

Freedom of Movement Restrictions Inhibit the Social Integration of Refugees ^{*}

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Abstract

How do freedom of movement restrictions affect refugee integration? While a growing body of research studies the initial spatial allocation of refugees, there is little causal evidence on subsequent policies that restrict residential mobility. We study a contentious law in Germany, which barred refugees from moving to a location different from the one they were randomly assigned to. To identify the causal effect of the movement restriction on integration, we utilize a sharp date cutoff that governs whether refugees are affected by the policy. We demonstrate that restricting freedom of movement had pronounced negative effects on refugees' sense of belonging in Germany, while increasing identification with their home countries. In addition, the policy decreased engagement in a variety of social activities. Our findings suggest that discriminatory policies send a negative signal about the inclusiveness of the host society and thereby reduce incentives for refugee integration.

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1 Introduction

As the number of forcibly displaced people approaches one percent of the world population, the integration of refugees has become a critical public policy challenge. To facilitate integration, governments in destination countries frequently enact restrictive integration policies, ranging from employment bans to limits on religious expression (Abdelgadir and Fouka 2020). The earliest experience of restrictions often comes in the form of initial spatial allocation policies, which have received considerable scholarly attention (see e.g. Bratsberg et al. 2020). Less is known about integration policies that limit residential mobility beyond the initial allocation. However, a large number of countries including Switzerland, the Netherlands, and France either outright prohibit or penalize refugees for leaving their assigned location. Beyond scholarly interest, understanding the impact of such policies has important legal implications. According to a recent ruling by the European Court of Justice, movement restrictions are permissible only if they facilitate the integration of refugees.¹

We study the case of Germany, where a 2016 law forced more than one million refugees to remain at the location they were randomly assigned to upon arrival in the country (BAMS 2016). While intended to facilitate integration, the effect of such restrictions on residential mobility is far from clear. Proponents of the restriction argue that permitting refugees to freely choose their place of residence would result in the formation of urban ethnic enclaves, reducing incentives to assimilate (see Cutler, Glaeser and Vigdor 2008). On the other hand, the policy makes it harder for refugees to find jobs through local ethnic networks and may hence slow down their integration into the German labor market (see also Damm 2014). Finally, movement restrictions may lead refugees to view the host society as discriminatory, inhibiting their willingness to integrate (Adida, Laitin and Valfort 2014).

To estimate the causal effect of the movement restriction, we leverage a sharp date cutoff that governs the application of the policy. Only refugees whose asylum applications were

¹Court of Justice of the European Union (CJEU) cases C443/14 and C444/14.

approved after January 1, 2016 are banned from relocating. We exploit this discontinuity and compare otherwise similar refugees within a small bandwidth around this treatment assignment cutoff. Drawing on a panel survey of about 5,000 refugees, we estimate the causal effect of movement restrictions on indicators for contact, feelings of belonging, social integration, and economic integration, measured in 2017 and 2018.

We first document that restricted refugees were not significantly more likely to relocate than those who are not restricted. We then demonstrate that movement restrictions made refugees markedly more pessimistic about their employment prospects in Germany. Turning to measures of identity and belonging, we find pronounced negative effects. Affected refugees felt less welcome in Germany, identified more strongly with their home countries, and participated less in social activities. However, we do not find that restrictions affected the frequency of contact with natives, co-ethnics, or other immigrants. Finally, we do not observe negative effects on labor market integration, as employment is higher among restricted refugees. However, we emphasize that this effect may stem from the fact that employment exempts refugees from the movement ban, and therefore incentivizes them to seek employment.

Taken together, our results suggest that restricting residential mobility negatively affects refugees' social engagement and feelings of belonging. In the absence of effects on relocation decisions, we argue that movement restrictions hinder refugee integration because such discriminatory policies send a negative signal about the inclusiveness of the host society. Strikingly, a policy that was designed to inhibit ethnic segregation and the entrenchment of separate communities resulted in *stronger* identification with refugees' country of origin.

Our results contribute to a growing literature on restrictive or exclusionary policies that affect minority populations, such as restrictions on religious expression (Abdelgadir and Fouka 2020), citizenship (Avitabile, Clots-Figueras and Masella 2013) or language education (Fouka 2019). Previous research has shown that severe intrusions into individual rights, such as internment, can negatively affect the incorporation of minority populations (Komisarchik, Sen and Velez 2020). We demonstrate that even less severe intrusions, such as restricting

internal migration, may create a rift between the affected group and the majority population. Finally, our results can shed light on the repercussions of a broader class of policies that restrict internal migration of minority populations, such as the Chinese *Hukou* or the Vietnamese *Ho Khau* system.

2 Background

Spatial dispersion policies are typically enacted to prevent the formation of ethnic enclaves. Such policies rest on the assumption that, when allowed to move freely, refugees would sort into residential areas with a high concentration of refugees or other immigrants. This derives from the general concept of social homophily – a preference for being around others who are similar to themselves, allowing for easier communication and cooperation (Cutler, Glaeser and Vigdor 2008). Prior research has yielded mixed results on the effects of ethnic segregation. On the one hand, as ethnic enclaves provide immigrants access to social networks and economic opportunities, they might reduce incentives to assimilate (see e.g. Lazear 1999). At the same time, ethnic networks can facilitate labor market integration of refugees by disseminating job information and improving the job-worker match quality (Damm 2014).

Discriminatory policies can inhibit refugee integration even without directly affecting relocation decisions. Movement restrictions are an intrusion into a fundamental liberty – the right to freely choose one’s place of residence. As movement restrictions generally only apply to refugees, they send a strong negative signal about the inclusiveness of the host society. Discriminatory policies may entrench group identities, rendering assimilation and social integration more costly. Prior research has shown that institutionalized discrimination has negative effects on immigrant integration. Adida, Laitin and Valfort (2014) provide evidence that Muslims in France are reluctant to assimilate, in part because they perceive French institutions as systematically discriminatory. This aligns with recent research by Abdelgadir and Fouka (2020), who find 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. Restrictive policies can aggravate the already precarious state of subjective well-being and mental health of many refugees after fleeing from war and persecution (Fazel, Wheeler and Danesh 2005).

3 Movement restrictions in Germany

Beginning in 2015, Germany experienced an unprecedented inflow of refugees. Today, refugees constitute more than two percent of the country’s population. To facilitate the integration of refugees into German society, the government passed the wide-ranging *Integrationsgesetz* (‘integration law’) in August 2016. The law introduced mandatory movement restrictions for refugees in Germany, motivated by (i) the desire to prevent enclave formation and (ii) calls to equally distribute the costs of integrating refugees across the country.

After arriving in Germany, refugees are randomly allocated to one of the 16 federal states through a computer algorithm. The total number of refugees a state receives is proportional to its population size and tax revenue. Within each state, refugees are then allocated to counties. With few exceptions, the number of refugees a county receives is proportional to its population size. The allocation of refugees to counties is independent of the individual characteristics of the refugees. Therefore, we would not expect that certain types of refugees are more likely to be placed in, for example, urban areas. The August 2016 movement restriction policy 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. The policy prevents refugees from freely choosing their *place of residence* but does not prohibit them from traveling within the country.

4 Data and Empirical Strategy

Our main data source is the *IAB-BAMF-SOEP Survey of Refugees in Germany (SOEP)*, a panel of about 5,000 refugees surveyed in 2016, 2017, and 2018. In addition to demographics, asylum status, and labor market indicators, the survey contains several items on

refugee integration. We focus on outcome variables measuring social contact, participation in social activities, subjective feelings of belonging, and labor market integration. Most of our outcomes are measured twice after the policy was enacted, in 2017 and 2018. 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 do not have information on the county or city where refugees reside.

Refugees are only subject to the movement restriction if their asylum application was approved after January 1, 2016. To estimate the causal effect of movement restrictions on integration, we compare refugees who are similar with respect to background characteristics, arrived in Germany at the same time, but whose asylum applications were approved right before or after the treatment assignment cutoff. Importantly, we do not estimate the effect of relocating on integration, but rather the effect of being subject to the movement restriction.

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. 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 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 asylum application was approved after January 1, 2016. We estimate a total of $M = 47$ (2017) or

$M = 45$ (2018) block fixed effects β_j in the case of a two-month bandwidth, where $B_{i,j} = 1$ 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. Exemptions from the policy can be granted if refugees take up employment, vocational training, or distant tertiary education. We do not match on these variables to avoid post-treatment bias. However, noncompliance with the policy is rare – only 3.2% of refugees in the 2016 sample were in employment that made them eligible for an exemption.

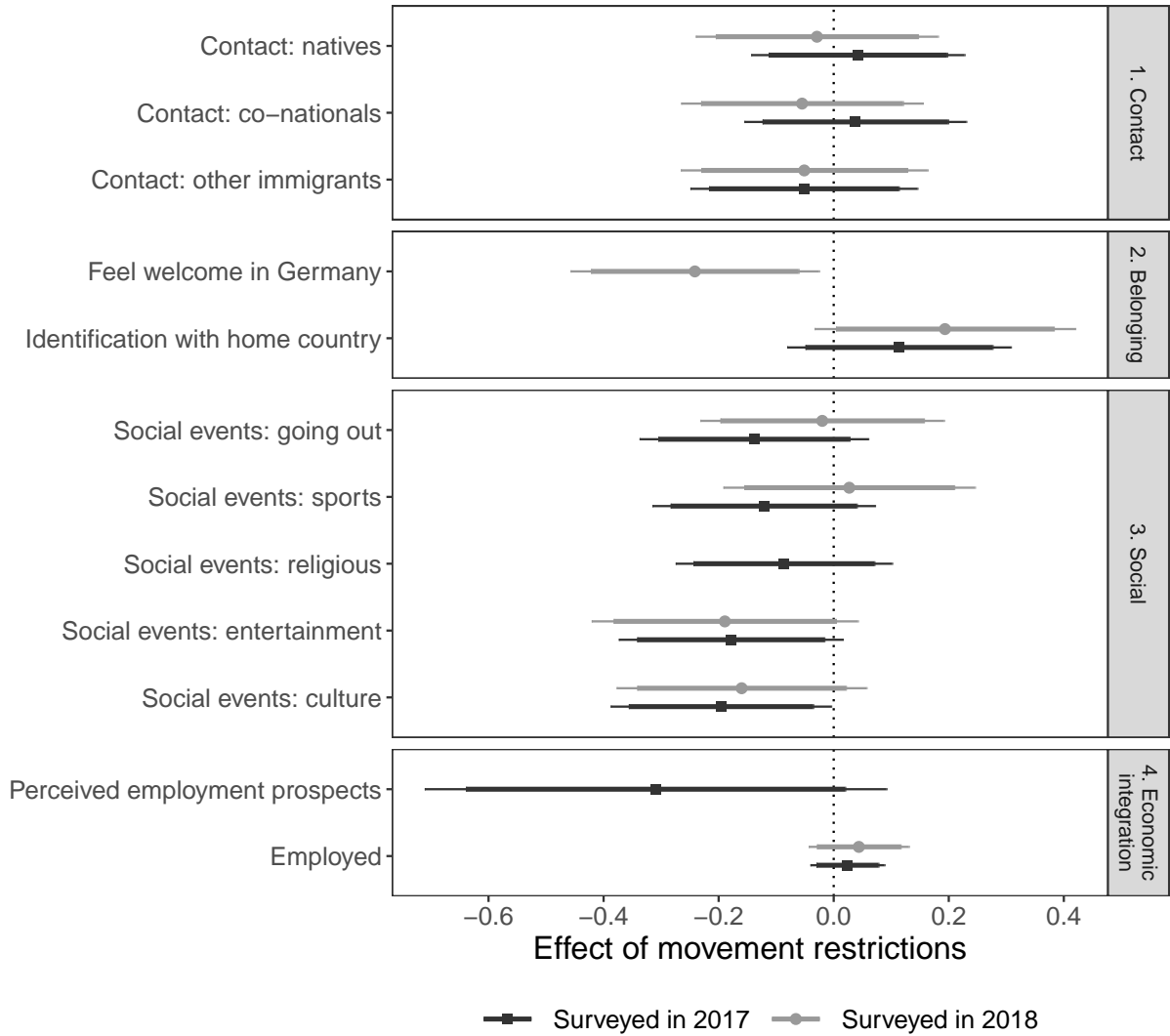
Our identification strategy requires that the timing of approval decisions around the January 1 cutoff is uncorrelated with unobserved individual or institutional characteristics. In section A.6, we provide evidence in support of this identification assumption. Relatedly, we employ the panel structure of the SOEP to show that there is no evidence for pre-treatment differences around the treatment assignment cutoff. Finally, we emphasize that sorting around the treatment assignment cutoff is not a concern. All asylum decisions in our matched sample were made between November 2015 and February 2016, several months before the integration law was passed in August 2016. To our knowledge, the possibility of a sharp date cutoff was first discussed in May 2016 (BAMS 2016). Refugees and local bureaucrats were unaware of the sharp date cutoff when asylum decisions were made.

5 Results

First, we show that restrictions had little effect on refugee movement. More than 60% of restricted refugees report that they would relocate within Germany if permitted. In figures A.2 and A.3 in the SI, we however demonstrate that unrestricted refugees are not significantly more likely to move between two survey waves than those refugees whose movement is restricted. We discuss potential reasons for this finding in section A.4 in the SI. In addition, we show that unrestricted refugees are not significantly more likely to live in urban areas.

In Figure 1, we present the main effects of the relocation ban, separately for the 2017 and

Figure 1: Effect of movement restrictions on integration



Note: The figure shows estimated effects 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. The sample size is between 194 and 242 for all outcomes except perceived employment prospects, where it is 67. More details are given in table A.5 in the SI.

2018 survey waves.² Our first set of outcomes considers contact with natives, co-nationals, and other immigrants. We observe point estimates close to zero for all outcomes, suggesting that movement restrictions had little effect on contact. Moving to the two items that measure 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

²Not all survey items were asked in both years. We present pooled results in figure A.10.

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. Regarding social integration, we observe negative effect estimates of similar magnitude across all five social engagement items in 2017. These effects, however, are not observed in 2018. While this may seem at odds with our results on contact, two thirds of the refugees in our sample are married, and likely attend social events primarily with their families. As a result, the observed effects on attending social events do not automatically translate into effects on contact.

Regarding labor market integration, we first show that restricted refugees were initially much more pessimistic about their labor market prospects. However, we find no difference in employment in 2017, while employment among restricted refugees is higher in 2018 than among unrestricted refugees. While ostensibly positive, we note that the employment result should be considered in light of (i) the incentives created by the movement restrictions and (ii) the fact that additional employment is often in low-wage occupations. Refugees were incentivized to find employment, as this exempts them from the movement restriction (see section 4). Locational characteristics are unlikely to drive the employment effect, as we do not find that unrestricted refugees are more likely to move.³ Second, we stress that most employed refugees end up in low-skill occupations that pay little more than the lowest level of welfare benefits. At 1,500 euros per month, the median net household income of employed refugees in our sample falls just below the national poverty line.

In addition to the main results, section A.6 in the SI contains a series of robustness checks. Most importantly, we provide evidence that faster asylum decisions do not correlate with individual integration outcomes. Our identification strategy exploits the fact that some asylum applications are accepted slightly quicker than others, which may prove problematic if acceptance speed is correlated with unobserved characteristics of the local bureaucracy

³In a supplementary analysis, we show that the movement restrictions did not have differential impacts on refugees who live in urban compared to rural areas (see figure A.11).

or the affected refugees. Through a number of additional analyses, we validate that this is not the case. We also conduct additional checks to ensure that our results are robust across different model specifications and bandwidths.

6 Conclusion

Does restricting residential mobility benefit or hinder refugee integration? In this paper, we provide the first causal estimates of the effect of domestic movement restrictions after the 2015 refugee influx in Germany. We show that movement restrictions negatively affect integration, especially with respect to social engagement and belonging. We argue that the observed effect likely stems from the negative signal discriminatory policies send about the inclusiveness of the host society. The negative effects of discriminatory policies on refugee integration, in particular with respect to social integration and feelings of belonging, has been documented for discriminatory policies in other contexts (see e.g. Adida, Laitin and Valfort 2014; Fouka 2019; Abdelgadir and Fouka 2020).

Instead of restricting residential mobility, we argue that governments should focus their attention on the spatial allocation of refugees upon arrival. We found no evidence that movement restrictions have a substantial impact on relocation decisions. This calls into question the necessity of movement restrictions to (i) inhibit the formation of ethnic enclaves or (ii) prevent an unequal distribution of the costs of integrating refugees. However, prior research confirms the relevance of locational characteristics (Bratsberg et al. 2020) for integration. Consequently, a more efficient initial placement regime could achieve superior integration outcomes without the negative repercussions of the current movement restrictions.

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A Supporting Information (Online Only)

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A.1 Summary statistics and details on survey outcomes

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

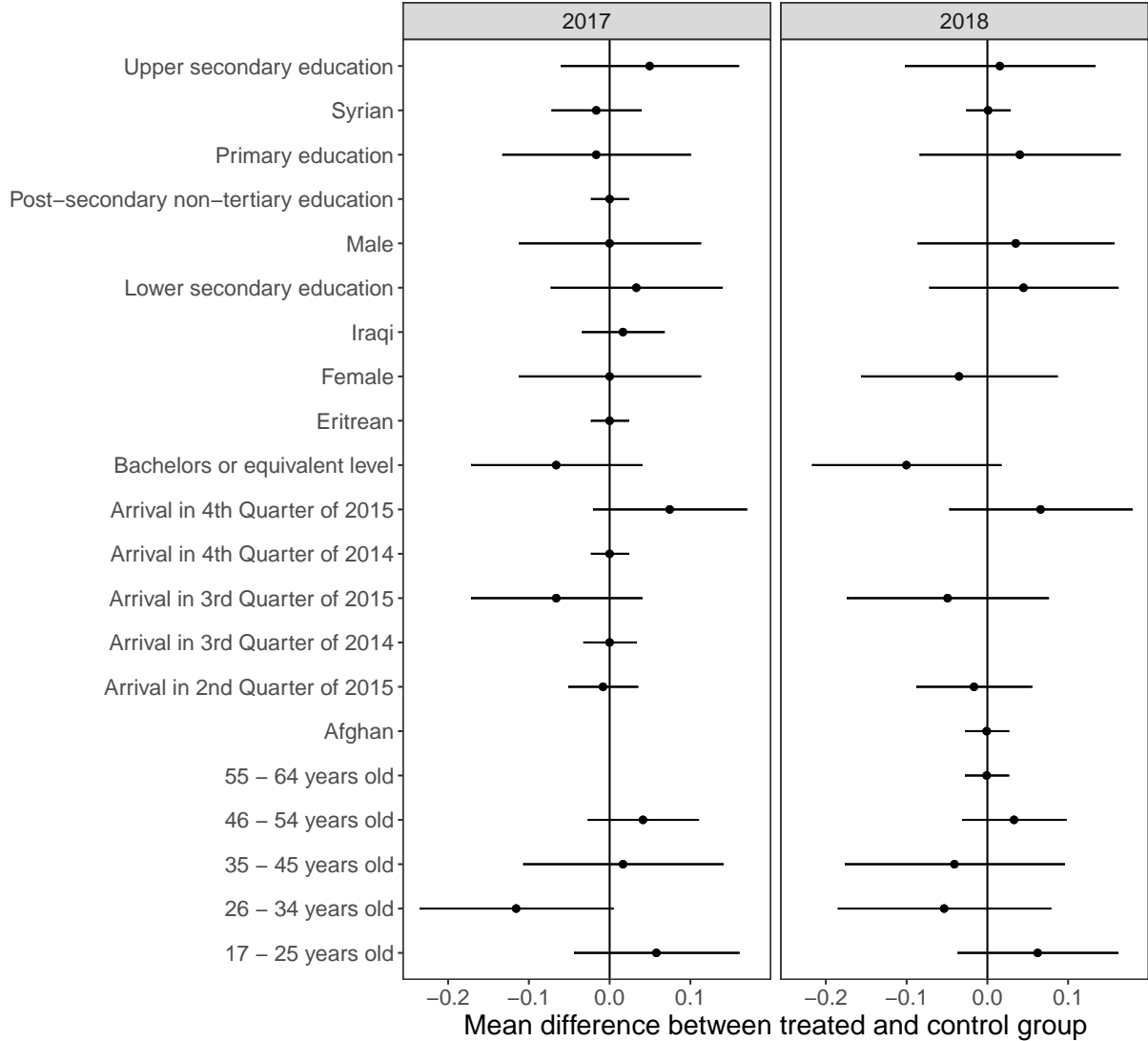
Note: The table contains summary statistics for all background variables and outcomes. All covariates are binary. In the main analyses, we standardize all outcomes except employment.

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)

A.2 Pre-treatment covariate balance

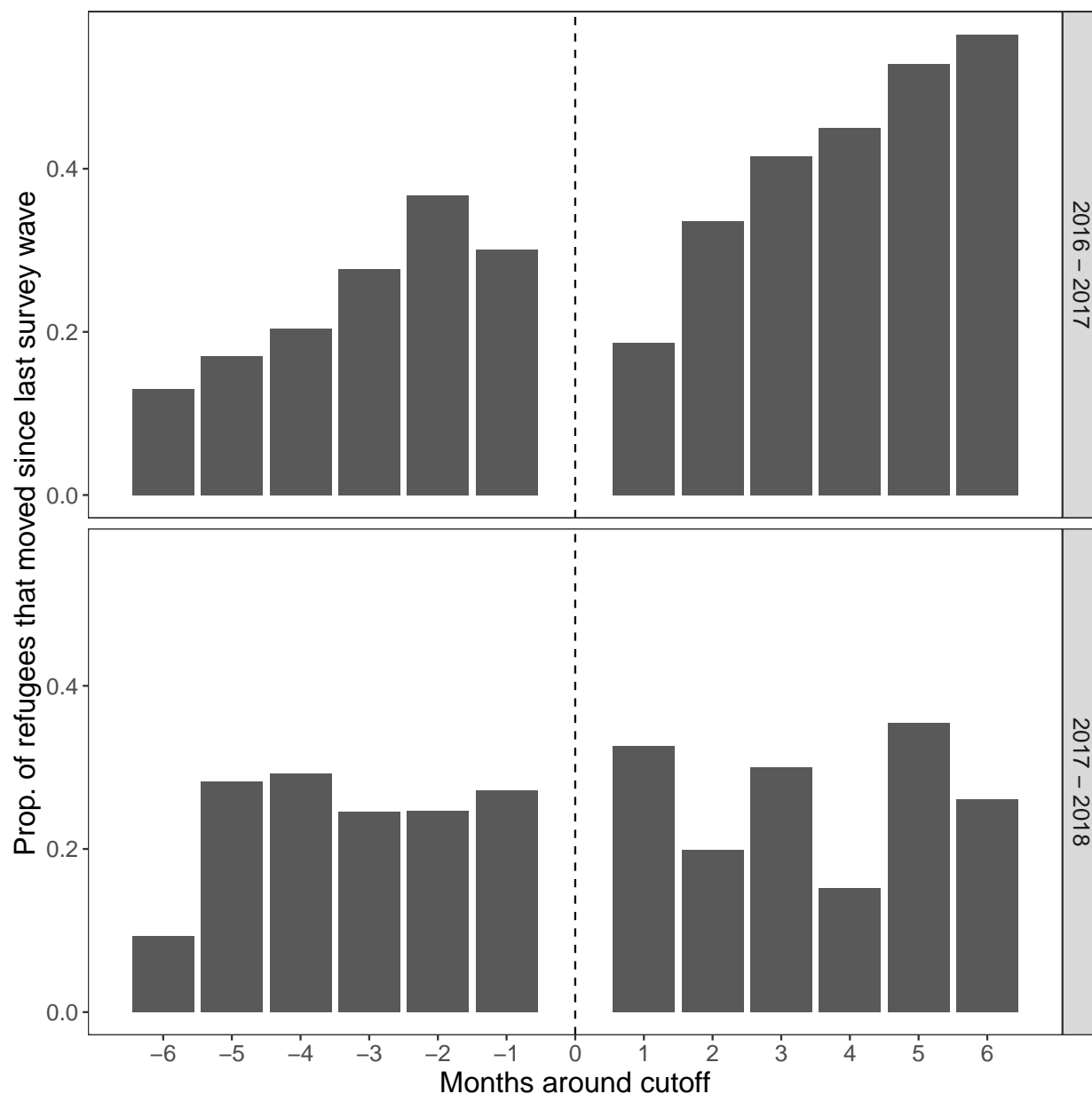
Figure A.1: Covariate balance (raw, across blocks)



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.

A.3 Movement within Germany conditional on asylum approval date

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. The survey item we use 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. We also do not observe the county that refugees were initially assigned to after arrival in Germany.

A.4 Effect on relocations within Germany

As reported in the main body of the paper, we do not find statistically significant differences in the propensity to move between restricted and unrestricted refugees (see figure A.3). Why do movement restrictions have little impact on relocation decisions? Additional restrictions during the asylum decision process and the relatively high cost of relocating may account for this finding. All refugees are prohibited from relocating until their asylum application is approved.⁴ During the often lengthy asylum application process, refugees may have become accustomed to life at the initial assignment location, increasing the cost of relocation after they are permitted to do so. As a result, refugees may find it more convenient to remain at the location they were originally assigned to, even if they are permitted to move. This would suggest a rather low baseline propensity of refugees to move after the initial random assignment to a location. While likely not precisely equal to zero, the treatment effect of the restriction on movement is likely of small magnitude.

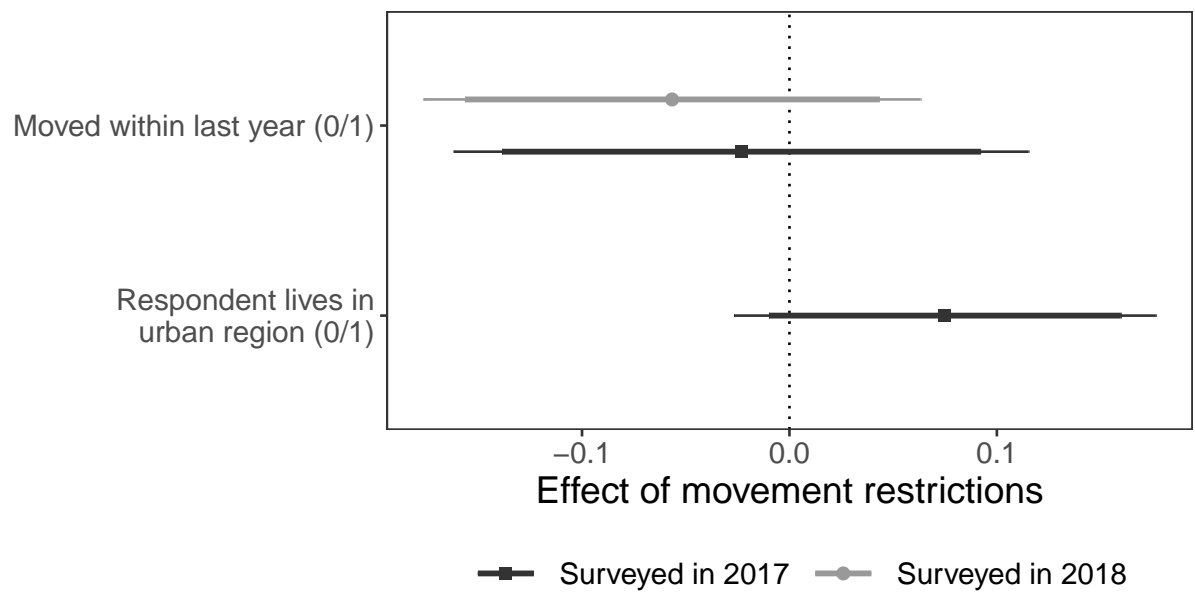
Table A.4: Duration since entry and movement restrictions

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.

⁴Movement restrictions during the asylum application process (*‘Residenzpflicht’*) generally apply for up to three months after arrival in Germany.

Figure A.3: Effect of movement restrictions on relocating and likelihood of living in an urban region



Note: The figure shows estimated effects of being subject to movement restrictions on (i) whether respondents moved since the last survey wave and (ii) whether a respondent lives in an ‘urban’ area. Both outcomes are binary. The SOEP uses the urban/rural definition of the Federal Institute for Research on Building, Urban Affairs, and Spatial Development (BBSR). The item asking whether respondents live in urban areas was only asked in the 2017 survey wave. The horizontal bars represent 90% (thick lines) / 95% (thin lines) confidence intervals.

A.5 Detailed main results

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.

A.6 Robustness

Our identification strategy requires that the exact timing of approval decisions around the January 1 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 analyses 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. If faster decisions are correlated with individual or institutional characteristics, we would expect to find differences between refugees with earlier and later decisions. 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.12 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 2016.⁵ If approval decision timing rather

⁵We 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

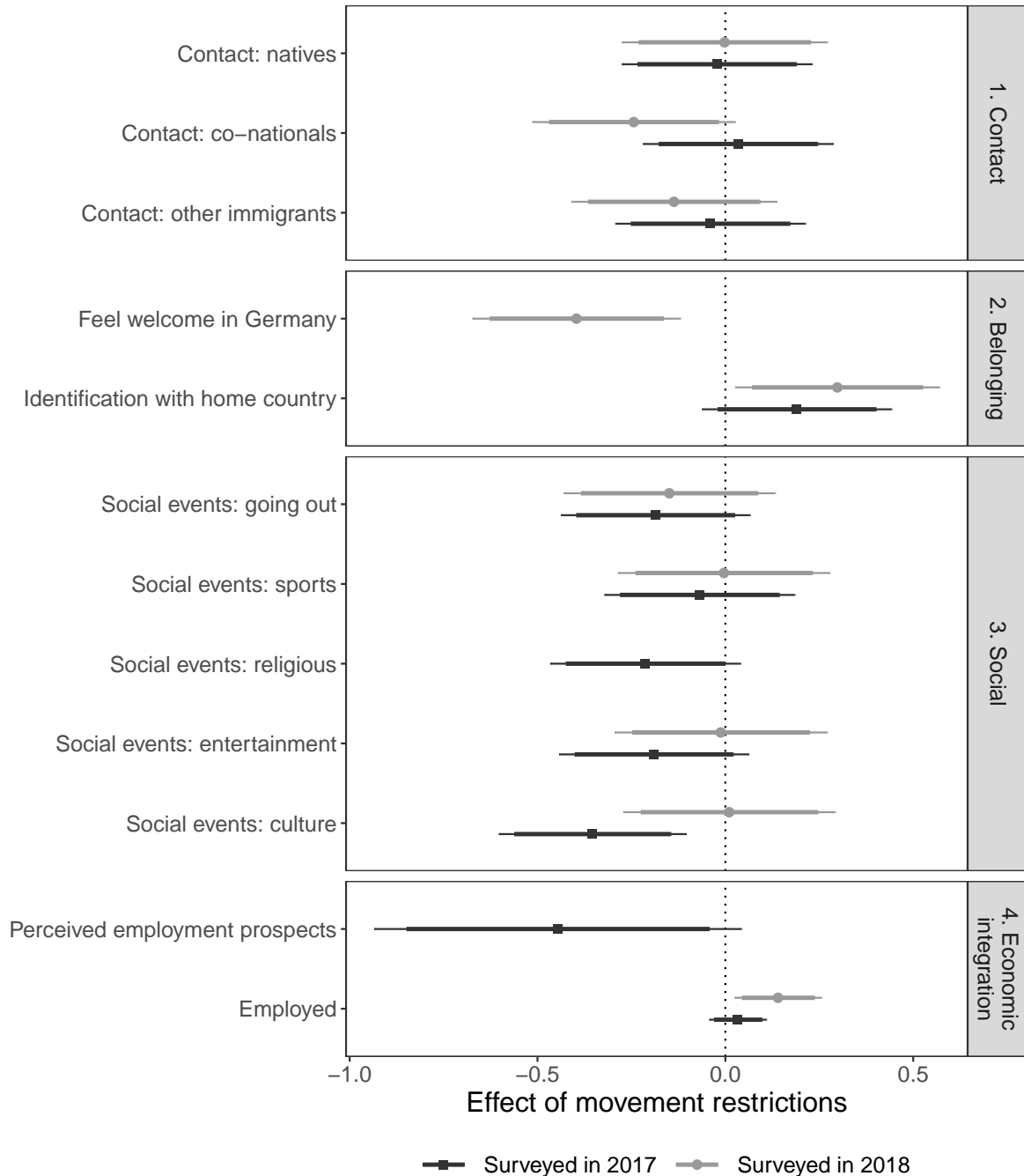
than movement restrictions affects integration, we would expect to find significant differences in this analysis. Reassuringly, we do not find significant differences between restricted and unrestricted refugees before the policy took effect (see figure A.9 in the SI). Taken together, the two supplemental analyses provide strong evidence that slightly faster asylum decisions do not directly affect integration, other than through their effect on movement restrictions.

We conduct four 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 1 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 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. Fourth, we estimated pooled models, where we do not estimate separate effects for the 2017 and 2018 survey waves. Rather, we pool both waves, adding block*year fixed effects and standard errors clustered by respondent. We again find negative effects on both belonging and social integration (see figure A.10 in the SI).

of the law within the states was often delayed by several months.

A.6.1 Omitting block FEs

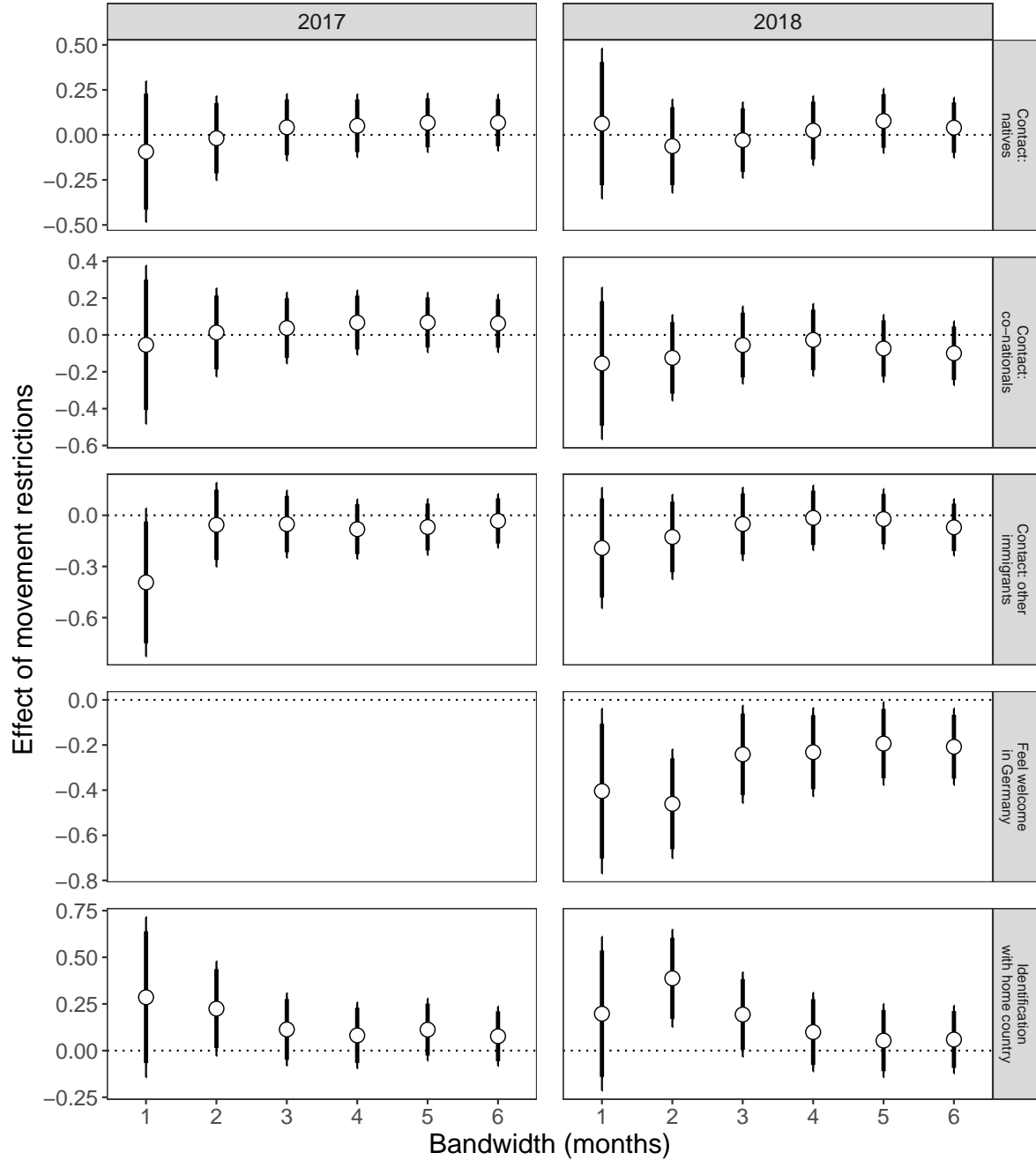
Figure A.4: Effect of movement restrictions, OLS without block FE



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.

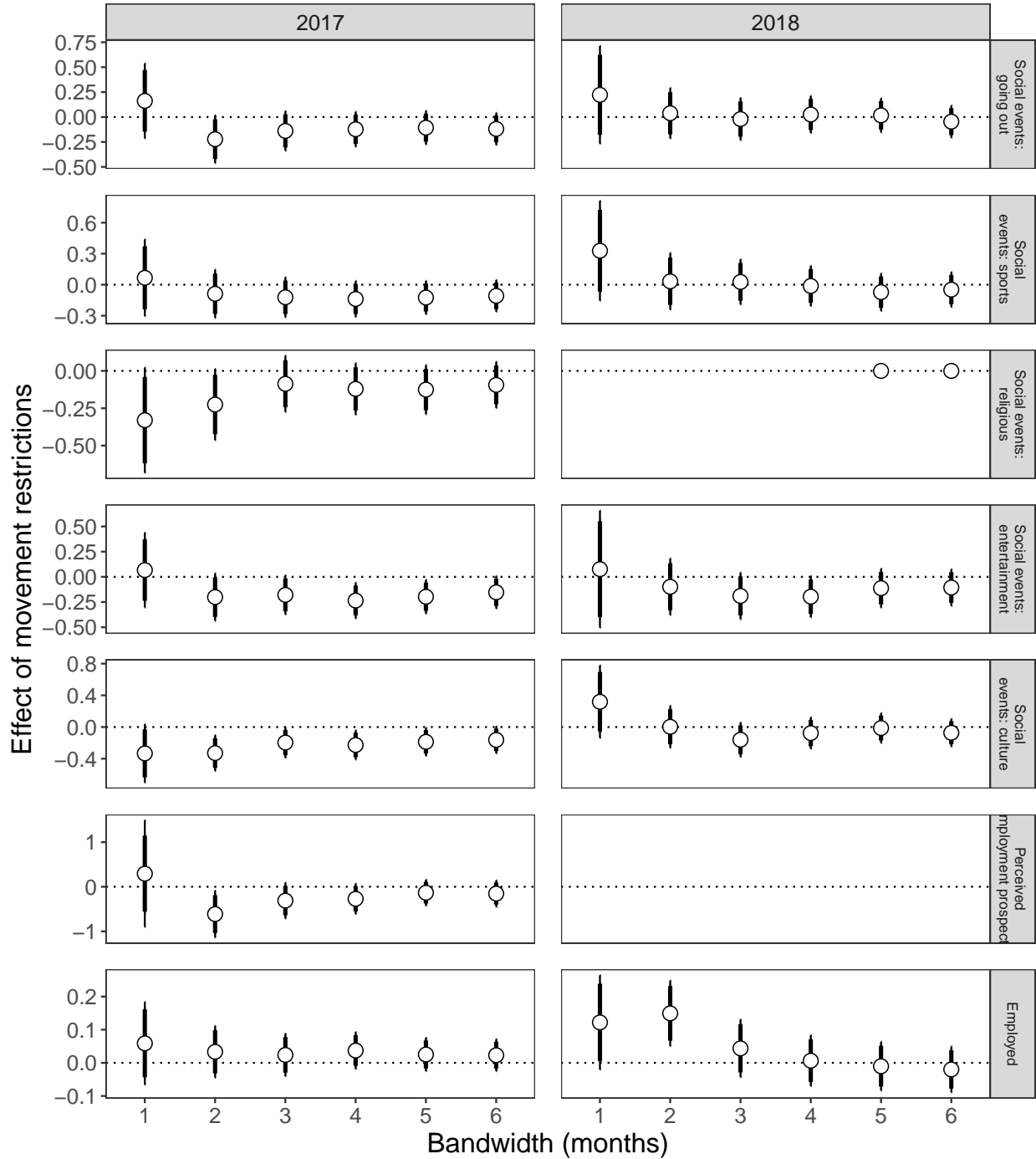
A.6.2 Bandwidth sensitivity

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.

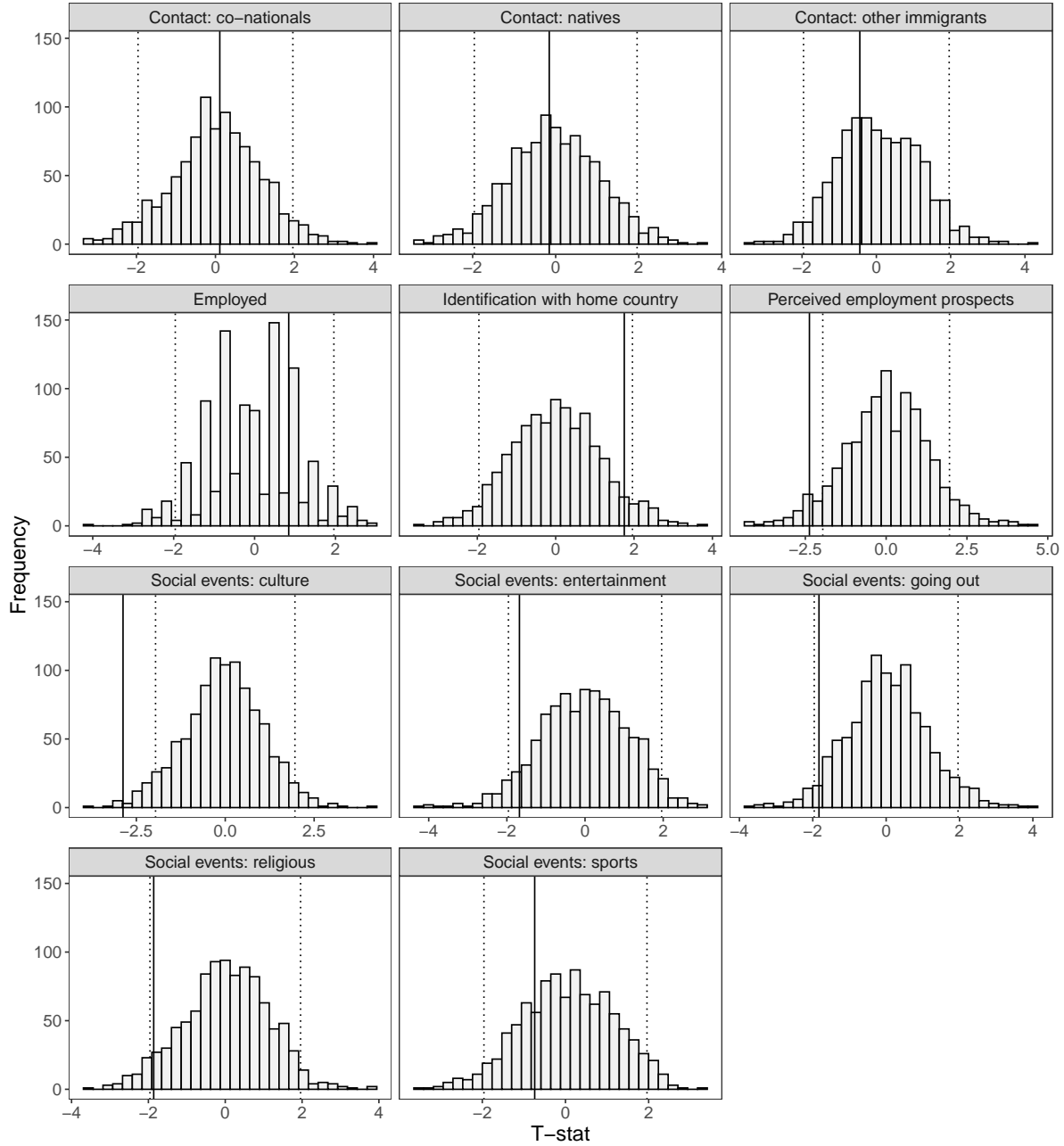
Figure A.6: Bandwidth sensitivity, social and economic integration



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.

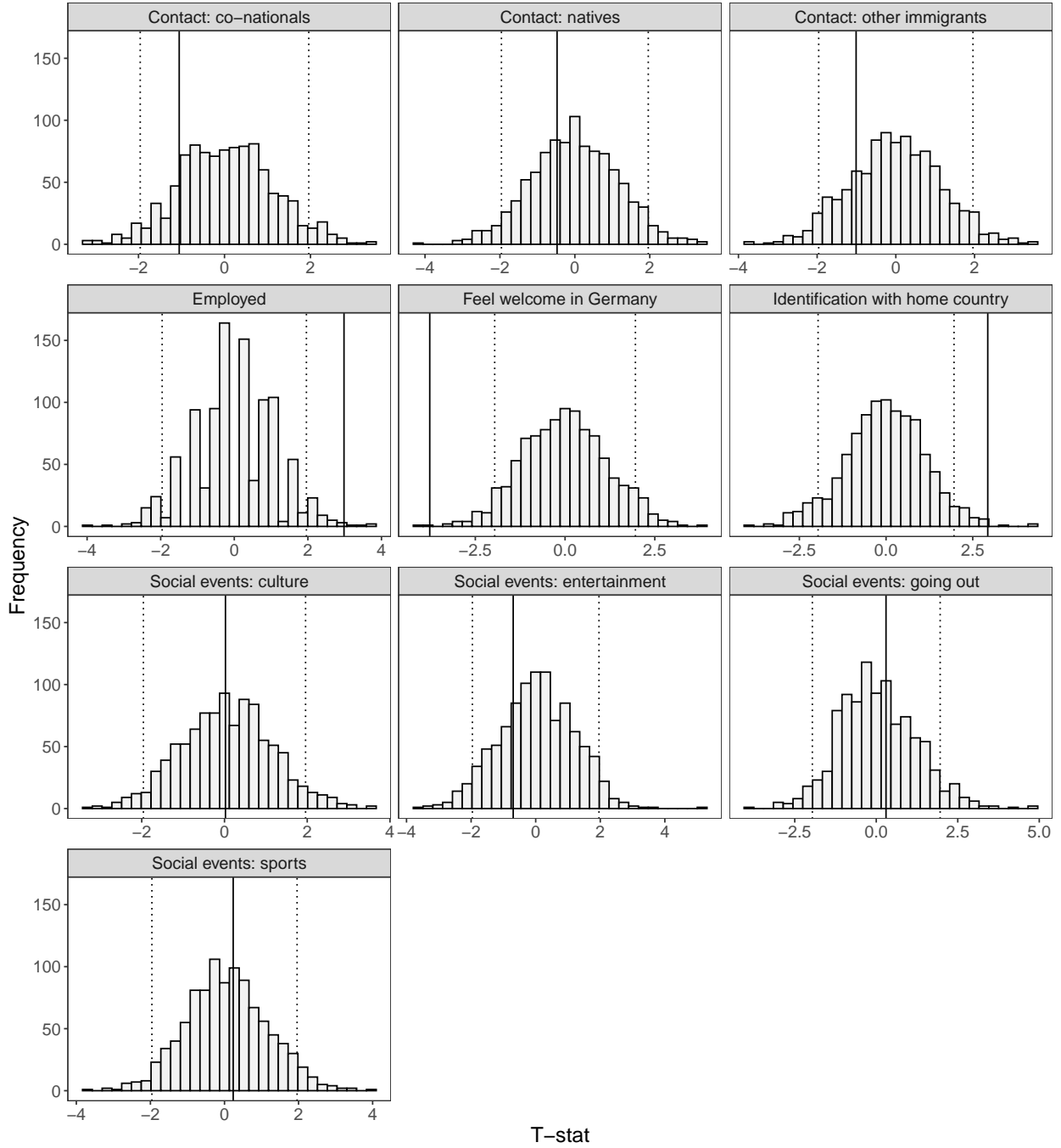
A.6.3 Randomization inference

Figure A.7: Randomization inference, 2017



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 2017.

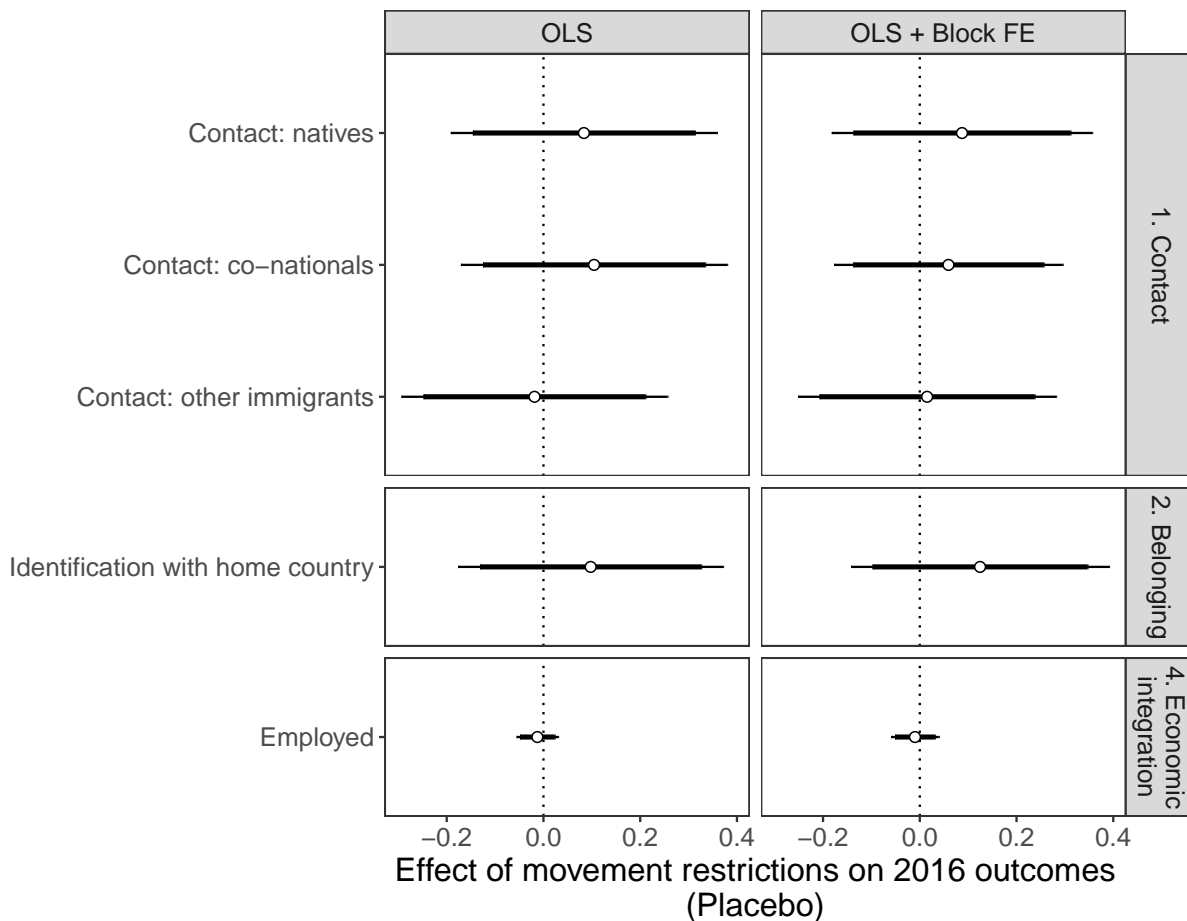
Figure A.8: Randomization inference, 2018



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.

A.6.4 Effect on outcomes measured prior to treatment (placebo)

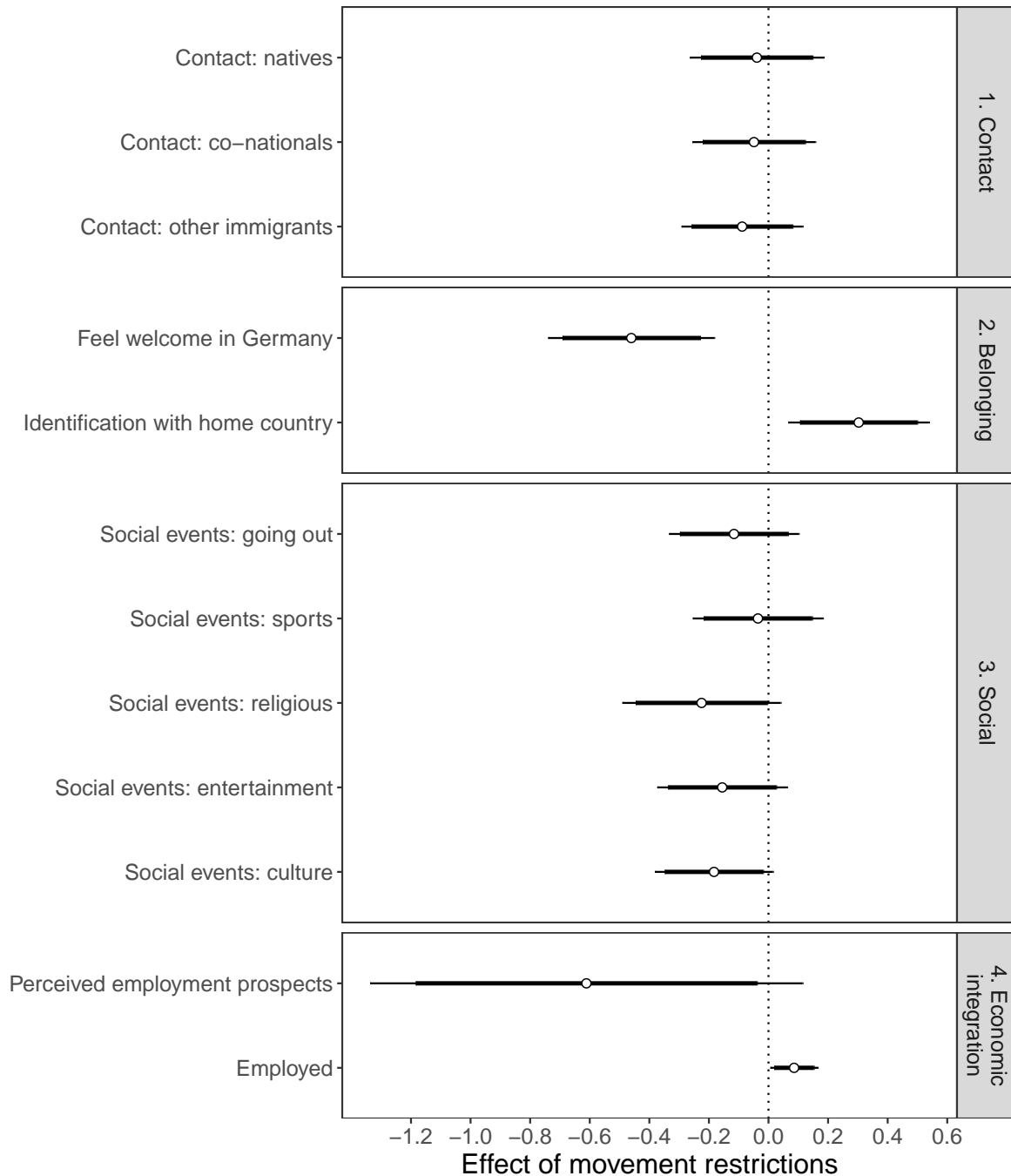
Figure A.9: Effect of movement restrictions on 2016 outcomes



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. The left-hand side panel is a simple OLS specification, the right-hand panel includes block fixed effects. The outcomes were measured as part of the 2016 survey wave of the SOEP panel. A subset of the survey interviews was conducted after the law entered into force in August 2016. However, depending on the federal state, regulations to remain in a specific county were only passed a couple of weeks or months after August 2016. We hence consider this analysis as a test for pre-treatment differences around the assignment cutoff.

A.6.5 Pooling 2017 and 2018 survey waves

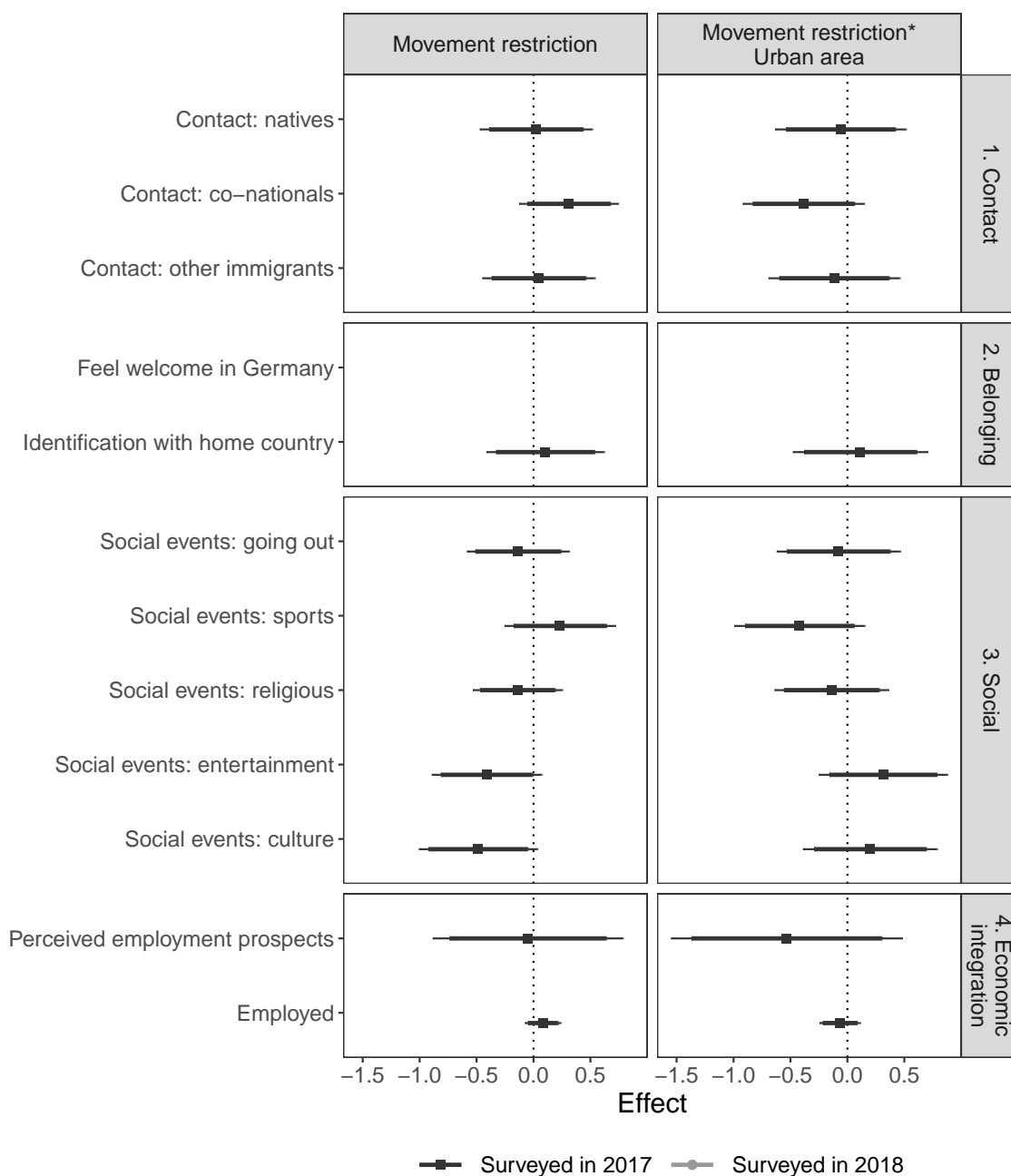
Figure A.10: Effect of movement restrictions, pooling 2017 and 2018 SOEP waves



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. In contrast to the main model, we pool the 2017 and 2018 survey waves. All models include Block*Year fixed effects. We cluster standard errors by respondent.

A.6.6 Interacting movement restrictions with urban/rural status

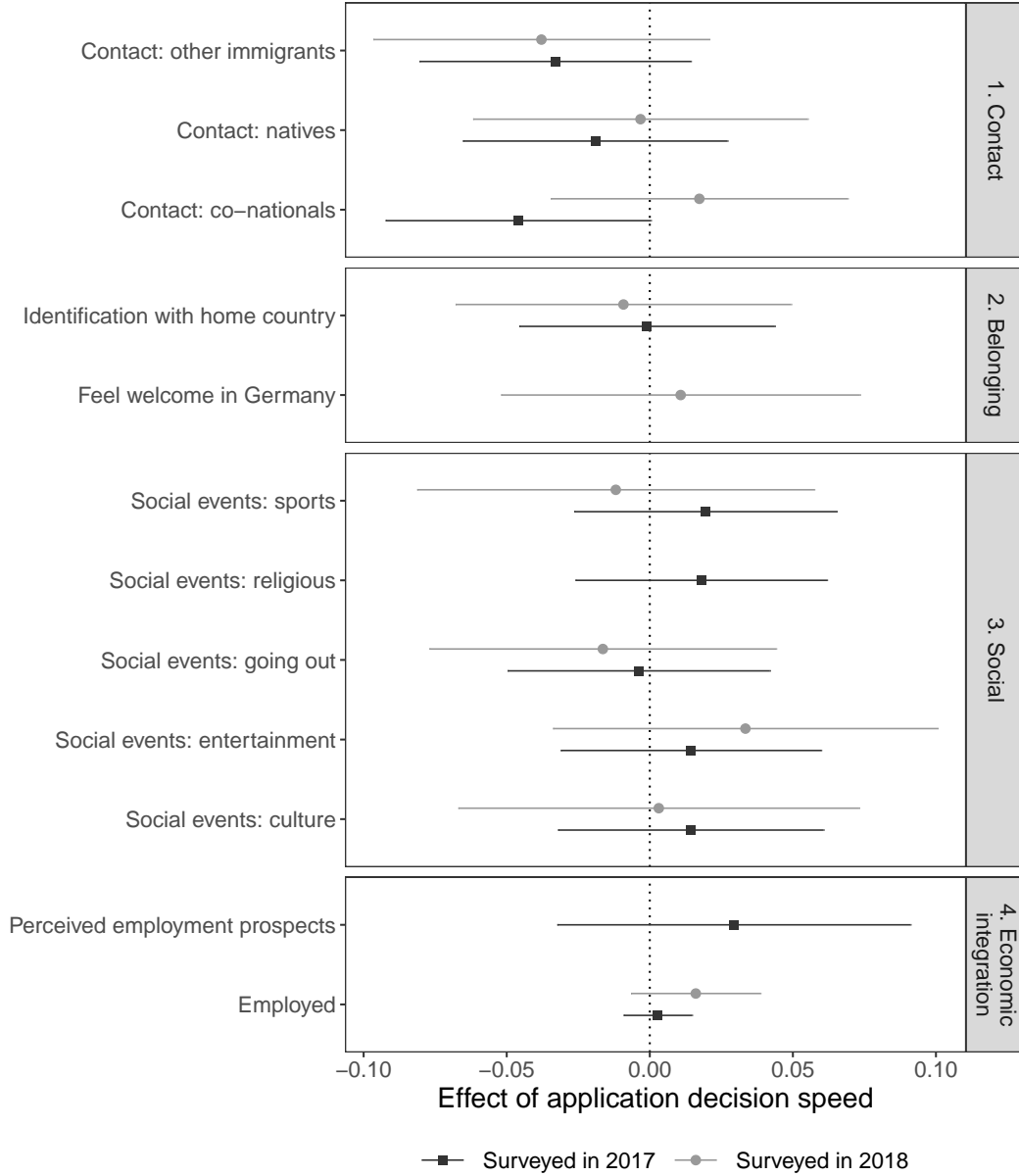
Figure A.11: Interacting movement restrictions with rural/urban status



Note: We present results from interacting movement restriction with a binary rural/urban indicator, as defined by the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR). The left-hand side panel represents the effect of movement restrictions for refugees in rural areas. The right-hand side panel is the restriction*urban interaction. The rural/urban indicator is not available in the 2018 survey wave. 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.

A.6.7 Effect of application decision speed

Figure A.12: 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.