Freedom of Movement Restrictions Inhibit the Social Integration of Refugees *

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Abstract

How do freedom of movement restrictions affect refugee integration in destination countries? While a growing body of research studies the initial allocation of refugees, there is little causal evidence on subsequent policies that affect refugee location decisions. We study a contentious law in Germany, which barred newly arrived refugees from relocating to a location different from the one they were assigned to. Crucially, the law was only applied to asylum applications approved after January 1th, 2016. We utilize the resulting treatment assignment cutoff to identify the causal effect of movement restrictions on refugee integration. We demonstrate that movement restrictions had pronounced negative effects on refugees' perceived sense of belonging in Germany, while increasing identification with their home countries. In addition, the policy decreased participation in a variety of social activities, but had no detectable effects on contact with natives or co-ethnics. We argue that detrimental effects of the policy stem from the fact that discriminatory policies send a negative signal about the inclusiveness of the host society.

1 Introduction

As the number of forcibly displaced people approaches one percent of the world population (Reuters 2020), the integration of refugees has become a core challenge for governments in destination countries. A crucial first step in this process is the spatial allocation of refugees in the receiving country, which has received considerable scholarly attention (see e.g. Edin, Fredriksson and Åslund 2003; Åslund and Rooth 2007; Damm 2009b; Auer 2018; Bratsberg et al. 2020). However, less is known about integration policies that affect the geographic distribution of refugees beyond the initial allocation. Several national governments either outright ban refugees from relocating, or penalize refugees from moving away from their assigned location.[1] We study the case of Germany, where a 2016 law forced refugees to remain at the location they were assigned to upon arrival in the country. Refugees are only subject to the restriction if their asylum application was approved after January 1$^{\text{st}}$ 2016, allowing us to compare otherwise similar refugees around this date cutoff.

Proponents of the restrictions have argued that allowing refugees to move freely will induce the formation of ethnic enclaves in large urban areas. Such enclaves will inhibit integration, language acquisition, and contact between refugees and natives, while straining urban real estate markets. In contrast, opponents of the policy claim that movement restrictions violate basic civil liberties and human rights. What is more, movement restrictions may hinder refugee integration by making access to co-ethnic networks more difficult. Prior research suggests that such co-ethnic networks can facilitate the integration of refugees into local labor markets (Damm 2009a,b; Edin, Fredriksson and Åslund 2003). Finally, an emerging line of research shows that restrictive policies have effects beyond economic integration, affecting identity, sense of belonging and perceived discrimination (Abdelgadir and Fouka

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[1]Examples of countries that entirely ban relocating are Germany and Switzerland (Martén, Hainmueller and Hangartner 2019). Examples of countries that withhold services or penalize refugees for relocating are France (Asylum information database 2018) and the Netherlands (UNHCR 2018).

2020; Fouka 2019). Assessing the legality of movement restrictions, the EU Court of Justice ruled in 2016 that movement restrictions are permissible only if they facilitate the integration of refugees (*CJEU C-443/14* 2016). Evaluating the impact of movement restrictions is therefore not only important for the design of effective integration policies, but also matters for their compliance with international law.

To causally identify the effects of movement restrictions, we utilize the fact that restrictions were retroactively applied based on a sharp date cutoff. A key feature of the policy is that it only affects refugees whose asylum applications were approved after January 1st, 2016. The law therefore created a discontinuity in the probability of being restricted. As a result, we can compare otherwise similar refugees on either side of this sharp cutoff, enabling us to hold constant potential confounders. We implement this strategy through an exact matching algorithm. Measuring outcomes either one or two years after the policy was implemented, we compare restricted and unrestricted refugees within a small bandwidth around the treatment assignment cutoff. Importantly, we also match on the date of entry into the country, ensuring that we are comparing refugees who have had a similar amount of time to settle in Germany.

Our results show that the majority of affected refugees express the desire to move if they were permitted to do so. Yet, we find that exempted refugees are not more likely to relocate in between successive survey waves. We then show that that movement restrictions make refugees markedly more pessimistic about their employment prospects in Germany. Turning to measures of identity and belonging, we find pronounced negative effects of the movement restriction policy. Affected refugees feel less welcome in Germany and identify more strongly with their home countries. Relatedly, we observe that participation in a variety of social activities decreased as a result of movement restrictions. We do, however, not observe that restrictions affected the frequency of contact with natives, co-ethnics, or other immigrants. This aligns with our finding that most refugees do not relocate, regardless of legal restrictions. Our results call into question the validity of the empirical claims that

are commonly used in the public discourse to justify the restrictive refugee policies. Finally, we do not observe negative effects on employment. In fact, there is suggestive evidence that restricted refugees are more likely to be employed about two years after the restrictions were enacted. Feasibly, the effects on employment could be an unintended consequence of the policy: refugees might be incentivized to seek employment, since getting a job exempts refugees from the restrictions.

We demonstrate that movement restrictions can have detrimental effects on the social and economic integration of refugees. Going beyond effects on employment or contact, we show that restrictive policies induce alienation from the host society and withdrawal from social life. We argue that restrictive policies constitute a strong negative signal about the inclusiveness of the host society, a possibility that received scant attention when the policy was first discussed in the German parliament. What is more, movement restrictions induce pessimism about economic prospects. Our results add to a growing literature showing that restrictive policies negatively affect immigrants after they have settled in the destination country (Fouka 2019; Abdelgadir and Fouka 2020). We argue that policymakers must therefore balance the goal of equally distributing refugees against potential detrimental effects on refugee integration.

2 Background

Spatial dispersion policies build on the assumption that, in the absence of restrictions, immigrants would sort into 'ethnic enclaves' – residential areas with a high concentration of immigrants from the same region of origin. This derives from the general concept of social homophily – the observation that individuals have a preference for being around others who are similar to themselves (McPherson, Smith-Lovin and Cook 2001). Following this view, immigrants might prefer to live among co-ethnics that they can easily communicate and cooperate with (Cutler, Glaeser and Vigdor 2008a; Habyarimana et al. 2007).

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Prior research has yielded mixed results on the effects of ethnic segregation on immigrant integration. On the one hand, ethnic communities reduce the cost of maintaining a separate identity and might thereby slow down the cultural assimilation of immigrants. Lazear (1999) for example argues that individuals from minority groups are more likely to adopt the culture and language of the majority when the minority group accounts for a small proportion of the total population (see also Cutler, Glaeser and Vigdor 2008b). When immigrants rarely come into contact with natives, they may retain group-specific characteristics for a longer period of time. As ethnic enclaves provide immigrants access to social networks and economic opportunities independent of the host country's norms and language, they might reduce incentives to assimilate.

At the same time, ethnic enclaves can have sizable positive effects on the economic integration of refugees. Drawing on the example of Cuban refugees in the US, Portes and Jensen (1989) show that refugees can benefit substantially from participating in the 'enclave economy' - a cluster of small businesses run by co-ethnics. Damm (2009b, 2014) presents similar evidence from the Danish context, where ethnic networks facilitated the labor market integration of refugees by disseminating job information and improving the job-worker match quality (see also Beaman 2012). Edin, Fredriksson and Åslund (2003) argue that residing in an ethnic enclave can be particularly beneficial for low-skilled refugees, who might otherwise have difficulties integrating into the host country's labor market.

Finally, movement restrictions constitute an intrusion into a fundamental liberty, i.e the right to freely choose one's place residence within a country. The movement restriction applies only to refugees, potentially sending a strong negative signal about the inclusiveness of the host society. This might add to the precarious state of well-being and mental health of many refugees after fleeing from war and persecution (Fazel, Wheeler and Danesh 2005). Porte and Torney-Purta (1987) show that institutional features of the asylum process, including barriers to work and social services, can induce severe stress among refugees. Restrictive policies may compound the stressors affecting an already traumatized group.

Going bezong economic integration, discriminatory policies have been shown to negatively affect immigrant integration in a variety of contexts. Abdelgadir and Fouka (2020) show that a national headscarf ban in France reduced the secondary educational attainment of Muslim girls, increased perceptions of discrimination, and overall reduced Muslim girls' sense of belonging in French society. Fouka (2019) demonstrates similar effects for discriminatory language bans against Germans in the context of World War I. Prohibitions on German language instruction in US schools stifled assimilation among the German minority and increased its sense of a separate cultural identity. Inclusionary policies such as birthright citizenship, on the other hand, may boost integration outcomes. Avitabile, Clots-Figueras and Masella (2013) for example use exogenous variation provided by the 1999 reform of the German nationality law to show that foreign-born parents are most likely to interact with the local community and use the German language if their children are entitled to German citizenship at birth.

3   Movement restrictions in Germany

Beginning in 2015, Germany experienced an unprecedented inflow of refugees. Refugees now constitute more than two percent of the country's population. To facilitate the economic and cultural integration of refugees into German society, the federal government passed the wide-ranging *Integrationsgesetz* ('integration law') in August 2016. Among other provisions, the law introduced mandatory movement restrictions for refugees in Germany.[2] The policy was enacted to counter the expected concentration of refugees in large urban areas, a process that was claimed to (1) hinder integration and (2) put a greater fiscal burden on affected areas.

After arriving in Germany, refugees are allocated to one of the 16 federal states in propor-

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[2]The *Integrationsgestz* contained modifications to §12a of the *Aufenthaltsgesetz*, which already mandated movement restrictions for refugees prior to asylum approval.

tion to a state's population and per capita GDP. With few exceptions, states then allocate refugees to counties, aiming for similar proportions of refugees across all counties. This assignment process is random – the location where refugees are placed is independent of characteristics such as as education, gender, or age. Therefore, we would not expect that different types of refugees are more likely to be placed in, for example, urban areas than in others. During this process, refugees live in different types of housing, and are generally not permitted to move.[3] Once assigned to a county, the local government has to provide housing. This mainly takes the form of shared accommodations. A small share of refugees are housed in private accommodations, e.g. for health reasons. If granted asylum, refugees can then freely choose an accommodation, which is paid for by local governments if the refugee has no own source of income. Local governments are obliged to pay for accommodations that offer a minimum standard of living, a loose definition that varies throughout the country.

The August 2016 movement restriction requires approved refugees to reside in the state that they were initially assigned to for a period of three years. Seven large states, accounting for 73% of Germany's population, further require that refugees must live in the specific county that they were assigned to (Renner 2018). While there is variance in the implementation of the policy across states, we stress that the initial assignment of refugees to states is independent of the individual background characteristics of refugees. Finally, we note that the policy prevents refugees from freely choosing their *place of residence* but does not prohibit them from traveling within the country.

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[3]We note that regulations that govern location decisions while asylum applications are processed differ from the movement restriction that we discuss, which only applies to refugees after their asylum applications were approved.

4 Empirical Strategy

4.1 Data

Our main data source is the *IAB-BAMF-SOEP Survey of Refugees in Germany*, a panel of about 5,000 refugees surveyed in 2016 (mostly prior to the enactment of the policy) as well as 2017 and 2018 (after the enactment of the policy). The IAB-BAMF-SOEP Survey (henceforth *SOEP*) was aimed at providing a broadly representative sample of the refugee population, and therefore includes refugees from a variety of national and demographic backgrounds. In addition to basic demographics, asylum status and labor market indicators, the survey contains a wide range of items relating to the economic, cultural, and social integration of refugees in Germany. Based on prior work on multiple dimensions of immigrant integration (Koopmans 2013), we select relevant integration outcomes and group them into four categories: (1) contact with natives and other immigrants, (2) perceptions of belonging and identification with the home country, (3) social engagement and (4) employment. Most outcomes are measured twice after the policy was enacted, in 2017 and 2018. Two outcomes are only measured in one of those years, which we discuss further in section 5. We provide summary statistics on all variables in tables A.1 and A.2. In table A.3, we list the survey questions corresponding to the outcomes used in our main analysis. For data protection reasons, we currently have no information on the county or city where refugees reside.

4.2 Identification

Refugees are subject to the movement restrictions if their asylum application was approved after January 1st, 2016. To estimate the causal effect of movement restrictions on integration, we compare otherwise similar refugees on both sides of the approval date cutoff. Importantly, we do not estimate the effect of relocating on integration, but rather the effect of being subject

to the movement restriction.

The SOEP data allows us to reconstruct whether refugees were subject to the movement restrictions. We observe the year and month in which asylum applications were decided on. Consequently, we can compare refugees who are similar with respect to background characteristics and arrival date, but whose asylum applications were approved right before or after the treatment assignment cutoff. Our key identification assumption is that, within blocks of refugees defined by background characteristics and arrival date, the asylum application approval date is independent of refugees' potential integration outcomes. Crucially, refugees and bureaucrats were unaware of the policy assignment cutoff when decisions were made. The policy and the assignment cutoff were introduced in August 2016, obviating concerns about sorting around the assignment cutoff.[4]

We use a matching design to implement our identification strategy. We first subset our sample to applications approved between November 2015 and February 2016, resulting in a two-month bandwidth around the assignment cutoff.[5] We then match each treated unit to all control units with *exactly* the same covariate values. Our covariates are age (discretized), country of origin, gender, education, and the quarter-year of arrival in Germany. We form blocks of treated and control units close to the treatment assignment cutoff, such that within each block, all units (treatment and control) have the same covariate values. By design, covariate balance is perfect within blocks. Across blocks, the treated and control group are similarly balanced on all relevant covariates (see figure A.1). After matching, we estimate a

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[4]We note that the policy was already discussed in March 2016, which may have induced refugees to relocate in anticipation of the impending ban. This would pose a problem if there had been differential behavioral responses depending on approval date. However, the January 1st, 2016 approval date cutoff was only announced about two months prior to August 2016, giving us little reason to believe that differential behavioral responses are an issue (Caspari 2016). In addition, the policy was applied retroactively, meaning that refugees had to return to their asignment location if they relocated prior to August 2016.

[5]In section 5, we verify that our results are robust to bandwidths as large as six months and as small as one month.

series of OLS models with block fixed effects of the following form:

$$y_{i,j} = \alpha + \tau D_i + \sum_{j=2}^{M} \beta_j B_{i,j} + \epsilon_i$$

Here, $y_{i,j}$ is the outcome variable observed for individual i nested in covariate-block j. D_i is a binary indicator for the treatment assignment and equals one if a refugee's application was approved January 1st, 2016. We estimate a total of $M = 47$ (2017) or $M = 45$ (2018) block fixed effects in the case of a two-month bandwidth, such that $B_{i,j} = 1$ if individual i is a member of covariate-block j. The key parameter of interest is τ, the effect of the movement restrictions. We use heteroskedasticity-robust standard-errors for all of our analyses.

We note two additional points. First, exemptions from the policy can be granted if refugees take up employment, vocational training, or distant tertiary education. In our case, employment and education are textbook examples of collider variables. We therefore do not condition on these characteristics to avoid post-treatment bias (Elwert and Winship 2014). We are in essence estimating the intent-to-treat effect of the movement restriction, as compliance with the policy is likely not perfect. However, noncompliance is a rare phenomenon. Only 3.2% of the refugees in our 2016 sample were in employment that made them eligible for an exemption. We hence expect to slightly underestimate the complier average causal effect.

Second, faster asylum decisions may correlate with unobserved individual or institutional characteristics. These could, in turn, affect integration, leading to biased estimates. We present two pieces of evidence that suggest that this is not the case. Our design requires balance on relevant demographic background variables. In section 5.1, we additionally show that four-month differences in approval speed do generally not correlate with integration outcomes.

Figure 1: Effect of movement restrictions on integration
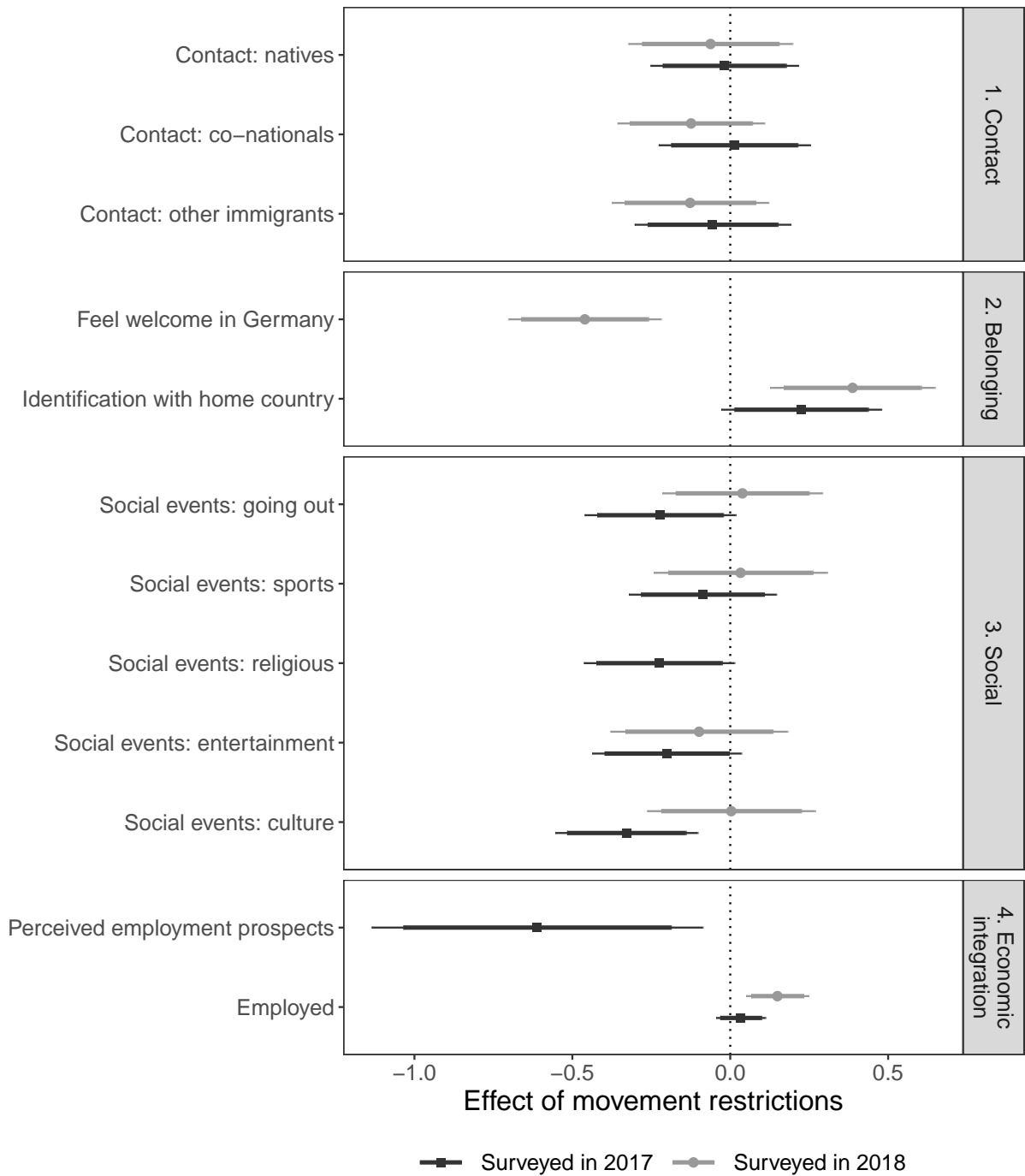
Note: The figure shows estimated effects of being subject to movement restrictions on the outcomes listed on the left-hand side. The horizontal bars represent 90% (thick lines) / 95% (thin lines) confidence intervals. All variables are standardized except employment status, which is binary. The sample is based on a two-month bandwidth around the Jan 1, 2016 cutoff. More details are given in table A.5 in the SI.

5 Results

Before turning to the main results, we describe intended and realized refugee relocation decisions. Based on a SOEP item, more than 65% of refugees whose movement is restricted under the policy indicate that they would relocate within Germany if they were allowed to.[6] In figure A.2 in the SI, we however demonstrate that there is a divergence between the stated intentions of refugees and observed behavior. Our survey data allows us to analyze whether the refugees in our panel have moved within Germany between survey waves.[7] Only a minority of refugees move, and we are unable to detect a discontinuity around the treatment assignment cutoff. This finding could stem from the fact that all refugees are prohibited from relocating until their asylum application is approved. Refugees may therefore have become accustomed to life at the initial assignment location, making relocation more costly.

In Figure 1, we present the effects of the relocation ban on the main integration outcomes. We show estimates from the baseline model, separately for 2017 and 2018. As stated previously, not all survey items were asked in both years. We begin by examining the effects on contact with German natives or other immigrants. We observe point estimates close to zero on all outcomes, suggesting that movement restrictions had little effect on contact with either natives, co-ethnics or other immigrants. Moving to the two items that measure refugees' sense of belonging in Germany, we find that the policy led to a 0.5 standard deviation decrease in the degree to which refugees feel welcome in Germany. Conversely, the strength of identification with their respective home countries increased substantially. These effects are already present in 2017 but persist into the year 2018, two years after the policy was enacted.

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[6]This counts all refugees who put the probability that they would move if they were allowed to at least at 50%.

[7]More specifically, the survey item asks respondents whether they reside at the same address as in the previous survey wave. We are not able to distinguish between movement across or within counties in this analysis.

Regarding social integration, we observe similar negative effects across all five items in 2017, demonstrating that refugees are less engaged in social life in general. These effects, however, are not observed in 2018. This suggests that the main effect of the movement restriction policy was to slow down the social integration of refugees.

Finally, we turn to labor market integration. We first show that, after the policy was enacted in 2017, affected refugees were much more pessimistic about their labor market prospects. Comparing perceptions with observed outcomes, we find no difference in employment in 2017, while employment among affected refugees is about 15 percentage points higher in 2018. Possibly, the restrictive policy incentivized refugees to seek employment, as this can gain them an exemption to the movement restriction (see section 4.2).

5.1   Robustness

Our identification strategy requires that the exact timing of approval decisions around the January 1st cutoff is uncorrelated with unobserved individual or institutional characteristics. However, faster decisions may correlate with more efficient local institutions, which in turn may affect integration. In addition, individual characteristics could affect how quickly decisions are made, although we are unaware of any systematic or anecdotal evidence that this is the case. We also emphasize that 90% of refugees in our sample are Syrians, for whom asylum approval was virtually guaranteed during the study period.

We conduct two supplementary analysis to verify that asylum decision speed is not correlated with integration outcomes independent of its effect on movement restrictions. First, we use the full sample of all refugees and regress the integration outcomes on the duration between the date of arrival and the date of approval. Akin to the main specification, we compare refugees whose asylum applications were approved within four-month intervals. We estimate OLS models with block fixed effects, separately for 2017 and 2018. The blocks are defined by the base covariates as well as the application decision time in intervals of four

months. As a result, we compare refugees with similar background characteristics whose asylum applications were processed at most four months apart. similar to the two-month bandwidth used in the main analysis. In figure A.10 in the SI, we demonstrate that slightly faster asylum decisions (i.e. at most four months difference) do generally not correlate with integration outcomes.

In a second step, we utilize the panel structure of the SOEP to test for pre-treatment differences between restricted and unrestricted refugees. In doing so, we re-estimate the main specification in figure 1 using outcomes measured in 2016, before movement restrictions were introduced.[8] If approval decision timing rather than movement restrictions affects integration, we would expect to find significant differences in this analysis. Reassuringly, we do not find significant differences between restricted and unrestricted refugees before the policy took effect (see figure A.9 in the SI). Taken together, the two supplemental analysis provide strong evidence that slightly faster asylum decisions do not directly affect integration, other than through their effect on movement restrictions.

We conduct three additional robustness checks to ensure that our results are not driven by the choice of specification, covariates or unobserved confounding. First, we present all main results without covariate block fixed effects (see figure A.4 in the SI). We find that our results remain unchanged when compared to figure 1. Second, we present the results for varying bandwidths around the January 1st cutoff, using the same matching procedure as described in section 4. In figures A.5 and A.6 in the SI, we show that our results are largely robust to varying the bandwidth. We observe that effects are generally stronger for smaller bandwidths, where the assumption of no unobserved confounding is more likely to hold. Third, we conduct randomization inference by creating 1,000 random permutations

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[8]We can only conduct this analysis for a subset of outcomes, as some items were only included in later waves of the survey. The movement restriction went into effect in August 2016. We consider the year 2016 pre-treatment as most SOEP-interviews are conducted over the summer. In addition, the implementation of the law within the states was often delayed by a several months.

of the treatment assignment vector within covariate blocks. We then re-estimate the results presented in figure 1. In figures A.7 and A.8, we present the resulting distributions of the t-statistic for the treatment effect estimates across 1,000 permutations. We show the corresponding two-sided p-values in table A.5 in the SI. We find that the resulting p-values are similar to the ones obtained from the base specifications.

6   Conclusion

Do movement restrictions benefit or hinder refugee integration? In this paper, we provide causal estimates of the effect of domestic movement restrictions in the wake of the 2015 refugee crisis in Germany. Utilizing a date cutoff that determines whether refugees are subject to the policy, we document that movement restrictions had negative effects on refugees' sense of belonging as well as their social engagement. We find no effects on contact with natives or co-ethnics. Finally, we show that restrictions initially induced pessimism about employment prospects, but then led to higher employment rates among affected refugees. Taken together, we observe that movement restrictions negatively affect integration, especially with respect to social engagement and belonging. We argue that the observed effect could stem from two related mechanisms. First, refugees may view German society as less inclusive if their rights are constrained by the movement ban, a process that has been observed for other restrictive policies (see e.g. Fouka 2019; Abdelgadir and Fouka 2020). Second, refugees may perceive that the movement ban worsens their own economic prospects, consistent with the findings reported in figure 1. As a result, refugees could be less inclined to integrate into German society.

Before moving on, we discuss two limitation of our study. First, we are only able to examine short- to medium-term effects of the movement restriction. As shown in Table A.4 in the SI, our outcomes are measured either one or two years after the enactment of the policy. We are therefore not able to address long-term integration outcomes, which are likely the

most relevant indicators of success or failure of the movement restrictions. Further research is needed to investigate how restrictive policies affect refugees in the long term. Second, our identification strategy necessitates that we only consider a specific subset of refugees. Since we only look at refugees whose asylum application was approved around January 1, 2016, our sample mainly consists of refugees who entered the country in the second half of 2015 (see also tables A.1 and A.2 in the SI). In addition, Syrians are relatively over-represented in out sample, since exact matching requires a fairly large number of potential control units. Against this background, we emphasize that we identify a *local* average treatment effect that may not be representative of the average treatment effect among all refugees.

Our findings have important implications for the design of future integration policies. Aside from a suggestive positive effect on employment, our results show that movement restrictions negatively affect refugees' sense of belonging, social engagement and perceived employment. In addition, we find no evidence that refugees exempted from the restrictions moved at greater rates than those who were restricted. Our results hence call into question the necessity of movement restrictions to prevent the formation of ethnic enclaves. Instead, our findings suggest that governments should focus their attention on the initial spatial allocation of refugees upon arrival. Given that only a minority of refugees relocate, the initial placement of refugees is crucial to maximize the likelihood of successful integration (Auer 2018).

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A Supporting Information (Online Only)

Table A.1: Summary statistics: 2017 matched dataset (2 months bandwidth)

Variable	Year	Mean	S.D.	N	Min	Max
Covariates (binary)						
17 - 25 years old	2017	0.20	0.40	242	0	1
26 - 34 years old	2017	0.34	0.47	242	0	1
35 - 45 years old	2017	0.38	0.49	242	0	1
46 - 54 years old	2017	0.08	0.27	242	0	1
Arrival in 2nd Quarter of 2015	2017	0.03	0.17	242	0	1
Arrival in 3rd Quarter of 2014	2017	0.02	0.13	242	0	1
Arrival in 3rd Quarter of 2015	2017	0.78	0.42	242	0	1
Arrival in 4th Quarter of 2014	2017	0.01	0.09	242	0	1
Arrival in 4th Quarter of 2015	2017	0.17	0.38	242	0	1
Bachelors or equivalent level	2017	0.22	0.42	242	0	1
Eritrean	2017	0.01	0.09	242	0	1
Female	2017	0.26	0.44	242	0	1
Iraqi	2017	0.04	0.20	242	0	1
Lower secondary education	2017	0.22	0.42	242	0	1
Male	2017	0.74	0.44	242	0	1
Movement restriction treatment	2017	0.50	0.50	242	0	1
Post-secondary non-tertiary education	2017	0.01	0.09	242	0	1
Primary education	2017	0.30	0.46	242	0	1
Syrian	2017	0.95	0.22	242	0	1
Upper secondary education	2017	0.25	0.43	242	0	1
Outcomes						
Contact: co-nationals	2017	4.09	1.47	242	1	6
Contact: natives	2017	3.86	1.82	242	1	6
Contact: other immigrants	2017	2.92	1.85	242	1	6
Employed	2017	0.10	0.30	242	0	1
Identification with home country	2017	3.67	1.15	241	1	5
Perceived employment prospects	2017	82.99	20.52	67	0	100
Social events: culture	2017	1.23	0.53	241	1	4
Social events: entertainment	2017	1.42	0.77	241	1	4
Social events: going out	2017	2.48	1.11	242	1	5
Social events: religious	2017	2.10	1.22	239	1	4
Social events: sports	2017	1.59	1.02	241	1	5

Note: The table contains summary statistics for all background variables and outcomes. All covariates are binary. In the main analyses, we standardize all outcomes except employment.

Table A.2: Summary statistics: 2018 matched dataset (2 months bandwidth)

Variable	Year	Mean	S.D.	N	Min	Max
Covariates (binary)						
17 - 25 years old	2018	0.15	0.36	208	0	1
26 - 34 years old	2018	0.36	0.48	208	0	1
35 - 45 years old	2018	0.42	0.50	208	0	1
46 - 54 years old	2018	0.06	0.23	208	0	1
55 - 64 years old	2018	0.01	0.10	208	0	1
Afghan	2018	0.01	0.10	208	0	1
Arrival in 2nd Quarter of 2015	2018	0.07	0.26	208	0	1
Arrival in 3rd Quarter of 2015	2018	0.71	0.45	208	0	1
Arrival in 4th Quarter of 2015	2018	0.22	0.41	208	0	1
Bachelors or equivalent level	2018	0.24	0.43	208	0	1
Female	2018	0.26	0.44	208	0	1
Lower secondary education	2018	0.24	0.43	208	0	1
Male	2018	0.74	0.44	208	0	1
Movement restriction treatment	2018	0.52	0.50	208	0	1
Primary education	2018	0.28	0.45	208	0	1
Syrian	2018	0.99	0.10	208	0	1
Upper secondary education	2018	0.24	0.43	208	0	1
Outcomes						
Contact: co-nationals	2018	4.00	1.47	207	1	6
Contact: natives	2018	3.94	1.96	207	1	6
Contact: other immigrants	2018	2.98	1.90	207	1	6
Employed	2018	0.25	0.43	208	0	1
Feel welcome in Germany	2018	4.06	0.89	194	1	5
Identification with home country	2018	3.55	1.30	207	1	5
Social events: culture	2018	1.29	0.57	195	1	5
Social events: entertainment	2018	1.50	0.83	194	1	4
Social events: going out	2018	2.74	1.07	194	1	5
Social events: religious	2018	1.67	1.23	12	1	4
Social events: sports	2018	1.68	1.08	195	1	5

Note: The table contains summary statistics for all background variables and outcomes. All covariates are binary. In the main analyses, we standardize all outcomes except employment.

Table A.3: Survey items corresponding to outcomes

| Variable	Survey Question (translated from German)	Coding
1. Contact		
Contact: natives	How often do you spend time with German people?	never [1], infrequent [2], every month [3], every week [4], several times per week [5], every day [6]
Contact: co-nationals	How often do you spend time with people from your country of origin who are not related to you?	never [1], infrequent [2], every month [3], every week [4], several times per week [5], every day [6]
Contact: other immigrants	How often do you spend time with people from other countries?	never [1], infrequent [2], every month [3], every week [4], several times per week [5], every day [6]
2. Belonging		
Feel welcome in Germany	To what degree do you feel welcome in Germany?	not at all [1], barely [2], somewhat [3], much [4], very much [5]
Identification with home country	How connected do you feel to your country of origin?	not at all [1], barely [2], in some respects [3], strongly [4], very strongly [5]
3. Social		
Social events: going out	How often do you go to eat or drink in a cafe, restaurant or bar?	never [1], infrequent [2], every month [3], every week [4], every day [5]
Social events: sports	How often do you attend sports events?	never [1], infrequent [2], every month [3], every week [4], every day [5]
Social events: religious	How often do you attend religious events?	never [1], infrequent [2], every month [3], every week [4]
Social events: entertainment	How often do you go to the cinema, pop concerts, dance events, clubs?	never [1], infrequent [2], every month [3], every week [4], every day [5]
Social events: culture	How often do you go to cultural events such as opera, classical concerts, theater, exhibitions	never [1], infrequent [2], every month [3], every week [4], every day [5]
4. Economic integration		
Perceived employment prospects	How likely is it that you will be employed in Germany at any time within the next five years?	0 – 100 % (in intervals of 10 percent each)
Employed	Are you currently employed?	Employed [1], Not employed [0] (both part-time and full-time employment are coded as one)

Figure A.1: Covariate balance (raw, across blocks)
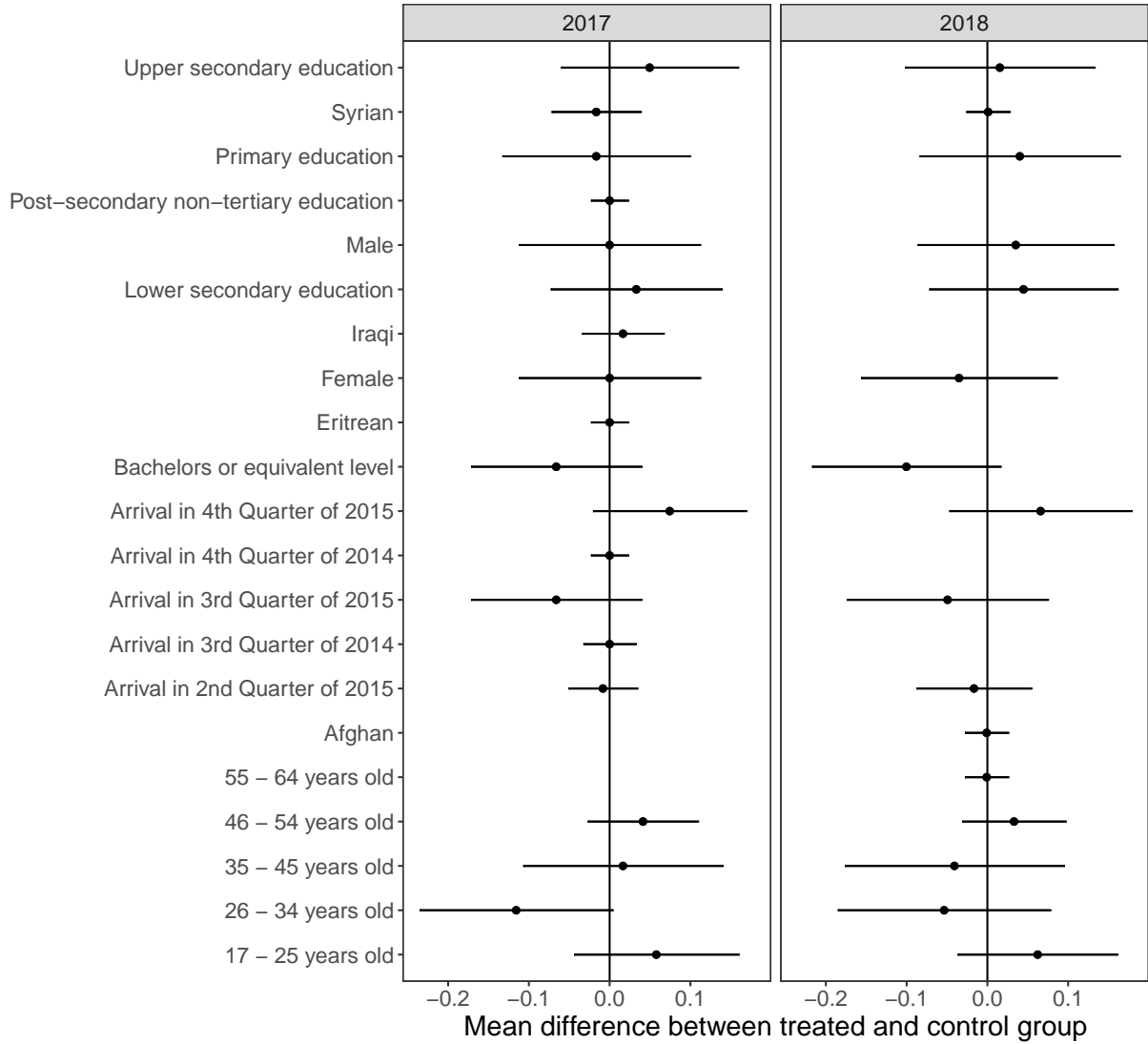
Note: The figure shows the mean difference between the treated and control groups for the sample we use to estimate our main results presented in figure 1. We matched respondents within 2 months around the treatment assignment cutoff. All covariates are binary indicator variables. Some covariate-categories are not represented in the matched datasets in either year. We emphasize that covariate-balance within the covariate blocks described in section 4 is perfect by construction. Comparing the treated and control groups across blocks results in some imbalance because we match each treated unit to all available control units. Because of this, the relative size of the treated and control group can vary across blocks.

Figure A.2: Movement within Germany
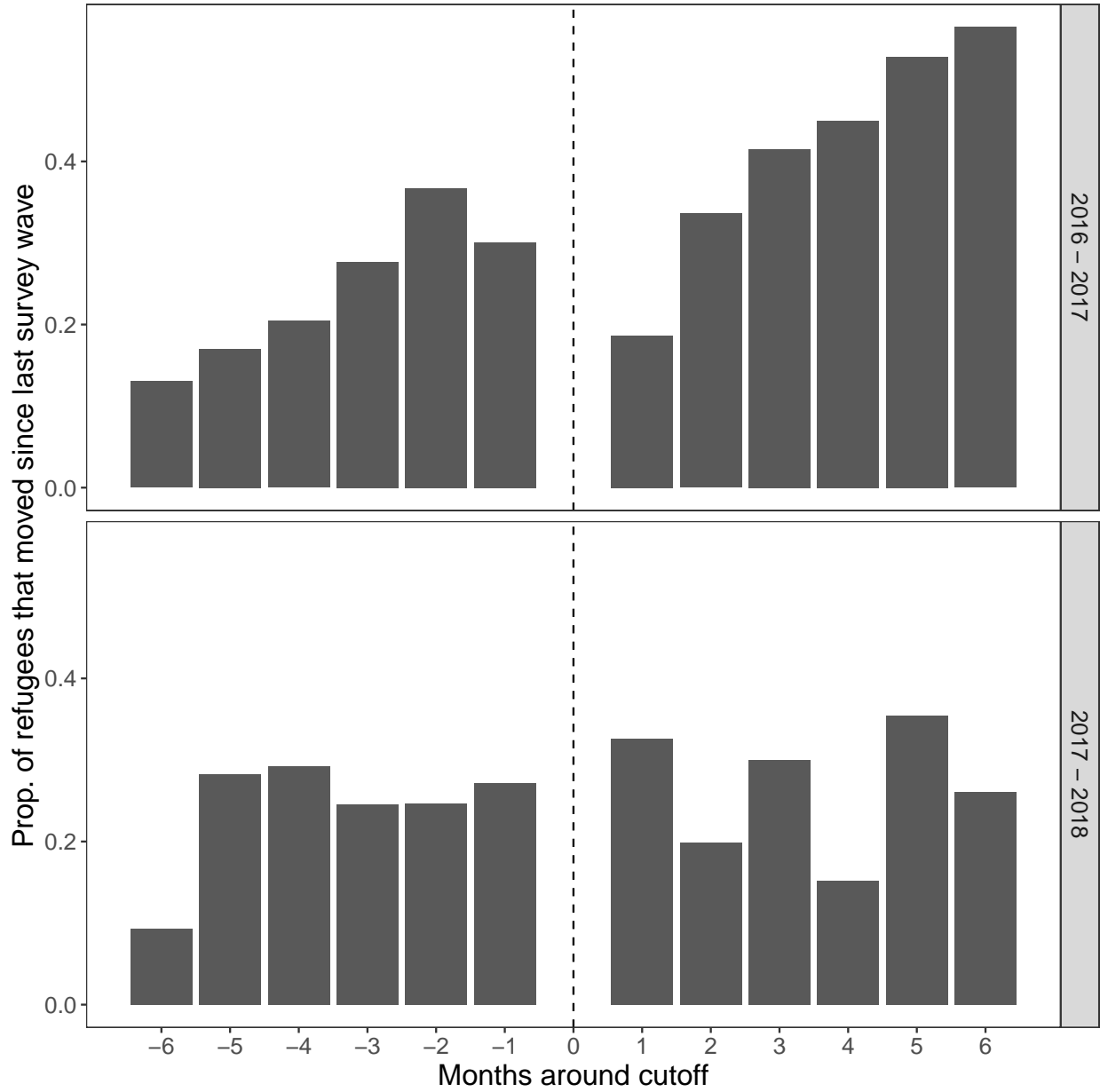
Note: The figure show the relative frequency of refugees who changed their place of residence between the 2016/2017 and 2017/2018 survey waves, grouped by months around the January 1, 2016 cutoff.

Figure A.3: Effect of movement restrictions on relocating
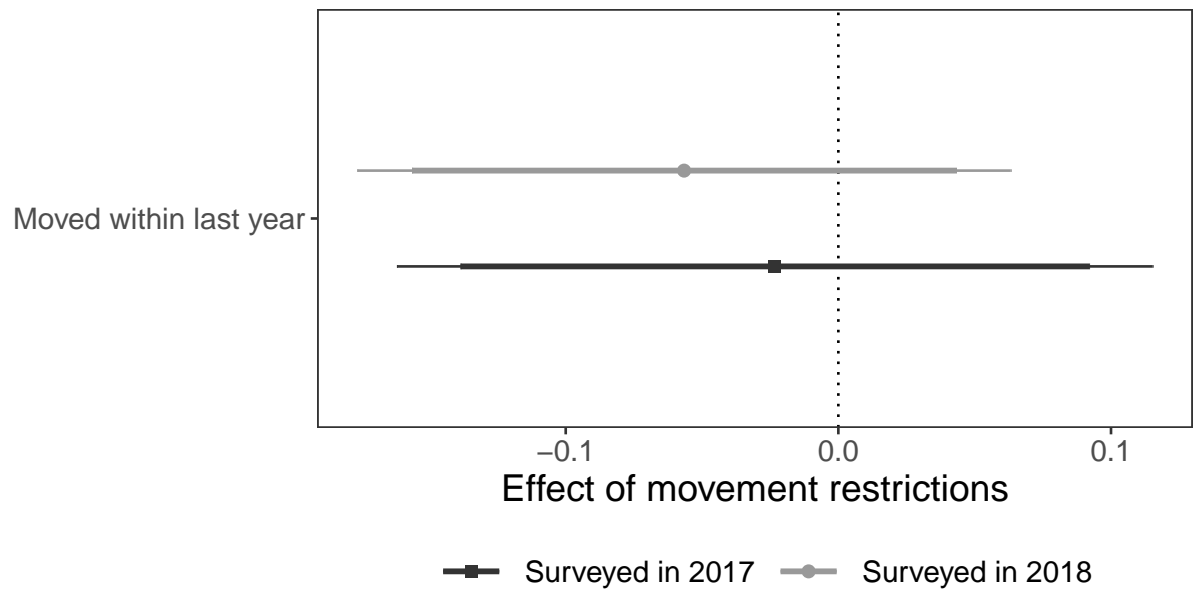
Note: The figure shows estimated effects of being subject to movement restrictions on whether respondents moved since the last survey wave. The horizontal bars represent 90% (thick lines) / 95% (thin lines) confidence intervals. The outcome is binary.

Table A.4: Duration since entry and movement restrictions

| Treatment status	Year	Avg. time spent in DE (years)	Avg. time since restrictions (years)
Not restricted	2017	1.93	0.91
Restricted	2017	1.98	0.98
Not restricted	2018	3.06	2.05
Restricted	2018	3.13	2.14

Note: The table shows how long, on average, respondents have lived in Germany when the SOEP survey was conducted. In addition, the table contains the time that has passed between the August 2016, when restrictions were first applied, and the date when survey data was collected. We show both statistics conditional on respondent restriction status.

Table A.5: Main results

| Outcome	Year	Estimate	SE	P	P (RI)	N
1. Contact						
Contact: natives	2017	-0.02	0.12	0.88	0.90	242
Contact: natives	2018	-0.06	0.13	0.64	0.68	207
Contact: co-nationals	2017	0.01	0.12	0.91	0.92	242
Contact: co-nationals	2018	-0.12	0.12	0.29	0.32	207
Contact: other immigrants	2017	-0.06	0.13	0.66	0.72	242
Contact: other immigrants	2018	-0.13	0.13	0.31	0.38	207
2. Belonging						
Feel welcome in Germany	2018	-0.46	0.12	0.00	0.00	194
Identification with home country	2017	0.23	0.13	0.08	0.13	241
Identification with home country	2018	0.39	0.13	0.00	0.01	207
3. Social						
Social events: going out	2017	-0.22	0.12	0.07	0.10	242
Social events: going out	2018	0.04	0.13	0.77	0.79	194
Social events: sports	2017	-0.09	0.12	0.46	0.53	241
Social events: sports	2018	0.03	0.14	0.81	0.83	195
Social events: religious	2017	-0.22	0.12	0.06	0.10	239
Social events: entertainment	2017	-0.20	0.12	0.09	0.13	241
Social events: entertainment	2018	-0.10	0.14	0.49	0.56	194
Social events: culture	2017	-0.33	0.11	0.00	0.01	241
Social events: culture	2018	0.00	0.13	0.98	0.98	195
4. Economic integration						
Perceived employment prospects	2017	-0.61	0.26	0.02	0.08	67
Employed	2017	0.03	0.04	0.40	0.46	242
Employed	2018	0.15	0.05	0.00	0.01	208

The table shows the main results discussed in the text. All outcomes except employment are standardized.

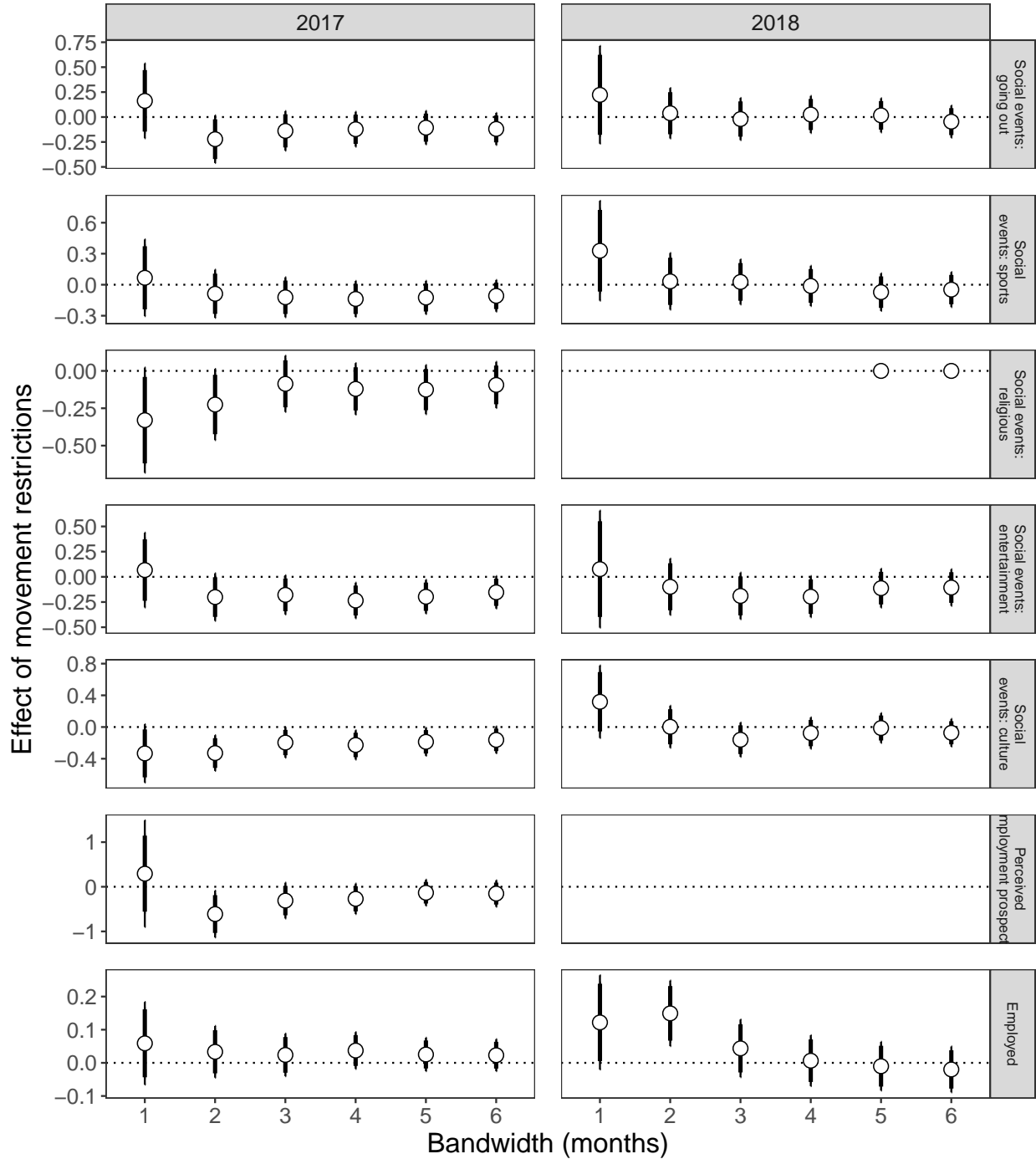Figure A.4: Effect of movement restrictions, OLS without block FE
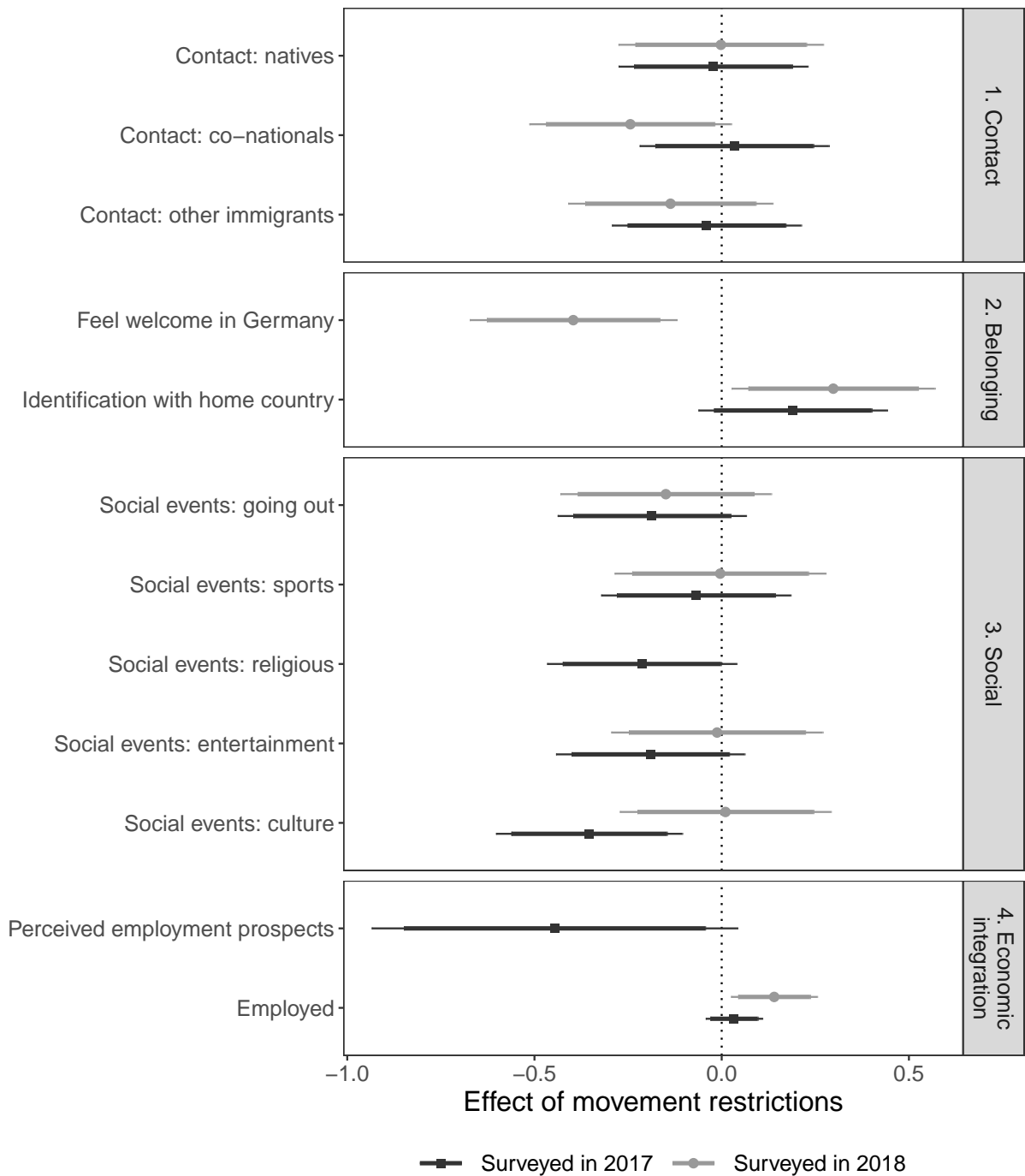
Effect of movement restrictions

Surveyed in 2017   Surveyed in 2018

Note: The figure shows estimated effects of being subject to movement restrictions on the outcomes listed on the left-hand side. The horizontal bars represent 90% (thick lines) / 95% (thin lines) confidence intervals. All variables are standardized except employment status.

Figure A.5: Bandwidth sensitivity, contact and sense of belonging
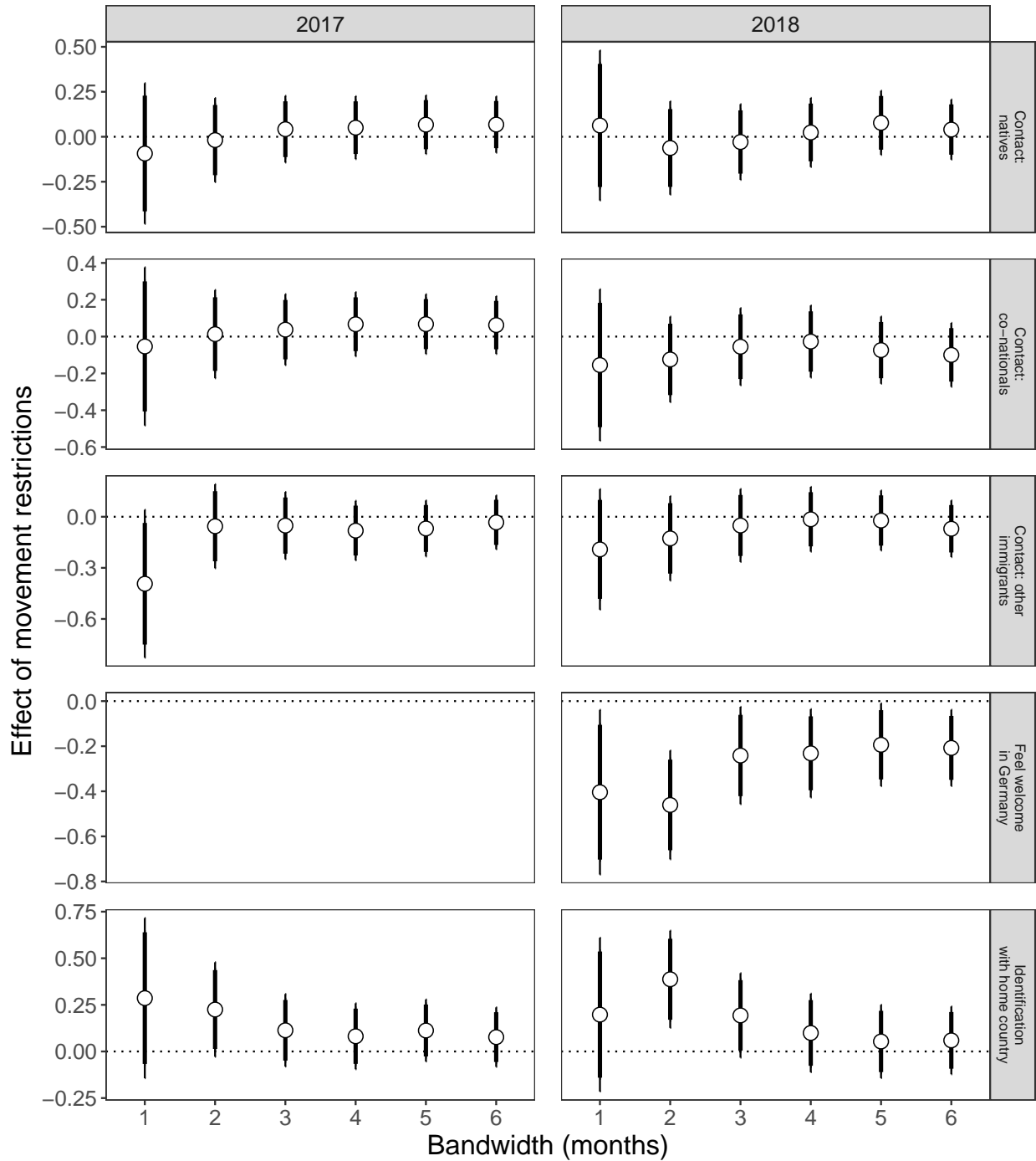
Note: The figure shows estimated effects of being subject to movement restrictions, conditional on bandwidth around the asylum application date cutoff and year when the survey was conducted. The horizontal bars represent 90% (thick lines) / 95% (thin lines) confidence intervals. All models include block fixed effects. In cases where we show no estimates, fewer than 20 individuals responded to the survey question, which means we did not estimate a model for the given year-bandwidth combination.

Figure A.6: Bandwidth sensitivity, social and economic integration

Note: The figure shows estimated effects of being subject to movement restrictions, conditional on bandwidth around the asylum application date cutoff and year when the survey was conducted. The horizontal bars represent 90% (thick lines) / 95% (thin lines) confidence intervals. All models include block fixed effects. In cases where we show no estimates, fewer than 20 individuals responded to the survey question, which means we did not estimate a model for the given year-bandwidth combination.

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Figure A.7: Randomization inference, 2018
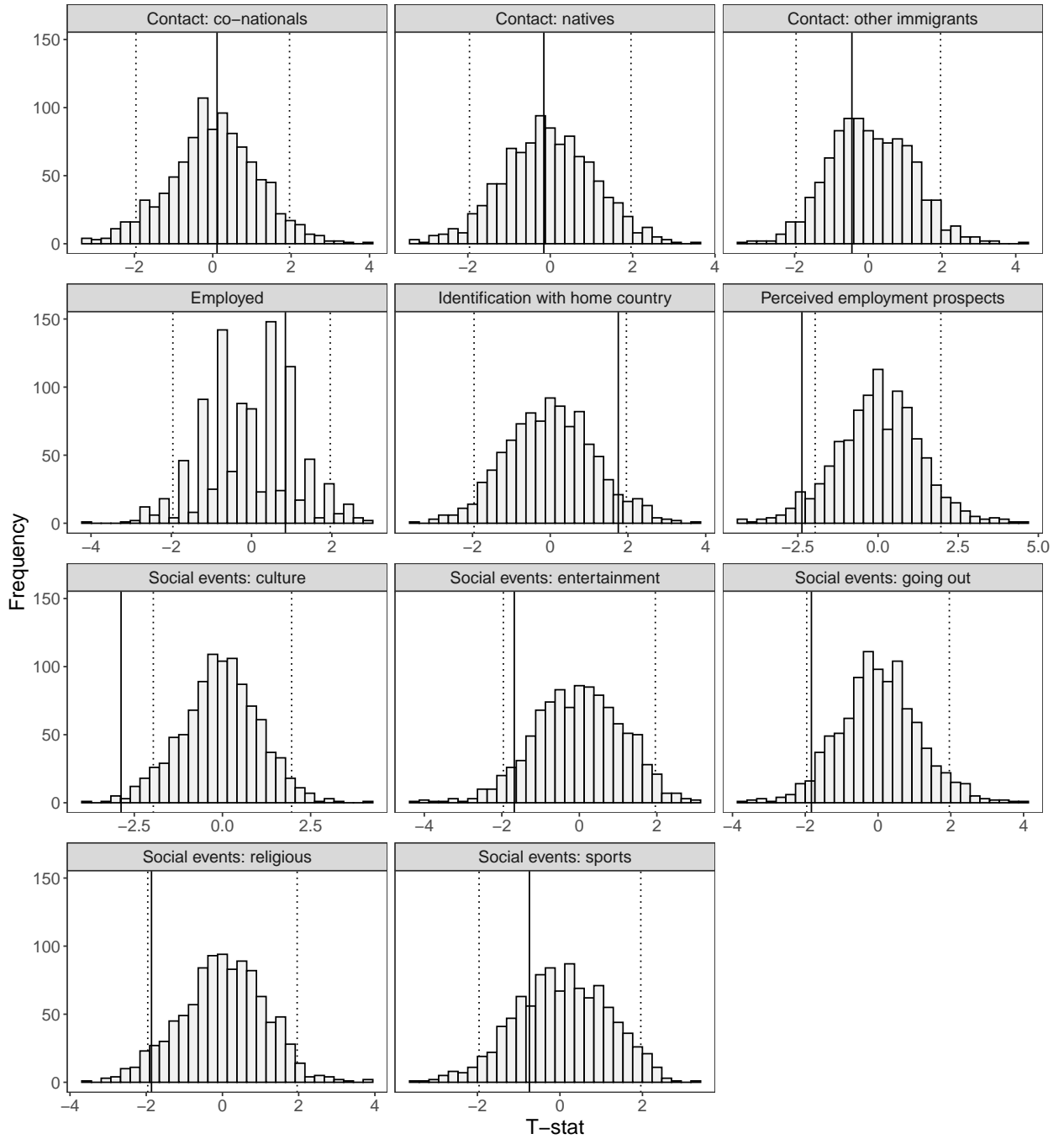
Note: The histograms show the distribution of the t-statistic for the treatment effect estimate over 1,000 random permutations of the treatment assignment vector. For each outcome variable, we conduct the same regression analysis with block-fixed-effects as for our main results presented in figure 1. The outcome variables are meausured in the survey year 2017.

Figure A.8: Randomization inference, 2018
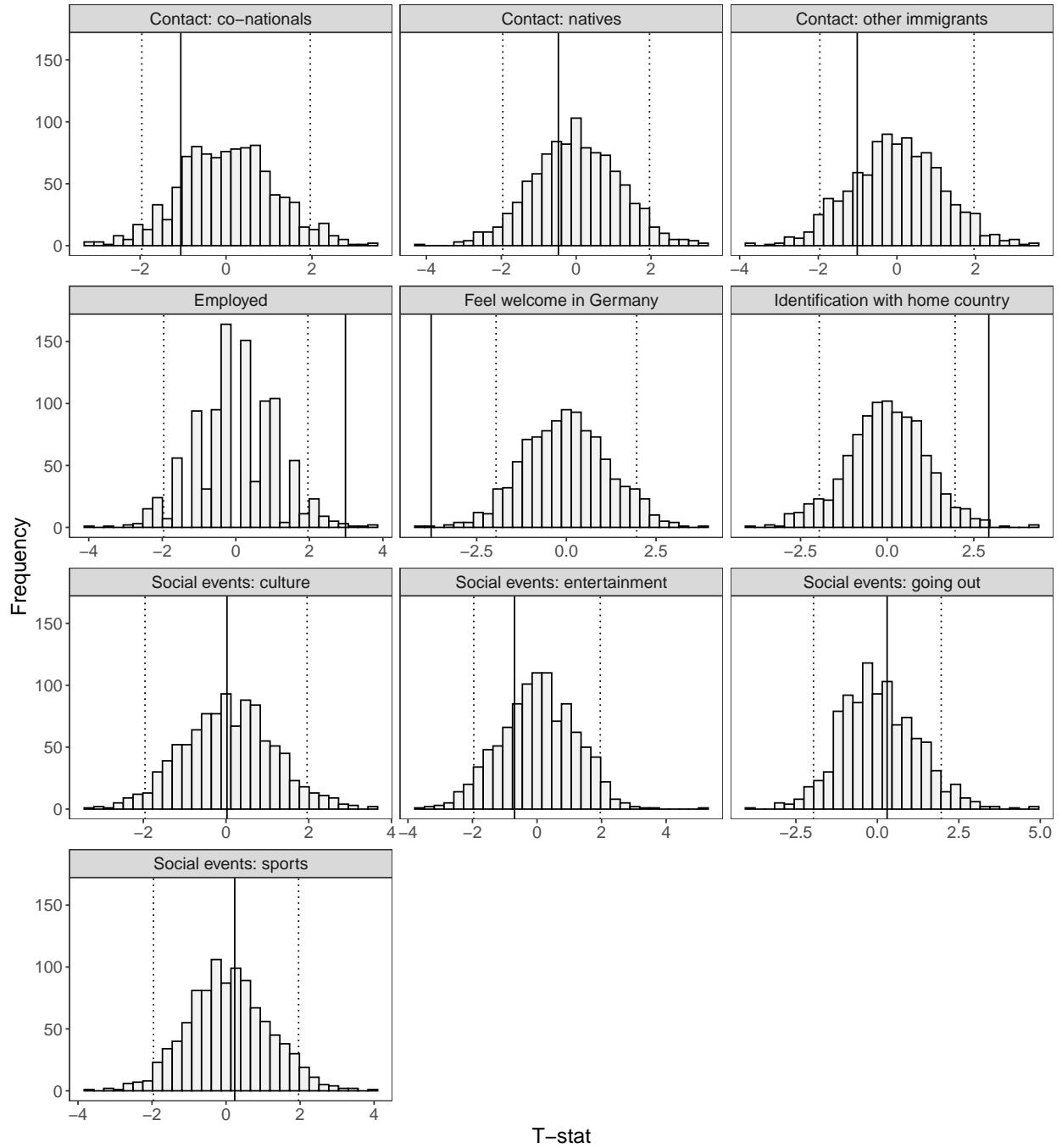
Note: The histograms show the distribution of the t-statistic for the treatment effect estimate over 1,000 random permutations of the treatment assignment vector. For each outcome variable, we conduct the same regression analysis with block-fixed-effects as for our main results presented in figure 1. The outcome variables are measured in 2018.

Figure A.9: Effect of movement restrictions on 2016 outcomes
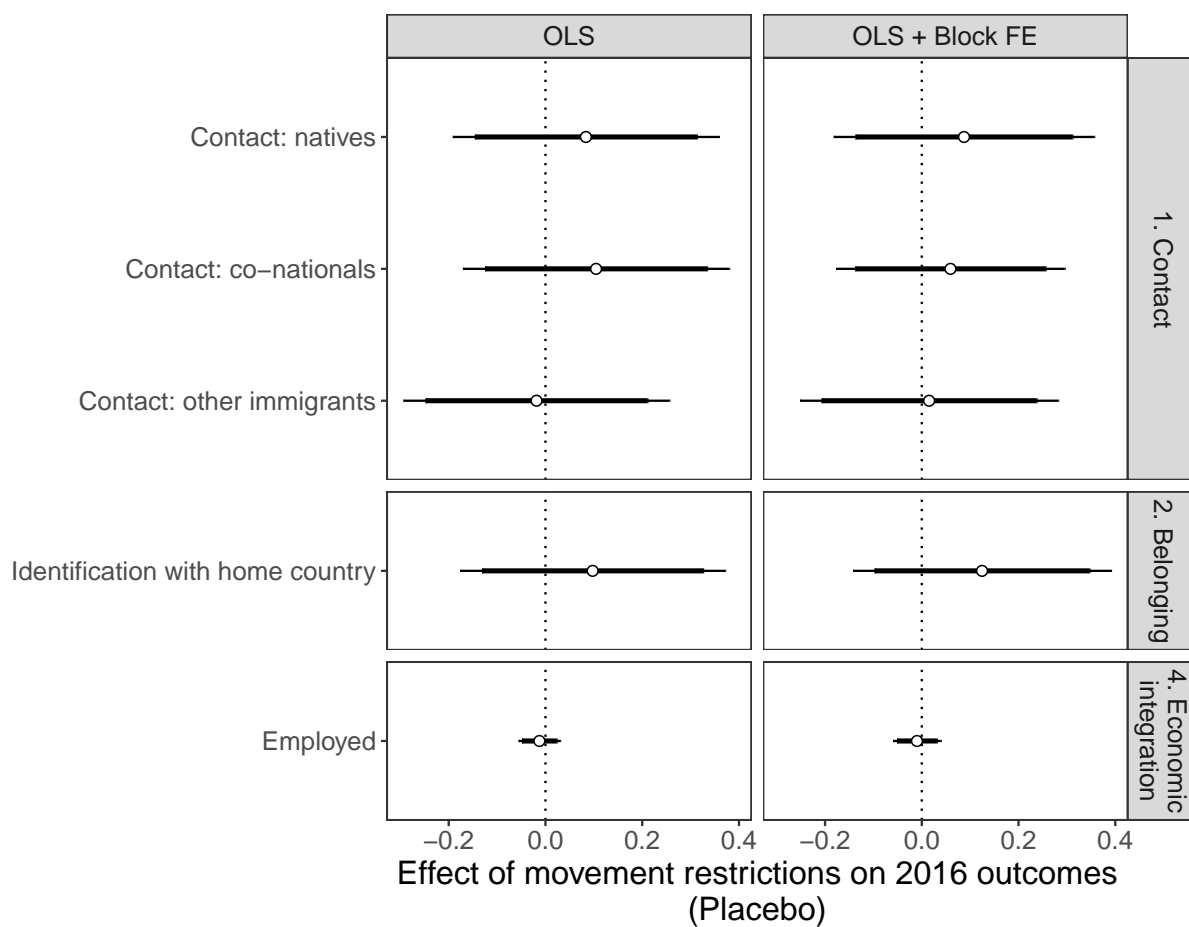
Note: The figure shows estimated effects of being subject to movement restrictions on the outcomes listed on the left-hand side. The horizontal bars represent 90% (thick lines) / 95% (thin lines) confidence intervals. All outcomes were measured in 2016, before the policy was enacted. The left-hand side panel is a simple OLS specification, the right-hand panel includes block fixed effects.

Figure A.10: Effect of application decision speed
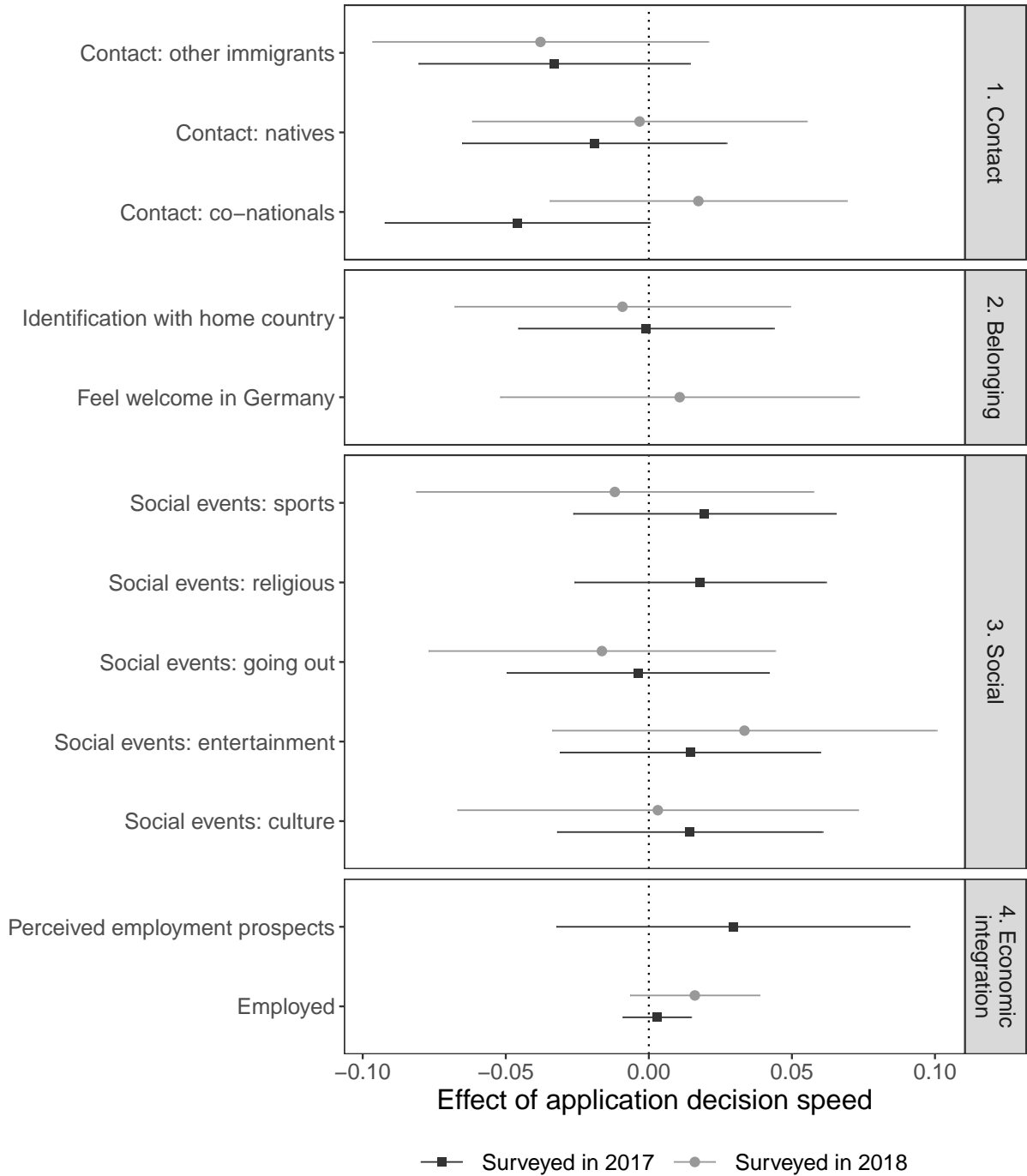
Note: The figure shows the results from OLS regressions where we regress the same outcome variables as in our main analysis on the time in months between the arrival date of a refugee and the asylum application decision date. Similar to our main analysis, the models include block fixed effects. The blocks are defined by the same covariates as for our main analysis (sex, age, education, nationality, and arrival quarter-year) *plus* the asylum application decision time in intervals of 4 months. We hence compare refugees with similar background characteristics whose asylum applications were processed within a time-window of 4 months. This corresponds to the 2-months bandwidth around the treatment assignment cutoff used for our main analysis. We exclude outcomes for which fewer than 200 observations are available within a given survey year.