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

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This paper studies the effect of a large poverty shock on local taxation and spending. We exploit the sudden arrival of eight million forced migrants in West Germany after World War II. The migrants were poorer than the local population, eligible for social welfare and had full voting rights. We find that high-inflow cities responded with greater redistribution: they selectively raised local taxes and shifted spending from infrastructure to welfare. We show that this policy shift can be partly explained by the forced migrants' political involvement and their voting behavior. We further document the persistence of these effects into the next generation. Even 50 years after the shock, people in high-inflow cities have stronger preferences for redistribution. These findings are consistent with standard political economy models of the welfare state.

Keywords: taxation, social welfare, voting, forced migration
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1 Introduction

Growing income and wealth inequality presents a serious challenge to modern welfare states. In many countries, it has revitalized the debate on taxation and redistribution and is often seen as a driving force behind the recent rise of populism (see, e.g., Pastor and Veronesi, 2018, Rodríguez-Pose, 2018). The link between inequality and redistribution is the subject of a long-standing debate in political economy. Median voter models predict that higher inequality shifts the median voter to poorer segments of society, thereby leading to greater redistribution (Romer, 1975, Roberts, 1977, Meltzer and Richard, 1981). Other theories predict less redistribution, for example, if groups cannot agree on the design of public policies (Alesina et al., 1999) or economic inequality increases the ability of the rich to influence politics (Benabou, 2000). To study which mechanism dominates, the empirical literature has largely focused on franchise extensions. The evidence, spanning over 200 years and multiple continents, clearly supports median voter models. Enabling minorities to vote increases the support for more generous welfare spending and higher taxation (Husted and Kenny, 1997, Lott et al., 1999, Cascio and Washington, 2013, Sabet and Winter, 2019) and improves the minorities' economic and health situation (Fujiwara, 2015).

In this paper, we highlight an alternative mechanism through which a change in inequality may affect the level of redistribution, namely internal migration. In many countries, the growing opportunities in modern cities attract workers from less developed regions (Desmet and Henderson, 2015). Because the internal migrants typically have lower incomes and less wealth than the incumbent urban population, their inflow changes the local level of inequality. If the newcomers have voting rights, we can expect that their inflow affects taxation and spending in cities with high inflows. However, empirically testing this mechanism is challenging because migrants may be attracted by the level of public spending or by economic factors that determine taxation and spending.[1]

We overcome this challenge by exploiting an episode of large-scale forced migration in post-war West Germany. After World War II, Germany had to cede 25% of its territory to Poland and the Soviet Union, and all Germans outside the country's new borders were to be expelled, which resulted in the displacement of over twelve million people. We focus on West Germany, where the inflow of eight million so-called *expellees* increased the population by almost 20% within just a couple of years. As German citizens, the expellees had voting rights upon arrival, such that their inflow significantly increased the electorate. Moreover, because the expellees had lost virtually all their assets in flight and/or transit, their arrival caused a substantial increase in poverty rates and inequality in the receiving regions. Using newly-digitized panel data for around 250 West German cities, we show that this large-scale population and poverty shock led to greater redistribution in the short to medium run, as well as a lasting shift in preferences for redistribution.

Identifying causal effects based on simple OLS regressions would require the expellee inflow into West German cities to be as good as random. This condition would be violated if, for example, the expellees were assigned to cities with higher taxes or more public spending. The same is true if there was any factor that would co-determine the immigration and public policy setting, such as a low degree of war destruction or a strong local economy after 1945. Balancing tests on pre-

[1] See Baum-Snow and Ferreira (2015) for an overview of potential omitted variable bias problems in regional economics, as well as Brülhart et al. (2015) for a theoretical discussion of tax-induced sorting.

war data indeed point to non-random assignment of expellees across West Germany. They were disproportionately assigned to places with low public spending and low tax rates before World War II, suggesting that OLS estimates are most likely biased *against* finding a positive effect of rising inequality on redistribution.

To establish causality, we construct an instrument for the regional expellee inflow, which rests on two factors that are orthogonal to economic conditions in West German cities. Based on pre-war data, we first predict the number of expellees for each county in the ceded territories. We then assign the expellees to each county in the West based on bilateral distances. Distance serves as a proxy for the Allied Forces' costs of allocating the expellees to different regions upon arrival. Thus, by construction, the instrument does not include any pull factors that may have attracted the expellees to certain West German cities.

The validity of the instrument rests on the identifying assumption that the predicted inflows are uncorrelated with local economic conditions in West Germany after 1945. An obvious concern is that the expulsions coincided with the division of Germany after World War II. Work by Redding and Sturm (2008) shows that the division had negative effects on growth in West German cities up to 75km from the inner-German border. We tackle this problem by controlling for whether a city is located within this zone and conditioning on state fixed effects. Moreover, we show that the results remain qualitatively and quantitatively unaffected when excluding cities close to the inner-German border. To corroborate the validity of the instrument, we further show that the instrument is uncorrelated with pre-war tax rates, spending and welfare generosity. Moreover, following Conley et al. (2012), we show that our causal inference is robust to possible, moderate violations of the exclusion restriction.

We find that the expellee inflow led to a substantial shift in taxation and spending policies in the short to medium run. High-inflow cities responded by increasing welfare spending (both per capita and relative to other items), while decreasing spending on public infrastructure. Our findings also point to selective tax increases: a ten-percentage-point increase in the share of expellees — about one standard deviation — led to increases in the local residential property tax rate and the local business tax rate by 82% and 89% of a standard deviation, respectively. On the contrary, we find no significant effects for the corresponding tax rates on agricultural land and firms' wage bills. We also show that high-inflow cities incurred greater debt. However, the increase in debt was proportional to the increase in population size, which suggests that it reacted to changes in population size rather than poverty rates. Overall, these results align with the predictions of median voter models such as Meltzer and Richard (1981).

We further present three pieces of evidence that point to the expellees' political involvement as an important mechanism behind the observed shifts in public policy setting. Survey data from 1953 show that the expellees were more politically active than the incumbent population and reported a stronger intention to vote in upcoming elections. They also reported a stronger preference for the GB/BHE — a party that explicitly catered for the interests of the expellees — and weaker preferences for the two major parties, the Christian Democrats (CDU/CSU) and the Social Democrats (SPD). These findings are mirrored in local election results. Our IV results show that high-inflow cities experienced a considerably higher vote share for the GB/BHE and lower vote shares for the two major parties. Nonetheless, expellees also appear to have played important roles in those major

parties. In constituencies with a more pronounced inflow, expellees had a higher likelihood of being nominated as a direct candidate for the federal parliament. This is remarkable because the expellees had no majority in any constituency and each party could only nominate one candidate for this winner-takes-all competition. Without a strong political engagement of the expellees, one would not expect any expellee to be nominated.

We rule out two competing mechanisms that could explain part of the results, namely government transfers and internal migration. Although we find that high-inflow cities received more transfers, there is no evidence that the inflow affected *per-capita* transfers. This suggests that transfers responded to the increase in population size but not to changes in poverty rates. Moreover, the fact that we see effects on taxation and spending despite an increase in total transfers suggests that the transfers alone were not sufficient to cover the cities' additional costs. A second potential mechanism is internal migration. Research on the Great Migration in the U.S. shows that the inflow of Blacks to northern cities triggered outflows of Whites (Boustan, 2010), which reduced the tax base and led to changes in public policy setting (Tabellini, 2020b). In our case, we find no evidence of out-migration in response to the expellee inflow. If anything, the expellees moved from their initial location to larger cities over the course of the 1950s.

In the final part of the paper, we show that the inflow of poor people had a lasting effect on preferences for redistribution. Using survey data from the early-2000s and applying our IV strategy, we show that people born after the expulsions and living in high-inflow regions have significantly stronger preferences for redistribution more than 50 years later. While we remain agnostic about the mechanisms behind this long-run effect — for instance, it could be due to the intergenerational transmission of preferences and/or sorting — the result is consistent with the observed effects on public policy setting.

The paper contributes to four strands of literature. First, it adds to the empirical literature on the political economy of the welfare state. The most closely-related work in this literature is the study by Tabellini (2020b) on the Great Migration in the United States, which shows that the inflow of Blacks into northern U.S. cities led to lower taxation and spending.[2] For post-war Germany, we find the opposite effect: an inflow of poor voters led to greater redistribution. These results are similar to those in studies on franchise extensions (Husted and Kenny, 1997, Lott et al., 1999, Cascio and Washington, 2013, Fujiwara, 2015, Sabet and Winter, 2019), and consistent with median voter models of the welfare state (e.g., Meltzer and Richard, 1981). The differences in results compared to the studies of the Great Migration point to the importance of the context in which the migration flow occurs. In post-war West Germany, expellees and natives mostly belonged to the same ethnic group. Moreover, unlike in northern U.S. cities, the inflow of expellees did not lead to significant outflows of the incumbent population.

Second, the paper speaks to the related literature on international migration and public policy setting. Tabellini (2020a) finds that immigration led to less redistribution during the era of mass migration to the U.S. These results are consistent with the model of Razin et al. (2002), wherein natives opt for less redistribution because migrants would be the main recipients of public spending

[2] In a related paper on the Great Migration, Derenoncourt (2019) shows that areas with greater inflows of Blacks increased their spending on policing but did not reduce education spending. Together with an increase in private school enrolment among Whites, these factors explain the low degree of upward mobility of Blacks in high-inflow areas.

while contributing little to tax revenue. Recent work by Bandiera et al. (2019) adds an important intergenerational dimension to this setting: if the children of immigrants become voters, it is beneficial to increase public spending for the first generation. In our setting, the immigrants had voting rights upon arrival and could, therefore, influence politics in their favor. Our results suggest that the public policy response to immigration crucially depends on whether the immigrants are eligible to vote.

Third, the paper contributes to the literature on the determinants of preferences for redistribution. Several studies document that these preferences 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 the shifts in preferences triggered by exposure to poverty can persist over multiple generations.

Finally, the paper contributes to the literature on the economic consequences of forced migration (see Becker and Ferrara, 2019, for a survey of the literature). Several studies exploit the resettlement of Germans after Word War II to study the economic impact of of forced migration on the receiving regions, in particular on labor market outcomes (Braun and Omar Mahmoud, 2014, Braun and Weber, 2016) and economic growth (Burchardi and Hassan, 2013, Braun and Kvasnicka, 2014, Peters, 2017).[3] Our paper adds to this literature by showing that the integration of forced migrants can have a profound impact on public policy setting, in particular when migrants have voting rights.

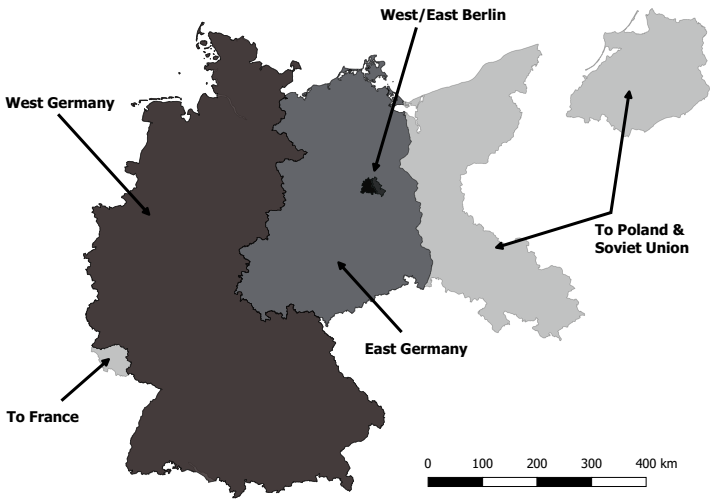
2 The Expulsion of Germans: Causes, Scale and Economic Impact

The background of this study is the expulsion and resettlement of over twelve million Germans in the aftermath of World War II. This episode is acknowledged as one of the largest forced population movements in history (Douglas, 2012). The expulsions were triggered by the end of the war, when the Allied Forces ceded around 25% of Germany's pre-war territory — Silesia, Pomerania, Brandenburg, and East Prussia — to Poland and the Soviet Union (see Figure 1). Any ethnic Germans outside of these newly-drawn borders were to be expelled and forced to resettle within the borders of post-war Germany. This affected all citizens from the ceded territories as well as German communities in Central and Eastern Europe, most importantly around three million ethnic Germans who lived in the Sudeten along the border between Czechoslovakia and Germany (Merten, 2012, ch. 1).

The expulsions occurred in two stages. First, during the final phase of World War II, people fled from the advancing troops of the Soviet Army as well as local militias. In the second phase, the U.S. and British forces organized mass transports on buses, trains and ships (Douglas, 2012). Overall, around eight million expellees arrived in the U.S., British and French occupation zones between 1944 and 1950. The three zones became the Federal Republic of Germany (FRG) — in short *West Germany* — in 1949. The remaining four million expellees arrived in the Soviet occupation zone — to become the German Democratic Republic (GDR), in short *East Germany* — or died in transit.

[3] A further branch of this literature documents the slow integration of the expellees into society (Bauer et al., 2013). Focusing on the forced migration of Poles after Word War II, Becker et al. (2020) further show that expellees were more likely to invest in the human capital accumulation of their children than those who were not expelled.

Figure 1: German Territory before and after World War II



Notes: This map shows Germany and its borders before and after World War II. The Saarland was ceded to France after Word War II but rejoined West 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).

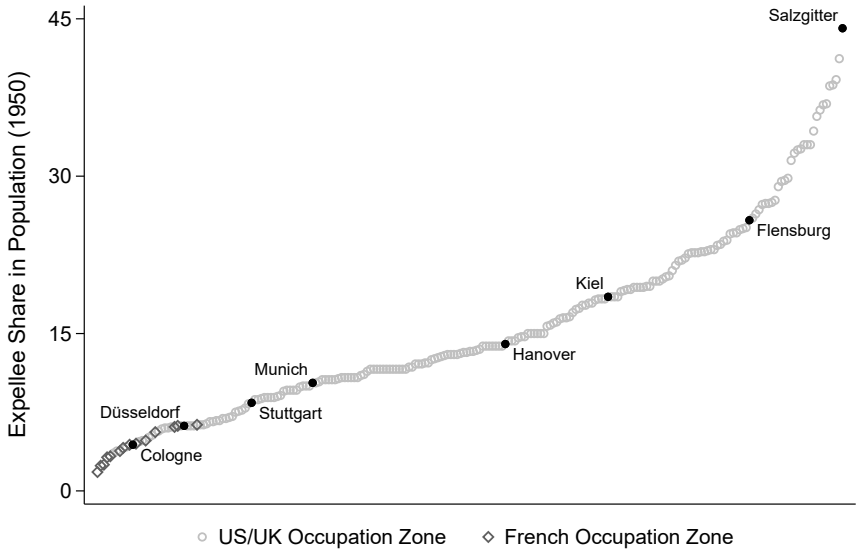
Arrival and Allocation In West Germany, the population increased by close to 20% in response to the expulsions (Kossert, 2008). Upon arrival, the expellees' first residence was usually determined by the Winning Allies. After reaching the West German territory, the majority of expellees were first transferred to temporary refugee camps and subsequently assigned to municipalities in the U.S. or British occupation zone. Because France suffered from greater war destruction compared with the U.S. and the UK, no expellees were assigned to the French occupation zone prior to mid-1949 (Douglas, 2012, ch. 6). Until 1949, the Winning Allies also enforced mobility restrictions for the expellees, preventing them from relocating within West Germany in the first years after arrival (Müller and Simon, 1959).

The allocation of expellees across West Germany did not follow a systematic protocol. Initially, the Allies aimed at allocating the expellees according to demographic and economic factors such as population density or economic potential. However, due to the severe destruction of most cities and the rapid inflow of refugees within a short time span, the availability of accommodation 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).

Figure 2 displays the distribution of expellees across West German counties as of 1950 in our sample (see Section 3 for details). The data come from the Statistical Yearbook of Expellees and constitute the first available nationwide description of the inflow. Two facts are striking: first, the size of the population shock was very heterogeneous across space, with the county-level share of expellees ranging from 1.8 to 44.1%; and second, in line with the imposed settlement restrictions, counties in the French zone were among those with the lowest population shares of expelles. Appendix

Figure B.1 highlights the corresponding distribution for all West German counties.[4] It shows that most expellees were allocated to the states of Schleswig-Holstein and Lower Saxony in the north, as well as Bavaria in the south-east of the country. By contrast, the population share of expellees was substantially lower in the federal states of North Rhine-Westphalia, Rhineland-Palatinate and Baden-Württemberg in the (south-)west.

Figure 2: Population Shares of Expellees by County in West Germany, 1950



Notes: This graph shows the county-level population share of expellees in West Germany in September 1950. Data are taken from the "Statistical Yearbook of Expellees" (Statistisches Bundesamt, 1953). The city of West-Berlin and the Saarland are excluded.

Integration of the Expellees Upon arrival, the economic situation of the expellees was dire. The expellees had lost their homes, jobs and virtually all of their possessions and real assets in flight and/or transit. While many West German natives had also experienced severe losses during the war, they still owned the remaining real assets such as agricultural land, livestock, properties or businesses. Unlike the expellees, many West Germans were also able to draw upon their existing social networks to find employment or obtain loans. Moreover, in addition to their economic deprivation and despite their shared ethnicity, expellees were not welcomed by the West German population upon arrival.[5] Anecdotal evidence abounds of West Germans expressing hostility towards the expellees, in an episode described as "*racism of Germans against German expellees*" (Kossert, 2008, ch. 4).

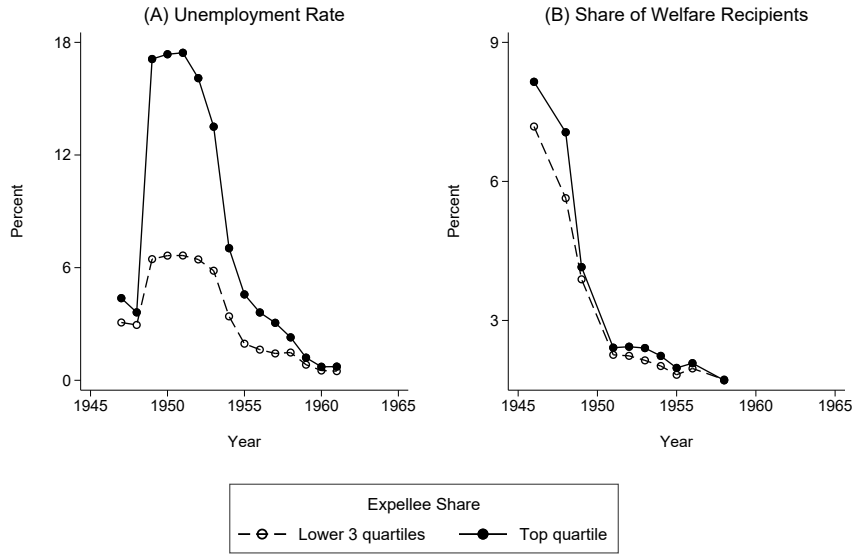
Accordingly, it is unsurprising that the inflow of eight million expellees presented a tremendous challenge to the newly-founded Federal Republic. While the provisional West German government and the Allied Forces initiated a set of comprehensive national-level policies, much of the administrative and fiscal burden was borne locally. Among others, municipalities were responsible for the

[4] Appendix Figure B.2 maps the cities covered in our baseline sample.

[5] In Appendix Figure B.3, we compare counties from the ceded Eastern Territories with those in the west of Germany along their pre-war characteristics. Apart from differences in their religious denomination, expellees and natives were quite similar on average.

provision of housing and social welfare for the expellees. At the same time, local unemployment surged. Figure 3 illustrates these two dimensions. From Panel (A), we infer that unemployment rates increased much more in cities with a high population share of expellees. This finding is in line with evidence from Braun and Omar Mahmoud (2014), who report negative effects of the expellee inflow on local wages and employment. At the same time, the surge in unemployment was — at least initially — accompanied by a rise in the number of welfare recipients (Panel (B)). In our empirical analysis, we investigate cities' policy responses to these shocks to population size and poverty in detail.

Figure 3: Forced Migration, Unemployment and the Share of Welfare Recipients



Notes: This graph shows how the average local unemployment rate (Panel A) and the population share of welfare recipients (Panel B) evolved over time in cities with low to medium and high inflows of expellees, respectively. See Appendix Table A.1 for definitions of the variables and the underlying data sources.

Voting Rights, Welfare Eligibility and Political Representation The expellees were considered German citizens upon arrival, which made them eligible for social welfare but also granted them full voting rights in local, state and federal elections.[6] However, until 1950, Allied law prohibited the expellees from forming parties (Connor, 2007). When the law was lifted, the expellees founded a political party, the GB/BHE (*Gesamtdeutscher Block/Bund der Heimatvertriebenen und Entrechteten*). The party's goal was to improve the expellees' economic situation in West Germany, as well as lobbying for a return of their properties in Germany's former Eastern Territories. Appendix Figures B.4 and B.5 show that the party's vote share and representation in local parliaments was substantially higher in cities with a larger share of expellees. We take this as first suggestive evidence that the expellees used their right to vote in their own interest. Moreover, it can be seen as an indication that established

[6] 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.

political parties had an incentive to cater for the interests of these new voters and account for their needs when setting public policies. We investigate this potential mechanism in more detail below.

3 Data

3.1 Main data source and descriptives

We combine our data on the local inflow of expellees with city-level information on public finances, voting and demographics. These data are taken from the *Statistical Yearbook of German Municipalities* (Statistisches Jahrbuch Deutscher Gemeinden), an annual statistical publication for all German municipalities with more than 10,000 inhabitants. Because the vast majority of these municipalities coincide with cities, we speak of *cities* throughout the rest of the paper. From the Statistical Yearbook, we digitized and assembled unbalanced panel data that spans the period between 1935 to 1964. However, data coverage slightly differs by outcomes; see Appendix Table A.1 for details.

3.1.1 City-Level Outcomes

The main outcomes are policy variables set by local governments: spending, tax rates, and debt. To understand the mechanisms behind the observed effects on public policy setting, we further look at city-level voting outcomes, transfers from higher levels of government, and the out-migration of the incumbent population or expellees in response to the expulsions.

Spending In West Germany, cities were (and still are) responsible for the financing and provision of a wide range of public goods and services. We look at four broad spending categories in our analysis: i) social welfare, ii) education (schools and culture) and administration, iii) public infrastructure and iv) health and housing.[7] Importantly, cities had considerable discretion over the level of spending on each item, including welfare. Welfare payments largely aligned with local costs of living and followed the principle that benefits had to be lower than local wages (Willing, 2001, Föcking, 2009), but still considerably varied across space. For each category, we report separate results for spending per capita as well as the category's share in total spending. This distinction allows us to study two different responses. The effect on *spending shares* informs us about the relative weight of each spending category, and how this weight changes when the population changes in size and composition. The effect on *per-capita spending* holds the population size constant and thus isolates the impact of a change in composition. In our baseline specification, we average our measures of city-level spending over the 1950-1959 period, although we also analyze the dynamics of the effects over the course of the 1950s.

Tax Rates Local taxation has been an important pillar of public finances in Germany for over 100 years and cities have long enjoyed far-reaching fiscal autonomy. We consider as outcomes four tax rates that were set annually by each municipality: i) a tax on the value of agricultural land

[7] Spending categories listed by the Statistical Yearbooks slightly vary over time. We aggregate categories in this specific way to obtain time-consistent measures of local spending.

(*Grundsteuer A*), ii) a tax on the value of residential property (*Grundsteuer B*), iii) a joint tax on business capital and profits (*Gewerbesteuer*), and iv) a tax on firms' wage bills (*Lohnsummensteuer*). For each of these taxes, the federal government defines the valuation of the tax base — for example, the value of a house — as well as a benchmark tax rate. Cities annually decide upon a so-called *collection rate* (*Hebesatz*), the share or multiple of the benchmark tax rate that it wants to set. During the sampling period, cities were able to choose any collection rate between zero and infinity. For the ease of interpretation, we use as outcomes the total tax rates, a simple percentage calculated as the federal benchmark rate times the collection rate.[8] In our main analysis, we look at average tax rates over the 1949-1965 period. During this time, the four local taxes under study accounted for 90% of the cities' overall tax revenue and more than 70% of their total revenue.

Debt Over the course of the 1950s, debt became an important source of revenue for municipalities. With the currency reform in 1948, 90% of municipal debt was eliminated. However, from 1950 onwards, municipalities' debt increased from 1.2% of all West German public debt to over 30% in 1965 (see Statistisches Bundesamt, 2016, Table 1.1). We take log total debt and debt per capita as outcomes. In our baseline analysis, we look at the average of both variables over the 1950-59 period.

Voting We analyze the role of expellees' political influence by using information on local elections over the 1946-1961 period. Outcomes include (i) voter turnout, (ii) party vote shares, and (iii) the degree of the expellee party's representation in local parliaments. On average, three local elections were held during the sampling period in each city at varying times. We report effects on average outcomes over this period.

Transfers Within Germany's federal system, cities receive transfers from their respective state or the federal government. Transfers may explain the observed local shifts in public spending if they were disproportionally targeted towards high-inflow regions and on an issue-specific basis, i.e. to be spent on the well-being of the expellees. We look at two outcomes to investigate this potential mechanism: (i) log total transfers, and (ii) transfers per capita. We observe both variables on an annual basis from 1950 to 1959 and use averages over this period as our baseline outcomes.

Out-Migration We also look at the out-migration of either the incumbent population or the expellees as a potential mechanism. For this purpose, we look at the respective group's population growth rate over the 1950-1961 period. Unlike the other outcome variables, we look at county-level out-migration using census data from 1950 and 1961.

County-Level Controls To control for pre-war differences in the economic and political situation of West German regions, we rely on county-level data from King et al. (2008). In addition, we digitized county-level population and labor market data from the Statistical Yearbook of the German Reich in 1939. Finally, to account for differences in the extent of war destruction — an important potential

[8] For instance, if the benchmark property tax rate was 1% and a municipality's collection rate 500%, the tax rate was $1\% \times 500\% = 5\%$.

confounder — we extracted county-level data on the share of destroyed housing from the statistical offices of several federal states. We managed to obtain this data for all states except Hesse, the city states Hamburg and Bremen as well as the parts of Baden-Württemberg that were administered by France after the end of the war. See Appendix Table A for details on each control variable and its respective source.

3.1.2 Main Estimation Sample: Descriptive Statistics

The number of observations in the estimation sample differs for each outcome variable, because not all outcomes are reported in all years and the Statistical Yearbooks only report some information for cities with more than 20,000 inhabitants. See Appendix Tables A.1 and A.2 for more information. We further restrict our sample to regions for which we have data on the extent of war destruction. Given the importance of available housing for the initial allocation of the expellees as well as its potential impact on public finances, we consider war destruction as an imperative control. However, its inclusion in the analysis reduces the number of observations for most outcomes to about 233 per year. In Appendix Table C.1, we show that — once controlling for city size — the absence of information on war destruction is unrelated to the share of expellees, which mitigates concerns that data may not be missing at random.

Table 1: Descriptive Statistics for the Main Variables

	Mean	Std Deviation	Minimum	Maximum	Observations
A. Expellee Inflow (in %)					
Expellee Share (1950)	15.439	8.67	1.80	44.10	233
B. Local Tax Rates (in %)					
Mean Agricultural Land Tax (1949-64)	1.533	0.43	1.00	3.00	233
Mean Residential Property Tax (1949-64)	2.258	0.38	1.24	3.26	233
Mean Business Capital/Profit Tax (1949-64)	0.537	0.06	0.44	0.70	233
Mean Business Wage Bill Tax (1949-64)	1.794	0.36	0.55	3.00	128
C. Spending, Debt and State Transfers					
Mean P.C. Spending on Welfare (1950-59)	13.401	6.39	0.84	30.81	233
Mean P.C. Spending on Education/Admin (1950-59)	59.427	14.70	26.93	111.99	233
Mean P.C. Spending on Public Infrastructure (1950-59)	35.957	15.98	11.63	165.30	233
Mean P.C. Spending on Health/Housing (1950-59)	8.949	10.32	0.65	132.77	233
Mean Spending Share on Welfare (1950-59)	0.114	0.04	0.01	0.24	233
Mean Spending Share on Education/Admin (1950-59)	0.500	0.05	0.25	0.67	233
Mean Spending Share on Public Infrastructure (1950-59)	0.291	0.06	0.16	0.51	233
Mean Spending Share on Health/Housing (1950-59)	0.096	0.05	0.01	0.29	233
Mean Total (Log) Debt (1950-59)	8.706	1.09	6.93	11.96	217
Mean P.C. Debt (1950-59)	139.487	77.04	28.22	473.00	217
Mean Total (Log) Transfers (1950-59)	7.279	1.36	1.79	10.85	233
Mean P.C. State Transfers (1950-59)	39.279	22.68	0.25	108.56	233
D. Voter Turnout and Vote Shares					
Mean Voter Turnout (1946-61)	0.748	0.05	0.57	0.89	217
Mean Vote Share CDU/CSU and SPD (1946-61)	0.740	0.10	0.48	0.96	217
Mean Vote Share GB/BHE (1953-61)	0.035	0.03	0.00	0.18	217
Mean Vote Share Other Parties (1953-61)	0.237	0.10	0.00	0.50	217
Mean Seat Share GB/BHE (1953-61)	0.031	0.04	0.00	0.19	199
GB/BHE In Local Parliament (1953-61)	0.422	0.50	0.00	1.00	199

Notes: This table presents descriptive statistics for our outcome variables. All monetary variables are expressed in 1950 prices.

Table 1 displays descriptive statistics for the main variables. Panel (A) shows that the average population share of expellees in 1950 was 15.4% among those cities included in the baseline estimation sample. This figure is slightly lower than for the whole of Germany, which stands at 19%. The discrepancy most likely results from our restriction of the sample to cities with more than 10,000 individuals. Recall that expellees were disproportionately allocated to smaller towns and rural places upon arrival.

Panels B and C summarize the public finance data. In line with the empirical analysis, we report averages over the respective sample periods. Panel B shows significant variation in average post-war tax rates. The variation was highest for the agricultural land tax ($sd/mean = 28\%$) and lowest for the tax on capital and profits ($sd/mean = 11\%$). Cities also considerably differed in the amount of spending on different items. On average, the largest share (59 DM per person per year or 50% of total spending) went to education and administration. This is followed by spending on public infrastructure (36 DM, 29% of total spending), welfare (13.4 DM, 11% of total spending) and health and housing (8.9 DM, 10% of total spending). However, variation across cities was substantial, amounting to 24% (sd/mean) in spending on education/administration and 48% in welfare spending. Average local debt amounted to 139.5 DM per person, and state transfers totaled 39.3 DM per capita.

Finally, Panel D summarizes the local election data. The average voter turnout — i.e. the number of votes cast divided by the number of eligible voters — was 75%. The large parties — the CDU/CSU and SPD — received the large majority of votes. The expellee party GB/BHE received only 3.5% of votes on average, although its average vote share reached 18% in Salzgitter, the city with the largest expellee share in our sample. The variation in the party's vote shares is also reflected in its representation in local parliaments. In councils in which the GB/BHE held at least one seat, the seat share ranged between 2% and 19%.

3.2 Survey Data

To study the expellees' political attitudes as well as the long-term effects of the expulsion on preferences for redistribution, we complement our main dataset with individual-level data from two surveys. The *Bundesstudie* — carried out in 1953 — was among the first representative surveys of West German citizens' political, social and cultural attitudes. Importantly, the survey includes an expellee identifier, which allows us to study the political preferences of the expellees shortly after arrival.

The German Socio-Economic Panel (SOEP) — established in 1984 — is a longitudinal survey of German households. In 1997 and 2002, the SOEP included questions on individuals' preferences for the role of the government in providing financial security (i) in case of unemployment, (ii) in case of illness, (iii) for families, (iv) when retired, or (v) when needing care. We use these variables to study the long-run impact on preferences for redistribution. Descriptive statistics for both datasets are shown in Appendix Tables A.3 and A.4, respectively.

4 Empirical Strategy

4.1 Empirical Model

We estimate the effect of the expellee inflow on policy variables such as tax rates, spending or debt based on the following regression model

$$y_{mcs} = \alpha + \beta\ ExpShare_{cs} + \boldsymbol{X'_{mcs}\rho} + \delta_s + \varepsilon_{mcs}, \tag{1}$$

whereby the post-war outcome y_{mcs} refers to a policy variable of city m, located in county c and state s. The regressor of interest $ExpShare_{cs}$ represents the initial share of expellees in the population of county c, as measured in 1950.[9] The vector X_{mcs} includes a rich set of control variables that account for social and economic differences across West Germany before World War II. In our most comprehensive specification, we also condition on state fixed effects, δ_s, which capture persistent differences across states due to geography or historical experiences. State fixed effects also account for regional clustering of public finance outcomes due to varying fiscal equalization schemes across federal states. Finally, the error term ε_{mcs} summarizes all determinants of public policies at the city level that are not captured by the control variables.

The coefficient of interest, β, is estimated from cross-city differences in the initial population share of expellees. It is to be interpreted as a reduced-form coefficient, which measures the *total* effect of the initial inflow on public finances. As such, it summarizes many channels through which the inflow of expellees affects policy setting, such as population growth, structural change of the local economy or changes in trade flows. Any such forces represent mediators, namely channels through which the initial inflow may have affected the outcome.

Control Variables We distinguish between two sets of control variables. *Institutional controls* include dummy variables for the three western occupation zones, and a dummy variable that equals unity if a city is located close to the border between East and West Germany. The occupation zone dummies control for common shocks within the occupation zones due to varying policies by the three Western Allies. The border dummy controls for the lower growth trajectory of cities close to the inner-German border after the war, as a direct consequence of Germany's division in 1949. Redding and Sturm (2008) show that the economic consequences of closeness to East Germany were concentrated within approximately 75km of the border, which is why we define our dummy variable accordingly.

The set of *historical political and economic controls* includes county-level measures of social and economic differences across West Germany before World War II. The average local vote share for the SPD in the national elections between 1924 and 1933 as well as the respective local population shares of Protestants and Jews in 1925 serve as proxies for potentially persistent differences in political attitudes, work ethic and social norms. The respective shares of self-employed and unemployed workers in 1933, the share of manufacturing workers in the workforce (as of 1933) and the (log)

[9] The share of expellees is measured at the county level whereas the outcomes are measured at the city level. We chose the main regressor at the county level because the 1950 census, which is only available at the county-level, provides the best measure of the initial allocation of the expellees. The first measure of the city-level share of expellees is available for 1952, and we use this in a robustness check.

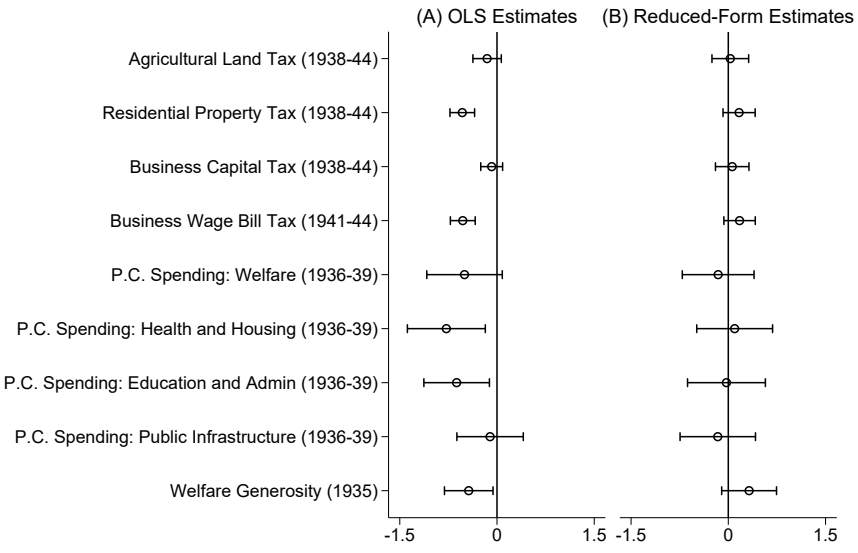
population density in 1939 account for local differences in economic prosperity before the war. Finally, to proxy the degree of local war destruction, vector X_{mcs} comprises the county-level share of housing units destroyed. This control holds particular importance because cities with greater housing destruction received fewer expellees. At the same time, they had good reasons to raise taxes to finance the city's reconstruction.

Inference In our baseline specification, we cluster the standard errors at the county level to account for any potential correlation in the error terms across cities within a county. In sensitivity checks, we show that inference is robust to adjustments for multiple hypothesis testing (Romano and Wolf, 2005) and spatial autocorrelation (Conley, 1999, Kelly, 2019).

4.2 Identification

To identify causal effects based on simple OLS regressions, the expellees would have to be randomly assigned across West Germany. This assumption is unlikely to hold if the Allied Forces assigned the expellees to their initial location of residence based on local amenities. Placebo regressions shown in Panel A of Figure 4 suggest that this was indeed the case. Here, we regress city-level outcomes before World War II — tax rates, spending and welfare generosity — on the share of expellees in 1950 and state fixed effects. We find that the expellees were disproportionately allocated to cities with lower tax rates, lower per-capita spending and lower welfare generosity before World War II.

Figure 4: Placebo Tests for Expellee Share and Instrument



Notes: This graph displays the point estimates and 95% confidence intervals of regressions of pre-war outcomes on the county-level share of expellee (Panel A) and the instrument (Panel B), respectively. The outcomes refer to (average) tax rates, spending per capita and welfare generosity before World War II. In all regressions, we condition on federal state fixed effects. Standard errors are clustered at the county level.

Instrumental Variables Strategy To address endogeneity due to non-random sorting, we apply an instrumental variable strategy that leverages push factors in the migrant-sending regions. The strategy follows work on the Great Migration in the U.S. by Boustan (2010), who predicts outflows based on local economic shocks in the sending regions and subsequently assigns these outflows to destinations based on existing travel linkages. By construction, the instrument is thus independent of pull factors in the destination cities that could have influenced migrants' location choices.

In our case, the push factor that caused the mass outflow was the end of World War II in 1945, which led to the expulsion of twelve million Germans from Central and Eastern Europe. The number of Germans in these sending regions before the war provides us with a prediction of the number of Germans forced to leave their homes at the end of World War II. The instrument assigns these varying local outflows to the receiving regions by means of bilateral distances.

Constructing the Instrument We construct the instrument in three steps. In a first step, we predict the outflows from each county i in the ceded Eastern Territories as well as Czechoslovakia.[10] Given that all Germans in these areas were forced to move after the end of the war, the number of Germans in county i in 1939, Pop_i^{1939}, serves as a predictor of the actual outflow. To facilitate interpretation, we scale the population size of a sending county i to the total German population in the Eastern Territories and Czechoslovakia and calculate the population share of county i in 1939:

$$sh_i^{1939} = Pop_i^{1939} / (\sum_i Pop_i^{1939}). \tag{2}$$

In a second step, we obtain a prediction of the bilateral flow from a sending county i to a receiving West German county c by multiplying the predicted outflows with a measure of the moving costs between the two counties. We use the simple crowfly distance, $dist_{ic}$, as a proxy for the Allied Forces' costs of allocating migrants from county i to county c. Finally, to obtain a prediction of the inflow into each western county c, we sum over all predicted bilateral flows into county c:

$$\Delta Pop_{cs} = \sum_i sh_i^{1939} \times dist_{ic}. \tag{3}$$

First Stage The corresponding first-stage relationship between the instrument, ΔPop_{cs}, and the county-level share of expellees is then defined by

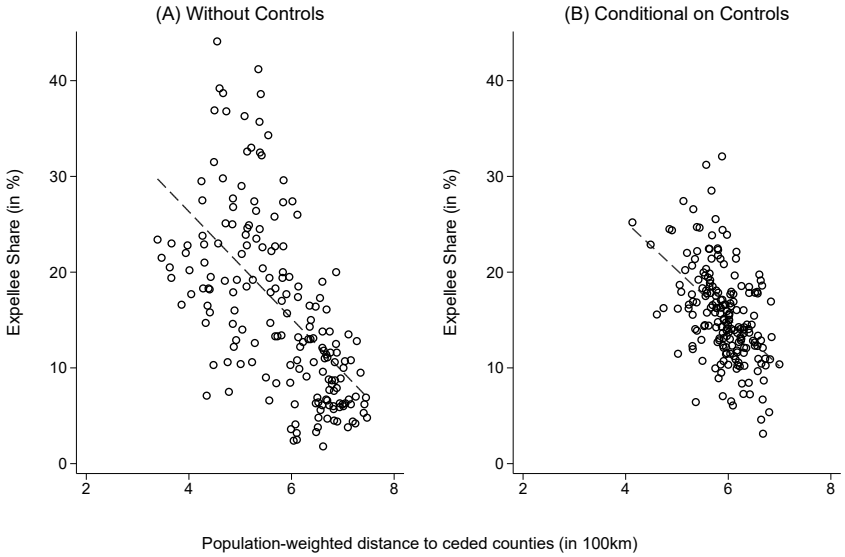
$$ExpShare_{cs} = \gamma_0 + \gamma_1 \Delta Pop_{cs} + \boldsymbol{X'_{mcs}}\psi + \delta_s + \eta_{mcs}. \tag{4}$$

As expected, Figure 5 shows a negative correlation between the instrument and the expellee share. The greater the distance between a receiving county in West Germany and the sending counties in the ceded Eastern Territories and Czechoslovakia, the lower the expellee share. This relationship holds true unconditional and conditional on the set of county-level controls described above; see Panels (A) and (B). This means that — even within federal states — fewer expellees arrived in the respective western than eastern parts. In Appendix Figure B.6, we plot the corresponding first-stage

[10] Note that this accounts for more than 80% of the expelled Germans. We do not have county-level information on the number of Germans who lived in other countries before World War II.

relationship separately for each state covered in the baseline sample. We find negative relationships for five of the six federal states. The fact that the correlation remains unaffected by the set of controls suggests that push factors and distance explain a significant share of the overall variation in the expellee inflow. The corresponding first-stage F-statistics are well above 20 for all samples, which suggests that the instrument is sufficiently strong. In Appendix Figure B.7, we present the geographic distribution of the (predicted) inflows in maps.

Figure 5: First-Stage 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 4.1 is included. In order to make two graphs comparable, we added the sample means of both variables to each observation.

Threats to Identification The proposed instrument only derives causal effects if it satisfies the exclusion restriction. Conditional on controls, the instrument should only affect the outcome through its impact on the inflow of the expellees. Thus, it has to be uncorrelated with any unobserved determinant of public finances in Equation (1), $Cov(\Delta Pop_{cs}, \varepsilon_{mcs} | \boldsymbol{X_{mcs}}, \delta_s) = 0$.

The most important threat to identification in this context is the division of Germany, which coincided with the inflow of the expellees. West German regions along the newly-established Iron Curtain became economically remote overnight and were particularly affected by the loss of trading partners on the other side of the Iron Curtain (Redding and Sturm, 2008). Without appropriate controls, the proposed instrument might thus affect public policy setting through additional channels besides the expellee inflow. For this purpose, we consider our most comprehensive specification of the IV strategy — including the full set of controls — as the preferred one throughout the paper. We argue that conditional on controls — in particular federal state fixed effects and the dummy variable indicating whether a city is within 75km of the inner-German border — the instrument should have no direct effect on the outcomes.

Placebo Regressions To corroborate the exclusion restriction, we show that the instrument is uncorrelated with pre-war outcomes. In Panel B of Figure 4, we separately regress each pre-war outcome on the instrument, conditioning on state fixed effects. All coefficients are close to zero and statistically insignificant. We take this result as suggestive evidence for the quasi-random assignment of the instrument, a necessary condition for instrument validity. Nevertheless, we test the sensitivity of our IV strategy below. First, we drop cities close to the border. Second, we follow Conley et al. (2012) and assess the robustness of our IV results when allowing for violations of the exclusion restriction.

5 Main Results — The Effect of the Expellee Inflow on Public Finances

In this section, we present the main results of our empirical analysis. We show that West German cities reacted to the inflow of eight million poor voters with shifts in spending and selective tax raises. High-inflow cities also incurred more total debt, although they did not increase debt per person.

5.1 Effect on Spending, Taxes, Debt and Transfers

Spending We begin by analyzing the effect of the expellee inflow on public spending. For each spending category, we consider the amount spent per capita as well as its share in total spending. Both outcomes help us to answer different questions. Per-capita effects inform about shifts in the cities' spending priorities. For example, a positive effect would indicate that high-inflow cities spent disproportionally more on a given category, over and above the amount that would be due to the change in population size. In turn, effects on spending shares tell us whether the expellee inflow affected the relative importance of a given spending category in the cities' overall budget. This effect would then be the result of two forces, namely the change in population size and the change in population composition.

The results are displayed in Table 2. While we report the OLS results for reference, our focus is on the IV estimates. All outcomes refer to the average amount per capita or share within a municipality over the 1950-59 period. Below, we investigate the pattern of these effects over time. To make the coefficients comparable across outcomes, we standardize outcomes and the regressor of interest to a mean of zero and a standard deviation of one. Thus, each coefficient measures the effect of a one-standard-deviation increase in the share of expellees (around 8.7pp) on mean per-capita spending or, respectively, the mean spending share in a given category.

We begin with the effects on per-capita spending. The IV coefficient in Column (1) of Table 2 shows that the expellee inflow strongly increased cities' welfare spending per capita. On average, a one-standard-deviation increase in the population share of expellees raised per-capita welfare spending by around 71% of a standard deviation. Given the strong initial welfare dependence of the expellees, this effect may not be surprising. However, it does not reflect a purely mechanical shift, as cities had considerable discretion over the generosity of welfare payments. For all other spending categories, we do not find statistically significant effects (see Columns (2) to (4)). Notably, the negative effect on public infrastructure is large but imprecisely estimated.

Table 2: Expellee Inflows and Local Spending - Average Effect post WW II

	Local Per Capita Spending on				Local Spending Shares on			
	Public Welfare	Education/ Admin	Public Infrastr.	Health/ Housing	Public Welfare	Education/ Admin	Public Infrastr.	Health/ Housing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. OLS Estimates								
Expellee Share	0.065	-0.294***	-0.368***	-0.241**	-0.095	0.264	-0.050	-0.152
	(0.110)	(0.093)	(0.108)	(0.106)	(0.161)	(0.186)	(0.138)	(0.147)
B. 2SLS Estimates								
Expellee Share	0.709**	0.301	-0.476	-0.135	0.873**	0.420	-0.836**	-0.197
	(0.325)	(0.335)	(0.370)	(0.364)	(0.412)	(0.437)	(0.404)	(0.389)
C. Reduced Form								
Population-Weighted Distance	-0.261***	-0.111	0.175	0.050	-0.321**	-0.155	0.307**	0.073
	(0.099)	(0.119)	(0.134)	(0.135)	(0.131)	(0.161)	(0.131)	(0.144)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	233	233	233	233	233	233	233	233
Kleibergen-Paap F-Statistic	24.95	24.95	24.95	24.95	24.95	24.95	24.95	24.95

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on mean post-WW II per capita spending and spending shares on different items using simple OLS and the IV strategy laid out in Equations (1)-(4). Annual information on local post-war spending post is given for the period 1950-1959. The set of controls includes measures for institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 4.1 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Columns (5)-(8), we report the results for each category's share in total spending. We find that the inflow of the expellees increased the share of welfare spending, and reduced the share of spending on public infrastructure (see Columns (5) and (7)). On average, a one-standard-deviation higher share of expellees increased the share of welfare spending in total spending by 87% of a standard deviation (3.48 percentage points), and reduced the share of public infrastructure by 81.5% of a standard deviation (5.85 percentage points).

Tax Rates We next investigate whether the shifts in spending are accompanied by changes in local taxation. Table 3 displays the effects on average tax rates over the 1949-1964 period. The results indicate that local tax rates were an important adjustment channel for public finances. We find large and statistically significant effects for the two most revenue-intensive taxes: the residential property tax and the tax on businesses' capital and profits. On average, a one-standard-deviation increase in the share of expellees is associated with an increase in the property tax rate by around 82% of a standard deviation (0.31 percentage points, 13.8% of the mean) and an increase in the local business tax by 89% of a standard deviation (0.05 percentage points, 9.94% of the mean). The effects on the agricultural property tax (+23% of a standard deviation, 6.4% of the mean) and the wage bill tax (+47% of a standard deviation, 9.4% of the mean) are also positive, but smaller and not statistically significant.

Debt In addition to the increase in local tax rates, cities might have financed the inflow-induced shifts in spending via debt. In Columns (1) and (2) of Appendix Table C.2, we report the corresponding IV estimates for total debt (in logs) and debt per capita, respectively. The outcomes refer to the average level of total debt (per capita) between 1950 and 1959. From Column (1), we infer that

Table 3: Expellee Inflows and Local Tax Rates - Average Effect post WW II

	Local Tax Rate on			
	Agricultural Land	Residential Property	Business Capital/Profits	Business Wage Bill
	(1)	(2)	(3)	(4)
A. OLS Estimates				
Expellee Share	0.050	-0.112	0.060	0.111
	(0.112)	(0.108)	(0.127)	(0.295)
B. 2SLS Estimates				
Expellee Share	0.232	0.823**	0.892**	0.470
	(0.280)	(0.318)	(0.396)	(0.401)
C. Reduced Form				
Population-Weighted Distance	-0.085	-0.303***	-0.328***	-0.255
	(0.103)	(0.099)	(0.126)	(0.220)
Controls	Yes	Yes	Yes	Yes
Number of Observations	233	233	233	128
Kleibergen-Paap *F*-Statistic	24.95	24.95	24.95	35.43

Notes: This table shows the effect of a one standard deviation increase in the expellee share on mean post-WW II tax rates using simple OLS and the IV strategy as laid out in Equations (1)-(4). Annual information on local tax rates is given for the period 1945-1964. 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 4.1 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the expellee share had a positive and statistically significant effect on log total debt. On average, a one-standard-deviation increase in the share of expellees increased the (log) total amount of debt by 88% of a standard deviation. However, the increase appears to be entirely driven by the change in population size rather than its composition. The effect on debt per capita is small and imprecisely estimated; see Column (2) of Table C.2.

Dynamic Effects So far, the results of this section point to substantial shifts in cities' public finances in response to the expellee inflow. In the final part of this section, we analyze the dynamics of the observed effects. This will allow us to learn about both the persistence of the effects over the sampling period and the factors driving the average effects. Based on the same IV model as before, we now estimate separate effects for two years at a time (i.e. 1950/51, 1952/53, etc.).

Panel (A) of Appendix Figure C.1 shows that the effect on per-capita welfare spending is highly persistent over the course of the 1950s. We take this persistence as suggestive evidence against a purely mechanical increase in welfare spending right after the expellee inflow; rather, it points to changes in locals' preferences. This is corroborated by Appendix Figure C.2, which shows the corresponding effects on spending shares over time. We find a persistent — albeit less clear cut — positive effect on the share of welfare spending, along with a persistent, negative effect on the share of public infrastructure spending.

The results for local taxation reveal slight differences in the dynamic patterns across tax rates. As displayed in Panel (B) of Appendix Figure C.3, the overall increase in the residential property tax rate is mainly due to increases in the late-1940s and early-1950s, while effects are smaller but still positive and statistically significant thereafter. By contrast, the positive and statistically significant effect on the business capital tax rate even increases over the sample period (see Panel (C) of Appendix Figure

C.3).

Finally, in Panels (A) and (B) of Appendix Figure C.4, we analyze the effects on debt (per capita) over time. We find that the effect of the expellee inflow on total debt increases over the course of the 1950s, whereas effects on per-capita debt are close to zero and statistically insignificant over the entire sampling period.

Discussion The results in this section show that cities adjusted their public finances in response to the expellee inflow along several margins. Cities with a higher population share of expellees increased spending on public welfare (at the expense of investments in local public infrastructure) and selectively raised local taxes. These changes in local public finance persisted over the course of our sampling period, i.e., at least until the early-1960s. These results are consistent with median voter models of the welfare state such as Meltzer and Richard (1981). The inflow of poor voters led to an increase in poverty rates, which in turn affected the local level of taxation and spending. Although these models make no prediction regarding *which* spending items and tax rates should be changed most, the observed changes appear plausible. On the spending side, cities had to cope with a sharp increase in welfare spending. One way balance their books was to spend less on infrastructure while spending for education and the local administration are rather complementary to welfare spending.

On the taxation side we see raises for taxes on residential property and taxes on business capital, but not for taxes on agricultural property or businesses' wage bill. One potential reason why these taxes were chosen was their revenue generating potential, which was a multiple of the potential of the other taxes. Moreover, property and to a lesser extent businesses are fairly immobile and, hence, inelastic assets, which means that an increase in taxes should not trigger strong behavioral responses.

The persistence of local tax rate differences may appear surprising in light of standard models of tax competition. With perfect mobility of firms, these models would not allow for a tax wedge between regions because the region with a higher tax would lose all its firms (Wilson, 1986). Instead, our findings are consistent with models that incorporate agglomeration effects (Andersson and Forslid, 2003, Baldwin and Krugman, 2004). It is plausible that the arrival of the expellees — for example, by raising the quantity of cheap labor — increased agglomeration rents and therefore helped to sustain a tax wedge between high- and low-inflow cities.

5.2 Robustness Checks

To assess the robustness of these baseline results, we perform a large series of robustness checks. We summarize the main results below. Detailed results can be found in the appendix.

OLS vs. IV Throughout Section 5, we reported coefficients from simple OLS regressions along with the IV results. Almost without exception, the 2SLS coefficients are considerably larger in absolute value than the OLS estimates. This finding is consistent with the results from Panel (A) of Figure 4 and suggests that endogeneity works against finding significant effects. In Tables 2 and 3, we also report the reduced-form results of the instrument. It is reassuring to observe that statistically significant 2SLS and reduced-form estimates go hand in hand. This indicates that the 2SLS results do not result from weak instruments.

Varying Sets of Controls In Appendix Tables C.3-C.7, we report IV estimates when using varying sets of controls. In each table, we move from a minimal to a comprehensive set of controls. For the majority of outcomes, the sign and statistical significance of the estimates is robust to different specifications. In addition, effect sizes increase as more controls are added.

Municipality-Level Expellee Shares Our regressor of interest is the *county-level* share of expellees in 1950. It is the first available country-wide measure of the expellee inflow in post-war Germany. We choose this variable because it best reflects the initial assignment of the expellees. However, it differs from the set of city-level outcome variables regarding its level of aggregation. In Appendix Table C.8, we re-estimate the main regressions with the *city-level* share of expellees in 1952. The effects are of similar sign and size but slightly weaker, perhaps due to internal migration between 1950 and 1952.

Excluding Cities Close to the Border To address the concern that cities close to the inner-German border experienced slower growth due to remoteness (Redding and Sturm, 2008), we re-estimate the main effects when excluding cities within 75km from the border. The effects based on this restricted sample — shown in Appendix Table C.9 — are very similar to our baseline effects. This suggests that the division of Germany does not confound our estimates.

Direct Effects of the Instrument Using the method of Conley et al. (2012), we further assess the robustness of the IV estimates with respect to small violations of the exclusion restriction. Importantly, even if the exclusion restriction — $Cov(\varepsilon_{mcst}, \Delta Pop_{cs} | \boldsymbol{X_{mcs}}, \delta_s) = 0$ — does not hold exactly, the estimates may still have a causal interpretation if this correlation is relatively small. In Appendix Figures C.5-C.8, we calculate the largest direct effect of the instrument on each outcome for which the corresponding IV coefficient would still be statistically significant at the 10% level. To render most results insignificant, one would require an implausibly large degree of correlation between the instrument and the error term.

Inference: Multiple Testing and Spatial Autocorrelation In our main analysis, we cluster standard errors at the county level. In Appendix Table C.10, we show that our inference is robust to a correction for multiple hypothesis testing (Romano and Wolf, 2005), as well as spatial autocorrelation based on Conley (1999) standard errors.

6 Potential Mechanisms

We next investigate potential mechanisms behind the observed effects on public policy setting. First, in Section 6.1, we analyze the role of expellees' political engagement. In Section 6.2, we discuss additional — and to some extent competing — explanations, namely transfers from higher levels of government and internal migration.

6.1 Political Engagement of the Expellees

Political Interest We begin by documenting differences in political interest, engagement and attitudes between the expellees and the incumbent population shortly after the expulsions. For this purpose, we use individual-level data from the *Bundesstudie*, a representative survey of West Germans in 1953. The dataset covers 3,246 individuals and provides information about their family history, economic situation, as well as political attitudes. Importantly, it also includes information on the respondents' place of residence before World War II, which allows us to distinguish natives from expellees. Among all respondents, the share of expellees is 22%, which is comparable to their overall population share.

Table 4: Political Participation and Preferences: Natives vs. Expellees

	Political Interest	Political Participation	Intention to Vote	Preference for GB/BHE	Preference for CDU or SPD
	(1)	(2)	(3)	(4)	(5)
Expellee	0.058***	0.040**	0.024**	0.179***	-0.074***
	(0.021)	(0.019)	(0.011)	(0.015)	(0.022)
Number of Observations	3,225	3,234	2,805	3,244	3,244
Variable Mean	0.38	0.25	0.92	0.04	0.56
Controls	Yes	Yes	Yes	Yes	Yes

Notes: This table compares natives and expellees with regard their political participation and preferences. Results are based on individual-level survey data from the Federal Study 1953. The set of controls comprises indivduals' gender, age (in bins), denomination and federal state of residence. Standard errors are heteroscedasticity robust. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The OLS results are displayed in Table 4. They point to a significantly stronger political involvement of the expellees, as well as different political attitudes. Conditional on age, gender, religious denomination as well as federal state fixed effects, we find that the expellees were more likely to (i) be interested in politics, (ii) engage in politics, and (iii) show intentions to vote. These differences are economically relevant. For example, the results displayed in Columns (1) and (2) indicate that the expellees were 5.8 percentage points more likely to express political interest and four percentage points more likely to attend political events. These effects amout to around 15% of the respective outcome mean. At the same time, the results suggest that expellees were less likely to support the two major West German parties — CDU/CSU and SPD — while showing stronger support for the expellee party (GB/BHE). As displayed in Columns (4) and (5) of Table 4, the likelihood of expellees reporting an intention to vote for either CDU/CSU or SPD was 7.4 percentage points lower and voting for the GB/BHE was 17.9 percentage points higher than among natives.

Voting in Local Elections We next investigate whether the observed differences in individual political interest and involvement translate into differential voting outcomes across regions with low and high inflows of expellees. We use our instrumental variables strategy and focus on outcomes of city-level elections, i.e. we refer to the same level of government as before. In line with Section 5, we also report average effects over multiple elections over time, using information from 1946 and 1961.

The corresponding effects on local elections are displayed in Table 5. We first investigate the effect

of the expellee inflow on voter turnout. Theoretically, the effect is ambiguous. Standard voting models would predict that a larger electorate reduces turnout because each vote is less likely to be pivotal (Downs, 1957). On the other hand, the expellee inflow may have increased the economic and political stakes for different parts of the population, which may have had positive effects on turnout (Andersen et al., 2014). In the given setup, effects on turnout are close to zero and statistically insignificant (see Column (1) of Table 5).

Next, we look at party vote shares. Column (2) of Table 5 shows that the two major West German parties — CDU/CSU and SPD — received considerably fewer votes in those regions with a more substantial inflow of expellees. For a one-standard-deviation increase in the share of expellees, the joint vote share of the two parties decreased by 4.8 percentage points on average. By contrast, the expellee party as well as other political parties received more votes in these regions. From Columns (3) and (4), we infer that a one-standard-deviation increase in the expellee share raised the average vote share for the GB/BHE by 2.5 percentage points and for other parties by 3.7 percentage points, although the latter coefficient is imprecisely estimated. These results are consistent with the observed differences in self-reported preferences, and of considerable magnitude.

Table 5: Expellee Inflows and Local Voting - Average Effect post WW II

	Voter Turnout	Vote Share CDU/SPD	Vote Share GB/BHE	Vote Share Other Parties	GB/BHE in Council	Seat Share GB/BHE
	(1)	(2)	(3)	(4)	(5)	(6)
A. OLS Estimates						
Expellee Share	0.004	-0.006	0.020***	-0.005	0.129**	0.028***
	(0.008)	(0.012)	(0.005)	(0.013)	(0.065)	(0.006)
B. 2SLS Estimates						
Expellee Share	-0.009	-0.048*	0.025**	0.037	0.206	0.026**
	(0.019)	(0.028)	(0.010)	(0.027)	(0.148)	(0.013)
C. Reduced Form						
Population-Weighted Distance	0.004	0.021*	-0.011**	-0.016	-0.090	-0.011**
	(0.008)	(0.012)	(0.005)	(0.012)	(0.063)	(0.006)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	217	217	217	217	199	199
Kleibergen-Paap *F*-Statistic	33.97	33.97	33.97	33.97	32.25	32.25

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on mean post-WW II voter turnout, party vote shares and the presence of the GB/BHE in local councils using simple OLS and the IV strategy laid out in Equations (1)-(4). For voter turnout as well as the CDU/CSU and SPD vote share, the mean over the period 1946-61 is taken. Note that the GB/BHE first run in elections in 1953, limiting our observation perdod to 1953-61. The set of controls includes measures for institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing after the war (see Section 4.1 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

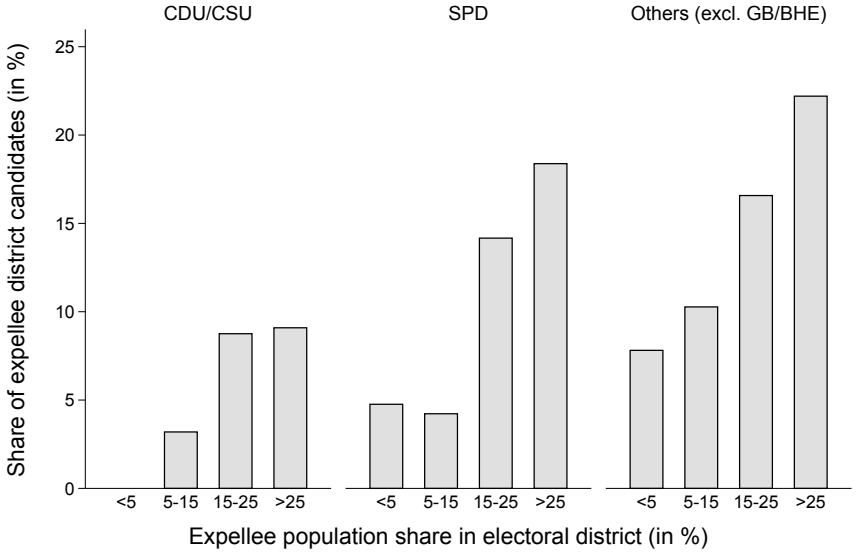
In Columns (5) and (6), we analyze whether a higher expellee share actually raised the expellee party's presence and strength in local parliaments. Directly investigating this question is important in our context because a non-zero vote share does not automatically translate into a seat in local parliaments. Columns (5) and (6) of Table 5 present the corresponding intensive and extensive margin results. Overall, the results point to stronger political representation of the GB/BHE in those areas with a more substantial inflow of expellees. Column (5) shows that a one-standard-deviation increase in the share of expellees increased the party's likelihood of having at least one seat in parliament by around nineteen percentage points, although this effect is imprecisely estimated. Moreover, within

those municipalities with a non-zero seat share for the GB/BHE, a larger expellee share among the population significantly raised the party's seat share in local parliament (see Column (6)). [11]

Candidate Selection As a final piece of evidence, we investigate the expellees' political engagement in parties besides the GB/BHE. We proxy their varying local influence via the number of expellees selected to run for office as so-called *direct candidates* in the federal elections between 1949 and 1961.[12] As each constituency only elects one representatives in a winner-takes-all contest, candidates are usually very carefully chosen and need to be well connected/influential within a party. In Figure 6, we correlate this measure of expellees' local political influence with the city-level population share of expellees. The figure suggests that an expellee's likelihood of nomination was substantially higher in areas with a large expellee inflow. This proportional relationship is remarkable because expellees had neither strong (political) networks upon arrival nor a majority in any constituency.

Based on the three pieces of evidence, we conclude that expellees played an important role in post-war West German politics. The stronger political engagement of the expellees is one explanation why their inflow led to greater redistribution.

Figure 6: Expellee Candidates in Federal Elections 1949–1961



Notes: This graph shows the share of direct candidates who were expellees in the federal elections in 1949, 1953, 1957 and 1961 for the Christian Democrats (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.

[11] In the Appendix, we show that the results on local voting are robust to the set of specification tests laid out in Section 5.2.

[12] Note that there is no equivalent information on candidates' place of birth for local elections.

6.2 Alternative Mechanisms

We next investigate possible mechanisms other than expellees' political influence: the role of (i) transfers from higher levels of government, and (ii) out-migration of natives, migrants, or both.

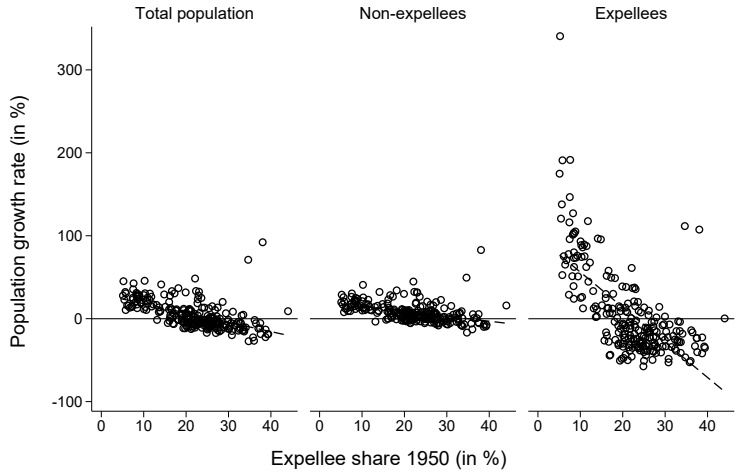
Transfers to cities and expellees Transfers from higher levels of government may explain the observed shifts in spending if transfers were disproportionately targeted towards high-inflow regions and issue-specific, i.e. to be spent for the well-being of the expellees only. Transfers came in two different forms. First, the federal and state governments set up transfer systems that supported cities subject to their economic situation. These were largely funded by federal tax revenues and the European Recovery Program. Second, transfers — in the form of compensation — were also directly given to the expellees.

Our IV estimates suggest that transfers cannot explain the observed effects on public policy setting. While we find a positive and statistically significant effect of the expellee share on the overall amount of transfers received, the effect on per-capita transfers is small and statistically insignificant (see Columns (3) and (4) of Appendix Table C.2). Hence, the shift towards more redistributive policies cannot be explained by policy responses at higher levels of government.

Out-Migration of Non-Expellees Research on the Great Migration in the U.S. suggests that the negative effect of internal migration on taxation and spending is largely driven by the out-migration of locals (Boustan, 2010, Tabellini, 2020b). This explanation is less plausible in post-war West Germany. In Figure 7, we document the internal migration patterns of expellees and non-expellees between 1950 and 1961. We find significant out-migration of the expellees from their initial residence to cities with a low initial expellee share, in many cases larger cities that were heavily destroyed during the war and re-built over the course of the 1950s. On the other hand, we observe no similar movements for natives.[13]

[13]Moreover, research by Schumann (2014) shows that even the out-migration of expellees from their initial location was sluggish. He exploits the fact that very few expellees initially settled in the French occupation zone and shows that the population gap between neighboring municipalities in the French and American zones persisted until the 1970s.

Figure 7: Population Growth for Natives and Expellees, 1950–1961



Notes: This graph displays the correlation between the share of expellees in 1950 and the population growth rates from 1950 to 1961 for the total population, the non-expellee (native) population and the expellee population. The dashed lines indicate fitted linear regressions. All variables are measured at the county level, based on census data from Schmitt et al. (1994). In this dataset, the 1950 population share of expellees is missing for all counties in the states of North Rhine-Westphalia and Rhineland-Palatinate, which had low population shares of expellees (cf. Figure 2).

7 Long-Run Effects: Preferences for Redistribution 50 Years Later

In Section 5, we documented a substantial and lasting shift towards more redistributive spending in response to the expellee inflow. Even ten years after the expulsions, high-inflow cities showed higher per-capita welfare spending than cities less affected by the migration shock. In the final part of this paper, we investigate whether the impact of the expellee inflow persisted over several decades.[14] For this purpose, we link our county-level data to rich individual-level survey data from the SOEP and assign treatment based on the respondents' current county of residence.[15] 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.

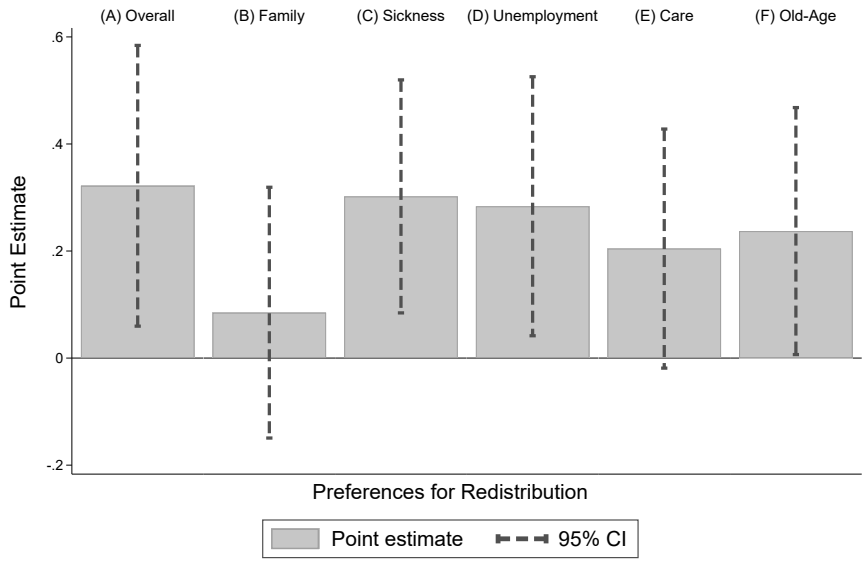
To measure individual preferences for redistribution, we follow Alesina and Fuchs-Schündeln (2007) and use questions from the 1997 and 2002 waves of the survey about respondents' preferred role of the state in different domains of social security: financial protection (i) for the family, (ii) when old, (iii) when in need of care, (iv) when sick, and (v) when unemployed. The response options for each domain were provided on a five-point scale, with higher values indicating a preference for a stronger role of the state in these matters (see Appendix Table A for details). In our analysis, we use dummy variables that equal one if a person states that the responsibility for a given domain should rest solely or mostly with the state. We also construct a standardized index, adding up the five outcomes for each individual and standardizing this sum to a mean of zero and a standard

[14] One way to study long-run effects would be to consider taxation and spending in the same cities over an even longer period than 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, thus making it difficult to link the data over time.

[15] SOEP, data for 1984-2016, version 33, SOEP, 2017, doi: 10.5684/soep.v33. See Goebel et al. (2018) and Appendix Tables A.3 for more details.

deviation of one.

Figure 8: Effect of Expellee Inflows on Preferences for Redistribution 50 Years later - 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 given on a five-point scale, where higher values indicate stronger preferences for redistribution. We employ the IV model laid out in Equations (1)–(4). The set of controls comprises (i) respondents' characteristics, and (ii) historical controls (see Section 4 for details) to capture persistent differences across regions. Cross-sectional weights are used. Standard errors are clustered at the county level.

Applying the instrumental variables strategy from Section 4 to this setup, we find that individuals in counties with a larger expellee share in 1950 express substantially stronger preferences for redistribution in the early-2000s. The corresponding results are displayed in Figure 8. Each bar represents the point estimate of a one-standard-deviation increase in the share of expellees on a given outcome. Bar (A) indicates a strong positive and statistically significant effect of the expellee inflow effect on overall preferences for redistribution. On average, a one-standard-deviation increase in the expellee share raises preferences for redistribution by 30% of a standard deviation. The remaining bars also reveal strong positive effects for most individual domains. We find that a one-standard-deviation increase in the county-level share of expellees increases the share of people in favor of government intervention by around 8 and 32 percentage points on average. These effects are substantial, accounting for 25-100% of the respective variable mean (see Appendix Table A.3).

These results suggest that the sudden arrival of eight million expellees was a sufficiently large shock to persistently change the preferences of society. Moreover, they show that the effect of the inflow was anything but mechanical. While the inflow of poor people may have mechanically increased welfare spending in the short run, it is difficult to make a similar argument for the preferences of the next generations. Although it is beyond the scope of this paper to analyze in detail potential mechanisms that may explain this long-term effect, two potential explanations come to mind. One is the intergenerational transmission of preferences; for example, through the narrative of poor people receiving help through housing and welfare payments. Another potential channel is sorting based on preferences (Tiebout, 1956). As shown in previous sections, the initial inflow of expellees led to

more redistributive policies in high-inflow cities, which may have attracted people with stronger preferences for redistribution. However, regardless which channel dominates, 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 today.

8 Conclusion

We have studied the effect of the inflow of eight million forced migrants on public policy setting in post-war West Germany. Based on newly-digitized city-level data on public finances, we have shown that cities reacted to this poverty shock by implementing more redistributive policies. Cities shifted spending from infrastructure to welfare and selectively raised taxes. We further provide evidence that this effect was at least partly driven by the migrants' political engagement and preferences.

Our results provide new insights into the political economy of redistribution. They are in line with median voter models of redistribution such as Meltzer and Richard (1981), while providing little empirical support for models that emphasize the role of ethnic heterogeneity (Alesina et al., 1999) or the concentration of political influence (Benabou, 2000). On the empirical side, our results are in contrast to those in studies on the Great Migration in the U.S. (Boustan, 2010, Tabellini, 2020b, Derenoncourt, 2019). These studies find that the migration of Blacks into northern U.S. cities reduced redistribution at the local level, which goes hand in hand with outflows of Whites and changes in private school enrolment. While we cannot fully pin down the ultimate cause of the difference in effects, we provide several potential explanations. The expellees' economic situation was similar to that of many West Germans, many of whom suffered economic losses during the war. Consequently, many West Germans had an incentive to support the same policies as the expellees. In addition, the expellees and non-expellees were part of the same ethnic group, which lessened the scope for conflicts over optimal policy setting. Finally, unlike in the U.S., the inflow of expellees did not trigger outflows of the incumbent population.

While the expulsions of Germans may not be representative of most migration flows, our study holds two important implications for understanding the economic and political effects of migration. One is that the impact of migration depends on the distribution of voting rights. In post-war Germany, the migrants had voting rights upon arrival and could therefore influence policy setting. Although most international migrants do not have voting rights, our study provides an idea of the effect one that could expect if they did. If the migrants are poorer than natives — which is the case with most migration flows worldwide — we should expect to see greater support for redistribution.

A second implication concerns internal migration. With increasing urbanization, many countries experience the migration of poorer citizens from rural areas into cities. Our study suggests that cities with high inflows of internal migrants may expect a change in the support of public policies and ultimately policy setting.

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Appendix

A Data Appendix and Descriptive Statistics

For 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 in 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 available 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). In order 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).

In order 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 and describes all variables used in our analysis and details its corresponding source. Descriptive statistics for the set of city- and county-level variables are provided in **Table A.2**, for individual-level outcomes and controls in **Tables A.3** and **A.4**.

Data Description

Table A.1: Variables and Data Sources

Variable	Years	Source
Panel A – Expellee Data		
Expellee Share	1950	Information on the expellee share at the county level in 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	1950-1959	Information on cities' debt is 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	1936-1939, 1950-1959	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, (ii) education and administration, (iii) public infrastructure, and (iv) health and housing. The definition of these groups follows the general presentation in the "Statistical Yearbooks of German Municipalities". As the information on spending items varies in the degree of detail 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. Data coverage is somewhat lower before Word War II.
Unemployment Rates	1946-1962	Information on local unemployment is taken from the "Statistical Yearbooks of German Municipalities". Information is available for all cities as well as municipalities with more than 20,000 inhabitants in a given year.

continued

Table A.1 continued

Variable	Years	Source
Voting results	1946-1962	Data on voter turnout and party vote shares 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.
Welfare Generosity	1935	Information on local differences in welfare generosity is obtained from the "Statistical Yearbook of German Municipalities, 1935". Information on welfare generosity (*Fürsorgerichtsätze*) is given for cities with more than 20,000 inhabitants.
Welfare Recipients	1946-1959	Data on the number of individuals receiving social welfare benefits (*Fürsorge*) is taken from the "Statistical Yearbooks of German Municipalities". Information on the number of recipients is given for cities with more than 20,000 inhabitants only.
Panel C – City- and County-Level Controls		
Border Region Dummy		Following 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 4.2. 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 respective population shares of Protestants and Jews in 1925, (ii) the mean election vote share for the Social Democratic Party (SPD) in the elections between 1925 to 1933, and (iii) the respective shares of self-employed and unemployed workers in the population, as well as the share of manufacturing workers in the workforce in 1933. All data are taken from King et al. (2008).

continued

Table A.1 continued

Variable	Years	Source
Housing Destruction	1945-1950	Information on the extent of destroyed housing 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.*
Occupation Zone Dummies		We assign each county to the respective occupation zone administrated by the US, GB 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)".
State FE		In our most comprehensive specification, we control for differences across federal states in a non-parametric way.
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.

continued

Table A.1 continued

Variable	Years	Source
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*". For each outcome, we create a binary indicator that equals unity if the response is "*mostly the state*", to "*only the state*" and zero otherwise.
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 in 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.
Panel F – Survey Data on Expellees		
Political Preferences/Interest	1953	To investigate differences in political preferences and participation between expellees and natives, we draw on survey data from the *Bundesstudie 1953*. In this survey, West Germans were, among others, asked about their general lifestyle, employment and political participation/interest. Importantly, the survey also provides information on respondents' place of residence before World War II, which allows us to distinguish expellees from "native" West Germans in the survey. The data set is accessible via the GESIS - The Leibniz Institute for Social Sciences: *Reigrotzki, Erich (2015): Bundesstudie 1953. GESIS Datenarchiv, Köln. ZA0145 Datenfile Version 2.0.0, https://doi.org/10.4232/1.11992*.

Descriptive Statistics

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

	Mean	Std Deviation	Minimum	Maximum	Observations
A. Expellee Inflow (in %)					
Expellee Share (1950)	15.439	8.67	1.80	44.10	233
B. Local Tax Rates (in %)					
Mean Agricultural Land Tax (1949-64)	1.533	0.43	1.00	3.00	233
Mean Residential Property Tax (1949-64)	2.258	0.38	1.24	3.26	233
Mean Business Capital/Profit Tax (1949-64)	0.537	0.06	0.44	0.70	233
Mean Business Wage Bill Tax (1949-64)	1.794	0.36	0.55	3.00	128
C. Spending, Debt and State Transfers					
Mean P.C. Spending on Welfare (1950-59)	13.401	6.39	0.84	30.81	233
Mean P.C. Spending on Education/Admin (1950-59)	59.427	14.70	26.93	111.99	233
Mean P.C. Spending on Public Infrastructure (1950-59)	35.957	15.98	11.63	165.30	233
Mean P.C. Spending on Health/Housing (1950-59)	8.949	10.32	0.65	132.77	233
Mean Spending Share on Welfare (1950-59)	0.114	0.04	0.01	0.24	233
Mean Spending Share on Education/Admin (1950-59)	0.500	0.05	0.25	0.67	233
Mean Spending Share on Public Infrastructure (1950-59)	0.291	0.06	0.16	0.51	233
Mean Spending Share on Health/Housing (1950-59)	0.096	0.05	0.01	0.29	233
Mean Total (Log) Debt (1950-59)	8.706	1.09	6.93	11.96	217
Mean P.C. Debt (1950-59)	139.487	77.04	28.22	473.00	217
Mean Total (Log) Transfers (1950-59)	7.279	1.36	1.79	10.85	233
Mean P.C. State Transfers (1950-59)	39.279	22.68	0.25	108.56	233
D. Voter Turnout and Vote Shares					
Mean Voter Turnout (1946-61)	0.748	0.05	0.57	0.89	217
Mean Vote Share CDU/CSU and SPD (1946-61)	0.740	0.10	0.48	0.96	217
Mean Vote Share GB/BHE (1953-61)	0.035	0.03	0.00	0.18	217
Mean Vote Share Other Parties (1953-61)	0.237	0.10	0.00	0.50	217
Mean Seat Share GB/BHE (1953-61)	0.031	0.04	0.00	0.19	199
GB/BHE In Local Parliament (1953-61)	0.422	0.50	0.00	1.00	199
E. Controls					
State of Schleswig-Holstein	0.056	0.23	0.00	1.00	233
State of Lower Saxony	0.124	0.33	0.00	1.00	233
State of North Rhine-Westphalia	0.489	0.50	0.00	1.00	233
State of Hesse	0.000	0.00	0.00	0.00	233
State of Rhineland-Palatinate	0.060	0.24	0.00	1.00	233
State of Baden-Wuerttemberg	0.103	0.30	0.00	1.00	233
State of Bavaria	0.167	0.37	0.00	1.00	233
City in former Prussia	0.657	0.48	0.00	1.00	233
City close to Iron Curtain	0.094	0.29	0.00	1.00	233
US Occupation Zone	0.670	0.47	0.00	1.00	233
UK Occupation Zone	0.270	0.45	0.00	1.00	233
French Occupation Zone	0.060	0.24	0.00	1.00	233
Share Unemployed (1933)	0.179	0.08	0.03	0.38	233
Share Self-Employed (1933)	0.158	0.05	0.06	0.27	233
Share Manufacturing Workers (1933)	0.427	0.25	0.15	3.85	233
Share Protestants (1925)	0.490	0.33	0.01	0.98	233
Share Jews (1925)	0.005	0.00	0.00	0.03	233
Vote Share SPD (1924-33)	0.190	0.09	0.03	0.45	233
Log Pop. Density (1939)	5.838	1.12	3.76	8.17	233
Share Destroyed Housing	0.201	0.17	0.00	0.78	233
F. Instrument					
Distance to East (in 100km)	5.951	0.98	3.39	7.47	233

Notes: This table presents descriptive statistics for our 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 – Expellee Share								
Expellee Share (1950)	0.15	0.09	0.08	0.13	0.22	0.02	0.44	8,986
Panel B – Dependent Variables								
State's Responsibility When Sick	0.39	0.49	0.00	0.00	1.00	0.00	1.00	8,986
State's Responsibility When Unemployed	0.65	0.48	0.00	1.00	1.00	0.00	1.00	8,986
State's Responsibility When Needing Care	0.45	0.50	0.00	0.00	1.00	0.00	1.00	8,986
State's Responsibility When Old	0.40	0.49	0.00	0.00	1.00	0.00	1.00	8,986
State's Responsibility For Families	0.34	0.47	0.00	0.00	1.00	0.00	1.00	8,986
Overall Preference for State Intervention	0.45	0.34	0.20	0.40	0.80	0.00	1.00	8,986
Panel C – Control Variables								
Age	34.20	9.22	27.00	34.00	41.00	17.00	52.00	8,986
Male	0.48	0.50	0.00	0.00	1.00	0.00	1.00	8,986
Education	2.84	1.52	2.00	3.00	4.00	0.00	6.00	8,986
Marital Status	1.73	0.60	1.00	2.00	2.00	1.00	3.00	8,986
Household Size	3.19	1.21	2.00	3.00	4.00	1.00	5.00	8,986
(Log) Household Income	7.99	0.56	7.66	8.01	8.35	2.30	10.31	8,986

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

Table A.4: Descriptive Statistics - Bundesstudie 1953

	Mean (1)	SD (2)	P25 (3)	P50 (4)	P75 (5)	Min (6)	Max (7)	N (8)
Panel A – Expellee Dummy								
Expellee	0.21	0.41	0.00	0.00	0.00	0.00	1.00	3,246
Panel B – Dependent Variables								
Interested in Politics	0.37	0.48	0.00	0.00	1.00	0.00	1.00	3,225
Political Participation	0.24	0.43	0.00	0.00	0.00	0.00	1.00	3,234
Intention to Vote	0.92	0.27	1.00	1.00	1.00	0.00	1.00	2,805
Stated Party Preference: GB/BHE	0.04	0.20	0.00	0.00	0.00	0.00	1.00	3,244
Stated Party Preference: CDU/CSU or SPD	0.55	0.50	0.00	1.00	1.00	0.00	1.00	3,244
Panel C – Control Variables								
Male	0.42	0.49	0.00	0.00	1.00	0.00	1.00	3,246
Confession: Protestant	0.49	0.50	0.00	0.00	1.00	0.00	1.00	3,246
Confession: Catholic	0.47	0.50	0.00	0.00	1.00	0.00	1.00	3,246
Confession: Jewish	0.04	0.19	0.00	0.00	0.00	0.00	1.00	3,246
Age: 18-24	0.12	0.33	0.00	0.00	0.00	0.00	1.00	3,246
Age: 25-29	0.11	0.31	0.00	0.00	0.00	0.00	1.00	3,246
Age: 30-44	0.32	0.47	0.00	0.00	1.00	0.00	1.00	3,246
Age: 45-59	0.28	0.45	0.00	0.00	1.00	0.00	1.00	3,246
Age: 60+	0.17	0.37	0.00	0.00	0.00	0.00	1.00	3,246
State of Residence: Schleswig-Holstein	0.05	0.21	0.00	0.00	0.00	0.00	1.00	3,246
State of Residence: Hamburg	0.03	0.17	0.00	0.00	0.00	0.00	1.00	3,246
State of Residence: Lower Saxony	0.14	0.35	0.00	0.00	0.00	0.00	1.00	3,246
State of Residence: Bremen	0.01	0.08	0.00	0.00	0.00	0.00	1.00	3,246
State of Residence: North Rhine-Westphalia	0.29	0.45	0.00	0.00	1.00	0.00	1.00	3,246
State of Residence: Hesse	0.09	0.29	0.00	0.00	0.00	0.00	1.00	3,246
State of Residence: Rhineland-Palatinate	0.05	0.23	0.00	0.00	0.00	0.00	1.00	3,246
State of Residence: Baden-Wuerttemberg	0.15	0.35	0.00	0.00	0.00	0.00	1.00	3,246
State of Residence: Bavaria	0.19	0.40	0.00	0.00	0.00	0.00	1.00	3,246

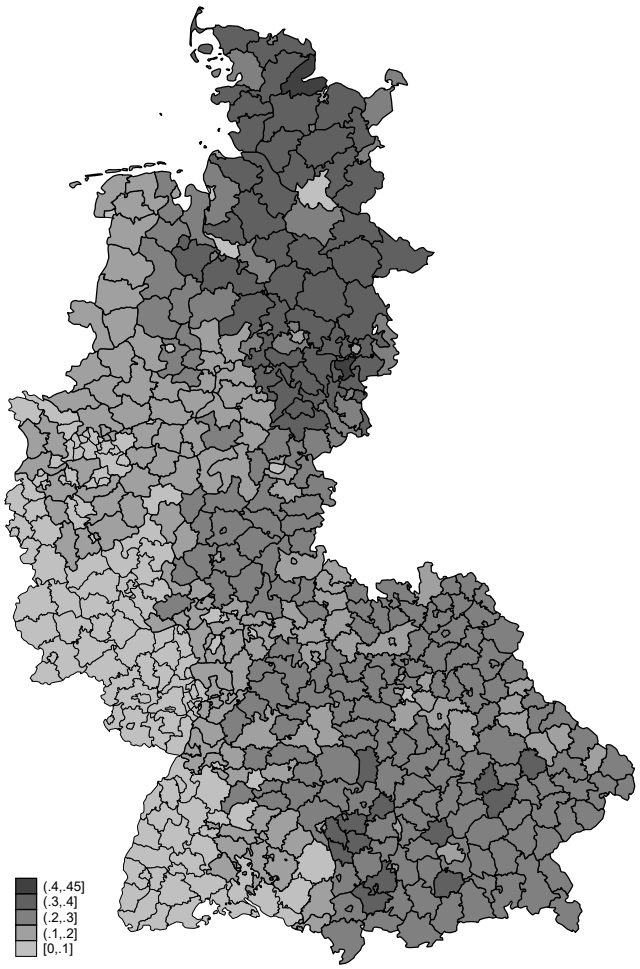
Notes: This table presents descriptive statistics on individual outcome and control variables from the Bundesstudie 1953.

B Additional Descriptive Figures

The Expellee Inflow across West Germany

Figure B.1 maps the county-level expellee share as of 1950 for all West German counties.

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



Notes: This map shows the county-level population share of expellees in West Germany in 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).

Figure B.2 displays the location of cities covered in the estimation sample.

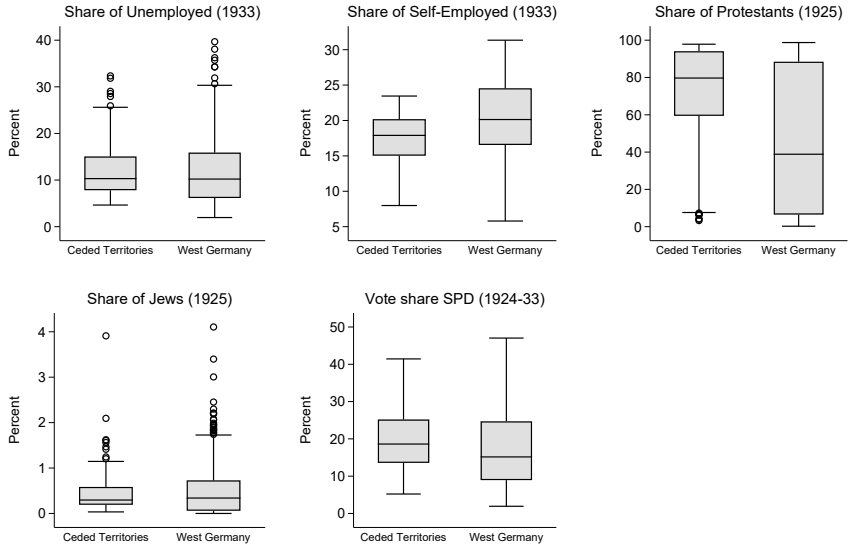
Figure B.2: Location of Cities in Estimation Sample

Notes: This map shows the location of each city covered by the "Statistical Yearbooks of German Municipalities" (see Panel (B) of Table A.1). The county boundaries correspond to those shown in Figure B.1. 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).

Differences between Expellees and Non-Expellees

Figure **B.3** compares characteristics of expellees and natives *before* World War II. To this end, we rely on county-level data for both the Eastern Territories and the western part of Germany.

Figure B.3: Differences Between Expellees and Non-Expellees – Pre-WWII Variables

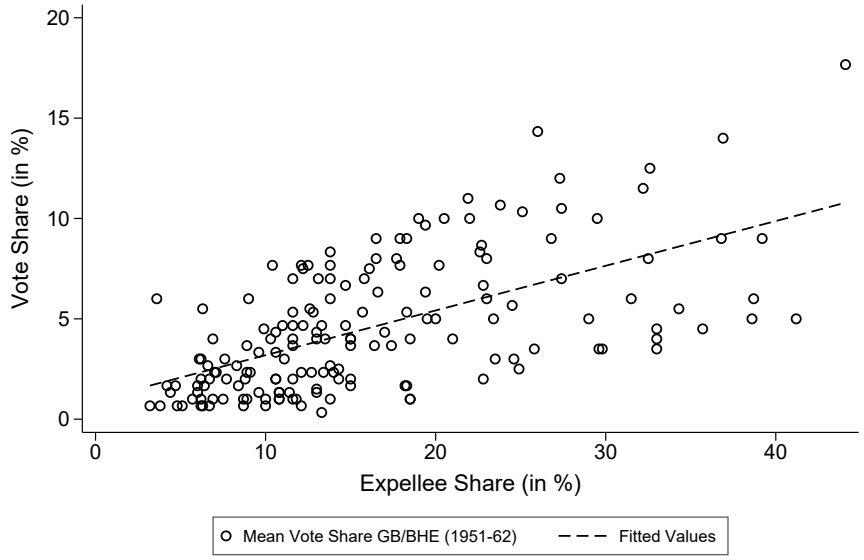


Notes: This graph compares counties from the eastern and western part of the German Reich before WW II in terms of observable characteristics. Data are taken from King et al. (2008). See Appendix Tables A.1 and A.2 for further information.

Expellee Inflow and Political Representation

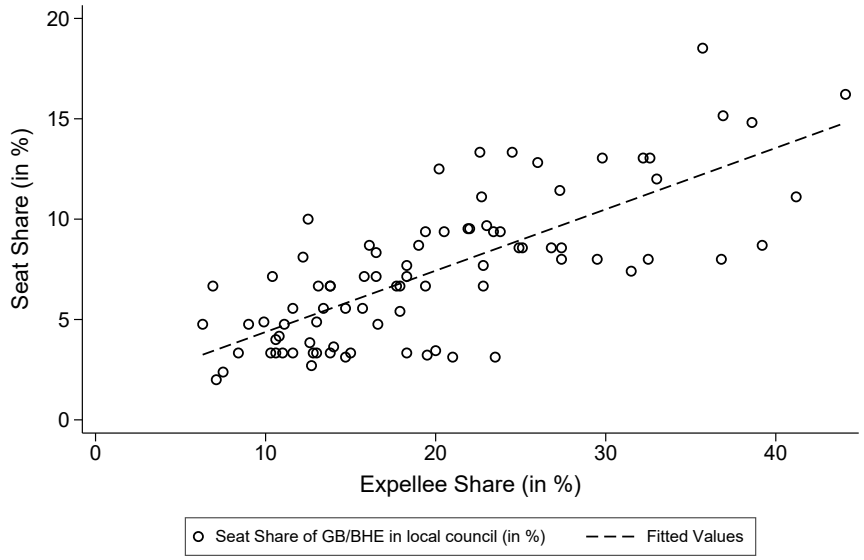
Figures **B.4** and **B.5** correlate local differences in the expellee inflow with measures of the expellees' political representation, the expellee party's (i) vote share and (ii) seat share in local parliaments.

Figure B.4: Population Shares of Expellees and GB/BHE Vote Shares



Notes: This graph displays the correlation between the population share of expellees in 1950 and the share of votes for the GB/BHE in local elections between 1950 and 1961. See Appendix Table A.1 for further information son the variables.

Figure B.5: Population Shares of Expellees and GB/BHE Seat Shares in Local Parliaments

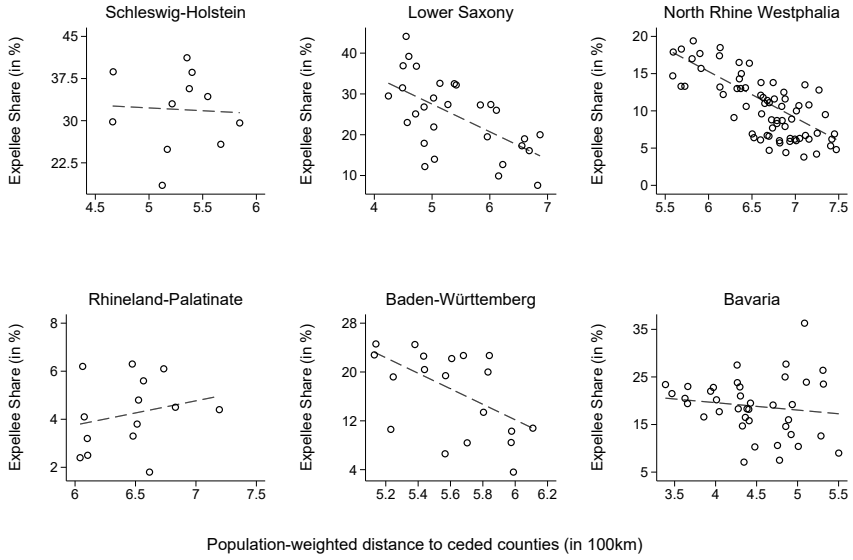


Notes: This graph displays the correlation between the population share of expellees in 1950 and the expellee party's seat share in local parliaments. See Appendix Table A.1 for further information on the variables.

IV: First-Stage Relationship

Figure **B.6** displays the first-stage relationship between the instrument and the county-level expellee share separately for each state covered in the estimation sample.

Figure B.6: IV: First-Stage Relationship by Federal State



Notes: This graph plots the simple relationship between the county-level expellee share and the instrument, a county's distance to the ceded territories in the east weighted by the territories respective population), for each of the six federal states covered in our data. See Figure 5 for the overall correlation between the two variables.

Figure **B.7** illustrates the first-stage correlation via maps.

Figure B.7: Regional Expellee Share and Predicted Inflow

Notes: Panel (A) shows the county-level share of expellees in our estimation sample, Panel (B) the corresponding residual expellee share net of our set of control variables. Panel (C) plots geographic distribution of our instrument, Panel (D) the respective residual instrument net of our control variables. 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).

C Additional Results and Robustness Checks

Assessing Sample Selection

Table **C.1** assesses whether the baseline estimation sample differs in the intensity of the expellee inflow from the counties not considered in the empirical analysis.

Table C.1: Testing for Selection in Estimation Sample

	Expellee Share				
	(1)	(2)	(3)	(4)	(5)
In Estimation Sample	-0.023**	-0.015***	0.006	-0.001	-0.008
	(0.010)	(0.005)	(0.008)	(0.005)	(0.005)
State FE		Yes		Yes	Yes
(Log) Population Density 1939			Yes	Yes	Yes
Observations	521	521	521	521	468

Notes: This table tests for selection into our estimation sample. The estimation sample comprises all cities that provide information on local spending and has non-missing ccovariates. In column (5), we exclude cities from the French Occupation Zone. Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Baseline Effects on Debt and Transfers

Table **C.2** presents our baseline results for various measures of local debt and transfers from higher levels of government.

Table C.2: Expellee Inflows and Local Debt/State Transfers - Average Effect post WW II

	Local Debt		State Transfers	
	Total	Per Capita	Total	Per Capita
	(1)	(2)	(3)	(4)
A. OLS Estimates				
Expellee Share	0.078	0.053	-0.069	-0.109
	(0.112)	(0.187)	(0.146)	(0.184)
B. 2SLS Estimates				
Expellee Share	0.881***	0.250	0.985***	0.251
	(0.319)	(0.340)	(0.354)	(0.399)
C. Reduced Form				
Population-Weighted Distance	-0.373***	-0.106	-0.362***	-0.092
	(0.110)	(0.140)	(0.096)	(0.144)
Controls	Yes	Yes	Yes	Yes
Number of Observations	217	217	233	233
Variable Mean				
Kleibergen-Paap *F*-Statistic	30.87	30.87	24.95	24.95

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on mean post-WW II (p.c.) debt and (p.c.) state transfers using simple OLS and the IV strategy laid out in Equations (1)-(4). Annual information on local debt and transfers is given for the period 1950-1959. 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 4.1 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

IV Estimation — Additional Results

In Tables **C.3-C.7**, we present IV estimates for public policy outcomes and voting with varying sets of controls. In each table, Column (5) reports the baseline coefficient as reported in the manuscript. In Column (1), we only control for occupation zone dummies. In Column (2), we additionally control for housing destruction, an important potential confounder. In Columns (3) and (4), we additionally control for a cities closeness to the inner-German border, as well as local economic and political differences before World War II.

Table C.3: The Effect of Expellee Inflows on Per Capita Spending - Average Effect post WW II

	Instrumental Variables				
	(1)	(2)	(3)	(4)	(5)
Panel A – Welfare					
Expellee Share	0.045	0.289**	0.230*	0.759***	0.709**
	(0.115)	(0.121)	(0.129)	(0.221)	(0.325)
Number of Observations	233	233	233	233	233
Kleibergen-Paap *F*-Statistic	130.16	120.89	85.53	53.06	24.95
Panel B – Education/Admin					
Expellee Share	-0.397***	-0.258*	-0.463***	-0.622***	0.301
	(0.118)	(0.134)	(0.143)	(0.200)	(0.335)
Number of Observations	233	233	233	233	233
Kleibergen-Paap *F*-Statistic	130.16	120.89	85.53	53.06	24.95
Panel C – Public Infrastructure					
Expellee Share	-0.493***	-0.503***	-0.671***	-1.080***	-0.476
	(0.130)	(0.155)	(0.170)	(0.278)	(0.370)
Number of Observations	233	233	233	233	233
Kleibergen-Paap *F*-Statistic	130.16	120.89	85.53	53.06	24.95
Panel D – Health/Housing					
Expellee Share	-0.067	-0.014	-0.093	-0.291	-0.135
	(0.123)	(0.148)	(0.153)	(0.288)	(0.364)
Number of Observations	233	233	233	233	233
Kleibergen-Paap *F*-Statistic	130.16	120.89	85.53	53.06	24.95
Occupational Zone FE	Yes	Yes	Yes	Yes	
Destroyed Housing		Yes	Yes	Yes	Yes
Dummy: Less than 75km to inner-German border			Yes	Yes	Yes
Pre-WWII Controls				Yes	Yes
Federal State FE					Yes

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on mean post-WW II per capital spending on different items using the IV strategy laid out in Equations (1)-(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 4.1 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 Expellee Inflows on Spending Shares - Average Effect post WW II

	Instrumental Variables				
	(1)	(2)	(3)	(4)	(5)
Panel A – Welfare					
Expellee Share	0.360***	0.557***	0.622***	1.424***	0.873**
	(0.133)	(0.152)	(0.185)	(0.314)	(0.412)
Number of Observations	233	233	233	233	233
Kleibergen-Paap *F*-Statistic	130.16	120.89	85.53	53.06	24.95
Panel B – Education/Admin					
Expellee Share	0.106	0.116	0.138	0.136	0.420
	(0.109)	(0.129)	(0.123)	(0.243)	(0.437)
Number of Observations	233	233	233	233	233
Kleibergen-Paap *F*-Statistic	130.16	120.89	85.53	53.06	24.95
Panel C – Public Infrastructure					
Expellee Share	-0.504***	-0.668***	-0.739***	-1.210***	-0.836**
	(0.105)	(0.127)	(0.154)	(0.257)	(0.404)
Number of Observations	233	233	233	233	233
Kleibergen-Paap *F*-Statistic	130.16	120.89	85.53	53.06	24.95
Panel D – Health/Housing					
Expellee Share	0.199*	0.222*	0.231*	0.112	-0.197
	(0.102)	(0.122)	(0.137)	(0.257)	(0.389)
Number of Observations	233	233	233	233	233
Kleibergen-Paap *F*-Statistic	130.16	120.89	85.53	53.06	24.95
Occupational Zone FE	Yes	Yes	Yes	Yes	
Destroyed Housing		Yes	Yes	Yes	Yes
Dummy: Less than 75km to inner-German border			Yes	Yes	Yes
Pre-WWII Controls				Yes	Yes
Federal State FE					Yes

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on mean post-WW II spending shares on different items using the IV strategy laid out in Equations (1)-(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 4.1 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 Expellee Inflows on Local Tax Rates - Average Effect post WW II

	Instrumental Variables				
	(1)	(2)	(3)	(4)	(5)
Panel A – Agricultural Land Tax					
Expellee Share	0.791***	0.956***	0.897***	0.924***	0.232
	(0.101)	(0.120)	(0.117)	(0.208)	(0.280)
Number of Observations	233	233	233	233	233
Kleibergen-Paap *F*-Statistic	130.16	120.89	85.53	53.06	24.95
Panel B – Residential Property Tax					
Expellee Share	0.556***	0.803***	0.781***	1.428***	0.823**
	(0.135)	(0.154)	(0.169)	(0.284)	(0.318)
Number of Observations	233	233	233	233	233
Kleibergen-Paap *F*-Statistic	130.16	120.89	85.53	53.06	24.95
Panel C – Business Capital/Profit Tax					
Expellee Share	0.537***	0.699***	0.766***	0.664***	0.892**
	(0.114)	(0.128)	(0.135)	(0.219)	(0.396)
Number of Observations	233	233	233	233	233
Kleibergen-Paap *F*-Statistic	130.16	120.89	85.53	53.06	24.95
Panel D – Business Wage Bill Tax					
Expellee Share	-0.447***	-0.449***	-0.384**	0.251	0.470
	(0.143)	(0.154)	(0.166)	(0.305)	(0.401)
Number of Observations	128	128	128	128	128
Kleibergen-Paap *F*-Statistic	139.08	152.52	102.11	36.98	35.43
Occupational Zone FE	Yes	Yes	Yes	Yes	
Destroyed Housing		Yes	Yes	Yes	Yes
Dummy: Less than 75km to inner-German border			Yes	Yes	Yes
Pre-WWII Controls				Yes	Yes
Federal State FE					Yes

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on mean post-WW II local tax rates using the IV strategy laid out in Equations (1)-(4). The varying 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 4.1 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 Expellee Inflows on Debt and Transfers - Average Effect post WW II

	Instrumental Variables				
	(1)	(2)	(3)	(4)	(5)
Panel A – (Log) Debt					
Expellee Share	0.009	0.265**	0.252**	0.516***	0.881***
	(0.108)	(0.105)	(0.117)	(0.169)	(0.319)
Number of Observations	217	217	217	217	217
Kleibergen-Paap *F*-Statistic	136.30	131.47	96.98	63.75	30.87
Panel B – P.C. Debt					
Expellee Share	0.012	0.094	0.151	0.006	0.250
	(0.104)	(0.122)	(0.150)	(0.182)	(0.340)
Number of Observations	217	217	217	217	217
Kleibergen-Paap *F*-Statistic	136.30	131.47	96.98	63.75	30.87
Panel C – (Log) State Transfers					
Expellee Share	-0.010	0.274**	0.171	0.796***	0.985***
	(0.128)	(0.139)	(0.158)	(0.210)	(0.354)
Number of Observations	233	233	233	233	233
Kleibergen-Paap *F*-Statistic	130.16	120.89	85.53	53.06	24.95
Panel D – P.C. State Transfers					
Expellee Share	0.169	0.403***	0.261	0.691***	0.251
	(0.136)	(0.147)	(0.158)	(0.266)	(0.399)
Number of Observations	233	233	233	233	233
Kleibergen-Paap *F*-Statistic	130.16	120.89	85.53	53.06	24.95
Occupational Zone FE	Yes	Yes	Yes	Yes	
Destroyed Housing		Yes	Yes	Yes	Yes
Dummy: Less than 75km to inner-German border			Yes	Yes	Yes
Pre-WWII Controls				Yes	Yes
Federal State FE					Yes

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on mean post-WW II (per capita) debt and (per capita) state transfers using the IV strategy laid out in Equations (1)-(4). The varying 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 4.1 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 Expellee Inflows on Voter Turnout/Vote Shares - Average Effect post WW II

	Instrumental Variables				
	(1)	(2)	(3)	(4)	(5)
Panel A – Voter Turnout					
Expellee Share	0.005	0.001	0.003	0.018	-0.009
	(0.006)	(0.007)	(0.007)	(0.011)	(0.019)
Number of Observations	217	217	217	217	217
Kleibergen-Paap *F*-Statistic	136.49	132.32	96.98	66.70	33.97
Panel B – Vote Share CDU/CSU/SPD					
Expellee Share	-0.037***	-0.038***	-0.035***	-0.038**	-0.048*
	(0.010)	(0.011)	(0.012)	(0.017)	(0.028)
Number of Observations	217	217	217	217	217
Kleibergen-Paap *F*-Statistic	136.49	132.32	96.98	66.70	33.97
Panel C – Vote Share GB/BHE					
Expellee Share	0.031***	0.034***	0.027***	0.029***	0.025**
	(0.004)	(0.005)	(0.005)	(0.007)	(0.010)
Number of Observations	217	217	217	217	217
Kleibergen-Paap *F*-Statistic	136.49	132.32	96.98	66.70	33.97
Panel D – Vote Share Other Parties					
Expellee Share	0.020**	0.019*	0.021*	0.023	0.037
	(0.009)	(0.011)	(0.012)	(0.016)	(0.027)
Number of Observations	217	217	217	217	217
Kleibergen-Paap *F*-Statistic	136.49	132.32	96.98	66.70	33.97
Panel E – GB/BHE in Local Council					
Expellee Share	0.366***	0.417***	0.402***	0.396***	0.206
	(0.047)	(0.060)	(0.069)	(0.095)	(0.148)
Number of Observations	199	199	199	199	199
Kleibergen-Paap *F*-Statistic	134.14	126.40	96.64	64.43	32.25
Panel F – Seat Share GB/BHE					
Expellee Share	0.038***	0.042***	0.037***	0.036***	0.026**
	(0.004)	(0.005)	(0.005)	(0.008)	(0.013)
Number of Observations	199	199	199	199	199
Kleibergen-Paap *F*-Statistic	134.14	126.40	96.64	64.43	32.25
Occupational Zone FE	Yes	Yes	Yes	Yes	
Destroyed Housing		Yes	Yes	Yes	Yes
Dummy: Less than 75km to inner-German border			Yes	Yes	Yes
Pre-WWII Controls				Yes	Yes
Federal State FE					Yes

Notes: This table shows the effect of a one standard deviation increase in the expelleee share on mean post-WW II voter turnout, party vote shares and the presence of the GB/BHE in local councils using the IV strategy laid out in Equations (1)-(4). The varying 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 4.1 for details). Standard errors are clustered at the county level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Using the Expellee Share in 1952

In Table **C.8**, we replace the regressor of interest — the share of expellees in the population of a *county* in 1950 — with the share of expellees in the population of a *municipality* in 1952.

Table C.8: Robustness Check - Muncipality-Level Expellee Share as of 1952

	A. Local Tax Rates on			
	Agricultural Land	Residential Property	Business Capital	Business Wage Bill
Expellee Share 1952	0.370	1.179**	1.219*	0.561
	(0.387)	(0.511)	(0.647)	(0.435)
Number of Observations	229	229	229	126
Kleibergen-Paap *F*-Statistic	8.71	8.71	8.71	21.37
	B. Per Capita Spending on			
	Welfare	Education/Admin	Publ. Infrastructure	Health/Housing
Expellee Share 1952	0.965*	0.380	-0.638	-0.215
	(0.533)	(0.479)	(0.562)	(0.531)
Number of Observations	229	229	229	229
Kleibergen-Paap *F*-Statistic	8.07	8.07	8.07	8.07
	C. Spending Share on			
	Welfare	Education/Admin	Publ. Infrastructure	Health/Housing
Expellee Share 1952	1.174*	0.503	-1.037*	-0.304
	(0.676)	(0.633)	(0.608)	(0.568)
Number of Observations	229	229	229	229
Kleibergen-Paap *F*-Statistic	8.07	8.07	8.07	8.07
	D. Local Debt and State Transfers			
	Log Debt	P.C. Debt	Log Transfers	P.C. Transfers
Expellee Share 1952	1.121**	0.310	1.343**	0.294
	(0.496)	(0.461)	(0.618)	(0.566)
Number of Observations	213	213	229	229
Kleibergen-Paap *F*-Statistic	13.05	13.05	8.07	8.07
	E. Voter Turnout and Vote Shares			
		Vote Share		
	Turnout	CDU/SPD	GB/BGHE	Others
Expellee Share 1952	-0.011	-0.057	0.030**	0.044
	(0.024)	(0.037)	(0.012)	(0.036)
Number of Observations	213	213	213	213
Kleibergen-Paap *F*-Statistic	14.99	14.99	14.99	14.99
Controls	Yes	Yes	Yes	Yes

Notes: This table shows the effect of a one standard deviation increase in the municipality-level expelleee share as of 1952 on mean post-WW II public finance outcomes using the IV strategy laid out in Equations (1)-(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 4.1 for details). Standard errors are clustered at the municipality level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Excluding Cities Close to the Border

In **Table C.9** we present IV results from our most comprehensive specification when discarding cities within 75km from the border.

Table C.9: Robustness Check - Muncipality-Level Expellee Share as of 1952

	A. Local Tax Rates on			
	Agricultural Land	Residential Property	Business Capital	Business Wage Bill
Expellee Share 1950	-0.101	0.730**	1.063***	0.626
	(0.243)	(0.307)	(0.402)	(0.404)
Number of Observations	211	211	211	121
Kleibergen-Paap *F*-Statistic	25.27	25.27	25.27	48.58
	B. Per Capita Spending on			
	Welfare	Education/Admin	Publ. Infrastructure	Health/Housing
Expellee Share 1950	0.494	0.153	-0.526	-0.332
	(0.339)	(0.311)	(0.333)	(0.354)
Number of Observations	211	211	211	211
Kleibergen-Paap *F*-Statistic	22.46	22.46	22.46	22.46
	C. Spending Share on			
	Welfare	Education/Admin	Publ. Infrastructure	Health/Housing
Expellee Share 1950	0.755*	0.529	-0.749*	-0.330
	(0.423)	(0.420)	(0.403)	(0.390)
Number of Observations	211	211	211	211
Kleibergen-Paap *F*-Statistic	22.46	22.46	22.46	22.46
	D. Local Debt and State Transfers			
	Log Debt	P.C. Debt	Log Transfers	P.C. Transfers
Expellee Share 1950	0.801**	0.155	0.817**	0.063
	(0.341)	(0.374)	(0.343)	(0.390)
Number of Observations	197	197	211	211
Kleibergen-Paap *F*-Statistic	25.94	25.94	25.27	25.27
	E. Voter Turnout and Vote Shares			
		Vote Share		
	Turnout	CDU/SPD	GB/BGHE	Others
Expellee Share 1950	0.012	-0.057*	0.028**	0.044
	(0.019)	(0.031)	(0.012)	(0.030)
Number of Observations	197	197	197	197
Kleibergen-Paap *F*-Statistic	27.51	27.51	27.51	27.51
Controls	Yes	Yes	Yes	Yes

Notes: This table shows the effect of a one standard deviation increase in the municipality-level expelleee share as of 1952 on mean post-WW II public finance outcomes using the IV strategy laid out in Equations (1)-(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 4.1 for details). Standard errors are clustered at the municipality level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Inference

In Table **C.10**, we address two major challenges for our inference. One is multiple hypothesis testing, the problem that multiple tests of hypotheses about correlated outcomes may lead to over-rejection of the null hypothesis. The second challenge is spatial autocorrelation in the error terms, which may also lead to over-rejection of the null hypothesis. To address both challenges, we focus on the reduced-form rather than the 2SLS estimates because some of the adjustment procedures are only available for OLS regressions. In each panel of **Table C.10**, we present the reduced-form estimates from the main tables along with the p-values from two-sided zero mean hypothesis tests, whereby the standard errors are adjusted based on different procedures. The p-values in the first row correspond to standard errors clustered at the county level, our baseline specification.

Multiple Hypothesis Testing One challenge with our inference is that we consider the effect of the expellee inflows on many outcomes at a time. Because some of the outcomes may be correlated, hypothesis tests based on conventional or cluster-robust standard errors would lead to an over-rejection of the null hypothesis of no effect. To account for this problem, we adjust the standard errors based on the step-down procedure proposed by Romano and Wolf (2005). This procedure adjusts the p-values for over-rejection within families of outcomes, whereby a family is constituted by outcomes that may be strongly correlated. In our case, we consider four different families, namely tax rates, spending, debt and transfers, and voting outcomes.

In general, our inference is robust to the correction. For most outcomes, the adjusted p-values are slightly larger than the cluster-robust standard errors reported in the main text, but results that are statistically significant in the main text are also significant when we adjust for the family-wise error rate using the Romano and Wolf (2005) procedure.

Adjusting for Spatial Autocorrelation A further challenge for inference is spatial autocorrelation of the error terms. A recent paper by Kelly (2019) shows that, if unaccounted, spatial autocorrelation can lead to spurious regression results. Clustering of the standard errors may not solve this problem due to the underlying assumption that the error terms are only correlated within but not across clusters. To circumvent this problem, we adjust the standard errors for spatial autocorrelation using the variance-covariance matrix adjustment by Conley (1999) and Stata code from Hsiang (2010) with two different cut-offs, 50km and 100km. This correction allows for a correlation in the error terms with a decay — error terms of observations that are closer together are assumed to have a stronger correlation — up until the cut-off. Observations that are further apart than the cut-off are assumed to have no correlation in their error terms.

In each panel of **Table C.10**, the third and fourth row show the p-values of two-sided zero mean hypothesis tests with Conley standard errors. In most cases the p-values are *smaller* than those based on cluster-robust standard errors. While this does not prove that the standard erros in our main analysis are the correct ones, it suggests that we are being conservative by clustering at the county level.

Table C.10: Robustness Checks - Inference

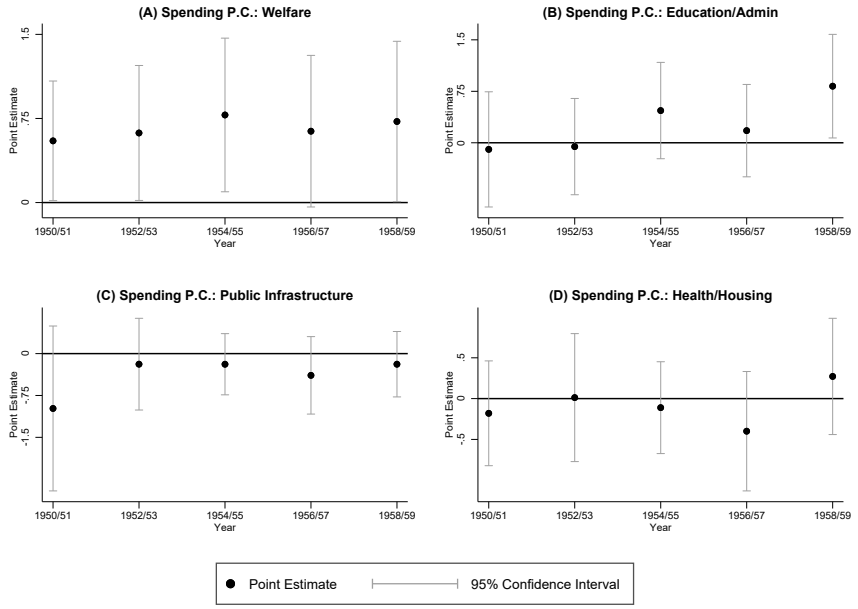
A. Tax Rate on	Agricultural Land (1)	Residential Property (2)	Business Capital (3)	Business Wage Bill (4)
Baseline Estimate	-0.090	-0.319	-0.345	-0.188
P-Value:				
Cluster at the County Level	[0.409]	[0.002]	[0.010]	[0.251]
Romano-Wolf Multiple Hypothesis Correction	[0.309]	[0.007]	[0.012]	[0.307]
Conley (2008) Spatial HAC Standard Erros (Distance Cutoff: 50km)	[0.432]	[0.006]	[0.004]	[0.274]
Conley (2008) Spatial HAC Standard Erros (Distance Cutoff: 100km)	[0.263]	[0.015]	[0.002]	[0.219]
B. P.C. Spending on	Public Welfare (1)	Education/ Admin (2)	Public Infra. (3)	Health/ Housing (4)
Baseline Estimate	-0.261	-0.111	0.175	0.050
P-Value:				
Cluster at the County Level	[0.009]	[0.353]	[0.194]	[0.714]
Romano-Wolf Multiple Hypothesis Correction	[0.010]	[0.302]	[0.673]	[0.436]
Conley (2008) Spatial HAC Standard Erros (Distance Cutoff: 50km)	[0.006]	[0.164]	[0.690]	[0.258]
Conley (2008) Spatial HAC Standard Erros (Distance Cutoff: 100km)	[0.037]	[0.176]	[0.618]	[0.354]
C. Spending Share on	Public Welfare (1)	Education/ /Admin (2)	Public Infra. (3)	Health/ Housing (4)
Baseline Estimate	-0.321	-0.155	0.307	0.073
P-Value:				
Cluster at the County Level	[0.015]	[0.337]	[0.020]	[0.615]
Romano-Wolf Multiple Hypothesis Correction	[0.012]	[0.017]	[0.564]	[0.429]
Conley (2008) Spatial HAC Standard Erros (Distance Cutoff: 50km)	[0.009]	[0.006]	[0.599]	[0.214]
Conley (2008) Spatial HAC Standard Erros (Distance Cutoff: 100km)	[0.003]	[0.000]	[0.424]	[0.151]
D. Local Debt and State Transfers	Log Debt (1)	P.C. Debt (2)	Log Transfers (3)	P.C. Transfers (4)
Baseline Estimate	-0.373	-0.106	-0.092	-0.362
P-Value:				
Cluster at the County Level	[0.001]	[0.450]	[0.521]	[0.000]
Romano-Wolf Multiple Hypothesis Correction	[0.002]	[0.504]	[0.002]	[0.504]
Conley (2008) Spatial HAC Standard Erros (Distance Cutoff: 50km)	[0.000]	[0.295]	[0.373]	[0.000]
Conley (2008) Spatial HAC Standard Erros (Distance Cutoff: 100km)	[0.000]	[0.107]	[0.260]	[0.002]
E. Voter Turnout and Vote Shares	Voter Turnout (1)	Vote Share: CDU/SPD (2)	Vote Share: GB/BHE (3)	Vote Share: Other Parties (4)
Baseline Estimate	0.004	0.021	-0.011	-0.016
P-Value:				
Cluster at the County Level	[0.646]	[0.090]	[0.018]	[0.173]
Romano-Wolf Multiple Hypothesis Correction	[0.606]	[0.092]	[0.027]	[0.170]
Conley (2008) Spatial HAC Standard Erros (Distance Cutoff: 50km)	[0.650]	[0.035]	[0.001]	[0.121]
Conley (2008) Spatial HAC Standard Erros (Distance Cutoff: 100km)	[0.602]	[0.051]	[0.000]	[0.130]
F. GB/BHE Representation in Local Parliaments	In Parliament (1)	Seat Share (2)		
Baseline Estimate	-0.090	-0.011		
P-Value:				
Cluster at the County Level	[0.158]	[0.045]		
Romano-Wolf Multiple Hypothesis Correction	[0.072]	[0.022]		
Conley (2008) Spatial HAC Standard Erros (Distance Cutoff: 50km)	[0.033]	[0.016]		
Conley (2008) Spatial HAC Standard Erros (Distance Cutoff: 100km)	[0.004]	[0.011]		

Notes: This table presents robustness checks on inference four our baseline reduced-form estimates. First, we reproduce our baseline point estimates along with the corresponding p-values when clustering standard errors at the county level. Next, we report the respective Romano-Wolf step-down p-values for multiple hypothesis testing (Romano and Wolf (2005)). Multiple hypothesis testing is carried out for each panel (Panels A-F) separately and based on 400 replications of the bootstrap. Last, we provide p-values when accouting for spatial autocorrelation in spirit of Conley (1999). We derive the corresponding p-values using Stata code provided by Hsiang (2010) and choose two different cut-offs limiting potential spatial autocorrelation.

Dynamic Effects

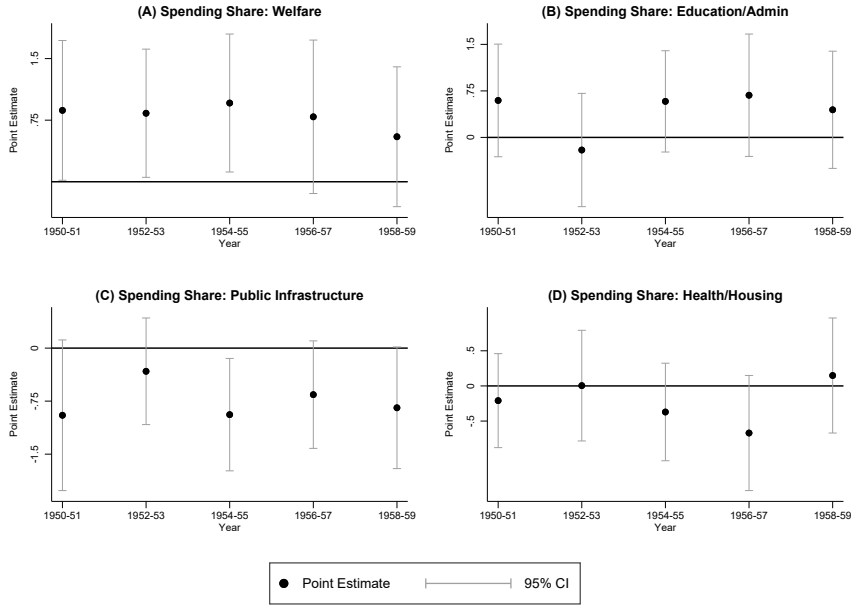
In Figures **C.1-C.4**, we present separate 2SLS estimates for two-year averages of the respective outcomes. Each coefficient displays the point estimate and corresponding 95% confidence interval. Specifications include the full set of covariates as introduced in 4.

Figure C.1: Expellee Inflows and Local P.C. Spending - Annual Estimates



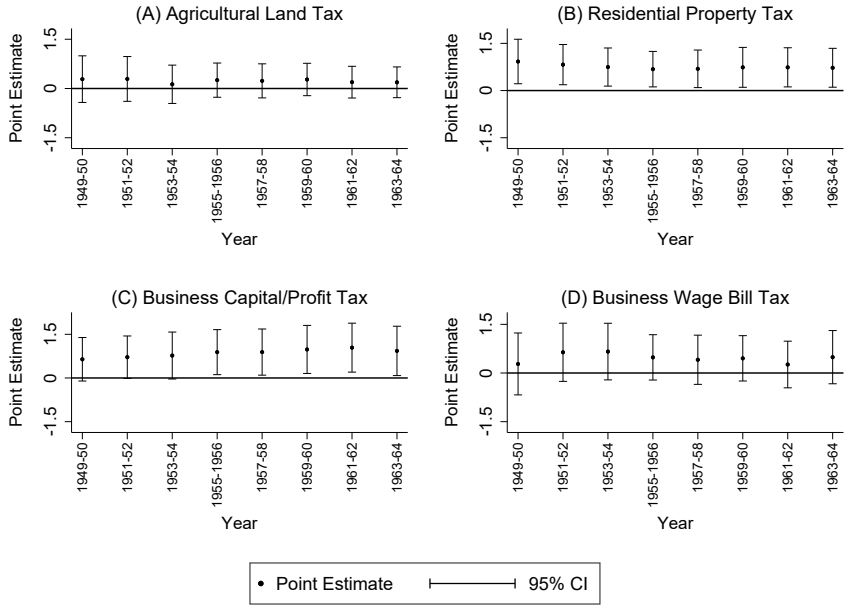
Notes: This graph shows the effect of a one-standard-deviation increase in the expellle share on p.c. spending on different items over the 1950-1959 period using the IV strategy laid out in Equations (1)-(4). The set of controls includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing space after the war (see Section 4.1 for details).

Figure C.2: Expellee Inflows and Local Spending Shares - Annual Estimates



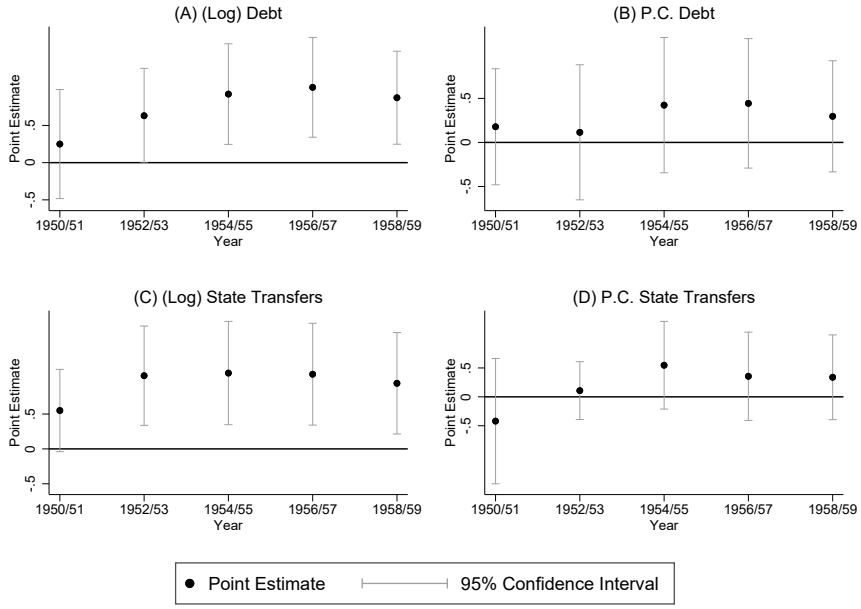
Notes: This graph shows the effect of a one-standard-deviation increase in the expellle share on spending shares on different items over the 1950-1959 period using the IV strategy laid out in Equations (1)-(4). The set of controls includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing space after the war (see Section 4.1 for details).

Figure C.3: Expellee Inflows and Local Tax Rates - Annual Estimates



Notes: This graph shows the effect of a one-standard-deviation increase in the expellle share on different local tax rates over the 1945-1964 period using the IV strategy laid out in Equations (1)-(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 space after the war (see Section 4.1 for details).

Figure C.4: Expellee Inflows and Local Debt and State Transfers - Annual Estimates



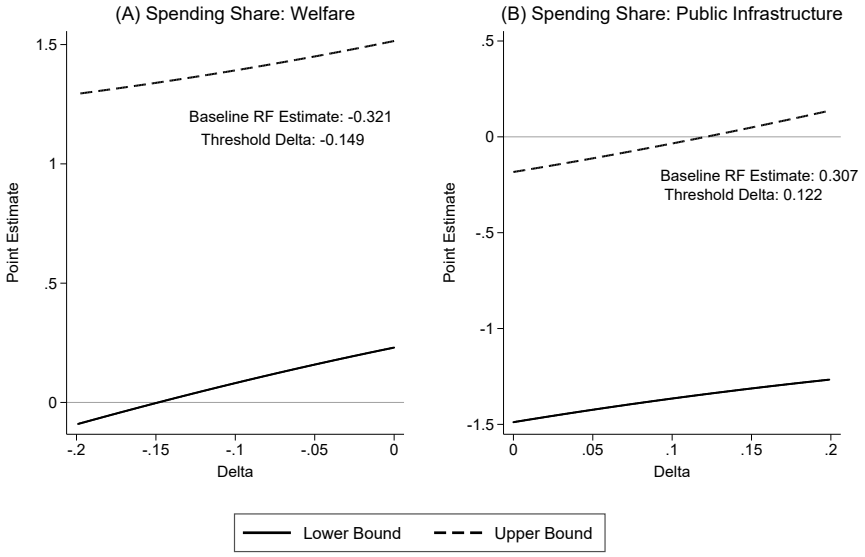
Notes: This graph shows the effect of a one-standard-deviation increase in the expellle share on local debt and state transfers over the 1950-1959 period using the IV strategy laid out in Equations (1)-(4). The set of controls includes measures of institutional differences, pre-WW II controls to capture persistent differences across regions, and the share of destroyed housing space after the war (see Section 4.1 for details).

Relaxing Instrument Exogeneity (Conley et al., 2012)

To assess the sensitivity of our IV estimates with regard to modest violations of the exclusion restriction, we make use of the generalized IV approach by Conley et al. (2012). The idea behind this approach is that the instrument may not be strictly exogenous, such that its direct effect, δ, is different from zero. However, if the violation is not severe, that is, $\delta \neq 0$ but small, the causal inference may be robust to this violation. In Figures C.5 - C.8, we plot the upper and lower bound of the 2SLS estimates with hypothetical direct effects that are uniformly distributed over the the interval $[0, \delta]$. Each figure presents the bounds for 90% confidence bands and varying sizes of δ. [16] This allows us to identify the threshold value at which the second-stage coefficient for the outcome variable becomes insignificant at the 10% level.

To gauge the relative magnitude of this threshold value, we compare it to the *reduced-form* effect of the instrument on the outcome variable. The ratio of the threshold value and the reduced- form coefficient allows us to assess whether a possible direct effect of the instrument may plausibly explain away the second-stage IV coefficient. For example, the direct effect of the instrument on the residential property tax rate would have to be about 60% (0.179/0.303) of the overall effect to render the second-stage IV coefficient insignificant. In light of our identification strategy, we consider this effect to be implausibly large.

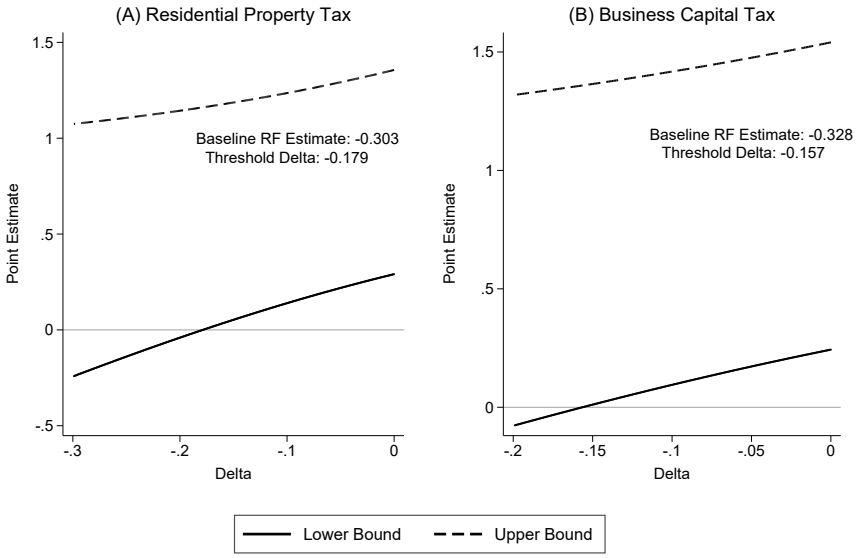
Figure C.5: Relaxing Instrument Exogeneity - Local Spending Shares



Notes: This graph shows the upper and lower bound of the 90% confidence interval of the IV coefficient on (a) the share of welfare spending and (b) the share of public infrastructure spending as given in our baseline specification (see Column (5) of Table 2) when allowing for a varying direct effect of the instrument on the respective outcome (Conley et al., 2012). We assume δ, the hypothetical direct effect of the instrument, to be uniformly distributed over the interval $[0,-\delta]/[0,\delta]$. The assumed size of the direct effect is plotted on the x-axis. The given threshold refers to the size of δ at which the respective second-stage coefficient becomes insignificant at the 10% level.

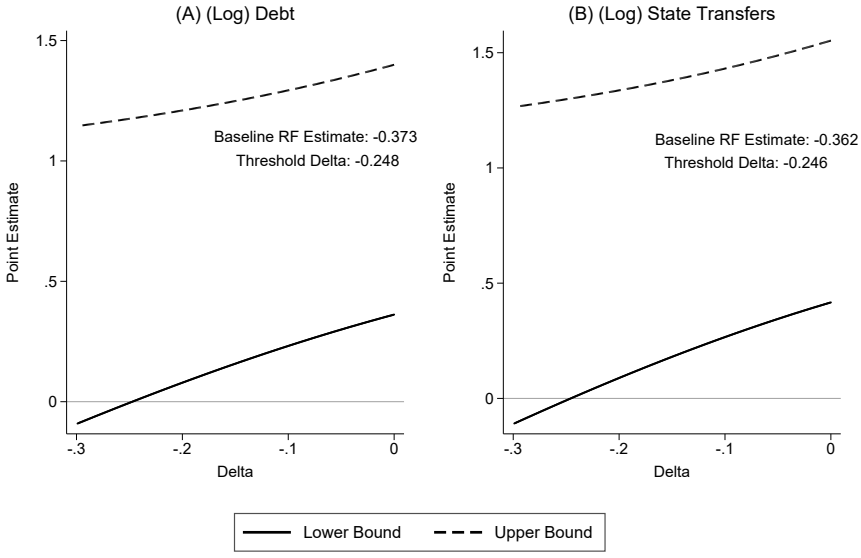
[16] We choose between positive and negative direct effects such that a δ that is larger in absolute value yields point estimates that are closer to zero.

Figure C.6: Relaxing Instrument Exogeneity - Local Tax Rates



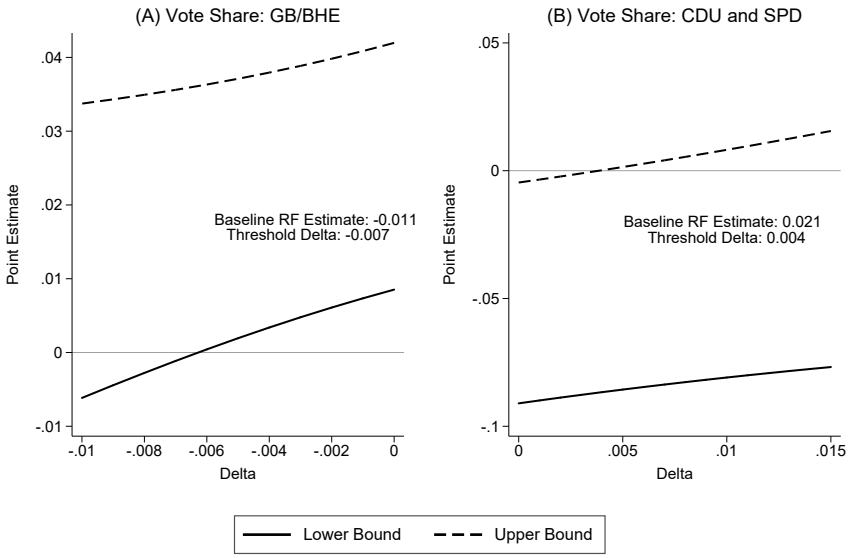
Notes: This graph shows the upper and lower bound of the 90% confidence interval of the IV coefficient on (a) the residential land tax and (b) the business capital tax as given in our baseline specification (see Column (5) of Table 3) when allowing for a varying direct effect of the instrument on the respective outcome (Conley et al., 2012). We assume δ, the hypothetical direct effect of the instrument, to be uniformly distributed over the interval $[0,-\delta]$. The assumed size of the direct effect is plotted on the x-axis. The given threshold refers to the size of δ at which the respective second-stage coefficient becomes insignificant at the 10% level.

Figure C.7: Relaxing Instrument Exogeneity - Local Debt and State Transfers



Notes: This graph shows the upper and lower bound of the 90% confidence interval of the IV coefficient on (a) the (log) local debt and (b) (log) state transfers (see Column (5) of Table C.2) when allowing for a varying direct effect of the instrument on the respective outcome (Conley et al., 2012). We assume δ, the hypothetical direct effect of the instrument, to be uniformly distributed over the interval $[0,-\delta]$. The assumed size of the direct effect is plotted on the x-axis. The given threshold refers to the size of δ at which the respective second-stage coefficient becomes insignificant at the 10% level.

Figure C.8: Relaxing Instrument Exogeneity - Vote Shares



Notes: This graph shows the upper and lower bound of the 90% confidence interval of the IV coefficient on (a) the local GB/BHE vote share and (b) the combined CDU/CSU and SPD vote share (see Column (5) of Table 5) when allowing for a varying direct effect of the instrument on the respective outcome (Conley et al., 2012). We assume δ, the hypothetical direct effect of the instrument, to be uniformly distributed over the interval $[0,-\delta]/[0,\delta]$. The assumed size of the direct effect is plotted on the x-axis. The given threshold refers to the size of δ at which the respective second-stage coefficient becomes insignificant at the 10% level.