Immigrant Voters and the Size of the Welfare State*

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This paper studies the impact of immigration on public policy setting. We exploit as a natural experiment the sudden arrival of eight million forced migrants in West Germany after World War II. These migrants were on average poorer than the West German population, but in contrast to most international migrants, they had full voting rights and were eligible for social welfare. Using panel data for West German cities and applying difference-in-differences and an instrumental variables approach, we show that local governments responded to this migration shock with selective and persistent raises in local taxes and welfare spending and reductions in spending on infrastructure and housing. Data on local elections suggest that immigrants used their voting rights to influence local policy setting. We further document that this episode of mass immigration had lasting effects on people's preferences for redistribution. More than 50 years later, individual demand for redistributive policies is substantially higher in areas with larger inflows in the 1940s.

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1 Introduction

Immigration presents a major challenge to modern welfare states. A recurring concern is that generous welfare states attract low-skilled immigrants who supposedly benefit from public spending while contributing little in taxes. At the same time, immigration may reduce the level of taxation and spending if it lowers native voters' support for redistributive policies.[1] A critical determinant of natives' support for redistribution is the fact that immigrants typically have no voting rights, which means that natives can decide on taxation and spending purely based on their own preferences. In this paper, we provide contrasting evidence by focusing on a setting where immigrants *do* have voting rights. Based on a historical episode of mass migration in post-war Germany, we show that the mass inflow of poor immigrants led to a more generous welfare state and had a lasting impact on preferences for redistribution.

West Germany after World War II provides an ideal laboratory to study this issue. At the end of the war, the winning allies decided that Germany had to cede around 25% of its territory to Poland and the Soviet Union. All Germans who had previously been living in those Eastern territories of the German Reich were to be expelled and forced to move within the newly drawn borders of Germany. This decision resulted in the displacement of over twelve million people, of which around eight million settled in West Germany (Merten, 2012). Within four years, this unprecedented migration shock increased the West German population by almost 20%. These migrants — often called "expellees" — were similar to the West German native population in terms of culture and human capital, and as German citizens had voting rights and were eligible for social welfare from the time of arrival. However, after having lost virtually all of their assets during the expulsions, they were considerably poorer than the average person in West Germany.

The initial placement of the expellees gives rise to substantial geographic variation in the size of the inflow. Using panel data from West German cities, we exploit this variation to analyze whether cities responded to this migration shock by changing their tax and public spending policies. Within Germany's federal system, cities have enjoyed a high degree of fiscal autonomy; they could set their own business and property taxes and decide on a large number of spending items. Because most immigrants were poor and initially faced disadvantages in the labor market, many required social welfare, which at the time was mainly financed by the cities. This led to a situation in which cities with larger inflows were forced to raise their welfare spending. To cover the higher expenditures, cities had three major margins of adjustment, namely raising local taxes, reducing spending on other items, and incurring debt. While it may appear mechanical that high-inflow cities had to change their taxes and spending, it is far from clear *which* taxes and spending items they would change in response to the inflow. Our study focuses on exactly this question.

An obvious challenge to the estimation of a causal effect is the potentially endogenous location choice of the expellees after their arrival in West Germany. Immigrants may have been drawn to

[1] See Giulietti (2014) for a summary of the literature on the so-called "welfare magnet hypothesis". Studies by Borjas (1999) , de Giorgi and Pellizzari (2009) and Razin and Wahba (2015), among others, show that more generous welfare states attract larger numbers of low-skilled immigrants, whereas Levine and Zimmerman (1999) and Kaushal (2005) find little effect. Several studies provide evidence in favor of this hypothesis, showing that natives in states with high immigration prefer lower taxes and spending (e.g. Luttmer, 2001, Senik et al., 2009, Alesina et al., 2018). Razin et al. (2002) provide a political economy theory linking immigration to the size of the welfare state.

cities that changed their public policies for reasons other than the expellee inflow. We limit this concern by only using the *initial* allocation of expellees in West Germany throughout the analysis. Historical accounts suggest that the expellees' initial location choice was substantially constrained by housing shortages. Indeed, around half of the expellees were assigned to their initial housing by the allied administration (Kossert, 2008). To address the remaining concerns about endogeneity, we apply two complementary identification strategies.

To study the impact on local taxes, we collected panel data going back to the late 1930s and estimate a difference-in-differences (DiD) model in spirit of Duflo (2001) and Moser et al. (2014). This strategy allows us to compare the evolution of tax rates in high- and low-inflow cities while holding time-invariant city characteristics fixed. Our results show that tax rates in high- and low-inflow cities followed the same trend up until the inflows but significantly diverged thereafter. High-inflow cities significantly raised taxes on agricultural land as well as firms' capital and profits, and this gap persisted until at least the mid-1960s.[2] At the same time, we find no effect on the rates of two other important local taxes at the time, namely on residential property and a firm's wage bill. We see this as evidence that cities chose not to raise taxes on items that were most needed by poorer parts of society, namely housing and jobs, while shifting the administrative burden of taxation to farmers and business owners.

For all other outcomes, where data is only available for the post-war period, we pursue an instrumental variable (IV) strategy. We construct an instrument that predicts the inflow of expellees into each city based on the gravity forces that were important before the war but not thereafter. To build the instrument, we interact the pre-war population for each county in the ceded Eastern territories and Sudeten with the distance to each city in West Germany. For each West German city, the instrument predicts the inflow that would have occurred based on the city's location in relation to the ceded territories. The identifying assumption is that, conditional on control variables, the geographic distribution of Germans in the ceded territories before 1939 is orthogonal to economic conditions in West Germany after 1945. In our case, a particularly important control variable is a cities' closeness to the newly drawn inner-German border, as it was shown that West German cities close to the border had weaker economic growth over our sampling period and beyond (Redding and Sturm, 2008).

While the exclusion restriction — no direct effect of the pre-1939 gravity on taxes and spending — is not testable, we perform two plausibility tests that support the validity of the instrument. First, using tax rates as the outcome, we compare the results of the cross-sectional IV estimator to the (panel-)DiD estimator. This comparison is informative given that the DiD estimator controls for many factors that could potentially invalidate the exclusion restriction of the IV, such as pre-war differences in economic structure and political preferences, as well as time trends that differ by the degree of housing destruction and a host of pre-war economic, political and institutional characteristics. The IV estimator almost exactly replicates the DiD estimates, which we view as strong evidence in support of instrument validity. Second, following Conley et al. (2012), we assess the robustness of the IV to small violations of the exclusion restriction and show that the causal interpretation holds for

[2] From 1965 to 1975, territorial reforms substantially changed the borders of many West German counties which makes it difficult to investigate the effects of interest thereafter.

significant deviations from the assumption of strict exogeneity of the instrument.

The IV estimates show that high-inflow cities significantly shifted their spending away from non-welfare related items. While they decreased overall per capita spending, they increased their spending on social welfare while reducing their spending on local infrastructure, housing and schools. We also find suggestive evidence that high-inflow cities shifted part of the fiscal burden to future generations by incurring more debt, although we cannot rule out a zero effect.

We further show that changes in the local political economy may explain why cities responded to the expellee inflow with more redistributive taxation as well as shifts in spending. Using data on local elections from 1946 onwards, we find large positive effects on voter turnout as well as votes shares for the GB/BHE, a party that emerged in 1950 with the explicit goal of representing the interests of the expellees. We take these results as suggestive evidence that expellees made use of their voting rights and, despite being a minority in all cities, could exert influence on local politics.

In the final part of the paper, we show that this episode of mass migration had a lasting impact on people's preferences for redistribution. Using individual-level survey data, we focus on people born after the arrival of the expellees and compare those living in cities with high inflows of expellees to those in low-inflow cities. IV estimates show that more than 50 years after the expulsions individuals living in high-inflow cities show substantially stronger preferences for redistribution than people in low-inflow cities. This suggests that the arrival of the expellees is partly responsible for the significant differences in preferences for redistribution across German cities to this day.

Despite the peculiar historical setting — German citizens settling in another part of the country — these findings hold two general lessons for immigration and its impact on the welfare state. First, while most international migrants do not have voting rights, there are debates in several countries whether to grant these rights to long-term immigrants. For example, migrants from within the EU are allowed to vote in local elections their EU country of residence, and if the number of migrants increases, they may at some point demand voting rights at higher levels of government. Second, the largest migration flows in the world occur within countries. Low- and middle-income countries experience vast rural-to-urban migration, and to the extent that these migrants have voting rights, migration may have similar effects on the size of the welfare state.

Besides these general lessons, the paper contributes to four strands of literature. First, it makes an important qualification to the existing literature on the political economy of immigration. Theories by Razin et al. (2002) and Alesina and Glaeser (2004) predict that immigration lowers native support for redistribution as long as migrants have no voting rights.[3] The paper most closely related to ours — Tabellini (2017), written simultaneously and independently from our work — confirms the existence of this mechanism for the mass migrations to the US in the early 20th century. It shows that cities with higher inflows of migrants were more likely to vote conservative, lowered taxes and reduced spending on public goods. Our paper finds the exact opposite: cities with high inflows of expellees implemented *more* redistributive policies; they raised taxes and spending. Given that the expellees faced significant hostility upon arrival in West Germany, we hypothesize that the crucial difference explaining the opposing results is voting rights. Unlike the immigrants that came to the

[3] There is empirical evidence on this mechanism by Dahlberg et al. (2012), although the original findings have been called into question by Nekby and Pettersson-Lidbom (2017).

US in the early 20th century, the expellees had voting rights upon arrival and, therefore, could exert their influence on the political process.

Second, our results highlight the importance of personal experiences in shaping people's preferences for redistribution. Previous studies have shown that exposure to political and economic conditions during formative life periods determine people's preferences for income taxation and the provision of public goods (Corneo and Grüner, 2002, Alesina and Fuchs-Schündeln, 2007, Giuliano and Spilimbergo, 2014, Fuchs-Schündeln and Schündeln, 2015). We find that the inflow of expellees shapes preferences for redistribution more than half a century later, which partly explains the large variation in taxation and spending across German cities today.

Third, our paper provides a new perspective on the impact of migration on natives' economic outcomes. While in many countries the general public is concerned about migration, most studies find small effects of immigration on natives' wages and employment.[4] Our paper illustrates an economic impact that mainly operates outside the labor market, namely on local public policies. West Germans who lived in cities with high immigration in the 1940s faced higher local taxes as well as a shift in public spending. This is not to say that the overall impact of the expellees was negative — work by Braun and Kvasnicka (2014) and Peters (2017) suggests that their inflow significantly contributed to structural change and economic growth in the 1950s — but their arrival affected redistribution *within* cities, which was costly to some individual while being beneficial to others.

Finally, the paper relates to the literature on the economic consequences of forced migration. Conflicts and wars have triggered large migration waves in the past, and presumably will do so in the foreseeable future. As one of the largest episodes of forced migration in history, the population transfers in Europe in the 1940s have been used in several studies to illustrate the impact of migration on labor markets (Braun and Mahmoud, 2014), structural change (Braun and Kvasnicka, 2014, Peters, 2017), investment in education (Semrad, 2015, Becker et al., 2018), as well as to demonstrate the strong persistence of population shocks (Schumann, 2014) and the importance of social ties for economic development (Burchardi and Hassan, 2013).[5] Our paper adds to this literature by showing that forced migration — even when immigrants are similar to natives — can have profound consequences for public policy setting, which might affect large parts of the native and immigrant population in turn.

The remainder of the paper unfolds as follows. Section 2 provides the historical background about the expulsions of Germans after World War II as well as an overview of local public policy setting in West Germany. Section 3 presents our analysis on the impact of immigration on taxation, spending, debt and transfers. Section 4 investigates whether changes in the local political equilibrium can explain the observed effects on public policy setting. Section 5 presents the long-run effects of immigration in the 1940s on people's preferences for redistribution 50 years later. Section 6 concludes.

[4] See Dustmann et al. (2016) for a discussion of the empirical findings and the underlying methodologies.

[5] In addition, studies by Falck et al. (2012) and Bauer et al. (2013) analyze the economic integration of the expellees in West Germany. Other examples for forced population transfers are the population exchange between Greece and Turkey in the 1920s (Murard and Sakalli, 2015) and the forced resettlement of parts of the Finnish population after WW II (Sarvimäki et al., 2016).

2 Historical and Institutional Background

The expulsion and resettlement of over 12 million Germans in the aftermath of World War II is widely acknowledged as one of the largest forced population movements in history (Douglas, 2012). In this section, we provide an overview of the historical events that led to the expulsions as well as the context of the expellees' economic and political integration in West Germany. In particular, we explain why this inflow led to greater demand for social welfare, and why this makes post-war Germany an exemplary setting for studying the impact of immigration on public policy setting. We then turn to our main outcome variables, namely local taxation, spending and debt and provide a brief historical account of German cities' far-reaching autonomy in public policy setting and their obligation to provide social welfare.

2.1 The Forced Migration of Germans after World War II

Between 1944 and 1950, more than twelve million ethnic Germans were expelled and re-settled from former territories of the German Reich in Eastern Europe as well as from Central and East European countries, where German communities had been living since the Middle Ages (Merten, 2012, ch.1).[6]

Migration flows to the West began in the final phase of WW II when inhabitants of the Eastern territories fled from the advancing troops of the Soviet Army, and intensified when local militia began to seize German property, particularly in East Prussia, Pomerania and Silesia (Douglas, 2012). In June 1945, after Nazi Germany's unconditional surrender, the expulsions were institutionalized when the Winning Allies agreed upon the delineation of Germany's boundaries and ordered that all Germans living outside these new borders had to be re-settled. Germany had to cede its territories east of the rivers Oder and Neisse — East Prussia as well as large parts of Pomerania, Silesia and Brandenburg — to Poland and the Soviet Union (see Appendix Figure A.1 for details). The remaining German territory was first occupied by the Winning Allies and later — from 1949 until the reunification in 1990 — formed the Federal Republic of Germany (*West Germany*) on the territory of the American, British and French occupation zones and the German Democratic Republic (*East Germany*) on the territory of the Soviet occupation zone.

Size of the population shock and initial settlement. Out of more than twelve million expellees, around eight million arrived in West Germany between 1944 and 1950. The remaining four million either died in transit or settled in East Germany. In West Germany, this inflow increased the country's population by almost 20% (Kossert, 2008). After reaching the West German territory, many expellees were first transferred to temporary refugee camps and subsequently assigned to municipalities in the American and British occupation zones. Because France suffered from greater war damage than the US and the UK, no expellees were allowed to settle the French occupation zone before mid-1949 (Douglas, 2012, ch. 6).

Data from the "Statistical Yearbook of the Expellees" (*Statistisches Taschenbuch über die Heimatvertriebenen*, Statistisches Bundesamt (1953)) allow us to precisely measure the initial inflow and

[6] Among others, large German minorities had been living in Czechoslovakia, the Baltic countries, Poland, Hungary, Romania, Ukraine, Yugoslavia and parts of the Soviet Union.

geographic distribution of the expellees. For each West German county (*Kreis*), the yearbook provides detailed information on the total number and population share of expellees as of September 1950, as well as aggregate information on the expellees' region of origin, religious composition, and further population characteristics. Therefore, it represents the earliest consistent account of the stock of expellees in West Germany.[7]

As of 1950, the average share of expellees among the population was 16.7%, although inflow intensities differed remarkably across West Germany, ranging from 1.8% in Pirmasens/Rhineland-Palatinate to 44.1% in Salzgitter/Lower Saxony. Figure 1 illustrates the geographic distribution of expellees across West Germany.[8] Most expellees arrived in the states of Schleswig-Holstein and Lower Saxony in the North, as well as in Bavaria in the South-East of the country, whereas a substantially lower number settled in the federal states of North Rhine-Westphalia, Rhineland-Palatinate and Baden-Württemberg in the (South-)West. It becomes apparent that distance from the former German territories in the East substantially affected the distribution of migrants across West Germany — a feature we exploit in the empirical analysis below.

The initial settlement of expellees across Germany did not follow a systematic protocol. Initially, the Allies' plan was to allocate them according to demographic and economic factors such as population density or economic potential. However, due to the severe destruction of the housing stock in most German cities and the rapid inflow of refugees within a short time span, the availability of accommodation soon became the decisive factor. As a consequence, the expellees were mostly allocated to rural areas and smaller cities, where the destruction of the housing stock was less severe (Henke, 1985).[9]

Economic and social integration of the expellees. Because most migrants were expelled from former German territories (Statistisches Bundesamt, 1953, p. 4), they had been subject to the same political and economic institutions as the West German population prior to WW II. Appendix Figure A.3 documents that ceded counties in the East did not systematically differ from the Western parts of the German Reich with respect to the occupational composition of the workforce or the political orientation of the electorate before WW II. Both parts differed, however, in their religious composition. Compared to West Germany, the Eastern territories had a significantly higher share of protestants, such that the inflow the expellees changed the local religious composition of the population in some regions in the West (Kossert, 2008, ch. 7).

Despite the apparent cultural similarity between the expellees and the incumbent population, the economic and social integration of the expellees presented a major challenge to the West German society.[10] Historical accounts document that the West German population was all but welcoming

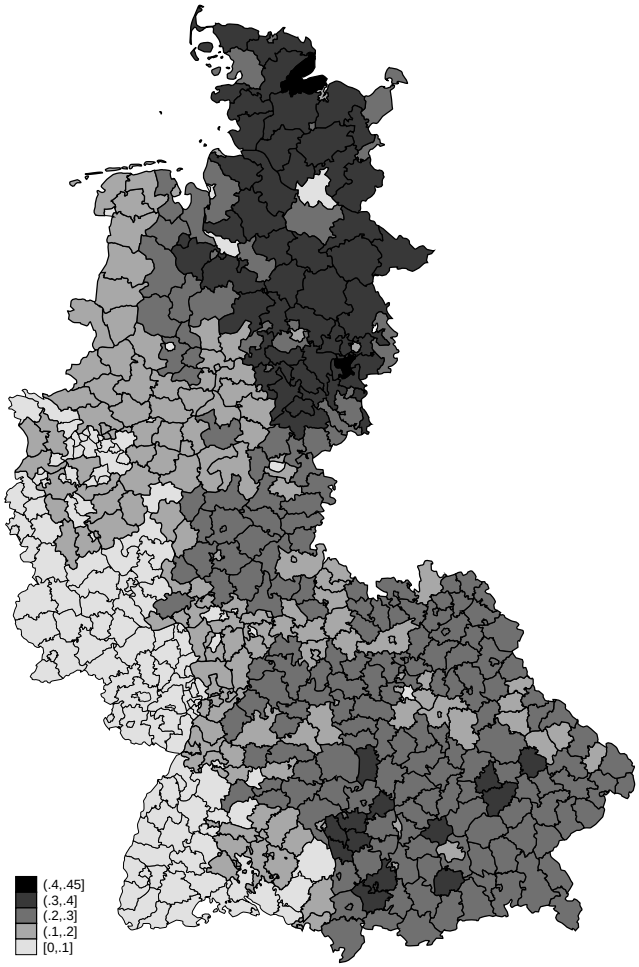
[7] Several state-level yearbooks provide data from as early as 1948. For the whole of West Germany such data is only available for 1950.

[8] We exclude the city of West-Berlin from our analysis due to its very specific geographic position and structure. The Saarland, which was administered by France from 1947 to 1956 and rejoined West Germany in 1957, is not covered by our data.

[9] While many expellees subsequently moved to larger cities, Schumann (2014) shows that this initial population shock was remarkably persistent across regions until the 1970s.

[10] The provisional West German government and the Allied Forces initiated a set of comprehensive policies to improve the economic conditions of the expellees (*Soforthilfegesetze*), which included basic social assistance, once-off transfers to families, subsidies for education and training, credits for business creation, and funds for housing construction

Figure 1: Shares of Expellees by County in West Germany, 1950



Notes: This map shows the county-level population share of expellees in West Germany as of September 1950. Data are taken from the "Statistical Yearbook of Expellees" (Statistisches Bundesamt, 1953). The city of West-Berlin and the Saarland are excluded. The figure is based on shapefiles provided by Max Planck Institute for Demographic Research (MPIDR) and Chair for Geodesy and Geoinformatics, University of Rostock (CGG) (2011).

towards the expellees. While natives and migrants shared the same ethnicity and citizenship, many West Germans expressed their hostility towards the expellees — an episode described as "*racism of Germans against German expellees*" (Kossert, 2008, ch. 4).

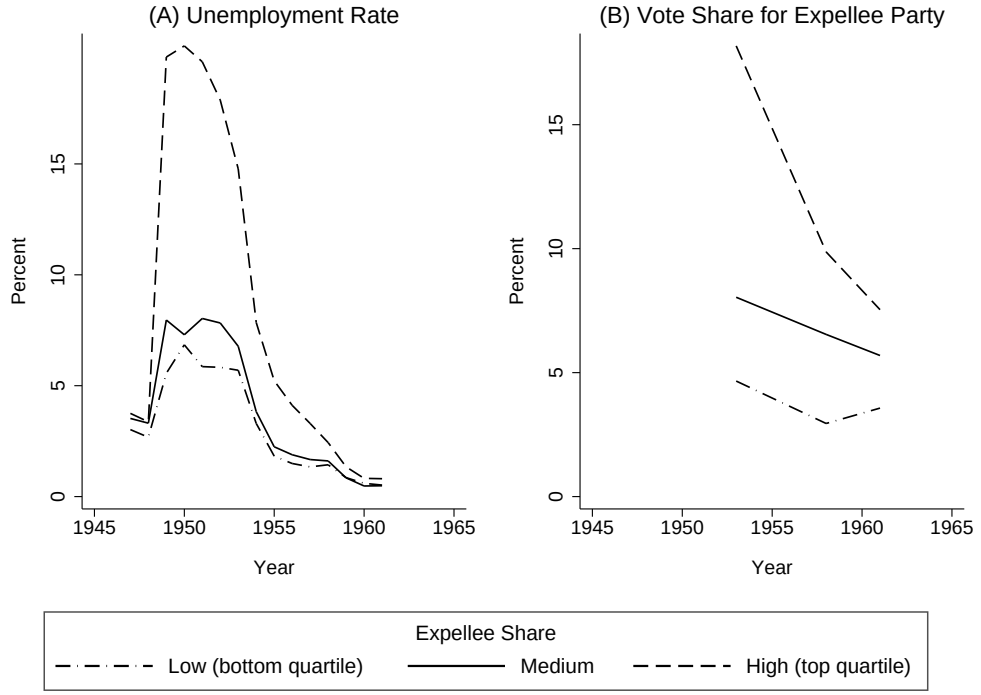
A key difference between both population groups was the severe poverty of the expellees relative to the incumbent population. During the expulsions, the expellees had lost their homes, jobs and virtually all of their possessions and real assets. Therefore, cities with a large inflow of expelled Germans experienced a significant shock to the local income and wealth distribution among their population. While many West Germans undoubtedly experienced severe losses from war destruction as well as economic deprivation during the early post-war years, a considerable number of people owned the remaining real assets such as agricultural land, livestock, properties and businesses.

(Schillinger, 1985). These measures, along with a second redistribution program introduced in 1952 (*Lastenausgleichsgesetz*), were funded by a federal tax on assets and a tax on gains from debt relief after the introduction of the Deutschmark (Schmölders, 1955, ch.2).

Moreover, unlike the expellees, West Germans could draw on their pre-existing social networks to find employment or obtain loans.

Among the expellees, the initial economic disadvantage led to persistently lower earnings and higher unemployment.[11] This is reflected in Panel (A) of Figure 2, which displays the evolution of the unemployment rate for the total population in West German cities with different expellees shares. From the late 1940s to the mid-1950s — that is, before Germany's post-war growth miracle (*Wirtschaftswunder*) — unemployment was particularly high (above 15%) in cities with a large share of expellees among the population. While the economic upswing of the mid 1950s substantially reduced unemployment rates across the country, high-inflow cities were characterized by above-average unemployment levels until the early 1960s. This difference can be partly explained by limited employment opportunities in more rural areas where most expellees initially settled, a greater mismatch between local labor demand and the expellees' skills, as well as labor market discrimination by West German employers. Paired with the low wealth levels, the limited success in the labor market meant that welfare benefits presented a critical source of income for a substantial fraction of the expellees.

Figure 2: Expellee Inflow, Local Unemployment and Political Representation



Notes: This graph shows how the average local unemployment rate (Panel A) and the vote share for the expellee party GB/BHE in local elections (Panel B) evolved over time in cities with low, medium and high inflow of expelled Germans in 1950. See Appendix Table A.1 for definitions of the variables and the underlying data sources.

[11] Evidence abounds that the German governmentâ€™s efforts of integrating the expellees was only partially successful, if at all. As shown by Falck et al. (2012) and Bauer et al. (2013), the Federal Expellee Law (*Vertriebenengesetz*), introduced in 1953 to foster the integration of expellees in the West German labor market, neither met its goals in the early post-war period nor in the longer run. In the 1970s, the first- and second-generation migrants were still lagging behind West German natives in earnings, homeownership rates, and education.

Citizenship and political representation of the expellees. In contrast to most other immigrants, the expellees were considered German citizens upon arrival. This granted them two fundamental rights. First, they were eligible for means-tested social welfare, which comprised social benefits, housing assistance, access to health care, and support with nutrition and clothing. Second, they had full voting rights in local, state and federal elections.[12]

Panel (B) of Figure 2 provides evidence that the expellees exerted their voting rights. The figure shows the vote shares in local elections for the GB/BHE (*Gesamtdeutscher Block/Bund der Heimatvertriebenen und Entrechteten*), a party founded by expellees in 1950 with the goal of improving the group's economic situation in West Germany as well as lobbying for a return of their properties in Germany's former Eastern territories. The vote share for this party was substantially higher in cities with a larger share of expellees, suggesting that the expellees — despite not being a majority in any city — could influence local politics. This also meant that political parties had an incentive to cater for the interests of these new voters and to account for their needs when setting public policies.

2.2 Local Public Policy in West Germany

Our outcomes of interest are policy variables that are set by the cities, namely local business and property taxes, spending on amenities, and debt.

Since the early 19th century, German cities and municipalities have had far-reaching autonomy in fiscal matters. These rights were substantially expanded and harmonized during the 1930s, when the Nazi regime re-organized the political organization as well as the system of public finances for all municipalities in the German Reich (*Deutsche Gemeindeordnung*). The general principles of this code have served as the basis for the fiscal self-government rules of cities in West Germany after World War II and – with some modifications – have been in place until today.[13]

Local taxation. The municipal code obliges cities to set local tax rates on an annual basis along with their budget plan for the following year.[14] Importantly, the legal definition and the valuation procedure of the respective tax base are set by the federal government, such that cities' only margin of adjustment is the actual tax rate. This margin of fiscal autonomy was only constrained during WW II, when the Nazi regime prohibited tax rate increases above the respective level of 1939. Only in 1942 and 1943, the regime allowed some limited increases in tax rates above the level of 1939 (Voigt, 1975).

Until today, the actual tax rate consists of two elements: the uniform basic rate (*Steuermesszahl*), which is set by the federal government, and the city-specific tax collection rate (*Hebesatz*). In the empirical analysis, we use these tax rates as outcome variables. We focus on the five most important local tax rates, namely the

[12] The electoral law for the first election of the West German Federal Parliament (*Bundestag*) in 1949 ruled that German citizens as well as individuals of German ethnic origin who were permanent residents of West Germany could vote. Electoral laws on state and local level contained similar provisions.

[13] While the original code specified that mayors and local council members (*Mitglieder des Gemeinderats*) had to be appointed by the Nazi party, since the end of World War II, mayors and local councils have been elected.

[14] Since 1946, the tax rates and budget plan have to be passed by the elected local council. Before the end of WW II, the rates were set by the appointed major.

- *Agricultural Land Tax (Grundsteuer A)*, a tax on the value of agricultural enterprises (farmland),
- *Residential Property Tax (Grundsteuer B)*, a tax on the value of non-agricultural real estate,

- *Business Capital Tax (Gewerbekapitalsteuer)*, a tax on firms' capital stocks,
- *Business Profit Tax (Gewerbeertragssteuer)*, a tax on firms' profits,[15]
- *Wage bill tax (Lohnsummensteuer)*, a tax on a firm's total wage bill.

Over the sampling period, these five taxes accounted for up to 90% of cities' overall tax and more than 70% of their total revenue. Although cities received additional transfers from the federal and state governments in relation to their population size and economic situation and could incur debt to finance their expenditures, local taxation was their most important source of revenue.

Given that initially the expellees neither owned properties nor businesses, the administrative burden of these taxes lay almost entirely on the incumbent West German population. However, the economic burden of these taxes may have been passed on to the expellees and West Germans without any asset holdings. For example, higher residential property taxes may have resulted in higher rents rents, a wage bill tax may have curbed workers' wages or employment opportunities, and higher agricultural taxes may have led to higher food prices.

Local public spending. While cities enjoyed autonomy in levying taxes, they also were — and still are — responsible for the financing and provision of a wide range of public goods and services. Examples include the provision of social welfare, the upkeep of public safety and order, the provision of public and cultural amenities such as parks, sports facilities, museums and theaters, the building and upkeep of local infrastructure such as roads and public transport, the co-financing of hospitals and other health care facilities, or the provision of school buildings.

Given the expellees' dire economic situation after arrival, the majority of this group required social welfare, which posed a tremendous logistical and financial challenge to cities (Föcking, 2009). Throughout the 1940s and 1950s, local authorities had considerable discretion with regard to welfare provision. Before 1962, there was no legal entitlement for social welfare, such that benefit levels varied across cities. By and large, the payments aligned with local costs of living and followed the principle that benefits must be lower than local wages. However, to some degree the variation in benefit levels also reflected the municipalities decisions to spend their revenues on public amenities rather than welfare (Willing, 2001, Föcking, 2009).

3 Main Analysis: Immigration and Public Policy Setting

In this section, we analyze if West German cities responded to the inflow of 8 million forced migrants by changing their public policies. We begin by focusing on tax rates, where panel data allow us to apply a difference-in-difference model. For all other outcomes, for which data are only available for the post-war period, we apply an instrumental variable strategy and provide a detailed discussion on the validity of the identifying assumption. While it may appear mechanical that cities with high inflows of poor immigrants were forced to raise welfare spending and, consequently, needed to raise

[15] As the city-specific collection rates on business' capital and profits had to be identical by law, we report estimates for only one (the tax on business capital) of the two tax rates below.

taxes and reduce spending on non-welfare items, it is far from clear *which* taxes they would raise and on which items they would spend less. Our detailed tax and spending data allow us provide a nuanced picture of the impact of the inflow on the local policy mix.

We describe the data sources along with the results. A more detailed description of the entire dataset can be found in Appendix A.

3.1 The Effect of Immigration on Local Taxation

Theoretical considerations. We begin by investigating the effect of the expellee inflow on local tax setting. As explained in Section 2, with the exception of the first half of the 1940s, cities had full discretion in setting local property and business tax rates. Standard models of optimal taxation (e.g., Ramsey, 1927) would predict that cities respond to the inflow of poor migrants and the need for higher fiscal revenues by increasing tax rates on less mobile assets or agents. Therefore, among the four main local tax rates, we would expect to see steeper raises in property tax rates than in tax rates on a firm's capital or wage bill. Properties are immobile by definition, while firms may shift their operations to places with lower taxes.

However, in the context of post-war West Germany, the theoretical predictions may not be as clear-cut. For each tax, cities had to weigh the marginal increase in revenue against the marginal costs for all or some of their citizens. For example, in light of the severe housing shortages after World War II, cities had every reason to provide an incentive for construction by keep taxes on residential properties low. Likewise, high taxes on agricultural properties could have led to higher food prices, which would have hurt poorer parts of society who had to spend an even larger share of their income on food. Similar arguments apply to tax increases on firms' capital and wage bill. Higher taxes on a firm's capital may have lowered incentives to invest, while a higher tax on a firm's wage bill may have reduced incentives to hire new workers in the short run or even induced a shift in production towards less labor-intensive production in the longer run. Ultimately, which of these tax rates cities decided to adjust and to what extent remains an empirical question.

Empirical model. To analyze the effect of immigration on local taxation we collected panel data on local tax rates for the 400 largest German cities from the 'Statistical Yearbooks of German Municipalities' for the period from 1938-1965.[16] The fact that we observe tax rates before and after the inflow of expellees allows us to estimate a causal effect using a difference-in-differences (DiD) design.

Cross-sectional OLS estimates would most likely be biased because the same unobserved factors that determined the size of the expellee inflow into a city may have also determined a city's tax setting. Our DiD design enables us to absorb time-invariant city characteristics and compare the evolution of local tax rates in cities with high and low inflows of expellees before and after the expulsions. While almost all cities considerably raised their local taxes after World War II (see Appendix Figure A.4), our model allows for the estimation of the *differential* effect of the expellee inflow on tax setting, i.e., the extent to which cities with higher shares of expellees raised their taxes *more* than those with lower shares.

[16] *Statistische Jahrbücher Deutscher Gemeinden*; see Appendix Table A.1 for details.

The specification of the regression model follows Duflo (2001) and Moser et al. (2014) and takes the form

$$y_{mct} = \sum_{t \neq 1944} \delta_t (ExpShare_c \times \tau_t) + \sum_{t \neq 1944} \rho_t (X_{mc} \times \tau_t) + \phi_t + \phi_m + \varepsilon_{mct}, \quad (1)$$

whereby we regress the respective tax rate of city m in county c in year t on the interaction terms of the expellee share in county c and year dummies ($ExpShare_c \times \tau_t$). To exploit variation within cities over time, we control for city fixed effects (ϕ_m). Year fixed effects (ϕ_t) further absorb changes in tax rates that are common to all cities in West Germany. We choose 1944, the year before the onset of the migration flow, as the base year. Therefore, our coefficients of interest δ_t measure the effect of an increase in the share of expellees within a city on the local tax rate relative to the base year 1944.

While tax rates and all other outcome variables vary at the city level, our regressor of interest, the share of expellees in 1950, varies at the county level. These data represent the earliest available comprehensive data source to consistently measure the spatial distribution of expellees in Germany. Despite the potential risk of measurement error, we explicitly chose these data to capture the *initial* allocation of the expellees to the best possible degree. Because the expellees could freely move after their initial assignment, later measures of the share of expellees would potentially be endogenous.[17]

In addition to the city and time fixed effects, we further account for historical and institutional differences that may have had persistent but *time-varying* effects on tax rates while also explaining the settling pattern of the expellees. The vector (X_{mc}) includes measures of institutional, economic and social differences as well as the local extent of housing destruction after the end of the war. To allow for a time-varying effect on taxation, we interact each variable with year dummies.

Specifically, the set of institutional controls comprises dummy variables for the three Western occupation zones, an indicator whether a city was part of Prussia, and a dummy variable that equals unity if a city is located closer than 75km to the inner-German border. The occupation zone dummies explicitly control for common shocks within the occupation zones due to varying policies by the three Western Allies. The Prussia dummy, in turn, accounts for historical institutional differences between Prussia and the rest of the former German Empire. Finally, the border dummy controls for the lower growth trajectory of cities close to the inner-German border, a direct consequence of the division of Germany into East and West in 1945. Cities that were located in the center of the country up until 1945 found themselves in a remote location thereafter, which meant reduced access to markets and lower subsequent growth. Redding and Sturm (2008) show that the economic consequences of closeness to the border are concentrated within approximately 75km of the border, which is why we define our dummy variable accordingly.

The vector X_{mc} further comprises county-level measures of social and economic differences across West Germany before WW II, namely the average vote share for the Social Democratic Party (SPD) in the federal elections between 1924-1933, the share of protestants in 1925 — both proxies for potentially persistent differences in political attitudes, work ethic and values —, the respective share of civil servants and unemployed workers in 1933, and the (log) population density in 1939 — proxies for economic prosperity before the war. All data on pre-war social and economic differences are

[17] In our view, this also holds true for the earliest city-level dataset that measures the share of expellees among the local population as of 1952. Nevertheless, estimation results are very similar when we use the data from 1952.

taken from King et al. (2008) (see Appendix Tables A.1 and A.2 for details). Finally, to proxy for the degree of local war destruction, X_{mc} comprises the county-level share of destroyed housing units. In our setting, this control is important because cities with greater housing destruction received fewer expellees while having had good reasons to raise taxes to finance reconstruction.

The error term ε_{mct} summarizes all determinants of local tax rates that are not captured by our set of regressors in Equation (1). Throughout the analysis, we cluster standard errors at the county level to explicitly account for any potential correlation in the error terms across cities within a county and within counties over time.

Identification. As standard in DiD designs, causal identification of the parameters of interest δ_t rests upon the assumption that, conditional on covariates, tax rates in cities with a low and high inflow of expellees would have followed the same evolution in the absence of treatment. Our DiD approach allows us to corroborate the identifying assumption through the inspection of pre-trends, i.e., by considering the effect of the expellees share on tax rates prior to the inflow of migrants. If the expellees were to have any effect on tax rates, we would expect statistically significant estimates after the inflow, but not before. Significant effects before 1945 would, in turn, invalidate our research design and indicate that low and high-inflow cities were already on different trends in their tax setting *before* the actual inflow.

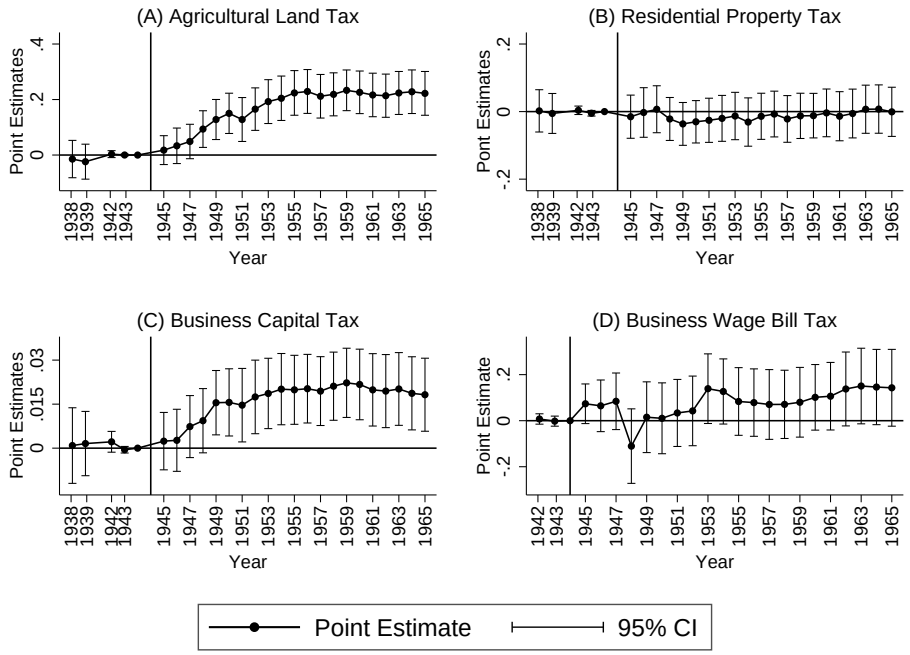
Effects on local tax rates. Figure 3 displays the estimated coefficients of our DiD appproach for the four tax rates of interest.[18] To make the effects comparable across outcomes, we standardize the share of expellees by dividing it by the sample standard deviation. In each sub-figure, the horizontal line at $\widehat{\delta_t} = 0$ represents the (regression-adjusted) trend in the respective tax rate for cities with a mean share of expellees (16.7%). The vertical line marks the arrival of the first wave of expellees in late 1944, and thus the beginning of treatment.

Figure 3 reveals two central results. First, cities responded to the expellees inflow with selective changes in tax rates. Cities responded to a high inflow of expellees by raising tax rates on agricultural land and firmsâ€™ capital (Panels (A) and (C)), while we find no effect on residential property and wage bill taxes (see Panels (B) and (D)). A one-standard-deviation increase in the share of expellees — corresponding to 9 percentage points — led to an additional raise in the agricultural land tax by 0.2 percentage points, corresponding to 18.7% of the mean tax rate in 1944 and to a raise in the capital tax by 0.015 percentage points, corresponding to 3% of the mean. While the effect on the capital tax appears small relative to the effect on the agricultural land tax, the additional revenue generated by the raise in the capital tax was substantial. For an average city, these effects imply an additional annual revenue of 8,500 DM for the agricultural land tax and an additional revenue of 316,000 DM for the capital tax.

Second, the initial changes in tax rates remained persistent over time. The gap in tax rates on agricultural land and business' capital between high- and low-inflow cities opens shortly after the inflow, and remains at a similar level until the end of our sampling period in 1965.

[18] As explained in Section 2, the tax rate indicates the percentage of the tax base that has to be paid to the city in a given year. The tax rate is given by the city-specific collection rate multiplied by a common basic rate that is set by the federal government.

Figure 3: The Effect of Mass Migration on Local Tax Rates: DiD Estimates



Notes: This figure displays the point estimates and 95%-confidence intervals for the effect of a one standard deviation increase in the expellee share on local tax rates using the DiD model in Equation (1). Estimates are based on the most comprehensive specification that includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.1 for details). Standard errors are clustered at the county level.

This persistence may appear surprising as it cannot be reconciled with standard theories of tax competition (e.g. Wilson, 1986, Zodrow and Mieszkowski, 1986). These theories predict that cities undercut each other's tax rates to attract businesses, such that in equilibrium all cities have the same tax rates. If this was true, we would expect that tax rates in high-inflow cities, after getting into an initial disequilibrium, would swiftly revert back to those of low-inflow cities. A potential explanation for the observed persistence is differences in agglomeration rents. As highlighted by the literature on economic geography (Andersson and Forslid, 2003, Baldwin and Krugman, 2004, Luthi and Schmidheiny, 2014), cities that offer higher agglomeration rents can afford to tax firms more. In this case spatial equilibrium with diverging tax rates is sustainable because firms moving to places with lower taxes would lose their agglomeration rents. In the context of the expellee inflow, this explanation appears plausible. Peters (2017) finds that cities with high initial migrant inflows grew faster over the 1950s and 1960s, which gives rise to higher agglomeration rents.

Panels (A)–(D) further corroborate the causal interpretation of the estimates, as trends in tax rates before the inflow are close to zero and statistically insignificant.[19] The parallel pre-trends lend support to the validity of our identifying assumption and indicate the absence of systematic sorting of expellees into cities with divergent levels or trends in tax rates.

The estimates $\widehat{\delta_t}$ for all years after 1944 represent reduced-form coefficients that describe the *total*

[19] The year 1938 is the earliest year for which data on tax rates is available. The most relevant estimates for evaluating the pre-trends are those for 1938 and 1939. For the years 1940 and 1941, the Nazi regime prohibited changes in local tax rates, such that point estimates δ_t for these years are less informative (see Section 2 for details).

effect of the inflow of expellees on tax rates. As such, they summarize a wide variety of causal pathways. For example, changes in voting patterns, internal migration or changes in firms' location decisions, which may all, in turn, affect tax setting. However, because these processes are direct consequences of the inflow, they represent adjustment channels through which the expellee inflows affect tax rates but — importantly — do not confound the estimation of a causal effect.

In a set of robustness checks, shown in Appendix B, we re-estimate the above regressions with different sets of controls. The results, displayed in Tables B.4–B.7), suggest that it is important to control for institutional differences, namely the occupation zone and whether a city was part of Prussia. While without these controls the pre-treatment coefficients for both business taxes are positive statistically significant, they are close to zero and statistically insignificant once we add these controls. The results without controls suggests that areas with different institutional histories differed in their pre-1945 tax rates as well as the number of expellees post-1945, which may also explain why in these regressions we find larger coefficients post-1945. Once the controls are included, the pre-trends are zero and the size of the post-treatment coefficients is smaller. If we add further controls, for example pre-war differences in vote shares or the extent of destroyed housing in the war, the pre-trends remain insignificant and the estimates remain stable. We also test whether the effect is non-linear along the distribution of expellees but find little evidence thereof.

3.2 The Effect of Immigration on Spending and Debt

In addition to changing tax rates, cities could respond to the expellee inflow and its fiscal consequences along two other margins, namely reducing spending on non-welfare related and incurring debt. To analyze these effects, we collected city-level data on spending and debt levels from the Statistical Yearbooks of German Municipalities (see Table A.1 for details). Because these data are only available from 1950 onwards, we apply an instrumental variables (IV) strategy to estimate a causal effect. In the following, we first describe the construction of the instrument and why it yields a sufficiently strong first stage, before dicussing the plausibility of the exclusion restriction in general. Finally, we address commonly known challenges to the validity of the exclusion restriction, by carrying out falsification and plausibility tests.

Instrumental Variables Strategy. The relationship of interest is summarized by the cross-sectional regression

$$y_{mct} = \delta_0 + \delta_1 ExpShare_c + \boldsymbol{X}'_{\boldsymbol{mc}}\boldsymbol{\rho} + \varepsilon_{mct}, \tag{2}$$

where outcome y_{mct} is a function of the share of expellees $ExpShare_c$ and a vector of city- and county-level controls, $\boldsymbol{X_{mc}}$.

The challenge to identification is that the share of expellees is potentially correlated with unobservable city characteristics that determine local spending and debt. To estimate a causal effect, we require an instrument that determines the size of the expellee inflow in West German cities while being uncorrelated with local conditions after 1944.

We instrument for the share of expellees in a West German city with the interaction of two gravity

forces, namely "push" factors in the sending regions that were only relevant before World War II as well as the geographic proximity of the sending regions to a given city in the West.[20] The push factor is the number of Germans living in each sending county i in the ceded Eastern territories and Sudeten in 1939, which provides a prediction for the population outflow after 1944. While the pre-war population is not a push factor for migration in the same sense as local economic conditions or extreme weather events would be (Boustan et al., 2010, Boustan, 2010), in our case the "push" was triggered by the expulsions, during which the entire German population was forced to move westwards. Because urban counties such as Breslau or Königsberg had much larger populations than rural counties, there is significant geographic variation in predicted outflows.

In a second step, we assign the predicted outflows from the source counties in the East to the destination cities in the West based on Euclidean distance between source-destination pairs. It is generally established that distance is a main determinant of migration, and this also holds true for the flow of expellees. For those expellees who made their own way to West Germany, it was less costly to move initially to a place closer to the border. Likewise, for the Allied forces who temporarily hosted expellees in refugee camps along to the border, it was less costly to assign the expellees to places in closer proximity.

An example helps to illustrate the variation in the expellee inflow that is predicted by the instrument. From Breslau — with 600,000 Germans one of the largest city in the Eastern territories in 1939 — the shortest distance to the West German border is around 400km. Our instrument predicts that more people from Breslau settle in Frankfurt (486km from Breslau) than in Cologne (700km from Breslau).

Formally, we construct the instrument for receiving city c by multiplying the population share of a sending county i among the entire population of the Eastern territories and Sudeten[21] (first term in the equation below) with the distance between sending county i and receiving city c,

$$\Delta Pop_c = \sum_i \left(\frac{Pop_i^{1939}}{\sum_i Pop_i^{1939}} \right) \times dist_{ic}. \tag{3}$$

By taking the sum over all sending counties i, we obtain a prediction of the total inflow into each Western city c. The first-stage relationship between the predicted population change and the share of expellees is given by

$$ExpShare_c = \delta_0 + \delta_1 \Delta Pop_c + \boldsymbol{X'_{mc}}\gamma + \eta_{mc}. \tag{4}$$

As shown in Figure A.2 in Appendix A, there is a strong negative correlation between both variables, which prevails when the full set of controls $\boldsymbol{X_{mc}}$ is added. The F-statistic of the instrument in the first stage, depending on the sample and the set of controls, ranges between $F = 35$ and $F = 80$, indicating that the instrument is a sufficiently strong predictor for the share of expellees.

Identification. The validity of our IV approach hinges on the assumption that the instrument has no direct effect on the outcomes of interest. For the exclusion restriction to hold, the predicted

[20] This IV strategy of interacting push factors in the sending regions with geographic proximity to the destination bears resemblance with Boustan et al. (2010), Boustan (2010) and Llull (2017).

[21] County-level information on pre-war population is taken from the census in 1939, see Appendix Table A.1.

population change in West German cities based on the two gravity forces may only affect our outcomes through the inflow of expellees but no other channel. Put differently, we need to assume that the spatial distribution of Germans in the ceded territories *before* World War II is orthogonal to local economic conditions in West German cities after the war.

While not testable, the particular institutional set-up and the decisions made by the Allied Forces in the aftermath of WW II lend support to this assumption. Before the war, the Western and Eastern parts of the German Reich held important economic ties through trade, internal migration, or knowledge flows. Most pre-existing linkages were eliminated when the Eastern territories were ceded to Poland and the Soviet Union, and the Iron Curtain separated Western Europe from the Soviet Bloc. Therefore, the gravity forces that affected the flow of expellees and may have shaped economic development before the war were no longer at play thereafter.

Nonetheless, we acknowledge that the 40-year-long division of the remaining German territory into West and East Germany may threaten the validity of the instrument. The foundation of the GDR on the territory of the Soviet occupation zone and the subsequent isolation of the Soviet Bloc particularly affected West German cities close to the Iron Curtain that lost market access and trading partners within close proximity. Redding and Sturm (2008) show that this economic remoteness considerably slowed the growth of cities close to the inner-German border. Their estimated effect is non-linear and mainly concentrated within a 75km-corridor along the inner-German border. Such differential economic trajectories could invalidate our exclusion restriction if cities closer to the inner-German border set systematically different public policies independent of the fact that they received a higher share of expellees. To alleviate this concern, we control for an indicator that equals unity if a city is located closer than 75km to the Iron Curtain.

In the analysis to follow, we interpret our IV estimates as causal under the maintained assumption that, conditional on these controls, the exclusion restriction $cov(\varepsilon_{mct}, \Delta Pop_c | \boldsymbol{X_{mc}}) = 0$ holds and the instrument is valid. Below, we carry out two plausibility tests. First, further in this section, we compare the IV estimates for local tax rates to the DiD estimates. The difference between both is informative about the validity of the instrument given that the DiD controls for many omitted variables that could confound the IV estimates. Second, in the Appendix, we apply the method of Conley et al. (2012) and assess the extent to which the causal inference is robust to violations of the exclusion restriction. We also assess the quality of our through non-parametric permutation tests.

Effects on Tax Rates Revisited: Testing the Plausibility of the Exclusion Restriction. The fact that tax rates are available for the pre-treatment period provides us with the opportunity to corroborate the instrument validity by comparing the DiD to the IV estimates. Because the DiD approach in Section 3.1 includes city fixed effects and controls for differential time trends by interacting control variables with time dummies, it controls for many variables that could violate the exclusion restriction in the IV approach. If our cross-sectional IV estimates turn out to be similar to the (panel-)DiD estimates, this would support our maintained assumption that the instrument is uncorrelated with the error term.

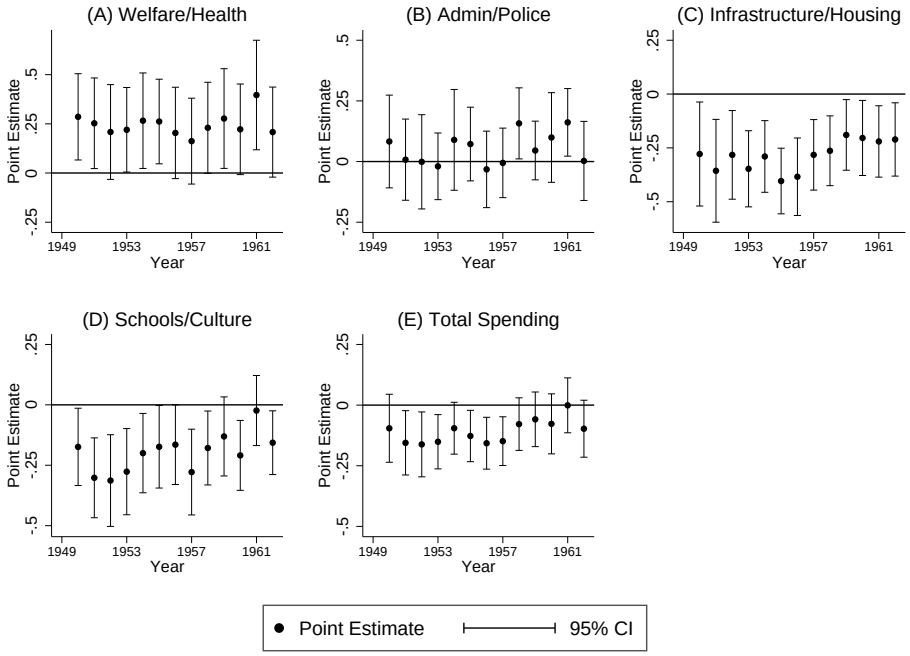
To make the IV estimates comparable to the DiD results, we use as outcomes the differences in tax rates between year t and our base year 1944, i.e., $y_{mct} = tax_{mct} - tax_{mc1944}$. We then estimate our

IV model as displayed in Equation (2) separately for every year $t = [1938, \ldots, 1965]$, using the same control variables as in the DiD regressions in Section 3.1. As before, we cluster the standard errors at the county level.

The results, displayed in Figure B.5 in the appendix, confirm that the IV and DiD estimates are indeed very similar. We find no significant effect of the expellee share on tax rates before 1944, while the effects after 1944 are similar in magnitude and persistence. While this comparison cannot prove the validity of the instrument, it provides strong support in favor of it.

Effects on Spending. We now apply the IV approach to estimate the impact of the expellee inflow on public spending. We collected panel data on cities' expenditures from the statistical yearbook of German municipalities for the period 1950-1962. In the regressions, we use as outcomes the inflation-adjusted log per-capita spending in four broad categories, namely (i) social welfare and health, (ii) public administration and the police, (iii) infrastructure and housing, and (iv) schools and culture.[22] For each year $t = [1950..1962]$, we run a separate IV regression using the same controls as in the DiD approach.

Figure 4: The Effect of Mass Migration on Local Spending - IV Estimates



Notes: This graph displays the point estimates and the 95%-confidence intervals for the effect of a one standard deviation increase in the expellee share on local per-capita spending (in logs) using the IV strategy laid out in Equations (2)–(4). Each point represents the result of a separate regression. Estimates are based on the most comprehensive IV specification that includes measures of institutional differences and pre-WW II and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level.

The results are displayed in Figure 4. In line with the higher welfare dependence of the expellees,

[22] The statistical yearbooks provide information on more fine-grained levels of spending. However, the exact definitions of the spending categories differ from year to year, which which why we aggregated them to larger categories. The analysis is limited to the period from 1950 to 1962 due to large structural changes in fiscal laws that particularly affected cities' local spending.

Panel (A) of Figure 4 shows that cities with a higher expellee inflow significantly raised their per-capita spending on social welfare. For a one-standard-deviation increase in the share of expellees, per-capita welfare spending increased by around 22% (or 10 DM). This increase remains persistent until the end of our sample period in 1962. Upon first glance, the persistence of the effect over time appears surprising in light of Germany's substantial period of economic growth during the 1950s, with per-capita GDP doubling within a decade (Eichengreen and Ritschl, 2009). However, as shown by Bauer et al. (2013), even in the 1970s the expellees were lagging behind the incumbent population in terms of labor force participation, employment, income and homeownership. Therefore, it is plausible that their welfare dependence remained high until at least the 1960s.

Panels (B)–(E) of Figure 4 show the corresponding effects for the remaining spending categories as well as total spending per capita. The results point to substantial shifts in spending. High-inflow cities reduced spending on local infrastructure and housing as well as schools and culture, although, over the course of the 1950s, these differences slowly fade out. In terms of magnitude, the observed shifts in spending are substantial. A one-standard-deviation increase in the share of expellees reduced per-capita spending on infrastructure and housing by around 33% and spending on schools and culture by 24%. Moreover, Panel (E) shows that overall per-capita spending on local amenities was significantly lower in cities with larger inflows.

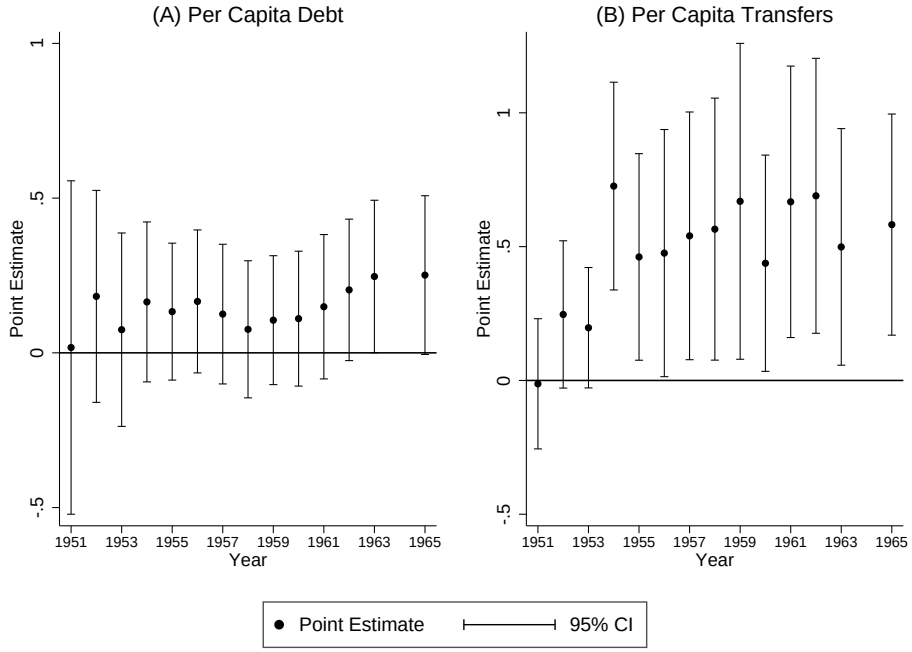
The Effect on Debt. An additional margin of fiscal adjustment is incurring debt. To quantify the importance of this margin, we collected data on city-level per-capita debt for the period 1951-1965 from the Statistical Yearbook of German Municipalities. Figure 5 displays the results based on the same regression model as before. The outcome variable is log per-capita debt.

In contrast to the strong and precisely estimated effects on spending, the evidence on debt is weaker. Most point estimates point to positive effects of the expellee inflow on debt. From 1952 onwards, for a one-standard deviation increase in the share of expellees, debt per capita increased between 7.5% and 25%. However, given the low precision in the estimates, we cannot rule out that the effects are zero.

Robustness and Plausibility Checks. In Appendices B to D, we carry out a series of checks to assess the robustness of our estimates and inference, as well as the plausibility of the instrumental variable strategy. For all IV results presented in figures in this section, we report the corresponding OLS results for comparison. In addition, we assess the sensitivity of the OLS and IV coefficients to the inclusion of various sets of controls. The IV coefficients are generally larger than the OLS coefficients, although — some few exceptions aside — the difference is fairly small. The difference between both coefficients may either be explained by endogeneity in the share of expellees in a city, or a discrepancy between the average treatment effect identified by OLS and the local average treatment effect identified by the IV, or both.

We also assess the robustness of our inference using non-parametric permutation tests. These tests allow us to relax two important assumptions, namely that the error terms are normally distributed in the population and that there is no systematic correlation in the error terms between counties. The results strongly confirm the significance levels found with parametric standard errors. For the

Figure 5: The Effect of Mass Migration on Local Debt and Transfers - IV Estimates



Notes: This graph displays the point estimates and the corresponding 95%-confidence intervals for the effect of a one standard deviation increase in the expellee share on local debt per capita (in logs) using the IV strategy laid out in Equations (2)–(4). Each point represents the result of a separate regression. Estimates are based on the most comprehensive IV specification that includes measures of institutional differences and pre-WW II and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level.

effect on the agricultural land tax, for example, the p-value is close to zero, indicating a high level of statistical significance of the estimate, whereas for the effect on the residential property tax, with an empirical p-value of $p = 0.44$, we fail to reject the null hypothesis of no effect at any reasonable significance level.

Finally, using the method by Conley et al. (2012), we assess the robustness of the causal inference to violations of the exclusion restriction of the IV. The idea behind this method is that while the exclusion restriction $cov(\varepsilon_{mct}, \Delta Pop_c | \boldsymbol{X_{mc}}) = 0$ may not hold exactly, the estimates may still have a causal interpretation if this correlation is small. In Appendix D, we perform a plausibility test by calculating the maximum correlation for which the IV coefficient would be statistically significant at the 10%-level. The results suggest that our causal inference is highly robust to violations of the exclusion restriction. The correlation between the regressor and the error term would have to be between 30% and 55% of the IV estimate to render the estimate statistically insignificant, and in most cases the correlation would have to go in the opposite direction than the IV estimate. In combination with the comparison of the IV and the DiD, we view these results as strong evidence in favor of our IV strategy and and its ability to deliver causal estimates.

4 Political Economy: Voting as a Potential Mechanism

The results of the previous section show that the vast inflow of forced migrants into West Germany had a significant impact on public policy setting. Cities with high inflows adopted more redistributive policies; they selectively raised taxes, spent more on social benefits, and reduced spending on other items. In this section, we explore to what extent these effects can be explained by changes in the local political economy.

Theoretical Considerations. Theories in political economy, for example Razin et al. (2002) and Alesina and Glaeser (2004), mainly deliver predictions about the size of the welfare state in a world where migrants *cannot* vote. These models predict that the inflow of low-skilled immigrants should lead to higher votes for parties that favor less redistribution and should ultimately lead to lower taxes and spending. This is the case because part of the taxes paid by the native population would go towards public amenities that mainly benefit the migrants. Recent work by Tabellini (2017) provides evidence for this mechanism based on the mass immigration to the US in the early 20th century, showing that counties with higher inflows of migrants voted more conservative, and reduced taxes and spending.[23]

A crucial difference between the case of the German expellees and most international migrant flows is that the expellees had voting rights immediately upon arrival and, therefore, could directly influence politics. Moreover, they accounted for around 20% of the population, such that political parties could not afford to ignore them. Given the expellees' low levels of wealth and their disadvantages in the labor market, their inflow was both a shock to population levels in cities as well as to wealth and income inequality. Cities with many expellees — all else equal — had a larger share of poor people than they would have had without the inflow. Political economy theory, most notably Meltzer and Richard (1981), predicts that areas with greater inequality vote more in favor of redistribution and implement more redistributive policies. As a society becomes more unequal, the median voter shifts towards poorer segments of the population that would benefit from higher spending and hence rationally votes for higher taxation. While this model is not fully congruent with our historical setting — the model considers a linear income tax rather than a tax on assets or profits, and the size of the voter base is assumed to be fixed — its basic mechanics can guide our analysis. In our context, a more pronounced inflow of expellees should result in higher vote shares for parties that support higher local taxes and more public spending.

However, given the political system in Germany, these theories hold no clear predictions about voting for specific parties. Among the two major German parties, the *Social Democratic Party* (SPD) has traditionally supported redistribution while the *Christian Democratic Union of Germany* (CDU/CSU) has generally pursued more business-friendly policies, although the parties' positions on the size of the welfare state have not been as divergent as for example in the US or the UK. Moreover, in line with Germanyâ€™s federal political culture, both parties tolerate that local policies diverge from the

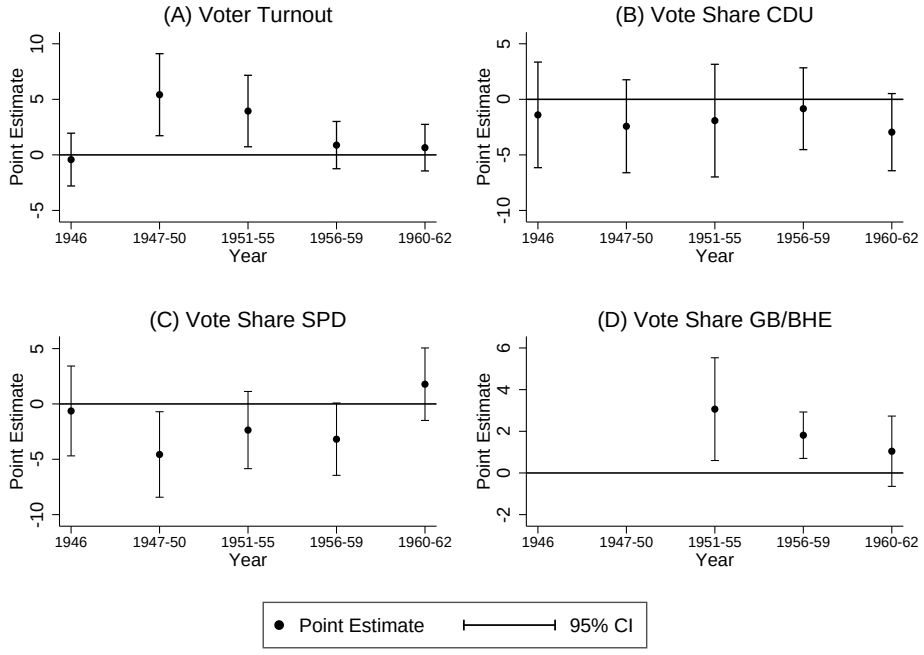
[23] Similar evidence on immigration and voting is provided by Mayda et al. (2018), who show that low-skilled immigration in the late 20th century increased Republican vote shares in the US, Dustmann et al. (forthcoming), who show that refugee inflows increase right-wing voting in Denmark. In contrast, Facchini et al. (2018) find no effect of immigration on right-wing voting in Sweden, whereas Steinmayr (2016) finds mixed results for Austria — negative effects when refugees are hosted locally, and positive effects when larger numbers of refugees are passing through a municipality.

party line.

A direct manifestation of the expellees' political influence may be captured by the local vote share for the GB/BHE[24], which was founded in 1950 and explicitly represented the expellees' interests. The party followed two main goals, namely to improve the economic situation of the expellees in West Germany, and to lobby for a return of the expellees' properties in the ceded Eastern territories. The GB/BHE was part of the federal government between 1953 and 1955, and of several state governments between 1950 and 1966.

Impact on Voter Turnout and Vote Shares. To test whether the inflow affected voting at the local level, we collected data on voter turnout and party vote shares for city elections from the Statistical Yearbook of German Municipalities for the period of 1946 to 1962.[25] Local elections — as opposed to state and federal ones — are the relevant elections in our context, because local taxes and spending are decided by municipal and city councils, which are elected every four to five years. Due to the federal structure of Germany, elections took place at different points in time across the country, which is why we divide the elections into five cycles (1946, 1947-50, 1951-55, 1956-59, and 1960-1962). Within each cycle, the majority of municipalities held an election.

Figure 6: The Effect of Mass Migration on Voter Turnout & Vote Shares - IV Estimates



Notes: This graph shows the point estimates and 95%-confidence intervals of the effect of a one standard deviation increase in the expellee share on local voter turnout and party vote shares (in %) using the IV strategy laid out in Equations (3)–(4). Estimates are based on the most comprehensive specification that includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level.

Figure 6 reports the estimates from our preferred specification of the IV model (for more details see

[24] Gesamtdeutscher Block/Bund der Heimatvertriebenen und Entrechteten.
[25] See Appendix Table A.1 for details and Figure A.4 for the evolution of voting outcomes over time.

Appendix Table B.23). We first consider voter turnout, which provides an indication of the political stakes in an election. Standard voting models predict a decline in voter turnout when the voter base becomes larger, because each vote is less likely to be decisive (Downs, 1957). In contrast, voter turnout may increase if a larger voter base increases the stakes in the election (Andersen et al., 2014), for example, if rich voters support low taxes while poor voters demand more redistribution. Our IV results, displayed in Panel A of Figure 6, show that in the early 1950s a larger inflow of expellees led to higher voter turnout in local elections. In elections held between 1947 and 1955, a one-standard deviation increase in the share of expellees increased voter turnout by around 4–5.4 pp. This is a substantial effect given the mean voter turnout of around 74% and points to high stakes in the elections for both groups. At the same time, this result appears plausible in light of the economic differences between expellees and natives, and points to high stakes in the elections for both groups. For example, there is abundant evidence that the West German population considered the expellees as their competitors for scarce resources and initially did not accept them as fellow citizens (Kossert, 2008).

In Panels (B) and (C), we investigate the impact of the expellee inflow on the vote shares of the two major parties in post-war West Germany, namely the Conservatives (CDU/CSU) and Social Democrats (SPD). During the time of our analysis, both parties jointly achieved an average vote share of 72.5% in municipal elections. Our estimates suggest that the inflow of expellees had little effect on the conservatives' vote share, whereas the vote share for the social democrats slightly declined in areas with a large inflow of migrants. While this finding may appear surprising, it should be noted that our estimates only capture the extensive margin of local politics. It is well possible that both parties responded to the mass inflow of potential voters by changing their party program along the expellees' needs. Historical accounts indeed suggest that the CDU and SPD explicitly competed for the votes of the expellees; in particular by promising a fast improvement of their economic situation in West Germany as well as advocating for the possibility to reclaim their lost properties in the ceded territories (Kossert, 2008, pp. 165). Therefore, the expellees may influenced local politics even without significantly affecting the local vote shares for the major parties.

In contrast, the results in Panel (D) indicate that the expellees used their voting rights to influence politics. As shown earlier in Figure 2, a higher expellee share strongly increased the vote share of the GB/BHE, in particular during the 1950s. A one-standard deviation increase in the expellee share raised the party's vote share by around 2pp, equivalent to 27 percent of the mean of 7.2% (see Appendix Tables B.24). This finding provides evidence that the expellees influenced the political process in local elections, which may be one of the explanations for the implementation of more redistributive policies in cities with high inflows.

Robustness Checks. In Appendix B, we report OLS and IV results for all regressions displayed in Figure 6 and assess the robustness of our estimates and inference in a series of sensitivity checks. While most OLS results are small and statistically insignificant, the IV coefficients are large and statistically significant, which indicates considerable selection of expellees into areas with certain voting patterns.

The permutation tests, displayed in Appendix C, confirm the statistical significance of most effects,

with the exception of the effect on mean post-war voter turnout over the entire sampling period, for which the empirical p-value points to a statistically insignificant effect ($p = 0.16$). However, as shown in Panel (A) of Figure 6, the effect on turnout was strongly positive in the early 1950s and reverted to zero after 1955, which can explain why the average effect from 1947 to 1962 is statistically insignificant.

We also assess the robustness of the causal inference to violations of the exclusion restriction (**?**). The effect on vote shares for the GB/BHE proves highly robust. The causal interpretation would hold up to a correlation between the error term and the instrument amounting to 52% of the original IV estimate. For mean voter turnout over the period 1947 to 1962, the robustness is weaker, but the causal interpretation would still permit a correlation of 9% of the IV estimate. We view this as strong evidence of a causal effect even if one doubts that the exclusion restriction exactly holds.

5 Long-Run Effects: Preferences for Redistribution today

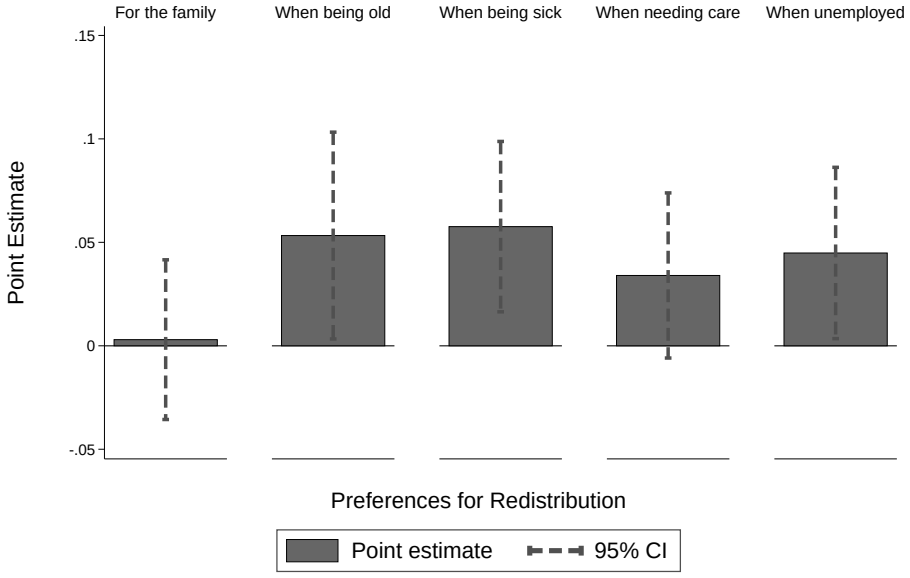
So far, our analysis documents a short- to medium-run effect of the expellee inflow on redistribution. Cities with high inflows almost immediately implemented more redistributive tax and spending policies, and these changes were persistent until at least the mid-1960s. In this section, we turn to the long-run effects and investigate whether the impact of the expellee inflow persists over several decades.[26] Based on survey data from the early 2000s, we study to what extent people living in cities that experienced high inflows in the 1940s differ in their preferences for redistribution from people living in low-inflow cities.

For this purpose, we link rich individual-level survey data from the German Socio-Economic Panel (see Wagner et al. (2007) and Appendix Tables A.3 for more details) to the inflow of forced migrants based on the respondents' current county of residence.[27] Because we are interested in the impact of the expellee inflow on the non-expellee population, we restrict the sample to individuals born after the arrival of the expellees (i.e., after 1949). To measure preferences for redistribution, we follow Alesina and Fuchs-Schündeln (2007) and use the two waves of 1997 and 2002 that include questions about the respondents' preferred role of the state in different domains of social security, namely financial protection (i) for the family, (ii) when being old, (iii) when needing care, (iv) when being sick, and (v) when being unemployed. As described in detail in Table A.1, response options were provided on a five point scale, with higher values indicating a preference for a stronger role of the state in these matters (responsibility should rest "only [with] the state", "mostly [with] the state"). Low values, in contrast, indicate a preference for people being individually responsible for financial protection ("mostly [by] private forces", "only [by] private forces"), while individuals can also prefer shared responsibilities in these matters ("state and private forces"). For each domain, our outcome is a binary indicator that equals one if a person answers that the responsibility should rest only or mostly with the state, and zero otherwise.

[26] One way to study long-run effects would be to look at taxation and spending in the same cities over an even longer period than we do in this paper. However, several territorial reforms in the 1960s and 1970s prevent us from doing so in a meaningful way. In these reforms, many municipalities that were previously cities in their own right became part of larger adjacent cities, making it difficult to link the data over time.

[27] We assign treatment at the county level because this is the most disaggregated geographic unit we can analyze by remote computing.

Figure 7: The Effects of Mass Migration on Preferences for Redistribution - IV Estimates



Notes: This graph shows the estimates and 95%-confidence intervals for the effect of a one standard deviation increase in the expellee share on individuals' preferences for redistribution. The outcome is a binary indicator that equals one if a person sees the responsibility in a given domain with the state. We the IV model laid out in Equations (2)–(4). The set of controls comprises (i) respondents' characteristics, and (ii) historical controls (see Section 3.2 for details) to capture persistent differences across regions. Cross-sectional weights are used. Standard errors are clustered at the county level.

Applying our instrumental variables strategy with the same set of county-level controls as in the previous analysis, we find that preferences for redistribution in the early 2000s are substantially higher in counties that experienced a larger inflow of expellees in the 1940s. When it comes to financial security in case of unemployment, sickness, need for care or when being old, individuals in high-inflow counties prefer a more active role of the state. Figure 7 indicates that one standard deviation increase in the share of expellees raises individuals' support for the welfare state by 3.5–5.1 percentage points, or 4.5-7.5% relative to the variables' mean (for further details see Appendix Tables A.3 and B.25).

To exclude that these effects merely reflect income differences across counties, we control for a person's individual labor income, current county-level employment rates as well as the current share of foreigners in an additional specification. The results, displayed in column (6) of Table B.25, remain unchanged when we add these controls. However, given that these variables may have been influenced by the expellees themselves and, therefore, may be considered as "bad controls", our preferred specification is one without these variables.

These results suggest that the sudden arrival of eight million expellees was a large enough shock to persistently change the preferences of society. There are several plausible explanations for this long-run effect. One is intergenerational transmission of preference. People who lived in high-inflow cities in the 1940s experienced an increase in the size of the welfare state while at the same time being confronted with the greater poverty the expellees. These experiences may have shaped the local

narrative about poverty and redistribution, and may have been passed on to the next generations.[28] Another potential explanation is endogenous sorting based on preferences (Tiebout, 1956). The inflow of expellees triggered to changes in public policies, which may have led to subsequent inflows of individuals with greater preferences for redistribution. While our data do not allow us to disentangle these channels, the overall result suggests that the inflow of expellees is partly responsible for the significant differences in preferences for redistribution and welfare cultures across West German cities.

6 Conclusion

In this paper we show that immigration can have profound effects on the size of the welfare state. Using the arrival of eight million forced migrants in post-war West Germany as a natural experiment, we document that cities with high inflows of migrants selectively raised taxes and spent more on social welfare, while spending less on infrastructure and housing, and these policy changes were persistent until at least the mid-1960s. Moreover, we show that impact of the expellee inflow had a lasting effect on preferences for redistribution. People who live today in cities that received more expellees almost 70 years ago show considerably stronger support for a larger welfare state.

Upon first glance, the policy responses may appear mechanical. The sudden inflow of poor people meant that cities were forced to spend more on social welfare, which they had to finance by raising taxes, shifting spending and incurring debt. But what is more interesting is *which* taxes and spending items were raised and which were left unchanged or lowered. Cities had significant degrees of freedom in these choices, and yet, a consistent pattern emerges. Groups that were fairly immobile in the 1940s, namely farmers and business owners, were taxed more on their assets, while for items that mainly benefited poorer segments of society, namely houses and jobs, taxes remained unchanged. On the spending side, the greater welfare spending mainly benefited the migrants as well as poorer natives.

The observed policy changes point to the importance of immigrantsâ€™ voting rights. Unlike most international migrants, the expellees had voting rights from the time of arrival and, therefore, politicians could not ignore them in their policy setting. And while upon first glance our historical setting may appear peculiar, we believe that it can illuminate two important aspects of migration today. First, many international migrants have been residing and paying taxes in receiving countries for a very long time, and there are ongoing debates about granting them voting rights. Intra-EU migrants, for example, are currently allowed to vote in local elections in their country of residence, and with greater European integration they may receive full voting rights in the future. Second, the largest global migration flows happen within rather than across countries. In the past three decades low- and middle income countries have seen vast rural-to-urban migration, and this trend continues. Our results suggest that if migrants have the same voting rights as the incumbent population, this may ensue political changes in the receiving areas.

[28] For example, Dohmen et al. (2012) show that attitudes (in their case the willingness to take risk and trust others) are transferred from parents but also the broader local environment to the next generation.

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Appendix

A Data Appendix and Descriptive Statistics

Table A.1: Variables and Data Sources

Variable	Years	Source
Panel A – Expellee Data		
Expellee Share	1950	Information on the county-level expellee share as of 1950 is taken from the "Statistisches Taschenbuch über die Heimatvertriebenen", published by the Federal Statistical Office of West Germany in 1953.
Panel B – City-Level Outcomes		
Debt	1951-1965	Information on cities' debt are taken from the "Statistical Yearbooks of German Municipalities". For every year, debt is reported for cities with more than 20,000 inhabitants.
Tax Rates	1938-1965	Information on city-level tax rates are taken from the "Statistical Yearbooks of German Municipalities". In every year, tax rates for all cities with more than 10,000 inhabitants are reported. The agricultural land and residential property taxes (*Grundsteuer A / Grundsteuer B*) are levied on the value of (agricultural) land and structures. The value of the land (the tax base) is uniformly determined at the state level and reassessed every three years. It is multiplied by a municipality-specific tax rate that comprises the uniform basic rate, which is set by the federal government, and the tax collection rate defined by each municipality on an annual basis. The same logic applies to the tax rates on firms' business profits (*Gewerbeertragssteuer*), capital (*Gewerbekapitalsteuer*), and overall wage bill (*Lohnsummensteuer*).
Spending	1950-1962	Annual expenses at the city level are taken from the "Statistical Yearbooks of German Municipalities". We focus on four types of local spending that cover all local expenses: spending for (i) welfare and health, (ii) the administration and the police, (iii) public infrastructure and housing, and (iv) schools, sports and culture. The definition of these groups follows the general presentation in the "Statistical Yearbooks of German Municipalities". As the detail degree on spending items varies over time, we harmonized spending groups accordingly. Information on spending is given for cities with at least 20,000 inhabitants in a given year.
Unemployment Rates		Data on unemployment at the city level is taken from the "Statistical Yearbooks of German Municipalities". Information is given for all cities with more than 20,000 inhabitants in a given year.

continued

Table A.1 continued

Variable	Years	Source
Voting results	1946-1962	Data on voter turnout and party vote shares at the local elections between 1946 and 1962 are taken from the "Statistical Yearbooks of German Municipalities". On average, each municipality held three elections during the sampling period. Variables of interest comprise (i) the overall voter turnout, (ii) the vote share for the Christian Democrats (CDU), (iii) the votte share for the Social Democrats (SPD), and (iv) the vote share for the expellee party (GB/BHE). All West German cities with more than 20,000 inhabitants are covered.
Panel C – City- and County-Level Controls		
Border Region Dummy		In spirit of Redding and Sturm (2008), we create a dummy variable that assigns the value of one to all counties that were less than 75 kilometers away from the inner German border. A county's crow fly distance is used.
Occupation Zone Dummies		We assign each county to the respective occupation zone administrated by the US, UK or French forces, respectively.
Pre-War Population Density	1939	Information on pre-war population density is taken from the "Statistical Yearbook of the German Reich (1939)".
Prussia Dummy		We create a dummy variable that indicates whether a county was part of Prussia during the times of the Weimar Republic.
Historical Economic and Political Differences	1925-1939	We account for historical economic and political differences by controlling for (i) the (log) population density in 1939, (ii) the share of civil servants and unemployed workers in 1933, (iii) the population share of Protestants as of 1925, and (iv) the mean election vote share for the Social Democratic Party (SPD) in the elections between 1925 to 1933. All data are taken from King et al. (2008).

continued

Table A.1 continued

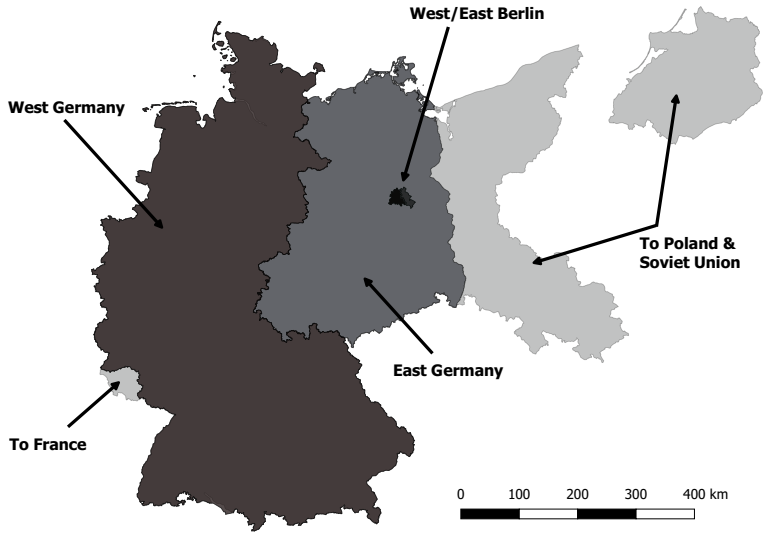
Variable	Years	Source
Housing destruction	1945-1950	Information on the extent of destroyed housing at the county-level has been collected from the Federal Statistical Offices of the German states (*Landesämter für Statistik*). The corresponding sources are: • *Statistik von Baden Württemberg - Band 6. Ergebnisse der Gebäude- und Wohnzählung vom 13. September 1950. Tabellenband II. Statistisches Landesamt Baden-Württemberg. Stuttgart 1953.* • *Statistisches Landesamt Schleswig-Holstein. Statistisches Handbuch für Schleswig-Holstein. Kiel 1951.* • *Niedersächsisches Amt für Landesplanung und Statistik. Zählung der Bevölkerung, Gebäude, Wohnungen und nichtlandwirtschaftlichen Arbeitsstätten. Gebäude- und Wohnungszählung in Niedersachsen 1950. B. Tabellenteil. Hannover 1952.* • *Statistisches Landesamt der Hansestadt Hamburg. Hamburg in Zahlen. Nr. 13, Jahrgang 1948. Hamburg 1948.* • *Statistisches Landesamt Bremen. Statistische Mitteilungen aus Bremen. Die Wohnungszählung am 13.09.1950 im Lande Bremen. Bremen o.J.* • *Wirtschaftsministerium des Landes Nordrhein-Westfalen. Wirtschaftsbeobachtung und Statistik. Nordrhein-Westfalen in Zahlen. O.O 1948.* • *Badisches Statistisches Landesamt. Statistische Zahlen aus Nordbaden. Kurzbericht Nr. 9. Allgemeine Wirtschaftsstatistik. Karlsruhe 1947.* item*Statistisches Handbuch für das Land Hessen. Kriegsschäden an Wohnungen. Wiesbaden 1948* • *Statistisches Landesamt Rheinland-Pfalz. Volkszählung am 13. September 1950. Die Wohnungszählung in Rheinland-Pfalz. Bad-Ems 1952.* • *Bayerisches Statistisches Landesamt. Mitteilungen des Bayerischen Statistischen Landesamtes. Heft 5, München 1945.*
Panel D – SOEP data		
Controls	1997,2002	At the individual level, the set of controls comprises the respondents' age (squared and cubed), gender, educational and marital status, household size, federal state of residence and (log) household income. We further control for the county-level employment rate and share of foreigners among the population. All variables are provided by SOEPRemote.

continued

Table A.1 continued

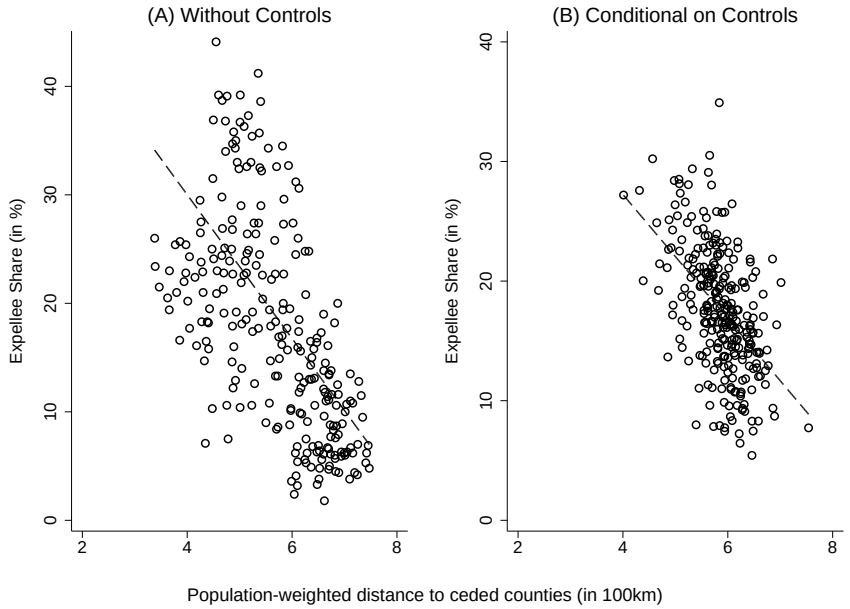
Variable	Years	Source
Individual Preferences for Redistribution	1997,2002	Respondents are asked about their preferred role of the state with regard to different areas of social security. The question reads as follows: "At present, a multitude of social services are provided not only by the state but also by private free market enterprises, organizations, associations, or private citizens. What is your opinion on this? Who should be responsible for (i) financial security in case of unemployment, (ii) financial security in case of illness, (iii) financial security of families, (iv) financial security for old-age, (v) financial security for persons needing care." Response options are given on a five point scale, ranging from "only private forces", "mostly private forces', "state and private forces", "mostly the state", to "only the state". Following Alesina and Fuchs-Schündeln (2007), we use binary outcomes that equal unity if the responsibility should rest only or mostly with the state, and zero otherwise.

Figure A.1: German Territory before and after World War II



Notes: This map shows Germany in its pre- and pos- World War II borders. The Saarland was ceded to France but re-joined Germany in 1956. The figure is based on shapefiles provided by Max Planck Institute for Demographic Research (MPIDR) and Chair for Geodesy and Geoinformatics, University of Rostock (CGG) (2011).

Figure A.2: IV First Stage: (Conditional) Correlation



Notes: This graph shows the correlation between our instrument and the expellee share at the county level. Panel (A) displays the raw correlation. In Panel (B), the full set of controls as defined in Section 3 is included.

Table A.2: Descriptive Statistics on City-Level Outcomes and Controls

	Mean	Std Deviation	Minimum	Maximum	Observations
Exepllee Share					
Expellee Share (1950)	0.17	0.09	0.02	0.44	431
Local Tax Rates					
Agricultural Land Tax	1.44	0.47	0.40	3.35	9,772
Residential Property Tax	2.04	0.45	0.48	3.75	9,774
Business Capital Tax	0.53	0.07	0.20	0.77	9,774
Business Wage Bill Tax	1.74	0.45	0.20	4.38	3,773
State Transfers, Debt and Spending					
P.c. Transfers	42.64	33.73	0.00	249.35	3,349
P.c. Debt	203.01	171.44	0.04	1,580.36	3,200
P.c. Expenses	134.60	56.33	34.02	725.36	2,848
Exp. Welfare/Health	30.76	35.38	1.20	462.28	3,188
Exp. Administration/Police	26.99	12.82	-6.57	121.53	2,848
Exp. Infrastructure/Housing	45.98	25.48	-1.10	320.83	2,848
Exp. Schools/Culture	38.23	17.33	4.89	288.73	2,848
Voter Turnout and Party Shares					
Voter Turnout	74.49	7.73	42.50	93.40	997
Vote Share CDU	34.38	11.72	5.00	70.80	941
Vote Share SPD	38.86	9.59	7.40	70.00	997
Vote Share GBBHE	7.10	4.69	0.60	28.70	354
Controls					
Occupation Force USA	0.59	0.49	0.00	1.00	256
Occupation Force UK	0.32	0.47	0.00	1.00	256
Occupation Force France	0.09	0.29	0.00	1.00	256
City in former Prussia	0.56	0.50	0.00	1.00	256
City close to Iron Curtain	0.12	0.33	0.00	1.00	256
Population density (1939)	5.51	1.21	3.63	8.17	256
Vote Share SPD (1924-1933)	0.20	0.10	0.02	0.45	256
Share Protestants (1925)	0.52	0.35	0.01	0.98	256
Share Unemployed (1933)	0.16	0.08	0.03	0.38	256
Share Civil Servants (1933)	0.05	0.03	0.02	0.39	256
Share Destroyed Housing	18.79	17.73	0.00	78.22	256
Instrument					
Distance to East (in 100km)	5.92	0.95	3.38	7.47	431

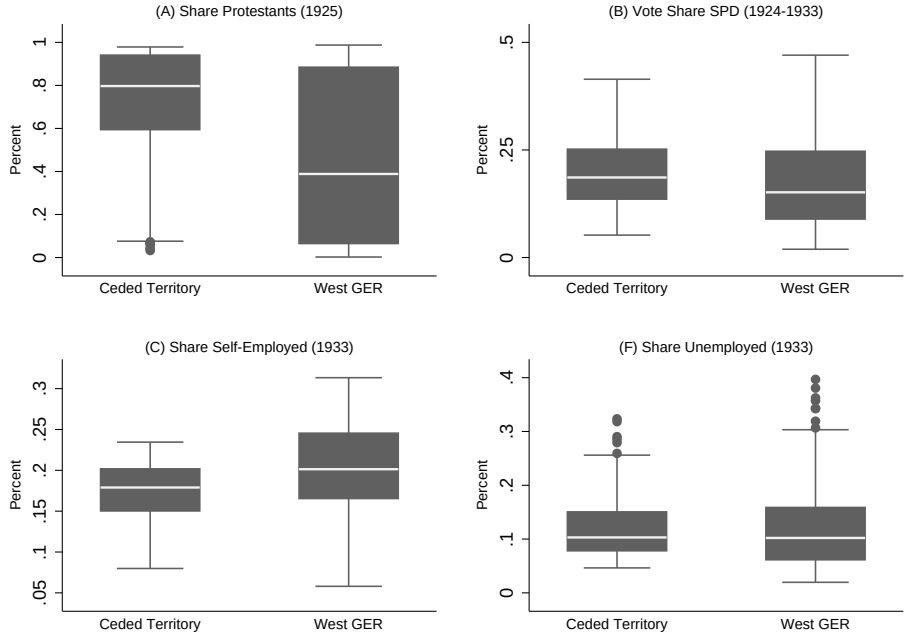
Notes: This table presents descriptive statistics on outcome and control variables at the city and county level, respectively. All monetary variables are adjusted for inflation (as of 1950). For detailed information on the variables and the underlying data sources, see Appendix Table A.1.

Table A.3: Descriptive Statistics - SOEP Sample

	Mean (1)	SD (2)	P25 (3)	P50 (4)	P75 (5)	Min (6)	Max (7)	N (8)
Panel A – Expellee Share								
Expellee Share (1950)	0.15	0.09	0.08	0.13	0.22	0.02	0.44	8,974
Panel B – Dependent Variables								
State's Responsibility When Sick	0.68	0.16	0.60	0.60	0.80	0.20	1.00	8,974
State's Responsibility When Unemployed	0.77	0.16	0.60	0.80	0.80	0.20	1.00	8,974
State's Responsibility When Needing Care	0.70	0.16	0.60	0.60	0.80	0.20	1.00	8,974
State's Responsibility When Old	0.68	0.17	0.60	0.60	0.80	0.20	1.00	8,974
State's Responsibility For Families	0.66	0.17	0.60	0.60	0.80	0.20	1.00	8,974
Panel C – Control Variables								
Age	34.19	9.21	27.00	34.00	41.00	17.00	52.00	8,974
Male	0.48	0.50	0.00	0.00	1.00	0.00	1.00	8,974
Education	2.84	1.52	2.00	3.00	4.00	0.00	6.00	8,974
Marital Status	1.73	0.60	1.00	2.00	2.00	1.00	3.00	8,974
Household Size	3.19	1.21	2.00	3.00	4.00	1.00	5.00	8,974
Federal State	5.75	2.24	5.00	5.00	8.00	1.00	9.00	8,974
(Log) Household Income	7.99	0.56	7.65	8.01	8.35	2.30	10.31	8,974

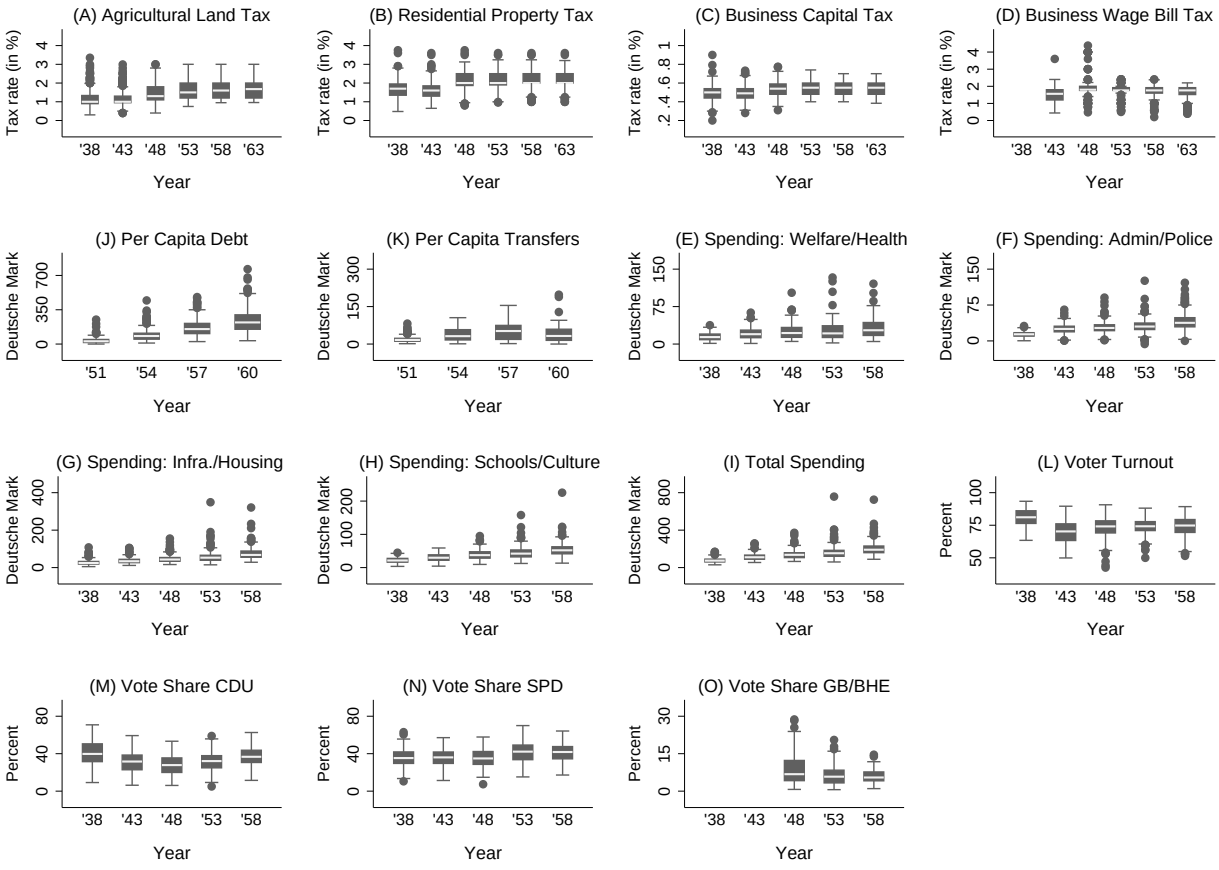
Notes: This table presents descriptive statistics on individual outcome and control variables from the German Socio-Economic Panel. To allow compar sion across samples, the sample-specific expellee share is given. For detailed information on each variable and the underlying data sources, see Appendix Table A.1.

Figure A.3: Differences Between Migrant and Natives – Pre-WW II Variables



Notes: This graph plots differences between the migrant population from the ceded territories in the East and the population in West Germany before WW II. Data are taken from King et al. (2008). See Appendix Tables A.1 and A.2 for further information the variables and additional descriptive statistics.

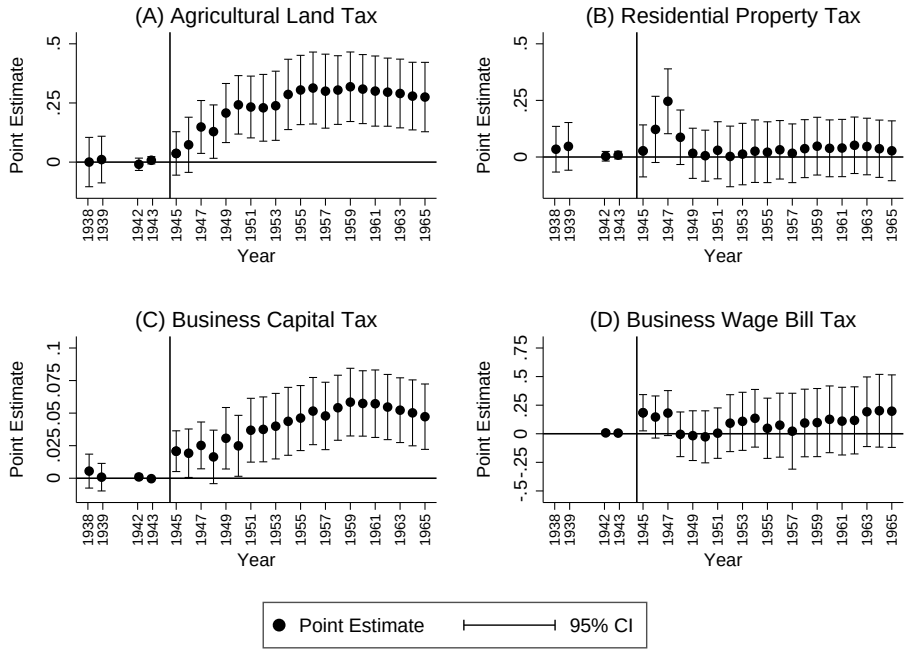
Figure A.4: Evolution of Outcome Variables Over Time



Notes: This graph plots the evolution of our outcome variables over time. See Appendix Tables A.1 and A.2 for information on data sources and definitions as well as additional descriptive statistics.

B Additional Regression Results

Figure B.5: The Effect of Mass Migration on Local Taxation: IV Estimates



Notes: This graph shows the annual effect of a one standard deviation increase in the expellee share on our four local tax rates using the IV strategy laid out in equations (2)–(4). Estimates are based on the most comprehensive specification that includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level.

Table B.4: The Effect of Mass Migration on Agricultural Land Tax Rates - DiD Estimates

	(1)	(2)	(3)	(4)	(5)
Expellees Share × 1938	0.026	0.014	-0.000	0.001	-0.014
	(0.017)	(0.019)	(0.024)	(0.026)	(0.034)
Expellees Share × 1939	0.039**	0.023	-0.013	-0.011	-0.024
	(0.019)	(0.020)	(0.021)	(0.024)	(0.032)
Expellees Share × 1942	0.002	0.003	0.003	0.005	0.003
	(0.004)	(0.003)	(0.004)	(0.004)	(0.006)
Expellees Share × 1943	0.002	0.002	0.000	-0.001	0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
Expellees Share × 1945	-0.006	0.015	0.028	0.006	0.018
	(0.014)	(0.013)	(0.017)	(0.019)	(0.026)
Expellees Share × 1946	-0.018	0.019	0.031	0.009	0.033
	(0.016)	(0.015)	(0.021)	(0.023)	(0.033)
Expellees Share × 1947	0.007	0.041**	0.053**	0.029	0.049
	(0.017)	(0.016)	(0.021)	(0.024)	(0.031)
Expellees Share × 1948	0.040**	0.074***	0.085***	0.078***	0.094***
	(0.020)	(0.017)	(0.022)	(0.028)	(0.034)
Expellees Share × 1949	0.104***	0.137***	0.150***	0.129***	0.128***
	(0.020)	(0.018)	(0.025)	(0.031)	(0.037)
Expellees Share × 1950	0.107***	0.145***	0.160***	0.142***	0.150***
	(0.020)	(0.018)	(0.025)	(0.031)	(0.037)
Expellees Share × 1951	0.105***	0.147***	0.148***	0.122***	0.128***
	(0.022)	(0.019)	(0.026)	(0.034)	(0.040)
Expellees Share × 1952	0.129***	0.172***	0.176***	0.149***	0.165***
	(0.021)	(0.018)	(0.024)	(0.032)	(0.039)
Expellees Share × 1953	0.149***	0.194***	0.202***	0.178***	0.193***
	(0.020)	(0.018)	(0.024)	(0.032)	(0.040)
Expellees Share × 1954	0.162***	0.207***	0.219***	0.197***	0.205***
	(0.020)	(0.018)	(0.025)	(0.033)	(0.041)
Expellees Share × 1955	0.173***	0.220***	0.232***	0.212***	0.224***
	(0.020)	(0.019)	(0.025)	(0.033)	(0.041)
Expellees Share × 1956	0.177***	0.221***	0.231***	0.213***	0.229***
	(0.020)	(0.019)	(0.025)	(0.033)	(0.040)
Expellees Share × 1957	0.184***	0.226***	0.232***	0.212***	0.212***
	(0.019)	(0.019)	(0.025)	(0.032)	(0.040)
Expellees Share × 1958	0.184***	0.231***	0.235***	0.217***	0.219***
	(0.020)	(0.019)	(0.025)	(0.032)	(0.039)
Expellees Share × 1959	0.192***	0.236***	0.243***	0.229***	0.233***
	(0.019)	(0.018)	(0.024)	(0.031)	(0.037)
Expellees Share × 1960	0.194***	0.237***	0.241***	0.225***	0.226***
	(0.019)	(0.018)	(0.024)	(0.032)	(0.039)
Expellees Share × 1961	0.197***	0.239***	0.237***	0.222***	0.216***
	(0.019)	(0.019)	(0.025)	(0.033)	(0.040)
Expellees Share × 1962	0.204***	0.247***	0.242***	0.224***	0.214***
	(0.020)	(0.020)	(0.026)	(0.033)	(0.039)
Expellees Share × 1963	0.211***	0.254***	0.251***	0.235***	0.224***
	(0.020)	(0.019)	(0.025)	(0.033)	(0.039)
Expellees Share × 1964	0.214***	0.257***	0.253***	0.238***	0.228***
	(0.020)	(0.020)	(0.025)	(0.034)	(0.040)
Expellees Share × 1965	0.215***	0.256***	0.252***	0.233***	0.222***
	(0.020)	(0.020)	(0.025)	(0.034)	(0.040)
Year FE	Yes		Yes	Yes	Yes
Year × Region FE		Yes	Yes	Yes	Yes
Geographical Controls			Yes	Yes	Yes
Pre WW-II Controls				Yes	Yes
WW-II Housing Destruction					Yes
Observations	11690	11690	11690	11690	9820
Adjusted R^2	0.483	0.522	0.528	0.531	0.579

Notes: This table shows the effect of a one standard deviation increase in the expellee share on local tax rates using the DiD model laid out in equation (1). All specifications include city-level fixed effects. The varying set of controls comprises measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.1 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5: The Effect of Mass Migration on Residential Property Tax Rates - DiD Estimates

	(1)	(2)	(3)	(4)	(5)
Expellees Share × 1938	-0.008	-0.007	0.011	0.031	0.002
	(0.019)	(0.020)	(0.022)	(0.027)	(0.032)
Expellees Share × 1939	-0.001	-0.003	-0.003	0.018	-0.005
	(0.019)	(0.020)	(0.020)	(0.024)	(0.030)
Expellees Share × 1942	-0.007	-0.006	-0.002	0.003	0.004
	(0.005)	(0.004)	(0.004)	(0.005)	(0.006)
Expellees Share × 1943	-0.002	-0.002	-0.002	-0.004	-0.005
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)
Expellees Share × 1945	0.015	0.033*	0.026	-0.017	-0.015
	(0.020)	(0.019)	(0.024)	(0.025)	(0.032)
Expellees Share × 1946	0.022	0.038*	0.028	-0.031	-0.003
	(0.020)	(0.019)	(0.027)	(0.029)	(0.037)
Expellees Share × 1947	0.032*	0.055***	0.047**	-0.005	0.007
	(0.019)	(0.018)	(0.023)	(0.028)	(0.035)
Expellees Share × 1948	0.012	0.017	-0.006	-0.032	-0.022
	(0.018)	(0.018)	(0.021)	(0.027)	(0.032)
Expellees Share × 1949	0.022	0.025	-0.012	-0.036	-0.036
	(0.017)	(0.017)	(0.020)	(0.027)	(0.032)
Expellees Share × 1950	0.028	0.035**	-0.002	-0.034	-0.030
	(0.017)	(0.017)	(0.020)	(0.026)	(0.032)
Expellees Share × 1951	0.026	0.036**	0.003	-0.037	-0.026
	(0.018)	(0.018)	(0.020)	(0.027)	(0.033)
Expellees Share × 1952	0.029	0.036*	0.003	-0.038	-0.020
	(0.019)	(0.019)	(0.020)	(0.028)	(0.034)
Expellees Share × 1953	0.037**	0.043**	0.011	-0.026	-0.013
	(0.017)	(0.018)	(0.021)	(0.028)	(0.036)
Expellees Share × 1954	0.042**	0.045**	0.012	-0.026	-0.031
	(0.018)	(0.019)	(0.021)	(0.030)	(0.036)
Expellees Share × 1955	0.052***	0.055***	0.023	-0.012	-0.014
	(0.018)	(0.019)	(0.021)	(0.029)	(0.035)
Expellees Share × 1956	0.055***	0.053***	0.024	-0.010	-0.007
	(0.018)	(0.019)	(0.021)	(0.029)	(0.034)
Expellees Share × 1957	0.064***	0.064***	0.027	-0.006	-0.022
	(0.019)	(0.020)	(0.022)	(0.029)	(0.035)
Expellees Share × 1958	0.066***	0.068***	0.033	0.004	-0.013
	(0.019)	(0.019)	(0.021)	(0.028)	(0.034)
Expellees Share × 1959	0.065***	0.067***	0.034	0.007	-0.012
	(0.019)	(0.019)	(0.021)	(0.028)	(0.033)
Expellees Share × 1960	0.069***	0.073***	0.039*	0.013	-0.004
	(0.019)	(0.020)	(0.023)	(0.029)	(0.036)
Expellees Share × 1961	0.070***	0.073***	0.035	0.007	-0.014
	(0.020)	(0.020)	(0.023)	(0.031)	(0.037)
Expellees Share × 1962	0.081***	0.083***	0.043*	0.018	-0.006
	(0.019)	(0.020)	(0.024)	(0.031)	(0.037)
Expellees Share × 1963	0.090***	0.094***	0.057**	0.031	0.007
	(0.020)	(0.020)	(0.024)	(0.030)	(0.036)
Expellees Share × 1964	0.092***	0.095***	0.057**	0.031	0.007
	(0.020)	(0.020)	(0.024)	(0.031)	(0.036)
Expellees Share × 1965	0.091***	0.091***	0.053**	0.022	-0.001
	(0.020)	(0.020)	(0.024)	(0.031)	(0.037)
Year FE	Yes		Yes	Yes	Yes
Year × Region FE		Yes	Yes	Yes	Yes
Geographical Controls			Yes	Yes	Yes
Pre WW-II Controls				Yes	Yes
WW-II Housing Destruction					Yes
Observations	11692	11692	11692	11692	9822
Adjusted R^2	0.545	0.572	0.575	0.591	0.619

Notes: This table shows the effect of a one standard deviation increase in the expellee share on local tax rates using the DiD model laid out in equation (1). All specifications include city-level fixed effects. The varying set of controls comprises measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.1 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.6: The Effect of Mass Migration on Business Capital Tax Rates - DiD Estimates

	(1)	(2)	(3)	(4)	(5)
Expellees Share × 1938	0.003	0.002	-0.001	0.001	0.001
	(0.004)	(0.004)	(0.005)	(0.006)	(0.007)
Expellees Share × 1939	0.003	0.002	0.002	0.003	0.002
	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)
Expellees Share × 1942	0.002*	0.002*	0.002*	0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Expellees Share × 1943	-0.000	-0.000	-0.000	-0.000	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Expellees Share × 1945	0.010***	0.011***	0.010**	0.002	0.002
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)
Expellees Share × 1946	0.015***	0.014***	0.013***	0.001	0.003
	(0.003)	(0.004)	(0.005)	(0.004)	(0.005)
Expellees Share × 1947	0.019***	0.018***	0.017***	0.005	0.007
	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)
Expellees Share × 1948	0.020***	0.020***	0.018***	0.009*	0.009*
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Expellees Share × 1949	0.026***	0.027***	0.023***	0.015***	0.015***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Expellees Share × 1950	0.025***	0.026***	0.024***	0.015***	0.016***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Expellees Share × 1951	0.025***	0.025***	0.022***	0.013**	0.015**
	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)
Expellees Share × 1952	0.026***	0.026***	0.024***	0.014***	0.017***
	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)
Expellees Share × 1953	0.028***	0.028***	0.026***	0.016***	0.019***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Expellees Share × 1954	0.029***	0.030***	0.028***	0.019***	0.020***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Expellees Share × 1955	0.029***	0.029***	0.027***	0.019***	0.020***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Expellees Share × 1956	0.029***	0.029***	0.027***	0.019***	0.020***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Expellees Share × 1957	0.029***	0.029***	0.026***	0.019***	0.019***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Expellees Share × 1958	0.030***	0.030***	0.028***	0.021***	0.021***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Expellees Share × 1959	0.030***	0.031***	0.028***	0.023***	0.022***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Expellees Share × 1960	0.031***	0.032***	0.028***	0.022***	0.022***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Expellees Share × 1961	0.031***	0.032***	0.027***	0.021***	0.020***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Expellees Share × 1962	0.031***	0.032***	0.027***	0.021***	0.019***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Expellees Share × 1963	0.031***	0.032***	0.028***	0.021***	0.020***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Expellees Share × 1964	0.031***	0.032***	0.028***	0.021***	0.019***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Expellees Share × 1965	0.031***	0.031***	0.027***	0.020***	0.018***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Year FE	Yes		Yes	Yes	Yes
Year × Region FE		Yes	Yes	Yes	Yes
Geographical Controls			Yes	Yes	Yes
Pre WW-II Controls				Yes	Yes
WW-II Housing Destruction					Yes
Observations	11692	11692	11692	11692	9822
Adjusted R^2	0.304	0.327	0.329	0.355	0.369

Notes: This table shows the effect of a one standard deviation increase in the expellee share on local tax rates using the DiD model laid out in equation (1). All specifications include city-level fixed effects. The varying set of controls comprises measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.1 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.7: The Effect of Mass Migration on Business Wage Bill Tax Rates - DiD Estimates

	(1)	(2)	(3)	(4)	(5)
Expellees Share × 1942	0.019	0.014*	0.008	0.014	0.007
	(0.011)	(0.007)	(0.007)	(0.009)	(0.011)
Expellees Share × 1943	0.011	0.006	0.001	0.003	-0.002
	(0.010)	(0.005)	(0.005)	(0.008)	(0.011)
Expellees Share × 1945	0.017	0.022	0.033	0.058	0.073*
	(0.019)	(0.018)	(0.022)	(0.037)	(0.043)
Expellees Share × 1946	0.045	0.032	0.083*	0.039	0.065
	(0.031)	(0.032)	(0.045)	(0.049)	(0.057)
Expellees Share × 1947	0.061*	0.043	0.093*	0.048	0.084
	(0.035)	(0.036)	(0.052)	(0.058)	(0.062)
Expellees Share × 1948	-0.175***	-0.063	-0.058	-0.174**	-0.110
	(0.057)	(0.041)	(0.061)	(0.079)	(0.082)
Expellees Share × 1949	0.035	0.055	0.040	-0.061	0.015
	(0.051)	(0.046)	(0.059)	(0.075)	(0.078)
Expellees Share × 1950	-0.004	0.034	0.077	-0.006	0.010
	(0.046)	(0.042)	(0.055)	(0.073)	(0.078)
Expellees Share × 1951	-0.015	0.025	0.086*	0.009	0.034
	(0.045)	(0.040)	(0.052)	(0.068)	(0.074)
Expellees Share × 1952	0.120***	0.066*	0.095*	0.046	0.043
	(0.043)	(0.039)	(0.052)	(0.071)	(0.076)
Expellees Share × 1953	0.121***	0.078**	0.117**	0.077	0.139*
	(0.041)	(0.039)	(0.053)	(0.070)	(0.076)
Expellees Share × 1954	0.122***	0.076**	0.103**	0.068	0.127*
	(0.040)	(0.037)	(0.050)	(0.067)	(0.071)
Expellees Share × 1955	0.114**	0.055	0.063	0.041	0.083
	(0.045)	(0.042)	(0.057)	(0.068)	(0.074)
Expellees Share × 1956	0.114**	0.052	0.061	0.028	0.079
	(0.045)	(0.041)	(0.056)	(0.066)	(0.074)
Expellees Share × 1957	0.114***	0.063	0.072	0.037	0.070
	(0.043)	(0.040)	(0.054)	(0.069)	(0.076)
Expellees Share × 1958	0.125***	0.075*	0.083*	0.039	0.071
	(0.041)	(0.039)	(0.050)	(0.069)	(0.075)
Expellees Share × 1959	0.121***	0.065*	0.078	0.047	0.080
	(0.043)	(0.039)	(0.052)	(0.071)	(0.076)
Expellees Share × 1960	0.129***	0.074*	0.082	0.069	0.101
	(0.042)	(0.038)	(0.051)	(0.068)	(0.072)
Expellees Share × 1961	0.130***	0.072*	0.085	0.066	0.106
	(0.043)	(0.038)	(0.052)	(0.070)	(0.074)
Expellees Share × 1962	0.139***	0.082**	0.093*	0.092	0.138*
	(0.045)	(0.040)	(0.053)	(0.075)	(0.081)
Expellees Share × 1963	0.146***	0.088**	0.102*	0.105	0.151*
	(0.046)	(0.041)	(0.052)	(0.076)	(0.083)
Expellees Share × 1964	0.153***	0.089**	0.092*	0.080	0.146*
	(0.046)	(0.042)	(0.054)	(0.078)	(0.083)
Expellees Share × 1965	0.147***	0.083*	0.091*	0.081	0.143*
	(0.047)	(0.042)	(0.054)	(0.080)	(0.084)
Year FE	Yes		Yes	Yes	Yes
Year × Region FE		Yes	Yes	Yes	Yes
Geographical Controls			Yes	Yes	Yes
Pre WW-II Controls				Yes	Yes
WW-II Housing Destruction					Yes
Observations	4141	4141	4141	4141	3773
Adjusted R^2	0.152	0.382	0.383	0.384	0.400

Notes: This table shows the effect of a one standard deviation increase in the expellee share on local tax rates using the DiD model laid out in equation (1). All specifications include city-level fixed effects. The varying set of controls comprises measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.1 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.8: The Effect of Mass Migration on Agricultural Land Tax Rates - IV Estimates

	1938	1939	1942	1943	1945	1946	1947	1948	1949
Expellee Share	0.000	0.011	-0.009	0.008	0.037	0.073	0.149***	0.129**	0.207***
	(0.053)	(0.050)	(0.013)	(0.008)	(0.047)	(0.059)	(0.057)	(0.058)	(0.064)
Kleibergen-Paap F-Test	64.81	60.58	78.77	78.77	78.77	78.77	78.00	72.08	75.58
Number of observations	242	240	357	357	357	357	356	357	344
	1950	1951	1952	1953	1954	1955	1956	1957	1958
Expellee Share	0.242***	0.233***	0.230***	0.238***	0.286***	0.305***	0.313***	0.300***	0.305***
	(0.063)	(0.067)	(0.072)	(0.075)	(0.076)	(0.075)	(0.078)	(0.080)	(0.074)
Kleibergen-Paap F-Test	75.58	72.86	72.86	73.73	73.87	73.55	73.33	71.89	76.21
Number of observations	344	351	351	349	346	347	346	343	344
	1959	1960	1961	1962	1963	1964	1965		
Expellee Share	0.318***	0.309***	0.300***	0.296***	0.290***	0.279***	0.275***		
	(0.075)	(0.074)	(0.076)	(0.073)	(0.074)	(0.073)	(0.075)		
Kleibergen-Paap F-Test	76.21	76.14	76.14	75.61	75.61	76.46	76.46		
Number of observations	344	345	345	345	345	345	345		

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on annual local tax rates using the IV strategy laid out in equations (2)-(4). All tax rates are relative to the respective tax rate in 1944. Estimates are based on the most comprehensive specification that includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.9: The Effect of Mass Migration on Agricultural Land Tax Rates - OLS Estimates

	1938	1939	1942	1943	1945	1946	1947	1948	1949
Expellee Share	-0.003	-0.018	0.003	0.000	0.018	0.033	0.049	0.056	0.102***
	(0.024)	(0.021)	(0.006)	(0.002)	(0.026)	(0.033)	(0.032)	(0.034)	(0.036)
Adjusted R-Squared	0.052	0.085	0.022	0.002	0.126	0.159	0.199	0.246	0.306
Number of observations	242	240	359	359	359	359	358	359	346
	1950	1951	1952	1953	1954	1955	1956	1957	1958
Expellee Share	0.125***	0.106***	0.134***	0.158***	0.172***	0.196***	0.209***	0.196***	0.210***
	(0.037)	(0.041)	(0.040)	(0.041)	(0.042)	(0.043)	(0.042)	(0.041)	(0.040)
Adjusted R-Squared	0.321	0.317	0.348	0.363	0.384	0.390	0.411	0.408	0.411
Number of observations	346	353	353	351	348	349	348	345	346
	1959	1960	1961	1962	1963	1964	1965		
Expellee Share	0.224***	0.207***	0.204***	0.203***	0.210***	0.212***	0.203***		
	(0.037)	(0.041)	(0.042)	(0.042)	(0.041)	(0.042)	(0.042)		
Adjusted R-Squared	0.432	0.412	0.408	0.413	0.421	0.429	0.433		
Number of observations	346	347	347	347	347	347	347		

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on annual local tax rates using simple OLS. All tax rates are relative to the respective tax rate in 1944. Estimates are based on the most comprehensive specification that includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.10: The Effect of Mass Migration on Residential Property Tax Rates - IV Estimates

	1938	1939	1942	1943	1945	1946	1947	1948	1949
Expellee Share	0.034	0.047	0.003	0.008	0.027	0.122	0.246***	0.087	0.016
	(0.051)	(0.054)	(0.011)	(0.009)	(0.059)	(0.075)	(0.073)	(0.061)	(0.057)
Kleibergen-Paap F-Test	65.17	60.95	78.77	78.77	78.77	78.77	78.00	78.70	81.43
Number of observations	243	241	357	357	357	357	356	357	344
	1950	1951	1952	1953	1954	1955	1956	1957	1958
Expellee Share	0.006	0.030	0.003	0.013	0.026	0.021	0.032	0.017	0.037
	(0.057)	(0.064)	(0.068)	(0.069)	(0.070)	(0.069)	(0.066)	(0.066)	(0.065)
Kleibergen-Paap F-Test	81.43	78.67	78.67	79.60	79.46	78.94	78.64	77.49	82.16
Number of observations	344	351	351	349	346	347	346	343	344
	1959	1960	1961	1962	1963	1964	1965		
Expellee Share	0.048	0.038	0.040	0.052	0.046	0.037	0.027		
	(0.065)	(0.064)	(0.065)	(0.064)	(0.064)	(0.065)	(0.067)		
Kleibergen-Paap F-Test	82.16	81.85	81.85	81.41	81.41	81.70	81.70		
Number of observations	344	345	345	345	345	345	345		

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on annual local tax rates using the IV strategy laid out in equations (2)-(4). All tax rates are relative to the respective tax rate in 1944. Estimates are based on the most comprehensive specification that includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.11: The Effect of Mass Migration on Residential Property Tax Rates - OLS Estimates

	1938	1939	1942	1943	1945	1946	1947	1948	1949
Expellee Share	0.008	0.004	0.004	-0.005	-0.015	-0.003	0.007	-0.024	-0.049
	(0.027)	(0.027)	(0.006)	(0.004)	(0.032)	(0.037)	(0.035)	(0.032)	(0.031)
Adjusted R-Squared	0.080	0.092	0.130	0.050	0.164	0.264	0.204	0.145	0.163
Number of observations	243	241	359	359	359	359	358	359	346
	1950	1951	1952	1953	1954	1955	1956	1957	1958
Expellee Share	-0.044	-0.040	-0.041	-0.029	-0.047	-0.025	-0.017	-0.025	-0.013
	(0.031)	(0.032)	(0.034)	(0.035)	(0.036)	(0.034)	(0.034)	(0.034)	(0.033)
Adjusted R-Squared	0.185	0.214	0.233	0.217	0.190	0.176	0.180	0.186	0.179
Number of observations	346	353	353	351	348	349	348	345	346
	1959	1960	1961	1962	1963	1964	1965		
Expellee Share	-0.013	-0.008	-0.016	-0.005	-0.001	-0.001	-0.012		
	(0.032)	(0.035)	(0.036)	(0.035)	(0.035)	(0.035)	(0.035)		
Adjusted R-Squared	0.190	0.206	0.205	0.216	0.229	0.230	0.245		
Number of observations	346	347	347	347	347	347	347		

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on annual local tax rates using simple OLS. All tax rates are relative to the respective tax rate in 1944. Estimates are based on the most comprehensive specification that includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.12: The Effect of Mass Migration on Business Capital Tax Rates - IV Estimates

	1938	1939	1942	1943	1945	1946	1947	1948	1949
Expellee Share	0.005	0.001	0.001	-0.000	0.021***	0.019**	0.025***	0.016	0.031**
	(0.007)	(0.005)	(0.001)	(0.000)	(0.008)	(0.009)	(0.009)	(0.010)	(0.012)
Kleibergen-Paap F-Test	65.17	60.95	78.77	78.77	78.77	78.77	78.00	77.35	80.04
Number of observations	243	241	357	357	357	357	356	357	344
	1950	1951	1952	1953	1954	1955	1956	1957	1958
Expellee Share	0.025**	0.037***	0.038***	0.040***	0.044***	0.046***	0.052***	0.048***	0.054***
	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Kleibergen-Paap F-Test	80.04	77.36	77.36	77.91	77.12	76.69	76.38	75.27	79.48
Number of observations	344	351	351	349	346	347	346	343	344
	1959	1960	1961	1962	1963	1964	1965		
Expellee Share	0.058***	0.057***	0.057***	0.055***	0.052***	0.050***	0.047***		
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)		
Kleibergen-Paap F-Test	79.48	78.73	78.73	78.95	78.95	79.68	79.68		
Number of observations	344	345	345	345	345	345	345		

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on annual local tax rates using the IV strategy laid out in equations (2)-(4). All tax rates are relative to the respective tax rate in 1944. Estimates are based on the most comprehensive specification that includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.13: The Effect of Mass Migration on Business Capital Tax Rates - OLS Estimates

	1938	1939	1942	1943	1945	1946	1947	1948	1949
Expellee Share	0.001	0.001	0.002	-0.001	0.002	0.003	0.007	0.008	0.012*
	(0.005)	(0.004)	(0.002)	(0.001)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
Adjusted R-Squared	0.002	-0.003	0.033	0.027	0.180	0.261	0.286	0.268	0.256
Number of observations	243	241	359	359	359	359	358	359	346
	1950	1951	1952	1953	1954	1955	1956	1957	1958
Expellee Share	0.012*	0.014**	0.015**	0.017**	0.017**	0.018***	0.021***	0.021***	0.022***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Adjusted R-Squared	0.239	0.222	0.232	0.246	0.227	0.211	0.216	0.204	0.208
Number of observations	346	353	353	351	348	349	348	345	346
	1959	1960	1961	1962	1963	1964	1965		
Expellee Share	0.023***	0.021***	0.019***	0.018***	0.018***	0.016**	0.016**		
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)		
Adjusted R-Squared	0.214	0.205	0.208	0.213	0.213	0.200	0.200		
Number of observations	346	347	347	347	347	347	347		

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on annual local tax rates using simple OLS. All tax rates are relative to the respective tax rate in 1944. Estimates are based on the most comprehensive specification that includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.14: The Effect of Mass Migration on Business Wage Bill Tax Rates - IV Estimates

	1942	1943	1945	1946	1947	1948	1949	1950	1951
Expellee Share	0.008	0.006	0.184**	0.146	0.181*	-0.005	-0.017	-0.027	0.005
	(0.009)	(0.007)	(0.081)	(0.094)	(0.100)	(0.100)	(0.111)	(0.116)	(0.112)
Kleibergen-Paap F-Test	48.79	48.93	49.80	47.86	32.39	48.46	48.42	48.42	45.82
Number of observations	126	127	127	124	122	129	127	127	126
	1952	1953	1954	1955	1956	1957	1958	1959	1960
Expellee Share	0.093	0.109	0.135	0.048	0.075	0.022	0.094	0.098	0.126
	(0.127)	(0.130)	(0.129)	(0.134)	(0.143)	(0.169)	(0.151)	(0.152)	(0.149)
Kleibergen-Paap F-Test	45.82	45.95	48.51	48.37	48.37	45.72	47.30	47.30	44.64
Number of observations	126	125	124	125	125	122	123	123	122
	1961	1962	1963	1964	1965				
Expellee Share	0.111	0.117	0.192	0.201	0.198				
	(0.151)	(0.150)	(0.155)	(0.162)	(0.162)				
Kleibergen-Paap F-Test	44.37	51.74	49.55	45.06	44.58				
Number of observations	121	120	118	113	112				

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on annual local tax rates using the IV strategy laid out in equations (2)-(4). All tax rates are relative to the respective tax rate in 1944. Estimates are based on the most comprehensive specification that includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.15: The Effect of Mass Migration on Business Wage Bill Tax Rates - OLS Estimates

	1942	1943	1945	1946	1947	1948	1949	1950	1951
Expellee Share	0.015	0.003	0.083*	0.075	0.077	0.009	-0.005	-0.005	0.013
	(0.009)	(0.003)	(0.042)	(0.057)	(0.059)	(0.053)	(0.065)	(0.065)	(0.062)
Adjusted R-Squared	0.083	-0.063	-0.033	0.101	0.116	-0.018	0.130	0.122	0.127
Number of observations	126	127	127	124	122	129	127	127	126
	1952	1953	1954	1955	1956	1957	1958	1959	1960
Expellee Share	0.093	0.152*	0.115*	0.024	0.028	0.014	0.041	0.048	0.060
	(0.069)	(0.078)	(0.067)	(0.074)	(0.074)	(0.092)	(0.077)	(0.079)	(0.072)
Adjusted R-Squared	0.134	0.151	0.154	0.095	0.096	0.095	0.082	0.080	0.111
Number of observations	126	125	124	125	125	122	123	123	122
	1961	1962	1963	1964	1965				
Expellee Share	0.065	0.124	0.155*	0.139*	0.135				
	(0.074)	(0.080)	(0.082)	(0.083)	(0.086)				
Adjusted R-Squared	0.097	0.103	0.115	0.130	0.111				
Number of observations	121	120	118	113	112				

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on annual local tax rates using simple OLS. All tax rates are relative to the respective tax rate in 1944. Estimates are based on the most comprehensive specification that includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.16: The Effect of Mass Migration on Local Tax Rates - Average Effect post WW II

	OLS				Instrumental Variables			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A – Agricultural Land Tax								
Expellee Share	0.108***	0.139***	0.131***	0.148***	0.211***	0.225***	0.222***	0.260***
	(0.021)	(0.022)	(0.031)	(0.034)	(0.028)	(0.031)	(0.051)	(0.062)
Number of Observations	357	357	357	357	357	357	357	357
Kleibergen-Paap *F*-Statistic					141.22	125.97	85.13	72.08
Panel B – Residential Property Tax								
Expellee Share	0.025	0.036**	-0.029	-0.021	0.050*	0.091***	0.024	0.045
	(0.016)	(0.016)	(0.024)	(0.028)	(0.026)	(0.028)	(0.045)	(0.055)
Number of Observations	357	357	357	357	357	357	357	357
Kleibergen-Paap *F*-Statistic					103.31	135.95	92.62	78.70
Panel C – Business Capital Tax								
Expellee Share	0.023***	0.021***	0.011**	0.014**	0.030***	0.037***	0.032***	0.041***
	(0.004)	(0.004)	(0.005)	(0.006)	(0.005)	(0.006)	(0.009)	(0.011)
Number of Observations	357	357	357	357	357	357	357	357
Kleibergen-Paap *F*-Statistic					107.12	139.70	91.55	77.35
Panel D – Business Wage Bill Tax								
Expellee Share	0.062*	0.091**	0.034	0.065	0.070	0.101*	0.056	0.080
	(0.032)	(0.035)	(0.055)	(0.056)	(0.044)	(0.054)	(0.107)	(0.112)
Number of Observations	129	129	129	129	129	129	129	129
Kleibergen-Paap *F*-Statistic					122.96	71.26	54.93	48.46
Geography Controls		Yes	Yes	Yes		Yes	Yes	Yes
Pre-WWII Controls			Yes	Yes			Yes	Yes
Share Destroyed Housing				Yes				Yes

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on mean post-WW II local tax rate changes using simple OLS and the IV strategy laid out in equations (2)-(4). Mean tax rates (post war) are relative to the respective tax rate in 1944. The set of controls includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.17: The Effect of Migration on Per Capita Spending (in logs) - IV Estimates

	1950	1951	1952	1953	1954	1955	1956	1957	1958	1959	1960	1961	1962
Panel A – Welfare/Health													
Expellee Share	0.286**	0.253**	0.208*	0.220**	0.266**	0.262**	0.204*	0.162	0.230*	0.277**	0.222*	0.397***	0.208*
	(0.112)	(0.118)	(0.123)	(0.110)	(0.124)	(0.110)	(0.118)	(0.111)	(0.118)	(0.129)	(0.117)	(0.142)	(0.117)
Kleibergen-Paap F-Test	63.83	64.39	56.91	55.13	54.38	54.75	57.81	52.38	56.87	59.40	60.87	59.90	58.29
Number of observations	189	193	205	212	208	212	216	219	229	235	238	243	249
Panel B – Admin/Police													
Expellee Share	0.083	0.008	-0.001	-0.020	0.089	0.072	-0.032	-0.005	0.157**	0.046	0.099	0.161**	0.003
	(0.098)	(0.085)	(0.099)	(0.070)	(0.106)	(0.078)	(0.080)	(0.073)	(0.075)	(0.062)	(0.094)	(0.071)	(0.083)
Kleibergen-Paap F-Test	63.83	64.47	56.91	55.13	54.38	54.75	57.81	52.38	56.87	58.96	60.87	59.90	58.29
Number of observations	189	192	205	212	208	212	216	219	229	233	238	243	249
Panel C – Infrastructure/Housing													
Expellee Share	-0.278**	-0.356***	-0.282***	-0.347***	-0.290***	-0.404***	-0.384***	-0.282***	-0.263***	-0.190**	-0.204**	-0.220**	-0.211**
	(0.123)	(0.122)	(0.105)	(0.090)	(0.085)	(0.078)	(0.092)	(0.084)	(0.083)	(0.084)	(0.089)	(0.085)	(0.087)
Kleibergen-Paap F-Test	63.83	64.37	56.91	55.13	54.38	54.75	57.81	52.38	56.87	59.40	60.87	59.90	58.29
Number of observations	189	192	205	212	208	212	216	219	229	235	238	243	249
Panel D – Schools/Culture													
Expellee Share	-0.174**	-0.302***	-0.313***	-0.277***	-0.200**	-0.174**	-0.165*	-0.278***	-0.179**	-0.131	-0.209***	-0.024	-0.157**
	(0.082)	(0.084)	(0.097)	(0.091)	(0.084)	(0.087)	(0.084)	(0.091)	(0.078)	(0.083)	(0.074)	(0.074)	(0.067)
Kleibergen-Paap F-Test	63.83	64.39	56.91	55.13	54.38	54.75	57.81	52.38	56.87	59.40	60.87	59.90	58.29
Number of observations	189	193	205	212	208	212	216	219	229	235	238	243	249
Panel E – Total Spending													
Expellee Share	-0.096	-0.155**	-0.162**	-0.151***	-0.095*	-0.127**	-0.157***	-0.149***	-0.078	-0.059	-0.077	-0.001	-0.097
	(0.072)	(0.068)	(0.068)	(0.057)	(0.055)	(0.054)	(0.055)	(0.051)	(0.055)	(0.057)	(0.063)	(0.058)	(0.060)
Kleibergen-Paap F-Test	63.83	64.39	56.91	55.13	54.38	54.75	57.81	52.38	56.87	59.40	60.87	59.90	58.29
Number of observations	189	193	205	212	208	212	216	219	229	235	238	243	249

Note: This table shows the effect of a one standard deviation increase in the expelleee share on annual per capita spending (in logs) using the IV strategy laid out in equations (2)-(4). The set of controls includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.18: The Effect of Migration on Per Capita Spending (in logs) - OLS Estimates

	1950	1951	1952	1953	1954	1955	1956	1957	1958	1959	1960	1961	1962
Panel A – Welfare/Health													
Expellee Share	0.057	0.090	0.016	-0.016	-0.029	-0.001	-0.052	-0.028	-0.057	0.041	0.043	-0.015	0.017
	(0.062)	(0.071)	(0.069)	(0.069)	(0.069)	(0.063)	(0.062)	(0.075)	(0.066)	(0.070)	(0.067)	(0.081)	(0.068)
Number of observations	189	193	205	212	208	212	216	219	229	235	238	243	249
Panel B – Admin/Police													
Expellee Share	-0.033	-0.060	-0.069	-0.095***	-0.056	-0.003	-0.041	-0.017	0.029	0.033	0.077	0.042	0.030
	(0.045)	(0.042)	(0.049)	(0.036)	(0.046)	(0.038)	(0.044)	(0.041)	(0.046)	(0.037)	(0.051)	(0.047)	(0.043)
Number of observations	189	192	205	212	208	212	216	219	229	233	238	243	249
Panel C – Infrastructure/Housing													
Expellee Share	-0.156***	-0.214***	-0.183***	-0.207***	-0.189***	-0.208***	-0.184***	-0.167***	-0.164***	-0.134***	-0.072	-0.119**	-0.095**
	(0.056)	(0.064)	(0.054)	(0.055)	(0.049)	(0.052)	(0.047)	(0.043)	(0.047)	(0.039)	(0.049)	(0.047)	(0.038)
Number of observations	189	192	205	212	208	212	216	219	229	235	238	243	249
Panel D – Schools/Culture													
Expellee Share	-0.047	-0.083**	-0.112**	-0.075	-0.064	-0.067	-0.074*	-0.111**	-0.130***	-0.090**	-0.070	-0.077**	-0.083***
	(0.040)	(0.040)	(0.052)	(0.047)	(0.046)	(0.043)	(0.043)	(0.044)	(0.042)	(0.039)	(0.046)	(0.035)	(0.031)
Number of observations	189	193	205	212	208	212	216	219	229	235	238	243	249
Panel E – Total Spending													
Expellee Share	-0.062*	-0.082**	-0.100***	-0.109***	-0.097***	-0.093***	-0.105***	-0.095***	-0.101***	-0.063**	-0.022	-0.058*	-0.053*
	(0.032)	(0.035)	(0.036)	(0.035)	(0.032)	(0.033)	(0.030)	(0.029)	(0.032)	(0.028)	(0.034)	(0.030)	(0.028)
Number of observations	189	193	205	212	208	212	216	219	229	235	238	243	249

Note: This table shows the effect of a one standard deviation increase in the expelleee share on per capita spending (in logs) using simple OLS. The set of controls comprises occupation zone dummies, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.1). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.19: The Effect of Mass Migration on Per Capita Spending - Average Effect post WW II

	OLS				Instrumental Variables			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A – Welfare/Health								
Expellee Share	-0.132***	-0.163***	-0.053	0.007	0.040	0.011	0.097	0.220**
	(0.037)	(0.041)	(0.053)	(0.059)	(0.055)	(0.068)	(0.069)	(0.101)
Number of Observations	235	235	235	235	235	235	235	235
Kleibergen-Paap *F*-Statistic					132.94	71.59	78.69	59.40
Panel B – Admin/Police								
Expellee Share	-0.051**	-0.087***	-0.066**	-0.015	0.154***	-0.068*	-0.032	-0.013
	(0.023)	(0.024)	(0.031)	(0.032)	(0.052)	(0.041)	(0.047)	(0.054)
Number of Observations	235	235	235	235	235	235	235	235
Kleibergen-Paap *F*-Statistic					132.94	71.59	78.69	59.40
Panel C – Infrastructure/Housing								
Expellee Share	-0.116***	-0.146***	-0.192***	-0.165***	-0.037	-0.244***	-0.247***	-0.327***
	(0.027)	(0.027)	(0.035)	(0.038)	(0.035)	(0.048)	(0.057)	(0.074)
Number of Observations	235	235	235	235	235	235	235	235
Kleibergen-Paap *F*-Statistic					132.94	71.59	78.69	59.40
Panel D – Schools/Culture								
Expellee Share	-0.090***	-0.127***	-0.148***	-0.102***	-0.137***	-0.197***	-0.181***	-0.237***
	(0.020)	(0.021)	(0.028)	(0.031)	(0.038)	(0.048)	(0.055)	(0.066)
Number of Observations	235	235	235	235	235	235	235	235
Kleibergen-Paap *F*-Statistic					132.94	71.59	78.69	59.40
Panel E – Total Spending								
Expellee Share	-0.093***	-0.126***	-0.128***	-0.087***	-0.008	-0.151***	-0.128***	-0.147***
	(0.020)	(0.020)	(0.024)	(0.026)	(0.029)	(0.037)	(0.041)	(0.050)
Number of Observations	235	235	235	235	235	235	235	235
Kleibergen-Paap *F*-Statistic					132.94	71.59	78.69	59.40
Geography Controls		Yes	Yes	Yes		Yes	Yes	Yes
Pre-WWII Controls			Yes	Yes				Yes
Share Destroyed Housing				Yes			Yes	Yes

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on mean post-WW II per capita spending (in logs) using simple OLS and the IV strategy laid out in equations (2)-(4). The set of controls includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.20: The Effect of Mass Migration on Per Capita Debt and Transfers (in logs) - IV and OLS Results

	1951	1952	1953	1954	1955	1956	1957	1958	1959	1960	1961	1962	1963	1965
Panel A – Per Capita Debt (IV Estimates)														
Expellee Share	0.017	0.182	0.075	0.165	0.133	0.166	0.125	0.076	0.106	0.110	0.149	0.203*	0.247*	0.251*
	(0.275)	(0.175)	(0.159)	(0.132)	(0.113)	(0.118)	(0.115)	(0.113)	(0.106)	(0.111)	(0.119)	(0.117)	(0.126)	(0.131)
Kleibergen-Paap F-Test	78.93	74.30	67.35	64.66	66.62	66.62	68.19	61.75	66.36	74.65	76.52	73.19	71.99	74.03
Number of observations	180	182	193	199	200	200	205	207	217	225	234	239	239	241
Panel B – Per Capita Debt (OLS Estimates)														
Expellee Share	0.029	0.112	0.097	0.090	0.048	0.063	0.050	0.050	0.069	0.055	0.052	0.072	0.099	0.098
	(0.149)	(0.105)	(0.091)	(0.080)	(0.066)	(0.061)	(0.058)	(0.060)	(0.060)	(0.056)	(0.064)	(0.059)	(0.060)	(0.061)
Number of observations	180	182	193	199	200	200	205	207	217	225	234	239	239	241
Panel C – Per Capita Transfers (IV Estimates)														
Expellee Share	-0.013	0.247*	0.197*	0.726***	0.462**	0.476**	0.540**	0.565**	0.669**	0.438**	0.668**	0.690***	0.499**	0.582***
	(0.124)	(0.140)	(0.115)	(0.198)	(0.197)	(0.236)	(0.236)	(0.250)	(0.301)	(0.206)	(0.259)	(0.262)	(0.226)	(0.211)
Kleibergen-Paap F-Test	64.39	58.03	55.13	54.38	54.75	57.81	52.38	54.91	59.40	59.17	61.35	58.29	57.76	59.43
Number of observations	193	204	212	208	212	216	219	228	235	237	244	249	249	251
Panel D – Per Capita Transfers (OLS Estimates)														
Expellee Share	0.044	0.187**	0.121**	0.128	-0.001	-0.032	-0.058	-0.128	-0.043	-0.065	-0.111	-0.054	-0.106	0.002
	(0.079)	(0.079)	(0.060)	(0.105)	(0.102)	(0.109)	(0.103)	(0.107)	(0.141)	(0.113)	(0.110)	(0.109)	(0.105)	(0.086)
Number of observations	193	204	212	208	212	216	219	228	235	237	244	249	249	251

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on annual per capita debt and state transfers (in logs) using simple OLS and the IV strategy laid out in equations (2)-(4). The set of controls includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.21: The Effect of Mass Migration on Debt and Transfers (in logs) - Average Effect post WW II

	OLS				Instrumental Variables			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A – Per Capita Debt								
Expellee Share	-0.026	0.010	-0.005	0.059	0.175***	0.097	0.024	0.091
	(0.034)	(0.037)	(0.050)	(0.055)	(0.060)	(0.063)	(0.091)	(0.105)
Number of Observations	240	240	240	240	240	240	240	240
Kleibergen-Paap *F*-Statistic					117.23	76.26	80.27	73.20
Panel B – Per Capita Transfers								
Expellee Share	-0.129**	-0.251***	-0.156**	-0.057	0.288***	0.015	0.189	0.360**
	(0.055)	(0.057)	(0.076)	(0.083)	(0.086)	(0.109)	(0.145)	(0.165)
Number of Observations	247	247	247	247	247	247	247	247
Kleibergen-Paap *F*-Statistic					135.04	74.70	73.39	64.62
Geography Controls		Yes	Yes	Yes		Yes	Yes	Yes
Pre-WWII Controls			Yes	Yes			Yes	Yes
Share Destroyed Housing				Yes				Yes

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on mean post-WW II per capita debt and transfers (in logs) using simple OLS and the IV strategy laid out in equations (2)-(4). The set of controls includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.22: The Effect of Migration on Voter Turnout & Vote Shares - IV Estimates

	1946	1947-50	1951-55	1956-59	1960-62
Panel A – Voter Turnout					
Expellee Share	-0.424	5.416***	3.946**	0.880	0.650
	(1.211)	(1.883)	(1.641)	(1.085)	(1.069)
Kleibergen-Paap F-Test	68.18	72.16	66.78	69.66	73.19
Number of observations	168	170	201	219	239
Panel B – Vote Share CDU					
Expellee Share	-1.401	-2.424	-1.919	-0.844	-2.954*
	(2.423)	(2.134)	(2.586)	(1.878)	(1.771)
Kleibergen-Paap F-Test	65.33	73.17	24.67	70.38	73.22
Number of observations	165	169	164	211	232
Panel C – Vote Share SPD					
Expellee Share	-0.635	-4.567**	-2.360	-3.184*	1.782
	(2.070)	(1.971)	(1.779)	(1.667)	(1.670)
Kleibergen-Paap F-Test	68.18	72.16	66.78	69.66	73.19
Number of observations	168	170	201	219	239
Panel D – Vote Share GB/BHE					
Expellee Share	3.064**	3.064**	3.064**	1.813***	1.044
	(1.258)	(1.258)	(1.258)	(0.568)	(0.861)
Kleibergen-Paap F-Test	39.63	39.63	39.63	53.59	31.11
Number of observations	123	123	123	149	82

Note: This table shows the effect of a one standard deviation increase in the expelleee share on voter turnout and party vote shares over time using the IV strategy laid out in equations (2)-(4). The set of controls includes election year fixed effects, measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.23: The Effect of Migration on Voter Turnout & Vote Shares - OLS Estimates

	1946	1947-50	1951-55	1956-59	1960-62
Panel A – Voter Turnout					
Expellee Share	-1.331*	1.824	0.807	-0.317	-1.857***
	(0.768)	(1.148)	(0.926)	(0.588)	(0.687)
Number of observations	168	170	201	219	239
Panel B – Vote Share CDU					
Expellee Share	-1.299	-0.645	-0.880	1.204	-0.269
	(1.634)	(1.391)	(1.245)	(1.026)	(0.928)
Number of observations	165	169	164	211	232
Panel C – Vote Share SPD					
Expellee Share	-1.760	-1.482	-2.459**	-2.797***	-0.621
	(1.281)	(1.098)	(1.013)	(0.937)	(0.805)
Number of observations	168	170	201	219	239
Panel D – Vote Share GB/BHE					
Expellee Share	4.579***	4.579***	4.579***	2.243***	1.628***
	(0.941)	(0.941)	(0.941)	(0.436)	(0.434)
Number of observations	123	123	123	149	82

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on voter turnout and party vote shares using simple OLS. The set of controls comprises election year fixed effects, occupation zone dummies, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.1). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.24: The Effect of Mass Migration on Voter Turnout & Vote Shares - Average Effect post WW II

	OLS				Instrumental Variables			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A – Voter Turnout								
Expellee Share	-0.788**	-0.810*	0.433	-0.516	-0.721	0.772	2.264**	1.723*
	(0.349)	(0.429)	(0.586)	(0.634)	(0.597)	(0.721)	(0.933)	(0.999)
Number of Observations	216	216	216	216	216	216	216	216
Kleibergen-Paap *F*-Statistic					113.12	71.33	83.38	70.86
Panel B – Vote Share CDU								
Expellee Share	-2.930***	-2.815***	-0.081	0.327	-6.198***	-4.044***	-1.677	-1.632
	(0.643)	(0.725)	(0.923)	(1.015)	(1.167)	(1.221)	(1.512)	(1.749)
Number of Observations	215	215	215	215	215	215	215	215
Kleibergen-Paap *F*-Statistic					113.02	74.49	83.02	70.45
Panel C – Vote Share SPD								
Expellee Share	-0.977	-1.091	-1.378	-1.641*	-2.066*	0.425	-1.783	-2.143
	(0.620)	(0.767)	(0.961)	(0.935)	(1.059)	(1.148)	(1.402)	(1.579)
Number of Observations	216	216	216	216	216	216	216	216
Kleibergen-Paap *F*-Statistic					113.12	71.33	83.38	70.86
Panel D – Vote Share GB/BHE								
Expellee Share	3.395***	3.380***	3.217***	3.275***	3.192***	3.001***	2.546***	2.464***
	(0.225)	(0.287)	(0.424)	(0.448)	(0.316)	(0.392)	(0.615)	(0.711)
Number of Observations	116	116	116	116	116	116	116	116
Kleibergen-Paap *F*-Statistic					62.89	55.19	55.06	50.91
Geography Controls		Yes	Yes	Yes		Yes	Yes	Yes
Pre-WWII Controls			Yes	Yes			Yes	Yes
Share Destroyed Housing				Yes				Yes

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on mean post-WW II voter turnout and party vote shares using the IV strategy laid out in equations (2)-(4) and simple OLS. The set of controls includes measures of institutional difference, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 3.2 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

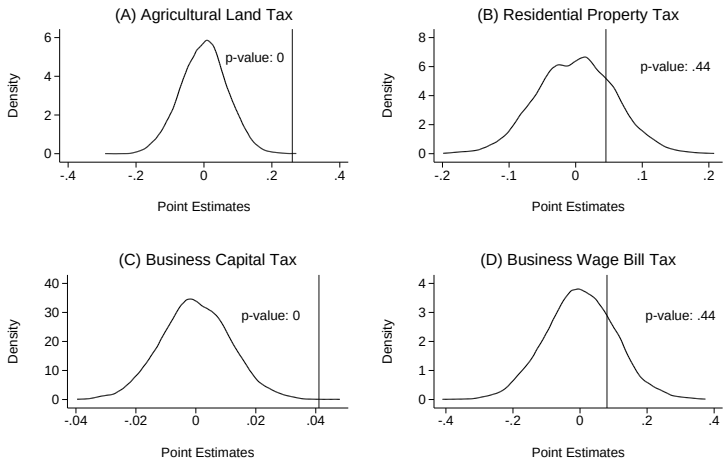
Table B.25: The Effect of Mass Migration on Preferences for Redistribution

	OLS			Instrumental Variables		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – For the family						
Expellee Share	0.005	0.004	0.004	0.024**	0.003	-0.005
	(0.006)	(0.007)	(0.007)	(0.011)	(0.020)	(0.019)
Number of Observations	8,974	8,974	8,974	8,974	8,974	8,974
Kleibergen-Paap *F*-Statistic				41.97	20.74	21.40
Panel B – When being old						
Expellee Share	-0.005	0.003	0.003	0.034**	0.053**	0.046**
	(0.004)	(0.007)	(0.006)	(0.014)	(0.025)	(0.023)
Number of Observations	8,974	8,974	8,974	8,974	8,974	8,974
Kleibergen-Paap *F*-Statistic				41.97	20.74	21.40
Panel C – When being sick						
Expellee Share	0.005	0.012**	0.013**	0.044***	0.058***	0.051***
	(0.005)	(0.006)	(0.005)	(0.012)	(0.021)	(0.019)
Number of Observations	8,974	8,974	8,974	8,974	8,974	8,974
Kleibergen-Paap *F*-Statistic				41.97	20.74	21.40
Panel D – When needing care						
Expellee Share	0.003	0.006	0.007	0.026**	0.034*	0.035*
	(0.004)	(0.006)	(0.006)	(0.011)	(0.020)	(0.021)
Number of Observations	8,974	8,974	8,974	8,974	8,974	8,974
Kleibergen-Paap *F*-Statistic				41.97	20.74	21.40
Panel E – When unemployed						
Expellee Share	-0.000	0.008	0.007	0.032**	0.045**	0.035*
	(0.007)	(0.009)	(0.008)	(0.013)	(0.021)	(0.018)
Number of Observations	8,974	8,974	8,974	8,974	8,974	8,974
Kleibergen-Paap *F*-Statistic				41.97	20.74	21.40
Historical county controls		Yes	Yes		Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Current county controls			Yes			Yes

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on individuals' preferences for redistribution (as measured by the respondents' preferred role of the state with regard to different areas of social security) using simple OLS and our IV strategy laid out in equations (2)-(4). The set of controls comprises (i) respondents' characteristics, (ii) current features of the county of residence, and (iii) historical controls to capture persistent differences a cross regions (see Sections 3.2 and 5 for deta ils). Cross-sectional weights are used. Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

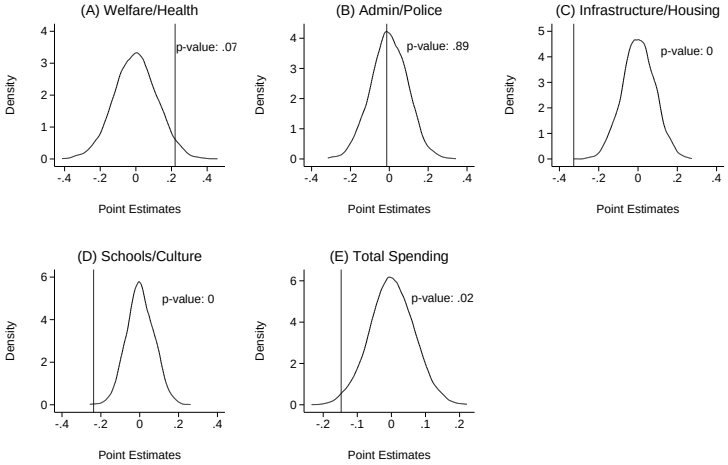
C Inference

Figure C.1: Distribution of Placebo IV Estimates - Tax Rates



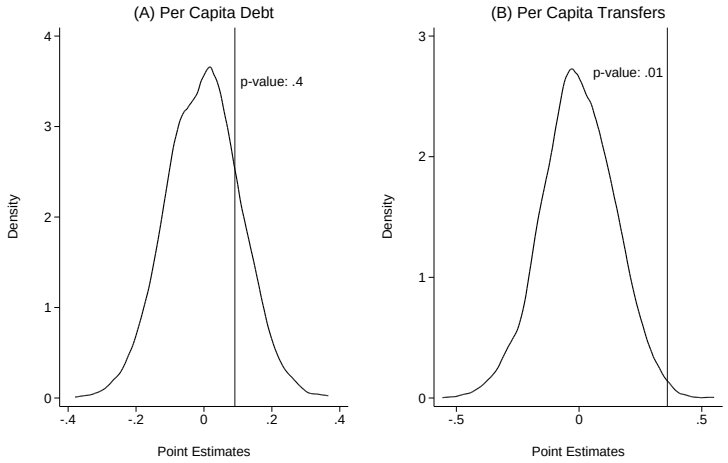
Notes: This figure shows the empirical distributions of placebo estimates for local tax rates on (A) agricultural land, (B) residential property, (C) business capital, and (D) business' wage bill. The cumulative distribution functions are based on 5000 estimates of β using the IV specification displayed in column (6) of Table B.16 and random permutations of the respective dependent variable. The vertical lines indicate the corresponding point estimate as shown in column (6) of Table B.16.

Figure C.2: Distribution of Placebo IV Estimates - Per Capita Spending



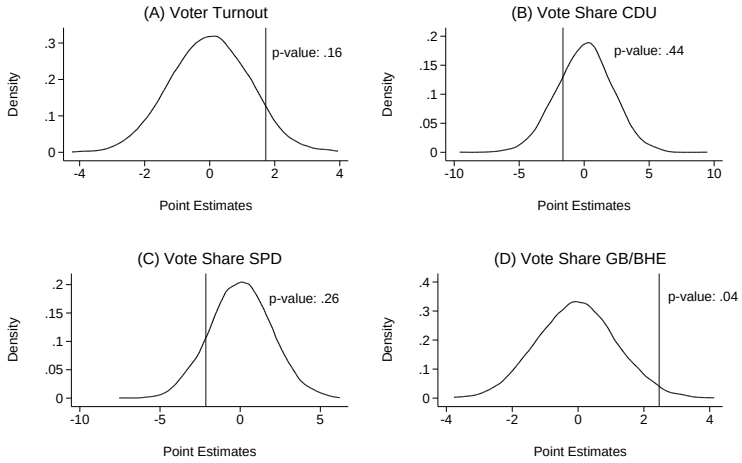
Notes: This figure shows the empirical distributions of placebo estimates for per capita spending on (A) welfare and health, (B) administration and the police, (C) public infrastructure and housing, (D) schools and culture, (E) all items. The cumulative distribution functions are based on 5000 estimates of β using the IV specification displayed in column (6) of Table B.19 and random permutations of the respective dependent variable. The vertical lines indicate the corresponding point estimate as shown in column (6) of Table B.19.

Figure C.3: Distribution of Placebo IV Estimates - Per Capita Debt



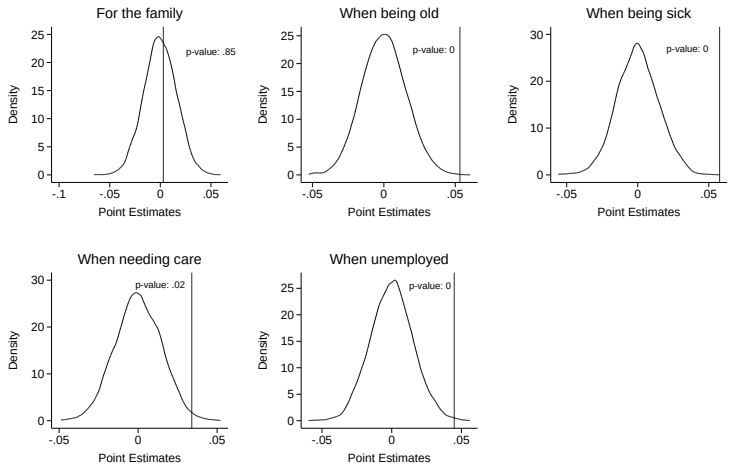
Notes: This figure shows the empirical distributions of placebo estimates for (A) per capita debt and (B) per capita transfers. The cumulative distribution functions are based on 5000 estimates of β using the IV specification displayed in column (6) of Table B.21 and random permutations of the respective dependent variable. The vertical lines indicate the corresponding point estimate as shown in column (6) of Table B.21.

Figure C.4: Distribution of Placebo IV Estimates - Vote Turnout & Vote Shares



Notes: This figure shows the empirical distributions of placebo estimates for (A) voter turnout, (B) the CDU vote share, (C) SPD vote share, and (D) the GB/BHE vote share. The cumulative distribution functions are based on 5000 estimates of β using the IV specification displayed in column (6) of Table B.24 and random permutations of the respective dependent variable. The vertical lines indicate the corresponding point estimate as shown in column (6) of Table B.24.

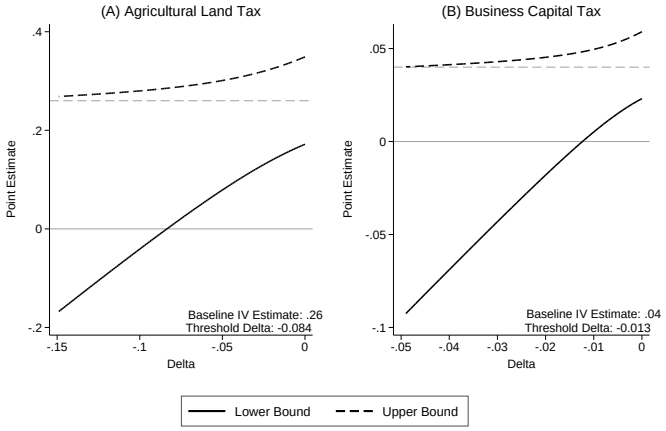
Figure C.5: Distribution of Placebo IV Estimates - Preferences for Redistribution



Notes: This figure shows the empirical distributions of placebo estimates for individuals' preferences for redistribution. The cumulative distribution functions are based on 5000 estimates of β using the IV specification displayed in column (6) of Table B.25 and random permutations of the respective dependent variable. The vertical lines indicate the corresponding point estimate as shown in column (6) of Table B.25.

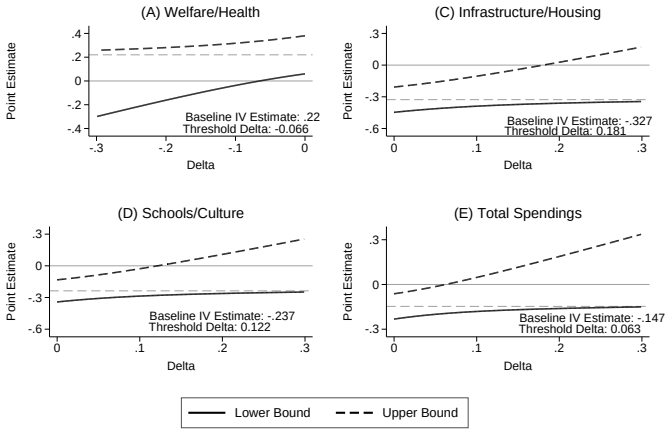
D Test of Exclusion Restriction

Figure D.1: Relaxing the Exclusion Restriction - Local Tax Rates



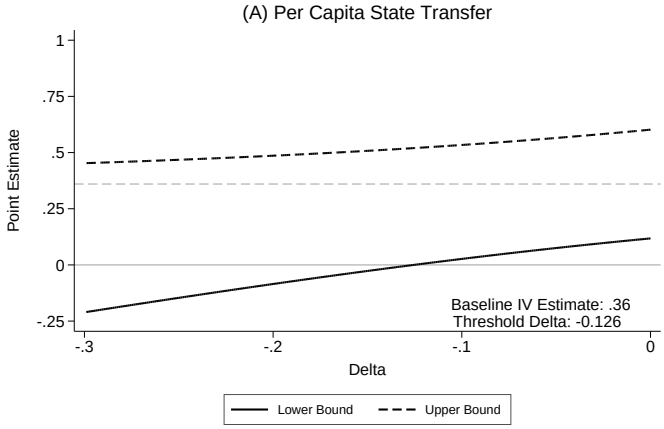
Notes: This figure shows the upper and lower bound of the 90% confidence interval of the second-stage point estimates for the expellee share on (A) the mean agricultural land tax rate and (B) the mean business capital tax rate when allowing for a direct effect of the instrument on the outcome (as indicated by coefficient δ depicted at the x-line). Following Conley et al. (2012), it is assumed that the possible direct effect of the instrument on the outcome of interest is uniformly distributed over the interval [-δ,0]. At the indicated threshold value of δ, the second-stage estimate becomes insignificant at the 10% level. For completeness, the dashed lines indicate our preferred IV point estimates when assuming $\delta = 0$, see column (6) of Table B.16.

Figure D.2: Relaxing the Exclusion Restriction - Local Spending



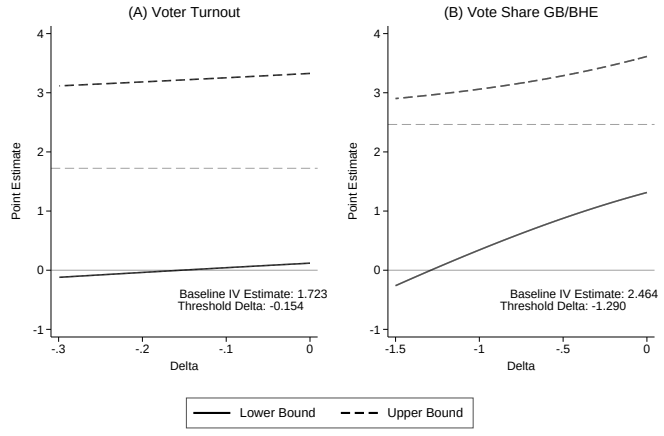
Notes: This figure shows the upper and lower bound of the 90% confidence interval of the second-stage point estimates for the expellee share on per capita spendings on (A) welfare/health, (B) infrastructure/housing, (C) schools/culture, and (D) all items when allowing for a direct effect of the instrument on the outcome (as indicated by coefficient δ depicted at the x-line). Following Conley et al. (2012), it is assumed that the possible direct effect of the instrument on the outcome of interest is uniformly distributed over the interval [0,δ]. At the indicated threshold value of δ, the second-stage estimate becomes insignificant at the 10% level. For completeness, the dashed lines indicate our preferred IV point estimates when assuming $\delta = 0$, see column (6) of Table B.19.

Figure D.3: Relaxing the Exclusion Restriction - Per Capita Transfers



Notes: This figure shows the upper and lower bound of the 90% confidence interval of the second-stage point estimates for the expellee share on per capita transfers when allowing for a direct effect of the instrument on the outcome (as indicated by coefficient δ depicted at the x-line). Following Conley et al. (2012), it is assumed that the possible direct effect of the instrument on the outcome of interest is uniformly distributed over the interval [0,δ]. At the indicated threshold value of δ, the second-stage estimate becomes insignificant at the 10% level. For completeness, the dashed lines indicate our preferred IV point estimate when assuming $\delta = 0$, see column (6) of Panel (B), Table B.21.

Figure D.4: Relaxing the Exclusion Restriction - Voter Turnout & Party Vote Shares



Notes: This figure shows the upper and lower bound of the 90% confidence interval of the second-stage point estimates for the expellee share on (A) voter turnout and (B) the GB/BHE vote share when allowing for a direct effect of the instrument on the outcome (as indicated by coefficient δ depicted at the x-line). Following Conley et al. (2012), it is assumed that the possible direct effect of the instrument on the outcome of interest is uniformly distributed over the interval [0,δ]. At the indicated threshold value of δ, the second-stage estimate becomes insignificant at the 10% level. For completeness, the dashed lines indicate our preferred IV point estimate when assuming $\delta = 0$, see column (6) of Panel (B), Table B.24.