ECONSTOR
Make Your Publications Visible.

A Service of

ZBW Leibniz-Informationszentrum Wirtschaft
Leibniz Information Centre for Economics

Chevalier, Arnaud; Elsner, Benjamin; Lichter, Andreas; Pestel, Nico

Working Paper

Immigrant Voters, Taxation and the Size of the Welfare State

IZA Discussion Papers, No. 11725

Provided in Cooperation with:
IZA – Institute of Labor Economics

Suggested Citation: Chevalier, Arnaud; Elsner, Benjamin; Lichter, Andreas; Pestel, Nico (2018) : Immigrant Voters, Taxation and the Size of the Welfare State, IZA Discussion Papers, No. 11725, Institute of Labor Economics (IZA), Bonn

This Version is available at:
https://hdl.handle.net/10419/185185

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

WWW.ECONSTOR.EU

Mitglied der Leibniz-Gemeinschaft

I Z A Institute
of Labor Economics
Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 11725

Immigrant Voters, Taxation and the Size of the Welfare State

Arnaud Chevalier
Benjamin Elsner
Andreas Lichter
Nico Pestel

AUGUST 2018

I Z A Institute
of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 11725

Immigrant Voters, Taxation and the Size of the Welfare State

Arnaud Chevalier
Royal Holloway University London and IZA

Andreas Lichter
IZA

Benjamin Elsner
University College Dublin, CReAM and IZA

Nico Pestel
ZEW Mannheim and IZA

AUGUST 2018

Any opinions expressed in this paper are those of the author(s) and not those of IZA. Research published in this series may include views on policy, but IZA takes no institutional policy positions. The IZA research network is committed to the IZA Guiding Principles of Research Integrity.

The IZA Institute of Labor Economics is an independent economic research institute that conducts research in labor economics and offers evidence-based policy advice on labor market issues. Supported by the Deutsche Post Foundation, IZA runs the world's largest network of economists, whose research aims to provide answers to the global labor market challenges of our time. Our key objective is to build bridges between academic research, policymakers and society.

IZA Discussion Papers often represent preliminary work and are circulated to encourage discussion. Citation of such a paper should account for its provisional character. A revised version may be available directly from the author.

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Immigrant Voters, Taxation and the Size of the Welfare State*

This paper studies the impact of immigration on public policy setting. As a natural experiment, we exploit 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 unlike 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 tax raises as well as shifts in spending. In response to the inflow, farm and business owners were taxed more while residential property and wage bill taxes were left unchanged. Moreover, high-inflow cities significantly raised welfare spending while reducing spending on infrastructure and housing. Election data suggest that these policy changes were partly driven by the political influence of the immigrants: in high-inflow regions, the major parties were more likely to nominate immigrants as candidates, and a pro-immigrant party received high vote shares. We further document that this episode of mass immigration had lasting effects on people's preferences for redistribution. In areas with larger inflows in the 1940s, people have substantially higher demand for redistribution more than 50 years later.

JEL Classification: J61, H20

Keywords: migration, taxation, spending, welfare state

Corresponding author:
Benjamin Elsner
School of Economics
University College Dublin
Belfield, Dublin 4
Ireland

E-mail: benjamin.elsner@ucd.ie

* An earlier version of this paper circulated as "Immigration and Redistribution: Evidence from 8 Million Forced Migrants". We would like to thank Stefano DellaVigna, Dave Donaldson, Dirk Foremny, Stephan Heblich, Peter Kuhn, Mark Rosenzweig, Jesse Rothstein, Marvin Suesse, Guido Tabellini, Felipe Valencia, Nico Voigtl¨ander, Joachim Voth, Fabian Waldinger as well as the audiences at IZA, ZEW, U Duisburg-Essen, U Bayreuth, U Bonn, U Kent, U Maastricht, U K¨oln, U Antwerp, U Lancaster, IIPF 2017, SOLE 2018 and the IZA World of Labor Conference 2018 for helpful comments and suggestions. Lisa Jaschke, Margard Ody, Georgios Tassoukis, Theresa Markefke, Filippo Ricordi and Nicolas Zimmer provided outstanding research assistance.

1 Introduction

Immigration presents a major challenge to modern welfare states. A recurring concern in the public debate 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 reduces 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, such 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 to post-war West Germany, we show that the 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. In addition, all Germans who had previously been living outside the newly-drawn borders were to be expelled and forced to move to either East or West Germany. This decision resulted in the displacement of over twelve million people, of who 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 they had voting rights and were eligible for social welfare from their time of arrival. However, after losing virtually all of their assets during the expulsions, they were considerably poorer than the average person in West Germany (Bauer et al., 2013).

The initial placement of the expellees gives rise to substantial geographic variation in the size of the inflow, which forms the basis of our identification strategy. Using panel data, we exploit this variation to analyze whether West German cities responded to the migration shock by changing their tax and spending policies. Within Germany's federal system, cities have long enjoyed a high degree of fiscal autonomy; for instance, they 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. To cover the higher welfare expenditures triggered by the inflow, cities had three major margins of adjustment, namely raising local taxes, reducing spending on items other than welfare and incurring debt. While it may appear mechanical that high-inflow cities had to change their taxes and spending, it is important to investigate *which* adjustment channels cities chose in response to the inflow, as well as quantifying their importance. 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 show that natives in states with high immigration prefer lower taxes and spending (e.g. Luttmer, 2001, Senik et al., 2009, Alesina et al., 2018a). 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 dating back to the late 1930s and estimate a difference-in-differences (DiD) model with the share of expellees as a continuous treatment. 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 expellee inflow but significantly diverged thereafter. High-inflow cities significantly raised taxes on agricultural land as well as firms' capital and profits, and the gap in tax rates 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 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 West German county based on gravity forces that were important before the war but not thereafter. Our instrument assigns all expellees to West German counties based on their place of residence in 1939 and the distance from these places to each county in West Germany. Given that the Allied Forces mainly assigned the expellees to housing closer to the border, the interaction of these two gravity forces is a strong predictor of the actual initial settlement of the expellees. The identifying assumption is that the geographic distribution of Germans before 1939 is orthogonal to economic conditions in West Germany after 1945. We believe that this assumption is plausible given that the entire German population outside the newly drawn borders had to leave their homelands after 1945 and all economic exchange between West Germany and the former Eastern territories ceased after the territories were ceded to other countries. However, an obvious challenge to the exclusion restriction is that West German cities that were closer to the new inner-German border had weaker economic growth over the post-war period (Redding and Sturm, 2008). We alleviate this concern by controlling for the closeness to the border between East and West Germany.

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 corroborate 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 differential time trends. The IV estimator almost exactly replicates the DiD estimates, which we view as strong evidence in support

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

of instrument validity. Second, following Conley et al. (2012), we show that the causal interpretation of the IV even holds if we allow for large direct effects of the instrument.

The IV estimates further show that high-inflow cities significantly shifted their spending away from non-welfare related items. While they decreased overall per capita spending, they increased spending on social welfare and reduced 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.

Using data on elections, we further document that the expellee inflow changed the political landscape in West Germany. We find that high-inflow cities had considerably higher turnout in local elections, which indicates that the inflow raised the economic and political stakes for the population. We also show that high-inflow cities had significantly larger vote shares for the GB/BHE, a party that represented the interests of the expellee. This provides evidence that the expellees used their voting rights to influence local policy setting. Moreover, while we find no significant effect on the vote shares of the two major parties — namely the conservatives (CDU/CSU) and the social democrats (SPD) — we find evidence that both parties responded to the inflow by nominating more expellees as direct candidates in federal elections. This is remarkable given that the expellees were in the minority in all constituencies, and that nominating a candidate comes with a large opportunity cost for a party. We also explore the extent to which the observed policy responses depended on the cultural similarity between the expellees and the local population. While we find strong responses in cities where the two groups were similar in terms of religion and country of origin, we observe no significant responses in areas where the two groups were culturally different. These results are consistent with political economy models such as Alesina et al. (1999), which predict that ethnically-diverse areas are less likely to agree on the optimal policies and therefore choose lower levels of taxation and spending than ethnically more homogeneous areas.

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 counties with high inflows of expellees to those living in low-inflow counties. IV estimates show that more than 50 years after the expulsions individuals in high-inflow counties show substantially stronger preferences for redistribution than those in low-inflow counties. This suggests that the arrival of the expellees is partly responsible for the significant differences in preferences for redistribution across German regions 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 of 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 occur within rather than between countries. Many 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 taxation and spending.

Besides these general lessons, the paper contributes to four strands of literature. First, it provides

an empirical test of standard political economy models of the welfare state, which deliver different predictions depending on the political influence of the immigrants. The seminal model by Meltzer and Richard (1981) predicts that as societies become more unequal, the median voter shifts to poorer segments of society, resulting in greater support for higher taxes and spending. While this model does not consider immigration, it yields predictions for post-war Germany given that the expellees were poor and had voting rights. Other models predict that immigration leads to lower taxation and spending, either because immigrants benefit from spending while taxes are predominantly paid by natives (Razin et al., 2002), or because ethnically-diverse groups disagree on the optimal level of taxation and spending (Alesina et al., 1999). These theories have been recently tested by Tabellini (2018a,b) for two migration waves in the US, namely the mass immigration of Europeans in the early 20th century and the Great Migration of blacks into northern US cities in the mid 20th century. He shows that both inflows led to lower public spending, and in the case of the European immigration, to lower local taxes.[3] The policy responses in post-war Germany are in stark contrast to those found in the US. Consistent with the Meltzer and Richard (1981) model, we find that the immigration of poor people resulted in *higher* taxes and spending and, more generally, greater support for redistributive policies. We see two differences to the US as critical for explaining these results. First, unlike the European immigrants in the US, the German expellees had voting rights and thus could influence policy setting. Second, in contrast to the Great Migration of blacks, there was no selection into migration among the expellees and there was no significant response of the local population moving out of the destination cities.

Second, our work contributes to the literature on the determinants of preferences for redistribution. Several studies document that individual preferences for redistribution are determined by exposure to political and economic conditions early in life, such as growing up during a recession or under a different political system (Corneo and Grüner, 2002, Alesina and Fuchs-Schündeln, 2007, Giuliano and Spilimbergo, 2014, Fuchs-Schündeln and Schündeln, 2015). Our results indicate that such shifts in preferences triggered by historical events can persist over multiple generations. We find that people in areas with high inflows in the 1940s show greater support for redistribution more than 50 years later. These long-run effects complement recent findings by Alesina et al. (2018b), who show that people holding severe misperceptions about the immigrants show lower support for redistribution, and that this cannot be changed by providing more accurate information about immigration. By contrast, our findings paint a more optimistic picture: people living in areas that had gone through the painstaking experience of integrating poor immigrants in the post-war years show *greater* support for redistribution 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 through 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

[3] Similar evidence has been found in Sweden by Dahlberg et al. (2012), although the original findings have been called into question by Nekby and Pettersson-Lidbom (2017).

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

— 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 individuals and 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 they 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) and investment in education (Semrad, 2015, Becker et al., 2018), as well as demonstrating the persistence of population shocks (Schumann, 2014, Braun et al., 2017) 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 WW 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 and debt. 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. Finally, Section 6 concludes.

2 Historical and Institutional Background

The expulsion and resettlement of over 12 million Germans in the aftermath of WW 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 local 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

[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, 2018) and the forced resettlement of parts of the Finnish population after WW II (Sarvimäki et al., 2016).

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 B.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 in 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 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 the size of the inflow 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 substantially fewer 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

[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.

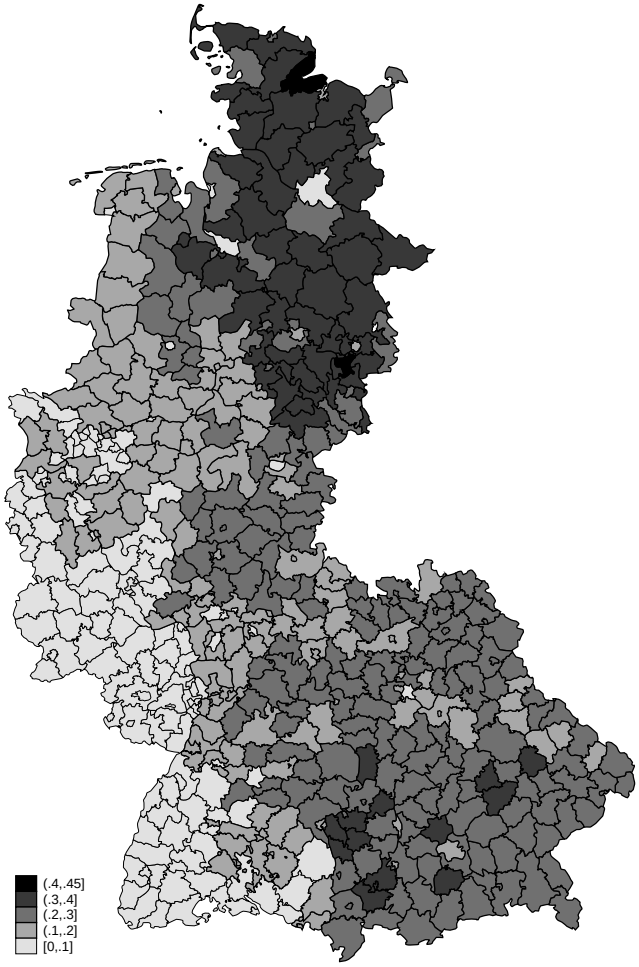
[7] Several state-level yearbooks provide data from as early as 1948. For the whole of West Germany such data is only available for 1950.

[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.

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. Consequently, the expellees were mostly allocated to rural areas and smaller cities, where the destruction of the housing stock was less severe (Henke, 1985).[9]

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 the Max Planck Institute for Demographic Research (MPIDR) and the Chair for Geodesy and Geoinformatics, University of Rostock (2011).

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

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 B.2 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. However, the two parts differed in their religious composition: compared to West Germany, the Eastern territories had a significantly higher share of Protestants, such that the inflow of the expellees changed the local religious composition of the population in some regions in the West (Kossert, 2008, ch. 7).

The economic and social integration of the expellees presented a major challenge to the West German society. Historical accounts document that the West German population was anything but welcoming towards the expellees. While natives and migrants shared the same ethnicity and citizenship, many West Germans expressed their hostility towards the expellees, in an episode described as "*racism of Germans against German expellees*" (Kossert, 2008, ch. 4).

A key difference between the two population groups was the severe poverty of the expellees relative to the native population. During the resettlement, 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. Moreover, unlike the expellees, West Germans could draw on their pre-existing social networks to find employment or obtain loans.

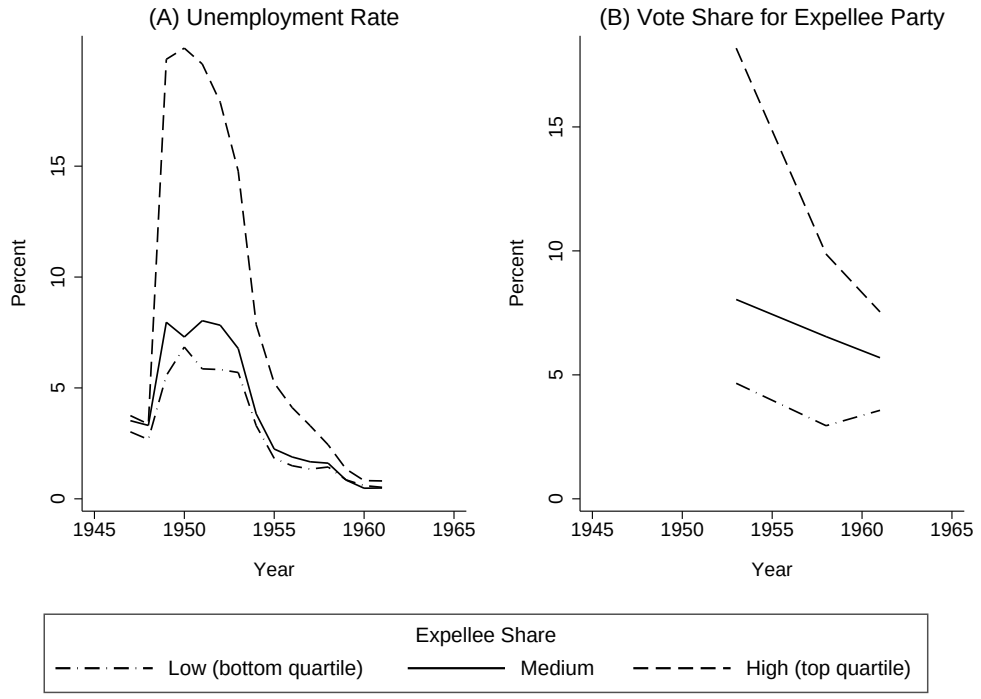
The provisional West German government and the Allied Forces initiated a set of comprehensive policies to improve the economic conditions of the expellees (*Soforthilfegesetze*), which included basic social assistance, once-off transfers to families, subsidies for education and training, credits for business creation, and funds for housing construction (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). While the transfers from both programs were provided to individuals and companies, there was no comprehensive transfer program between regions.

Despite these programs, the initial economic disadvantage of the expellees led to persistently lower earnings and higher unemployment.[10] 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 — namely 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

[10] Evidence abounds that the German government'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, home ownership rates and education.

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: Descriptive Evidence: Mass Immigration, 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 inflows of expelled Germans. See Appendix Table A.1 for definitions of the variables and the underlying data sources.

Citizenship and political representation of the expellees. In contrast to most other immigrants, the expellees were considered German citizens upon arrival, which 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; and second, they had full voting rights in local, state and federal elections.[11]

Panel (B) of Figure 2 provides descriptive 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

[11] 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 at the state and local level contained similar provisions.

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 account for their needs when setting public policies.

2.2 Local Public Policy in West Germany

Our outcomes of interest are policy variables that were — and still 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 cities and municipalities in the German Reich (*Deutsche Gemeindeordnung*). The general principles of this code served as the basis for the fiscal self-government rules of cities in West Germany after WW II and — with some modifications — remain in place until today.[12]

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.[13] 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 comprises two elements: the uniform basic rate (*Steuermesszahl*), which is set by the federal government, and the city-specific tax collection rate (*Hebesatz*). We focus on the five most important local tax rates, namely:

- *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; and
- *Wage Bill Tax (Lohnsummensteuer)*, a tax on a firm's total wage bill.[14]

Over the sampling period, these five taxes accounted for up to 90% of cities' overall tax revenue 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.

[12] 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.

[13] 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 mayor.

[14] 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.

Given that the expellees initially owned neither 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, 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 were — and still are — also 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, and 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 regarding welfare provision. Before 1962, there were no unified and clear rules about the provision of benefits, such that benefit levels varied across cities. The payments largely 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 other than welfare (Willing, 2001, Föcking, 2009).

Debt. A further source of revenue for cities was public debt. Initially, municipalities' post-war level of debt was close to zero after the currency reform of 1948, which implied that 90% of the pre-1948 debt in Reichsmark was effectively eliminated (10 RM of debt became 1 DM of debt) while 100% of municipalities' deposits in Reichsmark were invalidated. Consequently, municipalities' aggregate debt was 106 million Euros or two Euros per capita in 1950, corresponding to 1.2% of overall public debt (the sum of debt at the federal, state and municipal level). Starting in 1950, municipalities' indebtedness continuously increased to 241 Euros per capita in 1965, corresponding to 31.9% of overall public debt (see Statistisches Bundesamt (2016), Table 1.1).

3 Main Analysis: Immigration and Public Policy Setting

In this section, we analyze whether West German cities responded to the inflow of roughly eight 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 assumptions. While it may appear mechanical that cities with high inflows of poor immigrants were forced to raise welfare spending and, consequently, needed to raise 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 to provide a nuanced picture of the impact of the expellee 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 responded 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 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 WW II, cities had every reason to provide an incentive for construction by keeping 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.[15] The fact that we observe tax rates before and after the inflow of expellees allows us to estimate a causal effect using a DiD design with a continuous treatment.

Simple 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 tax rates after WW II (see Appendix Figure A.1), 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

[15] *Statistische Jahrbücher Deutscher Gemeinden*; see Appendix Table A.1 for details. Our sample period ends in 1965 because municipalities could no longer be exactly matched thereafter. Starting in 1966, several territorial reforms changed the definition of city and municipality boundaries.

those with lower shares.

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 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.[16]

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 after the war, 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 were 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 — as well as the respective share of civil servants and unemployed workers in 1933, and the (log) population density in 1939 — proxies

[16] 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.

for economic prosperity before the war. All data on pre-war social and economic differences are 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 arrival of the expellees.

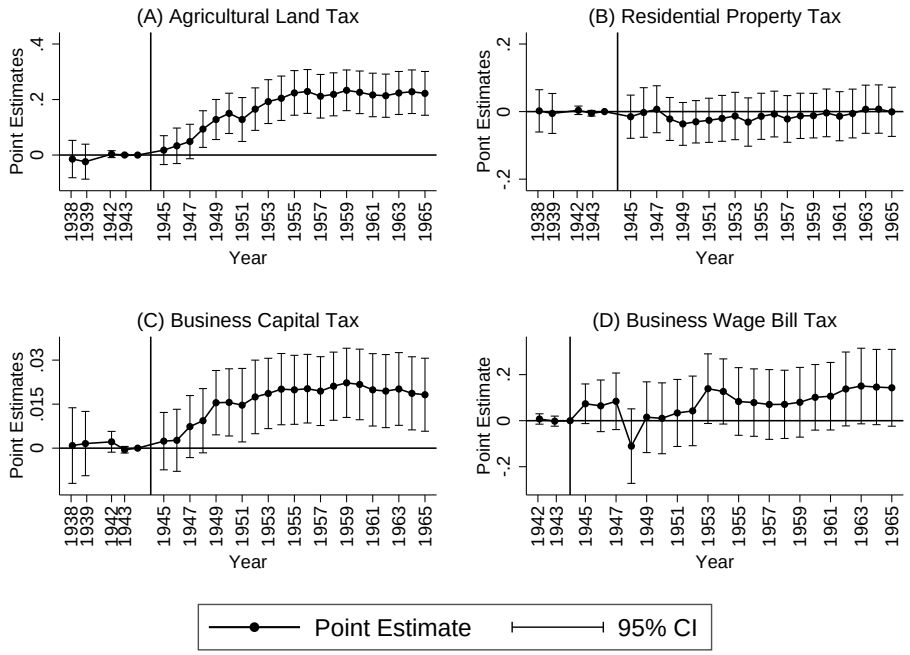
Effects on local tax rates. Figure 3 displays the estimated coefficients of our DiD approach for the four tax rates of interest.[17] To make the effects comparable across outcomes, we standardize the share of expellees by dividing it by the sample standard deviation. The vertical line marks the arrival 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 with a high inflow of expellees raised tax rates on agricultural land and firms' capital (Panels (A) and (C)), while we find no effect on taxes on residential property and firms' wage bill (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 a raise in the capital tax by 0.015 percentage points, corresponding to 3% of the mean.

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. 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

[17] 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. Because by law the taxes on a firm's profits and capital have to be identical, we only report here the results for the tax on capital.

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.

disequilibrium, would swiftly revert to those of low-inflow cities. One 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, a spatial equilibrium with diverging tax rates is sustainable because firms that move to places with lower taxes would lose parts of 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 gave 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.[18] 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* 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

[18] 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 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).

affected tax rates but — importantly — do not confound the estimation of a causal effect.

In a set of robustness checks, shown in Appendix C, we re-estimate the above regressions with different sets of controls. The results, displayed in Tables C.1–C.4, 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 and statistically significant, they are close to zero and statistically insignificant once we add these controls. The results without controls suggests that areas with different historical institutions differed in their pre-1945 tax rates as well as the number of expellees post 1945, which may also explain why we find larger coefficients after 1945. Once these 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 1945 — the pre-trends remain insignificant and the estimated post-war differences are stable. We also test whether the effect is non-linear with respect to the size of the inflow 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 items other than welfare 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 detail. 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 expellee share $ExpShare_c$ 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 expellee share in a West German city with the interaction of two gravity forces, namely a "push" factor in the sending regions that is only relevant before WW II as well as the geographic proximity of the sending regions to a given city in the West.[19] The push factor is the

[19] 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).

number of Germans living in each county i in the ceded Eastern Territories of the German Reich 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 the Euclidean distance between source-destination pairs. It is generally established that distance represents 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, after a tedious journey it was less costly to move to a place closer to the border. A similar argument holds for the Allied Forces, who temporarily hosted expellees in refugee camps along the border, and for whom it was less costly to assign the expellees to places in closer proximity.

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[20] (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

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

As shown in Appendix Figure B.3, there is a strong negative correlation between the two 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 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* WW 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

[20] County-level information on pre-war population is taken from the census in 1939, see Appendix Table A.1. In 1938, following the Munich Agreement, Sudeten was annexed by the German Reich, which is why population statistics on the Sudeten are available in the German census.

knowledge flows. However, 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 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 the two is informative about the validity of the instrument given that the DiD approach 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 inference through non-parametric permutation tests.

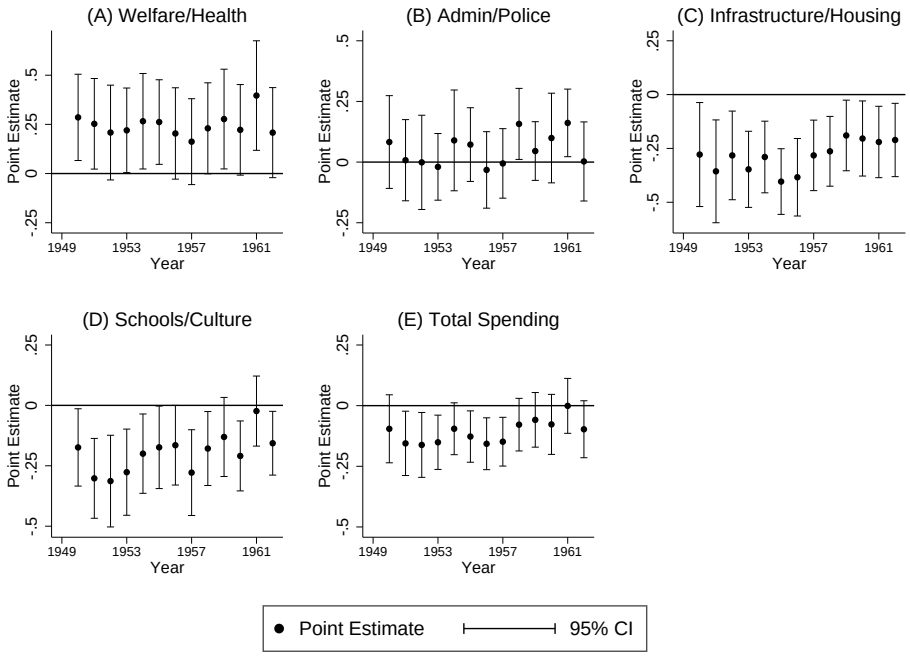
Effects on Tax Rates Revisited: Testing the Plausibility of the Exclusion Restriction. The fact that tax rates are available for the pre-treatment period provides us with the opportunity to corroborate the instrument validity by comparing the DiD to the IV estimates. Because the DiD approach in Section 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 the 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 C.1 in the appendix, confirm that the IV and DiD estimates are indeed similar. We find no significant effect of the expellee share on tax rates before 1944, while the effects after 1944 are similar in terms of both 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. To this end, we collected panel data on cities' expenditures from the Statistical Yearbook of German Municipalities for the period 1950-1962.[21] In the regressions, we use as outcomes log per capita spending (in 1950 DM) 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 coefficient 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, 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,

[21] As detailed in Appendix A, information on the local spending, debt and voting are only available for larger cities. The effects on tax rates are virtually identical when limiting our sample to those cities for that we observe local spending, debt and electoral results, see Figures C.2 and C.3.

[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.

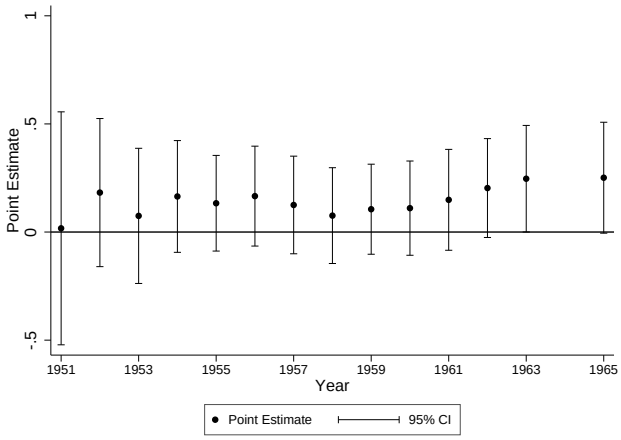
with per capita GDP doubling within a decade (Eichengreen and Ritschl, 2009). However, as shown by Bauer et al. (2013), even in the 1970s the expellees were lagging behind the incumbent population in terms of labor force participation, employment, income and home ownership. Therefore, it is plausible 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 faded 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 at least until the 1950s.

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 1950 DM).

In contrast to the strong and precisely estimated effects on spending, the evidence on debt is weaker. Most point estimates indicate a positive effect 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 of the estimates, we cannot rule out that the effects are zero.

Figure 5: The Effect of Mass Migration on Local Debt - 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.

Robustness and Plausibility Checks. In Appendices C to E, 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 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 the two coefficients may be explained by either 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 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 of Conley et al. (2012), we assess the robustness of the causal inference to violations of the exclusion restriction of the IV. The basic 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 E, we perform a plausibility test by calculating the maximum correlation for which the IV coefficient would still be statistically significant at the 10% level. The results suggest that our estimates are 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. 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 and Ethnic Diversity

The results of the previous section show that the arrival of eight million forced migrants changed public policy setting in West Germany. Cities with high inflows adopted more redistributive policies; they selectively raised taxes, spent more on social welfare while reducing spending on other items. In this section, we assess whether our results are consistent with standard political economy models of the welfare state. These models highlight two mechanisms, namely shifts in voting — a higher share of poor people increases the vote share for parties in favor of redistribution — and ethnic diversity — higher diversity leads to lower support for redistribution. In the following, we provide evidence of the relevance of both mechanisms.

4.1 The Political Economy of the Welfare State

The impact of the expellee inflow on local taxation and spending can be viewed through the lens of two broad theories of the political economy of the welfare state. One theory, pioneered by Meltzer and Richard (1981), links income inequality to the size of the welfare state. In this model, greater inequality shifts the median voter towards poorer segments of the electorate, who benefit from transfers while paying little in taxes. Consequently, areas with greater inequality are expected to support higher taxes and more generous transfers. Because the expellees were poorer than the local population in most cities, their mass inflow meant an increase in the share of poor voters, which could give rise to the *inequality effect* proposed by this theory.

A second theory links ethnic diversity to the size of the welfare state. The model of Alesina et al. (1999) predicts that areas with a greater ethnic diversity choose to have lower taxation and spending. This effect may arise if groups have different preferences over the optimal policy, leading to a compromise of lower taxes and transfers; alternatively, it may arise if the majority derives a disutility from sharing public goods with the minority. For our setting, the *diversity effect* proposed in this model could be relevant in light of the historical evidence suggesting that the local population initially met the expellees with resentment (Kossert, 2008, ch. 4).

The results presented so far — an increase in the share of poor voters leads to higher taxes and welfare spending — are broadly consistent with Meltzer and Richard (1981). On the other hand, the fact that high-inflow cities reduced their spending on infrastructure, schools and public order is in line with Alesina et al. (1999). In fact, both models emphasize different underlying mechanisms and, therefore, are not mutually exclusive. It is possible that in some cities the diversity effect was stronger while in others the inequality effect dominated.

4.2 Evidence on the Political Influence of the Expellees

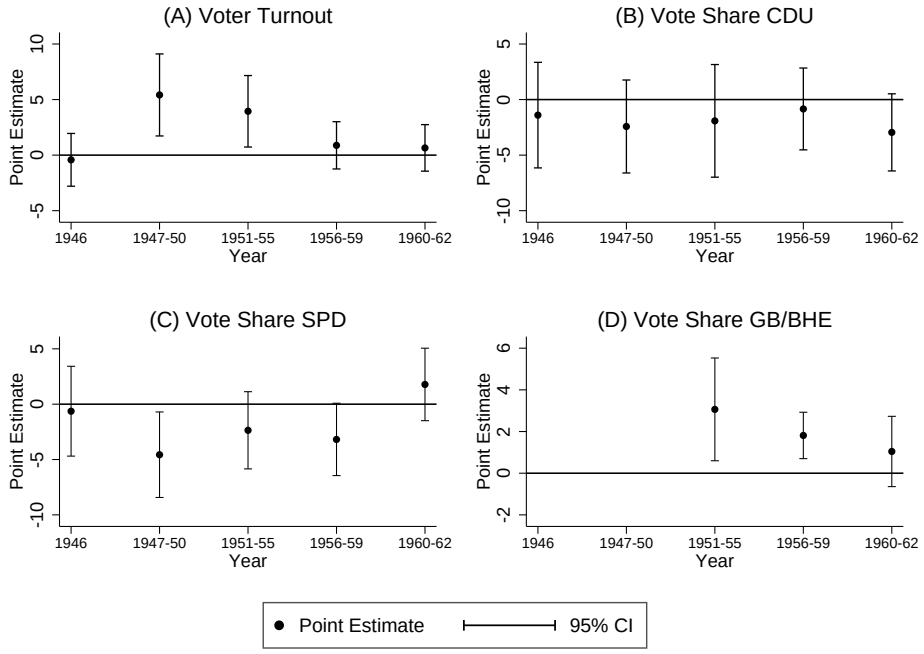
A critical mechanism highlighted in the model by Meltzer and Richard (1981) is that greater inequality shifts the median voter towards poorer segments of the electorate. In our context, this would imply higher vote shares for parties that are in favor of redistribution. Motivated by this idea, we investigate whether higher expellee inflows changed voting patterns in local elections, and if so, whether the observed changes are in line with the theory. We first consider the effect of the expellees on voting in municipal elections. For this purpose, we collected data on voter turnout and party vote shares for city elections from the Statistical Yearbook of German Municipalities for the period from 1946 to 1962.[23] Municipal elections are the relevant elections in this context, because local tax rates and spending are decided by municipal and city councils, which are elected every four to five years. German municipal elections have very low thresholds for parties to gain seats in the councils. Most states had instituted electoral systems following proportional representation at the local level, such that vote shares translated almost 1:1 into seat representations. Local 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. We consider four outcomes, namely voter turnout, the vote shares of the two major parties — the

[23] See Appendix Table A.1 for details and Figure A.1 for the evolution of voting outcomes over time.

conservatives (CDU/CSU) and the social democrats (SPD) — as well as the vote shares of the GB/BHE, a party that specifically represented the interests of the expellees.[24]

Voter turnout. The effect on voter turnout provides indirect evidence of the economic and political stakes of different groups in a local election. Standard voting models predict that an increase in the size of the voter base decreases turnout because each vote is less likely to be decisive (Downs, 1957). In contrast, if a larger voter base increases the stakes of different groups in the election, voter turnout may actually increase (Andersen et al., 2014). This may be the case — for instance — if rich voters support low taxes while poor voters demand more redistribution. Panel A of Figure 6 presents the IV estimates for the effect of the expellee inflow on voter turnout. In the early 1950s, a larger inflow of expellees significantly increased 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 suggests that the expellee inflow affected the voting behavior of the population.

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.

[24] The abbreviation stands for "*Gesamtdeutscher Block/Bund der Heimatvertriebenen und Entrechteten*" (All-German Bloc/League of Expellees and Deprived of Rights).

Vote Shares of the Major Parties (CDU/CSU and SPD). The effect on vote shares for the two major parties can further indicate shifts in people's preferences for redistribution. Among both parties, the SPD has traditionally supported a more generous welfare state, while the CDU/CSU has pursued more business-friendly policies. However, it should be noted that the parties' positions on the size of the welfare state have not been as divergent as in the US or the UK. Moreover, in line with Germany's federal political culture, both parties tolerate that local policies diverge from the party line. In Panels (B) and (C), we investigate the impact of the expellee inflow on the vote shares of the two major parties. During the sample period, 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. These findings provide no evidence that the inflow led to an increase in the vote share for the party that traditionally supports a larger welfare state.

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 programs to address the expellees' needs. In fact, historical accounts suggest that the CDU/CSU and SPD explicitly competed for the votes of the expellees by promising a fast improvement of their economic situation in West Germany as well as advocating the possibility of reclaiming their lost properties in the ceded territories (Kossert, 2008, pp. 165). Therefore, the expellees may have influenced local politics even without significantly affecting the local vote shares for the two major parties. In this context, Figure B.5 provides further suggestive evidence for the expellees' political influence. It shows that in federal elections parties were more likely to select expellees as direct candidates in electoral districts with higher expellee shares. For this purpose, we collected biographical data for all direct candidates in the federal elections from 1949 and 1961. These data include information on a person's birthplace, and we consider as expellee every candidate who was born outside the post-1945 borders of Germany in the expellees' regions of origin. The figure implies that the expellees indirectly influenced politics through the choice of candidates. This is remarkable because each party can only nominate one direct candidate per constituency, such that the decision for any particular candidate comes with high opportunity costs. Moreover, as newcomers, the expellees could not rely on the same social connections that are helpful for becoming a candidate within a party.

Vote Share of the Expellee Party (GB/BHE). Last, the effect on vote shares for the GB/BHE can provide an indication of the expellees' direct influence on local politics. The party was founded in 1950 and explicitly represented the expellees' interests. It pursued two main goals, namely improving the economic situation of the expellees in West Germany, and lobbying 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 1961. In Panel (D), we find that 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 increased the party's vote share by around 2pp, which is equivalent to 27% of the mean of 7.2% (see Appendix Tables C.21). In Figure B.4 in the appendix, we additionally show that these vote shares are highly correlated with actual seats in

municipal councils, suggesting that in cities with higher shares of expellees, the expellees actually had a greater political representation. These findings provide 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 higher inflows.

4.3 The Role of Ethnic Diversity.

To assess the role of ethnic diversity in explaining the effect of migration on public policies, we exploit that in some regions the expellees were more similar to the local population than in others. If we observe weaker policy responses in areas with more pronounced cultural differences between both groups, this would be evidence of the diversity effect discussed in Alesina et al. (1999).

We use tax rates as outcome, which allow us to estimate a triple-DiD model of the form

$$\begin{aligned} tax_{mct} = \beta_1(ExpShare_c \times post_t) + \beta_2(ExpShare_c \times post_t \times diff_c) + \beta_3(diff_c \times post_t) \\ + \rho(X_{mc} \times post_t) + post_t + \phi_m + \varepsilon_{mct}, \end{aligned} \tag{5}$$

where $post_t$ is a binary indicator that equals one for all years after 1944 and zero otherwise, and $diff_c$ is a binary indicator that equals one if the expellees and the local population are culturally very different. The vector X_{mc} includes the same control variables as before.

We focus on two indicators of cultural difference, namely differences in terms of religion and the country of origin. Before 1939, around half the overall German population was catholic while the other half was protestant. However, because catholics were more concentrated in the South West and Protestants more in the North East, the majority of the local West German population was catholic whereas most expellees were protestant.[25] Recent work by Braun and Dwenger (2017) identifies religion as a key determinant of the economic and social integration of the expellees. Regions where most expellees had a different religious denomination from the local population had lower intermarriage rates and higher degrees of polarization in votes for pro and anti-expellee parties. This suggests that expellees of a different denomination were more likely to be considered a separate ethnic group. To construct a binary indicator for high religious diversity, we first calculate for each county the difference in the shares of catholics among both groups, and classify as *high diversity* those counties in the upper quartile of the distribution.

A further indicator of cultural difference is the country of origin of the expellees. More than half of all expellees came from within the German Reich and, thus, had lived in the same country as the local West German population. By contrast, many other expellees came from German speaking communities in countries such as Czechoslovakia, Romania or the Soviet Union. Many German communities had lived in the respective countries for centuries and, over time, nurtured their distinct norms and customs. It appears plausible that West Germans viewed the newcomers from the German Reich as part of their own ethnic group and those coming from other countries as part of a different one. To test for different policy responses by country of origin, we first calculate for each country the

[25] See Figure B.2 in the Appendix for further information.

share of expellees who came from outside the 1937 borders of the German Reich. We then construct a binary indicator that equals one if this share is in the upper quartile among all counties.

Table 1: The Effect of Mass Migration on Tax Rates - Heterogeneous DiD Results

	Baseline DiD	Heterogeneous Effects	
	(1)	(2)	(3)
Panel A – Agricultural Land Tax			
Expellee Share × Post War	0.172***	0.176***	0.196***
	(0.034)	(0.034)	(0.033)
Expellee Share × Post War × Large Religious Differences		-0.110*	
		(0.061)	
Expellee Share × Post War × Many Expellees Outside GER Territory			-0.154**
			(0.065)
Number of Observations	9,820	9,820	9,820
Panel B – Residential Property Tax			
Expellee Share × Post War	-0.017	-0.013	-0.014
	(0.029)	(0.029)	(0.032)
Expellee Share × Post War × Large Religious Differences		-0.055	
		(0.050)	
Expellee Share × Post War × Many Expellees Outside GER Territory			-0.017
			(0.046)
Number of Observations	9,822	9,822	9,822
Panel C – Business Capital Tax			
Expellee Share × Post War	0.015***	0.015***	0.018***
	(0.005)	(0.005)	(0.005)
Expellee Share × Post War × Large Religious Differences		-0.009	
		(0.012)	
Expellee Share × Post War × Many Expellees Outside GER Territory			-0.019**
			(0.008)
Number of Observations	9,822	9,822	9,822
Panel D – Business Wage Bill Tax			
Expellee Share × Post War	0.074	0.075	0.084
	(0.053)	(0.054)	(0.059)
Expellee Share × Post War × Large Religious Differences		-0.171	
		(0.139)	
Expellee Share × Post War × Many Expellees Outside GER Territory			-0.029
			(0.070)
Number of Observations	3,773	3,773	3,773

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 (5) and allowing for differential effects by the expellees' composition. Coefficients are allowed to vary by differences in the religious composition of the migrant and native population, as well as by the share of expellees that came from outside the pre-war German territory. In Column (2) we interact the expellee share with a dummy variable that turns one if differences between the natives' and migrants' religious composition were in the upper quartile of the distribution. In Column (3) we use a dummy variable that turns one if the share of expellees from outside the pre-war German territory was in the upper quartile of the distribution. All specifications include city-level fixed effects and our most comprehensive set of controls (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 1 displays our estimates for the four tax rates.[26] The DiD results in Column (1) confirm our previous finding that cities with a higher inflow of expellees raised taxes on farmland and business

[26] We only focus on taxes because the panel data allow us to estimate DiD models. For spending and debt, our panel data do not go back to the pre-treatment period.

capital but neither on residential properties nor businesses' wage bill. In Columns (2) and (3), we report the interaction effects of the DiD model displayed in Equation (5). The estimates indicate a significant degree of heterogeneity in the policy response, with much smaller effects in cities with large cultural differences. Whereas in areas with high similarity the effects on farm and business capital tax rates is as large as in the average city, the effects are close to zero in areas where the expellees and the local population were very different. These results suggest that the policy responses were concentrated in areas where the expellees were more likely to be part of the German in-group rather than being considered a distinct ethnic group.

Overall, these results suggest that, in areas where the expellees were part of the in-group, the expellees and the local population could reach an agreement on the optimal tax policy. By contrast, in areas where the expellees were more likely considered an out-group, taxes were not raised, which is in line with the prediction of Alesina et al. (1999).

4.4 Robustness Checks.

In Appendix C, 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 D, 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 but reverted to zero after 1955, which may 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 (Conley et al., 2012). 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 equal to 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

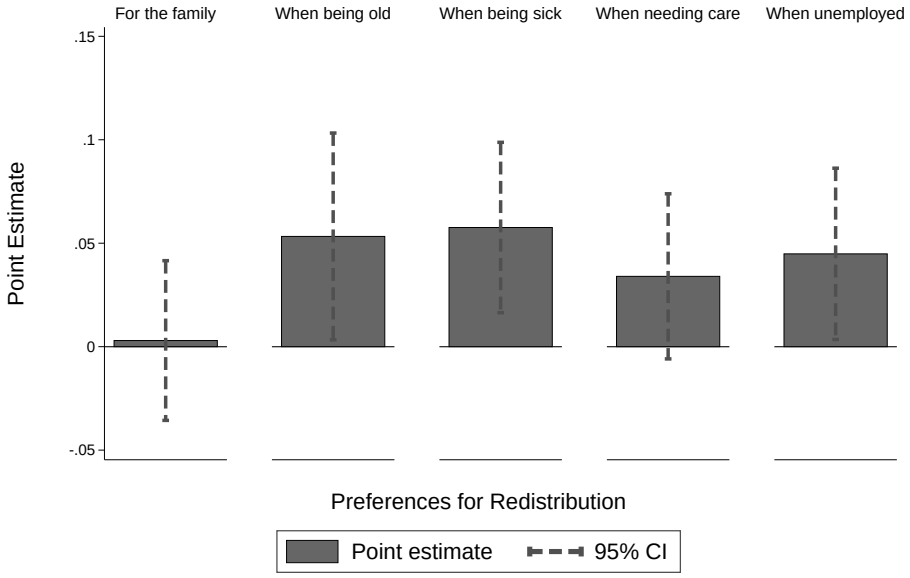
Thus far, our analysis has documented a short to medium-run effect of the expellee inflow on redistribution. Cities with high inflows almost immediately implemented more redistributive tax and spending policies, and these changes were persistent until at least the mid-1960s. In this section, we turn to the long-run effects and investigate whether the impact of the expellee inflow persists over several decades.[27] Based on survey data from the early 2000s, we study the extent to which 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. 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. The 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

[27] 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.

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").[28]

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 employ 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. In terms of 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 respective variable means (for further details see Appendix Tables A.3 and C.22).

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 C.22, 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 that excludes these variables.

These results suggest that the sudden arrival of eight million expellees was a sufficiently large

[28] For each domain, we use as outcome the answer score between 1 and 5. See A.1 for a detailed description.

shock to persistently change the preferences of society. There are several plausible explanations for this long-run effect, one of which is the intergenerational transmission of preferences. 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 of the expellees. These experiences may have shaped the local narrative about poverty and redistribution, and may have been passed on to the next generations.[29] Another potential explanation is endogenous sorting based on preferences (Tiebout, 1956). The inflow of expellees triggered 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 the impact of the expellee inflow had a lasting effect on preferences for redistribution. People who live today in cities that received more expellees more than 50 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. However, 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, 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 immigrants' voting rights. Unlike most international migrants, the expellees had voting rights from the time of arrival, which meant that 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 voting rights to these immigrants. For example, intra-EU migrants 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. Over the past

[29] 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.

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.

References

Alesina, A., Baqir, R. and Easterly, W. (1999). Public Goods and Ethnic Divisions. *The Quarterly Journal of Economics* 114: 1243–1284.

Alesina, A. and Fuchs-Schündeln, N. (2007). Good-Bye Lenin (or Not?): The Effect of Communism on People's Preferences. *American Economic Review* 97: 1507–1528.

Alesina, A., Harnoss, J. and Rapoport, H. (2018a). Immigration and the Future of the Welfare State in Europe. *PSE Working Papers* 04.

Alesina, A., Miano, A. and Stantcheva, S. (2018b). Immigration and Redistribution. *NBER Working Paper* 24733.

Andersen, J. J., Fiva, J. H. and Natvik, G. J. (2014). Voting when Stakes are High. *Journal of Public Economics* 110: 157–166.

Andersson, F. and Forslid, R. (2003). Tax Competition and Economic Geography. *Journal of Public Economic Theory* 5: 279–303.

Baldwin, R. E. and Krugman, P. (2004). Agglomeration, Integration and Tax Harmonization. *European Economic Review* 48: 1–23.

Bauer, T. K., Braun, S. and Kvasnicka, M. (2013). The Economic Integration of Forced Migrants: Evidence for Post-War Germany. *Economic Journal* 123: 998–1024.

Becker, S. O., Grosfeld, I., Grosjean, P. A., Voigtländer, N. and Zhuravskaya, E. (2018). Forced Migration and Human Capital: Evidence from Post-WWII Population Transfers. *University of Warwick* mimeo.

Borjas, G. J. (1999). Immigration and Welfare Magnets. *Journal of Labor Economics* 17: 607–637.

Boustan, L. P. (2010). Was Postwar Suburbanization 'White Flight"? Evidence from the Black Migration. *The Quarterly Journal of Economics* 125: 417–443, doi:10.1162/qjec.2010.125.1.417.

Boustan, L. P., Fishback, P. V. and Kantor, S. (2010). The Effect of Internal Migration on Local Labor Markets: American Cities during the Great Depression. *Journal of Labor Economics* 28: 719–746, doi:10.1086/653488.

Braun, S. and Kvasnicka, M. (2014). Immigration and Structural Change: Evidence from Post-War Germany. *Journal of International Economics* 93: 253–269.

Braun, S. and Mahmoud, T. O. (2014). The Employment Effects of Immigration: Evidence from the Mass Arrival of German Expellees in Post-war Germany. *The Journal of Economic History* 74: 69–108.

Braun, S. T. and Dwenger, N. (2017). The Local Environment Shapes Refugee Integration: Evidence from Post-war Germany. *Discussion Paper Series, Department of Economics, University of St. Andrews* .

Braun, S. T., Kramer, A. and Kvasnicka, M. (2017). Local Labor Markets and the Persistence of Population Shocks. *Discussion Paper Series, Department of Economics University of St. Andrews* .

Burchardi, K. B. and Hassan, T. A. (2013). The Economic Impact of Social Ties: Evidence from German Reunification. *The Quarterly Journal of Economics* 128: 1219–1271.

Conley, T. G., Hansen, C. B. and Rossi, P. E. (2012). Plausibly Exogenous. *Review of Economics and Statistics* 94: 260–272.

Corneo, G. and Grüner, H.-P. (2002). Individual Preferences for Political Redistribution. *Journal of Public Economics* 83: 83–107.

Dahlberg, M., Edmark, K. and Lundqvist, H. (2012). Ethnic Diversity and Preferences for Redistribution. *Journal of Political Economy* 120: 41–76.

de Giorgi, G. and Pellizzari, M. (2009). Welfare Migration in Europe. *Labour Economics* 16: 353–363.

Dohmen, T., Falk, A., Huffman, D. and Sunde, U. (2012). The Intergenerational Transmission of Risk and Trust Attitudes. *Review of Economic Studies* 79: 645–677.

Douglas, R. M. (2012). *Orderly and Humane. The Expulsions of the Germans after the Second World War*. Yale University Press, New Haven.

Downs, A. (1957). *An Economic Theory of Democracy*. New York: Harper.

Duflo, E. (2001). Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence From an Unusual Policy Experiment. *American Economic Review* 91: 795–813.

Dustmann, C., Schönberg, U. and Stuhler, J. (2016). The Impact of Immigration: Why Do Studies Reach Such Different Results? *Journal of Economic Perspectives* 30: 31–56.

Eichengreen, B. and Ritschl, A. (2009). Understanding West German Economic Growth in the 1950s. *Cliometrica* 3: 191–219.

Falck, O., Heblich, S. and Link, S. (2012). Forced Migration and the Effects of an Integration Policy in Post-WWII Germany. *B.E. Journal of Economic Analysis & Policy: Topics* 12.

Föcking, F. (2009). *Fürsorge im Wirtschaftsboom. Die Entstehung des Bundessozialhilfegesetzes von 1961*. Oldenbourg Wissenschaftsverlag.

Fuchs-Schündeln, N. and Schündeln, M. (2015). On the Endogeneity of Political Preferences: Evidence from Individual Experience with Democracy. *Science* 347: 1145–1148.

Giuliano, P. and Spilimbergo, A. (2014). Growing up in a Recession. *Review of Economic Studies* 81: 787–817.

Giulietti, C. (2014). The Welfare Magnet Hypothesis and the Welfare Take-up of Migrants. *IZA World of Labor* 37.

Henke, K.-D. (1985). Der Weg nach Potsdam - Die Alliierten und die Vertreibung. In Benz, W. (ed.), *Die Vertreibung der Deutschen aus dem Osten*. Fischer Taschenbuch Verlag, Frankfurt am Main, 58–85.

Kaushal, N. (2005). New Immigrants' Location Choices: Magnets without Welfare. *Journal of Labor Economics* 23: 59–80.

King, G., Rosen, O., Tanner, M. and Wagner, A. F. (2008). Ordinary Economic Voting Behavior in the Extraordinary Election of Adolf Hitler. *Journal of Economic History* 68: 951–996.

Knies, G. and Spiess, C. K. (2007). Regional Data in the German Socio-Economic Panel Study (SOEP). *DIW Data Documentation* 17.

Kossert, A. (2008). *Kalte Heimat: Die Geschichte der deutschen Vertriebenen nach 1945*. Siedler Verlag, 4th ed.

Levine, P. B. and Zimmerman, D. J. (1999). An Empirical Analysis of the Welfare Magnet Debate Using the NLSY. *Journal of Population Economics* 12: 391–409.

Llull, J. (2017). The Effect of Immigration on Wages: Exploiting Exogenous Variation at the National Level. *Journal of Human Resources* doi: 10.3368/jhr.53.3.0315-7032R2, doi:10.3368/jhr.53.3.0315-7032R2.

Luthi, E. and Schmidheiny, K. (2014). The Effect of Agglomeration Size on Local Taxes. *Journal of Economic Geography* 14: 265–287.

Luttmer, E. F. P. (2001). Group Loyalty and the Taste for Redistribution. *The Journal of Political Economy* 109: 500–528.

Max Planck Institute for Demographic Research (MPIDR) and the Chair for Geodesy and Geoinformatics, University of Rostock (2011). *MPIDR Population History GIS Collection*. Rostock, (Partly based on Hubatsch, W. and T. Klein (eds) 1975 ff.: Grundriß der deutschen Verwaltungsgeschichte, Marburg, and Bundesamt für Kartographie und Geodäsie: VG 2500 Verwaltungsgebiete [Ebenen] 1:2.500.000, Stand 01.01.2009, Frankfurt am Main).

Meltzer, A. H. and Richard, S. F. (1981). A Rational Theory of the Size of Government. *Journal of Political Economy* 914-927: 3222–3255.

Merten, U. (2012). *Forgotten Voices. The Expulsion of the Germans from Eastern Europe after World War II*. Transaction Publishers, New Brunswick.

Moser, P., Voena, A. and Waldinger, F. (2014). German-Jewish Emigres and U.S. Invention. *American Economic Review* 104: 3222–3255.

Murard, E. and Sakalli, S. O. (2018). IZA DP No. 11613: Mass Refugee Inflow and Long-Run Prosperity: Lessons from the Greek Population Resettlement. *IZA Discussion Paper* 11613.

Nekby, L. and Pettersson-Lidbom, P. (2017). Revisiting the Relationship between Ethnic Diversity and Preferences for Redistribution. *Scandinavian Journal of Economics* 119: 268–287.

Ourednicek, M., Kupkova, L., Pospisilova, L., Simon, M., Svoboda, P., Soukup, M., Marvanova, P. and Krivka, M. (2015). Administrative boundaries of judicial districts 1930. Geodatabase. *Prague: Charles University, Faculty of Science, Urban and Regional Laboratory.* .

Peters, M. (2017). Refugees and Local Agglomeration: Evidence from Germany's Post-War Population Expulsions. *Yale University, mimeo* .

Ramsey, F. P. (1927). A Contribution to the Theory of Taxation. *Economic Journal* 37: 47–61.

Razin, A., Sadka, E. and Swagel, P. (2002). Tax Burden and Migration: a Political Economy Theory and Evidence. *Journal of Public Economics* 85: 167–190.

Razin, A. and Wahba, J. (2015). Welfare Magnet Hypothesis, Fiscal Burden, and Immigration Skill Selectivity. *Scandinavian Journal of Economics* 117: 369–402.

Redding, S. and Sturm, D. (2008). The Costs of Remoteness: Evidence from German Division and Reunification. *American Economic Review* 98: 1766–1797.

Sarvimäki, M., Uusitalo, R. and Jäntti, M. (2016). Habit Formation and the Misallocation of Labor: Evidence from Forced Migrations. *Aalto University, mimeo* .

Satyanath, S., Voigtländer, N. and Voth, H.-J. (2017). Bowling for Fascism: Social Capital and the Rise of the Nazi Party. *Journal of Political Economy* 125.

Schillinger, R. (1985). Der Lastenausgleich. In Benz, W. (ed.), *Die Vertreibung der Deutschen aus dem Osten*. Fischer Taschenbuch Verlag, Frankfurt am Main, 231–243.

Schmölders, G. (1955). Finanzierungsprobleme im Zusammenhang mit der wirtschaftlichen Eingliederung der Heimatvertriebenen. In *Schriften des Vereins für Sozialpolitik*. Verlag von Duncker & Humblot, Berlin, *6/II*.

Schumacher, M. (2006). M.d.B. - Die Volksvertretung 1946-1972.

Schumann, A. (2014). Persistence of Population Shocks: Evidence from the Occupation of West Germany after World War II. *American Economic Journal: Applied Economics* 6: 189–205.

Semrad, A. (2015). Immigration and Educational Spillovers: Evidence from Sudeten German Expellees in Post-war Bavaria. *Munich Discussion Paper* 7.

Senik, C., Stichnoth, H. and van der Straeten, K. (2009). Immigration and Natives' Attitudes towards the Welfare State: Evidence from the European Social Survey. *Social Indicators Research* 91: 345–370.

Statistisches Bundesamt (1953). *Statistisches Taschenbuch über die Heimatvertriebenen*. Wiesbaden.

Statistisches Bundesamt (2016). Schulden des Öffentlichen Gesamthaushalts. *Fachserie 14 Reihe 5* .

Tabellini, M. (2018a). Gifts of the Immigrants, Woes of the Natives: Lessons from the Age of Mass Migration. *Massachusetts Institute of Technology, mimeo* .

Tabellini, M. (2018b). Racial Heterogeneity and Local Government Finances: Evidence from the Great Migration. *Massachusetts Institute of Technology, mimeo* .

Tiebout, C. M. (1956). A Pure Theory of Local Expenditures. *The Journal of Political Economy* 64: 416–424.

Voigt, R. (1975). *Die Auswirkungen des Finanzausgleichs zwischen Staat und Gemeinden auf die kommunale Selbstverwaltung von 1919 bis zur Gegenwart*. Ducker und Humboldt, Berlin.

Wagner, G. G., Frick, J. R. and Schupp., J. (2007). The German Socio-Economic Panel Study (SOEP) - Scope, Evolution and Enhancements. *Schmöllers Jahrbuch* 127: 139–169.

Willing, M. (2001). Fürsorge/Sozialhilfe/Sozialfürsorge. In Bundesministerium für Arbeit und Sozialordnung und Bundesarchiv (ed.), *Geschichte der Sozialpolitik in Deutschland seit 1945, Band 2/1: Die Zeit der Besatzungszonen 1945-1949. Sozialpolitik zwischen Kriegsende und der Gründung zweier deutscher Staaten*.

Wilson, J. D. (1986). A Theory of Interregional Tax Competition. *Journal of Urban Economics* 19: 249–382.

Zodrow, G. R. and Mieszkowski, P. (1986). Pigou, Tiebout, Property Taxation, and the Underprovision of Local Public Goods. *Journal of Urban Economics* 19: 356–370.

Appendix

A Data Appendix and Descriptive Statistics

For the purpose of our analysis, we draw upon a variety of datasets, which we explain in detail below. First, to investigate the short- to medium-run effects of the expellee inflow on public policy setting, we collected and harmonized historical city- and county-level data from various (statistical) publications. Data on the county-level share of expellees as of 1950 was taken from the "Statistical Yearbook of Expellees" (*Statistisches Jahrbuch über die Heimatvertriebenen*), published by the Federal Statistical Office of West Germany in 1953. Our outcome variables on city-level tax rates, spending (by category), debt and voting have been collected from the "Statistical Yearbooks of German Municipalities" (*Statistische Jahrbücher Deutscher Gemeinden, Jhg. 1938-1965*). Data on tax rates are available from 1938 onwards, while information on public spending, debt and local elections are only given for the post-war period (1946-1965). In addition, the coverage of cities differs by outcome variable. Data on public spending, debt and electoral results are only given for cities (*Kreisfreie Städte*), as well as municipalities with at least 20,000 inhabitants. By contrast, data on tax rates is given for cities, as well as municipalities with at least 10,000 inhabitants. County-level control variables on institutional, economic and social differences prior to the inflow of expellees (i.e. prior to World War II) are taken from King et al. (2008) and are available for download from Gary King's website (`https://gking.harvard.edu/data`). Information on the local extent of destroyed housing stock after the war have been collected from the Federal Statistical Offices of the German States (*Landesämter für Statistik*); see Table A.1 for details. For the construction of our instrument, we collected county-level population data from the "Statistical Yearbook of the German Reich 1939" (for the ceded Eastern Territories of the German Reich), as well county-level data on the German population in Sudeten from Ourednicek et al. (2015). Euclidean distances between source and destination counties are calculated by means of historical shapefiles for the German Reich and the Czech Republic, provided by the Max Planck Institute for Demographic Research (MPIDR) and the Chair for Geodesy and Geoinformatics, University of Rostock (2011) and Ourednicek et al. (2015). To calculate the share of expellee candidates in federal elections, we used data from the German Statistical Office, which lists the names of all direct candidates for the German parliament in a brochure (*Die Wahlbewerber zum Deutschen Bundestag*). We additionally extracted biographical information on all candidates from Schumacher (2006).

Second, to analyze the long-run effects on individuals' preferences for redistribution, we use data from the German Socio-Economic Panel (SOEP) and link individual-level measures of preferences for redistributive policies to the local inflow of expellees using the respondents' county of residence at the time of the interview. Information on individuals' county of residence is available via remote computing (SOEPRemote), see Knies and Spiess (2007) for details.

Table A.1 defines all variables used in our analysis and details its corresponding source. Descriptive statistics for the set of city- and county-level variables are given in Table A.2, for individual-level outcomes and controls in Table A.3. For all time-invariant county-level variables (i.e. the share of expellees, the instrument, and the set of historical pre-war controls), we present descriptive statistics at the city level (N=431). For our outcomes, we provide statistics based on the respective full sample that covers multiple years. In addition to these statistics, Figure A.1 plots the evolution of our (city-level) outcome variables over time.

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 as well as municipalities 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 as well as municipalities 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 federal level and reassessed every three years. It is multiplied by a city-specific tax rate that comprises the uniform basic rate, which is set by the federal government, and the tax collection rate defined by each city 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	Information on annual spending 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 all cities as well as municipalities with at least 20,000 inhabitants in a given year.
Unemployment Rates	1946-1962	Information on local unemployment is taken from the "Statistical Yearbooks of German Municipalities". Information is given for all cities as well as municipalities with more than 20,000 inhabitants in a given year.
Voting results	1946-1962	Data on voter turnout and party vote shares in 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. We construct four different variables: (i) overall voter turnout, (ii) the vote share for the Christian Democrats (CDU/CSU), (iii) the vote share for the Social Democrats (SPD), and (iv) the vote share for the expellee party (GB/BHE). All West German cities as well as municipalities with more than 20,000 inhabitants are covered by the data.

continued

Table A.1 continued

Variable	Years	Source
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.
Gravity Forces (Instrument)		The logic of our instrument is described in Section 3.2. For its construction, we use county-level population data from the "Statistical Yearbook of the German Reich 1939" and Ourednicek et al. (2015). Distances between the ceded territories in the East (and Sudeten) to West Germany are calculated using shapefiles provided by the Max Planck Institute for Demographic Research (MPIDR) and the Chair for Geodesy and Geoinformatics, University of Rostock (2011).
Historical Economic & Political Differences	1925-1933	We account for historical economic and political differences by controlling for (i) the population share of Protestants as of 1925, (ii) the mean election vote share for the Social Democratic Party (SPD) in the elections between 1925 to 1933, and (iii) the respective share of civil servants and unemployed workers in 1933. All data are taken from King et al. (2008).
Housing destruction	1945-1950	Information on the extent of destroyed housing units 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.*

continued

Table A.1 continued

Variable	Years	Source
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 the pre-war population density in West German counties 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.
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 and the federal state of residence. In some specifications, we further control for individuals' (log) household income, the county-level employment rate and the county-level share of foreigners among the population. All variables are provided by the SOEP.
Individual Preferences for Redistribution	1997,2002	Respondents are asked about their preferred role of the state regarding 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*".
Panel E – Data on Direct Candidates in Federal Elections		
Expellee Candidates	1949-1961	The information on district candidates for the federal parliament (Bundestag) were collected from the German Statistical Office's publications of all candidates running for parliament in the 1949, 1953, 1957 and 1961 elections (*Die Wahlbewerber zum Deutschen Bundestag*) by parties and electoral districts. The number of districts was 242 in 1949/1953 and increased to 247 in 1957/1961 (due to the reunification with the Saarland). The candidate publications provide information on how the electoral districts are composed with respect to administrative county borders. This allows us to assign counties to electoral districts and compute the population-weighted expellee share by electoral district based on the county population share of expellees as of 1950 (Statistisches Bundesamt, 1953). About 90% of counties are nested in electoral districts. In the remaining cases where a county is split across more than one electoral district the population weights are adjusted accordingly. The 1950 expellee share by electoral district is then merged with biographical information on candidates running for West German parliaments after World War II provided in Schumacher (2006), which documents short biographies of candidates, in most cases including the place of birth. We were able to assign the place of birth to 4,273 out of 6,646 candidacies (about 64%), including individuals who ran in multiple elections over this period. Overall, 627 candidate birth places (14.7%) were assigned to expellees' regions of origin.

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 (in %)					
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
Debt and Spending (in 1950 DM)					
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
Welfare/Health	30.76	35.38	1.20	462.28	3,188
Admin./Police	26.99	12.82	-6.57	121.53	2,848
Infra./Housing	45.98	25.48	-1.10	320.83	2,848
Schools/Culture	38.23	17.33	4.89	288.73	2,848
Voter Turnout and Vote Shares (in %)					
Voter Turnout	74.49	7.73	42.50	93.40	997
Vote Share CDU/CSU	34.38	11.72	5.00	70.80	941
Vote Share SPD	38.86	9.59	7.40	70.00	997
Vote Share GB/BHE	7.10	4.69	0.60	28.70	354
Controls					
Occupation Zone USA	0.71	0.46	0.00	1.00	431
Occupation Zone UK	0.23	0.42	0.00	1.00	431
Occupation Zone France	0.06	0.24	0.00	1.00	431
City in former Prussia	0.66	0.47	0.00	1.00	431
City close to Iron Curtain	0.11	0.31	0.00	1.00	431
Log Pop. Density (1939)	5.50	1.10	3.63	8.17	431
Vote Share SPD (1924-1933)	0.19	0.09	0.02	0.45	431
Share Protestants (1925)	0.49	0.35	0.01	0.98	431
Share Unemployed (1933)	0.16	0.08	0.03	0.38	431
Share Civil Servants (1933)	0.04	0.03	0.02	0.39	431
Share Destroyed Housing	17.07	15.10	0.00	78.22	431
Instrument					
Distance to East (in 100km)	5.92	0.95	3.38	7.47	431

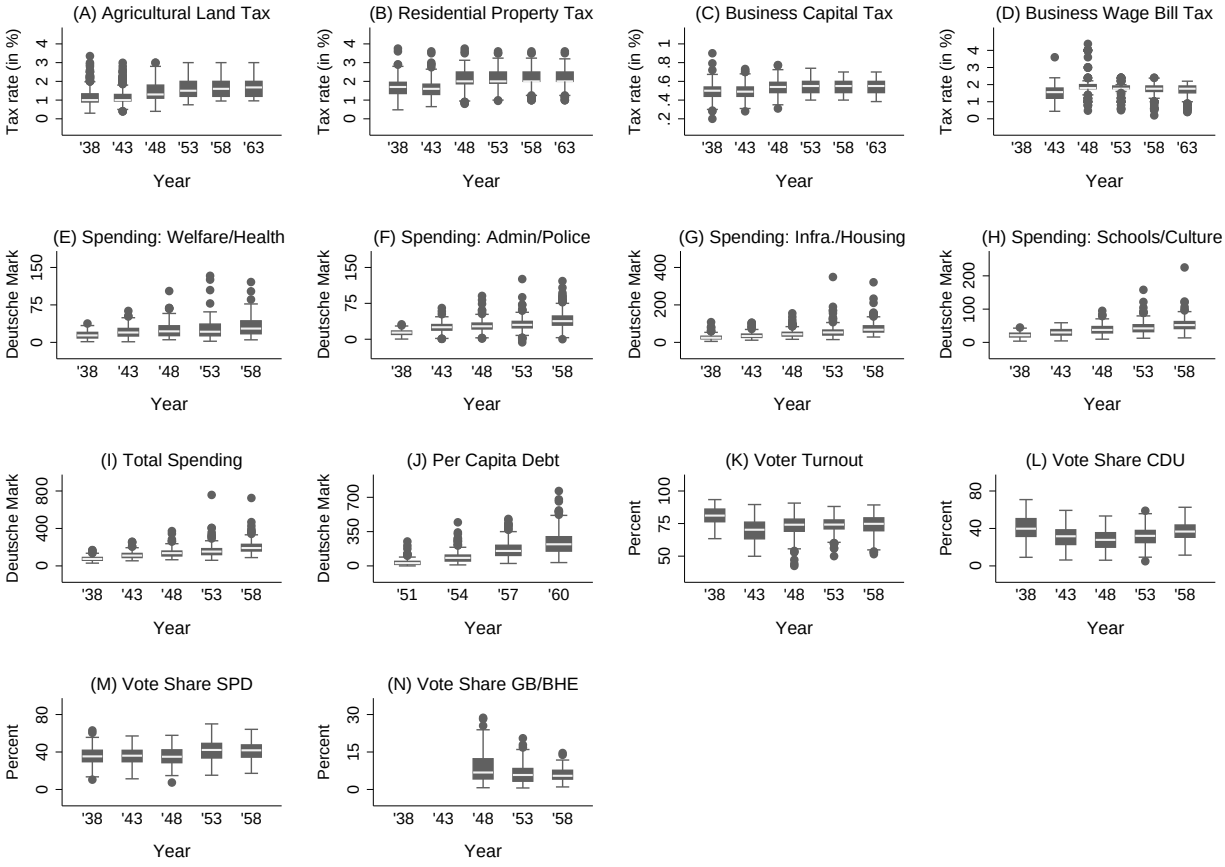
Notes: This table presents descriptive statistics for the outcome and control variables at the city and county level. All monetary variables are expressed in 1950 prices.

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 – 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 B – 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
(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-level outcome and control variables from the German Socio-Economic Panel. For detailed information on the variables' definitions, see Appendix Table A.1.

Figure A.1: Evolution of Outcome Variables Over Time

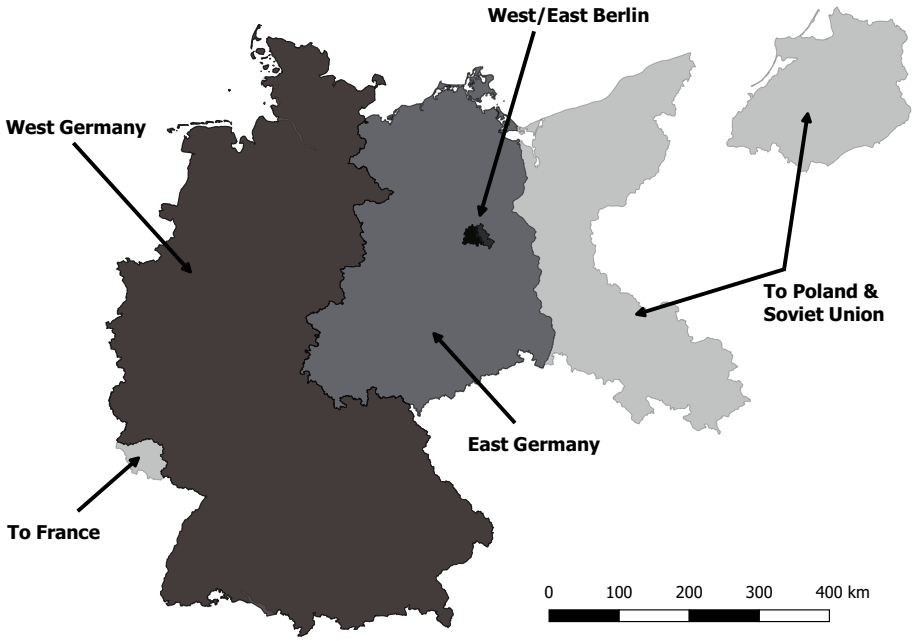


Notes: This graph plots the evolution of our outcome variables over time. See Appendix Tables A.1 and A.2 for a definition of each variable as well as additional descriptive statistics.

B Additional Figures

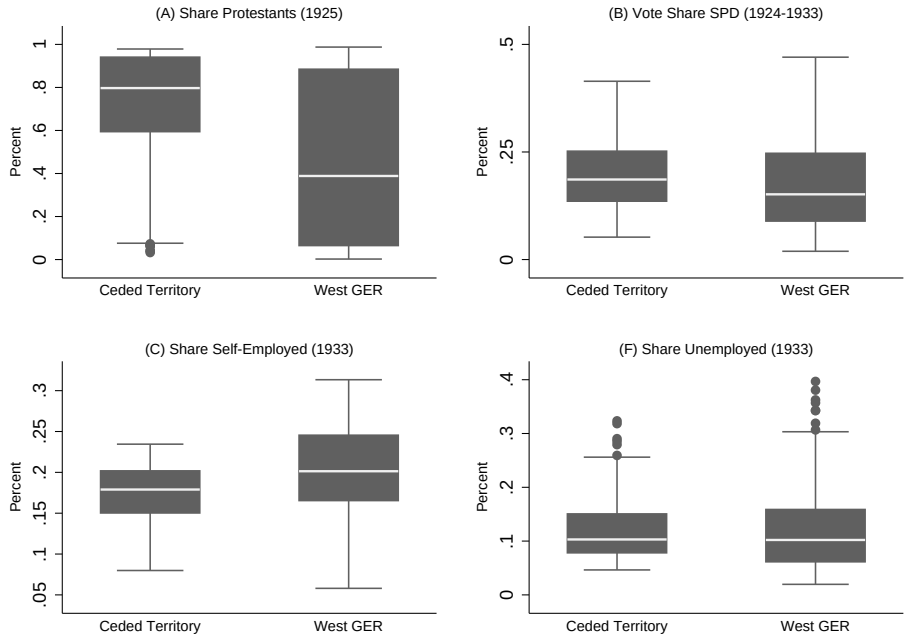
Appendix B provides additional figures. Figure B.1 depicts Germany in its pre- and post-WW II borders, Figure B.2 shows county-level similarities and differences between the Eastern and Western part of the German Reich before WW II. Last, Figure B.3 shows the (conditional) correlation of the expellee share and our instrument, the population-weighted distance between West German cities and the ceded counties in the East and Sudeten.

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



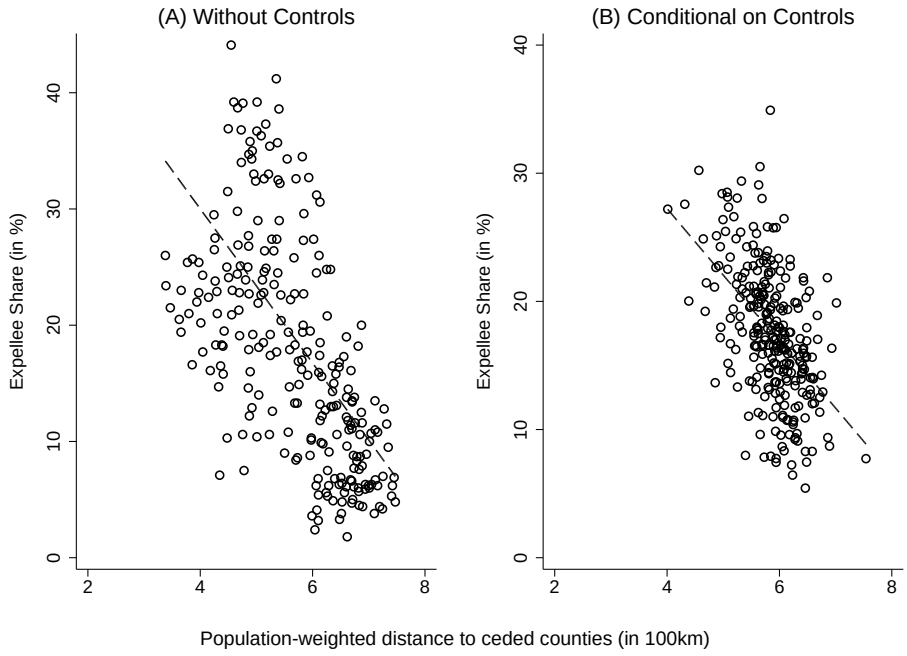
Notes: This map shows Germany in its pre- and post- World War II borders. The Saarland was ceded to France after WW II but rejoined Germany in 1957. The figure is based on shapefiles provided by the Max Planck Institute for Demographic Research (MPIDR) and the Chair for Geodesy and Geoinformatics, University of Rostock (2011).

Figure B.2: Differences Between Expellees and Natives – Pre-WW II Variables



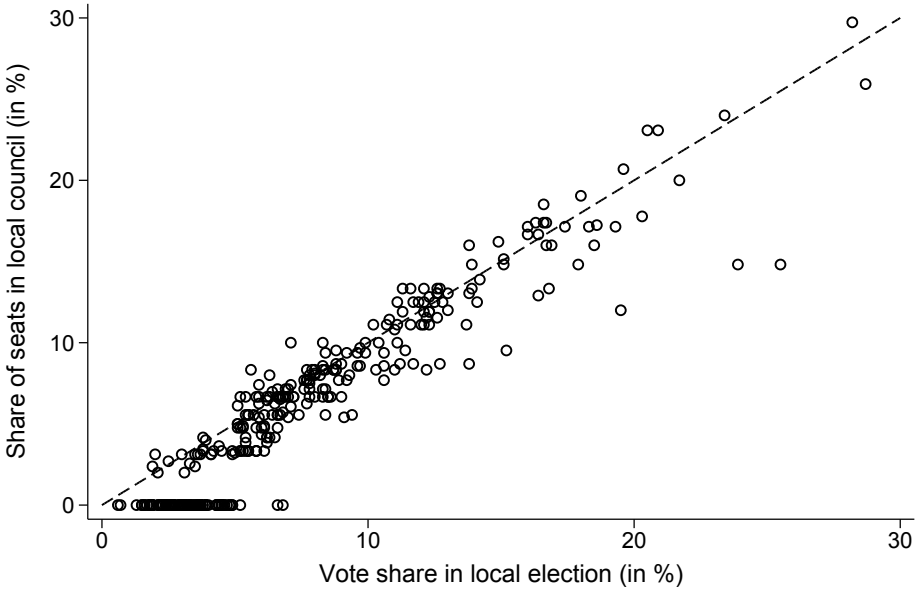
Notes: This graph shows similarities and differences between the Eastern and Western part of the German Reich before WW II. Data are taken from King et al. (2008). See Appendix Tables A.1 and A.2 for further information on the variables and additional descriptive statistics.

Figure B.3: IV First Stage: (Conditional) Correlation



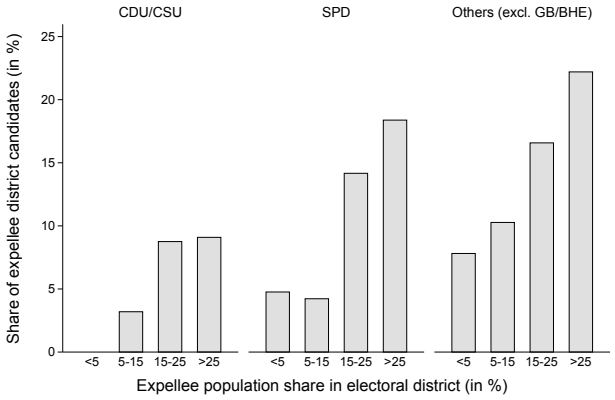
Notes: This graph shows the correlation between our instrument and the expellee share at the county level. Panel (A) displays the raw correlation. In Panel (B), the full set of controls as defined in Section 3 is included. To make both graphs comparable, we added the sample means of both variables to each observation.

Figure B.4: Vote Shares vs. Seats for the GB/BHE in Municipal Elections



Notes: This graph displays the correlation between the share of votes for the GB/BHE in municipal elections between 1950 and 1961, and the share of seats in municipal councils. See Appendix Table A.1 for further information on the variables.

Figure B.5: Expellee Candidates in Federal Elections 1949-1961



Notes: This graph shows the share of direct candidates that were expellees in the federal elections 1949, 1953, 1957 and 1961 for the conservatives (CDU/CSU), social democrats (SPD), and other parties. The category *other* does not include the GB/BHE, where the share of expellee candidates exceeded 60%. The numbers at the bottom indicate the share of expellees in the corresponding counties. See Appendix Table A.1 for further information on the variables.

C Additional Regression Results

Appendix C provides additional regression results. Tables C.1-C.4 present the estimation results for the effect of migration on local tax rates when using our DiD design as laid out in Equation (1) and varying sets of controls. The estimates presented in column (5) of each table show our preferred specification that is displayed in Figure 3 in the paper. Table 1 shows DiD results when allowing for heterogeneous treatment effects. Tables C.5-C.12 present the corresponding IV and cross-sectional OLS results for the annual effect of mass migration on local tax rates; using the full set of controls. The corresponding IV results are also visualized in Figure C.1. Table C.13 further presents cross-sectional OLS and IV estimates for the effect of mass migration on mean post-WW II local tax rates using varying sets of controls. While being informative in its own, we also use these estimates to test the robustness of our IV strategy (see Appendices D and E). Last — as information on local tax rates is available for a larger number of cities than data on local spending, debt and voting — we estimate the effect of mass migration on tax rates when limiting our sample to those cities that offer information on public spending at least once during our sampling period. The corresponding DiD and IV results (using the full set of controls) are displayed in Figures C.2 and C.3.

Regression results for the effect of migration on local spending are displayed in Tables C.14-C.16. Using our most comprehensive set of controls, Tables C.14 and C.15 show IV and cross-sectional OLS estimates for the annual effect of migration on spending, respectively. Table C.16 presents cross-sectional OLS and IV estimates for the effect of migration on mean post-WW II spending using varying sets of controls. The corresponding results on debt and voting are given in Tables C.17-C.21.

Last, Table C.22 presents cross-sectional OLS and IV estimates for the effect of migration on individual preferences with varying sets of controls. Results presented in column (5) indicate our preferred specification, which is plotted in Figure 7 in the paper.

Table C.1: 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 C.2: 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 C.3: 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 C.4: 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 C.5: 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). The outcome is the difference in the tax rate between the respective year and the baseline year 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 C.6: 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. The outcome is the difference in the tax rate between the respective year and the baseline year 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 C.7: 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). The outcome is the difference in the tax rate between the respective year and the baseline year 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 C.8: 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. The outcome is the difference in the tax rate between the respective year and the baseline year 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 C.9: 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). The outcome is the difference in the tax rate between the respective year and the baseline year 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 C.10: 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. The outcome is the difference in the tax rate between the respective year and the baseline year 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 C.11: 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				

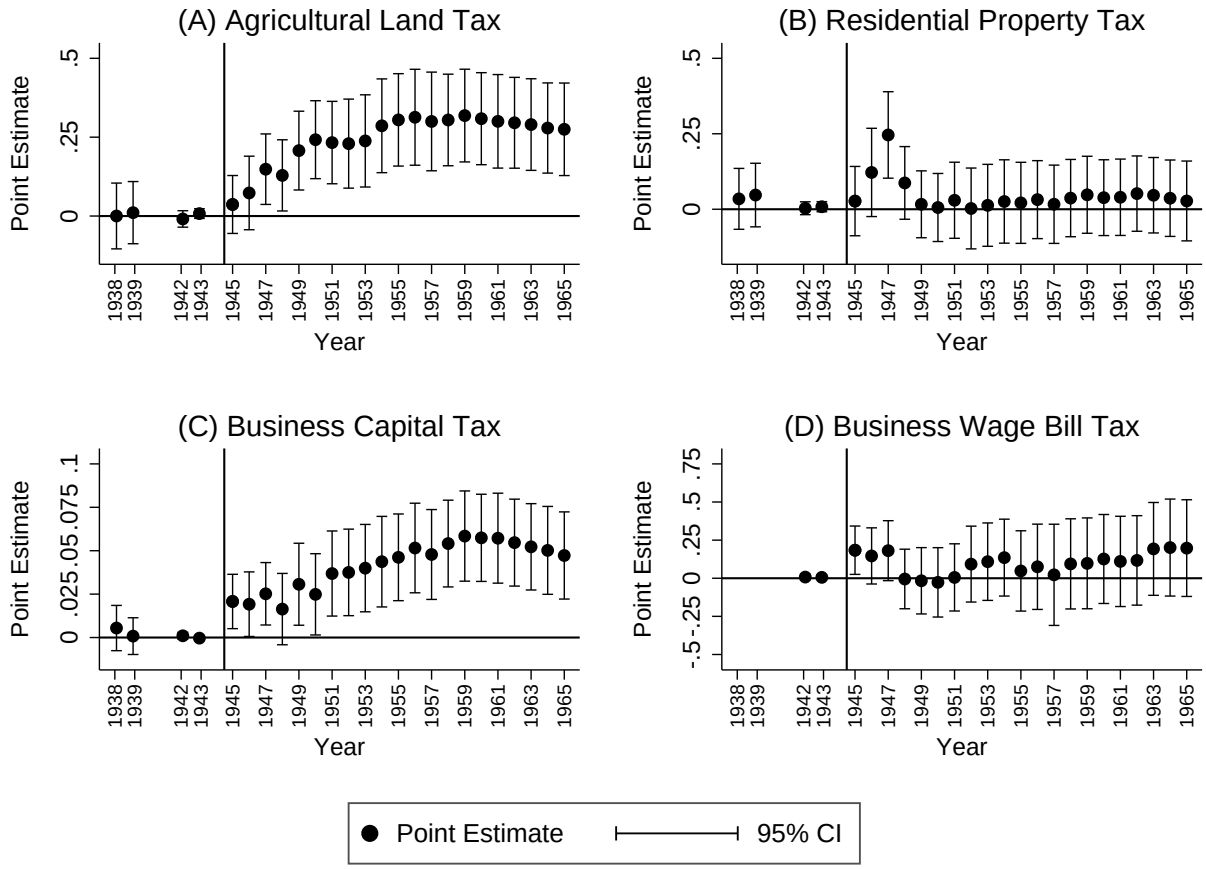
Notes: This table shows the effect of a one standard deviation increase in the expelleee share on annual local tax rates using the IV strategy laid out in Equations (2)-(4). The outcome is the difference in the tax rate between the respective year and the baseline year 1944. Estimates are based on the 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 C.12: 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. The outcome is the difference in the tax rate between the respective year and the baseline year 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.1: The Effect of Mass Migration on Local Taxation: IV Estimates



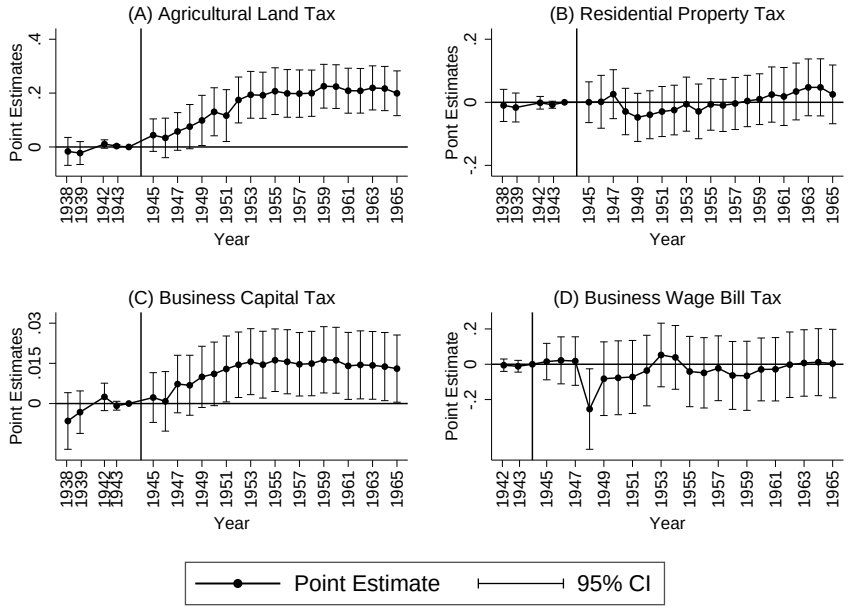
Notes: This graph shows the effect of a one standard deviation increase in the expellee share on our four local tax rates in a given year, using the IV strategy laid out in Equations (2)–(4). Estimates are based on the most comprehensive specification that includes measures of institutional differences, 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 C.13: The Effect of Mass Migration on Local Tax Rates - Average Effect post WW II

	OLS			Instrumental Variables		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – Agricultural Land Tax						
Expellee Share	0.139***	0.131***	0.148***	0.225***	0.222***	0.260***
	(0.022)	(0.031)	(0.034)	(0.031)	(0.051)	(0.062)
Number of Observations	357	357	357	357	357	357
Kleibergen-Paap *F*-Statistic				125.97	85.13	72.08
Panel B – Residential Property Tax						
Expellee Share	0.036**	-0.029	-0.021	0.091***	0.024	0.045
	(0.016)	(0.024)	(0.028)	(0.028)	(0.045)	(0.055)
Number of Observations	357	357	357	357	357	357
Kleibergen-Paap *F*-Statistic				135.95	92.62	78.70
Panel C – Business Capital Tax						
Expellee Share	0.021***	0.011**	0.014**	0.037***	0.032***	0.041***
	(0.004)	(0.005)	(0.006)	(0.006)	(0.009)	(0.011)
Number of Observations	357	357	357	357	357	357
Kleibergen-Paap *F*-Statistic				139.70	91.55	77.35
Panel D – Business Wage Bill Tax						
Expellee Share	0.091**	0.034	0.065	0.101*	0.056	0.080
	(0.035)	(0.055)	(0.056)	(0.054)	(0.107)	(0.112)
Number of Observations	129	129	129	129	129	129
Kleibergen-Paap *F*-Statistic				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

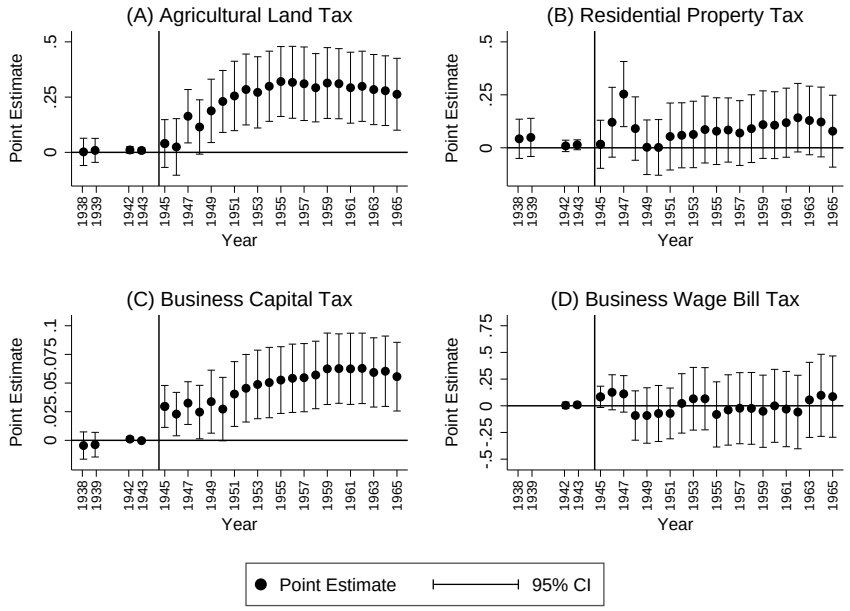
Notes: This table shows the effect of a one standard deviation increase in the expelleee share on mean post-WW II local tax rate changes using simple OLS and the IV strategy laid out in Equations (2)-(4). Mean tax rates (post war) are relative to the respective tax rate in 1944. The set of controls includes 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.2: The Effect of Mass Migration on Local Tax Rates: DiD Estimates Using Restricted Samples



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). The sample is restricted to those cities for which we observe local spending in at least one year. 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.

Figure C.3: The Effect of Mass Migration on Local Taxation: IV Estimates Using Restricted Samples



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). The sample is restricted to those cities for which we observe local spending in at least one year. 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 C.14: 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 C.15: 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 C.16: The Effect of Mass Migration on Per Capita Spending - Average Effect post WW II

	OLS			Instrumental Variables		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – Welfare/Health						
Expellee Share	-0.163***	-0.053	0.007	0.011	0.124	0.220**
	(0.041)	(0.053)	(0.059)	(0.068)	(0.086)	(0.101)
Number of Observations	235	235	235	235	235	235
Kleibergen-Paap *F*-Statistic				71.59	66.73	59.40
Panel B – Admin/Police						
Expellee Share	-0.087***	-0.066**	-0.015	-0.068*	-0.065	-0.013
	(0.024)	(0.031)	(0.032)	(0.041)	(0.048)	(0.054)
Number of Observations	235	235	235	235	235	235
Kleibergen-Paap *F*-Statistic				71.59	66.73	59.40
Panel C – Infrastructure/Housing						
Expellee Share	-0.146***	-0.192***	-0.165***	-0.244***	-0.327***	-0.327***
	(0.027)	(0.035)	(0.038)	(0.048)	(0.063)	(0.074)
Number of Observations	235	235	235	235	235	235
Kleibergen-Paap *F*-Statistic				71.59	66.73	59.40
Panel D – Schools/Culture						
Expellee Share	-0.127***	-0.148***	-0.102***	-0.197***	-0.260***	-0.237***
	(0.021)	(0.028)	(0.031)	(0.048)	(0.058)	(0.066)
Number of Observations	235	235	235	235	235	235
Kleibergen-Paap *F*-Statistic				71.59	66.73	59.40
Panel E – Total Spending						
Expellee Share	-0.126***	-0.128***	-0.087***	-0.151***	-0.178***	-0.147***
	(0.020)	(0.024)	(0.026)	(0.037)	(0.045)	(0.050)
Number of Observations	235	235	235	235	235	235
Kleibergen-Paap *F*-Statistic				71.59	66.73	59.40
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 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 C.17: The Effect of Mass Migration on Per Capita Debt - 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

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on annual per capita debt (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 C.18: The Effect of Mass Migration on Per Capita Debt - Average Effect post WW II

	OLS			Instrumental Variables		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – Per Capita Debt						
Expellee Share	0.010	-0.005	0.059	0.097	0.024	0.091
	(0.037)	(0.050)	(0.055)	(0.063)	(0.091)	(0.105)
Number of Observations	240	240	240	240	240	240
Kleibergen-Paap *F*-Statistic				76.26	80.27	73.20
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 (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 C.19: 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 C.20: 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 C.21: The Effect of Mass Migration on Voter Turnout & Vote Shares - Average Effect post WW II

	OLS			Instrumental Variables		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – Voter Turnout						
Expellee Share	-0.810*	0.433	-0.516	0.772	2.264**	1.723*
	(0.429)	(0.586)	(0.634)	(0.721)	(0.933)	(0.999)
Number of Observations	216	216	216	216	216	216
Kleibergen-Paap *F*-Statistic				71.33	83.38	70.86
Panel B – Vote Share CDU						
Expellee Share	-2.815***	-0.081	0.327	-4.044***	-1.677	-1.632
	(0.725)	(0.923)	(1.015)	(1.221)	(1.512)	(1.749)
Number of Observations	215	215	215	215	215	215
Kleibergen-Paap *F*-Statistic				74.49	83.02	70.45
Panel C – Vote Share SPD						
Expellee Share	-1.091	-1.378	-1.641*	0.425	-1.783	-2.143
	(0.767)	(0.961)	(0.935)	(1.148)	(1.402)	(1.579)
Number of Observations	216	216	216	216	216	216
Kleibergen-Paap *F*-Statistic				71.33	83.38	70.86
Panel D – Vote Share GB/BHE						
Expellee Share	3.380***	3.217***	3.275***	3.001***	2.546***	2.464***
	(0.287)	(0.424)	(0.448)	(0.392)	(0.615)	(0.711)
Number of Observations	116	116	116	116	116	116
Kleibergen-Paap *F*-Statistic				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 C.22: The Effect of Mass Migration on Preferences for Redistribution

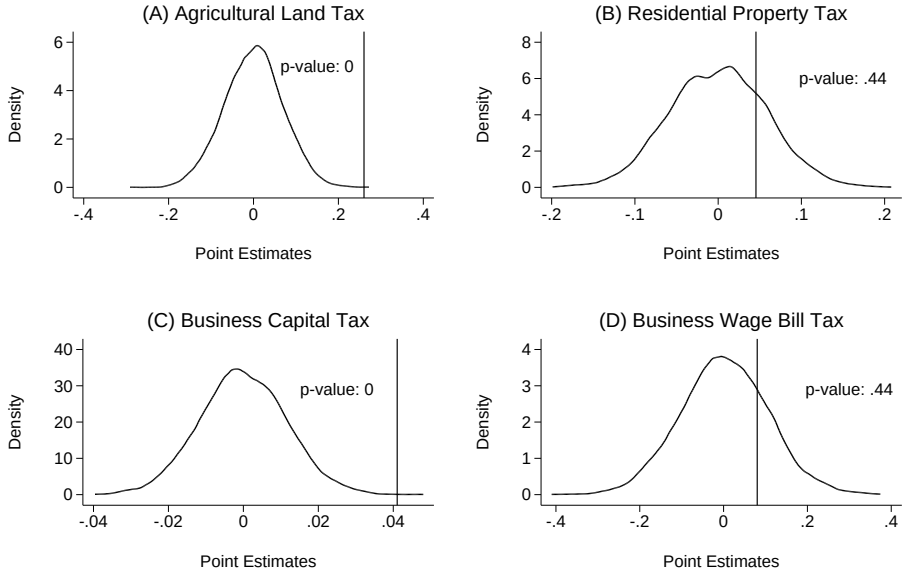
	OLS			Instrumental Variables		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – For the family						
Expellee Share	0.005	0.004	0.004	0.024**	0.003	-0.005
	(0.006)	(0.007)	(0.007)	(0.011)	(0.020)	(0.019)
Number of Observations	8,974	8,974	8,974	8,974	8,974	8,974
Kleibergen-Paap *F*-Statistic				41.97	20.74	21.40
Panel 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 details). Cross-sectional weights are used. Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D Inference

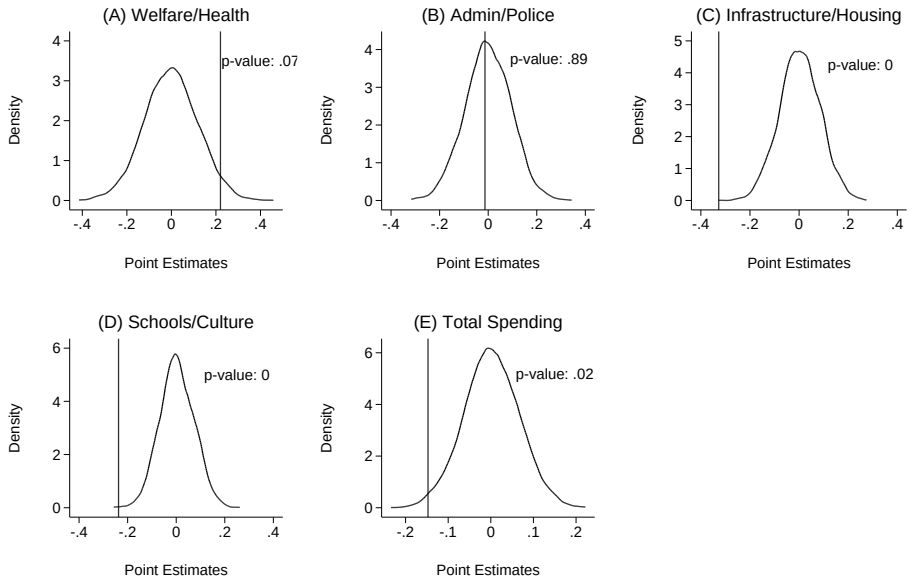
Appendix D presents the results of our permutation tests to re-assess the statistical significance of our estimates. In our baseline estimations, we cluster standard errors at the county level to account for any potential correlation in the error terms across cities within counties and within counties over time. By using this procedure of inference, we assume that the error terms are not systematically correlated between counties and normally distributed in the population. Non-parametric permutation tests allow us to relax both assumptions. To obtain the relevant test statistics, we randomly shuffle the dependent variable in our sample and re-estimate our preferred IV specification (using the mean post-war outcomes) 5,000 times. The resulting empirical distributions of these placebo estimates allow us to calculate the corresponding p-values for the hypothesis $\delta_1 = 0$ by deriving the share of estimated coefficients that are larger (in absolute terms) than the point estimate of our preferred specification in the true model.

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



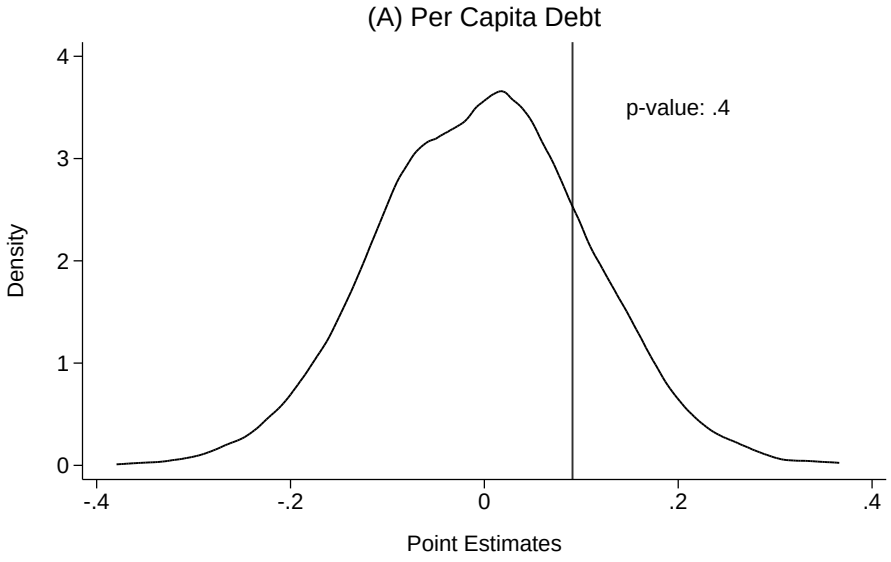
Notes: This figure shows the empirical distributions of placebo estimates for local tax rates on (A) agricultural land, (B) residential property, (C) business capital, and (D) business' wage bill. The cumulative distribution functions are based on 5000 estimates of β using the IV specification displayed in column (6) of Table C.13 and random permutations of the respective dependent variable. The vertical lines indicate the corresponding point estimate as shown in column (6) of Table C.13.

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



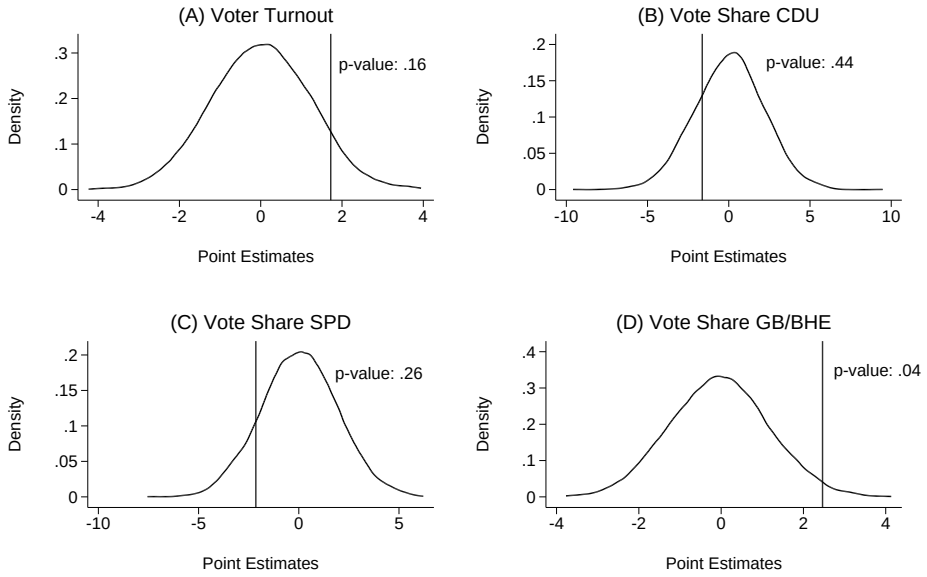
Notes: This figure shows the empirical distributions of placebo estimates for per capita spending on (A) welfare and health, (B) administration and the police, (C) public infrastructure and housing, (D) schools and culture, (E) all items. The cumulative distribution functions are based on 5000 estimates of β using the IV specification displayed in column (6) of Table C.16 and random permutations of the respective dependent variable. The vertical lines indicate the corresponding point estimate as shown in column (6) of Table C.16.

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



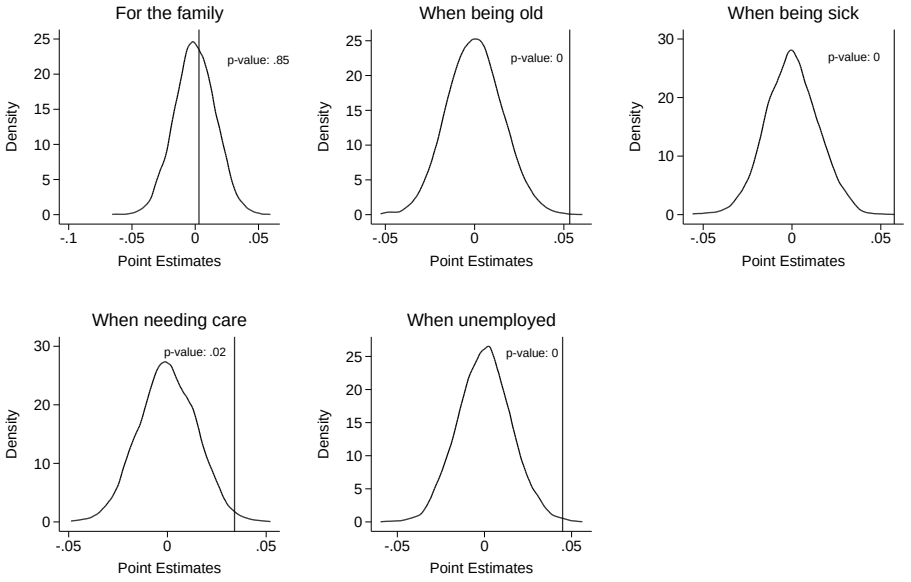
Notes: This figure shows the empirical distributions of placebo estimates for (A) per capita debt (in logs). The cumulative distribution functions are based on 5000 estimates of β using the IV specification displayed in column (6) of Table C.18 and random permutations of the respective dependent variable. The vertical lines indicate the corresponding point estimate as shown in column (6) of Table C.18.

Figure D.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 C.21 and random permutations of the respective dependent variable. The vertical lines indicate the corresponding point estimate as shown in column (6) of Table C.21.

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

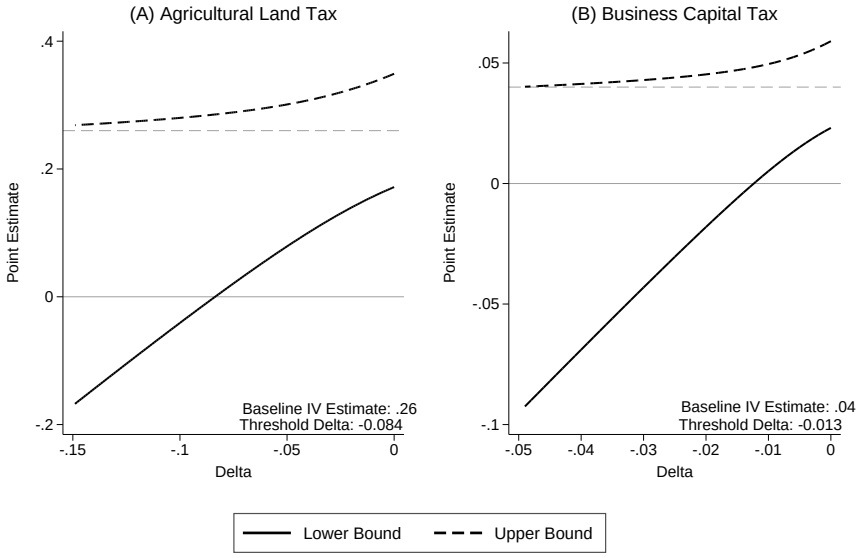


Notes: This figure shows the empirical distributions of placebo estimates for individuals' preferences for redistribution. The cumulative distribution functions are based on 5000 estimates of β using the IV specification displayed in column (6) of Table C.22 and random permutations of the respective dependent variable. The vertical lines indicate the corresponding point estimate as shown in column (6) of Table C.22.

E Test of Exclusion Restriction

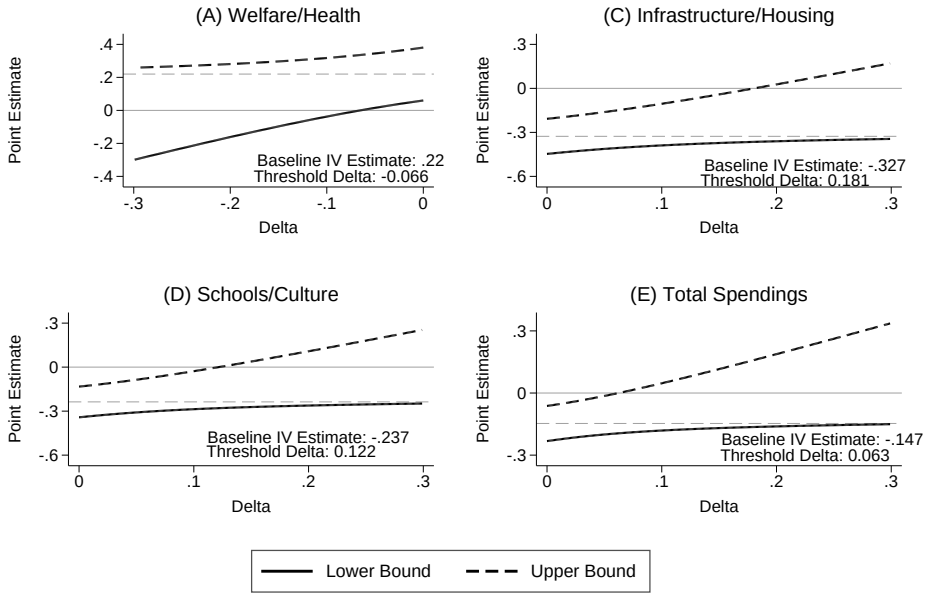
Appendix E assesses the robustness of our IV estimates regarding potential violations of the exclusion restriction. We follow Conley et al. (2012) and allow for a direct effect of the instrument on our outcomes, and calculate threshold values for the direct effect of the instrument that would completely explain away our second-stage results. In detail, and following Conley et al. (2012) as well as Satyanath et al. (2017), we assume that the (potential) direct effect of the instrument on the respective outcome is uniformly distributed in an interval [0,δ]. By gradually allowing for larger direct effects of the instrument, we are able to trace out the threshold value at which the second-stage estimate for the expellee share becomes insignificant at the 10% level.

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



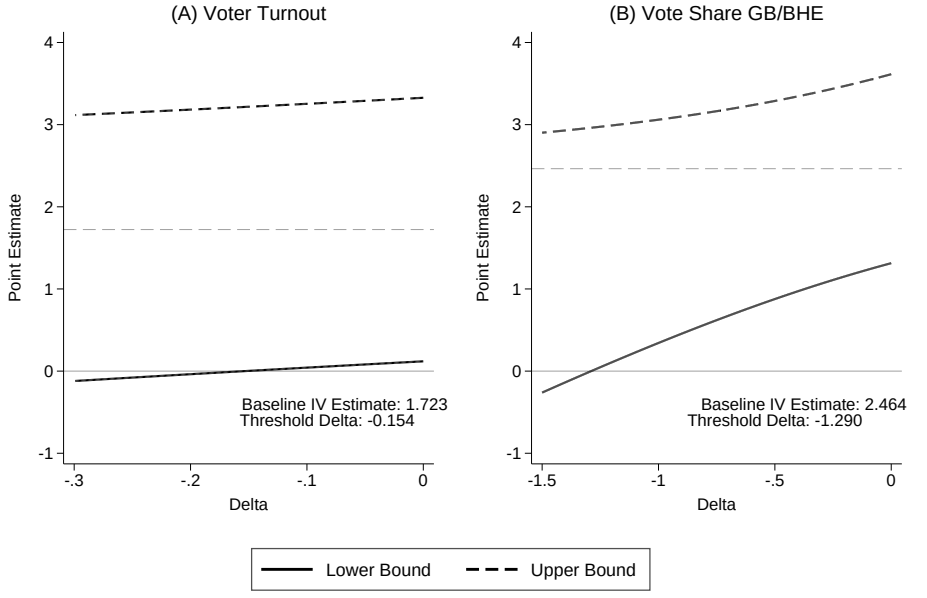
Notes: This figure shows the upper and lower bound of the 90% confidence interval of the second-stage point estimates for the expellee share on (A) the mean agricultural land tax rate and (B) the mean business capital tax rate when allowing for a direct effect of the instrument on the outcome (as indicated 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 C.13.

Figure E.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 C.16.

Figure E.3: 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 C.21.