Refugee Labor Market Access Increases Support for Migration

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Does the economic integration of refugees affect public opinion toward migration? On the one hand, providing refugees access to labor markets may increase competition, which may lead natives to oppose migration. On the other hand, the economic integration of refugees provides firms with human capital and also opens opportunities to build social rapport, which may lead natives to adopt more positive views toward migration. To make headway on the conflicting predictions, we take advantage of a natural experiment. In 2016, the German 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 percent. What is more, the policy also had a positive effect on natives' attitudes towards migration. Areas exposed to more refugees in the labor market were 2 percentage points less likely to vote for right-leaning parties across both state and federal elections. At the same time, left-leaning, pro-immigration parties gained significantly. Overall, the findings question sociotropic accounts of public opinion formation toward migration. Rather than viewing migrants as a threat, citizens react positively to refugees in the labor market.

Migration | Labor market | Populism | Refugees | Germany

Public support for migration varies starkly across industrialized democracies (1). What explains this variance? This paper hones in on the economic integration of refugees as a crucial predictor of public opinion toward migration. On the one hand, integrating refugees in the economy increases economic competition, which may lead natives to oppose migration (2, 3). This holds particularly true for low-skilled workers and in industries without strong unions. 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 migrants and natives work side by side, which may lead natives to espouse a more welcoming attitude toward migrants (4). Taken together, the effect of refuge 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 (5). Second, countries typically choose the degree of labor market access centrally. There is thus no institutional variation one could exploit. Third, the influx of migrants into any given local labor market is well-regulated and tends to unfold at low levels. Tracing the effect of 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 natural experiment that took place in Germany. Until 2016, refugees in Germany were only allowed to work if no native was available for the job. The policy was highly bureaucratic and cumbersome, and effectively prevented refugees from joining the work force. 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 year before meant that the policy had a real impact on local labor markets. Finally, refugees in Germany were 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 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) 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 survey evidence, we find that the policy had a *positive* effect on attitudes towards migrants. 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

Significance Statement

Does the economic integration of refugees affect public support for migration? We take advantage of a natural experiment in Germany where the Government recently significantly eased labor market access for refugees in 85% of employment agency districts. Using electoral and survey data, we show that the policy not only increased refugee employment, but also reduced the vote share of anti-immigration parties by 2 percentage points.

The authors contributed equally to this project.

The authors declare that they have no conflict of interest.

economic integration of refugees led citizens to espouse pro-immigration parties. Overall, the findings question sociotropic accounts of public opinion formation toward migration. Rather than viewing migrants as a threat, citizens react positively to refugees in the labor market.

1. Related literature

How does the economic integration of refugees affect natives' attitudes toward migration? The existing literature broadly delineates four causal channels that may explain how an influx of refugees into local labor markets can affect public attitudes toward migration. Importantly, the channels do not consistently point into the same direction, showcasing the need to adjudicate between them using rigorous empirical evidence.

Labor market competition The first channel linking the economic integration of refugees to natives' attitudes 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. The effect is expected to be particularly pronounced if native labor can more easily be substituted with immigrant labor (e.g., when jobs do not require language skills). Based on the factor proportion model, a variety of studies have hypothesized that natives, on average, are skeptical of immigration given the implied negative effects on the average native citizen (2, 6–10).

Welfare dependence A second channel linking the economic integration of refugees to natives' attitudes toward migration functions via the host population's expectation about immigrants' willingness to work. Specifically, the economic integration of migrants may have a positive effect on natives' attitudes because natives reward immigrants for working (11–13). If refugees are integrated into the labor market, they contribute toward 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 migrants 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 (14).

Intergroup contact A third channel that links the economic integration of refugees to natives' attitudes toward migration is intergroup contact. Migrants have been shown to settle in neighborhoods where other migrants with similar cultural backgrounds have already settled (15). Refugees, in particular, are often settled in housing that is shielded from the host population (more below). As a result, contact between refugees and natives is less pronounced than native-native contact. The economic integration of migrants can help mitigate this. If migrants join a local firm, contact with natives is facilitated. And such contact may help overcome deep-seated stereotypes (4), which may spread across natives' networks.

Cultural backlash A fourth, rivaling 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 (16, 17). 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 (18). Yet, the existing studies have not looked into the economic integration of refugees, which often includes individuals that 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.

2. Setting

To adjudicate between the opposing theoretical expectations how the economic integration of refugees affects public opinion toward migration, we draw on a natural experiment that took place in Germany. After a period of relatively low levels of immigration, Germany—like other parts of Europe—saw a stark increase in asylum requests beginning in 2014. In 2015 alone, the country received 1.3 million requests for asylum (19). Once in Germany, refugees are assigned to the sixteen federal states on the basis of a deterministic rule known as the Königstein key. The rule takes into consideration a federal state's population and its tax revenue. Once within a federal state, refugees are assigned to so-called intake facilities. Importantly, refugees are largely not allowed to relocate unless they have credible justification. What is more, most states do not allow refugees to travel to other states, including the two federal states we focus on.

In response to the 2015 refugee influx, the German Federal Government passed an integration law (*Integrationsgesetz*) 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.

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 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 for an open position had to obtain written approval from the Federal Employment Agency. The approval of the agency was subject to a 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.* If none of the alternative candidates were 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

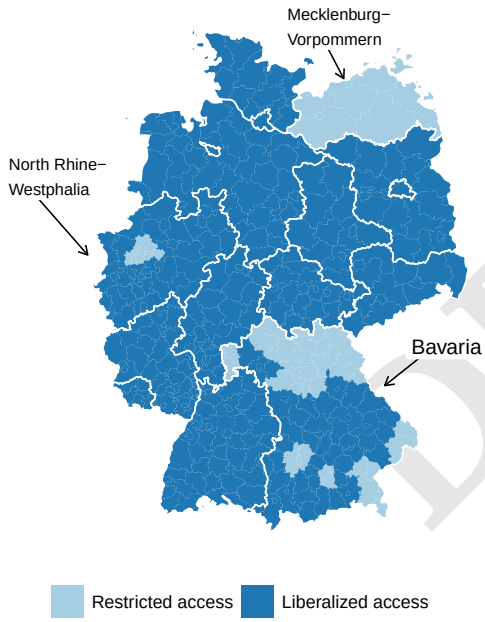
*Germans who receive unemployment benefits are generally required to present proof of unsuccessful job applications in order to keep receiving benefits. This policy applies also in cases where no refugees apply for a given position. The priority review is automatically lifted for refugees who have stayed in Germany for more than 15 months. It is also lifted after asylum application have been processed and approved. In 2017, the processing of asylum applications took 11 months on average.

disincentive for employers to hire refugees and was designed to facilitate refugees' integration into the German labor market.

In August 2016, the priority review was suspended in 133 out of 156 employment agency districts.[†] 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 implementation of the policy. In North Rhine-Westphalia, the Ruhr area region was exempted from the policy.

In Bavaria, the government chose a cutoff-based approach: employment agency districts with unemployment rates greater than the state average kept the priority review. Finally, in Mecklenburg-Vorpommern the policy was never implemented. To ensure the empirical validity of the results, we focus on within-state comparisons over time and avoid extrapolation across state boundaries. In what follows, the empirical analyses therefore focus on Bavaria and North Rhine-Westphalia. Reassuringly, the two states have a joint population of over 30 million people, while their combined GDP equals that of Spain.

Fig. 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.

3. Data

Refugee employment To examine the effect of the policy change on refugee employment, we obtained official data from the Federal Employment Agency. We observe the number of employed refugees in all German municipalities in every quarter between March 2008 and December 2018.[‡] The data captures both part-time and full-time employment. To ease interpretation, we scale the data by the total population of a given municipality in a given year.

Native employment and incomes To examine the effect of the policy change on natives' employment and wages, we gathered quarterly employment and wage data from the Federal Statistical Agency. Specifically, we observe the median monthly gross wage for 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 The primary political outcome is electoral behavior. We draw on municipality level voting data for the most recent federal and state elections before and after the policy change. 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. The key outcome of interest is the vote share of left- / right-leaning parties who favor / oppose migration, respectively (more below).

Party identification To supplant the electoral outcome and to assess mechanisms, we draw on individual-level panel data on residents' party identification from the German Socio-Economic Panel (see 20). 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 on residents' party identification. This variable captures whether respondents lean toward pro-immigration parties.

Attitudes Finally, to pick up broader changes in attitudes, we draw on a large-scale online survey conducted by the survey company *Civey* after the policy went into effect. The firm fielded a question from 2018 to 2019 to over 53,000 respondents, asking them whether they support or oppose the integration of refugees into the German labor market. The outcome variable is a binary indicator that equals one if a respondent supports the economic integration of refugees.

4. Empirical model

Identifying the causal effect of refugee labor market liberalization on political attitudes is difficult, 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 panel specification. The identification strategy rests on the assumption that treatment assignment is independent of *trends* in the outcome variables. Moreover,

[†] The Federal Employment Agency divides Germany into 156 agency districts, which form the agency's main organizational unit. Each agency district typically consist 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.

[‡] 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.

we focus on within-state comparisons over time, thus avoiding extrapolation across state boundaries.

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 framework:

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

Here, Y_{ijt} is 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 main parameters of interest are a series of 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 and allows us to causally identify the effect of the policy over time. The terms μ_i and δ_t denote municipality and quarter fixed 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:

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

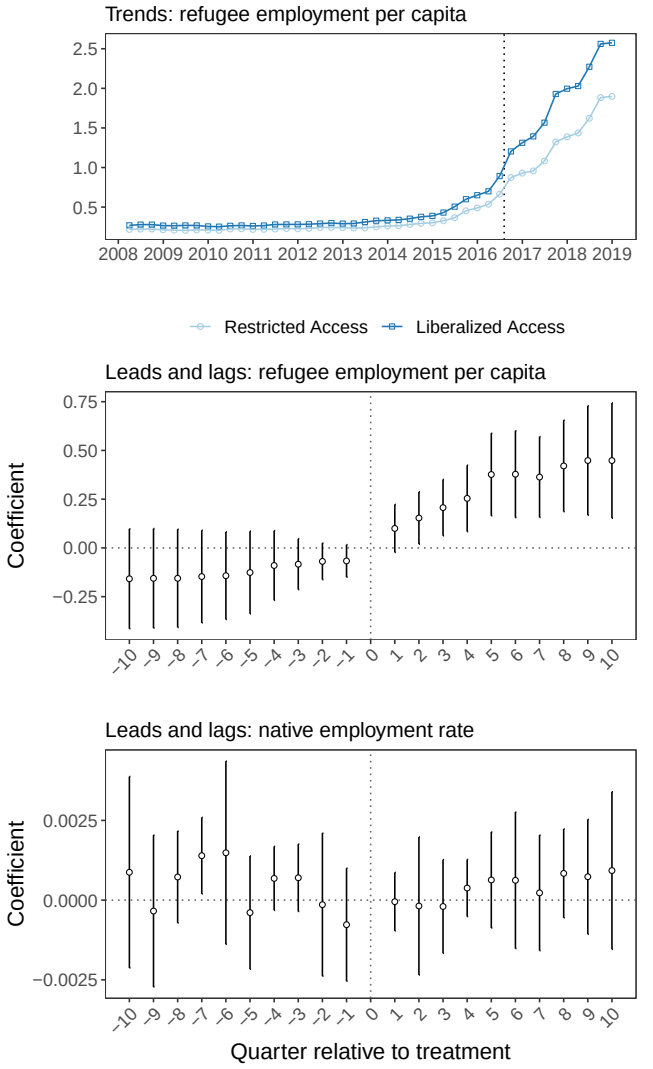
Here, Y_{ijt} is the election outcome for municipality i in agency district j in time period t. For treated districts where the labor market was liberalized after August 2016, $T_j = 1$ and $T_j = 0$ otherwise. Finally, $P_{it} = 1$ for municipality i when $t >$ August 2016. In all specifications, we cluster standard errors at the level of the agency district, the level of treatment assignment. 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.

Finally, to assess the effect of the labor market liberalization policy on pro-immigration attitudes, we use a geographic regression discontinuity design. Specifically, 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.8. 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. Table A.1 provides an overview of the main data sources.

5. Results

Employment In a first step, we demonstrate that the labor market liberalization policy substantially increased refugees' employment. We begin by graphically verifying the parallel trends assumption in the top panel of Figure 2. Prior to the policy change, refugee employment in treated and control municipalities evolved in parallel. Once the policy was implemented, refugee employment increased at a significantly faster rate in treated municipalities compared to the control group.

Fig. 2. Refugee Employment: Leads and Lags Estimation



Note: The top-panel shows the raw trends in refugee employment in treated and control districts between 2008 and 2018. The bottom two panels plot coefficients and 95 percent confidence intervals from leads and lags specifications as described in Section 4. Standard errors are clustered at the employment agency district level. The sample contains all municipalities in Bavaria and North Rhine-Westphalia.

In the center panel of Figure 2, we present results from a leads and lags specification (see Section 4), which confirms the descriptive pattern. The Figure shows statistically insignificant point estimates in all pre-treatment periods, confirming the parallel trends assumption that underpins the identification strategy. For the post-treatment periods, by contrast, we observe a divergence in refugee employment between treated and control areas. At the same time, there is no evidence that increased labor market participation by refugees reduced native employment. The bottom panel in Figure 2 estimates the aforementioned leads and lags specification using the total number of natives in full-time employment as the outcome. We estimate statistically insignificant point estimates for the

effect of the policy. Taken together, we find that the policy had a pronounced positive effect on refugee employment, while native employment remained unchanged.

Wages Did the labor market liberalization policy affect natives' wages? As mentioned above, one concern about the integration of migrants into local labor markets is that doing so may reduce wages of natives, particularly unskilled ones, which in turn may increase xenophobic attitudes. In Figure A.2, we use county-level panel data for the period 2014 – 2018, measuring the median monthly gross wages of native workers. Again estimating a leads and lags specification, we find no evidence that increased supply of refugee labor reduces natives' wages. The null finding holds regardless of workers' socio-demographic background. Even wages of low-skilled natives (e.g., those without any university or college education) are unaffected by the policy. Importantly, we should underscore 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.

Voting behavior Having established that the labor market liberalization increased refugee employment, we now turn to the political ramifications of the policy. The policy change went into effect 13 months prior to the 2017 federal election. In what follows, we aggregate Germany's major parties into left- and right-leaning.[§] We then estimate whether the policy affected the trends in the vote share of the two political camps. Evidence supporting the key identifying assumption—parallel trends—is provided in Figures A.5, A.6 and A.7.

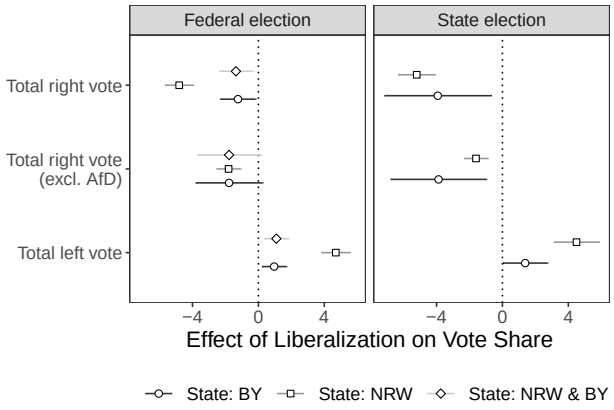
Results are presented in Figure 3. We find that the labor market liberalization policy reduced support for parties on the political right by two percentage points. The result is both detectable for the 2017 federal election as well as two state elections in Bavaria and North Rhine-Westphalia. Reassuringly, there is no evidence that the effect is driven by one of the two states alone. Both states show comparable effect sizes and estimates are statistically significant. We also note that the negative effect on right-wing parties' vote shares is present both when including or excluding the far-right Alternative for Germany.[¶] By the same token, we find that left-leaning parties experience a rise in support due to the labor market liberalization policy. Estimates range from 1.5 percentage points in federal elections to 4.0 percentage points in the state election in North Rhine-Westphalia. Taken together, the policy thus had a pronounced positive effect on left-leaning, pro-integration parties.

Finally, we address the possibility that regions with high and low unemployment were on different political trends regardless of the policy change. First, in Figure A.4 we show that the conclusions remain unchanged when we estimate the treatment effect in small bandwidths around the treatment assignment cutoff in Bavaria, where the policy change was conditional on the average unemployment rate in the labor market district. Second, we exploit variation in employment rates across municipalities nested within employment agency districts. In Table A.3, we show that local employment patterns are unrelated to trends in electoral support for right-wing parties.

[§] Specifically, we classify the Social Democratic Party, the Green Party and the Left Party as left-leaning, while the Christian Democratic Union, the Christian Social Union, the Free Democratic Party and the Alternative for Germany are classified as right (21).

[¶] We examine the results both including and excluding the AfD, since the AfD did not compete in the last state elections prior to the policy change. That is, AfD votes shares are not observed in some pre-treatment periods.

Fig. 3. Labor Market Liberalization and Electoral Behavior



Note: The Figure plots coefficients and 95 percent confidence intervals from two-period difference-in-differences models. The x-axis shows the estimated effect of labor market liberalization on the vote share of different party groups (in percentage points). We analyze elections for federal and state parliaments. Standard errors are clustered at the employment agency district level.

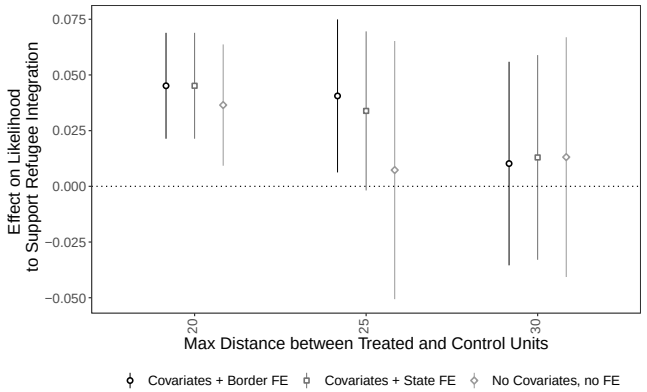
6. Mechanism

Why did the policy change voting behavior? More specifically, did the policy change voters' preferences or did it merely alter who turned out to vote? To adjudicate between both channels, we provide three pieces of evidence. First, Figure A.3 shows that the policy did *not* change turnout. This suggests that similar people went to vote across the treatment and control group, pointing toward a persuasion mechanism. If, by contrast, the policy had increased turnout, it would arguably be less convincing to infer a persuasion mechanism given that new people cast their ballot. That said, a null effect on turnout does not provide conclusive evidence for changed preferences because the policy may simply have changed who turns out to vote.

Second, to adjudicate between both channels with greater clarity, we turn to individual-level panel evidence, tracking people before and after the policy change. In Table A.2, 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. Table A.2 also subsets the data by respondents' prior attitudes toward immigration. Reassuringly, we find that the change in party identification is more pronounced among right-wing respondents who were worried about immigration prior to the labor market liberalization policy. This is sensible inasmuch as this subsample should i) be more receptive to the policy (given their interests), and ii) should have been positively surprised by the policy given the absence of any negative economic effects, despite widespread fears on the political right.

Third, we rely on additional local-level survey evidence to corroborate that the policy changed preferences. Using survey data from *Civey* coupled with a geographic regression discontinuity design, Figure 4 shows that natives' attitudes

Fig. 4. 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.

towards refugees became significantly more positive in regions where the labor market was liberalized. Importantly, since we only analyze residents living in geographically close regions with differing treatment statuses, the design assuages concerns about endogeneity. For the lowest reported distance of 20km, we find that liberalizing the labor market increased the likelihood of supporting refugee integration by between three and five percentage points. The result holds regardless of whether control variables, state fixed effects or border region fixed effects are included. Interestingly, the Figure shows that the effect size decreases as we increase the maximum permitted distance between treated and control areas. Put differently, we observe the strongest effects in the specification with the smallest bandwidth, where the treatment assignment is least likely to be subject to confounding.

7. Incumbency

Before concluding, we briefly discuss whether the treatment effect is a result of voters punishing the incumbent. At first glance, the results could, indeed, be interpreted as simple electoral punishment: We observe that right-wing parties are negatively affected by the labor market liberalization. This could suggest that voters punish Angela Merkel's ruling CDU/CSU party, which accounts for a large share of right-wing votes, for implementing a policy that constituents reject. Upon closer inspection, however, this argument has no empirical bearing. We find that the labor market liberalization policy *increased* support for parties to the left of the CDU/CSU. If constituents disliked the policy and punished the sitting government, it seems implausible that they would do so by electing parties that favor *more* immigration.

8. Conclusion

How does the economic integration of refugees affect public opinion toward migration? This paper made use of a natural experiment 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. Our study confirms that natives reward refugees for joining the labor force. Interestingly, we also show that increasing refugee employment has no effect on natives' wages. This suggests 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 incumbent parties, the center-right CDU and CSU, were punished for liberalizing labor market access offers one explanation for why conservative parties oppose pro-integration policies despite positive effects. In Germany, the pro-integration policy had a positive effect on refugee employment. Fears that increased refugee employment would lower incomes did not materialize. Despite these positive effects, however, conservative parties were punished for the policy at the voting booth. This finding implies that 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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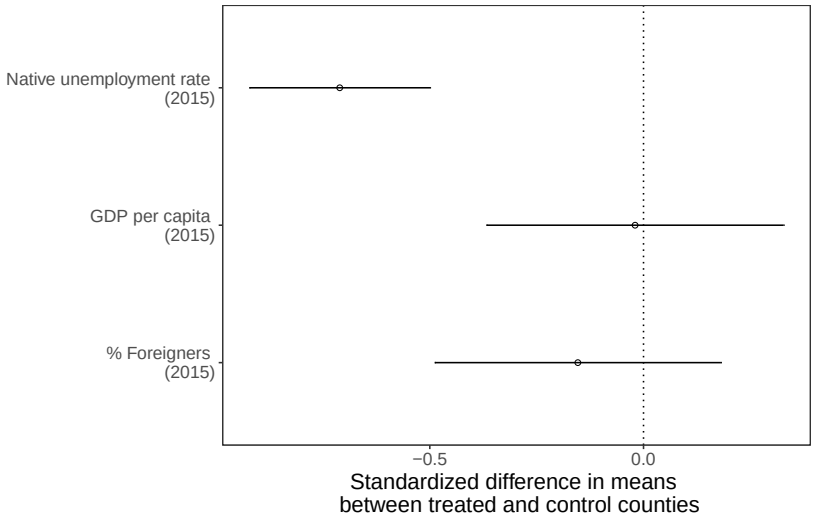
A. Supporting Information (Online Only)

Table A.1. Summary Statistics

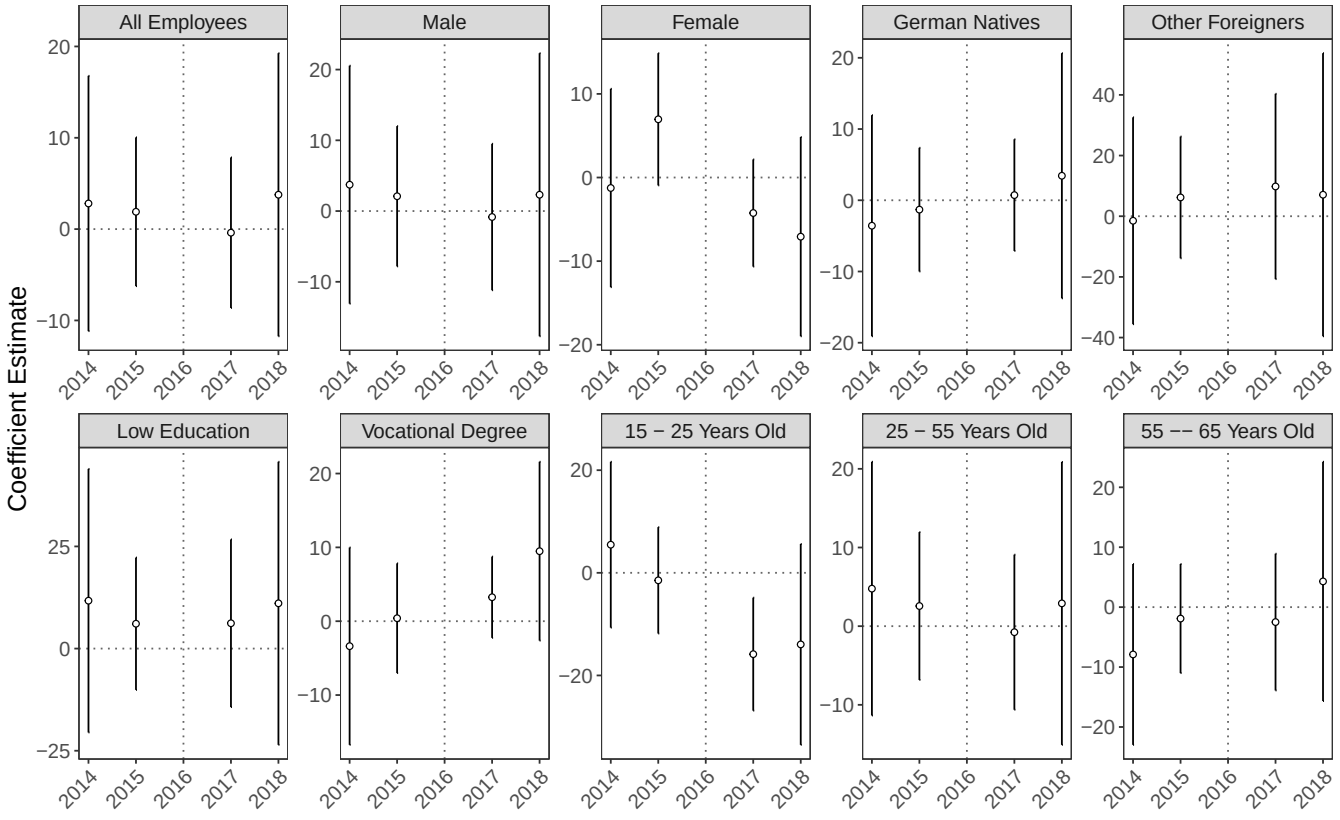
Variable	Dataset	Level of Analysis	Time-period	Subset	Mean	S.D.	N	Min	Max
Native Employment Rate	Employment Data	Municipality	2008 - 2018	Bavaria + NRW	0.46	0.04	107888	0.20	0.83
Refugee Employment per 1,000 capita	Employment Data	Municipality	2008 - 2018	Bavaria + NRW	0.64	1.13	107888	0.00	16.37
Monthly 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

Fig. A.1. Covariate balance: treated and control counties



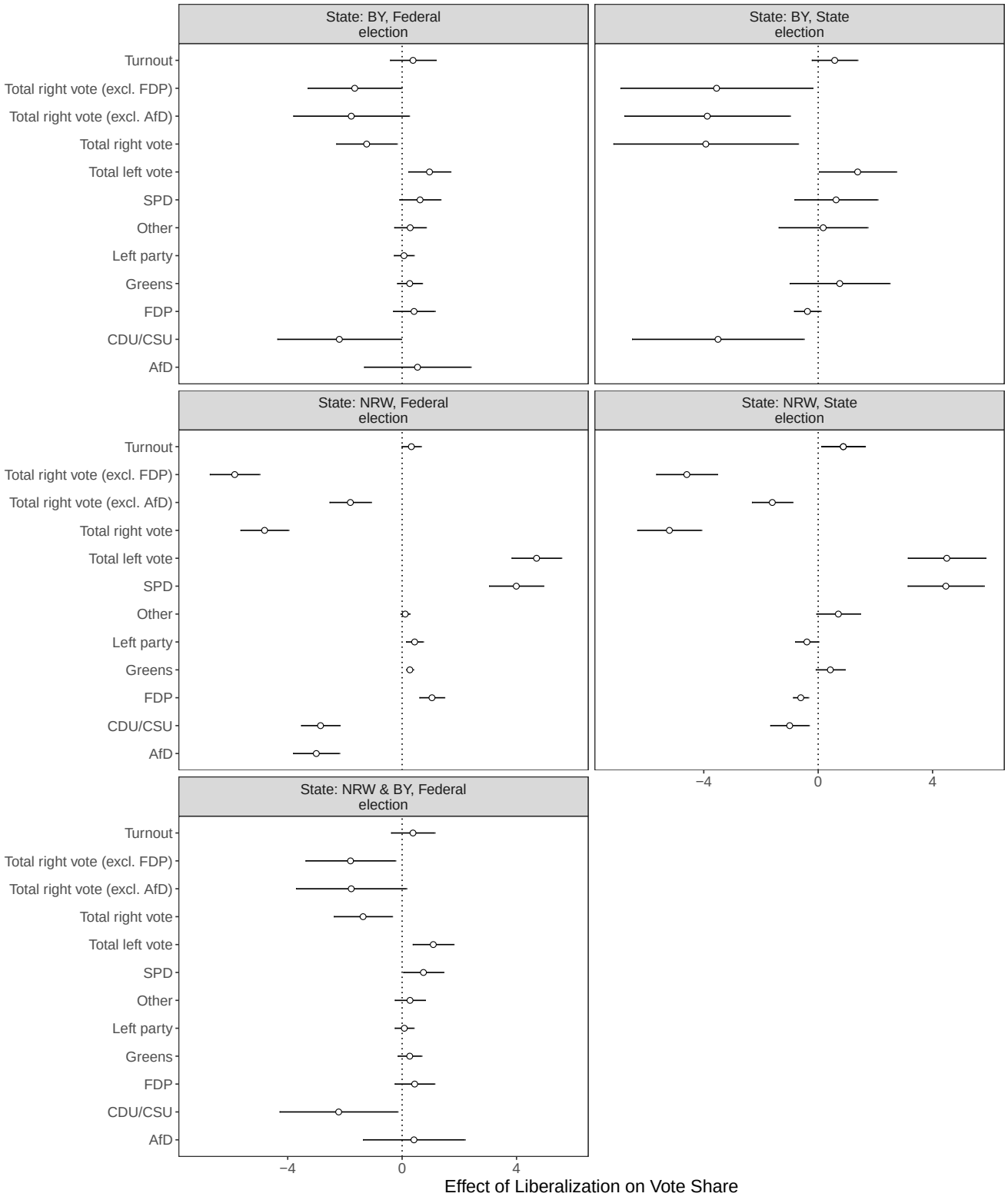
Note: The Figure shows the pre-treatement covariate balance between treated and control counties as of December 2015. We examine three covariates: the native unemployment rate, GDP per capita, and the local foreigner share in the population. The sample contains all counties in Bavaria and North Rhine-Westphalia.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

Fig. A.2. 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.

Fig. A.3. Labor Market Liberalization 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.

Table A.2. Effects of labor market liberalization on party identification

	DV: Identifying with any left-wing party (0/1)			
	All respondents	Right-wing respondents		
	(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

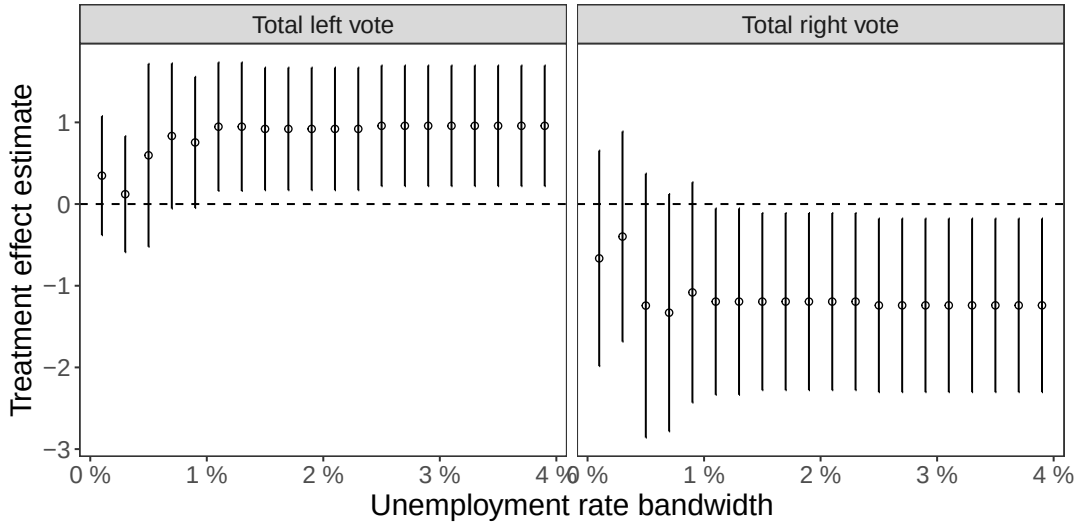
Notes: DiD estimates from two-period models for 2016 and 2017. The outcome is binary indicator of preferring any either any right-wing or any left-wing party. Standard errors are clustered at the employment agency district level. We subset the survey based on (1) stated party identification as well as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

Table A.3. Effect of employment on right-wing voting within employment agency districts

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

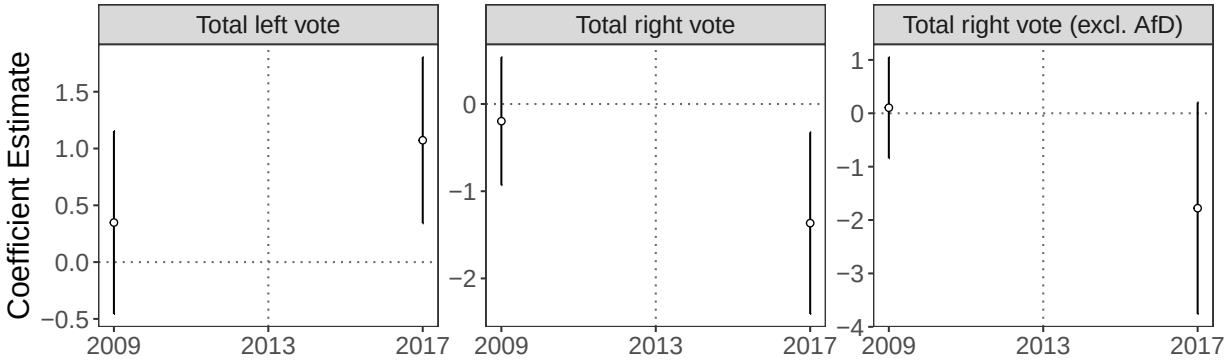
Notes: Estimates from OLS regression. The outcome is the change in the vote share for right-wing parties (including the AfD) between the federal elections 2017 and 2013, measured at the municipality-level. The main independent variable is the native full-time employment rate in 2016. This variable is 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. $^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Fig. A.4. DiD estimation around the treatment assignment cutoff



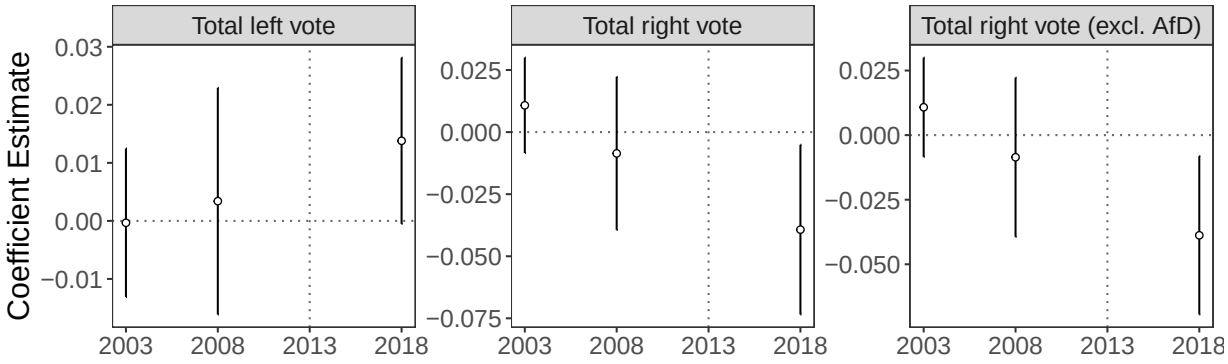
Note: The figure shows the results from two-period difference in differences models at varying bandwidths around the treatment assignment cutoff in Bavaria. The outcomes are the vote shares of parties in the 2013 and 2017 federal elections. The total vote share for right-wing parties includes the votes 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.

Fig. A.5. Leads and lags: federal elections



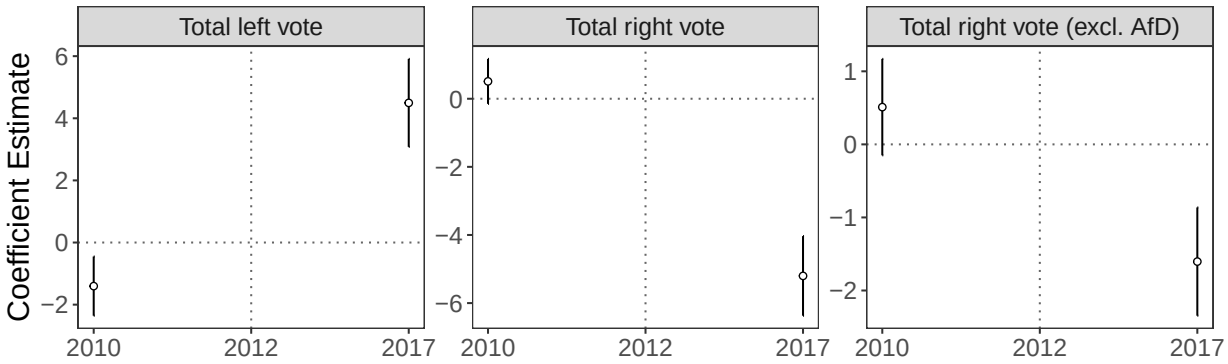
Note: Results from leads and lags analysis for federal elections between 2009 and 2017. We estimate the same generalized difference-in-differences specification as described in Section 4. We have two pre-treatment periods (2009 and 2013) and one post-treatment period (2017). The sample includes all municipalities in Bavaria and North Rhine-Westphalia.

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



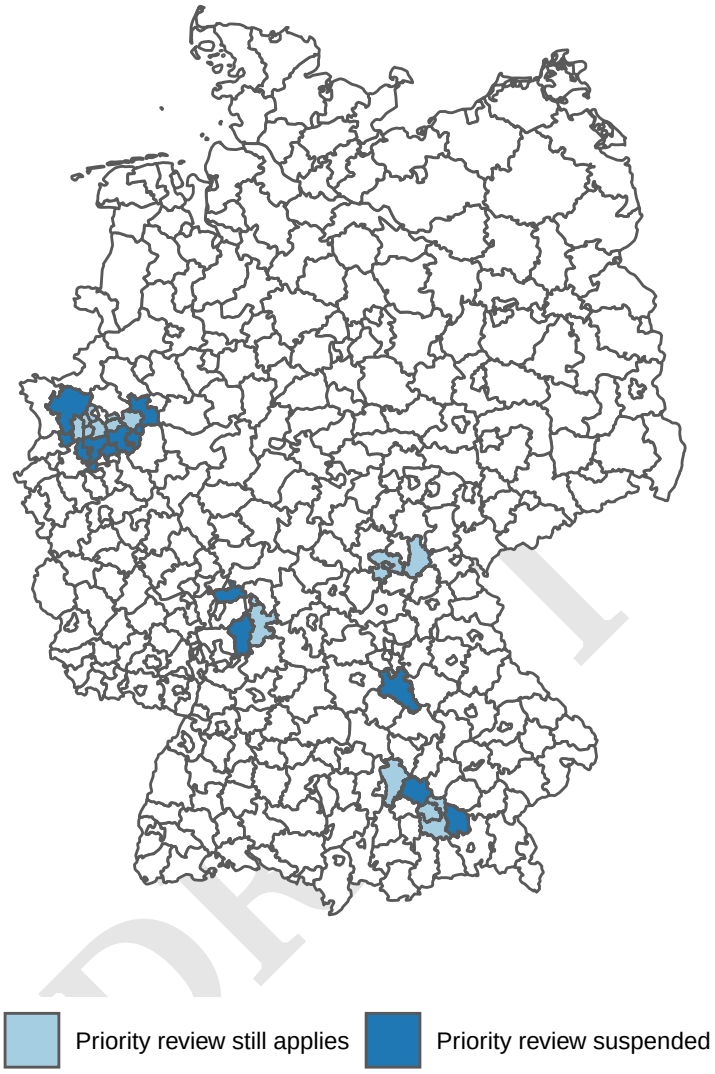
Note: Results from leads and lags analysis for state elections in Bavaria between 2003 and 2018. We estimate the same generalized difference-in-differences specification as described in Section 4. We have three pre-treatment periods (2003, 2008, and 2013) and one post-treatment period (2018). The sample includes all municipalities in Bavaria.

Fig. 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. We have two pre-treatment periods (2010 and 2012) and one post-treatment period (2017). The sample includes all municipalities in North Rhine-Westphalia.

Fig. A.8. Counties in civey sample



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.