771582	3721596	3771582

Notes: OLS regressions. This table estimates the treatment effect on moving destinations, with the move entailing either a change of home parish (Column 1–2), municipality (Columns 3–4) or county (Columns 5–6). The sample covers the period 1998–2012. *Treated* indicates that an individual lived in a property that was converted to condominiums in the following year. *Post* as a dummy taking value one starting in the year of the condominium conversion. Individual controls are fixed effects for birth year, age and home parish in 1998, as well as dummies for being female and having a college education. *Treated Trend* denotes the inclusion of separate cubic time trends for the treatment and control groups. Robust standard errors clustered at the apartment building level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 5.5: Robustness

Dependent variable:	**Residential mobility**			
	No recent in-movers		**Resident $\geq$ 1 years**	
	(1)	(2)	(3)	(4)
Treated×Post	0.045***	0.068***	0.027***	0.034***
	(0.001)	(0.002)	(0.002)	(0.002)
Individual controls	Yes	No	Yes	No
Parish 1998×Year FE	Yes	Yes	Yes	Yes
Treated Trend	Yes	Yes	Yes	Yes
Individual FE	No	Yes	No	Yes
Observations	3589594	3638639	3721596	3771582

Notes: OLS regressions. This table test for the robustness of estimated treatment effect to self-selection in to and out of treatment. Columns 1–2 exclude individuals who moved in to a property the year before treatment. Columns 3–4 define treatment based on living in the property two calendar years before treatment, including those who moved out before treatment and excluding those who moved in just before treatment. The sample covers the period 1998–2012. *Treated* indicates that an individual lived in a property that was converted to condominiums in the following year. *Post* as a dummy taking value one starting in the year of the condominium conversion. Individual controls are fixed effects for birth year, age and home parish in 1998, as well as dummies for being female and having a college education. *Treated Trend* denotes the inclusion of separate cubic time trends for the treatment and control groups. Robust standard errors clustered at the apartment building level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 5.6: Mechanism behind residential mobility

Dependent variable:	**Residential mobility**			
	(1)	(2)	(3)	(4)
Treated×Post	0.048***	0.048***	0.077***	0.081***
	(0.006)	(0.007)	(0.005)	(0.005)
Treated×Post×Wealth shock 1	-0.004***	-0.003*		
	(0.001)	(0.002)		
Treated×Post×Wealth shock 2			-0.007***	-0.007***
			(0.001)	(0.001)
Individual controls	Yes	No	Yes	No
Parish 1998×Year FE	Yes	Yes	Yes	Yes
Treated Trend	Yes	Yes	Yes	Yes
Individual FE	No	Yes	No	Yes
Observations	1500362	1519113	937860	949181

Notes: OLS regressions. This table estimates heterogeneous treatment effects by size of the wealth shock associated with treatment. The sample consists only of those who were ever treated. $WealthShock1$ indicates the average excess gross profit made by treated individuals in the same parish, estimated using condominium sales data. $WealthShock2$ indicates the change in net wealth observed in administrative data between the treatment year and the year after. This measure is only available 1999-2006. For both measures, we group individuals into percentiles of the distribution and take logs. $Treated$ indicates that an individual lived in a property that was converted to condominiums in the following year. *Post* as a dummy taking value one starting in the year of the condominium conversion. Individual controls are fixed effects for birth year, age and home parish in 1998, as well as dummies for being female and having a college education. *Treated Trend* denotes the inclusion of separate cubic time trends for the treatment and control groups. Robust standard errors clustered at the apartment building level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Table 5.7: Treatment and changing workplaces

Dependent variable:	**Workplace mobility**				
	(1)	(2)	(3)	(4)	(5)
Treated	0.026***	0.009***	0.007***		
	(0.001)	(0.001)	(0.001)		
Treated×Post	-0.005***	-0.007***	-0.002	-0.006***	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Individual controls	No	Yes	Yes	Yes	No
Parish 1998×Year FE	No	No	Yes	Yes	Yes
Treated Trend	No	No	No	Yes	Yes
Individual FE	No	No	No	No	Yes
Observations	3712921	3650004	3650004	3650004	3661016

Notes: OLS regressions. This table estimates the treatment effect on workplace mobility, defined as the probability of working in a new plant in a given year. The sample covers the period 1998–2012. *Treated* indicates that an individual lived in a property that was converted to condominiums in the following year. *Post* as a dummy taking value one starting in the year of the condominium conversion. Individual controls are fixed effects for birth year, age and home parish in 1998, as well as dummies for being female and having a college education. *Treated Trend* denotes the inclusion of separate cubic time trends for the treatment and control groups. Robust standard errors clustered at the apartment building level. *** - $p < 0.01$, ** - $p < 0.05$, * - $p < 0.1$.

Sammanfattning

Denna avhandling består av fyra separata uppsatser. Uppsatserna behandlar ett brett spann av ämnesområden, använder olika empiriska metoder och har urvalsstorlekar som sträcker sig från några hundra till flera miljoner. De är alla empiriska. Även om ekonomisk teori har varit en viktig inspiration för valet av ämnen, så har jag strävat efter att använda empiriska metoder för att inte bara visa att en idé är intressant i teori, utan också har bevisbara effekter på vår omvärld. De fyra uppsatserna i denna avhandling använder sig också uteslutande av svenska data. Det har inte nödvändigtvis varit min intention, men de tänkbara projekt som handlade om Sverige har visat sig vara de som varit både praktiskt genomförbara och tillräckligt intressant för att slutföra. Att det blev så är delvis på grund av slumpen, men också ett resultat av mängden högkvalitativa data som finns att tillgå i Sverige.

Mobilitet är den röda tråden som knyter samman uppsatserna i denna avhandling. Idag tycks konceptet mobilitet vara viktigare än någonsin. När människor från krigsdrabbade och diktatoriska länder söker sig till rikare och mer fredliga länder, finns det många som oroar sig för vad det har för effekt på de som inte kan flytta, och som lämnas kvar. Kapitel 2 och 3 i denna avhandling erbjuder historiska och kanske hoppfulla lärdomar om effekterna av internationell mobilitet på hemländer. Under 1800-talet, då var Sverige ett av de fattigaste länderna i Europa, lämnade över en miljon svenskar sina hem och emigrerade, främst till USA. Trots den starka oro som många hyste över Sveriges ekonomiska framtid efter emigrationen, finner vi att emigrationen ledde till både politisk och ekonomisk utveckling i kommunerna som skickade flest emigranter. Sveriges internationellt välkända fackföreningsrörelse och vänsterpartier stärktes i platser med mer emigration, vilket visas i kapitel 2. Utöver det visas även i kapitel 3 att ekonomisk utveckling kom hand i hand med emigrationen, då vi finner att teknologiska innovationer i form av patent ökar i dessa områden.

Mobilitet är viktigt även inom länder och mellan samhällsgrupper. Allt eftersom inkomstojämlikheten ökar i utvecklade länder har frågon om hur människor uppfattar sin ekonomiska status relativt andra fått större intresse. Uppsatsen i kapital 4 frågar hur väl svenskar kan placera sig själva i den svenska inkomstfördelningen. Vi finner att majoriteten av svenskarna underskattar sin relativa inkomstposition. Medianrespondenten tror att hen är nästan två deciler fattigare än vad hen i själva verket är. Denna felinformation får verkliga effekter när den korrigeras. När hälften av studiedeltagarna slumpmässigt får veta sin faktiska inkomstposition, svarar de som får veta att de är relativt rikare med att efterfråga mindre omfördelning i samhället. Ett intressant vidare resultat är att detta drivs helt av gruppen som redan tidigare hade politiska åsikter som lutade åt höger. Våra data antyder att detta resultat bygger på att individer som lutar mer åt höger också tenderar att tro att hårt arbete, snarare än slumpen, påverkar ens ekonomiska utfall samt att skatter har snedvridande effekter på människors arbetsutbud.

I den sista uppsatsen, kapitel 5, studerar vi hur residentiell mobilitet påverkas av hemägande samt förmögenhet. Med hjälp av data på boende i Stockholms län, undersöker vi vågen av ombildningar av hyresrätter som ledde till att det skapades över 100,000 bostadsrätter i länet sedan 1998. Hyresgäster som ombildar sina lägenheter erhåller stora rabatter jämfört med marknadspriset. Vi uppskattar att den genomsnittliga förmögenhetsökningen motsvarar två gånger medianårslönen 2005. Våra resultat fisar att hyresgäster som ombildade sina lägenheter uppvisar mycket högre residentiell mobilitet efter ombildningen, jämfört med kontrollgruppen av de som inte ombildade. De som ombildar sin lägenhet ökar sin årliga sannolikhet att flytta till en ny adress med 3 procentenheter, vilket är en relativt stor ökning då kontrollgruppens genomsnitt är 9.5 procent. Vi finner att storleken på inkomstshock *minskar* sannolikheten att flytta, medan själva ombildningen från prisreglerade hyresrätter till marknadsprissatta bostadrätter i sig förklarar den positiva genomsnittseffekten. En slutsats från denna studie är att den typ av hyresreglering som många städer använder sig av världen över kan leda till starkt minskad mobilitet på bostadsmarknaden och troligtvis leda till stora förluster i allokeringseffektivitet.

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