Refugee Labor Market Access Increases Support for Migration [*]

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Abstract

Does the economic integration of refugees affect public attitudes toward migration? We assess this pertinent question by making use of a recent policy change in Germany, where the government significantly eased labor market access for refugees in 85% of its employment districts. Using administrative employment data spanning ten years, we show that the policy increased refugee employment by 50%. The policy also had a *positive* effect on natives' attitudes toward migration. Voters exposed to more refugees in the labor market were two percentage points more likely to vote for pro-migration parties across both state and federal elections. Using panel evidence, we corroborate that the policy led to more favorable attitudes toward migration among natives. Turning to mechanisms, we find that increasing refugee labor market access had no effect on natives' economic situation, but arguably facilitated positive native-refugee interactions in the workplace.

1 Introduction

Public support for migration varies greatly across industrialized democracies. What explains this variance? This study hones in on the economic integration of refugees as a crucial determinant of public opinion toward migration. On the one hand, integrating refugees in the economy may spark labor market competition, which may lead natives to oppose migration. On the other hand, the economic integration of refugees provides firms with much-needed human capital and thus benefits society at large. What is more, the integration of refugees in the labor market means that migrants and natives work side by side, which may lead natives to espouse a more welcoming attitude toward migrants. Taken together, the effect of refugee labor market integration on public support for migration is thus unclear.

Besides unclear theoretical predictions, the link from refugee labor market access to public opinion toward migration is also empirically vexed. First, the influx and subsequent location of migrants is not random. Far from it, migrants typically choose areas with better economic conditions and a more welcoming host population (Scott and Brindley 2012). Second, countries typically choose the degree of labor market access centrally. There is thus no subnational variation one could exploit. Third, the influx of migrants into any given local labor market is well-regulated and tends to be rather low. Tracing the effect of refugee labor market access on public opinion is thus difficult, given the rather mild increase the overall population is exposed to.

The present study circumvents these challenges. We make use of a new law in Germany, where the government recently eased labor market access for refugees in 85% of its employment districts. Until 2016, refugees in Germany were only allowed to work if no native was available for the job. The policy was highly bureaucratic, and effectively prevented refugees from joining the workforce while their asylum application was pending. In August 2016, 85 percent of local employment districts abolished the policy, opening the labor market for all

refugees. Importantly, there was pronounced regional variation, which allows us to delineate the effect of open labor markets on public support for migration. What is more, the unprecedented influx of refugees in the previous year meant that the policy had a real impact on local labor markets. Finally, refugees in Germany are generally not allowed to relocate freely, alleviating concerns about sorting due to improved labor market access in some areas.

Using a series of difference-in-differences models, we first confirm that the labor market liberalization had a pronounced positive effect on refugee employment. Based on administrative time series data spanning ten years, we show that treatment and control municipalities had similar trends for eight years prior to the policy change. Once the policy was implemented, treated municipalities (i.e., those granting refugees access to the labor market) saw 0.45 more refugees in the labor market per 1,000 natives compared to the control group. The change translates into an increase in refugee employment of 50 percent, which underlines the policy's pronounced impact on local communities.

In a second step, we explore whether the policy changed natives' views about migration. Using administrative data on state and federal elections as well as panel survey evidence, we find that the policy had a *positive* effect on attitudes toward migration. Voters in treated areas were 2 percentage points less likely to vote for right-wing parties in both state and federal elections. At the same time, left-leaning, pro-immigration parties gained significantly. To ensure that the electoral changes are due to changed preferences, we use panel evidence. Comparing the same individuals before and after the policy took place across treatment and control areas, we are able to show that the economic integration of refugees decreased anti-immigrant sentiment among voters.

Why did the policy lead natives to adopt more favorable views on migration? We assess two broad families of mechanisms—one economic and one social. We first show that the integration of refugees in the labor market had *no* effect on natives' economic situation. Neither did the policy affect natives' wages, nor did it affect natives' employment. Any

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detrimental economic effects—e.g., those predicted by the influential labor market compe-
tition hypothesis—are thus mute in our setting. We then turn to the social effects of the
policy. While we lack individual-level evidence, we present several pieces of evidence that the
positive effect is likely due to interactions of natives and refugees at the workplace. Notably,
we show that the positive effect on natives attitudes toward migration is only present among
employed individuals and is absent among unemployed natives. Overall, the findings thus
support sociotropic accounts of public opinion formation toward migration. Rather than
viewing migrants as a threat, citizens react positively to refugees in the labor market.

2 Theoretical background

How does the economic integration of refugees affect natives' attitudes toward migration?
The existing literature broadly delineates four causal channels that help explain how an
influx of refugees into local labor markets can affect public attitudes toward migration. The
arguments can be grouped into a social as well as an economic channel. We discuss both
channels in turn and then revisit them in the empirical section.

2.1 Economic channels

The first economic channel linking the labor market integration of refugees to natives' atti-
tudes toward migration functions via **labor market competition**. Early research in the
social sciences was dominated by the factor proportion model, which predicts that an influx
of immigrants reduces employment and wages among natives. Such detrimental economic
effects, in turn, may then generate opposition toward migration. Based on the factor propor-
tion model, a variety of studies have hypothesized that anticipated labor market competition
drives anti-immigration attitudes among natives (Dancygier and Donnelly 2013; Finseraas,
Røed and Schøne 2017; Pardos-Prado and Xena 2019; Scheve and Slaughter 2001; Mayda

2006; Hainmueller and Hiscox 2010).

A second economic channel linking the labor market integration of refugees to natives' attitudes toward migration functions via the host population's expectation about immigrants' **welfare dependence**. Specifically, the economic integration of migrants may have a positive effect on natives' attitudes because natives reward immigrants for working (Hager and Veit 2019). If refugees are integrated into the labor market, they contribute to the economy and are less dependent on government support. As a result, natives may begin to view migration more positively if exposed to a greater share of refugees in the labor market. The channel is thus a theoretical cousin of the so-called "fiscal burden" model and rests on natives' prior beliefs about immigrants' intention to work (Razin and Wahba 2015).

2.2   Social channels

A first social channel that links the labor market integration of refugees to natives' attitudes toward migration is **intergroup contact**. Refugees have been shown to settle in neighborhoods where other migrants with similar cultural backgrounds have already settled (Korinek, Entwisle and Jampaklay 2005). As a result, contact between refugees and natives is less pronounced than contact among natives. The economic integration of refugees can help mitigate this. If refugees join a local firm, contact with natives is facilitated. And such contact may help overcome deep-seated stereotypes (Paluck, Green and Green 2019). The positive effects are likely more pronounced if low-skilled natives—who tend to hold more xenophobic views—are put in touch with new migrants.

A second, rivaling social channel linking the economic integration of refugees to natives' attitudes toward migration is cultural backlash. Intergroup contact need not be positive. Some authors, for instance, have found that refugee settlement benefits right-leaning parties (Dustmann, Vasiljeva and Piil Damm 2019; Hangartner et al. 2019). Negative experiences could well take place in the workplace where new arrivals have to come to terms with

established work routines. Generally speaking, diversity has been found to be a positive predictor of workplace performance (Bunderson and Sutcliffe 2002). Yet, previous studies have not looked into the economic integration of refugees, which often includes individuals who do not speak the host population's language. It may thus be the case that contact in the workplace does not meaningfully build rapport between natives and refugees, leading to an increase in xenophobic attitudes among natives.

3   Setting

Beginning in 2015, Germany experienced an unprecedented inflow of refugees. In 2015 alone, approximately 1 million refugees arrived in the country (BAMF 2015). Today, refugees constitute more than two percent of the country's population. Migration policy was one of the most salient political issues during our study period. In a representative survey fielded a couple of days before the 2017 federal election, more than 80% of respondents indicated that refugee policy was an 'important' or 'very important' determinant of their vote choice (Forschungsgruppe Wahlen 2017).

To facilitate the integration of refugees into German society, the government passed the wide-ranging *Integrationsgesetz* ('integration law') in August 2016. Aimed at integrating the unprecedented number of refugees, the law regulated the issuance of residency permits, refugees' domestic freedom of movement as well as labor market access. Most importantly, the new law simplified labor market access by suspending the so-called priority review (*Vorrangprüfung*) for refugees for a period of three years. The policy change constituted a significant labor market liberalization for refugees.[1]

Prior to the priority review suspension, refugees faced significant hurdles in the labor market. Refugees were not allowed to seek employment during the first three months after

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[1]We emphasize that the policy we study is the only component of the new law that was explicitly aimed at improving the immediate labor market opportunities of refugees.

their arrival. Starting in month four after arrival, refugees became, on paper, eligible for employment. In reality, however, refugee employment remained tightly regulated. Employers that wished to hire a refugee whose asylum application was not yet approved for an open position had to obtain written approval from the Federal Employment Agency. The approval of the agency was subject to the so-called priority review: for a period of up to six weeks, the local employment agency office needed to try to find an unemployed German or foreign permanent resident who fit the job description. These candidates would then be required to apply for the open position in order to remain eligible for unemployment benefits. This policy also applied in cases where no refugees had applied for a given position. Only if no other candidate could be hired within the six-week period, the refugee could take up the position. After the suspension of the priority review, employers were no longer forced to consider alternative employees. This change removed a major disincentive for employers to hire refugees and was designed to facilitate refugees' integration into the German labor market.[2]

The priority review was suspended in 133 out of 156 employment agency districts in August 2016. We will exploit this spatial and temporal variation in the implementation of the policy to analyze its economic and political effects. The Federal Employment Agency divides Germany into 156 agency districts, which form the agency's main organizational unit. Each agency district typically consists of 2-4 adjacent counties. Crucially, employment agency districts are of limited political and administrative importance in other domains apart from the labor market. Policy changes are generally not implemented at this level. The aggregation of votes for seats in federal or state legislatures likewise occurs at a lower geographic level.

Figure 1 shows a map of treated and non-treated counties. While the integration law was passed by the federal government, state governments had some influence on the implemen-

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[2]The priority review was automatically lifted after a refugee's asylum application was processed and approved, which in 2017 took 11 months on average.

Figure 1: Treated and Control Counties
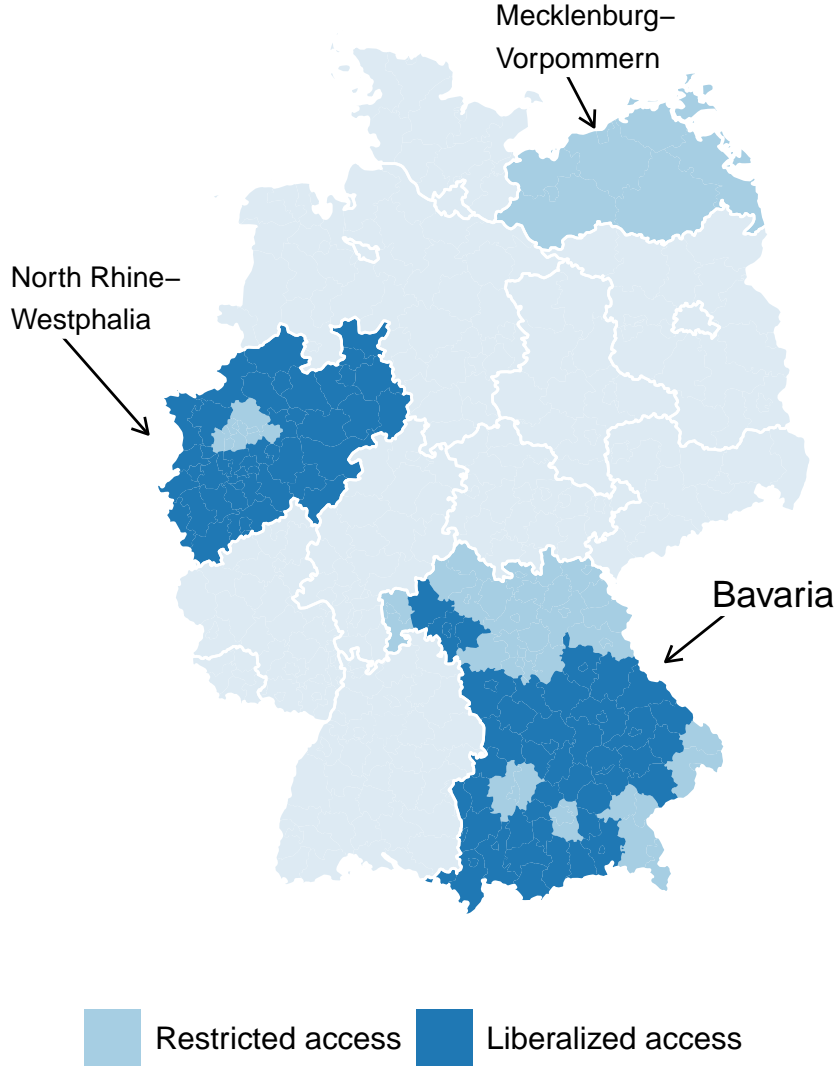
Note: The map shows counties where the labor market was liberalized for refugees (dark blue) and counties where the priority review remained in place (light blue). Note that the treatment was assigned at the level of the employment agency district, which consists of three or four adjacent counties. For reasons of causal identification, the empirical part of the paper focuses on two states with within-state variation, Bavaria and North Rhine-Westphalia. Regions shaded in the lightest color are states where labor market access was liberalized in all counties – these states are not part of our analyses.

tation of the policy. In North Rhine-Westphalia, the Ruhr region was exempted from the policy. Historically a center of the German manufacturing and coal industry, the *Ruhrgebiet* was slower to adjust to sectoral changes, resulting in higher unemployment rates than the rest of the state. In Bavaria, the government chose a cutoff-based approach: employment agency

districts with unemployment rates greater than the state average kept the priority review.[3] Finally, in Mecklenburg-Vorpommern the policy was never implemented. The treatment assignment is clearly not independent of level-differences on a variety of covariates, most importantly local unemployment (see also Figure A.1). To address this concern, we exploit panel data in a difference-in-differences design to account for pre-treatment differences on unobservables (see Section 4.2 for more details). In addition, we focus on within-state comparisons over time and avoid extrapolation across state boundaries. We therefore restrict our attention to the two regions that allow us to exploit within-state variation of the treatment over time: Bavaria and North Rhine-Westphalia. With a combined population of over 30 million, these states are the two largest in Germany in terms of population size, while their combined GDP equals that of Spain.

Before moving on to the empirical analysis in this paper, we discuss two concerns about selection into treatment in our setup. First, refugees might have incentives to relocate to counties where the priority review was suspended. However, in the two states we focus on, refugees are required to reside in the county that they were initially assigned to for a period of three years. This obviates concerns about selection into treatment at the individual-level. Second, the allocation scheme of refugees within Germany ensures that treated and control counties receive refugees that are similar in terms of pre-treatment characteristics (e.g. education, age, country of origin, etc.). After arriving in Germany, refugees are randomly allocated to one of the 16 federal states through a computer algorithm. The total number of refugees a state receives is proportional to its population size and tax revenue. Within each state, refugees are then allocated to counties. With few exceptions, the number of refugees a county receives is proportional to its population size. This allocation of refugees to counties is independent of the individual characteristics of the refugees.

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[3]Given the small number of employment agency districts, we are not able to exploit this cutoff through a regression discontinuity design (see also Figure A.8).

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4 Design

4.1 Data

To trace the economic and political effects of the policy, we collected panel data on i) refugee and native employment, ii) native wages, iii) voting results in statewide and federal elections, and iv) individual-level panel data on immigration attitudes among natives.

Employment and Wages   First, we obtained official data on employment from the German Federal Employment Agency. Specifically, we observe the number of employed refugees and natives in all German municipalities in every quarter between March 2008 and December 2018. Because data by residency status is only available for recent years, we focus on individuals from the eight largest sending countries of asylum seekers. These eight countries are Afghanistan, Eritrea, Iraq, Iran, Nigeria, Pakistan, Somalia, and Syria. The data captures both part-time and full-time employment. To ease interpretation, we scale refugee employment by municipality population. In addition, we collected data on the median monthly gross wage for native full-time employees at the county-level. Importantly, we are able to break this information down to different sub-groups of employees. We observe the evolution of wages for men, women, natives, foreigners, different age groups, and workers with differing educational backgrounds.

Voting behavior   Our primary political outcome is voting behavior. We draw on municipality level voting data for the most recent federal and state elections before and after the policy change. Whenever possible, we also add data on additional elections prior to 2016 to validate that the treatment assignment is exogenous to trends in political attitudes prior to

the policy change.[4]

The key outcome of interest is the vote share of left- / right-leaning parties who favor / oppose migration, respectively. To classify each party as pro- or anti-immigration, we rely on data on party positions from the Manifesto project (Volkens et al. 2020) (see Figure 2). Based on this data, we classify the Social Democratic Party (SPD), the Green Party, and the Left Party as left-leaning, while the Christian Democratic Union (CDU), the Christian Social Union (CSU), the Free Democratic Party (FDP) and the Alternative for Germany as right-leaning (see also Chou et al. 2018).

Before moving on, we discuss the classification of the FDP and CDU/CSU as right-leaning parties. First, we note that the FDP is generally considered a right-wing party in the German political spectrum. Its preferred coalition partner is the center-right CDU/CSU. However, the FDP is also generally supportive of labor migration (see Figure 2). We hence ensure that our results are robust to excluding the FDP in the group of right-wing parties.

Another potential concern regarding the party classification is that German chancellor and head of the CDU party, Angela Merkel, is generally perceived as a liberal politician with respect to asylum policy. We emphasize, however, that the CDU's position on asylum policy changed markedly between 2015 and 2017. For the 2017 federal election, the CDU heavily campaigned on policies to i) limit the influx of new migrants and ii) increase the rate of deportations of rejected asylum seekers (CDU/CSU 2017). At a CDU party congress in 2016, Merkel professed that "a situation like in the summer of 2015 must never recur" (Spiegel 2016). Merkel was also a strong proponent of the March 2016 EU-Turkey agreement to limit the influx of migrants into the EU. Finally, we note that the Bavarian branch of the party (CSU) has campaigned on an anti-immigration platform throughout our entire study period. In September 2015, CSU party leader Seehofer called Merkel's liberal refugee policy

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[4]In the mixed German electoral system, voters cast two votes. The first is for a 'direct' candidate in single member districts, while the second vote is cast for a party list. We focus on the second vote, which determines the proportional allocation of seats in the parliament.

a "mistake that the country will have to contend with for a long time" (Bayernkurier 2015). Among the major German parties, the CDU/CSU is ideologically closest to voters who want to limit immigration but refrain from supporting the far-right AfD.

Figure 2: Party positions on immigration
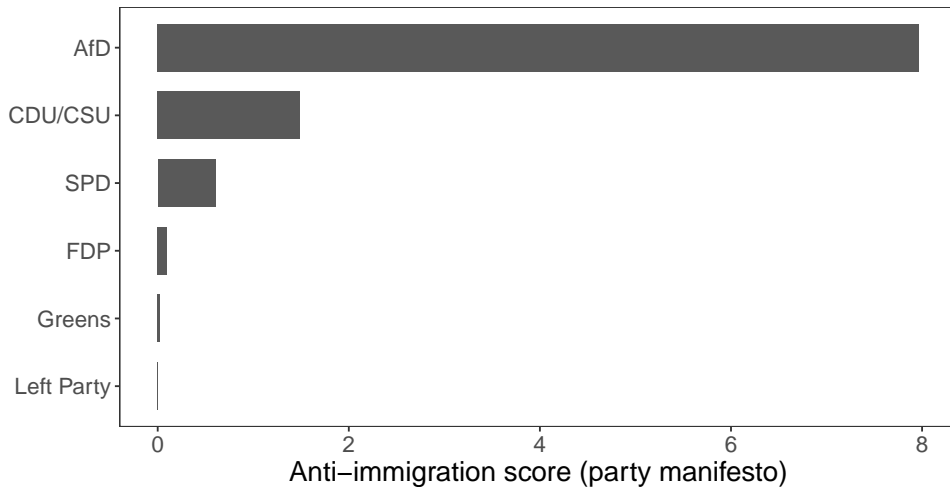
Note: The Figure plots the policy positions on immigration of the largest German parties in the 2017 federal election. Specifically, we show each party's score on the 'Immigration - negative' indicator of the Manifesto project (Volkens et al. 2020). Larger values indicate that a party is more hostile towards increasing immigration.

Attitudes toward migration   Finally, we measure attitudes toward migration and party identification over time by drawing on the German Socio-Economic Panel Survey (SOEP). The SOEP constitutes the largest (∼20,000 respondents) annual panel survey in Germany. In addition to standard demographic and socio-economic covariates, the SOEP includes an item that measures how 'worried' respondents are about immigration to Germany on a scale ranging from "very concerned" to "not concerned at all". For each respondent, we also observe the county in which he or she resides.

4.2 Identification strategy

Identifying the causal effect of refugee labor market liberalization on political attitudes is challenging since the treatment was not randomly assigned to employment agency districts. In Figure A.1, we examine the covariate balance between treated and control districts. We find that treated and control districts have similar income levels and foreigner shares. Unsurprisingly, the native unemployment rate was lower in treated districts, since the labor market liberalization was conditional on unemployment. To address the issue of unobserved confounding, we use a differences-in-differences design. This identification strategy rests on the assumption that treatment assignment is independent of *trends* in the outcome variables. Importantly, assignment independent of trends does not mean that liberalization has to be applied randomly. To substantiate our main identification assumption, we take a number of steps. First, we focus on within-state comparisons over time, thus avoiding extrapolation across state boundaries. Second, we examine outcome trends prior to the liberalization for our main outcomes, allowing us to compare trajectories for political and economic indicators. As we will discuss in more detail in the Results section, we find very little evidence that affected and unaffected counties followed differential trajectories prior to the labor market liberalization. The absence of divergence prior to the policy supports the parallel trends assumption.

Our estimation strategy depends on the outcome data at our disposal. Given the fine-grained quarterly data on refugee and native employment, we model these outcomes using the following generalized difference-in-differences specification:

$$Y_{ijt} = \mu_i + \delta_t + \sum_{k=-10}^{10} \beta_k \left(T_j \times \mathbf{1}_{t=k} \right) + \varepsilon_{ijt}$$

Here, Y_{ijt} is the total number of employed refugees or natives in municipality i nested in

employment agency district j in quarter t divided by the same municipality's total population in the same year. The terms μ_i and δ_t denote municipality and quarter fixed effects. The main parameters of interest are a series of coefficients on the leads and lags of the treatment, denoted by β_k. This parameter gives us the difference between treated and control regions for ten quarters before and after the treatment date. As is standard-practice in leads and lags regression analyses, we leave out one interaction for the last pre-treatment period (2nd quarter of 2016) which serves as the baseline for all estimated treatment effects. We use a similar specification for native wages, which we observe at the county-year level.

In contrast to the employment and wage data, both federal and state elections are held every 4–5 years and are thus not observed in similarly short time-intervals. We therefore model these outcomes using a standard two-period difference-in-differences specification:

$$Y_{ijt} = \beta_0 T_j + \beta_1 P_{it} + \tau(P_{it} \times T_j) + \varepsilon_{ijt}$$

Here, Y_{ijt} is the vote share of a given party in municipality i in agency district j in time period t. For treated districts where the labor market was liberalized after August 2016, $T_j = 1$ and $T_j = 0$ otherwise. Finally, $P_{it} = 1$ for municipality i when $t >$ August 2016. When we include both states in the same sample for the analysis, we interact T_j and P_{it} with a state-indicator variable, allowing for state-specific baseline differences and time trends. In this model specification, the key parameter of interest is $\hat{\tau}$, the difference-in-differences estimate for the causal effect of the policy on electoral behavior.

To provide additional evidence in support of our key identification assumption, parallel trends of electoral outcomes in the absence of the policy change, we also estimate a series of leads and lags models for federal and state elections. We present the results in Figures A.5, A.6 and A.7. Reassuringly, we find that treated and control municipalities were on similar trajectories with respect to electoral behavior prior to the policy change. The treatment does not predict changes in the level of electoral support for right-wing or left-wing parties

prior to the policy change in August 2016.

As explained in Section 4.1, the data we analyze was collected at different levels. The over 10,000 municipalities in Germany are the country's smallest administrative unit, with an average population size of about 7,400. German municipalities are nested in 401 counties, which are nested in 156 employment agency districts. For all our regression analyses, we cluster standard errors at the level of employment agency districts, the level of treatment assignment.

5   Results

5.1   Effect of liberalization policy on refugee employment

In a first step, we assess whether the labor market liberalization policy did, indeed, increase refugee employment. Figure 3 plots the mean number of refugees in employment on a per capita basis across the treatment and control group across time. The Figure underlines nearly identical pre-treatment trends prior to the policy change. Once the policy went into effect, in August 2016, the two lines diverge noticeably: opening the labor market to refugees, increased the share of refugees in the labor market significantly. To estimate the substantive impact of the policy in a more rigorous manner, Figure 4 presents a lag and lead specification (top panel). Reassuringly, we confirm statistically insignificant point estimates in all pre-treatment periods. The null finding underlines that affected and unaffected countries did not experience differential growth in refugee employment prior to the policy. As a result, the estimates in Figure 4 support the parallel trends assumption. For the post-treatment periods, by contrast, Figure 4 corroborates that the share of refugees in the labor market was significantly greater in treated counties compared to the control group. One potential objection to this finding is that the policy may have affected migration into treated counties from control counties. One could argue, for instance, that the policy led skilled refugees

to migrate into counties with a more liberal labor market access. Importantly, however, refugees in the states we examine were prohibited from moving to a county different from the one they were assigned for a period of three years after arrival (Hilbig and Riaz 2020).

Figure 3: Refugee labor market employment
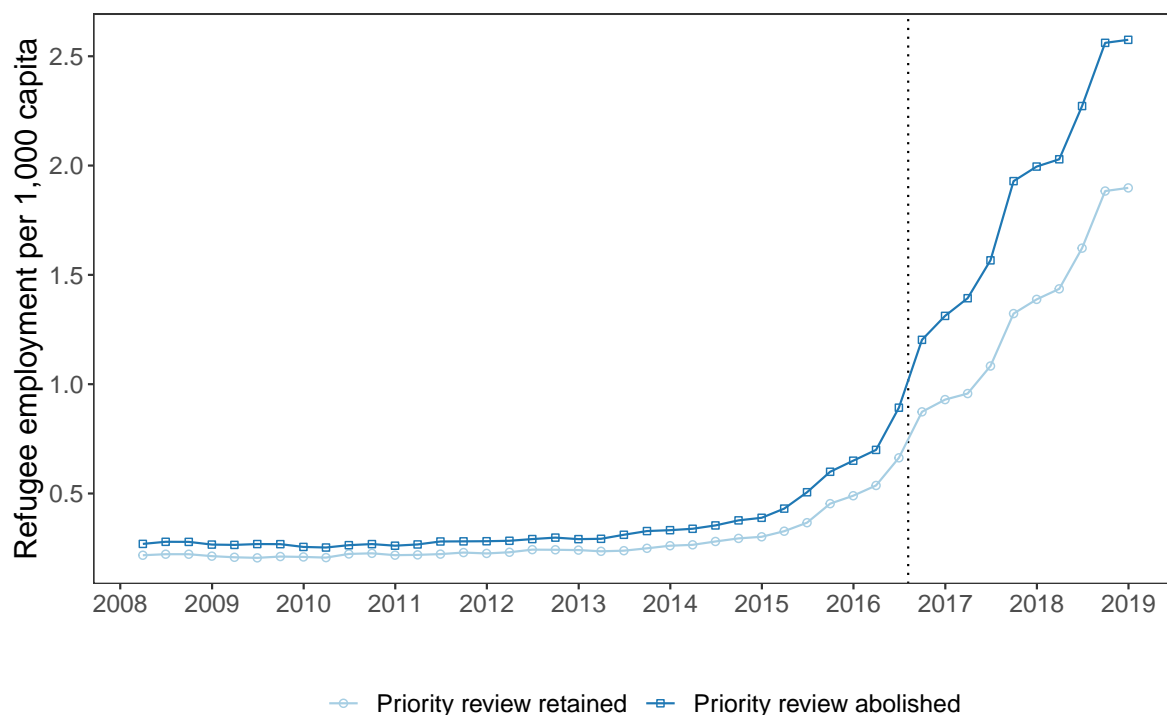
Note: The Figure shows the raw trends in refugee employment in treated and control municipalities between 2008 and 2018. For each quarter, we calculate the mean refugee employment per 1,000 capita in the group of treated and control municipalities. The sample contains all municipalities in Bavaria and North Rhine-Westphalia.

Figure 4: Refugee and native employment, lags and leads estimation
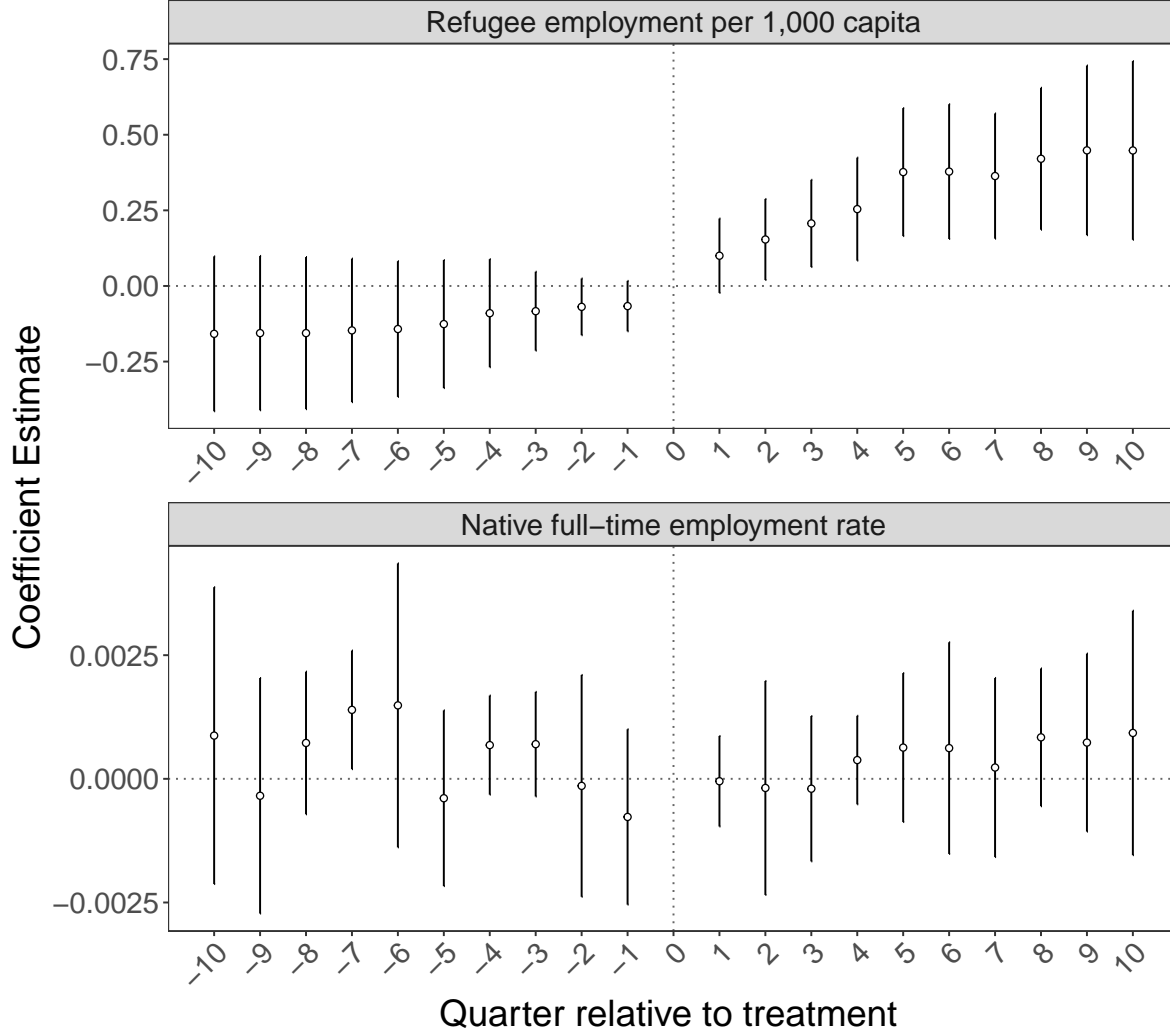
Note: The Figure shows present the effects of labor market liberalization for refugees on refugee employment (top panel) and native employment (bottom panel). The two panels plot coefficients and 95 percent confidence intervals from leads and lags specifications as described in Section 4.2. Standard errors are clustered at the employment agency district level. The sample contains all municipalities in Bavaria and North Rhine-Westphalia. As is standard-practice in leads and lags regression analyses, the last pre-treatment period is omitted as the baseline period. Coefficients hence correspond to the change in the difference between the treated and control groups on the outcome measure, relative to the last pre-treatment period.
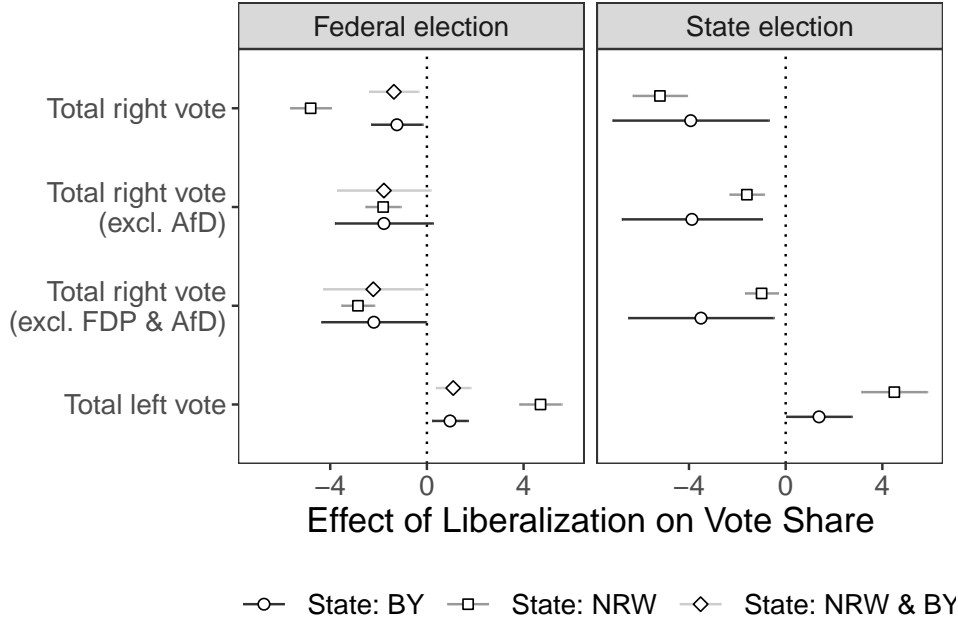
5.2 Effect of liberalization policy on natives' voting behavior

In the next step, we turn to our core outcome of interest: voting behavior. Did the policy affect the vote share of left- and right-leaning parties? Figure 5 shows that the labor market liberalization policy *reduced* support for parties on the political right by two percentage points. The result is detectable both for the 2017 federal election as well as two state elections in Bavaria and North Rhine-Westphalia. Reassuringly, there is no evidence that the effect is driven by one of the two states alone: We find statistically significant point estimates of similar magnitude in both states. We also note that the negative effect on right-wing vote shares is present both when including or excluding the far-right Alternative for Germany.[5] By the same token, we find that left-leaning parties experience a rise in support due to the labor market liberalization policy.

A potential concern for our difference-in-differences design is that the treatment and control districts were on different electoral trajectories prior to the policy change. We address this concern in two key ways. First, we examine the case of Bavaria, where the labor market liberalization was enacted conditional on a sharp, district-level unemployment rate cutoff. In Figure A.8, we show that the effect of the policy on right-wing parties remains similar when we estimate the treatment effect in small bandwidths around the assignment cutoff. This result implies that the effect of the labor market liberalization policy on voting behavior persists even when accounting for the key determinant of the labor market liberalization policy: prior unemployment rates. Second, we devise a placebo test to substantiate that initial differences in unemployment are not predictive of trends in political attitudes. Specifically, we analyze the relationship between changes in electoral support for right-wing parties between 2013 and 2017 and unemployment rates at the municipality level. Importantly, we include employment-district fixed effects in our estimation. By focusing on variation within

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[5]Since the AfD is a young party, it did not compete in some of the pre-treatment elections. We therefore present results with and without the AfD in the right-wing party group.

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Figure 5: Effect of Refugee Labor Market Access on Voting Behavior

employment agency districts, we hold the treatment status of municipalities constant. If differences in initial unemployment are associated with differential political trends, we would expect a non-zero relationship between unemployment and changes in electoral behavior. Reassuringly, Table A.2 shows that this is not the case. There is no statistically significant relationship between trends in electoral behavior and local unemployment rates within employment agency districts. This finding represents an additional piece of evidence buttressing the assumption that level differences in unemployment are not associated with time-varying confounding.

6   Mechanism

Why did the labor market liberalization policy change voting behavior? Even if the coefficients presented above can be interpreted as causal effects, different mechanisms may have given rise to the positive effect on left-leaning parties. To explore mechanisms, we proceed in three steps. In a first step, we explore whether the policy truly changed preferences, rather than changing who turns out to vote (i.e., differential mobilization). In a second step, we explore via what mechanism the policy itself affected preferences. We explore two main channels: an economic channel (specifically, natives' wages and employment) as well as a social channel (specifically, social contact).

6.1   Did the policy change preferences?

In a first step, we explore whether the policy changed natives' preferences, rather than simply shifting who turned out to vote. We do so in four steps. First, in Figure A.4 we show that the policy, by and large, had no effect on turnout. The evidence thus implies that the policy arguably did not mobilize new voter groups, pointing toward a persuasion mechanism. That said, the turnout results do not provide conclusive evidence for changed preferences at the individual level (ecological inference problem). Second, we make use of individual-level panel evidence, tracking people before and after the policy change. In Table A.3, we show that residents in treated areas are significantly more likely to identify with left-leaning parties after the policy went into effect compared to residents in the control group. Third and more relevant to our case, we also rely on evidence from the SOEP panel to corroborate that the policy changed preferences toward migration. Utilizing the panel structure of the survey, we confirm that natives' attitudes towards refugees became significantly more positive in regions where the labor market was liberalized. Specifically, Figure 6 shows the coefficient of the treatment dummy on a 3-point scale of concerns about

immigration to Germany. While we observe marginally significant results for all respondents, we find strong and significant effects for respondents in full-time employment, which we elaborate on below. Fourth, we also collected evidence on support for refugee integration based on a large-scale, cross-sectional online survey. While this data set includes an item that directly asks about refugee rather than immigrant integration, it does not allow us to estimate panel models akin to our main specification gleaned from the SOEP. Instead, we implement a geographic regression discontinuity design, comparing residents who live in geographically close regions with differing treatment status. In Figure A.9, we show evidence that liberalization induces respondents to view refugees in a more positive light. However, we emphasize that identification in the RDD setting likely requires stronger assumptions than in the case of the SOEP panel results. We provide more details on the RDD design and results in Section A.9 in the SI.

Figure 6: Effect of Policy on Attitudes towards Immigration
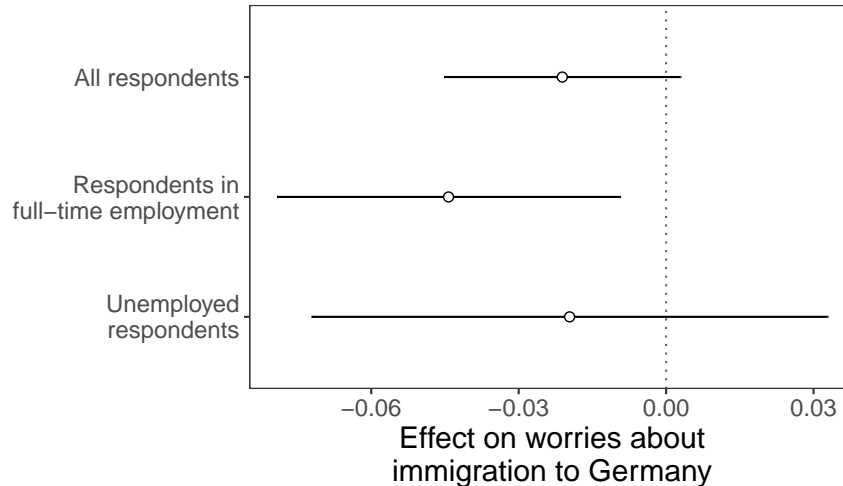
Note: Results from difference-in-difference using the SOEP panel survey. The outcome is a 3-point scale on how worried respondents are about immigration to Germany, where higher values indicate greater concern about immigration. We compare respondents in treated and untreated counties between 2016 and 2017. We show results separately for the full sample (N = 24,047), respondents in full-time employment (N = 9,213) and unemployed refugees (N = 8,786). Standard errors are clustered at the employment agency district levels.

6.2 How did the policy change preferences?

Having shown four pieces of evidence that the policy changed natives' preferences, we turn to the question of *how* the policy engendered this effect. In the theoretical section, we delineated two major causal channels: First, the policy may have positively affected economic fundamentals, leading natives to adopt more favorable views on migration. Second, the policy may have had an effect on social relations—particularly refugee-native contact—and thus spurred more positive attitudes toward migration.

6.3 Economic channel

We begin by exploring whether the policy had a positive effect on economic fundamentals. Two outcomes are particularly important: natives' employment and natives' wages. A reduction in the former and an increase in the latter could arguably both engender to more positive attitudes toward migration. First, in the bottom panel of Figure 4 we show that the policy had *no* effect on natives' employment. The first economic sub-mechanism—employment—is thus mute. Second, Figure 7 shows that the policy did not affect natives' wages. Similar null-evidence is shown in the panel specification in Figure A.3. As before, we present lags and leads of the treatment, this time using annual wage data. Akin to the employment results, we find that wage trends prior to the labor market liberalization are the same in affected and unaffected counties. However, we do not find any evidence that refugee labor market liberalization negatively affected native wages. Importantly, this also holds for low-skill workers, who might experience greater competition from refugee employees. We note that the null finding is not simply a result of low statistical power. The point estimates are generally below 20 Euros per month, while the average monthly gross wage for full-time employees was 4,575 Euros in Bavaria and 4,339 Euros in North Rhine-Westphalia in 2018. Taken together, we therefore conclude that there are no effects—neither positive

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nor negative— of the policy on economic outcomes. While the policy did raise employment for refugees, it did not change economic conditions of natives.

Figure 7: Effect of refugee labor market access on native wages
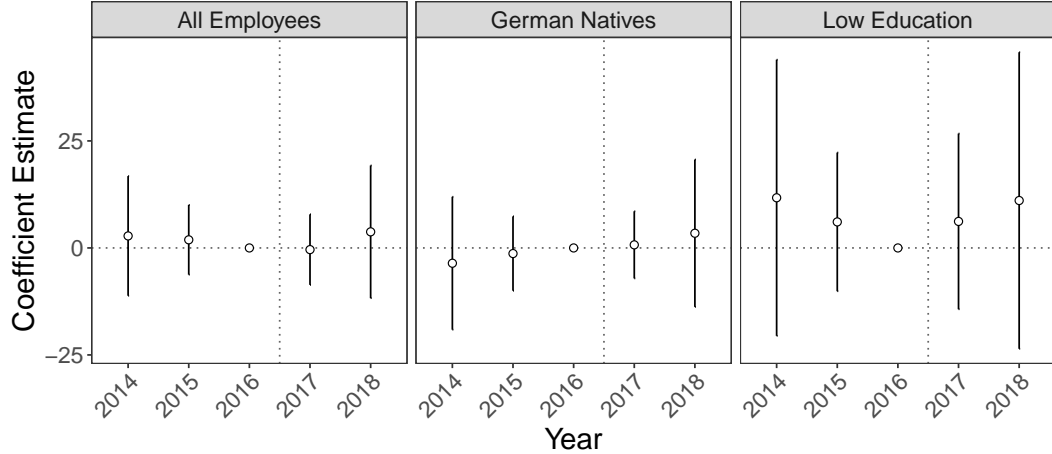
Note: The figure presents the effects of labor market liberalization for refugees on average monthly wages of all employees, German employees and employees with low educational attainment. We plot coefficients and 95 percent confidence intervals from leads and lags specifications as described in Section 4.2. Standard errors are clustered at the employment agency district level. The sample contains all municipalities in Bavaria and North Rhine-Westphalia. As is standard practice in leads and lags regression analyses, the last pre-treatment period is omitted as the baseline period. Coefficients hence correspond to the change in the difference between the treated and control groups on the outcome measure, relative to the last pre-treatment period, which is 2016 in our case.

6.4 Social channel

We next turn to outcomes pertaining to social interactions. While we lack direct individual-data on refugee-native interactions, below we attempt to triangulate possible social effects by drawing on different sources of heterogeneity, which are in line with the conjecture that the policy had a positive impact on social relations. First, Figure 6 shows that the negative effect of the treatment dummy on the 3-point scale capturing "migration concerns" is most pronounced among respondents in full-time employment. If the effect of the policy operated outside of the workplace, we would expect to see similar coefficients among full-time employees and part- or non-employed individuals. The fact that the coefficient is strongest among full-time employees suggests that the causal mechanism works via the workplace.

Second and related, if the effect is produced by work-place interactions, we should see no effect of the policy on unemployed individuals. We confirm as much in Figure 6. Third, if the effect truly operates via work-place interactions, we must ensure that there were no broader social discussions pertaining to the law. We confirm as much in Figure A.2. As can be seen, the law received almost no mentions in local newspapers. A final way to triangulate the social effect of the policy is to assess the subset of individuals opposed to migration. If the effect functions via positive interactions at the workplace, one would arguably *not* see a positive effect on left-leaning respondents. After all, left-leaning individuals already are pro-migration and would thus not be positively impacted by workplace interactions. Right-leaning respondents, by contrast, arguably harbor negative prejudice toward refugees and should thus—if the social mechanism is at play—change their minds. Evidence in favor of this channel is provided in Table A.3 where we subset the panel data by respondents' prior attitudes toward immigration. We find that the aforementioned change in party identification toward left-leaning parties is most pronounced among right-wing respondents who were worried about immigration prior to the labor market liberalization policy. This finding thus supports a reading whereby individuals had positive encounters at the workplace.

While we observe relatively moderate effects on refugee employment, we emphasize that it likely translates to a substantial increase in contact between natives and refugees. The follows from several facts. The average municipality in the two affected states has about 12,500 inhabitants. In a municipality of 12,500 people, we estimate that an additional 5.6 refugees find employment due to the labor market liberalization. Based on survey data from the SOEP[6], we know that most employed refugees work in medium-sized firms with between 20 and 100 employees. Given the relatively small firm size, all employees likely either knew of or directly interacted with the new refugee coworkers. Based on the modal firm with between 20 and 100 employees, we assume a mean firm size of 60 employees. As a result, about 335 (5.6*60) native employees had direct contact with refugees that gained employment because

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[6]Descriptive statistics based on the 2017 SOEP-IAB-BAMF refugee sub-sample.

of the policy change.

Beyond natives who had direct contact with refugees, many more may have indirectly learned of the newly employed refugees through interpersonal discussions (Druckman, Levendusky and McLain 2018). In the fall 2017 Eurobarometer survey, more than 90% of respondents indicated that they 'often' or occasionally discuss politics with their friends and family (Statista 2020). Since refugee integration was the most salient political issue in Germany during our study period (Infratest dimap 2017), the new employees likely featured prominently in conversations. According to a 2018 survey, Germans on average have about 11 friends, including 3-4 'close' friends (YouGov 2018). In light of this, we assume that directly affected natives shared information about their new coworkers with about 6 close social contacts. For this relatively realistic scenario, about 2000 natives (335*6) in a municipality of 12,500 inhabitants (16%) directly or indirectly learned about the newly employed refugees. Consequently, the share of natives who directly or indirectly learned of the policy is sizable and consistent with the magnitude of the effect on voting behavior.

7   Conclusion

How does the economic integration of refugees affect public opinion toward migration? This paper makes use of variation in labor market liberalization for refugees in Germany, where some employment districts granted refugees unrestricted labor market access, while access in others remained restricted. We found that the policy not only had a pronounced positive effect on refugee employment but that it also led natives to espouse pro-immigration parties in federal and state elections. The evidence adds to an ongoing debate in the social sciences whether a lack of economic opportunities and welfare dependence among refugees fuel xenophobic attitudes. In line with sociotropic accounts of attitude formation toward migration, our study demonstrates that natives reward refugees for joining the labor force. Interestingly, we also show that increasing refugee employment has no effect on natives' wages or

employment. Our findings suggest that the economic integration of refugees affects natives' attitudes via a "social" channel, not a purely economic one.

Our study offers a second important insight into political behavior concerning integration policy. The finding that the center-right CDU and CSU were punished for liberalizing labor market access offers one explanation for why conservative parties oppose liberal integration policies despite positive economic effects. In Germany, the labor market liberalization had a positive effect on refugee employment. Fears of labor market competition with natives did not materialize. Conservative parties might oppose pro-integration policies because they fear that voters will learn that the hypothesized negative effects—propelled by conservative parties—do not materialize and then shift to progressive parties. Conservative parties are thus trapped in their own rhetoric and may need to oppose pro-integration policies because implementing them—despite yielding positive effects—benefits the political left.

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Part I

Appendix

Table of Contents

A Supporting Information (Online Only)

A.1 Descriptive Statistics

Table A.1: Summary Statistics

| Variable	Dataset	Level of Analysis	Time-period	Subset	Mean	S.D.	N	Min	Max
Native Employment Rate	Employment Data	Municipality	2008 - 2018	Bavaria + NRW	0.46	0.04	107888	0.20	0.83
Refugee Employment per 1,000 capita	Employment Data	Municipality	2008 - 2018	Bavaria + NRW	0.64	1.13	107888	0.00	16.37
Monthly gross wage: all employees	Wages	County	2014 – 2018	Bavaria + NRW	3150.42	348.45	745	2483.00	4896.90
Monthly gross wage: men	Wages	County	2014 – 2018	Bavaria + NRW	3349.69	419.00	745	2619.66	5544.46
Monthly gross wage: women	Wages	County	2014 – 2018	Bavaria + NRW	2721.89	305.96	745	2033.92	3846.63
Monthly gross wage: german natives	Wages	County	2014 – 2018	Bavaria + NRW	3238.94	374.59	745	2491.26	5132.56
Monthly gross wage: foreigners	Wages	County	2014 – 2018	Bavaria + NRW	2427.42	339.92	704	1642.10	4082.02
Monthly gross wage: 15 - 25 years old	Wages	County	2014 – 2018	Bavaria + NRW	2339.70	164.38	740	1838.25	3105.57
Monthly gross wage: 25 - 55 years old	Wages	County	2014 – 2018	Bavaria + NRW	3207.98	364.22	745	2528.09	5136.45
Monthly gross wage: 55 - 65 years old	Wages	County	2014 – 2018	Bavaria + NRW	3399.55	407.09	745	2509.75	5294.02
Monthly gross wage: low education	Wages	County	2014 – 2018	Bavaria + NRW	2474.28	250.27	722	1922.87	3557.55
Monthly gross wage: vocational training	Wages	County	2014 – 2018	Bavaria + NRW	3119.25	277.29	745	2503.63	4593.24
AfD vote share	Federal Elections	Municipality	2013 – 2017	Bavaria + NRW	8.22	5.04	4895	0.00	28.11
CDU/CSU vote share	Federal Elections	Municipality	2013 – 2017	Bavaria + NRW	47.06	8.47	7358	21.26	82.66
FDP vote share	Federal Elections	Municipality	2013 – 2017	Bavaria + NRW	9.46	4.74	7358	0.40	30.74
Greens vote share	Federal Elections	Municipality	2013 – 2017	Bavaria + NRW	7.49	2.82	7358	0.67	22.41
Die Linke vote share	Federal Elections	Municipality	2013 – 2017	Bavaria + NRW	4.84	1.93	7358	0.00	18.18
Other parties vote share	Federal Elections	Municipality	2013 – 2017	Bavaria + NRW	8.46	2.86	7358	1.23	24.29
SPD vote share	Federal Elections	Municipality	2013 – 2017	Bavaria + NRW	17.23	6.85	7358	2.04	48.77
Turnout in %	Federal Elections	Municipality	2013 – 2017	Bavaria + NRW	74.10	6.29	7326	37.85	91.15
Total right vote share	Federal Elections	Municipality	2013 – 2017	Bavaria + NRW	61.98	7.26	7358	30.66	87.10
Total right (excl. AfD) vote share	Federal Elections	Municipality	2013 – 2017	Bavaria + NRW	56.52	7.82	7358	29.06	86.05
Total left vote share	Federal Elections	Municipality	2013 – 2017	Bavaria + NRW	29.56	8.25	7358	7.14	62.65
CSU vote share	Bavarian State Elections	Municipality	2013 – 2018	Bavaria	48.01	9.35	4112	18.84	83.96
SPD vote share	Bavarian State Elections	Municipality	2013 – 2018	Bavaria	12.03	6.86	4112	0.98	43.78
Freie Waehler vote share	Bavarian State Elections	Municipality	2013 – 2018	Bavaria	11.99	5.96	4112	2.12	47.94
Greens vote share	Bavarian State Elections	Municipality	2013 – 2018	Bavaria	9.92	5.31	4112	0.46	35.16
FDP vote share	Bavarian State Elections	Municipality	2013 – 2018	Bavaria	3.23	1.87	4112	0.27	39.58
Other parties vote share	Bavarian State Elections	Municipality	2013 – 2018	Bavaria	15.02	5.14	4112	3.48	40.39
AfD vote share	Bavarian State Elections	Municipality	2013 – 2018	Bavaria	10.80	3.12	2056	3.83	24.54
Total right vote share	Bavarian State Elections	Municipality	2013 – 2018	Bavaria	56.65	7.15	4112	28.58	85.19
Total right (excl. AfD) vote share	Bavarian State Elections	Municipality	2013 – 2018	Bavaria	51.25	8.68	4112	23.69	84.41
Total left vote share	Bavarian State Elections	Municipality	2013 – 2018	Bavaria	21.95	7.20	4112	4.94	49.90
Turnout in %	Bavarian State Elections	Municipality	2013 – 2018	Bavaria	70.77	6.84	4112	47.70	90.60
CDU vote share	NRW State Elections	Municipality	2012 – 2017	NRW	34.74	8.42	792	15.56	60.93
SPD vote share	NRW State Elections	Municipality	2012 – 2017	NRW	33.36	7.10	792	13.15	56.66
FDP vote share	NRW State Elections	Municipality	2012 – 2017	NRW	10.47	2.96	792	3.49	23.20
Greens vote share	NRW State Elections	Municipality	2012 – 2017	NRW	7.42	2.94	792	2.04	19.56
Other parties vote share	NRW State Elections	Municipality	2012 – 2017	NRW	7.82	4.06	792	1.77	16.12
AfD vote share	NRW State Elections	Municipality	2012 – 2017	NRW	6.72	1.79	396	3.09	14.59
Die Linke vote share	NRW State Elections	Municipality	2012 – 2017	NRW	2.83	1.21	792	0.80	8.40
Total right vote share	NRW State Elections	Municipality	2012 – 2017	NRW	48.57	11.44	792	19.04	76.92
Total right (excl. AfD) vote share	NRW State Elections	Municipality	2012 – 2017	NRW	45.21	9.69	792	19.04	72.16
Total left vote share	NRW State Elections	Municipality	2012 – 2017	NRW	43.61	8.48	792	16.82	67.52
Turnout in %	NRW State Elections	Municipality	2012 – 2017	NRW	63.98	5.30	792	48.70	78.20

Figure A.1: Covariate balance: treated and control counties
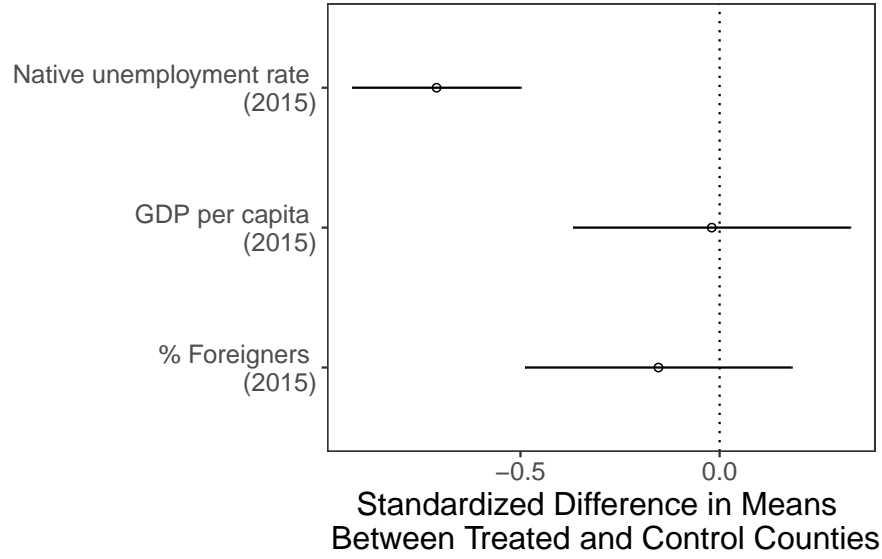
Note: The Figure shows the pre-treatement covariate balance between treated and control counties as of December 2015. We examine three covariates: the native unemployment rate, GDP per capita, and the local foreigner share in the population. The sample contains all counties in Bavaria and North Rhine-Westphalia. We adjusted for baseline differences between the two states using state-fixed effects. We are hence considering within-state covariate balance. All variables were standardized.

A.2 News Coverage of Labor Market Liberalization

Figure A.2: Media coverage of labor market liberalization
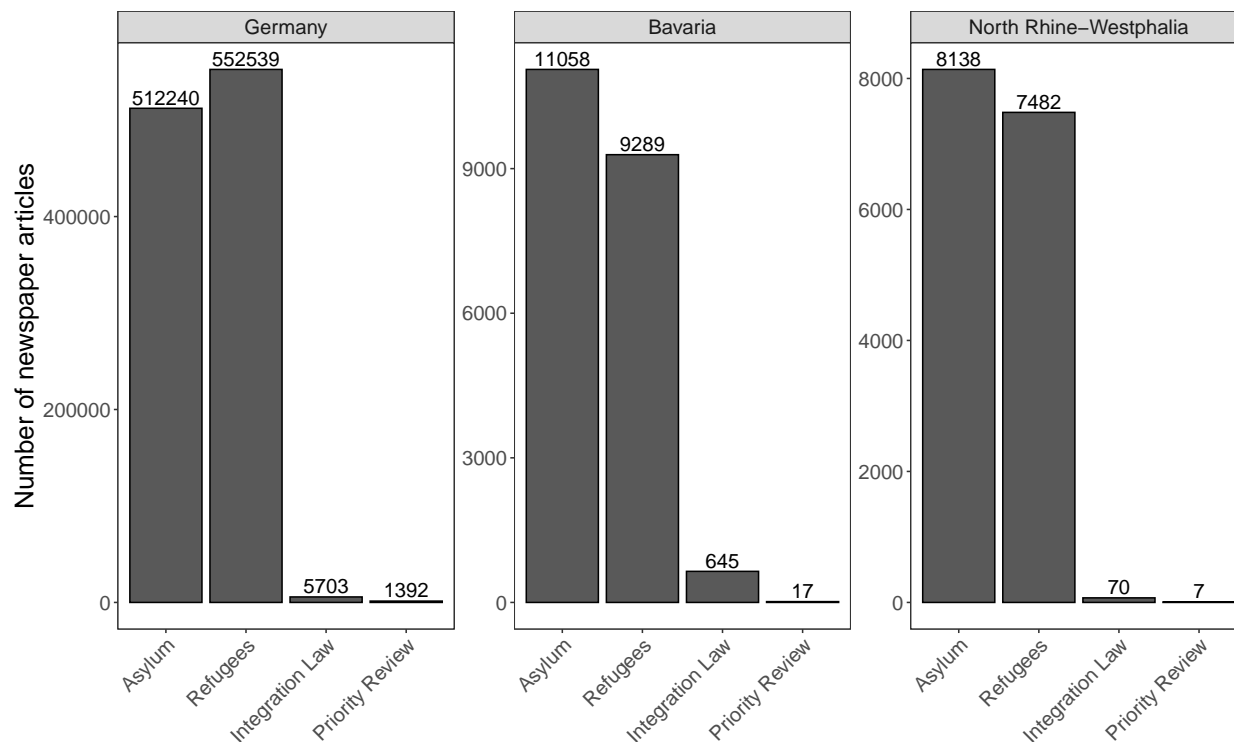
Note: The figure shows the number of newspaper articles published on the labor market liberalization and integration between January 2016 and December 2017 in Germany. We compare this to the total number of articles published on the topics of asylum and refugees during the same period. The left panel shows the numbers for all of Germany, including both local and national news. The other two panels focus on local news in Bavaria and North Rhine-Westphalia respectively. Data obtained from the online press-archive *Genios*.

## A.3   Labor Market Effects of the Policy

Figure A.3: Effect of labor market liberalization on wages
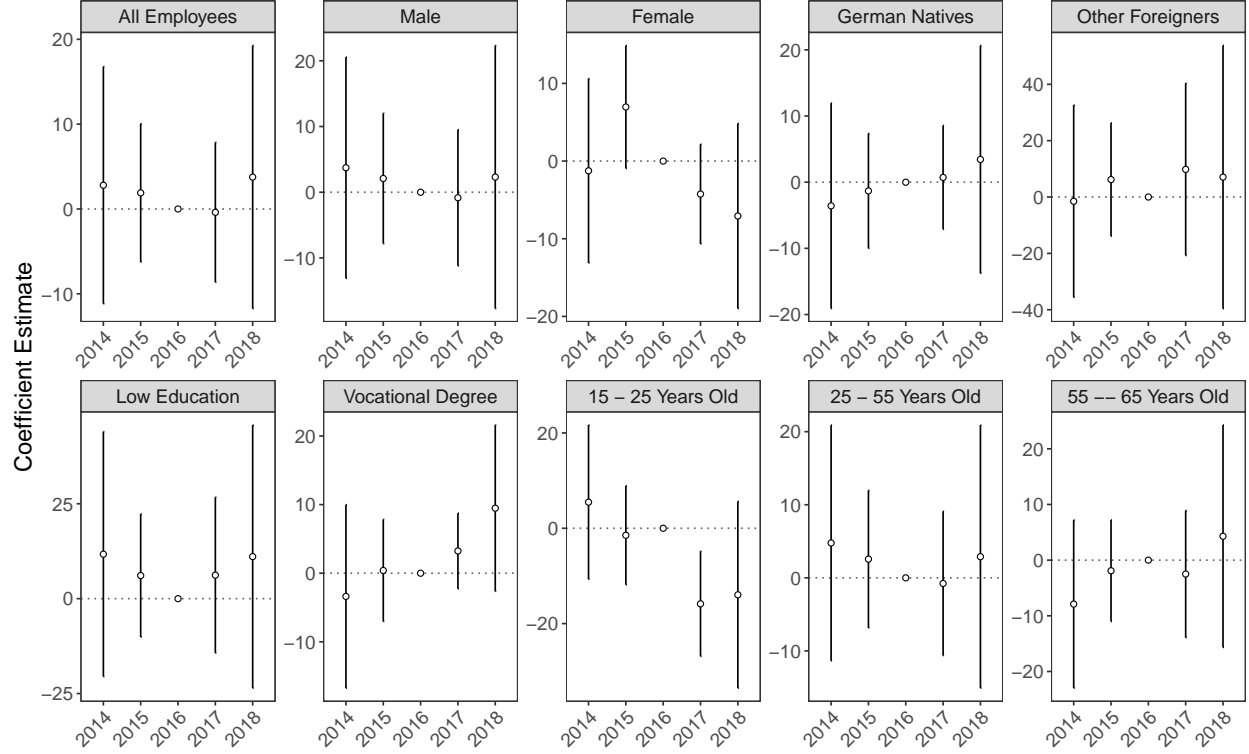
Note: Effect of the labor market liberalization on the median monthly gross wages of workers with varying socio-demographic characteristics. Treatments effects are estimated on the basis of county-level panel data 2014 – 2018. Standard errors are clustered at the employment agency district level. The sample consists of all counties in Bavaria and North Rhine-Westphalia. As is standard-practice in leads and lags regression analyses, the last pre-treatment period (2016) is omitted as the baseline period. Coefficients hence correspond to the change in the difference between the treated and control groups on the outcome measure, relative to the last pre-treatment period.

A.4   Effects on Electoral Behavior

Figure A.4: Refugee Labor Market Access and Electoral Behavior, Disaggregated by Party
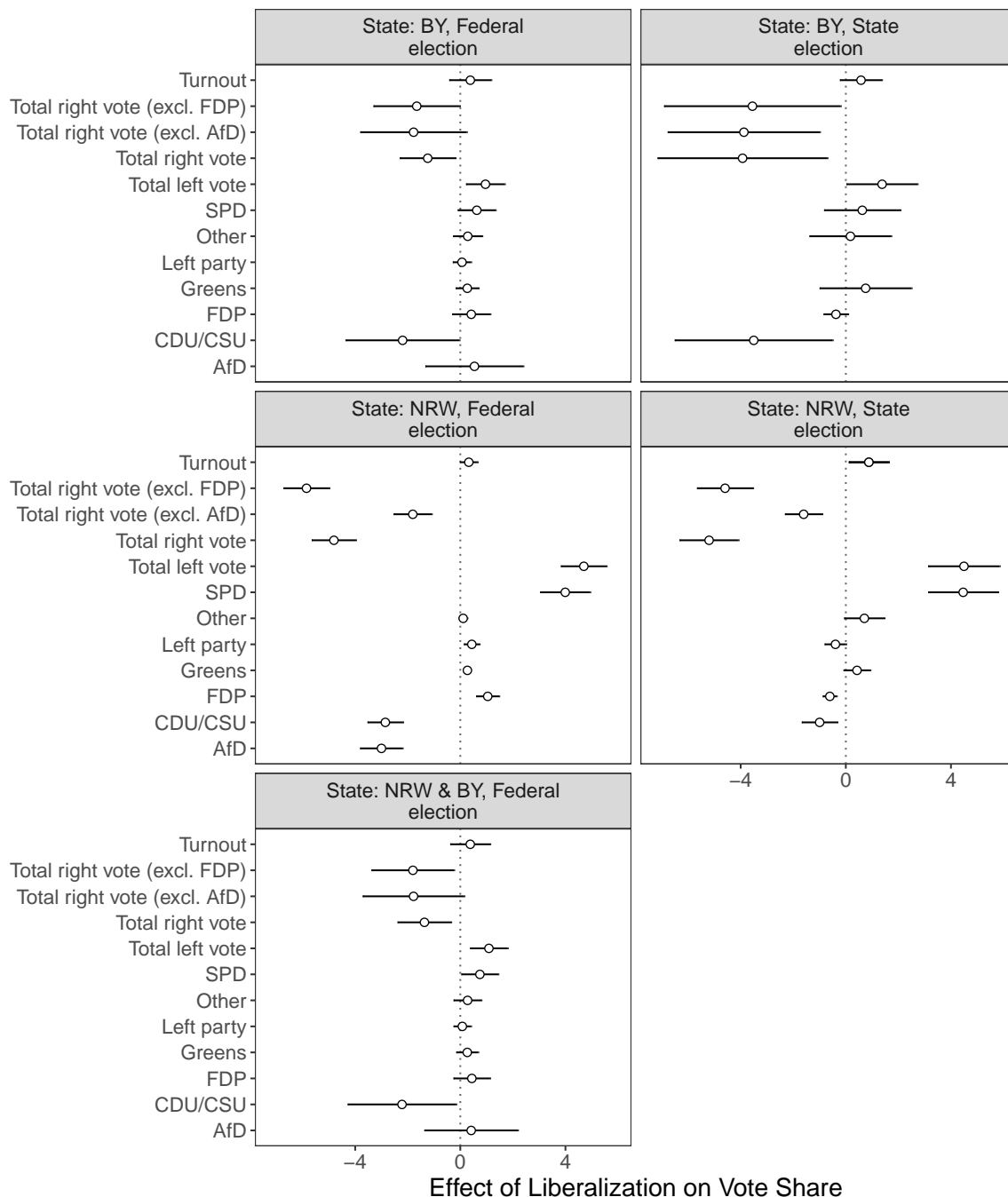
Note: Results from two-period difference-in-differences models. The x-axis shows the estimated effect of labor market liberalization on the vote share of different parties and party groups (in percentage points). We analyze elections for federal and state parliaments. Standard errors are clustered at the employment agency district level.
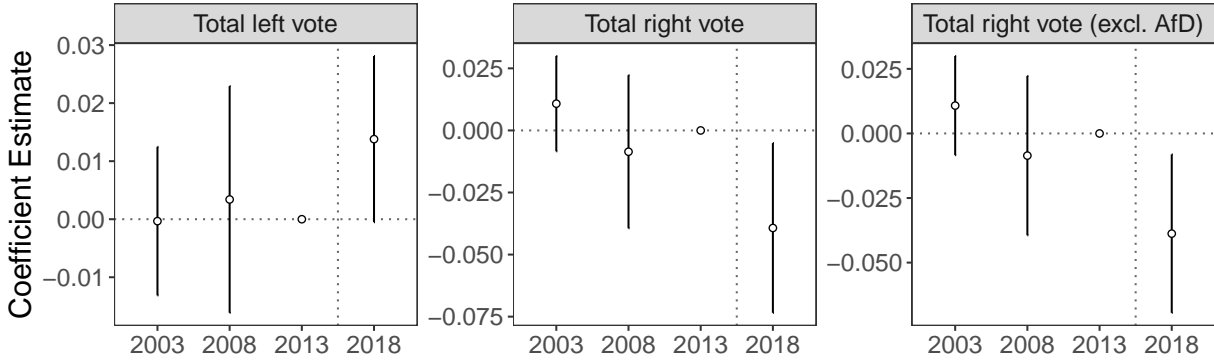
A.5 Lags and Leads Analyses: Elections

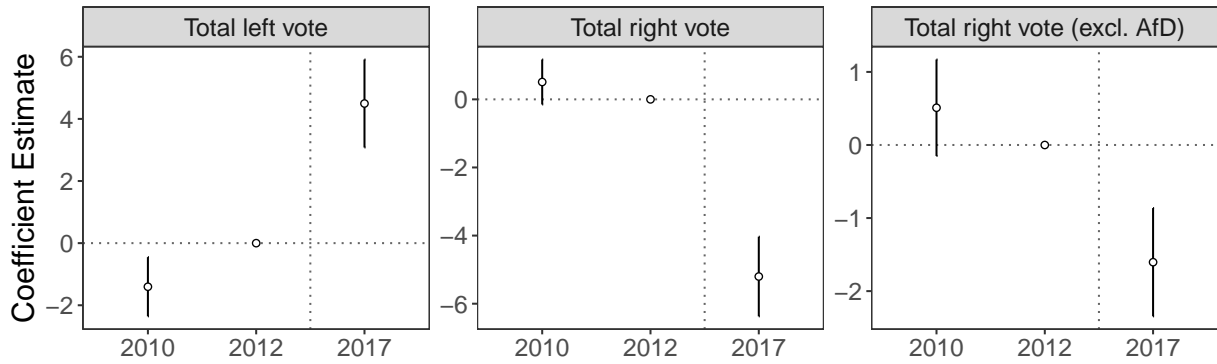Figure A.5: Leads and lags: federal elections
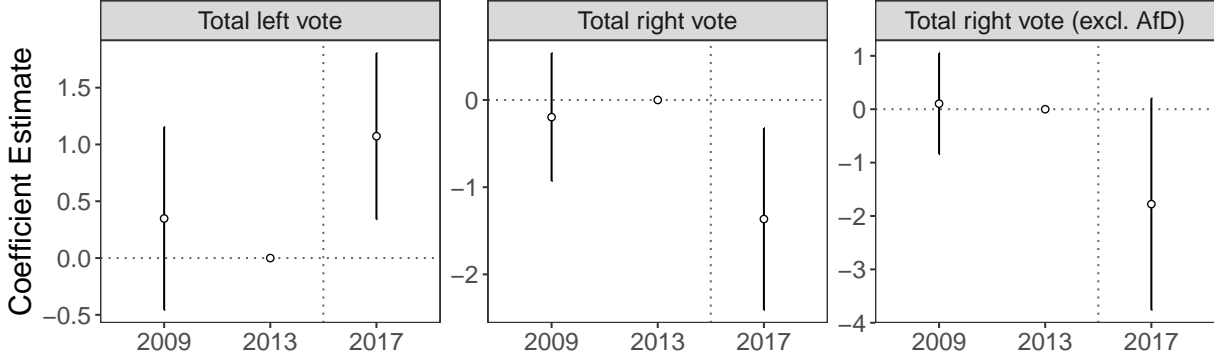
Note: Results from leads and lags analysis for federal elections between 2009 and 2017. We estimate the same generalized difference-in-differences specification as described in Section 4.2. We have two pre-treatment periods (2009 and 2013) and one post-treatment period (2017). The sample includes all municipalities in Bavaria and North Rhine-Westphalia. As is standard-practice in leads and lags regression analyses, the last pre-treatment period (2013) is omitted as the baseline period. Coefficients hence correspond to the change in the difference between the treated and control groups on the outcome measure, relative to the last pre-treatment period.

Figure A.6: Leads and lags: state elections in Bavaria

Note: Results from leads and lags analysis for state elections in Bavaria between 2003 and 2018. We estimate the same generalized difference-in-differences specification as described in Section 4.2. We have three pre-treatment periods (2003, 2008, and 2013) and one post-treatment period (2018). The periods correspond to the timing of state-elections in Bavaria. The sample includes all municipalities in Bavaria. As is standard-practice in leads and lags regression analyses, the last pre-treatment period (2013) is omitted as the baseline period. Coefficients hence correspond to the change in the difference between the treated and control groups on the outcome measure, relative to the last pre-treatment period.

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Figure A.7: Leads and lags: state elections in North Rhine-Westphalia

Note: Results from leads and lags analysis for state elections in North Rhine-Westphalia between 2010 and 2017. We estimate the same generalized difference-in-differences specification as described in Section 4.2. We have two pre-treatment periods (2010 and 2012) and one post-treatment period (2017). The periods correspond to the timing of state-elections in North Rhine-Westphalia. The sample includes all municipalities in North Rhine-Westphalia. As is standard-practice in leads and lags regression analyses, the last pre-treatment period (2012) is omitted as the baseline period. Coefficients hence correspond to the change in the difference between the treated and control groups on the outcome measure, relative to the last pre-treatment period.

A.6 DiD Estimation Around Treatment Assignment Cutoff in Bavaria

Figure A.8: DiD estimation around the treatment assignment cutoff
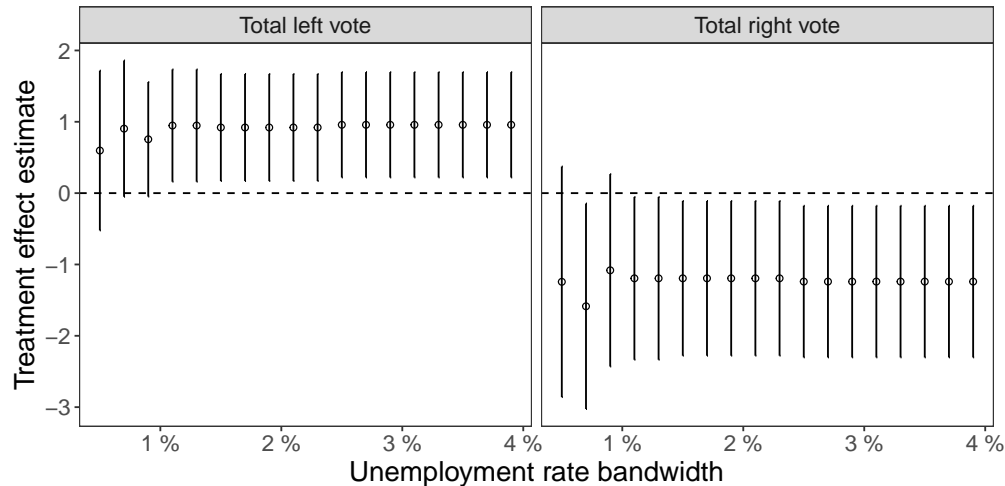
Note: The figure shows the results from two-period difference in differences models at varying bandwidths around the treatment assignment cutoff in Bavaria. The outcomes are the vote shares of parties in the 2013 and 2017 federal elections. The total vote share for right-wing parties includes the votes cast for the AfD. The sample includes all municipalities in Bavaria. The treatment is the labor market liberalization for refugees in August 2016. In Bavaria, employment agency district with an average unemployment rate of less than 3.6% in 2015 were treated. Employment Agency districts with a higher unemployment rate were exempted from the policy change. We present two-period treatment effect estimates for varying samples around the treatment assignment cutoff. The 1% bandwidth estimate, for example, contains all municipalities nested in employment agency districts with an unemployment rate between 2.6% and 4.6% in 2015. The error-bars indicate 95% confidence intervals. Standard errors are clustered at the employment agency district level. The confidence intervals are likely too narrow for small bandwidths, because we only have a small number of clusters in these restricted samples.

A.7 Employment Trends and Right-wing Voting

Table A.2: Effect of Employment on Right-wing voting Within Employment Agency Districts

	DV: Δ Total right vote
Native Employment Rate	0.568
	(1.386)
Employment Agency District FE	Yes
N	2442
R-squared	0.416

Notes: Estimates from OLS regression. The outcome is the change in the vote share for right-wing parties (including the AfD) between the federal elections 2017 and 2013, measured at the municipality-level. The main independent variable is the native full-time employment rate in 2016. This variable is measured as the share of individuals in full-time employment divided by the total population in the municipality. The sample consists of all municipalities in Bavaria and North Rhine-Westphalia. The model includes employment agency district fixed effects. ***p < .01; **p < .05; *p < .1

A.8 Effects on Individual Party Identification (SOEP)

Table A.3: Effects of labor market liberalization on party identification

	All respondents	Right-wing respondents		
	DV: Identifying with any left-wing party (0/1)			
	(1)	(2)	(3)	(4)
Liberalization	0.011*	0.017*	0.031*	0.005
	(0.006)	(0.009)	(0.018)	(0.007)
Sample	Full	Full	Worry ab. immigration	Don't worry ab. immigration
N	6648	3564	1608	1956

Notes: DiD estimates from two-period models for 2016 and 2017. The outcome is binary indicator of preferring any either any right-wing or any left-wing party. Standard errors are clustered at the employment agency district level. We subset the survey based on (1) stated party identification as well as (2) a survey item on worries about immigration to Germany. Both items were measured prior to the treatment in 2016. ***p < .01; **p < .05; *p < .1

A.9 Geographic RD Design

In the geographic RD design, we compare individuals in treated regions who live very close to individuals in regions where the labor market was not liberalized. We show the sample of treated and control counties that we retain after geographic distance matching in Figure A.10. We also control for individual-level background characteristics. These variables include age, gender, religious affiliation, marital status, and the educational background of respondents. The identification assumption is that, conditional on geographic proximity *and* covariates, the treatment assignment is independent of potential outcomes.

Figure A.9: Effect of Labor Market Liberalization on Attitudes towards Refugees
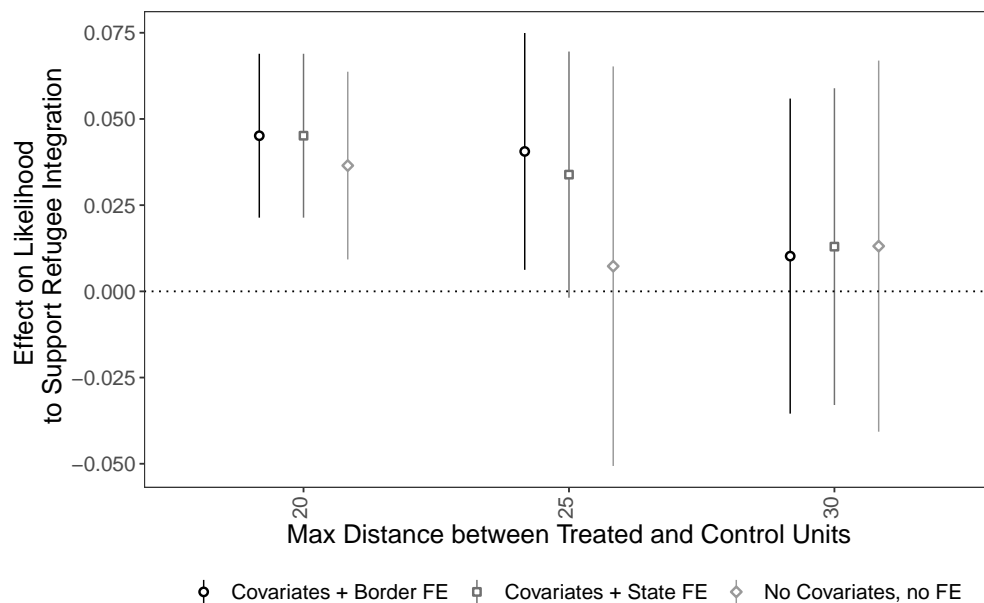
Note: Results from cross-sectional geographic RD OLS models. The y-axis shows the mean-difference in the likelihood to support refugee integration into the labor market between individuals in treated and control regions. We compare individuals living in regions that are geographically close but vary in terms of treatment status. Standard errors are clustered at the employment agency district levels.

Figure A.10: Counties in civey sample

Priority review still applies Priority review suspended
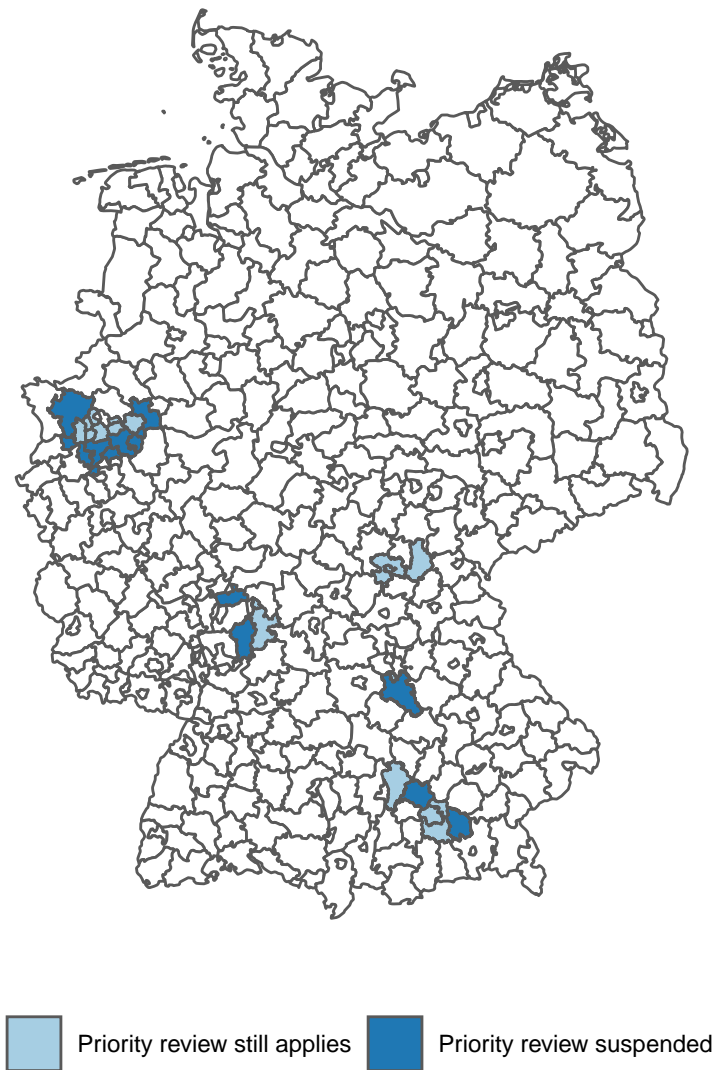
Note: The map shows the counties that were retained in the Civey sample. We pruned all respondents located in counties that are more than 25 kilometers away from a county of different treatment status.