IZA Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 18319

Random Placement but Real Bias

Marco Schmandt
Constantin Tielkes
Felix Weinhardt

DECEMBER 2025

I Z A Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 18319

Random Placement but Real Bias

Marco Schmandt
Technische Universtät Berlin

Constantin Tielkes
Europa-Universtät Viadrina

Felix Weinhardt
Europa-Universtät Viadrina, Berlin School of Economics, IZA, RFBerlin, CESifo and LSE

DECEMBER 2025

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Random Placement but Real Bias*

Many studies exploit the random placement of individuals into groups such as schools or regions to estimate the effects of group-level variables on these individuals. Assuming a simple data generating process, we show that the typical estimate contains three components: the causal effect of interest, ”multiple-treatment bias” (MTB), and ”mobility bias” (MB). The extent of these biases depends on the interrelations of group-level variables and onward mobility. We develop a checklist that can be used to assess the relevance of the biases based on observable quantities. We apply this framework to novel administrative data on randomly placed refugees in Germany and confirm empirically that MTB and MB cannot be ignored. The biases can even switch the signs of estimates of popular group-level variables, despite random placement. We discuss implications for the literature and alternative “ideal experiments”.

JEL Classification: F22, O15, R23

Keywords: random placement, group assignment, peer effects, refugee integration, random dispersal policy

Corresponding author:
Prof. Dr. Felix Weinhardt
Europa-Universtät Viadrina
Lehrstuhl für Public Economics
Große Scharrnstraße 59
15230 Frankfurt (Oder)
Germany
E-mail: weinhardt@europa-uni.de

* We are grateful for comments and suggestions from, Fabian Bald, David Green, Martin Koenen, Jan Nimczik, Dan-Olof Rooth, Olmo Silva, Axel Werwatz and participants of the 40th meeting of the European Economic Association, the 14th European Meeting of the Urban Economics Association, the 2025 VfS annual conference, and of the BENA summer workshop at the Rockwool Foundation Berlin. Weinhardt gratefully acknowledges research funding (DFG grant 518302089). All remaining errors are our own.

1 Introduction

Endogeneity concerns, and in particular individual selection, often complicate causal analysis. In response, many empirical studies in economics exploit settings where individuals are randomly or quasi-randomly placed into groups to study the effect of a particular (group-level) variable on later outcomes of the randomly placed individuals. Such a strategy is commonly used in the literature of peer or neighborhood effects on education or health outcomes, or in the context of the integration of refugees, who are randomly allocated to initial residence places through random dispersal policies (RDP).

The central theoretical and empirical finding of this study is that the causal interpretation of such estimates of any group-level variable on later outcomes requires remaining assumptions, even if there is initial random placement. Although these assumptions are not completely novel (we review the related literature in detail below), we are the first to provide a theoretical framework and empirical analysis that systematically examines their importance. We provide a checklist that allows researchers to investigate the presence of biases and apply our framework to the study of the integration of randomly placed migrants in Germany, Europe's largest refugee-hosting country. Utilizing novel administrative data, our headline empirical result is that the maintained assumptions are indeed highly relevant: we are able to shift the typical estimates of local conditions, despite the random placement, and change their sign.

Random placement of course does break the link between individual characteristics and group characteristics, as frequently demonstrated by "randomization tests" or "balancing tables". This is a great advantage over purely descriptive studies. But estimates from random placement require maintained assumptions, which can be classified into two groups. First, local group measures can be interrelated – in observed and unobserved ways. Consider the following example in the context of the estimation of peer effects: random placement into classes/groups does not generate random variation in, say, average peer achievement or the peer gender composition. It *only* randomly assigns group membership. And for the randomly placed refugee, the local residence is randomly assigned, but not for example the unemployment rate at the local residence. This subtle distinction means that controls for *other* group characteristics or local conditions in integration studies can move the estimates. We label this influence of group-level control variables as "multiple treatment bias" (MTB).[1]

The second obstacle to causal interpretation is onward/ secondary mobility, which can be related to measures of local conditions at the group or place of initial (random) placement, resulting in "mobility bias" (MB). We use our model to show that MB results in attenuation bias (due to mean reversion) under random onward mobility. However,

[1]Another way to see this is the following: the same random placement experiment is often used to estimate "causal" effects of different right-hand side group-level variables (literature review below). One treatment cannot inform multiple estimates without additional assumptions.

whenever onward mobility is not exogenously assigned or an outcome of initial-group characteristics (i.e. you change group because you do not like the first group), this can induce additional selection bias in the estimates of effects of specific group characteristics, even when groups were initially assigned randomly.

Our key theoretical result is that the typical random placement estimate of the influence of a specific group-level variable contains all of the following elements: the causal effect of interest, MTB, MB-attenuation, an interaction of MTB and MB, and selection bias from non-random onward mobility. This finding is conditional on full compliance with the initial randomization and our simplistic model/production function. We use our framework to derive the conditions that give rise to MTB and MB, which are partly observable and can therefore be used to inform the analysis. Both depend on the co-movements of variables at the initial group or location with (a) other variables and (b) onward mobility, and variables at the next group/location. We provide a checklist that can be used by random placement studies to assess the severity of MTB and MB in any given setting.

To underscore the relevance of our theoretical result empirically, we complement our argument with an analysis based on a novel administrative panel dataset, the German registry of foreigners (AZR), which contains the individual migration histories of more than 69,000 refugees who arrived in Germany between 2015 and 2018. This German setting is particularly informative because of several reasons: migrants were initially placed randomly, and the data allow us to precisely observe each individual's location and integration outcomes over time. Registration with the authorities is mandatory so that onward mobility induces zero sample attrition—an advantage of this administrative data source over household-level survey data. Moreover, the German case is important in its own right: Germany is the third-largest refugee-hosting country worldwide, and the largest of the European Union (UNHCR, 2025).

From the point of view of our checklist, the German random placement setting turns out to be a challenging situation to identify local variables that matter for refugee integration. We find only weak correlations between individual characteristics and (initial) local characteristics, as expected in an RDP setting. However, we find strong correlations between different local variables, and with mobility. This means that both MTB and MB likely matter in the German setting.

Our empirical analysis confirms this. Regarding MTB, we find that estimates of the effect of local conditions on a measure of integration depend on the set of regional control variables included in the regression specifications. For example, the estimates for the effects of the local unemployment rate or the share of the local AfD votership—a populist right-wing party in Germany—in the randomly assigned location on our integration measure both switch signs depending on the exact specification.

Regarding MB, we show that secondary migration also influences estimation results.

We show for a broad range of empirical specifications and under a set of highly stylized assumptions for the most popular local integration factors of the RDP literature that MB attenuates estimates substantially. The bias depends on prevalence of secondary migration and the change of local integration conditions that accompany secondary migration. MB differs for different regional covariates in the same empirical setting even under our very restrictive assumptions, i.e. comparing coefficients can be difficult even within the same empirical setting. This means that the seemingly attractive German empirical setting cannot be used to generate meaningful estimates of group-level variables for integration, based on the typical "random placement" approach.

Is this empirical finding specific to the German setting? We examine the correlation of typical group-level variables for the literature on refugee integration. Using additional data on local integration conditions at the NUTS-3-level we estimate varying correlations for five countries that implemented random dispersal policies. For example, the regional correlation of the right-wing vote share with the unemployment rate is positive in Germany, close to zero in Denmark and Sweden, and negative in Switzerland. Without this information, results between different studies and settings are not comparable, which has not been appreciated enough by the existing literature.

In our conclusions, we discuss implications regarding the "ideal experiment" for the estimation of group-level effects on individuals. Some of these do not require random placement, which opens up perspectives for future work in settings where random placement, which is a rare event, is not present.

Methodologically, a seminal study in the literature on how to study causal effects of group variables on individual outcomes is provided by Manski (1993). Many empirical papers focus on estimating effects of group-averages $\bar{X}$ on individual outcomes Y, Manski (1993)'s "exogenous effects". Other measures besides group averages include for example ranks or networks, which also vary at the individual level. For simplicity, we refer to these as well as group/local measures. This literature frequently discusses (i) if random placement generates enough variation in the measure of interest, (ii) if these should be computed with an individual leave-out, or (iii) the role of measurement error, in particular when not everyone is observed, see Angrist (2014) for an overview. These are important issues but we argue that the idea that random placement constitutes an "ideal experiment" to estimate the influence of a group variable on individual outcomes is flawed to begin with, as this assumes MTB and MB away. This misperception can wrongfully result in a triadic recipe: motivate a (novel) local/group factor; show balancing of this variable with individuals characteristics due to random placement; show "causal" estimates. We show theoretically, and also empirically for the case of refugee integration, that estimates that rely on such a research design are highly sensitive to the remaining biases that we discuss.

Consequently, our paper relates to a large body of empirical work that relies on random placement to estimate various effects of interest. A prominent example is the literature in economics that explores which local integration factors enhance the integration of migrants. As pointed out by Foged et al. (2024), the most credible estimates in this literature rely on settings with random dispersal policies. In this RDP literature, the main Y is the labor market success of refugees, often measured by labor market participation or wages. For the right-hand side, the most studied local integration factor is the quality of the local labor market, measured by the local (un)employment rate, see e.g. Foged et al. (2024); Azlor et al. (2020); Damm and Rosholm (2010) for Denmark; Müller et al. (2023) for Switzerland; Godøy (2017) for Norway; Aslund and Rooth (2007) for Sweden; Aksoy et al. (2023); Schilling and Stillman (2024); Tsolak and Bürmann (2023) for Germany.[2] The second most studied local integration factor that influences the labor market success of refugees is the size of the co-ethnic network: e.g. Damm (2009b) for Denmark; Beaman (2012) for the U.S.; Edin et al. (2003) for Sweden; Martén et al. (2019) for Switzerland; Battisti et al. (2022) for Germany. Other local factors are the availability of language courses (Kanas and Kosyakova, 2023), the local sentiment of natives (Aksoy et al., 2023), and the local presence of the gig economy (Degenhardt and Nimczik, 2025).[3] We provide an overview in Appendix Table A1. How does this literature deal with MTB, i.e. the fact that one treatment (random placement) does not identify causal effects of multiple local factors? Not systematically. Some studies do not even control for *any* other local factor, including those that are being used as RHS variables in other RDP studies. In our empirical application, we estimate the most common specifications of this literature and also iterate over possible combinations of local controls. This generates sizable coefficient movements for effects of the four most commonly used local integration measures of the literature on the right-hand side, and their effects on the integration of randomly placed refugees in Germany.[4]

Another literature relying on RDP instruments is the economics literature on the internal migration of refugees, which relates directly to MB. This literature has studied how onward mobility of refugees is related to assignment place characteristics *after* the initial random placement. Early studies again focus on the Scandinavian countries (Aslund,

[2]Some papers rely on alternative measures for the quality of the local labor market: Wett et al. (2024).

[3]There are also variations in Y, beyond labor market integration, e.g.: Jaschke et al. (2022) explore how local sentiment measured by vote shares for right-wing parties influences assimilation; Schilling and Stillman (2024) how native attitudes shape social integration; Kuhn and Maxwell (2023) how the local ethnic composition affects how welcomed refugees feel; Schilling and Höckel (2025) how language course supply affects educational outcomes; and Biddle and Bozorgmehr (2024) study the impact of social deprivation on refugees' mental health.

[4]A related literature uses quasi-random inflows to generate variation in local factors, and uses this variation to estimate effect on "natives/stayers". Such an approach is viable when not everyone is randomly assigned (i.e. to classrooms) but when a smaller number of individuals is randomly allocated to a larger group of "natives/stayers". This approach does not suffer from the same biases as the literature discussed here, and has been used to study neighborhood effects (e.g. Gibbons et al., 2013, 2017), peer effects (e.g. Imberman et al., 2012), labor markets (e.g. Glitz, 2012; Dustmann et al., 2016), electoral outcomes (e.g. Dustmann et al., 2019), cultural norms (e.g. Miho et al., 2023; Jessen et al., 2023), or house prices and amenities (Glitz et al., 2023).

2005; Damm, 2009a, 2014; Damm and Rosholm, 2010) and the Netherlands (Zorlu and Mulder, 2008), while later studies examine the German case (Baba et al., 2023; Weber, 2023; Wiedner and Schaeffer, 2024), Austria (Wett et al., 2024), the United States (Mossaad et al., 2020), or Europe (Fasani et al., 2022). This literature examines how *the same* local characteristics considered by the RDP integration literature drive secondary migration of refugees. Based on our theoretical arguments and empirical analysis we will conclude that it is imperative to also consider secondary migration when deriving conclusions about causal effects of local integration conditions from RDP-instrument-settings.[5]

As aforementioned, variants of MTB and MB have not been fully ignored. For instance in the education literature Golsteyn et al. (2021) carefully control for a large set of peer characteristics when interpreting peer effects of personalty in a random placement setting, and discuss the resulting coefficient (non-) movement. In an earlier non-experimental study, Lavy et al. (2012) discuss how estimates of mean ability peer effects change when additionally including measures of very high and very low achieving peers. But many studies rely primarily on random placement and balancing/randomization tests with respect to individual characteristics to defend their estimates. Bertoni and Nisticò (2023) demonstrate for the case of ability peer effects that this is not enough. They show that even experimental (random placement) estimates of mean-ability peer effects change with the inclusion of measures of a student's rank as additional RHS variable. This is one specific example of MTB, using the terminology we have introduced. To what extent MB affects estimates in the education literature, to the best of our knowledge, remains underexplored. For the case of randomly placed refugees, Eckert et al. (2022) formalize MB for place-based effects. We present a framework for MB for the estimation of group-level variables on later outcomes, which, in addition, also considers how MTB affects estimates. Based on our analysis, and the empirical findings on the relevance of MTB and MB in the German random placement setting, we conclude by discussing requirements, as well as alternative "ideal experiments" for future work on how group-specific variables affect individual outcomes. The most promising avenue for future work is settings with as-good-as random variation in group-level variables. These settings do not necessarily require initial random placement.

[5]Of course, there exist many more literatures that rely on versions of the Manski (1993) framework and (quasi-)random placement, which we cannot comprehensively review here. One example comes from the economics of education, where studies either rely on explicit controlled experiments, e.g. Sojourner (2013), who study effects of peer quality using project STAR, or settings where institutional assignment mechanisms lead to exogenous group membership, e.g. Sacerdote (2001) to study effects of room-mates, Carrell et al. (2009) or Feld and Zölitz (2017) to study effects of the academic achievement of peers, Goulas et al. (2023) of the gender peer composition, or Golsteyn et al. (2021) of peer personality. A related literature studies peer effects on health outcomes using quasi-random group assignments, i.e. Trogdon et al. (2008); Jeong (2025).

2 Identification with random placement

2.1 Group (or place) effects

We first want to clarify the nature of the treatment under random placement and the types of group-effects (rather than of group-level variables) that can be identified.

We generalize our argument to any setting where individuals can influence group membership but for exhibition consider a refugee. Such a refugee might choose a region of residence, not a level of unemployment or other specific local characteristics which the researcher is interested in. Since this is the case, the treatment is properly described as being multi-valued, discrete and unordered. This choice is likely endogenous. Therefore, researchers have recognized that group assignment might help to infer causal evidence on group(-level)-effects.

If group assignment were random and linked to the outcome of interest only through its influence on the location (the group choice) of the individuals, it could be used as an "instrumental variable" to study discrete group-effects. Such a situation, where the endogenous treatment and the as good as randomly assigned instrument are both multi-valued discrete and unordered is encountered frequently in economics. For such situations, causal effects of interest and the assumptions that are needed to estimate them are theoretically well explored (Heckman et al., 2008; Heckman and Pinto, 2018).

We formalize the setting. Individuals i, observed at time t choose between groups $d_{it} = 1, ..., D$. This group choice is endogenous, but the researcher observes group assignment $z_i = 1, ..., D$.[6] In many empirical applications z_i is correlated with d_{it}. If we were interested in the average of our outcome, Y_{it}, for each group of assignment, we could regress the individual outcome on dummy coefficients indicating the group:

$$Y_{it} = \sum_{z=1}^{D} 1(z_i = z) \cdot \delta_z + \epsilon_{it} \tag{1}$$

where OLS regression estimates for the group dummy variables indicating the assignment to group z_i would be equal to $\hat{\delta}_z = \hat{E}[Y_{it}|z_i = z]$, that is, they are equal to the average outcome of individuals who were assigned to group z_i.

What kind of effects can be identified in such as setting? This depends on onward mobility. With subsequent mobility, random group assignment can influence outcomes in many ways, through the group of assignment, but also through the effects of the other groups, if individuals move there subsequently. Thus, the effects of groups other than the randomly assigned group cannot in general be excluded from the outcome equation

[6] In many settings, e.g. for the local placement of refugees, the assignment is in fact not observable to the researchers. Instead they use the location (group) where they first observe the individuals. Non-compliance with (unknown) assignment will cause additional compliance problems which we abstract from in this paper.

even under random group-placement. This is because individuals can receive different treatment "dosages", i.e. the stable unit treatment value assumption (SUTVA) is violated. In relation to IV, this is a situation where non-compliers, i.e. individuals who move, receive a different treatment –rather than no treatment– from their next group. This means that when individuals can change groups before the outcome is realized, local average treatment effects (LATE) and other average treatment effects cannot be identified.

By virtue of random placement, what equation 1 identifies are reduced form ("intention-to-treat" – ITT) estimates of assignment groups. These ITT effects of group assignment should not be confused with causal estimates of group membership, unless there is zero onward mobility.

2.2 Group-level variables with no subsequent group changes

Even with no subsequent mobility, such group effects are rarely estimated. The information that a certain group results in high productivity, for example, can be relevant, but in many settings researchers have considered it as more interesting to understand *why* certain group membership produces a desired result. Therefore, the focus of the literature has been and our focus here is on how to obtain causal estimates of group-level variables, rather than of group membership, on individual outcomes.

And this is where much of literature that relies on random placement departs from the setting with well-identified ITT place-based effects. In this literature, we typically introduce another continuous variable (namely, a measure of some local/group feature) that is correlated with the ordinal treatment and ordinal instrument, and use this continuous variable instead of the original ordinal instrument to estimate some sort of (supposedly causal) effect of interest.[7] Denote these observed group variables as $Z_{ap}^{obs,k}$, where $k = 1, ..., k^{obs}$, and where a denotes the group/region of assignment and p the cohort of individual i. Note that here, and in the following, we think of the outcome generation as being stratified on individual characteristics, and also by the cohort of the individual, p. This is not necessary but we introduce this notation here so that we can later relate directly to the literature that uses across-cohort variation, rather than cross-sectional variation.

Given the above reasoning, and taking the nature of the treatment and instrument into account, we note that not only observed but potentially also other, unobserved group variables $Z_{ap}^{unobs,k}$, with $k = 1, ..., k^{unobs}$, are randomly allocated in the initial placement

[7] It can be shown that the estimates from a regression using a continuous group-level characteristic can be described as (weighted) averages of the actual "binary" treatment effects as described in Heckman et al. (2008).

of individuals. Under these conditions the outcome can be written as:

$$\begin{aligned} Y_{itap} &= \sum_{z=1}^{D} 1(z_i = z) + \epsilon_{it} \\ &= g(\boldsymbol{Z_{ap}^{obs}}, \boldsymbol{Z_{ap}^{unobs}}) + \epsilon_{itap} \end{aligned} \tag{2}$$

Suppose now (as does implicitly a large literature e.g. in the economics of migration or education) that the average outcome could be decomposed linearly into these observed (and unobserved) group variables. We can then rewrite the outcome as

$$\begin{aligned} Y_{itap} &= \sum_{k=1}^{k^{obs}} \delta_1^k Z_{ap}^{obs,k} + \sum_{k=1}^{k^{unobs}} \delta_2^k Z_{ap}^{unobs,k} + \epsilon_{it} \\ &= \boldsymbol{Z_{ap}^{obs}} \boldsymbol{\delta_1} + \boldsymbol{Z_{ap}^{unobs}} \boldsymbol{\delta_2} + \epsilon_{itap} \end{aligned} \tag{3}$$

where the matrices $\boldsymbol{Z_{ap}^{obs}}$ and $\boldsymbol{Z_{ap}^{unobs}}$ are $n \times k^{obs}$ and $n \times k^{unobs}$ matrices and $\boldsymbol{\delta_1}$, $\boldsymbol{\delta_2}$ are $k^{obs} \times 1$, $k^{unobs} \times 1$ coefficient vectors, and where it holds that $Cov(\boldsymbol{Z_{a,p}^{obs}}, \epsilon_{it}) = Cov(\boldsymbol{Z_{a,p}^{unobs}}, \epsilon_{it}) = 0$, i.e. the observed and unobserved group variables linearly map to the average effect of being assigned to a group/region a. It is by no means clear that the linear decomposition in equation 3 can be made. In the following we only think of the decomposition as being linear to keep our argument close to applications in the literature and simple. We now consider onward mobility, before providing a unifying interpretation of typical random-placement estimates.

2.3 Group-level variables with subsequent group changes

To better understand how onward mobility can affect random placement estimates of specific local characteristics, let us now consider a highly stylized theoretical framework of how the individual outcome Y_{it} of interest is being generated in the presence of additional mobility. We denote mobile individuals by $M = 1$ and immobile individuals by $M = 0$. Then we can write the potential outcome in the case where an individual does not move:

$$Y_{itap}^{M=0} = g(\boldsymbol{Z_{ap}^{obs}}, \boldsymbol{Z_{ap}^{unobs}}) = \boldsymbol{Z_{ap}^{obs}} \boldsymbol{\delta_1} + \boldsymbol{Z_{ap}^{unobs}} \boldsymbol{\delta_2} + \epsilon_{itap}^0 \tag{4}$$

That is, the potential outcome of individuals that do not move is determined solely by the characteristics of the groups at the time of initial placement, i.e. here later changes of group characteristics do not influence the outcome by assumption. In turn, mobile individuals chose to move at the point in time t_m to another group r. Assuming that they only move once and that only the group variables at the time of placement p and the time of the move t_m determine outcomes we have

$$Y_{itapr}^{M=1} = f(\boldsymbol{Z_{ap}^{obs}}, \boldsymbol{Z_{ap}^{unobs}}, \boldsymbol{Z_{rt_m}^{obs}}, \boldsymbol{Z_{rt_m}^{unobs}}, \epsilon_{itapr}^{1}) \tag{5}$$

Under the very restrictive assumptions of linear effects of the local group variable on the outcome of interest over time, and constant effects of groups, we can write the potential outcome if the individual moves as:

$$Y_{itapr}^{M=1} = (\frac{t_m}{t})[\boldsymbol{Z_{ap}^{obs}\delta_1} + \boldsymbol{Z_{ap}^{unobs}\delta_2}] + (1 - \frac{t_m}{t})[\boldsymbol{Z_{rt_m}^{obs}\delta_1} + \boldsymbol{Z_{rt_m}^{unobs}\delta_2}] + \epsilon_{itapr}^{1} \tag{6}$$

This allows us to rewrite the outcome Y_t as:

$$\begin{aligned}
Y_{itapr} &= Y_{it}^{M=0} \cdot (1 - M) + Y_{it}^{M=1} \cdot M \\
&= \boldsymbol{Z_{ap}^{obs}\delta_1} + \boldsymbol{Z_{ap}^{unobs}\delta_2} \\
&+ M \cdot \{(\frac{t - t_m}{t})[\boldsymbol{Z_{rt_m}^{obs}\delta_1} + \boldsymbol{Z_{rt_m}^{unobs}\delta_2}] - (\frac{t - t_m}{t})[\boldsymbol{Z_{ap}^{obs}\delta_1} + \boldsymbol{Z_{ap}^{unobs}\delta_2}]\} \\
&+ (\epsilon_{it}^{0} + M(\epsilon_{it}^{1} - \epsilon_{it}^{0})) \\
&= \boldsymbol{Z_{ap}^{obs}\delta_1} + \boldsymbol{Z_{ap}^{unobs}\delta_2} + M \cdot \alpha \cdot [\boldsymbol{\Delta_M^{obs}\delta_1} + \boldsymbol{\Delta_M^{unobs}\delta_2}] + \epsilon_{itapr}^{M}
\end{aligned} \tag{7}$$

where $\alpha = \frac{t-t_m}{t}$, $\boldsymbol{\Delta_M^{obs}} = \boldsymbol{Z_{r,t_m}^{obs}} - \boldsymbol{Z_{a,p}^{obs}}$ and $\boldsymbol{\Delta_M^{unobs}} = \boldsymbol{Z_{r,t_m}^{unobs}} - \boldsymbol{Z_{a,p}^{unobs}}$ are the changes of local characteristics between period p and moving time t_m, i.e. the changes of local characteristics that accompany a move, and ϵ_{itapr}^{M} is a composite error term.

This highly stylized framework drives home a few crucial points about how and which local group conditions influence individual outcomes. We want to emphasize that in general $\epsilon_{itapr}^{M} = \epsilon_{itapr}^{M}(Z^{obs}, Z^{unobs}, M, \alpha, \Delta_M^{obs}, \Delta_M^{unobs}, \epsilon_{itapr}^{0}, \epsilon_{itapr}^{1})$, i.e. the error term could in principle be related to any of the other determinants in this outcome equation. There are multiple reasons why this might be the case: maybe only highly motivated individuals move again, or they move to selected new groups? Such selection concerns can exist in the presence of initial random placement, which only relates to ϵ_{itapr}^{0}. To be precise, if one wanted to estimate such a model, for identification we require ϵ_{itapr}^{M} to be independent of the included right-hand side variables. This means, essentially, that onward mobility cannot be an outcome of initial-place characteristics, and that the decision to move at all, M, or when to move, α, or where to move to, Δ_M^{obs} and Δ_M^{unobs}, equally need to be unrelated to (individual) characteristics that matter for integration. Such assumptions can be summarized as "random onward mobility".

In the following, we use this simple theoretical model to study how multiple treatments, moving decisions, and the associated changes in group-level variables, can influence estimation results that are based on the typical random placement specifications, with and without random onward mobility.

2.4 Typical estimation and interpretation in the literature

One strand of the random placement literature relies on cross-sectional variation of observed group variables on a context-dependent outcome Y of individual i in group a and cohort p. This literature estimates a regression of the following form:

$$Y_{itapr} = \delta_0 + \sum_{k=1}^{k^{obs}} \delta_1^k Z_{ap}^{obs,k} + \beta X_i + \phi_p + \eta_{itapr} \tag{8}$$

The typical regression includes a limited number k of observed local indicators Z_{ap}. Other observed time-invariant characteristics of individuals X_i and cohort fixed effects ϕ_p are usually included, as well as an error term. δ_1 is the estimate of interest. In the peer effects education literature that relies on random placement, the group characteristics Z_{ap} could be measures of a specific peer characteristic at the classrooms or school-level, such as mean ability, and the individual characteristic X_i a lagged outcome, e.g. test scores from an earlier period and school, gender, etc. In the refugee integration literature, groups are regions or local labour markets and Z_{ap} for instance the local unemployment rate. For detailed references for this literature, see the overview in Appendix Table A1, with "CS" indicating this cross-sectional approach in column 2.

Another strand of literature relies for identification on variation in group-level variables over time within groups for different cohorts ("panel case"). Such a strategy is not limited to situations with explicit random placement. Experiments are rare, and many studies extract quasi-exogenous variation in group variables using a sequence of fixed effects. In a seminal paper, Hoxby (2000) introduced this approach to the quasi-experimental economics of education literature, exploiting across-cohort differences in peer composition conditional on school fixed effects. Some studies even exploit variation within the individual (Lavy et al., 2012). Examples of this approach in the refugee integration literature can again be found in Table A1 in the appendix, denoted by "FE" in column 2. The typical estimation specification takes the following form:

$$Y_{itapr} = \delta_0 + \sum_{k=1}^{k^{obs}} \delta_1^{FE,k} Z_{ap}^{obs,k} + \beta^{FE} X_i + \phi_p^{FE} + \phi_a^{FE} + \eta_{itapr} \tag{9}$$

The key difference to specification 8 is the inclusion of group fixed effects ϕ_a^{FE}.

2.5 Identification in the presence of MTB and MB

Under which conditions will regressions like 8 or 9 identify δ_1 from equation 3? We note that these regressions stratify on individual characteristics, cohort of arrival, and sometimes on some other group-level characteristics $Z_{ap}^{obs,i\neq k}$, i.e. all the considerations that follow have to be interpreted conditional on these characteristics as is the case for our outcome equation 7. Denote the residual of an auxiliary regression of the observed local conditions on these other observed characteristics as $\tilde{Z}^{obs,k}_{ap}$.[8] Then, using our outcome equation we can write the regression coefficient from a regression of equation 8 or 9 for our regressor of interest k, $\hat{\delta}^k_1$, as:

$$
\begin{aligned}
\hat{\delta}^k_1 &= \frac{Cov(Y_{itapr}, \tilde{Z}^{obs,k}_{ap})}{Var(\tilde{Z}^{obs,k}_{ap})} \\
&= \delta^k_1 \cdot \{1 + \underbrace{\frac{Cov(M \cdot \alpha \cdot \Delta^{obs,k}_{M}, \tilde{Z}^{obs,k}_{ap})}{Var(\tilde{Z}^{obs,k}_{ap})}}_{\text{MB}}\} \\
&+ \delta_2 \, (\underbrace{\frac{Cov(\boldsymbol{Z^{unobs}_{ap}}, \tilde{Z}^{obs,k}_{ap})}{Var(\tilde{Z}^{obs,k}_{ap})}}_{\text{MTB}} + \underbrace{\frac{Cov(M \cdot \alpha \cdot \boldsymbol{\Delta^{unobs}_{M}}, \tilde{Z}^{obs,k}_{ap})}{Var(\tilde{Z}^{obs,k}_{ap})}}_{\text{Interaction MTB and MB}}) \\
&+ \underbrace{\frac{Cov(\epsilon^M_{itapr}, \tilde{Z}^{obs,k}_{ap}))}{Var(\tilde{Z}^{obs,k}_{ap})}}_{\text{MB: Selection}}
\end{aligned} \tag{10}
$$

This equation makes clear that even under the high-level assumptions in our very simple theoretical framework, the estimate of $\hat{\delta}^k_1$ is a combination of different effects. It contains the causal effect of interest, but also (potentially) a bias through a moving channel (what we call "MB" and "MB: Selection"), and Multiple treatment bias (MTB).

We find it informative to discuss MB with and without the mover selection component, as onward mobility is sometimes assumed to merely result in attenuation: MB from the first row can indeed generate attenuation bias under certain conditions, in particular if mobile individuals chose the new group randomly. In that case, in our framework mobility merely generates a time-weighted average using a second random draw from the group distribution for movers. And because of mean-reversion, we get $Cov(M \cdot \alpha \cdot \Delta^{obs,k}_{M}, \tilde{Z}^{obs,k}_{ap}) < 0$. This attenuation effect can moreover be amplified by a specific kind of endogeneity in the moving decision: if individuals move because of an extreme initial realisation of $\tilde{Z}^{obs,k}_{ap}$, only those individuals now get a second draw, which is unlikely to be extreme again. Notice how this rationale for attenuation is very different to ITT in the IV framework, where non-compliers do not receive any treatment at all. Moreover, different assumptions about the moving process can reverse this result: if only individuals allocated to "average" initial conditions move again, we get $Cov(M \cdot \alpha \cdot \Delta^{obs,k}_{M}, \tilde{Z}^{obs,k}_{ap}) > 0$.

Onward mobility could be non-random in other ways. This introduces a number of problems. First of all, if mobility is an outcome, for example to initial conditions, this generates bias through MB, i.e. $Cov(M \cdot \alpha \cdot \Delta^{obs,k}_{M}, \tilde{Z}^{obs,k}_{ap}) \neq 0$. More fundamentally,

[8] The regression is for the cross-sectional case of equation 8: $Z^{obs,k}_{ap} = \gamma_0 + \gamma_1 X_{ip} + \sum_{i \neq k} Z^{obs,i}_{a,p} \gamma^i_2 + \psi_p + \tilde{Z}^{obs,k}_{ap}$. For the panel case of equation 9 it additionally includes a region of assignment fixed effect.

if the moving decision is an outcome, the model from specification 7 is not identified. Empirically, this will result in non-zero ϵ_{it}^{M}, which, if correlated to $\tilde{Z}_{ap}^{obs,k}$, introduces additional bias through the "MB: Selection" term. Notice that this includes relationships between unobserved individual characteristics and the characteristics in the group of random placement (we call this "mover selection"). This selection can contain many different parts, as ϵ_{it}^{M} may depend on all the other quantaties in our theoretical model.

In summary, whenever individuals can move, "MB" *could* be present. In the refugee placement literature individuals can almost always move, and moving propensities in most empirical settings are substantial and differ between zero and 66.6 percent across settings (see Appendix Table A2 for an overview for refugee migration studies).

Next, multiple treatment bias (MTB), the bias through unobserved local conditions discussed earlier, contains two parts. The first part that is irrespective to moving and the second part that is associated with moving. It will only be present at all, if unobservables indeed influence the outcome, i.e. $\delta_2 \neq 0$. If this is the case, and additionally either the observable local characteristics are correlated to the unobservables ($Cov(Z_{a,p}^{unobs}, \tilde{Z}_{a,p}^{obs,k}) \neq 0$), or the change of unobservables associated to moves ($Cov(\Delta_{M}^{unobs}, \tilde{Z}_{a,p}^{obs,k}) \neq 0$), the moving decision itself and the timing of the move, there will be this additional bias through "multiple treatments". In short, MTB is present whenever the local indicators used for identification correlate with other local indicators.[9]

2.6 The checklist: quantification of the bias terms using observable variables

We summarize in panel A of Table 1 the different selection terms that are potentially present in the RDP regression estimates (equations 8 and 9), according to our theoretical treatment of the setting (equation 10). Panel B shows the labels that we have introduced. In general identification of δ_1, that is, the true causal effect of a specific local integration factor on integration outcome Y (given that it is truly linear and additively separable from other effects!) in such a regression model is only possible, if it can be credibly argued that *all* of these correlations are negligible. In Panel C we indicate whether and how researchers can provide empirical evidence on these identification assumptions. The idea of this checklist is that relations can be tested using observable characteristics.

One of such tests is the typical "Table 1" of RDP papers that checks whether observed individual characteristics are uncorrelated with initial group characteristics. As discussed, this might not be enough. In the same vein researchers can also check for individual selection of movers by studying correlations between observable Xs, moving decision, moving timing and destination characteristics (row 5 of Table 1). Similarly, researchers

[9]As pointed out by Damm (2009b), estimating effects in a dynamic framework (equation 9) is moreover problematic if relevant local factors, such as the network size, change endogenously over time.

Table 1: Identification in random placement regressions and checklist

	Initial group characteristics		**Moving**		**Change of characteristics**	
	Observed	**Unobserved**	**Decision**	**Time**	**Observed**	**Unobserved**
A. Theoretical Quantaties						
I. Individual Selection	$\text{Cov}(\epsilon_{it}, Z^{obs,k})$	$\text{Cov}(\epsilon_{it}, Z^{unobs})$	$\text{Cov}(\epsilon_{it}, M)$	$\text{Cov}(\epsilon_{it}, \alpha)$	$\text{Cov}(\epsilon_{it}, \Delta^{obs})$	$\text{Cov}(\epsilon_{it}, \Delta^{unobs})$
II. "Local" Selection	$\text{Cov}(Z^{obs}, Z^{obs,k})$	$\text{Cov}(Z^{obs}, Z^{unobs})$	$\text{Cov}(Z^{obs}, M)$	$\text{Cov}(Z^{obs}, \alpha)$	$\text{Cov}(Z^{obs}, \Delta^{obs})$	$\text{Cov}(Z^{obs}, \Delta^{unobs})$
B. Label of "Bias" term						
I. Individual Selection	$= 0$ p.a. by random placement		Mover Selection		Mover Selection	
II. "Local" Selection	MTB	MTB	MB	MB	MB & MTB	MB & MTB
C. Possible Empirical Evidence (Checklist)						
I. Individual Selection	$\text{Cov}(X_i, Z^{obs,k})$	by extension	$\text{Cov}(X_i, M)$	$\text{Cov}(X_i, \alpha)$	$\text{Cov}(X_i, \Delta^{obs})$	by extension
II. "Local" Selection	$\text{Cov}(Z^{obs}, Z^{obs,k})$	by extension	$\text{Cov}(Z^{obs}, M)$	$\text{Cov}(Z^{obs}, \alpha)$	$\text{Cov}(Z^{obs}, \Delta^{obs})$	by extension

Notes: The different selection terms that are potentially present in the RDP regression estimates (equations 8 and 9) according to our theoretical treatment of the setting (equation 10) are summarized in the upper block and labeled in the middle block. The lower block illustrates how observables can be used to assess the relevance of the various bias terms, assuming that these correlate also with unobservables.

can study "local" selection by studying whether and how other observed characteristics of initial groups are related to the initial group characteristics of interest, moving, timing of moves and changes in local characteristics due to moves (last row of Table 1). By extension, one could then argue that unobserved individual and group characteristics may also not be related to these quantities.

To summarize this theoretical exercise: MTB and MB can give rise to bias in standard "random placement" estimates of group-level variables on individual outcomes, even under a set of restrictive linearity assumptions. This demonstrates that random placement does not necessarily constitute an "ideal experiment" for the study of peer- or other mechanisms of group-level effects.

Empirically, both biases depend on correlations between observed and unobserved group-level variables, the presence of subsequent group changes (onward mobility) and its relation to unobserved individual characteristics. These variables may well vary between settings. In the following, we turn our attention to the case of refugee integration in Germany to provide an empirical benchmark of the severity of MTB and MB in a popular case where random placement instruments are being used.

3 Empirical application: local conditions and refugee integration in Germany

3.1 Random placement policy

Random placement policies (RDP) are a common policy tool, used e.g. in Denmark, Sweden, Norway, Austria, Switzerland, the United States and Germany. In Germany, the RDP works as follows: refugees are first allocated between the 16 federal states according to the *Königsteiner Schlüssel*, which in practice leads to a distribution across Federal States that is proportional to population shares.[10] Refugees are then distributed within states by the state governments and eventually placed in one of the 400 counties in Germany. Each Federal state has separate legislation and special distributional mechanisms, but in the end refugees are also distributed more or less proportionally to county-level population shares within the Federal States (Baba et al., 2023). Crucially, the distribution of refugees is at the discretion of central state authorities. Local Authorities do not have a say in the allocation, so phenomena such as cream-skimming are in all likelihood not present.

3.2 Data and descriptive statistics

3.2.1 The Y: settlement permit as composite measure for successful integration

We use the acquisition of a permanent settlement permit (German: "Niederlassungserlaubnis") as our measure for successful integration. In general, refugees become eligible to apply for a permanent settlement permit five years after submitting their asylum application. To qualify for the permit, applicants must meet the statutory criteria set out in § 9(2) AufenthG: (1) financial independence, defined as deriving no more than 50% of household income from social transfers; (2) sufficient language proficiency (at least A2 level under the "Common European Framework of Reference for Languages"); (3) basic knowledge of the legal and social order in Germany; (4) adequate housing; (5) no evidence of posing a security risk. Refugees who have been granted protection under the Geneva Convention and who demonstrate very high language proficiency (C1 level) and substantial income (less than 25 % derived from transfers) may obtain a settlement permit after only three years pursuant to § 26(3) AufenthG.

Given these requirements, one can think of the residency permit as a composite integration measure, comparable to an index measuring social and economic integration as

[10] Formally, the quotas are a weighted average of the population share (weight of one third) and the state-share of the overall tax income (weight of two thirds). However, as the tax income *after* various redistributional mechanisms between the states is considered (Schmandt et al., 2023), the per-capita share of the tax income is virtually identical between states.

used e.g. by Schilling and Stillman (2024) or Aksoy et al. (2023). In the appendix we show with different data sources that the outcome of obtaining a residence permit is highly related to other measures for integration outcomes that have also been used frequently. E.g. refugees with a settlement permit have higher labor market participation in the employment statistics of the federal employment agency (see Appendix Table A3). The proportion working is about twice as high in this group, compared to statistics derived from the German socio-economic panel (SOEP). We can also show with the SOEP that for instance language skills are far higher for refugees with a settlement permit (Appendix Table A4).[11]

3.2.2 Construction of estimation sample

We study refugee integration in Germany using the central register of foreigners (AZR) research dataset, the so-called *AZR-Forschungsdatensatz* (BAMF-Forschungsdatenzentrum, 2021). The central register of foreigners contains locational and legal information on all foreigners that either live in Germany or have left the country within the last 10 years. The *AZR-Forschungsdatensatz* is a 20 % sample of the AZR as of June 30, 2021 (Hammerl and Janik, 2021).

We implement two sampling choices to identify the relevant group of refugees in the AZR (in the following we use the abbreviation AZR for the research dataset). We focus on refugees who have the right to stay in Germany at least temporarily because only this group can potentially get a settlement permit.[12] We only include individuals that are still alive and reside in Germany as of June 2021 and have not been forced to relocate by authorities in their integration history. Another necessary condition is that we can identify in the data at what time an individual first obtained the right to stay in Germany and that we observe their entire moving history and all relevant individual characteristics. The AZR contains $141,792$ individuals that meet all these criteria.

The second sample restriction that we implement is simply based on timing. It takes time to obtain the settlement permit and we only include refugees who had by June

[11]One concern with our integration measure could be that it depends on institutional capacity, which could be correlated with local integration conditions. For instance, in regions with high levels of local unemployment it might be the case that resources are primarily used to increase employment and local authorities cut on spending on local immigration offices which issue settlement permits. If this were the case, well-integrating refugees might have a lower chance to obtain a settlement permit for these institutional reasons. In such a situation, we would expect to observe comparably high employment rates among refugees with a settlement permit in high-unemployment districts. The reason would be that only the most integrated refugees in such regions would receive the settlement permit. However, in reality we observe the opposite: the employment share is comparably low for refugees with and without a settlement permit in high unemployment regions (Table A3).

[12]This includes the following groups: (1) persons who have been granted asylum pursuant to § 25(1) AufenthG or recognized as refugees under § 3 AsylG, as provided in § 25(2) sentence 1, alternative 1 AufenthG; (2) those who have been recognized as persons entitled to subsidiary protection pursuant to §4 (1) AsylG (as provided in §25(2) sentence 1, alternative 2 AufenthG); (3) Those to whom a residence permit has been issued for the first time pursuant to §§22, 23, or 25(3) AufenthG.

2021 spent enough time in Germany to be eligible. We therefore drop 7,956 persons who received asylum after June 2018, as they had not spent 3 years in Germany by June 2021. For migrants who received subsidiary protection, the cut-off is June 2016, since refugees with subsidiary protection need to work for at least 5 years in order to get the settlement permit. This drops another 46,519 individuals. We also need to apply restrictions to avoid including refugees who might have obtained the German citizenship. After a naturalization former foreigners are excluded from the AZR altogether. In our sampling period, citizenship is granted after having lived in Germany with a valid refugee status for a minimum of six years, even in cases of exceptionally successful integration.[13] Hence, we limit our sample to individuals who received asylum after June 2015 and did not yet have had the chance to be naturalized, which drops another 17,019 persons. Appendix Table A5 summarizes how these sampling restrictions based on the institutional setting and the resulting timing affect the number of observations in our final estimation sample, which consists of 69,558 refugees.

3.2.3 Descriptive statistics

Table 2 shows descriptive statistics of our estimation sample. The vast majority of refugees are married. Around 60 % are below the age of 30.[14] The majority of refugees is from Syria and was recognized as a refugee according to the Geneva Refugee Convention (89 %). Only a small minority (9 %) received merely subsidiary protection as their first status.[15]

In our sample as of June 30, 2021, around 35% of the individuals did not live in the county where they resided when they received asylum. Moving propensities differ for demographic groups and are higher among younger refugees, single males, and refugees from Syria (see Table 2). A large share of moves occurs early in the integration process, shortly after asylum was granted ($\alpha = 0.68$ is large in our sample). Refugees disproportionately left rural areas in eastern Germany and parts of Bavaria, concentrating in urban areas in western Germany, particularly in the Ruhr area (see Appendix Figure A1a), while moving times do not have a clear regional pattern (Appendix Figure A1b). Interestingly, there is no evidence of strong migration towards major cities like Berlin, Hamburg or Munich, or good labor markets in the south.[16]

[13] See § 10 StAG in the current (27.06.2024) and previous versions for the laws regarding naturalization.

[14] Note that even though the AZR-Forschungsdatensatz does not contain minors, we do observe individuals that came to Germany when they were below 18 years of age if they turned 18 before June 30, 2021.

[15] Note that in Table 2 we consider the first legal status received. Hence, the share of individuals with non-refugee status differs from Table A5, as in the latter we also considered whether a subsidiary protected individually received regular refugee status *later on* and was thus eligible to be awarded a settlement permit early.

[16] This is the German "refugee mobility puzzle" (Wiedner and Schaeffer, 2024). In Germany refugees tend to move to affordable cities with bad labor markets (see also: Weber, 2023; Baba et al., 2023).

Table 2: Descriptive statistics of AZR sample

	Number of Observations	Share in Sample	Share that moved away	Share with Settlement Permit
Gender				
Male	48,762	70%	36%	11%
Female	20,796	30%	31%	4%
Age when Asylum received				
u18	6,791	10%	30%	6%
18–25	21,117	30%	41%	10%
25–30	13,941	20%	37%	12%
30–u35	10,100	15%	33%	11%
35–u50	13,880	20%	28%	7%
über 50	3,729	5%	28%	2%
Family Status				
single	28,793	41%	38%	10%
married	31,313	45%	32%	9%
others/unknown	9,452	14%	33%	6%
Nationality				
Others	5,908	8%	28%	8%
Syria	40,579	58%	38%	12%
Iran	4,544	7%	30%	7%
Afghanistan	4,866	7%	22%	5%
Irak	7,923	11%	38%	3%
Eritrea	5,738	8%	27%	7%
Legal Status				
Asylum/Refug./Settlement	62,037	89%	35%	10%
Subsidiary Protection	5,977	9%	30%	4%
National ban on deportation	562	1%	23%	8%
Other protection status	982	1%	20%	2%
Total	69,558	100%	35%	9%

Notes: Data from AZR research dataset, authors' calculations.

Having obtained a settlement permit is our binary measure of integration. In our sample, around 9% of refugees obtained a settlement permit by June 30, 2021 (Table 2). The share is higher for males, refugees who arrived aged 25 to 30, who were from Syria and who received regular protection status (asylum, refugee status, resettlement). It varies barely between single or married individuals.

3.2.4 The Z^{obs} (or Z^{unobs}): measures of local integration conditions

The local (group-level) covariates that we use are on the level of 393 Foreigners' Registration Office districts, which are with very few exceptions identical to the 400 German counties.[17] We use the unemployment rate to measure the quality of the local labor market as is commonly done in the literature (e.g. Aslund and Rooth, 2007; Aksoy et al., 2023). The indicator for the size of the co-ethnic network is defined as the share of co-nationals

[17]The following districts are recorded as one foreigners' registration office: district Kassel and city of Kassel, district Oder-Spree and city of Cottbus, the six districts of the Federal State Saarland.

Table 3: Descriptive statistics of local integration conditions

Variable	**Year(s)**	**N**	**Mean**	**SD**
Cross-Sectional Data				
Unemployment Rate	2014	393	5.98	2.79
Share of academic employment	2014	393	0.107	0.0473
Syrian network (Share of total Population)	2014	393	0.00135	0.000906
Total Population	2014	393	206610	239357
Population density per km^2	2014	393	522	686
Average Income	2014	393	1708	200
Language courses per refugee	2016	393	0.0190	0.0117
Average asking rent in $Euro/m^2$	2014	393	6.26	1.46
Vote share for AfD in general election	2017	393	13.4	5.3
Panel Data				
Share of academic employment	2015–2018	1,572	0.118	0.0514
Population	2015–2018	1,572	210247	246083
Population density	2015–2018	1,572	533	703
Income	2015–2018	1,572	1819	216
Unemployment Rate	2015–2018	1,572	5.21	2.47
Average asking rent in $Euro/m^2$	2015–2018	1,572	6.86	1.73
Syrian network (Share of total Population)	2015–2018	1,572	0.00723	0.00475
Language courses per refugee	2016–2018	1,179	0.0175	0.0103
Vote share for AfD in general elections (interpolated)	2015–2018	1,572	11.6	4.9

Notes: N corresponds to number of region-year observations. Data sources are Federal Employment Agency (BA) for unemployment rate and share of academic employment; Destatis (Ausländerstatistik) for network sizes; Destatis for total population, population density and average incomes; BAMF-Integrationskursgeschäftsstatistik and Destatis (Ausländerstatistik) for language courses per refugee; VALUE Marktdatenbank for average rents; Destatis (Election statistics) for vote shares. Table only displays share of Syrian refugees, but the final indicator is based on the shares for 50 nationalities.

of the local population (as e.g. used by Foged et al., 2024; Andersson, 2021)[18]. The measure of sentiment of the local population towards migration is the vote share of the right-wing party "Alternative für Deutschland" (AFD) (as used e.g. by Aksoy et al., 2023; Schilling and Stillman, 2024). The local coverage with language courses is measured as the number of course starts divided through the number of refugees residing in the district (comparable to Kanas and Kosyakova, 2023, who use the number of new courses divided through the number of new course vouchers issued. The correlation between measures is 0.81.).[19] Apart of these commonly used variables of interest in RDP studies, we also use a small number of other local characteristics that may be correlated to the variables of interest and could potentially influence integration outcomes. Table 3 shows variables and descriptive statistics for the cross-sectional data in the upper block. We use data for years that pre-date the arrival whenever possible. The only two exceptions are the language courses, where the first available data point is 2016, and the AfD vote share, we use the year 2017, as the party only then turned to an anti-immigration party.

[18] Other options are to use the share of co-national employment on overall employment (Battisti et al., 2022) or simply the (log) number of co-nationals (Edin et al., 2003; Müller et al., 2023)

[19] We use the availability of language courses in 2016 since this was the first year in which statistics on language courses were made available for the public to download.

4 Results

4.1 Randomization tests and checklist

Table 4 presents the empirical counterpart to the testable assumptions derived in Table 1. Table 4 only reports the joint F-tests. All corresponding regression coefficients and standard errors are reported in the Appendix Tables A6 (cross-sectional, individual), A7 (cross-sectional, local), A8 (panel, individual), and A9 (panel, local).

We first turn our attention to the issue of potential self-selection of refugees into initial locations. This is a concern that is typically addressed in great detail by RDP papers to highlight the validity of the identification strategy. We study individual selection for the following individual-level covariates: a gender dummy, five nationality group dummies, three asylum status dummies, five age group dummies, and two family status dummies. To assess overall significance of these individual characteristics, we perform F-Tests, testing the short model including only dummies for arrival and asylum cohorts and federal state dummies against a longer model additionally including the 16 dummies for individual characteristics. The F statistics range from 2.1 (regional AfD vote share) to 4.0 (ethnic enclave), as shown in Table 4, columns 1-4, panel A.I., which are all significant at conventional levels. Hence, there is some evidence for selective assignment according to this test based on our administrative data.[20] Appendix Table A6 shows that the imbalance is driven by the gender dummy and some nationality dummies, where we cannot reject the null that they are uncorrelated to local integration conditions.[21] Note that the coefficient estimates for the single covariates are in the range of, or smaller than those in the balancing tests of Aksoy et al. (2023), who use the SOEP survey for Germany to test for balancing in the same setting. Their estimates are not significant though, because the SOEP has substantially fewer observations. Thus, there is some evidence that the German RDP was not fully random. But the degree of selection appears relatively small, in particular when compared to the next checks.

Refugees can move after assignment and it might be the case that specific individuals move at specific points in time to specific places. For Germany we find that all three forms of selection are likely important – despite the more or less random placement. Individual characteristics are highly indicative of moving decisions M ($F = 43$) and time α ($F = 49.4$). Individual characteristics can also explain destination choices to some extent (F statistics between 5.8 and 10.8, see Table 4, columns 5-10 of panel A.I.).

[20] E.g. Wett et al. (2024) also find some selection in extensive Austrian administrative data.

[21] This is not surprising in the German context, since certain nationalities with low numbers of applicants are predominantly allocated to specific regions in order to take advantage of scale effects in processing the asylum applications. Such responsibilities for almost 200 different countries are listed by the federal institute for migration (Bundesamt für Migration und Flüchtlinge, 2024). Women were prioritized in being redistributed from central initial registration facilities to the counties (Baba et al., 2023). The other individual characteristics mostly not explain the levels of regional characteristics.

Table 4: Individual and local selection in cross-sectional and panel specifications

	Initial group characteristics (MTB)				Moving (MB)		Change of characteristics (MTB & MB)			
	Unempl. rate	AfD vote	Ethnic network	Integr. course	Decision	Time	Unempl. rate	AfD vote	Ethnic network	Integr. course
A. Cross-sectional Case										
I. Individual Sel.	3.2	2.1	4	2.8	43	49.4	7.9	5.8	8	10.8
	(0.000)	(0.007)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
II. Local Sel.	42	29.9	17.2	22.8	40.8	30.5	28.4	7.5	25.1	25.3
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
B. Panel Case										
I. Individual Sel.	1.5	2.8	5.4	1.6	53.1	56.7	9.3	6.5	10.8	6.4
	(0.096)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
II. Local Sel.	24.6	29.9	6.3	34.2	18	30.1	16.9	35.8	2.8	38.4
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.005)	(0.000)

Notes: Table reports F-statistics comparing a regression including only dummies for month of arrival, month of receiving asylum, and 16 Federal State dummies (Panel A) or 393 region of assignment dummies (Panel B), with a regression including additionally in Panel A. 16 dummy indicators for individual characteristics ($F(16,392)$) and in Panel B. 8 variables for local characteristics ($F(8,392)$). In Panel B local indicators vary between cohorts. P-values for the tests against the hypotheses of no influence of the additional indicators are in parentheses. Standard errors are always clustered on the level of 393 counties of assignment. All covariates are standardized. Single regression coefficients and standard errors are reported in the Appendix Tables A6 (cross-sectional, individual), A7 (cross-sectional, local), A8 (panel, individual), A9 (panel, local).

Do correlations of local factors play a role? A first indication that this may be the case yield columns 1-4, in panel A.II. of Table 4. We test covariate "balancing" of the "causal" variables –i.e. the local integration characteristics of interest in many RDP studies– for our sample, but w.r.t. *other* local integration characteristics. We test the model including only the basic fixed effects (cohorts + Federal States) against the model including the eight local covariates. We observe substantial correlations between the variables. The F-statistics range from 17.2 (ethnic enclave) to 42 (unemployment rate) and are always highly significant. Note that this range is at a margin of 10 higher than for individual initial selection.[22] These correlations *could* give rise to MTB if omitted variables were also influencing the integration outcome of interest.

Place of assignment characteristics are also related to moving. They explain both decision ($F = 40.8$) and time ($F = 30.5$) and changes of characteristics associated with a move (F between 7.5 and 28.4).

One motivation to estimate a regression with additional region of assignment fixed effects exploiting across-cohort variation in integration conditions is to reduce local selection and individual selection. To assess this assumption, we additionally perform the same exercise for the panel case, where the reference model includes fixed effects for 393 regions of assignment instead of 16 Federal States and the local indicators vary between years. Panel B.I. of Table 4 shows that individual selection is somewhat lower. Local selection is still important, despite the region of assignment fixed effects (panel B.II). The reason is that local indicators correlate not only in levels but also in changes. I.e. the panel setting does not solve MTB.

[22] We have 8 (local) or 16 (individual) and 392 degrees of freedom. Thus, the critical values for the two tests do not differ much and can be compared.

Moving decisions, moving timing, and changes of local characteristics that follow a move are also related to individual and place of assignment characteristics in the panel case (Table 4 Panel B. columns 5-10). I.e. also the biases arising from moving are not solved by estimating a model with region of assignment fixed effects.

4.2 Multiple treatment bias (MTB)

In the economic literature, it is common practice to estimate different specifications with alternating sets of covariates and to choose a "preferred specification", based on econometric or economic arguments. In a non-experimental setting, such specifications may produce very different coefficient estimates of the regressor of interest. We argued above that even with random assignment, this remains a non-experimental setting because of the remaining required assumptions about $\boldsymbol{Z_{ap}^{unobs}}$. Given that regions, not local conditions, are randomly assigned, regression specifications with different sets of covariates could produce different estimates of the "causal" effect of local conditions of interest in our example.

To study whether there are indeed relevant local confounders in our empirical example, we have to check whether local characteristics are not only correlated, but also related to our integration outcome. We therefore estimate regressions like the one in equation 8 and 9 with our data, where Y_i is a binary indicator for whether the refugee obtained a settlement permit. We include as demographic controls X_i at the time of receiving protection the sex, nationality, age, legal title, month of arrival, and month of having received asylum for the refugee. For the cross-sectional regression equation 8 we include different sets of fixed effects for regions (none or for 16 Federal States or 38 NUTS-2 regions). To study MTB we always vary the set of regional control variables. Table 5 shows regression estimates for key explanatory variables: the local unemployment rate, local co-ethnic network size, local sentiment towards migration measured by the AfD vote share, and the supply of language courses measured by the number of courses per refugee in the district. All regressions include demographic characteristics, but the set of local controls and fixed effects changes. We observe that in our empirical setting, the 'causal' estimate of the local characteristics of interest changes depending on the specification. For instance, for the local unemployment rate, ethnic network and sentiment coefficients move between statistically significant negative and zero. Language course supply can be positive or zero, statistically significant depending on the exact specification.

To observe a more complete picture of the range of possible estimates, we produce $2^k - 1$ permutations, including k regional covariates as defined in section 3.2.4, in each possible combination in one separate regression model. Additionally we vary the sets of regional fixed effects that are included in the models, as do papers in the refugee RDP literature. Figure 1 shows the distributions of the resulting coefficient estimates. Our

Table 5: Varying local integration factors and refugee integration

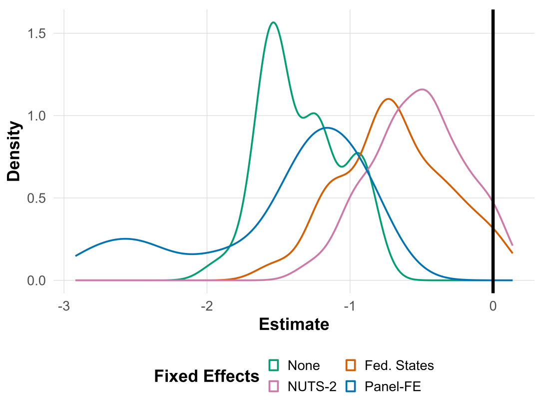
	Dependent variable: *obtained settlement permit*					
	CS	CS	CS	panel	panel	panel
Unemployment Rate	-0.0082***	-0.0076***	-0.0071***	-0.0249**	-0.0287**	-0.0084
	(0.0026)	(0.0026)	(0.0036)	(0.0150)	(0.0164)	(0.0156)
AfD Vote Share	0.0063	0.0059	-0.0049	0.0011	-0.0070	0.0060
	(0.0044)	(0.0042)	(0.0043)	(0.0083)	(0.0093)	(0.0103)
Ethnic Network	-0.0026***	-0.0017**	-0.0006	-0.0019**	-0.0018**	-0.0020***
	(0.0009)	(0.0009)	(0.0009)	(0.0011)	(0.0011)	(0.0010)
Language Courses	-0.0014	0.0005	0.0025	-0.0084	-0.0093***	-0.0038
	(0.0028)	(0.0028)	(0.0027)	(0.0044)	(0.0046)	(0.0040)
Regional Controls	No	Main	All	No	Main	All
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Month of Asylum Decision (Cohort)	Yes	Yes	Yes	Yes	Yes	Yes
Federal State FE	Yes	Yes	Yes	No	No	No
District FE	No	No	No	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Data from AZR research dataset, author calculations. Columns (1) and (4) show estimates of the importance of local factors for successful integration from separate regressions with only one regional control. Additional local controls are added across columns: *Main* regional control variables are the local unemployment rate, AFD vote share, co-ethnic networks. *All* regional controls additionally include income, rent levels, population size, population density and share of academic employment. Regional covariates are standardized across regions and years. Demographic controls include sex, nationality, age, legal title and fixed effects for month of arrival. Standard errors in parentheses. All regressions for unemployment rate, AfD vote share and ethnic network based on 69,558 observations. Panel rergessions for integration courses only based on years 2016-18 (n=59,631). Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

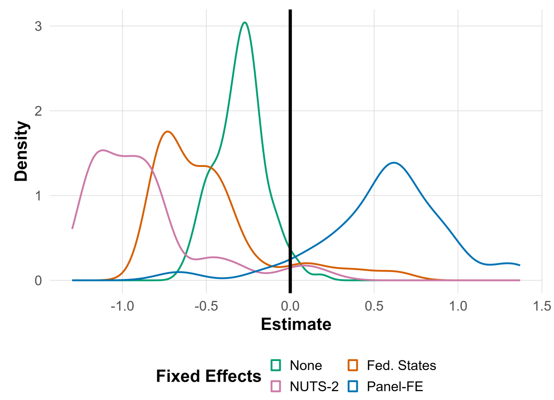
estimations produce for the covariates that are of most interest to the literature, the local unemployment rate (Figure 1a), local right-wing voting (AFD share, Figure 1b), the local supply of language courses (Figure 1c), and the size of the initial ethnic network (Figure 1d) different distributions for the coefficient estimates. The range of the coefficient estimates is substantial. Most notably, one can produce significant negative coefficient estimates, as well as zero estimates, depending on the specification for the local unemployment rate. For the supply of language courses we can produce positive significant or zero estimates. For right wing voting, we can produce negative significant, zero or positive significant estimates, depending on the specification and empirical model. In our setting only the ethnic network size always has a negative impact but the magnitude changes by a factor of 3 to 4, depending on the specification. Note that we observe the largest coefficient movements when we include more fine-grained regional fixed effects. This strong coefficient movement indicates that apart of the limited set of regional covariates that we use in our study, other, unobserved (to us) regional heterogeneity is significantly related to refugee integration in Germany.

The different specifications (no controls, only major controls, many controls) can be encountered in different RDP papers from a wide array of countries (see Table A1 in the appendix). Also different sets of regional fixed effects are included in different RDP studies that rely on cross-sectional variation of local integration conditions (e.g. Federal States or NUTS-2 in German RDP papers relying on cross-sectional variation of local characteristics). Given that local conditions are not as good as randomly assigned with

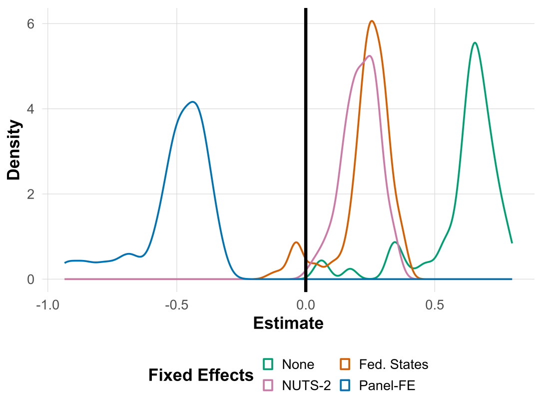
Figure 1: Coefficient estimates in different model specifications



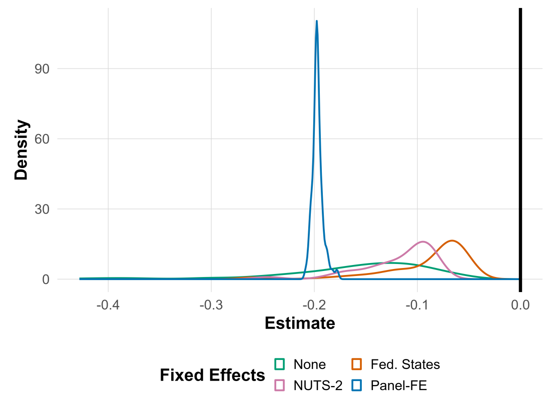
(a) Unemployment rate



(b) Afd share



(c) Integration courses



(d) Initial ethnic network size

Notes: Data from AZR research dataset, author calculations. Number of estimated regressions for the cross sectional regressions is always 255 (n=69,558). Alternating specifications include the variables: unemployment rate, AfD vote share, network size, income, rent level, population size, population density, share of academic employment, language courses per refugee and the indicated fixed effects. We estimate 127 Panel-FE models including 393 district of arrival fixed effects. Covariates are always on the level of the district of assignment and standardized. Regressions on integration courses are only based on years 2016-18 (n=59,631). Estimated effects rescaled by a factor of 100.

respect to other local characteristics (observed or unobserved) and that not only the levels but also the trends of local conditions are strongly correlated, such coefficient movements are not surprising.

4.3 Mobility Bias (MB)

Even if the true model was estimated, i.e. even if δ_2 was zero, the estimated effect would not be equal to a local treatment effect due to subsequent moving (i.e. choice of another level of the treatment). Recall the MB term from equation 10: "MB" $= \frac{Cov(M \cdot \alpha \cdot \Delta_M^{obs}, \tilde{Z}_{a,p}^{obs})}{Var(\tilde{Z}_{a,p}^{obs})}$. It indicates that the bias depends on refugee mobility and the exposure to local conditions other than the initial conditions. This bias is going to be present and large if there is a lot of mobility, if mobility occurs early in the integration process (α is large) and the change of observed local characteristics associated with a move is large. We next study the size of the bias under very restrictive assumptions.

4.3.1 Attenuation

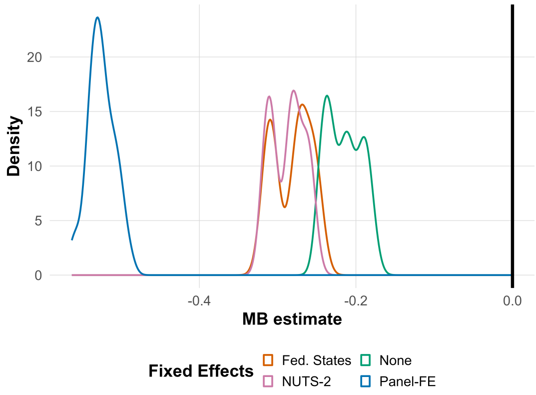
We have already shown in section 4.1 that local characteristics drive outmigration substantially. A graphical illustration give the relationships between assignment characteristics and moving in Appendix Figure A3a and A3d. Initial unemployment rates, but even more so initial AFD vote shares are related to moving. The majority of refugees assigned to regions with high right-wing vote shares leaves those regions. On the other hand, refugees stay in regions with low right-wing voting. Note that these correlations are not necessary to bias the result. High moving propensities will influence the result even if they were unrelated to local conditions, as can be seen in equation 10. The correlations, however, already indicate that moving may be relevant to the estimation of local characteristics on integration. Also note that similar correlations arise for the other two initial local conditions (Figure A3c and A3d), but here high levels are generally related to lower outmigration.

If refugees would move to regions with exactly the same characteristics as their initial regions, then there would be no bias (in our framework), even if every refugee moved. As we have shown in section 4.1 and show graphically for the Z_a^{obs} in Appendix Figure A2a-d, this is not the case. In our data, we observe a mean-reversion: refugees from assigned regions with high levels of the local characteristics tend to reduce the level, while refugees assigned to regions with low levels tend to increase the values. The correlations are very large and close to -1. This mean-reversion will bias the estimation results from the regression 8 mechanically towards zero.

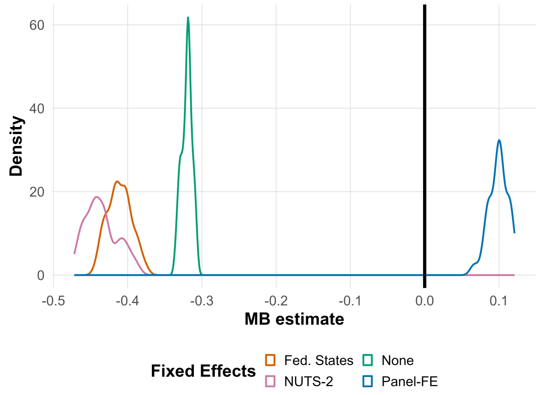
We next ask how relevant is the resulting "bias" of the estimated effect δ_1^k going to be empirically? (i.e. by how much would one have to rescale the estimated ITT in order to arrive at a local treatment effect?) As seen in the previous section, estimation results depend on the inclusion of other local covariates, i.e. in fact $\delta_2 \neq 0$. To take this fact into account, we again, as in the previous section, estimate $2^k - 1$ permutations of possible combinations of regression models and the resulting empirical mobility bias. I.e. we estimate the correlation as above, but we use the initial local conditions purged from the correlation with individual characteristics and other local conditions that may or may not be included in the empirical model. These models generate $2^k - 1$ different sets of $\tilde{Z}_{a,p}^{obs,k}$ values that we then use to calculate $2^k - 1$ different values of $\frac{Cov(M \cdot \alpha \cdot \Delta_M^{obs,k}, \tilde{Z}_{a,p}^{obs,k})}{Var(\tilde{Z}_{a,p}^{obs,k})}$.

The resulting empirical distributions for our main covariates of interest shows Figure 2. The "bias" through mobility does not so much depend on the exact regression specification, conditional on the set of regional fixed effects. In the cross-section the bias is always negative, which must be the case, given that a fraction of refugees is not exposed

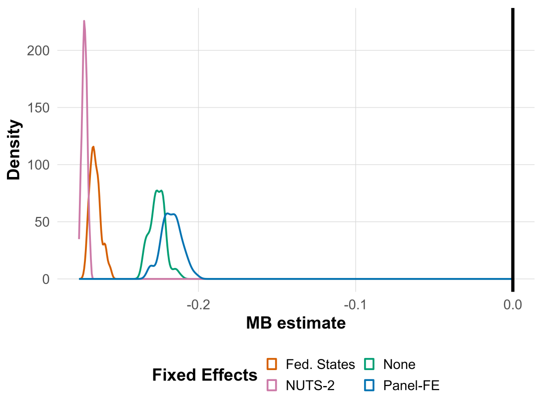
Figure 2: MB for different model specifications



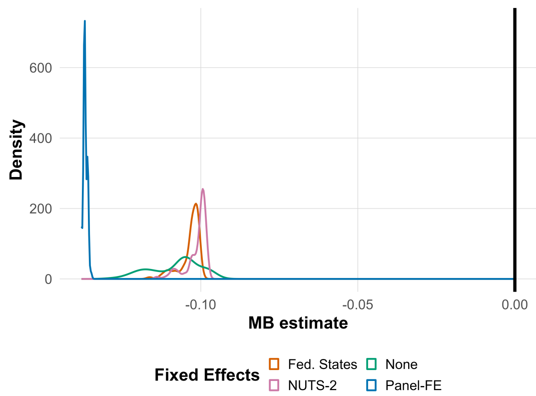
(a) Unemployment rate



(b) AfD share



(c) Integration courses



(d) Initial ethnic network size

Notes: Data from AZR research dataset, author calculations. Number of estimated regressions for the cross sectional regressions is always 255 (n=69,558). Alternating specifications include the variables: unemployment rate, AfD vote share, network size, income, rent level, population size, population density, share of academic employment, language courses per refugee and the indicated fixed effects. We estimate 127 Panel-FE models including 393 district of arrival fixed effects. Covariates are always on the level of the district of assignment and standardized. Regressions on integration courses only based on years 2016-18 (n=59,631).

to the local conditions for a certain amount of time. In the panel it can turn positive, because in our theoretical model (as does the literature) we made the far-fetched assumption that only the conditions at the time of assignment and at the time of the move enter the outome equation. But if the conditions changed a lot, the bias can turn even positive under these assumptions.

For the local unemployment rate the mobility bias attenuation ranges between -0.20 and -0.50, depending on the specification on the fixed effects (Figure 2a), i.e. depending on the specification one would have to double the effect to arrive at a local treatment effect. For language courses it lays between -0.2 and -0.3 (Figure 2c). For the right-wing vote share the bias lays between +0.1 and -0.5 (Figure 2b). The reason for the positive bias in the panel case is, as explained above, that the vote shares at the time of the move are being taken in the estimation and right-wing voting increased during the observed time span. MB is comparatively small for the ethnic network (around -0.10; Figure 2d) and does not depend on the regression specification or fixed effects.

It is worthwhile to note that because the mobility bias differs between the local factors even within one (our) empirical setting, without using IV, i.e. without rescaling the factor, it is not possible to compare their size even within one empirical setting/specification. This happens even if under the very restrictive assumptions of our theoretical framework –including the assumption of no selective onward mobility. We turn to the empirical relevance of this assumption in the estimation of the effects of local integration conditions in the next chapter.

4.3.2 Mover selection

We may observe mover selection in two ways. First, movers may be selected w.r.t. the characteristics of the region of assignment. Second, specific people may move, may move at specific times and to specific places. Our main theoretical results in equation 10 indicate that if this *selection* were related to the individual characteristics of interest $\tilde{Z}_{ap}$ this would pose a threat to identification as well. As we have shown in section 4.1 movers are selected. But are the initial place characteristics also related to this selection (which would, again, influence the estimation results of the "causal" estimates)?

We again vary our regression models to study whether this is the case. The exercise is simple. We re-estimate Table 5, but we omit the fairly limited set of individual controls (gender, nationality, legal title and age) from the estimation. The results shows Appendix Table A10. Depending on the specification we indeed observe some coefficient movements –although not as large as for MTB. Individual characteristics are not related to initial place characteristics directly. But they are related to changes in place characteristics and moves, which in turn is related to initial place characteristics. Therfore they matter in the empirical models, which would not be the case in a (quasi-)experimental setting.[23]

Taken together for our setting there is substantial evidence that individuals self-select into moving, timing and changes of characteristics. Also, initial characteristics drive not only moving decisions, but also destination choices. Self-selection of movers is thus relevant in our setting and will influence what the typical regression estimates. Random placement cannot and does not handle the biases that arise due to this self-selection.

5 Implications for the refugee integration literature

The theoretical and empirical arguments developed above suggest that the external validity and comparability of studies relying on continuous random placement policies as

[23]Note that in some settings individual characteristics are needed to ensure (conditional) random placement. For the individual characteristics that we vary here, this is not the case. They are (almost) as good as randomly assigned w.r.t initial place characteristics.

instruments is limited.

Empirical results in general depend on the set of covariates that are included in the empirical models. A large part of the literature relies on specifications with only a few or even without any additional regional control variables (see Table A1 in the appendix where we review the covariates included in a selected number of RDP papers). Depending on the included controls, the estimated effects will differ due to the fact that the local conditions under investigation are typically only as good as randomly assigned to individuals, but not w.r.t. other local covariates. As long as the resulting estimates are interpreted without reference to any sort of causal channels and without comparisons to estimates found in other settings (countries, times), this is not a problem. The estimates are ITT estimates of assignment in the sense that they contain all channels that are in some way related to the local characteristic of interest. But often authors compare ITT effects between different variables or different empirical settings. The implicit assumption in such statements is that all correlations to other local characteristics are also comparable – an assumption that is likely not fulfilled in many instances. For instance, correlations between right-wing vote shares and unemployment rates on the NUTS-3 level range from -0.5 (Switzerland) to +0.2 (Germany), see Appendix Table A11 for these correlations for five countries with random placement policies). If both influence labor market integration (as shown and argued e.g. by Aksoy et al., 2023), then estimates of local unemployment rates on refugee integration cannot be compared to German estimates if they do not (at least) control for local right-wing voting. The local sentiment towards migration similarly has not been taken into account by the Danish RDP literature (see Table A1).[24]

The crucial condition for a proper causal interpretation is that the local characteristic is as good as randomly assigned to individual and other local characteristics. Notable examples for such a case are Damm (2009b); Beaman (2012) and Auer et al. (2022). In these studies, the measure of the ethnic network is as good as randomly assigned to local characteristics: it is the share of refugees from the same nationality that were randomly assigned to the municipality in the previous period. Such a strategy relies on random placement to generate quasi-random variation in the network size in the first place. Unfortunately, this strategy does not easily extend to other local factors of interest.

[24] Can we ignore the issues discussed in this paper if interest was only in "what works" regarding the regional assignment of refugees, i.e. if interested in designing dispersal policies? We can if we only included exactly one local covariate in our regression. The estimate is the linear projection on that local covariate of the total effect of being assigned to regions with specific characteristics, including the effect of all correlated local characteristics and the effect of the assignment on the moving propensity and the corresponding indirect effect on integration through integration conditions in other places. However, it is not clear why in that case one would not want to use the causal averages of assignment from section 2.1 right away to design the dispersal policy, without imposing linearity in the local covariate of choice. Note that obviously this strategy is only informative if conditions (local integration conditions, local moving propensities, destination choices of movers) do not change for the cohort of refugees for which we want to design the policy and predict integration. Also note that using the local covariate does not add any substance to our prediction.

We want to emphasize that researchers can provide supporting evidence for causal claims by showing that the local characteristic of interest is indeed unrelated to other local conditions that are observable to them, following the checklist from Table 1.

Additionally, differences in moving propensities can hamper comparability. In our empirical application we make highly stylized assumptions (most notably: linearity, no selection of secondary movers and common effects of initial and subsequent local conditions) but show that even under these assumptions, non-compliance will result in biases (attenuation) that are substantial. More significantly, as seen in the previous section, for different combinations of local covariates, estimation results are going to be biased towards zero in a different manner, depending on the correlation of the initial condition of interest with moving propensity. Therefore, even comparisons of effects for different covariates estimated in the same regression model can be misleading if effects are not rescaled by 2SLS.

This also affects comparisons between estimates for different groups if their moving propensities are different. E.g. if men and women have different moving propensities (as they do have in Germany), then these moving propensities influence the size of the "intention-to-treat" estimates and have to be taken into account if comparisons regarding the mechanisms influencing integration are being made. The literature has identified some settings with random placement and zero onward mobility, i.e. Auer et al. (2022) or Martén et al. (2019) who study refugee integration in Switzerland, where some refugees are not free to move at all. From the identification perspective, this greatly simplifies the setting as all selection and attenuation from moving does not bias the results.

6 Conclusion

A large literature in economics derives findings about group variables on individual outcomes from random placement settings. This paper highlights that resulting estimates may be considerably different from the 'true' causal effect. First, the set of control variables that is included in the specification can considerably alter the magnitude of the results. Second, onward mobility introduces additional bias. We provide a checklist to identify the potential severity of these issues in any given setting.

The ways forward are twofold: first, some random placement settings may fulfill additional requirements for causal interpretation. In particular settings with zero onward mobility are helpful, from the perspective of identification, and "only" need to address MTB for a causal interpretation.

The second avenue for future work is to focus directly on settings with quasi-random variation in group-level variables. The literature that exploits quasi-random inflows to estimate effects on natives/locals is one example of this approach. Notably, with quasi-random variation in group-level variables due to factors other than migration, the estimation variation is orthogonal to individual sorting. This opens up the search for local variation to many settings that do not exhibit random placement policies.

References

Achard, P. (2025). Exposure to natives and cultural assimilation. *Journal of Human Resources*.

Aksoy, C. G., Poutvaara, P., and Schikora, F. (2023). First time around: Local conditions and multi-dimensional integration of refugees. *Journal of Urban Economics*, 137:103588.

Andersson, H. (2021). Ethnic enclaves, self-employment, and the economic performance of refugees: evidence from a Swedish dispersal policy. *International Migration Review*, 55(1):58–83.

Angrist, J. D. (2014). The perils of peer effects. *Labour Economics*, 30:98–108.

Aslund, O. (2005). Now and forever? Initial and subsequent location choices of immigrants. *Regional Science and Urban Economics*, 35(2):141–165.

Aslund, O. and Rooth, D. (2007). Do when and where matter? Initial labour market conditions and immigrant earnings. *The Economic Journal*, 117(518):422–448.

Auer, D., Egger, D., and Kunz, J. (2022). Effects of migrant networks on labor market integration, local firms and employees.

Azlor, L., Damm, A. P., and Schultz-Nielsen, M. L. (2020). Local labour demand and immigrant employment. *Labour Economics*, 63:101808.

Baba, L., Schmandt, M., Tielkes, C., Weinhardt, F., and Wilbert, K. (2023). Evaluation der Wohnsitzregelung nach § 12a AufenthG. *Beiträge zu Migration und Integration*. Publisher: Bundesamt für Migration und Flüchtlinge, Forschungszentrum Migration, Integration und Asyl Version Number: 1.0.

BAMF-Forschungsdatenzentrum (2021). AZR-Forschungsdatensatz 2021. Version 1.0. Research dataset.

Battisti, M., Peri, G., and Romiti, A. (2022). Dynamic effects of co-ethnic networks on immigrants' economic success. *The Economic Journal*, 132(641):58–88.

Beaman, L. A. (2012). Social networks and the dynamics of labour market outcomes: Evidence from refugees resettled in the U.S. *The Review of Economic Studies*, 79(1):128–161.

Bertoni, M. and Nisticò, R. (2023). Ordinal rank and the structure of ability peer effects. *Journal of Public Economics*, 217:104797.

Biddle, L. and Bozorgmehr, K. (2024). Effect of area-level socioeconomic deprivation on mental and physical health: A longitudinal natural experiment among refugees in Germany. *SSM-Population Health*, 25:101596.

Bundesamt für Migration und Flüchtlinge (2024). EASY-HKL-Liste (Version: 24.06.2024). Online. Access: 04.11.2025.

Carrell, S., Fullerton, R., and West, J. (2009). Does your cohort matter? Measuring peer effects in college achievement. *Journal of Labor Economics*, 27(3):439–464.

Damm, A. P. (2009a). Determinants of recent immigrants' location choices: Quasi-experimental evidence. *Journal of Population Economics*, 22(1):145–174.

Damm, A. P. (2009b). Ethnic enclaves and immigrant labor market outcomes: Quasi-experimental evidence. *Journal of Labor Economics*, 27(2):281–314.

Damm, A. P. (2014). Neighborhood quality and labor market outcomes: Evidence from quasi-random neighborhood assignment of immigrants. *Journal of Urban Economics*, 79:139–166.

Damm, A. P. and Dustmann, C. (2014). Does growing up in a high crime neighborhood affect youth criminal behavior? *American Economic Review*, 104(6):1806–1832.

Damm, A. P., Hassani, A., Jensen, T. S. H., and Schultz-Nielsen, M. L. (2022). Co-ethnic neighbors and investment in host-country language skills. Technical report, Rockwool Foundation Research Unit.

Damm, A. P. and Rosholm, M. (2010). Employment effects of spatial dispersal of refugees. *Review of Economics of the Household*, 8(1):105–146.

Degenhardt, F. and Nimczik, J. (2025). Is the gig economy a stepping stone for refugees? Evidence from administrative data. Technical report, IZA Discussion Paper No. 17928.

Dustmann, C., Schönberg, U., and Stuhler, J. (2016). Labor supply shocks, native wages, and the adjustment of local employment. *The Quarterly Journal of Economics*, 132(1):435–483.

Dustmann, C., Vasiljeva, K., and Piil Damm, A. (2019). Refugee migration and electoral outcomes. *The Review of Economic Studies*, 86(5):2035–2091.

Eckert, F., Hejlesen, M., and Walsh, C. (2022). The return to big-city experience: Evidence from refugees in Denmark. *Journal of Urban Economics*, 130:103454.

Edin, P.-A., Fredriksson, P., and Aslund, O. (2003). Ethnic Enclaves and the Economic Success of Immigrants–Evidence from a Natural Experiment. *The Quarterly Journal of Economics*, 118(1):329–357.

Fasani, F., Frattini, T., and Minale, L. (2022). (The Struggle for) Refugee integration into the labour market: Evidence from Europe. *Journal of Economic Geography*, 22(2):351–393.

Feld, J. and Zölitz, U. (2017). Understanding peer effects: On the nature, estimation, and channels of peer effects. *Journal of Labor Economics*, 35(2):387–428.

Foged, M., Hasager, L., and Peri, G. (2024). Comparing the effects of policies for the labor market integration of refugees. *Journal of Labor Economics*, 42(S1):S335–S377.

Gibbons, S., Silva, O., and Weinhardt, F. (2013). Everybody needs good neighbours? Evidence from students' outcomes in england. *The Economic Journal*, 123(571):831–874.

Gibbons, S., Silva, O., and Weinhardt, F. (2017). Neighbourhood turnover and teenage attainment. *Journal of the European Economic Association*, 15(4):746–783.

Glitz, A. (2012). The labor market impact of immigration: A quasi-experiment exploiting immigrant location rules in germany. *Journal of Labor Economics*, 30(1):175–213.

Glitz, A., Hörnig, L., Körner, K., and Monras, J. (2023). *The geography of refugee shocks*. Number 994. Ruhr Economic Papers.

Godøy, A. (2017). Local labor markets and earnings of refugee immigrants. *Empirical Economics*, 52(1):31–58.

Golsteyn, B. H. H., Non, A., and Zölitz, U. (2021). The impact of peer personality on academic achievement. *Journal of Political Economy*, 129(4):1052–1099.

Goulas, S., Megalokonomou, R., and Zhang, Y. (2023). Female classmates, disruption, and stem outcomes in disadvantaged schools: Evidence from a randomized natural experiment. CESifo Working Paper Series 10864, CESifo.

Hammerl, A. and Janik, L. (2021). Datenreport zum AZR-Forschungsdatensatz 2021. Veröffentlichungen des BAMF-FDZ no. 01/2021, BAMF-FDZ, Nürnberg.

Heckman, J. J. and Pinto, R. (2018). Unordered monotonicity. *Econometrica*, 86(1):1–35.

Heckman, J. J., Urzua, S., and Vytlacil, E. (2008). Instrumental variables in models with multiple outcomes: The general unordered case. *Annales d'Economie et de Statistique*, pages 151–174.

Hoxby, C. (2000). Peer effects in the classroom: Learning from gender and race variation. NBER Working Papers 7867, National Bureau of Economic Research.

Imberman, S. A., Kugler, A. D., and Sacerdote, B. I. (2012). Katrina's children: Evidence on the structure of peer effects from hurricane evacuees. *American Economic Review*, 102(5):2048–2082.

Jaschke, P. and Kosyakova, Y. (2021). Does facilitated and early access to the healthcare system improve refugees' health outcomes? Evidence from a natural experiment in Germany. *International Migration Review*, 55(3):812–842.

Jaschke, P., Sardoschau, S., and Tabellini, M. (2022). Scared straight? Threat and assimilation of refugees in Germany. NBER Working Papers 30381, National Bureau of Economic Research.

Jeong, Y. (2025). The effect of peers' genetic predisposition to depression on own mental health. *Journal of Health Economics*, 104:103053.

Jessen, J., Schmitz, S., and Weinhardt, F. (2023). Immigration, female labour supply and local cultural norms. *The Economic Journal*, 134(659):1146–1172.

Kanas, A. and Kosyakova, Y. (2023). Greater local supply of language courses improves refugees' labor market integration. *European Societies*, 25(1):1–36.

Kanas, A., Kosyakova, Y., and Vallizadeh, E. (2024). Linguistic enclaves, sorting, and language skills of immigrants. *Journal of Immigrant & Refugee Studies*, 22(4):847–861.

Kristiansen, M. H., Maas, I., Boschman, S., and Vrooman, J. C. (2022). Refugees' transition from welfare to work: A quasi-experimental approach of the impact of the neighbourhood context. *European Sociological Review*, 38(2):234–251.

Kuhn, E. and Maxwell, R. (2023). Asylum seekers feel more welcome in counties with more foreign-born residents. *West European Politics*, pages 1–27.

Lavy, V., Silva, O., and Weinhardt, F. (2012). The good, the bad, and the average: Evidence on ability peer effects in schools. *Journal of Labor Economics*, 30(2):367–414.

Manski, C. F. (1993). Identification of endogenous social effects: The reflection problem. *The Review of Economic Studies*, 60(3):531–542.

Martén, L., Hainmueller, J., and Hangartner, D. (2019). Ethnic networks can foster the economic integration of refugees. *Proceedings of the National Academy of Sciences*, 116(33):16280–16285.

Miho, A., Jarotschkin, A., and Zhuravskaya, E. (2023). Diffusion of gender norms: Evidence from stalin's ethnic deportations. *Journal of the European Economic Association*, 22(2):475–527.

Mossaad, N., Ferwerda, J., Lawrence, D., Weinstein, J., and Hainmueller, J. (2020). In search of opportunity and community: Internal migration of refugees in the United States. *Science Advances*, 6(32):eabb0295.

Müller, T., Pannatier, P., and Viarengo, M. (2023). Labor market integration, local conditions and inequalities: Evidence from refugees in Switzerland. *World Development*, 170:106288.

Sacerdote, B. (2001). Peer effects with random assignment: Results for Dartmouth roommates. *The Quarterly Journal of Economics*, 116(2):681–704.

Schilling, P. and Höckel, L. S. (2025). Starting off on the right foot – language learning classes and the educational success of refugee children. *The Economic Journal*, ueaf087.

Schilling, P. and Stillman, S. (2024). The impact of natives' attitudes on refugee integration. *Labour Economics*, 87:102465.

Schmandt, M., Tielkes, C., and Weinhardt, F. (2023). Königsteiner Schlüssel verteilt Gelder und Aufgaben zwischen Bundesländern kaum nach Wirtschaftskraft. *DIW Wochenbericht*, 90(18):204–209.

Sojourner, A. (2013). Identification of peer effects with missing peer data: Evidence from project star. *The Economic Journal*, 123(569):574–605.

Trogdon, J. G., Nonnemaker, J., and Pais, J. (2008). Peer effects in adolescent overweight. *Journal of Health Economics*, 27(5):1388–1399.

Tsolak, D. and Bürmann, M. (2023). Making the match: The importance of local labor markets for the employment prospects of refugees. *Social Sciences*, 12(6):339.

UNHCR (2025). Germany: 2025 contributions. Technical report, United Nations High Commissioner for Refugees.

Weber, J. (2023). Bedeutung raumstruktureller und arbeitsmarktrelevanter Faktoren bei innerdeutschen Wanderungen von Geflüchteten. *WISTA - Wirtschaft und Statistik*, 75(1):43–58.

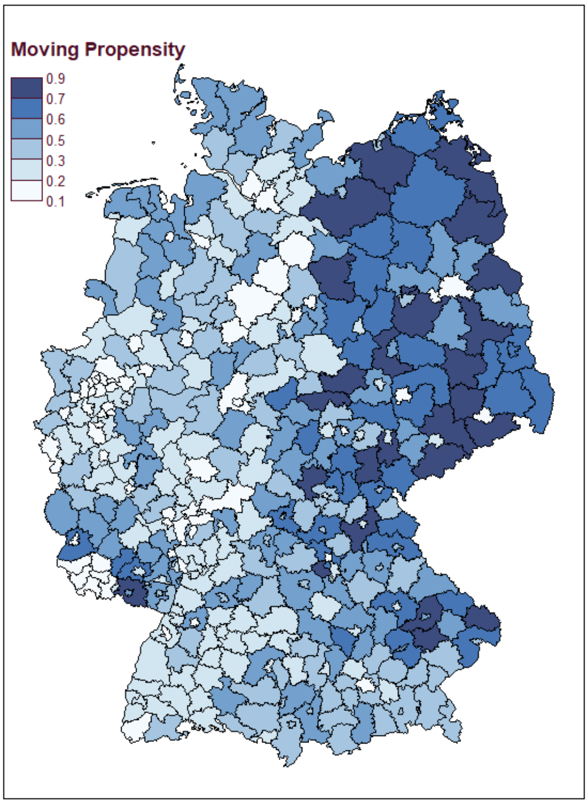
Wett, V., Torres, K. G., and Steinmayr, A. (2024). Opportunities or benefits: Local conditions and refugee labor market integration.

Wiedner, J. and Schaeffer, M. (2024). Spatial overlap: trade-offs in refugees' residential choices. *Journal of Ethnic and Migration Studies*, pages 1–23.

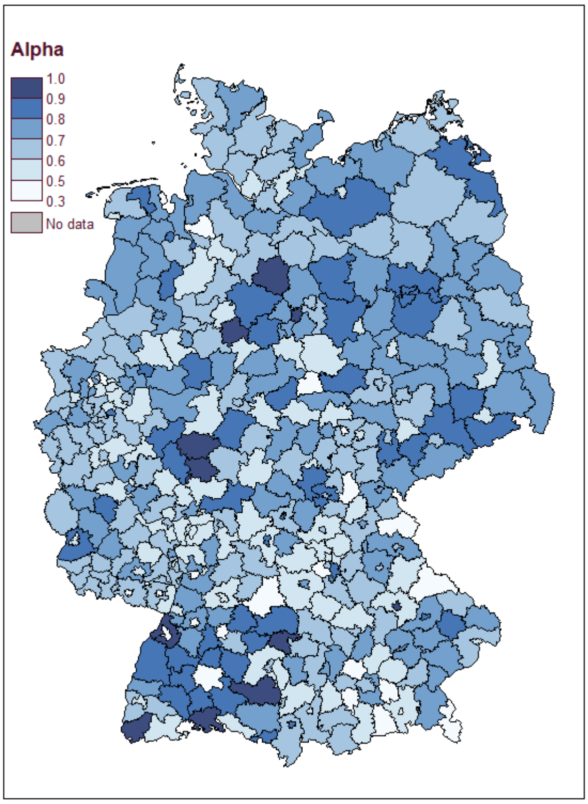
Zorlu, A. and Mulder, C. H. (2008). Initial and subsequent Location Choices of Immigrants to the Netherlands. *Regional Studies*, 42(2):245–264.

Appendix Figures and Tables

Figure A1: Regional distributions of moving propensity and average moving time



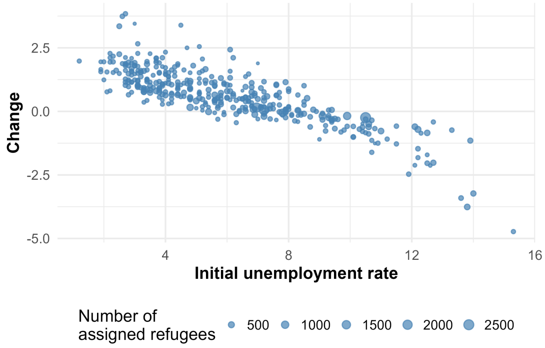
(a) Moving Decision (M)



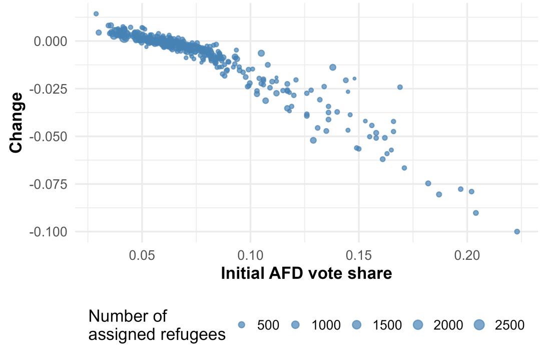
(b) Time of Move (α)

Notes: Data from AZR research dataset, author calculations.

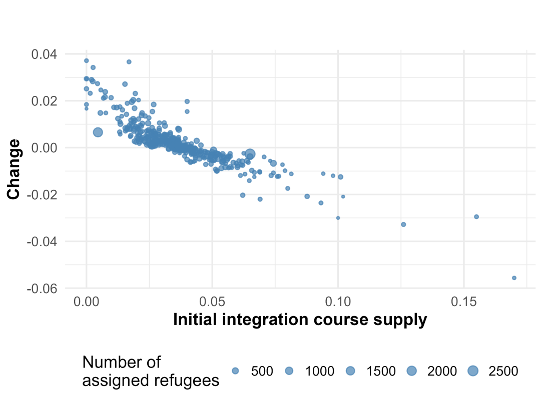
Figure A2: Initial conditions, mobility, and change of conditions



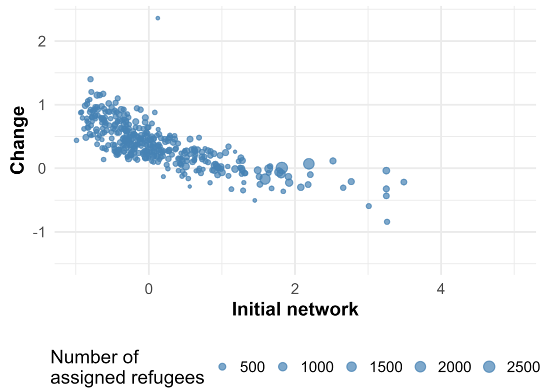
(a) Unemployment and change



(b) AfD share and change



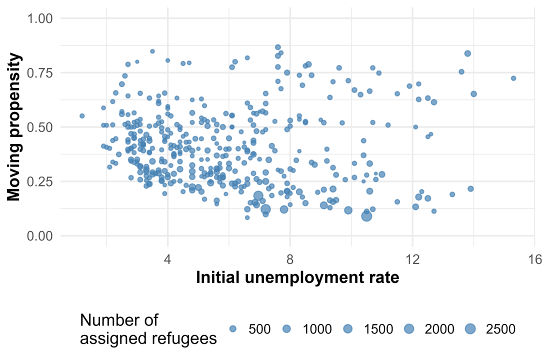
(c) Integration courses and change



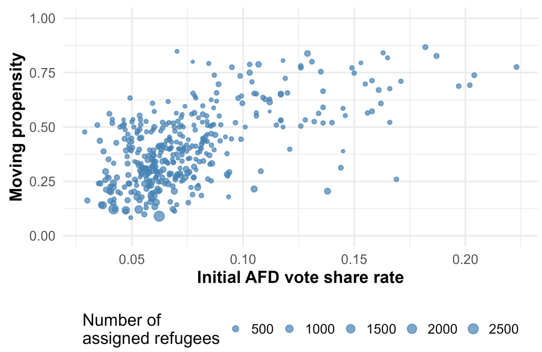
(d) Initial network and change

Notes: Data from AZR research dataset, author calculations. One region omitted from the plot for networks with initial network size of 10.

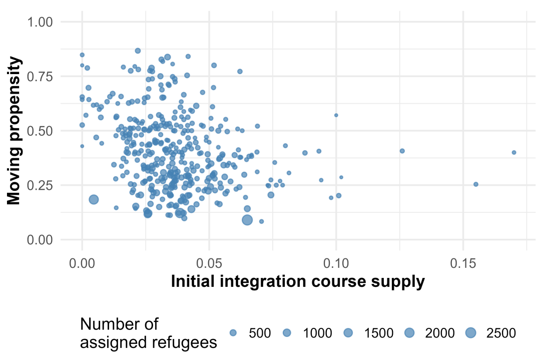
Figure A3: Initial conditions and onward mobility



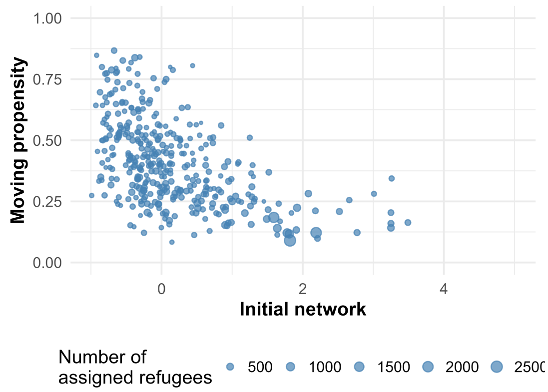
(a) Unemployment and mobility



(b) AfD vote share and mobility



(c) Integration courses and mobility



(d) Ethnic network and mobility

Notes: Data from AZR research dataset, author calculations. One region omitted from the plot for networks with initial network size of 10.

Table A1: Overview of local controls in selected RDP studies

Authors	Variation	Causal estimate	Local controls	Fixed Effects	Country (period)
Edin et al. (2003)	CS	ethn. netw.	sh. foreign, pop., unempl.	–	Sweden (1981/83, 1987/89)
Aslund and Rooth (2007)	CS*	unempl.	none	–	Sweden (1987–1991)
Damm and Rosholm (2010)	CS	pop., ethn. netw., sh. foreign	unempl., right-vote, soc.-house, priv.-house, educ. nr.	Counties (16)	Denmark (1993–2007)
Godøy (2017)	CS	immigr. empl.	unempl., migr. gr. empl., pop.	–	Norway (1993–2007)
Azlor et al. (2020)	CS	empl.	pop. sh., sh. foreign n.w.	–	Denmark (1999–2016)
Jaschke and Kosyakova (2021)	CS	Health care access	Pop. dens., Hosp. dens., sh. foreign	Federal States (16)	Germany (2013–2016)
Kristiansen et al. (2022)	CS	ethn. netw. (size & qual.), employm.	unempl., empl. sec., med. inc.	–	Netherlands (1999–2009)
Aksoy et al. (2023)	CS	unempl., sentiment	ethn. netw.	NUTS-2 Subregions (38)	Germany (2013–2016)
Kanas and Kosyakova (2023)	CS	Language course supply	Pop. dens., sh. foreign, immi. conc., cons. voting	Federal States (16)	Germany (2013–2016)
Kuhn and Maxwell (2023)	CS**	ethn. netw. (size)	–	county-year (1,200)	Germany (2013–2016)
Foged et al. (2024)	CS	immigr. empl., ethn. netw.	none	–	Denmark (1986–1998)
Biddle and Bozorgmehr (2024)	CS	soc.econ. depr.	none	Federal States (16)	Germany (2013–2016)
Kanas et al. (2024)	CS	ethn. new.	unempl., pop. dens., sh. foreign, rur. type	Federal States (16)	Germany (2013–2016)
Schilling and Stillman (2024)	CS**	sentiment, unempl. rate	See notes	Federal States (16)	Germany (2013–2016)
Damm (2009b)	FE	ethn. netw. (size)	–	Municipalities (275)	Denmark (1986–1993)
Damm et al. (2022)	FE	ethn. netw. (qual./size)	none	–	Denmark (2004–2015)
Beaman (2012)	FE	ethn. netw. (size)	–	MAs with IRC reg. office (15)	US (2001–2005)
Damm and Dustmann (2014)	FE	youth crime conv.	See notes	–	Denmark (1986–1998)
Martén et al. (2019)	FE	ethn. netw.	none	–	Switzerland (2008–2013)
Andersson (2021)	FE	sh. self-empl. co-ethn., sh. co-ethn.	none	–	Sweden (1990–1991)
Auer et al. (2022)	FE	ethn. netw.	none	–	Switzerland (2011–2017)
Battisti et al. (2022)	FE	ethn. netw. (size)	–		Germany (2013–2016)
Jaschke et al. (2022)	FE***	threat	See notes		Germany (2013–2016)
Müller et al. (2023)	FE	unempl., sentiment, ethn. netw.	pop., med. wage	–	Switzerland (1998–2018)
Tsolak and Bürmann (2023)	FE	unempl., open pos.	sh. foreign, pop. dens., gdp	–	Germany (2013–2016)
Wett et al. (2024)	FE	open pos., welfare ben.	none	–	Austria (2011–2018)
Achard (2025)	FE	sh. natives	neighborhood status score	–	Netherlands (1996–2012)
Degenhardt and Nimczik (2025)	FE	availability of GIG jobs	labor market tightness	–	Austria (2014–2022)

Notes: Local covariates as included in the main regression specifications. Abbreviations: ethn. netw. = ethnic network, sh. foreign = share foreigners, comm. = commuting, labor m. = labor market, pop. sh. = population share, pop. = population, pop. dens. = population density, unempl. = unemployment, qual. = quality, med. wage = median wage, immigr. empl. = immigrant employment rate, youth crime conv. = youth crime conviction rate, soc.econ. depr. = socioeconomic deprivation, n.w. = non-western, open pos. = open positions, welfare ben. = welfare benefits, sh. self-empl. co-ethn. = share of self-employed co-ethnics, migr. gr. empl. = migrant-group-specific employment rate, right. vote = right-wing voting share, soc.-house = share of social housing, priv.-house = share of private rentals, educ. nr. = number of education institutions, empl. sec. = employment by sectors, hosp. dens. = hospital beds density, c-r vote = center-right vote share, immi. conc. = concerns about immigration, cons. voting = conservative vote share, rur. type = rurality type. Damm and Dustmann (2014) include: poverty rate, immigrant share, population size, teacher hours/pupil, pupils/teacher ratio, crime detection rate, police officers/1,000 inhabitants. Schilling and Stillman (2024) include: log population, density, share foreign-born, share male, share aged 65+, unemployment, tourist beds per capita, empty apartments per capita, income tax per capita, share with academic degree, log GDP per capita, share of criminal cases with foreign suspect, cases per capita, refugee inflow rate. *Between-cohort variation including time but no region FE; **Includes local factors for region of residence instead of region of assignment; ***Includes unemployment rate, population density, and share of asylum seekers in baseline years interacted with year-dummies as controls. Variation used for identification: CS = cross-sectional variation across regions; FE = regional between-cohort variation with regional fixed effects.

Table A2: Overview of outmigration rates and level of local variation in selected RDP studies

Authors	**Outmigration rate (after years)**	**Local level of regressor**	**Local level of outmigration rate**
Edin et al. (2003)	48.8%–53.5% (8 years)	Municipalities (284)	Municipalities (284)
Aslund and Rooth (2007)	15–50% (1–11 years)	Labor market regions (109)	Labor market regions (109)
Damm and Rosholm (2010)	20% (20 months)	Municipalities (275)	Municipalities (275)
Godøy (2017)	38% (6 years)	Labor Market Regions (46)	Labor Market Regions (46)
Azlor et al. (2020)	17% (4 years)	Municipalities (98)	Municipalities (98)
Jaschke and Kosyakova (2021)	Not given	Counties (400)	–
Kristiansen et al. (2022)	Not given	Municipalites / 4-digit ZIP-codes	–
Aksoy et al. (2023)	8.0% (1–5 years)	Districts (259/401)	Federal States (16)
Kanas and Kosyakova (2023)	Not given	Counties (400)	–
Kuhn and Maxwell (2023)	Not given	Counties (400)	–
Biddle and Bozorgmehr (2024)	Not given	Municipalities (11,116)	–
Foged et al. (2024)	∼66.6% (15 years)	Municipalities (98)	Municipalities (98)
Kanas et al. (2024)	Not given	Counties (400)	–
Schilling and Stillman (2024)	Not given	Municipalities (10,938)	–
Damm (2009b)	52% (7 years)	Municipalites (275)	Municipalites (275)
Damm et al. (2022)	∼14% (4 years)	Neighborhoods (914/1961)	Municipalities (98)
Beaman (2012)	8.8% (90 days)	MAs with IRC reg. office (15)	MAs with IRC reg. office (15)
Damm and Dustmann (2014)	∼53% (9 years)	Municipalities (98)	Municipalities (98)
Martén et al. (2019)	0% (5 years)	Cantons (26)	Cantons (26)
Andersson (2021)	Not given	Municipalities (¡289)	–
Auer et al. (2022)	10% (9 years)	Cantons (26)	Cantons (26)
Battisti et al. (2022)	30% (7 years)	Counties (400)	Counties (400)
Jaschke et al. (2022)	25%	NUTS-2 Region (35)	NUTS-2 Region (35)
Müller et al. (2023)	12.2% (11.5 years on average)	Cantons (26)	Cantons (26)
Tsolak and Bürmann (2023)	Not given	Counties (400)	–
Wett et al. (2024)	25% (0.5 years)	Districts / Federal States (9)	Federal States (9)
Achard (2025)	–	–	–
Degenhardt and Nimczik (2025)	14% (1 year)	Cities (6)	Cities (6)

Notes: Table displays moving propensity corresponding to the main specification of the respective paper if available. If this is not given, another moving rate reported in the paper is displayed.

Table A3: Employment rates of residing refugees

Quintile of local unemployment rate	**Share of refugees with settlement permit (in percent)**	**Employment rate (in percent) of refugees with:**	
		Restricted residence permit	Settlement permit
Quintile 1	9.3	28.5	83.0
Quintile 2	8.4	23.8	78.4
Quintile 3	7.2	21.8	89.9
Quintile 4	7.0	17.9	68.6
Quintile 5	6.4	15.7	66.6

Notes: Labor market regions devided into quintiles of local unemployment rate. Data source for unemployment rate: Statistik der Bundesagentur für Arbeit (product ID: 1920); for employment rate of refugees: Statistik der Bundesagentur für Arbeit (Special analysis, Auftragsnummer 39115). Data source for share with settlment permit: Destatis (Schutzsuchendenstatistik). All variables expressed in percent. All values as of June 30, 2021. Unemployment and employment rates are aggregated to the 223 labor market regions that are present in the AZR.

Table A4: Settlement permit, employment, wage, and language proficiency

Dependent variable	Employment (any)	Full time	Wage	Language Skills
Constant	0.404***	0.285***	1,565.4***	0.432***
	(0.009)	(0.008)	(29.8)	(0.009)
Obtained Settlement permit	0.382***	0.341***	393.7***	0.307***
	(0.045)	(0.042)	(118)	(0.046)
Observations	3,256	3,256	901	3,180
R^2	0.022	0.020	0.012	0.014

Notes: Regression results for regressions of employment (any, including part time, column 1), wage (column 2), language proficiency (column 3) on whether the refugee possesses a permanent settlement permit or any other title. Data source: SOEPv37, survey year 2020. Sample includes individuals from samples M3–M5 with valid information on the respective variables. Language skills is a binary indicator that equals one if self-assessed language skills are rated at least as "good". Settlement permit equals one if the refugee reported having received a settlement permit, zero otherwise. Weighted results using cross-sectional sample weights. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A5: Refugees in AZR, legal status, and settlement permits

Legal Status (decision date)	**before 06/30/2015**	**07/01/2015 to 06/30/2016**	**07/01/2016 to 06/30/2018**	**after 06/30/2018**
Asylum, Refugee, Resettlement	8,756 (25.4%)	**28,619 (14.9%)**	**38,385 (5.3%)**	7,956 (1.5%)
Other status (among them subsidiary protection)	8,263 (9.0%)	**2,554 (3.4%)**	33,889 (0.5%)	12,630 (0.1%)

Notes: Data from AZR research dataset, author calculations. Cohorts are defined based on the month of the decision of the asylum process. Share of individuals that obtained a settlement permit by June 30, 2021 in parentheses. Red cells are excluded from the sample, green cells are included. Bold cells are in the estimation sample.

Table A6: Individual selection (cross-sectional case)

	Initial group characteristics						**Change of characteristics**			
	Unemploy-ment rate	**AfD vote**	**Ethnic network**	**Integration course**	**Moving Decision**	**Time of Move**	**Unemploy-ment rate**	**AfD vote**	**Ethnic network**	**Language course**
Female	0.02***	-0.015**	0.014**	-0.005	-0.006	0.001	-0.01	0.006	0	-0.025***
	(0.007)	(0.007)	(0.006)	(0.009)	(0.004)	(0.003)	(0.007)	(0.006)	(0.003)	(0.007)
Nationality: Other	0.016	0.008	0.213**	-0.02	-0.018**	0.019***	-0.032**	-0.004	0.036***	0.035**
	(0.019)	(0.015)	(0.088)	(0.019)	(0.009)	(0.006)	(0.015)	(0.012)	(0.012)	(0.017)
Nationality: Afghanistan	-0.018	-0.004	0.042	-0.06.	-0.046***	0.031***	-0.057***	-0.004	0.048***	0.035*
	(0.021)	(0.016)	(0.09)	(0.031)	(0.009)	(0.008)	(0.013)	(0.013)	(0.008)	(0.018)
Nationality: Irak	0.102***	-0.027	0.116*	0.063.	0.09***	-0.062***	-0.027	-0.056***	0.128***	-0.03*
	(0.029)	(0.018)	(0.062)	(0.034)	(0.015)	(0.009)	(0.018)	(0.012)	(0.019)	(0.018)
Nationality: Iran	-0.009	0.007	0.047	-0.027	0.049***	-0.039***	-0.013	-0.097***	0.089***	0.107***
	(0.026)	(0.017)	(0.038)	(0.031)	(0.013)	(0.011)	(0.016)	(0.014)	(0.011)	(0.019)
Nationality: Eritrea	-0.066***	0.017	0.051	-0.084**	-0.111***	0.047***	-0.149***	0.021	0.001	0
	(0.021)	(0.02)	(0.051)	(0.038)	(0.01)	(0.007)	(0.018)	(0.013)	(0.007)	(0.021)
Status: Subsidiary protection	-0.04**	0.016	0.001	-0.062**	0	-0.008	0.002	-0.013	0.005	0.03*
	(0.019)	(0.015)	(0.024)	(0.024)	(0.008)	(0.006)	(0.013)	(0.012)	(0.004)	(0.017)
Status: National deportation ban	0.031	-0.022	-0.079**	0.081.	-0.132***	0.037.	-0.128***	0.081***	-0.036**	-0.074*
	(0.027)	(0.022)	(0.033)	(0.045)	(0.022)	(0.022)	(0.03)	(0.027)	(0.017)	(0.038)
Status: Other	0.118***	-0.119***	0.024	0.238***	-0.244***	0.078***	-0.211***	0.187***	-0.047***	-0.195***
	(0.042)	(0.03)	(0.024)	(0.054)	(0.022)	(0.011)	(0.034)	(0.035)	(0.01)	(0.038)
Age: below 18	0	-0.017**	0.008	-0.011	-0.102***	0.059***	-0.043***	0.047***	-0.023***	-0.116***
	(0.012)	(0.008)	(0.007)	(0.016)	(0.008)	(0.006)	(0.013)	(0.01)	(0.005)	(0.016)
Age: 25 to under 30	-0.006	0.001	0.001	-0.007	-0.025***	0.018***	-0.005	0.001	-0.003	-0.017*
	(0.006)	(0.005)	(0.006)	(0.007)	(0.005)	(0.004)	(0.008)	(0.008)	(0.004)	(0.009)
Age: 30 to under 35	0.006	0	-0.009	-0.009	-0.055***	0.041***	-0.011	0.005	0.003	-0.035***
	(0.008)	(0.007)	(0.007)	(0.011)	(0.007)	(0.005)	(0.011)	(0.008)	(0.004)	(0.011)
Age: 35 to under 50	0.003	-0.003	-0.014*	-0.002	-0.099***	0.073***	-0.031***	0.025***	-0.006	-0.057***
	(0.009)	(0.006)	(0.008)	(0.011)	(0.006)	(0.005)	(0.011)	(0.009)	(0.004)	(0.013)
Age: above 50	0.007	-0.022**	0.022**	0.019	-0.083***	0.061***	-0.026*	0.015	0.001	-0.06***
	(0.013)	(0.009)	(0.01)	(0.016)	(0.008)	(0.006)	(0.014)	(0.01)	(0.005)	(0.017)
Marital status: married	-0.012	0.007	-0.006	-0.026.	-0.043***	0.04***	-0.013	0.019***	-0.009**	-0.083***
	(0.009)	(0.006)	(0.005)	(0.013)	(0.006)	(0.004)	(0.009)	(0.006)	(0.004)	(0.012)
Marital status: other	0.003	0.009	-0.007	-0.024	-0.007	0.002	0.019*	0.021**	-0.006	-0.024
	(0.02)	(0.013)	(0.007)	(0.023)	(0.007)	(0.005)	(0.011)	(0.009)	(0.004)	(0.016)
F(16,392)	3.2	2.1	4	2.8	43	49.4	7.91	5.8	7.98	10.8
Number of observations	69,558	69,558	69,558	69,558	69,558	69,558	69,558	69,558	69,558	69,558

Notes: Data from AZR research dataset, author calculations. Table displays empirical evidence for the identification assumptions from Table 1, Panel C for the AZR sample. All models additionally include fixed effects for month of arrival, month of receiving asylum and 16 Federal States of assignment. F-Tests test the short model including only the three sets of fixed effects against the long model including all other displayed covariates. Standard errors in parentheses clustered by 393 counties of assignment. Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A7: Local selection (cross-sectional case)

	Initial group characteristics						Change of characteristics			
	Unemploy-ment rate	**AfD vote**	**Ethnic network**	**Integration Course**	**Moving Decision**	**Time of Move**	**Unemploy-ment rate**	**AfD vote**	**Ethnic network**	**Language course**
Unemployment Rate		0.321***	0.042	0.153.	-0.022*	0.00		-0.01	-0.014*	-0.139***
		(0.048)	(0.026)	(0.083)	(0.012)	(0.006)		(0.022)	(0.007)	(0.041)
AfD-share	0.402***		-0.008	-0.266***	0.068***	-0.018***	-0.182***		0.016**	0.271***
	(0.068)		(0.017)	(0.101)	(0.015)	(0.006)	(0.045)		(0.008)	(0.052)
Ethnic Network Concentration	0.057*	0.004		-0.05	-0.064***	0.049***	-0.046***	-0.002		0.011
	(0.032)	(0.005)		(0.038)	(0.008)	(0.006)	(0.017)	(0.013)		(0.018)
Integration Course	0.07*	-0.056*	-0.017		-0.038***	0.011***	-0.076***	0.042*	-0.005	
	(0.04)	(0.028)	(0.013)		(0.007)	(0.004)	(0.023)	(0.024)	(0.004)	
Income	-0.349***	0.114***	-0.012	-0.185**	0.014	-0.016***	0.132***	0.03**	0.002	0.036
	(0.056)	(0.032)	(0.019)	(0.075)	(0.009)	(0.005)	(0.026)	(0.012)	(0.005)	(0.03)
Rent	-0.094*	-0.11***	0.026	0.063	0.027**	0	-0.039	-0.123***	0.007	-0.01
	(0.052)	(0.04)	(0.018)	(0.08)	(0.012)	(0.006)	(0.033)	(0.02)	(0.006)	(0.035)
Population	0.017	-0.013	-0.006	-0.185**	-0.024***	0.014**	-0.008	0.011	0.001	0.055*
	(0.046)	(0.027)	(0.022)	(0.089)	(0.009)	(0.005)	(0.014)	(0.01)	(0.005)	(0.033)
Population Density	0.333***	-0.03	0.064***	0.202**	0.005	0	-0.061***	0.008	-0.019***	-0.053*
	(0.039)	(0.031)	(0.019)	(0.08)	(0.009)	(0.005)	(0.017)	(0.012)	(0.005)	(0.028)
Academic Share	0.079	-0.091**	0.017	0.215**	-0.052***	0.018***	-0.055**	0.037**	-0.013**	-0.056
	(0.053)	(0.039)	(0.019)	(0.098)	(0.011)	(0.005)	(0.025)	(0.016)	(0.006)	(0.039)
F(8,392)	42	29.9	17.2	22.8	40.8	30.5	28.4	7.5	25.1	25.3
Number of observations	69,558	69,558	69,558	69,558	69,558	69,558	69,558	69,558	69,558	69,558

Notes: Data from AZR research dataset, author calculations. Table displays empirical evidence for the identification assumptions from Table 1, Panel C for the AZR sample. All models additionally include fixed effects for month of arrival, month of receiving asylum and 16 Federal States of assignment. F-Tests test the short model including only the three sets of fixed effects against the long model including all other displayed covariates. Standard errors in parentheses clustered by 393 counties of assignment. Local factors are standardized. Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A8: Individual selection (panel case)

	Initial group characteristics						Change of characteristics			
	Unemploy-ment rate	**AfD vote**	**Ethnic network**	**Integration Course**	**Moving Decision**	**Time of Move**	**Unemploy-ment rate**	**AfD vote**	**Ethnic network**	**Language course**
Female	0.000	-0.001	0.024	-0.009.	-0.002	-0.002	0.006	0.002	0.009	-0.019**
	(0.001)	(0.001)	(0.015)	(0.005)	(0.004)	(0.003)	(0.006)	(0.005)	(0.010)	(0.008)
Nationality: Other	0.002	0.001	1.145**	-0.003	-0.018**	0.02***	-0.022	0.007	0.097**	0.014
	(0.002)	(0.003)	(0.510)	(0.012)	(0.008)	(0.006)	(0.013)	(0.010)	(0.047)	(0.014)
Nationality: Afghanistan	-0.008**	0.024***	0.300	0.001	-0.051***	0.031***	-0.053***	0.010	-0.022	0.040**
	(0.004)	(0.008)	(0.216)	(0.014)	(0.008)	(0.007)	(0.011)	(0.013)	(0.025)	(0.016)
Nationality: Irak	-0.002	0.004	0.371***	-0.007	0.113***	-0.076***	-0.003	-0.063***	0.270***	-0.039***
	(0.002)	(0.003)	(0.119)	(0.012)	(0.011)	(0.007)	(0.013)	(0.011)	(0.039)	(0.015)
Nationality: Iran	-0.003	0.005	0.537***	0.008	0.047***	-0.04***	-0.006	-0.097***	0.232***	0.057***
	(0.004)	(0.008)	(0.188)	(0.012)	(0.011)	(0.011)	(0.012)	(0.014)	(0.041)	(0.017)
Nationality: Eritrea	-0.002	0.016***	0.310**	0.021	-0.125***	0.053***	-0.155***	0.014	-0.171***	-0.021
	(0.003)	(0.006)	(0.130)	(0.017)	(0.009)	(0.007)	(0.016)	(0.013)	(0.024)	(0.018)
Status: Subsidiary protection	-0.003	0.011***	0.080	-0.002	-0.012*	-0.005	-0.009	0.009	-0.006	-0.001
	(0.003)	(0.003)	(0.132)	(0.024)	(0.007)	(0.005)	(0.011)	(0.010)	(0.014)	(0.016)
Status: National deportation ban	0.008*	0.007	-0.310**	-0.055.	-0.124***	0.033	-0.104***	-0.002	-0.164***	-0.168***
	(0.005)	(0.008)	(0.138)	(0.032)	(0.021)	(0.022)	(0.029)	(0.024)	(0.047)	(0.042)
Status: Other	-0.016*	0.028**	-0.135	-0.062	-0.198***	0.058***	-0.149***	-0.005	-0.160***	-0.231***
	(0.008)	(0.013)	(0.101)	(0.082)	(0.021)	(0.011)	(0.029)	(0.024)	(0.038)	(0.047)
Age: below 18	-0.001	0.007***	0.024	-0.005	-0.099***	0.057***	-0.015	0.017*	-0.044**	-0.04***
	(0.002)	(0.002)	(0.028)	(0.007)	(0.008)	(0.006)	(0.011)	(0.010)	(0.018)	(0.014)
Age: 25 to under 30	0.002**	-0.002	0.009	-0.004	-0.026***	0.019***	0	0.002	-0.008	0.027**
	(0.001)	(0.002)	(0.017)	(0.006)	(0.005)	(0.004)	(0.008)	(0.007)	(0.011)	(0.011)
Age: 30 to under 35	0.002**	-0.003*	-0.024	0.008	-0.055***	0.041***	0.009	-0.004	0.008	0.023*
	(0.001)	(0.002)	(0.025)	(0.005)	(0.010)	(0.008)	(0.010)	(0.008)	(0.013)	(0.012)
Age: 35 to under 50	0.002**	-0.004*	-0.015	-0.002	-0.098***	0.073***	-0.009	0.015*	-0.020	0.024*
	(0.001)	(0.002)	(0.025)	(0.005)	(0.010)	(0.008)	(0.010)	(0.009)	(0.013)	(0.014)
Age: above 50	0.004**	-0.001	0.043	-0.006	-0.076***	0.058***	-0.002	-0.004	0.003	0.023
	(0.002)	(0.003)	(0.030)	(0.009)	(0.012)	(0.011)	(0.012)	(0.011)	(0.020)	(0.018)
Marital status: married	0.000	0.001	0.000	0.003	-0.048***	0.043***	-0.004	0.023***	-0.013	-0.023**
	(0.001)	(0.002)	(0.015)	(0.005)	(0.008)	(0.006)	(0.009)	(0.006)	(0.011)	(0.011)
Marital status: other	0.001	-0.001	-0.009	-0.005	-0.012**	0.005	0.017*	0.024***	-0.006	-0.016
	(0.001)	(0.002)	(0.012)	(0.006)	(0.009)	(0.005)	(0.009)	(0.008)	(0.013)	(0.013)
F(16,392)	1.5	2.8	5.4	1.6	53.1	56.7	9.3	6.5	10.8	6.4
Number of observations	69,558	69,558	69,558	69,558	69,558	69,558	69,558	69,558	69,558	69,558

Notes: Data from AZR research dataset, author calculations. Table displays empirical evidence for the identification assumptions from Table 1, Panel C for the AZR sample. All models additionally include fixed effects for month of arrival, month of receiving asylum and 16 Federal States of assignment. F-Tests test the short model including only the three sets of fixed effects against the long model including all other displayed covariates. Standard errors in parentheses clustered by 393 counties of assignment. Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A9: Local selection (panel case)

	Initial group characteristics						Change of characteristics			
	Unemploy-ment rate	**AfD vote**	**Ethnic network**	**Integration Course**	**Moving Decision**	**Time of Move**	**Unemploy-ment rate**	**AfD vote**	**Ethnic network**	**Language course**
Unemployment Rate		0.321***	0.296	-0.587***	-0.008	0.038*		-0.023	-0.349***	0.255*
		(0.048)	(0.301)	(0.201)	(0.030)	(0.021)		(0.050)	(0.108)	(0.137)
AfD-share	-0.261***		-0.267**	-0.270	-0.031	-0.027*	0.131***		-0.13**	-0.141*
	(0.027)		(0.110)	(0.167)	(0.019)	(0.014)	(0.043)		(0.065)	(0.080)
Ethnic Network Concentration	0.001	0.004		-0.011*	0.013***	-0.014***	-0.002	0.005**		0.013***
	(0.001)	(0.005)		(0.007)	(0.005)	(0.003)	(0.004)	(0.003)		(0.005)
Integration Course	-0.016**	-0.056*	0.403*		-0.006	0.002	-0.001	0.003	0.130	
	(0.007)	(0.028)	(0.235)		(0.004)	(0.003)	(0.008)	(0.007)	(0.106)	
Income	-0.053	0.114***	-1.193***	-0.458	-0.053*	-0.032*	-0.089*	-0.035	0.009	-0.124
	(0.052)	(0.032)	(0.328)	(0.471)	(0.030)	(0.019)	(0.054)	(0.048)	(0.123)	(0.130)
Rent	0.222***	-0.110***	0.403*	-0.511	0.068	-0.051	-0.266***	-0.115**	0.037	-0.019
	(0.082)	(0.040)	(0.235)	(0.789)	(0.055)	(0.038)	(0.100)	(0.053)	(0.365)	(0.149)
Population	-0.481	-0.013	-1.186	8.454***	-0.057	0.051	0.089	0.158	-0.100	0.003
	(0.298)	(0.027)	(0.235)	(2.649)	(0.170)	(0.128)	(0.269)	(0.183)	(0.953)	(0.479)
Population Density	-1.252	-0.030	3.311	0.139	0.457	0.334	1.507**	0.725	-0.162	2.095
	(0.813)	(0.031)	(2.495)	(0.657)	(0.435)	(0.348)	(0.729)	(0.512)	(0.206)	(1.420)
Academic Share	-0.084	-0.091**	0.218	-2264619**	0.078	0.063	0.244	0.266**	0.425**	0.859***
	(0.127)	(0.039)	(0.328)	(0.915)	(0.078)	(0.063)	(0.153)	(0.112)	(0.194)	(0.263)
F(8,392)	42	29.9	17.2	22.8	40.8	30.5	28.4	7.5	25.1	25.3
Number of observations	69,558	69,558	69,558	69,558	69,558	69,558	69,558	69,558	69,558	69,558

Notes: Data from AZR research dataset, author calculations. Standard errors in parentheses clustered by 393 counties of assignment. Local factors are standardized. Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A10: Varying Local Factors and refugee integration - no demographic controls

	Dependent variable: *obtained settlement permit*					
	CS	CS	CS	panel	panel	panel
Unemployment Rate	-0.0098***	-0.0116***	-0.0087***	-0.0212	-0.0302**	-0.0105
	(0.0027)	(0.0020)	(0.0037)	(0.0164)	(0.0177)	(0.0167)
AfD Vote Share	0.0088	0.0033	-0.0049	-0.0067	-0.0162**	-0.0017
	(0.0049)	(0.0022)	(0.0043)	(0.0085)	(0.0096)	(0.0107)
Ethnic Network	-0.0179***	-0.0160***	-0.0024***	-0.0019**	-0.0018**	-0.0020***
	(0.0059)	(0.0051)	(0.0012)	(0.0011)	(0.0011)	(0.0010)
Language Courses			0.0028	-0.0077	-0.0088*	-0.0032
			(0.0028)	(0.0047)	(0.0049)	(0.0041)
Regional Controls	No	Main	All	No	Main	All
Demographic Controls	*No*	*No*	*No*	*No*	*No*	*No*
Month of Asylum Decision (Cohort)	Yes	Yes	Yes	Yes	Yes	Yes
Federal State FE	Yes	Yes	Yes	No	No	No
District FE	No	No	No	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Columns (1) and (4) show estimates of the importance of local factors for successful integration from separate regressions with only one regional control. Additional local controls are added across columns: *Main* regional control variables are the local unemployment rate, AFD vote share, co-ethnic networks. *All* regional controls additionally include income, rent levels, population size, population density and share of academic employment. Regional covariates are standardized across regions and years. Demographic controls include sex, nationality, age, legal title and fixed effects for month of arrival. Standard errors in parentheses. All regressions for unemployment rate, AfD vote share and ethnic network based on 69,558 observations. Panel regressions for integration courses only based on years 2016-18 (n=59,631). Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A11: Cross-correlations between local factors in 5 RDP-countries

Variable of interest	**Correlation with...**	**Country**				
		Germany	Austria	Switzerland	Denmark	Sweden
Right-wing share	Unemployment Rate	0.20		-0.52	-0.09	0.05
	Share of Foreigners	-0.46		-0.28	-0.62	-0.06
	Population density	-0.29		-0.34	-0.69	-0.25
	Population	-0.16		-0.15	-0.01	-0.13
Unemployment Rate	Share of Foreigners	0.19	0.52	0.62	0.66	0.21
	Population density	0.44	0.69	0.44	0.62	0.12
	Population	0.18	0.61	0.21	0.48	0.18
Share of Foreigners	Population density	0.70	0.63	0.62	0.93	0.76
	Population	0.26	0.68	0.32	0.43	0.75
Population density	Population	0.49	0.95	0.07	0.36	0.87

Notes: Data collected from Eurostat, various sources. All indicators are analysed on the NUTS-3 level. Years differ across variables and countries: **Right-wing parties:** Germany 2021, Austria –, Switzerland 2023, Denmark 2022, Sweden 2022; **Unemployment:** Germany 2024, Austria 2021, Switzerland, Denmark 2025, Sweden 2024; **Share of foreigners:** Germany 2024, Austria, Switzerland, Denmark, Sweden 2021; **Population density:** Germany 2024, Austria, Switzerland, Denmark, Sweden 2023; **Population:** Germany 2024, Austria, Switzerland, Denmark, Sweden 2021.