How to Develop Policies That Foster Refugee Integration and Are Supported by Voters

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PhD Dissertation

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Preface

This report summarizes my PhD dissertation "How to Develop Policies that Foster Integration and are Supported by Voters" that was written at the Department of Political Science, Aarhus University. It consist of this summary and the following four self-contained articles:

A. Does Reducing the Social Assistance Benefits of Refugees Affect Their Residential Integration? Evidence from a Natural Experiment. *Revise and resubmit International Migration Review*.
B. Does Lower Benefits Incentivize Refugee Naturalization? Evidence from a Natural Experiment. *Working Paper*.
C. The Power of Place: The Effect of Forced Placement on Refugee Naturalization. *In review*.
D. Correcting Citizens' Misperceptions about non-Western Immigrants: Corrective Information, Interpretations, and Policy Opinions. *Accepted Journal of Experimental Political Science*. (Co-authored with Mathias Osmundsen).

The purpose of this summary report is to tie together the individual research articles above to provide empirical answers to the research problem that guides this dissertation. The summary aims to give a concise overview of the theoretical foundations, the research designs, data, and results contained in the articles and to present discussions that cut across the individual articles.

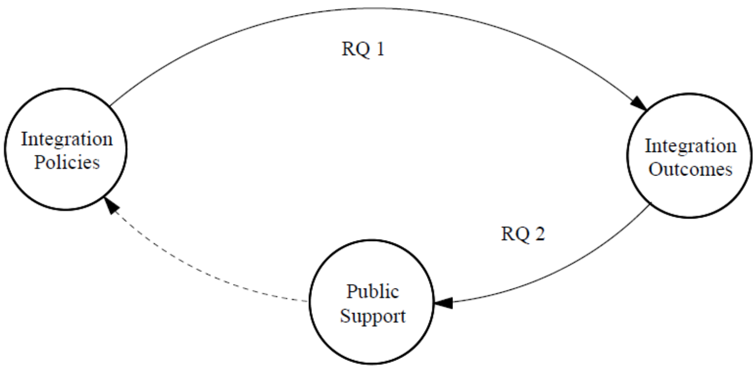
Chapter 1. Introduction

After having resettled an increasing number of refugees in recent years, many countries in Europe and the Americas have experienced marked increases in the size and diversity of their refugee populations. Many refugees arrive in their host countries very vulnerable, with few resources and face many challenges integrating into the host societies. Altogether, this means that refugee integration has become a fundamental policy challenge that has prompted governments to reassess their immigration and integration policies. In this process, much is at stake. On the refugees' side, unsuccessful integration threatens their well-being economically, socially, and mentally (Bloemraad et al. 2008; Algan et al. 2012). For the host society, integration failure has financial as well as social consequences. Financially, it hinders economic gains from the free movement of labor (Dancygier and Laitin 2014). Socially, it can fuel social conflict—given widespread perceptions that immigrants and refugees threaten the host society's culture and security—and undermine social cohesion (Fetzer 2000; Hainmueller and Hopkins 2014).

Policy makers in refugee-receiving countries are facing a fundamental challenge: how to develop policies that foster integration while simultaneously being supported by voters. The often heated debates over integration policy are structured by two contrasting paradigms. One paradigm, often supported by parties on the right, argues that strict policies—such as limited access to welfare benefits or forced placement—promote integration. The contrasting paradigm, often supported by parties on the left, holds that lenient policies—like equal access to welfare benefits or voluntary placement—catalyze social mobility and integration. The dissertation takes its theoretical point of departure in these contrasting paradigms and contributes to the ongoing debate about the fundamental policy challenge by offering empirical answers to its research problem: *How to develop policies that foster integration and are supported by voters?* The dissertation splits this problem into two parts. On the one hand, it attempts to provide a direction for future integration policies that promote integration. On the other hand, the dissertation acknowledges that policy makers face electoral constraints in the process of designing integration policies and explores concrete tools that policy makers can use to potentially remove or soften these constraints.

Figure 1 draws a stylized model of the research problem. The model is, of course, an abstraction. While there are a number of important research questions nested in the overall problem, this dissertation focuses on the two questions that are indicated by the solid arrows in Figure 1. The upper solid arrow indicates the first research question: *How do integration policies influence refugee integration*. The answer to this question speaks to the first part of the research problem by providing a direction for future policies because it gives an assessment of whether current integration policies are too restrictive if the aim is to maximize integration. The lower solid arrow reflects the second research question: *Does refugees' integration success or failure affect public support for policy?* Understanding the link between refugees' integration performance and the public's preferences regarding policy speaks to the second part of the research problem given the assumption that public opinion is an important constraint on policy design. In Figure 1, the dashed arrow indicates this assumption. Although the literature debates the degree of democratic responsiveness in policymaking, there is a consensus that public opinion does have some impact on policy. That is, while no one believes that public opinion always determines public policy, few believe that it never does (Burstein 2003).

Figure 1. Research problem



Note: Solid arrows indicate research question 1 and 2, respectively. The dashed arrow indicates the important assumption that there is a link between the public's preferences regarding policy and actual policies.

All questions asked in this dissertation are causal (as Figure 1 indicates), that is, questions about how one phenomenon, say policy X, *causes* another phenomenon, say integration outcome Y. Therefore, it is fundamental to operationalize the research questions into *researchable questions* to which I can provide credible causal answers. I translate the first research question—how does integration policies affect refugee integration—into three separate *researchable questions*. I examine these questions in three independent research articles that I discuss in detail below. In these articles, I focus on two crucial reforms that together have formed the backbone of Danish integration policy for the past two decades. I focus on Denmark, and these reforms, because they provide quasi-experimental[1] variation in important *treatments*. I leverage this variation to provide credible causal answers to questions about the impacts of these policies on important behavioral integration outcomes. Thus, I utilize that the Danish context makes it possible to link information about individuals' treatment status to important integration outcomes based on national Danish registers.

In research articles A and B, I study the start help policy that was introduced in 2002 and lowered social assistance benefits by up to 50 percent for new refugees (Huynh et al. 2007; Rosholm and Vejlin 2010). The Danish start help reform reduced social assistance benefits for refugees who obtained residency after July 1 2002, whereas refugees who obtained residency before were eligible for regular assistance. Building on the two contrasting paradigms laid out above, one view holds that reducing the assistance benefits of refugees gives them an incentive to integrate into the host society more broadly. The contrasting view sees lower benefits as a barrier to integration because they are believed to create a large underprivileged group who lives on a subsistence minimum and is denied an equal share of society's goods.

In article C, I examine the Danish dispersal policy that was fundamentally changed as of January 1 1999. More specifically, all refugees who obtained residency after this date were subject to forced placement, whereas refugees who arrived earlier were placed on a voluntary basis. According to one logic, forced placement can be expected to improve refugees' integration by securing a better geographical distribution of new refugees and thereby immersing them into ethnically Danish local communities. This should reduce their risk of becoming economically and socially marginalized in urban highly immigrant-dense areas. According to the contrasting logic, there are synergies between places and people. This means that forced placement can be expected to be a barrier to refugee integration because it eliminates individuals' ability to select into locations that match their own characteristics.

Both the Danish reform of the social assistance system and the dispersal reform provide cutoffs that sharply divide refugees into *treated* (affected by the respective reform) and *non-treated* (not affected by the reform). With traditional observational data, it is a hopeless endeavor to measure and statistically control for the myriad of confounding factors that simultaneously determine treatment and affect integration outcomes. However, these cutoffs constitute natural experiments that can be exploited in regression discontinuity

[1] Note that I use the terminology natural experiment, quasi-experiment, quasi-experimental, and quasi-random interchangeably to describe a situation where treatment is *as-if* randomly assigned to the population under investigation.

(RD) designs. Just like a controlled randomized experiment, RD designs control for *all* confounding factors *by design* given the identifying assumption that the potential integration outcomes are continuous across the cutoff. By assuming continuity across the cutoff—an assumption that I show is plausible below—I am able to provide new causal evidence for the individual effects of *reducing benefits* (start help reform) and *forced placement* (dispersal reform), respectively, on refugee integration. This evidence has important implications for theory as well as the design of policy.

Although the evidence does not provide a guide for the optimal level of policy strictness, it does give an assessment of whether current Danish policies—that have been the backbone of Danish integration policy for the past two decades—are too restrictive if the aim is to maximize integration. In this way, it answers the first research question. In addition, the evidence contributes to a relatively new literature on how policy *affects* integration. Previously, the literature was dominated by studies based on limited research designs and data that prevent them from isolating the independent treatment effects from the plethora of confounding factors that simultaneously determine treatment and affect the integration outcomes (e.g. Just and Anderson 2012; Larsen 2011; Joppke 2010; Spicer 2008; Bauböck et al. 2006; Robinson 1989). The new literature—like my own research—is sharply focused on causal identification (e.g., Rosholm and Vejlin 2010; Martén et al. 2019; Hainmueller et al. 2015, 2017, 2018).

I translate the second research question—does refugees' integration success or failure affect public support for policy—into the researchable question *would citizens hold more favorable preferences regarding integration policy had they been better informed about refugees' actual integration success or failure?* I analyze this question in the fourth article where we[2] embark on a large-scale survey experiment that isolates the effects of correct information—about the integration success/failure of the non-Western immigrant population in Denmark—on native-born Danes' preferences regarding integration policy. In particular, we provide natives with corrective information about non-Western immigrants' welfare dependency rates, their crime rates, and their overall size in relation to the total population.

Two opposing views structure the theoretical expectations to the impacts of this type of information. One view that draws on Bayesian learning models argues that citizens use information to update their evaluations of immigrants' integration performance into the host society. In this logic, the provision of information may be expected to promote more positive preferences regarding

[2] "We" refer to Mathias Osmundsen, who is my co-author in the fourth article, and me.

policy given that citizens commonly exaggerate problems related to immigrants (Sides and Citrin 2007; Nadeau et al. 1993). Another view holds that people acknowledge correct information and update their factual beliefs, but reinterpret the information in a selective fashion that justifies their existing opinions (Gaines et al. 2007). In this logic, the provision of information has little, if any, influence on citizens' policy preferences.

Besides providing an answer to the second research question, the survey experiment contributes to ongoing debates on whether correcting perceptual biases about immigrants can change citizens' preferences regarding integration policies. The study builds on Lawrence and Sides (2014) and Hopkins et al. (2019), but extends these studies in several ways. Empirically, we move the analysis to Denmark and use data from a large representative sample to test the generalizability of previous findings. Theoretically, we examine different types of correct information including crime and welfare dependency rates. Moreover, we examine mental strategies that people can use to refrain from updating their preferences when confronted with correct information.

Combined, the four research articles that make up this dissertation contribute to our understanding of how policy makers potentially can develop policies that foster integration while simultaneously being supported by voters. In the following chapter, I discuss these aspects in more detail and outline how each research article relates to ongoing debates. Chapter 3 gives an overview of the research designs and data employed by the dissertation. Chapter 4 presents the key findings of the dissertation. Finally, chapter 5 concludes by discussing the broader implications of the results for theory as well as policy design.

Although I try to keep it at a minimum, there will be some overlap between the discussions below and the discussions in the four individual research articles. This is inevitable in an article-based dissertation. For example, the discussions about the main findings of the dissertation, for the most part, build on the results included in the articles. However, whereas each individual article serves its own particular end and contributes to specific parts of literature, this report connects the individual contributions in one overall argument that answers the research problem of this dissertation. I believe that this highlights an important feature of the dissertation: there is a clear added value in seeing all the results together in one large picture—compared to seeing the results scattered across four individual articles—that makes it possible to answer the research problem of this dissertation. I hope this becomes clear when you read this report and the articles that form its foundations.

Chapter 2. Theoretical Foundations, Previous Work, and Contributions

In this chapter, I discuss the theoretical foundations of the dissertation and explain its contributions to the existing literature. I first discuss how reducing refugees' benefits might affect their integration. Second, I take up the question of forced placement and its impact on refugees' integration paths. Third, I discuss the opposite side of the research problem: the link between refugees' integration success or failure and citizens' preferences regarding policy. Finally, I summarize how the four individual research articles combined speak to the same research problem of this dissertation.

Low Benefits and Refugee Integration

Depending on household composition, the start help reform lowered assistance benefits by up to 50 percent and de facto placed refugees on a subsistence minimum in Denmark (Andersen et al. 2012). (The reader should consult the supplementary material of articles A and B for detailed information on concrete benefit levels across different family types). The political motivation of this massive benefit cut was to promote refugees' labor market integration and pave the way for integration into Danish society more broadly (Danish Prime Minister's Office 2002). Despite the immediate importance—for theory and policy—there is relatively little research that provides reliable causal evidence on the impacts of the Danish start help cuts on refugee integration.

Previous studies have demonstrated that the start help reform had a positive impact on refugees' *short-term* economic integration (Huynh et al. 2007; Rosholm and Vejlin 2010). For example, Huynh et al. (2007) show that while the employment rate is 14 percent among start help refugees 16 months after residency, the rate of regular assistance refugees is 9 percent. Although this corresponds to a 5 percentage point increase in the short-term employment rate, it also means that 86 percent still live on a subsistence minimum 16 months after their residency. The crucial question remains: how does this affect their integration? Moreover, do the short-term economic benefits outweigh the long-term costs? The few existing studies that move beyond short-term economic outcomes are based on limited data and research designs (e.g., Blauenfeldt et al. 2006; Ejernæs et al. 2010; Benjaminsen et al. 2012). These predominantly qualitative studies suggest that families under the start help policy experience severe economic deprivation that leads to *long-term* integration barriers. In articles A and B, I contribute to this literature by focusing on strong designs for causal identification and move beyond short-term economic integration outcomes.

Theoretically, two opposing viewpoints structure the debates over the social benefit levels of refugees. One view—that is often supported by parties on the rights—holds that reducing assistance to refugees is an incentive to get off welfare and find employment as a stepping-stone toward integration into the host society more broadly. The contrasting view—that is often supported by parties on the left—sees lower benefits as a barrier to the integration of refugees because it is believed to mainly create a large underprivileged group who lives on a subsistence minimum and is denied an equal share of society's goods.

The first view builds on the relatively clear-cut prediction that a reduction in benefits leads to lower reservation wages and higher job search intensity (e.g. Mortensen 1977). This has been supported in many empirical studies (e.g. Bover et al. 2002; Abbring et al. 2005; Van Ours and Vodopive 2004, 2006). The contrasting view has also found support in the literature. Economic deprivation has, for example, been linked to ethnic segregation (e.g., Borjas 1998; Crowder and Krysan 2016; Sager 2012), criminal offenses (e.g., Carr and Packham 2017; Corman et al. 2014; Foley 2011), and children's outcomes (e.g., Dahl and Lochner 2012; Duncan et al. 2011; Løken et al. 2012).

This means that one can expect lower benefits to influence refugee integration through two distinct mechanisms. In articles A and B—that study how *benefit reductions* affect refugee integration—I devise separate tests that get at each of the mechanisms, respectively. Article A examines the deprivation mechanism, which holds that benefit reductions pose a barrier to the integration of refugees. In this article, I focus on refugees' residential integration. Residential integration may be viewed as an important marker of integration into the host society in itself. For example, it affects refugees' likelihood of forming social ties and interactions with natives in the host country, which is viewed as important concepts behind the definition of social integration (Harder et al. 2018). Location matters because neighborhoods provide an important context for social interactions and shape residents' opportunities and life chances (Sampson et al. 2002). This means that residential integration is often viewed as a catalyst for further integration into the host society, whereas ethnic concentration is commonly recognized as an impediment to integration because it is perceived to slow down refugees' acquisition of country-specific human capital, including language skills and knowledge about the host country (Damm and Rosholm 2010).

Moreover, residential integration is relevant for capturing the deprivation mechanism because location choice is closely tied to the individual's economic capacity. From a socioeconomic point of view, ethnic differentials in resources explain the individual's residential choice (Borjas 1998; Crowder and Krysan 2016; Sager 2012). The start help reform de facto placed new refugees who obtained residency after the start help cutoff at a subsistence minimum in Denmark, whereas refugees who received residency before remained economically unaffected. Consequently, the reform can be expected to deprive start help refugees from location options that are economically available to regular assistance refugees. On the assumption that housing is less expensive in areas of high ethnic concentration, this deprivation mechanism predicts *that start help refugees settle in more ethnically concentrated neighborhoods compared to refugees who were eligible for regular assistance*. An additional implication of this assumption is *that start help refugees are settled in areas of relative deprivation—e.g., areas of higher welfare dependency—compared to regular assistance refugees*.

Article B examines the contrasting mechanism that benefit reductions give refugees an incentive to integrate into the host society. To tap into the incentive mechanism, this article uses citizenship acquisition as its main outcome measure of integration and short-term employment as its secondary outcome. Why should lower benefits incentivize naturalization? Naturalization comes with benefits as well as costs. Thus, from a rational choice perspective, refugees will pursue naturalization if the benefits of becoming citizen outweigh the costs. On the benefit side, "national citizenship is the highest standard of equal treatment because refugees become citizens with all the same rights, same responsibilities, and same voice in a democracy" (Bauböck et al. 2013, 40). Although refugees who are permanent residents typically have some security and protection against expulsion, naturalization ultimately transforms a foreigner into a citizen (Hainmueller, Hangartner, and Pietrantuono 2017; Vink, Prokic-Breuer, and Dronkers 2013). Moreover, in countries like Denmark where citizenship is not awarded based on place of birth, refugee children only acquire Danish citizenship if their parents naturalize. Economics studies have shown that citizenship increases refugees' employability and income because employers take into account administrative costs of verifying the right to work when hiring foreigners (Bratsberg, Ragan, Nasir 2002; Bevelander and Devoretz 2008; Steinhardt 2012). Naturalization might also signal higher levels of human capital (e.g., better language skills) and better integration of refugee applicants to employers. Empirically, citizenship acquisition has been shown to catalyze further economic, social, and political integration into the host society (Hainmueller Hangartner, and Ward 2019; Hainmueller, Hangartner, and Pietrantuono 2015, 2017).

On the cost side, a major cost is the loss of rights in the country of origin (when dual citizenship is not allowed). Loss of citizenship in the home country reduces or eliminates access to public benefits and restricts job opportunities and travel mobility there (Bratsberg, Ragan, Nasir 2002; Van Hook, Brown, and Bean 2006). Those who naturalize are also subject to the military draft (Bratsberg, Ragan, Nasir 2002). Moreover, they must bear the costs of acquiring citizenship, including bureaucratic hassles, fees and time spent on improving language skills and accumulating knowledge to pass language and civic tests (Bratsberg, Ragan, Nasir 2002; Van Hook, Brown, and Bean 2006; Bloemraad 2006; Steinhardt 2012). In addition, they have to pay a naturalization application fee, which is currently 3,800 DKK (~ 580 USD).

Altogether, citizenship acquisition is a relevant indicator for capturing the incentive mechanism because the start help reductions clearly increased the benefits of becoming citizen (or raised the costs of remaining non-citizen). In this view, one should expect *start help refugees to be more likely to naturalize compared to regular assistance refugees*. To further examine the incentive mechanism, article B also studies how start help affects refugees' likelihood of employment in the short-term. This measure taps directly into the mechanism that reducing assistance to refugees provides an *incentive to get off welfare and find employment as a stepping-stone toward integration into the host society more broadly*. In addition to the results on short-term employment, this report further provides results for the effect of lower benefits on medium- and long-term employment (for details see data section below).

The above discussions raise the important question of potential effect heterogeneity: the impact of lower benefits may very well be contingent on refugee characteristics rather than uniform across refugees. The low educated refugees face large resource constraints and marginalization that makes it difficult for them to get foothold on the labor market, which could give them the option of relocating into neighborhoods with better housing and amenities that would also imply greater co-residence and social interaction with natives. This makes for the prediction that *a negative deprivation effect (i.e., an increase in residential segregation) is concentrated among the low educated*. In contrast, the better educated refugees face fewer resource constraints and less marginalization, which make them more capable of getting a foothold on the labor market. Moreover, self-sufficiency—for the past five years—is a requirement for obtaining Danish citizenship. Combined, this makes for the expectation that *any positive incentives effect (i.e. an increase in the likelihood of naturalizing) is driven by the better educated*.

By providing causal evidence for the two separate mechanisms that shape the relationship between welfare benefits and refugee integration, articles A and B contribute to the often heated debates between two opposing camps who either promote or oppose reductions. For the design of policy, the results—that I discuss below—are important because they provide policy makers with a nuanced picture of how reducing refugees' benefits might affect their integration paths in the short and the long term. Theoretically, the results contribute to a longstanding literature on the explanations of ethnic residential segregation and a well-developed literature about the causes of naturalization. A common characteristic of these literatures is that most studies rely on limited research designs and therefore cannot isolate the independent effect of changes in benefits from the plethora of confounding factors that independently affect both the treatment and the integration outcome.

Forced Placement and Refugee Integration

Widespread concern that ethnic residential concentration hinders refugees' integration has prompted policy makers in several traditional refugee-receiving destination countries to adopt allocation policies that effectively disperse new refugees and balance ethnic compositions across geographic areas. This includes countries like Denmark, Sweden, Norway, the Netherlands, Germany, Switzerland, the United Kingdom, and the United States. In article C, I focus on the impacts of the Danish dispersal policy that is designed to direct new refugees away from immigrant-dense areas in the large cities and keep them in the areas where they are placed. The policy has two elements. First, it assigns all refugees to a specific municipality according to a pre-specified distribution key. Second, placement is *forced* in the sense that the policy stipulates that reception of social benefits is conditional on residing in the assigned municipality for the first three years. This essentially creates a geographical lock-in.

My research differs from previous studies by focusing directly on the impact of (changes to) *placement rules* on refugee integration. Prior work has focused on effects of living in ethnic enclaves, which the placement rules of course are designed to influence. Moreover, relatively little research has focused on refugees specifically, and most previous studies have problems isolating the independent effect of ethnic concentration on integration because place of residence and integration success or failure are not random. Rather, refugees select into neighborhoods with specific characteristics if they believe they will benefit from its networks. This makes it a fairly hopeless endeavor—in traditional observational studies—to isolate the independent placement effects from the plethora of confounding factors that simultaneously determine placement and affect the integration outcomes (e.g. Larsen 2011; Spicer 2008; Robinson and Coleman 2000; Robinson and Hale 1989; Robinson 1989;

Bloch and Schuster 2005). The few studies that exploit quasi-random variation typically invoke quite strong assumptions about the randomness of the assignment mechanism into placement locations (e.g. Damm et al. 2009b; Edin et al. 2003; Martén et al. 2019; Battisti et al. 2016). In particular, these studies assume that placement is exogenous to individuals' unobservable characteristics, when controlling for observable characteristics. In short, they stipulate that they *observe* the full selection mechanism.

By estimating the effect of a change in placement rules rather than the effect of living in ethnic clusters, I replace the strong assumption of selection on observables with the much weaker selection on unobservables assumption[3] (I discuss the specific assumption in detail in the section on research design below). Moreover, I would argue that the direct impacts of placement on integration are more policy relevant than the impacts of living in enclaves because the latter can only be affected indirectly by policy, whereas allocation policies of course affect placement directly.

Setting methodological considerations aside, the above studies give mixed empirical results. Studies from Denmark and Sweden show positive effects on earnings for refugees (Damm et al. 2009b; Edin et al. 2003), but no impact on employment. In contrast, Swiss and German evidence points to a positive, albeit short- to medium-term, employment effect, but no effects on earnings (Martén et al. 2019; Battisti et al. 2016). Research that moves beyond economic outcomes show effects on welfare dependency, educational attainment, and crime rates (Åslund and Frederiksson 2009; Beaman 2011; Åslund et al. 2011; Damm and Dustmann 2014).

Theoretically, one of the main motives for dispersing refugees is the hypothesis that ethnic concentration slows down the rate of host country-specific skill acquisition. According to this logic, living in an ethnic enclave means less interaction with natives and reduces incentives to acquire host country-specific human capital, including language skills, and host country-specific knowledge, which negatively affects refugees' transition into employment (Chiswick and Miller 1995, 1996; Lazear 1999). In consequence, living in an ethnic enclave is seen as obstructing integration into society. By diluting the concentration of refugees, forced placement is seen as a tool to address this challenge. In this view, *forced placement can be expected to have positive integration effects* by securing a better geographical distribution of new refugees and thereby immersing them in ethnically Danish local communities. This should reduce their risk of economic and social marginalization in urban highly immigrant-dense areas.

[3] More specifically, as I apply a regression discontinuity design, I assume that that the potential integration outcomes are continuous across the cutoff.

The opposing logic contends that certain characteristics make a refugee a better match for a particular location. This means that there are synergies between places and people that can be leveraged to optimize refugee integration (Bansak et al. 2018). From this viewpoint, forced placement is seen as a barrier to refugee integration because it eliminates individuals' ability to select into locations that match their own characteristics. Instrumentally, *forced placement can be expected to hamper refugee integration* because it creates mismatches between individual characteristics and placement locations. In particular, the geographical lock-in of the initial three-year period may very well exacerbate this problem. One important mechanism in the mismatch hypothesis may be social networks. From a social network perspective, locations matter because they can form a positive adaptive function of ethnic social networks and support. This makes initial placement crucial.

Instrumentally, networks offer assistance in practical matters such as accessing health and other welfare services, as well as interpretation. Moreover, social networks may improve labor market integration—as a first step toward broader integration into the host society—by disseminating important information about job (Portes 1987; Laezar 1999). Networks may also influence integration by transferring knowledge about social norms (e.g., attitudes toward receiving welfare, norms about early marriage, women's educational attainment, and division of labor between spouses) (Coleman et al. 1966; Wilson 1987; Case and Katz 1991; Bertrand, Luttmer, and Mullainathan 2000). Psychologically, networks may help develop confidence and self-esteem and reduce feelings of isolation and depression via emotional and financial support (Bertrand, Luttmer, and Mullainathan 2000; Chiswick and Miller 2005; Spicer 2008; Boswell 2001; Burnett and Peel 2001; Sales 2002; Zetter and Pearl 2000). Similarly, ethnic networks may guard against discrimination from natives found elsewhere (Portes 1987).

In article C, which studies the effect of forced placement, I focus on citizenship acquisition as a key measure of integration. With a high threshold[4] on acquisition of Danish citizenship, naturalization may be a good indicator for long-term integration into the society more broadly because it taps into many of the underlying aspects of successful integration. Similarly, naturalization is often used as a key measure of overall integration success in the literature (Mossaad et al. 2018). Hainmueller and Hangartner (2013) show that naturalization is an important overall indicator of successful linguistic, political, and economic integration, and Harder et al. (2018) demonstrate that these

[4] Refugees have to satisfy eight conditions to qualify for Danish citizenship. The reader should consult paper C for a detailed discussion of these criteria.

dimensions of integration are highly correlated with other dimensions including social, psychological, and navigational integration. Furthermore, citizenship acquisition has been shown to catalyze further economic, political, and social integration (Hainmueller, Hangartner, and Pietrantuono 2015, 2017; Hainmueller Hangartner, and Ward 2019).

To examine the mechanisms that link forced placement and citizenship acquisition, I use two separate sets of outcomes. First, I use secondary migration (i.e., refugees' likelihood of staying in their assigned municipality). Thus, the synergy mechanism makes for two predictions about how the relocation rates of forcibly placed refugees should develop relative to the rates of voluntarily placed refugees. Given the economic penalty that forcibly placed refugees face if they relocate, one implication of the synergy mechanism is that *forcibly placed refugees, in their first years of residency, can be expected to stay in their assigned municipalities at higher rates relative to voluntarily placed refugees*. Another implication is that *the relocation gap should slowly close over time as forcibly placed refugees become able to relocate freely without facing economic sanctions*. Second, I use local neighborhood concentrations of co-ethnics and immigrants as measures of social networks. As discussed above, one crucial aspect of the synergy mechanism may be social networks that potentially form a positive adaptive function of support. Based on the synergy mechanism, this means that *forcibly placed refugees, in their first years upon obtaining residency, can be expected to live in neighborhoods with lower concentrations of co-ethnics and immigrants* that are less conducive to the formation of social networks.

The above discussions raise the important question of potential effect heterogeneity. Thus, the forced placement effect may very well be contingent on refugees' education level. In particular, models featuring human capital externalities—that emphasize the quality of the ethnic enclave, e.g., the stock of human capital (Borjas 1995, 1998; Cutler and Glaeser 1997)—stipulates that disadvantaged member of an ethnic group benefit from living in enclaves with more advantaged members of the group. This makes for the prediction that potential *negative effects are concentrated among the less educated* who face the largest resource constraints, while the better educated and more advantaged may benefit from forced placement.

Article C contributes to the literature about placement and integration by addressing the challenges of existing studies as discussed above. From a policy perspective, the article helps remedy the lack of reliable causal evidence on the long-term integration impacts of allocation policies on more broad measures of integration. This provides policy makers with a more nuanced understanding of the consequences of their decisions when designing allocation policies.

For theory, the article contributes to understanding how initial placement matters for refugees' integration paths.

Perceptual Biases and Integration Policies

Having discussed how concrete policies might affect refugee integration, I now turn to the other side of the equation: the link between refugees' integration success or failure and citizens' preferences regarding policy. Thus, one reason we see these restrictive integration policies might be that citizens often exaggerate the prevalence of immigrants in their surroundings (Wong et al. 2012), just as they tend to hold inaccurate beliefs about the broader social and economic impacts of immigration. Moreover, people who overestimate problems with immigration are more likely to support anti-immigration policies (Sides and Citrin 2007; Nadeau et al. 1993). Taken together, this invites the question we examine in paper D of this dissertation: would citizens hold more favorable immigration policy opinions had they been better informed?

Normatively, we should expect political attitudes to bear some connection to relevant facts and to change as new facts come to the fore. As Eagly and Chaiken (1993, 103) argue, because beliefs and facts "are in some sense the basic building blocks of attitudes", changing those same building blocks ought to induce a corresponding shift in attitudes. Empirically, a series of findings in political science support to this ideal. For example, Gerber and Green (1998; 1999) draw on Bayesian learning models to argue that citizens use economic information to update their evaluations of incumbent performance. It is also well known that citizens' attitudes towards welfare policies are, at least partly, based on their stereotypic beliefs about welfare recipients (e.g., "are they lazy or motivated to work?", e.g., Oorschot 2000; Weiner 1995). However, by explicitly offering citizens factual information about the motivations of particular welfare recipients one can induce large changes in their attitudes towards them (e.g., Petersen 2012). Similarly, we may *expect that providing individuals with correct information about immigration and integration "quantities" leads to corresponding changes in preferences regarding immigration and integration policies*.

There are also reasons to believe that provision of correct information may fail to change policy opinions. Many studies on directional motivated reasoning (e.g., Kunda 1990; Taber and Lodge 2006) suggest that people respond to new information in patterns that reflect their preexisting beliefs. A well-known claim is that individuals readily accept information that fits with their preexisting worldview, but resist information and evidence that challenge or contradict them. Indeed, attempts to correct misperceptions may occasionally "backfire" and reinforce erroneous beliefs (Nyhan and Reifler 2010). Even when people accept the falsity of their misperceptions they may continue to be affected by them. In an experimental study on the effects of new information on people who were misinformed about welfare, Kuklinski et al. (2000, 802-03) found that their participants "absorb[ed] the facts", but "failed to change their preferences accordingly".

In the context of immigration and integration, this line of reasoning raises doubts that correct information would change opinions among individuals who overestimate immigrant-related problems. In one model, *the expectation is that they simply reject information offhand because it disconfirms their prior beliefs*. In another model, we might *expect that people acknowledge correct information and update their factual beliefs, but reinterpret the information in a selective fashion that justifies their existing opinions* (Gaines et al. 2007). This latter expectation is especially troublesome for democracy: if people can interpret information as they wish, they can always distort the causal chain from factual reality to political judgments.

To test the above expectations, article D utilizes a survey experiment that provides participants' with corrective information about the size of the non-Western population in Denmark, their crime rate, and their welfare dependency rate. We focus on the effects of this information on three distinct outcomes. First, to examine if participants in our survey willingly update their beliefs in light of new information, we asked them to report on their *posterior* beliefs about non-Western immigrants (relying on the same questions about beliefs as in the pre-treatment questionnaire, see research design below). Second, to study if participants adjust their policy preferences accordingly, we asked them to answer six questions about their preferences regarding immigration and integration policies in the context of an eight-item battery tapping. Finally, to examine the mental strategies people can use to refrain from updating their policy opinions—when confronted with correct information—we asked participants to indicate how they interpreted the immigration information presented to them.

Article D contributes to a relatively new, but quickly evolving literature that asks the question: would citizens hold more favorable immigration policy opinions had they been better informed? In this question lies a potential key for policy makers who are struggling to develop policies that foster integration, while simultaneously being supported by voters. However, according to two prominent studies, the answer to the question is no. Thus, Lawrence and Sides (2014) and Hopkins et al. (2019) recently showed that native-born Americans sharply overestimate the share of the foreign-born population, and that offering correct information about the size of the immigrant population does little to change their policy opinions. Instead, their findings suggest that immigration attitudes are rooted in deeply held convictions that make people discard counter-attitudinal information.

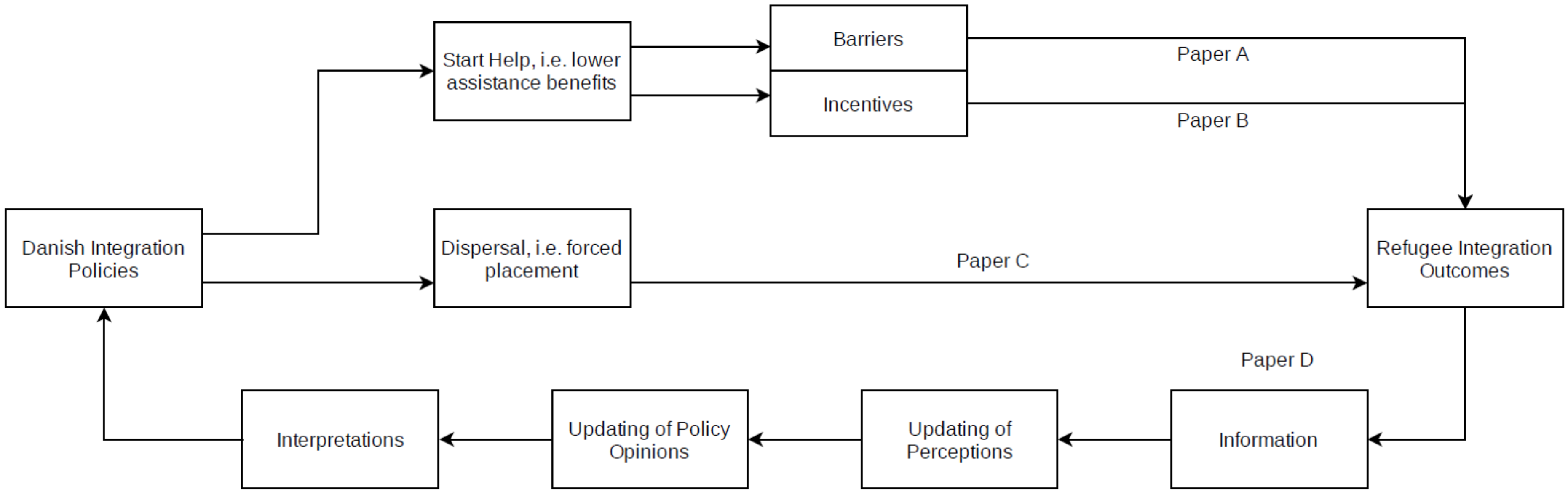
However, as the authors note, the studies have some limitations that our study seeks to remedy. First, we situate our study in Denmark to examine whether findings from the U.S. travel to other settings. Second, the studies focus on "immigrants" without identifying *specific* immigration groups. This may matter since people may feel hostility towards some immigrants (e.g. non-Western immigrants) but not others (e.g. Western immigrants) (Dennison and Geddes 2018; Hainmueller and Hangartner 2013). We focus on *non-Western* immigrants because they differ the most ethnically and culturally from native Danes and because they have received most attention in Europe, including in Denmark (Dinesen and Sønderskov 2015). Third, they only examine the effects of information about the *size* of immigrant groups. However, correcting misperceptions that touch upon cultural or economic threats posed by immigrants may have stronger effects on citizens' policy opinions. Therefore, our information treatments also provide participants with correct information about non-Western immigrants' crime and welfare dependency rates. Welfare information taps into the economic consequences of immigration, while crime information arguably touches upon cultural and security threats posed by immigrants. Consequently, we assume that natives use welfare and crime information as cues for the integration success of non-Western immigrants into Danish society. Finally, the studies do not examine *why* corrective information fails to change opinions. We examine *interpretations* as one potentially important mental tool that citizens can use to rationalize existing (anti)-immigration opinions in light of corrective information.

Bringing the Four Research Articles into One Dissertation

The four research articles and their questions might seem somewhat scattered. This is because this is an article-based dissertation, which necessarily limits how logically connected the overarching argument can be as each article serves its own particular ends. However, each of the four articles and their research questions follow from the dissertation's core question: *How to develop policies that foster integration while at the same time being supported by voters.*

Figure 2 illustrates how the articles relate to each other and to answering the overarching question. The figure draws a stylized model of how Danish integration policies affect refugee integration, and how integration success or failure feeds back into the development of policies by fostering support for either more strict or more lenient policies. Articles A and B examine the effect of the start help reform on refugee integration. Each article focuses on one of the mechanism specified above. Article C analyzes the impact of forced placement on refugee integration. Combined, articles A, B, and C give an overall assessment of how the backbone of Danish integration policy for the past decades has shaped the integration outcomes of refugees. Finally, article D contributes to understanding how information about refugees' integration success or failure may help to shape the development of new policies.

Figure 2. The contribution of each of the individual research articles of the dissertation



Note: The figure illustrates how the different contributions of the dissertation fit into a stylized model for the dissertation's research problem. Letters refer to individual research articles.

Chapter 3. Research Designs

The questions asked in this dissertation are all causal questions about how one phenomenon, say policy X, *causes* another phenomenon, say integration outcome Y. Examining causal questions empirically is challenging. I address this challenge by utilizing that the timing of specific reforms might be *exogenous* to refugees' integration paths. In this endeavor, the dissertation faces a two-stage selection problem. In the first stage, refugees decide whether to apply for asylum in Denmark or not. If refugees are able to anticipate the reforms under investigation, there is a risk that certain types select other destination countries. This represents a serious challenge to causal identification because one might mistake the effect of a compositional change in the group of refugees for the effect of the reform. In the second stage, applications are reviewed, and it is decided who obtains a permit and who does not. If decision-makers who review applications prefer specific refugees, it could create bias in the estimated effects because decision-makers typically decide on a much more informed basis about applicants than what the researcher observes. In the following sections, I discuss these inferential problems and explain how the individual research designs address the problems.

The Selection Problem

An overall aim of this dissertation is to explore how integration policies influence refugees' integration paths. Imagine, for example, that we want to know how placement affects refugee integration. Moreover, imagine that you observe two refugees, i_1 and i_2. They are placed in different locations, j_1 and j_2, and follow different integration paths. While i_1 thrives, i_2 struggles. Does this mean that placement influences refugees' subsequent integration? The causal claim that we would be making is that refugee i_1 thrives because she was placed in location j_1 and she would have struggled had she been placed in location j_2. However, because we can never observe what would have happened had refugee i_1 been placed in location j_2 rather than j_1, we can never be sure that it is in fact the placement that influenced the integration path for a given refugee. We can think of this as two potential outcomes: one where the refugee is treated and one where she is not. The "fundamental problem of causal inference" (Holland, 1986) in this setting is that we only get to observe one potential outcome for any given refugee. How then do we make causal assessments about the effects of policy on refugees' subsequent integration?

One guide for assessing the causal impact of place would be to use a cross-section of refugees settled in different places and then regress the integration outcome of interest on placement characteristics. Staying with the example above, one would simply compare the integration outcomes of refugees in location j_1 and location j_2. The mean difference between the two groups would be the estimate for the causal effect of place. That is, we impute the unobserved mean *counterfactual outcome* for refugees in location j_1 using the observed mean outcome of refugees in location j_2. However, most scholars would be skeptical about this design's ability to provide credible estimates for the actual causal impact of place because there are a plethora of confounding factors that simultaneously determine refugees' location and affect their integration paths. For example, it is plausible that refugees who are more likely to integrate select into specific enclaves and refugees who are less likely to integrate select into other enclaves. This raises concern over *selection bias*, i.e., the risk that we attribute the effect of these other outcome-related factors to the effect of place.

To address the possibility of selection bias, the dissertation leverages two major Danish reforms—that provide quasi-random assignment into treatment and control groups—as well as a fully randomized survey experiment. In the following sections, I detail how the dissertation uses the quasi-experiments and randomized experiments in causal identification strategies that provide credible answers to the research questions.

Exploiting the Start Help Reform

The Reform

In articles A and B, I exploit the start help reform to overcome the fundamental problem of causal inference. The start help reform implied that refugees who obtained residency after July 1 2002 were only eligible for reduced assistance—the so-called "start help"—whereas refugees who obtained residency before this cutoff were eligible for regular assistance (Rosholm and Vejlin 2010; Huynh et al. 2007). Here, residency refers to the date the municipal council takes over integration responsibility for a refugee, cf. the Integration Act § 4.[5] As an exception to this general rule, the reform capped the benefits

[5] According to Integration Act § 4, the municipal council takes over integration responsibility for a refugee after the end of the first whole month of obtaining residence permit. If, for example, a refugee received her permit March 8, the responsibility passes to the municipal council May 1. For reunified refugees, quota refugees, and refugees who applied for asylum at Danish embassies, the municipal council takes over integration responsibility when a refugee is registered as having arrived in the

of couples if one spouse obtained residency before the July 1 cutoff and the other after. In particular, total household benefits were capped such that it would not exceed the amount that two refugees on start help would receive. This means that a refugee who obtained residency before the reform, but whose spouse obtained after, is de facto under the start help rules. Reductions were substantial. Depending on household composition, start help was up to 50 percent lower than regular assistance and de facto placed new refugees on assistance levels that were at or below a subsistence minimum in Denmark (Ejrnæs 2003).

The motivation for these massive cuts was to promote labor market integration of refugees and thereby pave the way for integration more broadly (Danish Prime Minister's Office 2002). Importantly, only refugees and their reunified family members were affected systematically by the reform. Other types of immigrants, such as labor migrants, who wish to obtain residency have to provide for themselves or be provided for by their spouses (Huynh et al. 2007; Rosholm and Vejlin 2010). In the results section below, I therefore focus only on refugees and their families.

The start help reform was part of a larger reform package that also tightened the reunification law (reunification would now only be possible if both spouses were at least 24 years old), removed the possibility of seeking asylum at Danish embassies, and replaced the concept "de facto refugee" status with "protection status". These restrictions made it significantly more difficult to immigrate to Denmark both through family reunification and as a refugee (Huynh et al. 2007). With typical observational data, I would risk attributing the effects of these policy changes to the start help reform. However, the start help reform referred to the date of residency whereas the other policies referred to the asylum application date (Huynh et al. 2007). This means that refugees were affected equally across the July 1 cutoff by these additional policy changes, whereas only refugees who obtained residency after the cutoff were affected by the start help changes. This allows me to separate the independent start help effect from the effects of these immigration restrictions.

Empirical Strategy and Selection Biases

I leverage the fact that the Danish start help reform induced a discontinuity in refugees' social assistance benefits. In particular, I exploit this discontinuity in a regression discontinuity (RD) design that provides unbiased estimates under the assumption that the potential integration outcomes are continuous at the start help cutoff of July 1 (Hanh, Todd, and Van der Klaauw 2001). This

municipality or if the application is submitted in Denmark from the announcement of residence permit.

identifying assumption fails if refugees sort around the cutoff, which would only happen if manipulation with the date of residency occurs. Below, I *first* make a substantive argument why this manipulation is unlikely and that the start help reform constitutes a local experiment that—in the same way as a randomized experiment—enables me to isolate the causal effect of reducing assistance benefits and overcome the selection biases that I discuss below. *Second*, I validate the design-based causal identification empirically by exploiting the fact that the logic of a local experiment across the cutoff has some natural testable implications.

Manipulation could arise in two distinct stages. In the first stage, refugees decide whether to apply for asylum in Denmark. If refugees were able to anticipate the reform, there is a risk that the results would be contaminated by selection bias. However, there is strong reason to believe that refugees were not able to anticipate the reform. The start help reform had a very short parliamentary processing time of only four months: it was first proposed on March 1 2002 and was implemented as of July 1 2002. Moreover, the mean asylum application processing was about 15 months (Hvidtfeld et al. 2017). These factors imply that refugees did not have sufficient information to anticipate the reform when deciding in which country to apply for asylum.

In the second stage, applications are reviewed, and it is decided who obtains a permit and who does not. Decision-makers who review applications typically decide on a much more informed basis about applicants than what the researcher observes. If decision-makers prefer specific refugees, it could create bias in the estimated effects. However, three factors alleviate the concern over second-stage selection bias. First, the Danish Immigration Service—which decides whether there is a basis for granting asylum—bases its decision entirely on the legal criteria of the applicant's need for protection. Second, that decision-makers potentially prefer certain types of refugees to others is not enough to create bias in the estimates. They would also have to process their applications faster than other applicants' applications. This seems inconceivable. Third, I control for a rich set of covariates. Although this information does not match the full information available to decision-makers exactly, it measures the important characteristics that are available in the registers.

The above arguments are strong reasons to believe that refugees are as-if randomly distributed across the start help cutoff. However, the validity of the continuity assumption is ultimately an empirical question. I return to this question below. First, I discuss my data.

The Data

My source of data is the Danish national registers. I select all refugees who obtained their residency within ± 6 months of the July 1 cutoff, including individuals who obtained their residence permit through family reunification with a refugee. I focus only on refugees and their families because this is the only immigrant group that is systematically affected by the reform (other types of immigrants are supposed to provide for themselves or be provided for by their spouses, cf. above). Moreover, I include only refugees between ages 18 and 55 at the time of residency who were eligible for social assistance. Finally, I exclude refugees who re-migrate at some point during the period of analysis.

The registers hold a personal identifier that I to construct a dataset that links information about refugees' exact date of residency to information on the integration outcomes discussed in the theory section. In particular, I use the information on the date of residency to code the start help indicator (i.e., the treatment). The indicator is coded in accordance with the eligibility criteria (see the description of the start help reform above). In general, this means that the treatment is coded 1 for refugees who obtained residency after July 1 and 0 for refugees who obtained residency before July 1. However, spouses who obtained residency on each side of July 1 are both de facto only eligible for start help and are therefore both assigned 1 on the treatment indicator. To ensure consistency between date of residency and eligibility status, these spouses are assigned the date of residency of the last arriving spouse.

As discussed in the theory section, I employ two sets of outcomes. The first set is designed to tap into the economic deprivation mechanism, and the second set to tap into the incentives mechanism. For the first set of outcomes, I use residential integration (i.e., the concentration of non-Western immigrants[6] in the individual's residential surroundings) and neighborhood welfare dependency (i.e., share of unemployed in the individual's neighborhood). These outcomes are computed at the parish level, which is the smallest predefined administrative and geographically delimited unit in Denmark (average parish population size is 2,603). In particular, I have information on refugees' addresses as of January 1 every year from 2003 through 2015 and consequently compute the first set of outcomes for every year in this period.

In the second set of outcomes, I use employment and citizenship acquisition to tap into the incentives mechanism. Citizenship acquisition is a dummy that takes the value 100 if the refugee has acquired citizenship as of December

[6] I compute the measure as the share of people in the individual's surroundings who do not originate from the EU-15 countries, Iceland, Norway, Andorra, Liechtenstein, Monaco, San Marino, Switzerland, The Vatican State, Canada, USA, Australia, and New Zealand.

31 2015. Employment is a yearly measure that I compute from 2003 through 2015. It categorizes each refugee as *employed* (takes the value 100) if her main source of income over the preceding year is employment, that is, she was employed for at least half of the year. The measure categorizes refugees as *not employed* (takes the value 0) if they predominantly relied on welfare benefits or dropped out of the labor force.[7]

I group the outcomes that are measured on a yearly basis (i.e., ethnic residential concentration, neighborhood welfare dependency, and employment) into three intervals—including the short, medium, and long term—and take the average over the interval years. I define short-term integration as the years 2003-2005. The justification for this delineation is the Danish dispersion policy according to which refugees' reception of social assistance benefits is conditional on them residing in their assigned municipality the first three years of residency. I split the remaining 10 years into two 5-year intervals. That is, I define the medium term as effects in the years 2006-2010 and the long term in the years 2011-2015.

I merge the treatment and outcome information with information about the refugee's background characteristics based on the personal identifier as well as unique spouse and family identifiers. These covariates include sex, age, education, grouped origin, first region of residency, whether the refugee is married, and whether the refugee has children. Table 1 provides descriptive statistics for all variables described above for refugees within ± 6 months of the July 1 cutoff. The top panel presents the treatment data, the middle panel the individual characteristics, and the bottom panel the outcome data.

[7] This could happen if she was exempted from working, for example because of disabilities or if she dropped out of the welfare system.

Table 1. Descriptive statistics for refugees within ± 6 months of the July 1 cutoff

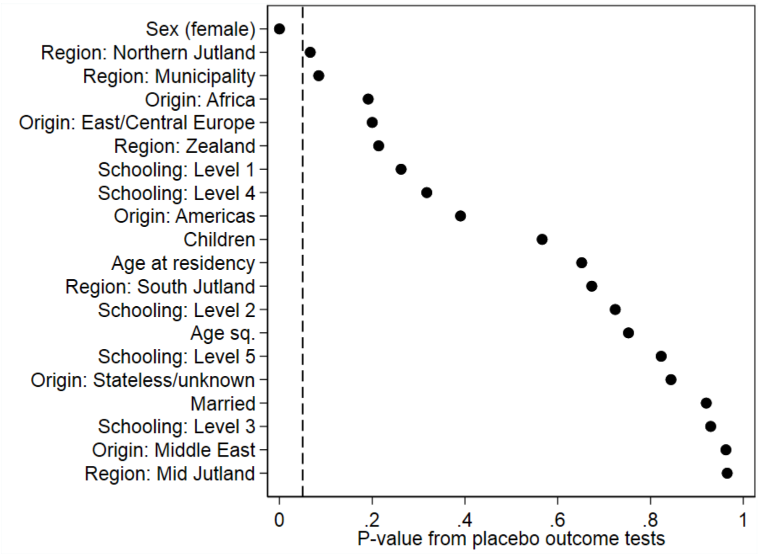
Variable	Mean	SD	Min	Max
Start help indicator	0.55	0.50	0.00	1.00
Residency date (centered on the cutoff)	12.77	111.27	-181.00	182.00
Age	32.61	8.00	18.00	54.92
Female	0.52	0.50	0.00	1.00
Education (months)	137.11	48.21	0.00	240.00
Education levels				
Level 1	0.17	0.38	0.00	1.00
Level 2	0.16	0.36	0.00	1.00
Level 3	0.38	0.49	0.00	1.00
Level 4	0.20	0.40	0.00	1.00
Level 5	0.09	0.29	0.00	1.00
Married	0.74	0.42	0.00	1.00
Children	0.74	0.44	0.00	1.00
Grouped origin				
East or Central Europe	0.16	0.37	0.00	1.00
Middle East	0.68	0.48	0.00	1.00
Africa	0.15	0.36	0.00	1.00
Americas	0.001	0.03	0.00	1.00
Stateless/unknown	0.01	0.09	0.00	1.00
Region of residency				
Municipality	0.16	0.37	0.00	1.00
Zealand	0.20	0.40	0.00	1.00
Southern Jutland	0.26	0.44	0.00	1.00
Mid Jutland	0.23	0.42	0.00	1.00
Northern Jutland	0.15	0.36	0.00	1.00
Ethnic concentration (short-term)	5.37	5.41	0.39	51.87
Ethnic concentration (medium-term)	11.05	10.47	0.81	59.11
Ethnic concentration (long-term)	15.36	12.94	1.31	71.80
Welfare dependency (short-term)	44.46	7.29	24.75	71.32
Welfare dependency (medium-term)	45.96	7.39	26.57	67.75
Welfare dependency (long-term)	50.38	7.93	31.03	74.86
Employment (short-term)	23.70	32.45	0.00	100.00
Employment (medium-term)	33.88	41.84	0.00	100.00
Employment (long-term)	41.17	42.09	0.00	100.00
Citizenship	19.54	39.67	0.00	100.00

Note: All variables are measured in the Danish administrative registers. The top panel of the table presents the treatment data. The middle panel presents all individual background characteristics that are measured by the date of residency (i.e., the first entry in the registers). Education: level 1 corresponds to primary school; level 2 is more than primary school but less than high school, level 3 is a high school education; level 4 is equivalent to a bachelor's degree or higher; level 5 is missing data. The bottom panel presents the outcome measures. Citizenship is measured as of December 31 2015. The remaining outcomes are measured in years 2003-2015: short-term is years 2003-2005; medium-term is years 2006-2010; long-term is years 2011-2015. N = 2,324.

Empirical Assessment of the Continuity Assumption

Figure 3 illustrates one important implication of the RD design-based identification. Building on the intuition of a local experiment, one should expect covariates to be balanced across the cutoff. I use this logic in a series of placebo outcome tests where I estimate placebo effects for each covariate by substituting in the covariate as outcome in the RD design. For each placebo outcome test, I plot the p-value from regressing the placebo outcome on the treatment, the residency date, and the interaction between the two within ± 6 months of the cutoff. This is the same specification as for the main results. The dashed vertical line indicates a p-value of 0.05. If selection in either the first or the second stage is a problem, one should expect a jump in the covariates at the cutoff. That is, one would expect to observe p-values below the 0.05 threshold.

Figure 3. Start help RD design: Placebo outcome tests



Note: Distribution of p-values from the placebo outcome tests where each placebo outcome is regressed on the treatment, the residency variable, and the interaction between the two. Bandwidth is ± 6 months from the cutoff. Vertical dashed line indicates a p-value of 0.05. There is no statistical evidence for discontinuities in background characteristics that potentially confound the comparison at the cutoff.

Instead, the figure displays a distribution of p-values that is consistent with what one would expect to observe at random. That is, only 1 of 20 covariates is imbalanced across the cutoff: there are fewer females above the cutoff. This imbalance reflects that men typically arrive first and are reunified with their spouses later. Given the treatment assignment, where both spouses would only be eligible for start help if one obtained residency above the cutoff, this means that men are disproportionately "moved" across the cutoff.[8] In conclusion, the p-values from the placebo outcome tests approximate the uniform distribution as expected given local randomization at the cutoff.

In article A and B, I provide further evidence on the robustness of the design-based identification. In particular, McCrary's (2008) test indicates no sign of sorting, there are no jumps at placebo cutoffs, and relatively stable effects across varying widths of the estimation window. Overall, the empirical results from the identification tests and the substantive justification suggest that the start help reform provides an ideal design for identifying the causal impact of reducing refugees' benefits on their subsequent integration.

Exploiting the Danish Dispersal Policy

The Reform

In article C, I exploit changes in the Danish dispersal policy to solve the causal identification problem. A period with marked increases in asylum applications in the 1980s prompted the government to make the first policy adjustment. In 1986, the government implemented a policy of voluntary dispersal. Under the charge of the Danish Refugee Council (DRC), refugees were offered assistance to find housing immediately upon approval of their asylum application. If the individual accepted, she would fill in a form about her background including family relations and nationality. The spatial dispersal was a two-stage process. First, the DRC would assign the individual to one of 15 counties approximately 10 days later. Second, having provided temporary housing in the receiving county, local offices assisted in finding permanent housing within the county (Damm 2009a, 2010). The DRC tried to distribute refugees evenly across counties based on their relative number of inhabitants. At the county level, the DRC aimed at balancing the number of refugees across municipalities with suitable housing, educational institutions, opportunities of employments, and co-nationals. Approximately 90 percent of the refugees accepted the offer of being provided with or assisted by the DRC in finding permanent housing under the terms of the dispersal policy (Damm 2005b). Importantly, however, placement was voluntary, and refugees faced no restrictions on relocation. They were allowed to relocate at any time without sanctions involving social benefits (Damm 2009a, 2010).

[8] The imbalance disappears if one uses their own date of residency (disregarding family reunifications) as the running variable.

January 1 1999 marks an important shift in Danish integration policy as the government introduced Denmark's first Integration Act (*Integrationsloven*). Whereas refugees who obtained residency before 1999 were subject to the old rules, the new law affected all refugees who obtained residency after January 1 1999. The law reformed the dispersal policy from a voluntary to a forced system. This means that all refugees with a residency date after January 1 1999 would be dispersed across municipalities according to a prespecified distribution key.[9] Moreover, placement in the assigned municipality was mandatory for the first three years. In contrast to the previous rules, reception of social benefits was conditional on residing in the assigned municipality for a three-year period. That is, refugees could only relocate if they found a job in a different municipality. This de facto created a geographical lock-in.[10]

According to the Danish Ministry of Integration, the new dispersion rules should both ensure a better geographical distribution of new refugees and secure that they remain in their assigned municipalities. The intention of this policy was to promote refugee integration by exposing new refugees to ethnically Danish local communities and thereby reduce their risk of becoming economically and socially marginalized in urban highly immigrant-dense areas. Under the former rules, refugees had been placed mainly in urban areas with relatively large immigrant and refugee populations, whereas the new law successfully dispersed newly arrived outside urban municipalities (Larsen 2011; Nielsen and Jensen 2006).

The Integration Law stipulated that refugees would receive *introductory benefits*—somewhat below regular social assistance—during this period (Nielsen and Jensen 2006). Moreover, it expanded the introductory integration program—which includes language courses and is mandatory for refugees—from 18 to 36 months. This means that I risk conflating the effects of these changes and the effect of the dispersal policy change. Importantly, however, the introductory benefits were revoked shortly after their implementation because of pressure from the UNHCR and a government evaluation that showed that the lower benefits did not have the desired effects. This alleviates the concern about attributing effects (especially in the long term) from this change to the effect of forced placement. Conflating the integration program change and

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[9] Computed as the municipality's share of the total population subtracted the difference between the municipality's share of the total number of immigrants and refugees and the municipality's share of the total population.

[10] Note, however, that the new dispersion rules de facto did not apply to refugees who obtain residency after January 1 but have a spouse who obtained residency before the cutoff because they are settled with their spouse.

the dispersal policy change remains a concern. As I show below and in greater detail in article C, the effect estimates display an overall negative impact of forced placement on refugee integration. However, one should expect increased training to positively influence integration. Therefore, if anything, I would argue that the results below are conservative estimates of the actual effect of forced placement.

Empirical Strategy and Selection Biases

Just like the start help cutoff, the forced placement cutoff introduces a discontinuity. I exploit this discontinuity in an RD design that under the same assumptions as described above identifies the forced placement effect on refugee integration across the January 1 1999 cutoff. Similar to the start help estimates, a two-selection bias threatens causal identification: researchers must isolate the causal effect of placement from the selection bias that determines which refugees (1) apply for asylum and (2) receive asylum.

First stage selection is only a concern if refugees had sufficient information to anticipate the reform. There is good reason to expect that refugees lacked this information as the reform was implemented over a 6-month period.[11] A mean asylum application processing time of about 5 months and considerable variation in processing times (Flygtningenævnet 1999) make it very unlikely that refugees were able to anticipate the cutoff and submit an asylum application in due time to get under the old placement rules. Consequently, first stage selection should not be a problem. However, second stage selection remains a concern. Like the start help reform, the same three factors alleviate concerns over second stage selection bias.[12]

The Data

Just as the data on lower benefits, these data are based on the Danish national registers. I select all refugees who obtained residency within ± 183 days (i.e., half a year) from the January 1 1999 cutoff. I focus only on refugees because this is the only immigrant group that is systematically affected by the reform. I exclude refugees with spouses who obtained residency outside this period.

[11] The reform was adopted July 1 1998 and took effect January 1 1999.

[12] These factors include (1) that the Danish Immigration Service bases its decision entirely on the legal criteria of the applicant's need for protection, (2) that decision-makers would not only have to prefer certain types of refugees to other, but also process their applications faster for it to generate bias, and (3) control for a rich set of covariates.

Moreover, I only include refugees who were adults (at least 18 years old) at their time of residence.

In this group of refugees, I construct a dataset that links information on refugees' exact date of residency (i.e., information on the treatment) and information on the outcomes discussed in the theory section above. Refugees above the cutoff are assigned 1 on the treatment indicator, and refugees below the cutoff are assigned 0. As the new dispersion rules de facto did not apply to new refugees whose spouse obtained residency before the cutoff (see explanation above), the treatment indicator also takes the value 0 if refugees arrive after the cutoff but their spouse arrive before. To ensure consistency between the treatment indicator and the date of residency, I assign the de facto untreated refugees—whose spouse arrive before the cutoff—their spouse's date of residency.

As discussed in the theory section, I use citizenship acquisition as a key measure of integration and my main outcome (citizenship status is measured by December 31 2015). To explore the mechanisms that link forced placement and citizenship acquisition, I use secondary migration (i.e., refugees' likelihood of staying in their assigned municipality) and local social networks (measured as local concentrations of co-ethnics and immigrants) as alternative outcomes. I merge the treatment and outcome data with background characteristics of the individual refugee based on the personal identifier as well as unique spouse and family identifiers. Except for first region of residency, which is in some way part of the forced placement treatment, these background characteristics correspond to the characteristics included in the lower benefit studies. Table 2 shows descriptive statistics for all variables described above for refugees within ±6 months of the July 1 cutoff. The top panel presents the treatment data, the middle panel the individual characteristics, and the bottom panel the outcome data.

Table 2. Descriptive statistics for refugees within ±6 months of the January 1 cutoff

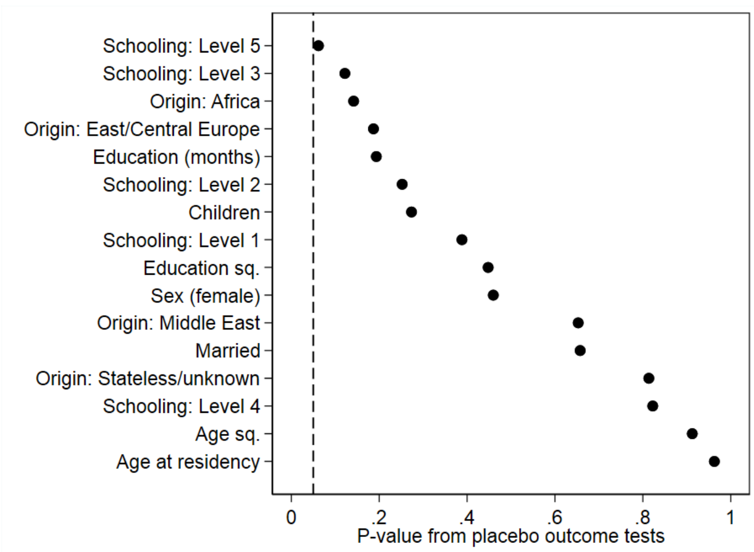
Variable	Mean	SD	Min	Max
Forced placement indicator	0.32	0.47	0.00	1.00
Residency date (centered on the cutoff)	-25.66	106.07	-183	182
Female	0.46	0.50	0.00	1.00
Age	33.76	11.68	18.04	87.60
Education (months)	137.28	41.32	0.00	204.00
Education levels				
Level 1	0.21	0.41	0.00	1.00
Level 2	0.09	0.29	0.00	1.00
Level 3	0.40	0.49	0.00	1.00
Level 4	0.16	0.36	0.00	1.00
Level 5	0.13	0.34	0.00	1.00
Married	0.68	0.47	0.00	1.00
Children	0.46	0.50	0.00	1.00
Grouped origin				
East or Central Europe	0.28	0.45	0.00	1.00
Middle East	0.56	0.50	0.00	1.00
Africa	0.16	0.37	0.00	1.00
Citizenship	47.88	49.97	0.00	100.00
Social networks				
Local conc. co-ethnics	5.44	8.40	0.09	49.42
Local conc. immigrants	10.63	12.19	0.26	58.84
Inter-municipality migration (stay)				
2000	67.21	48.24	0.00	100.00
2001	62.97	49.20	0.00	100.00
2002	58.30	49.62	0.00	100.00
2003	53.82	49.87	0.00	100.00
2004	51.27	50.00	0.00	100.00
2005	48.85	50.00	0.00	100.00
2006	47.64	49.96	0.00	100.00
2007	35.27	47.80	0.00	100.00

Note: All variables are measured in the Danish administrative registers. The top panel of the table presents the treatment data. The middle panel presents all individual background characteristics measured by date of residency (i.e., the first entry in the registers). Education: level 1 corresponds to primary school; level 2 is more than primary school but less than high school, level 3 is a high school education; level 4 is equivalent to a bachelor's degree or higher; level 5 is missing data. The bottom panel presents the outcome measures. Citizenship is measured as of December 31 2015. The social network outcomes are measured by the end of the refugees' first full year of residency. N = 1,650.

Empirical Assessment of the Continuity Assumption

Appreciating that the validity of the continuity assumption is ultimately an empirical question, it should be detectable in the placebo outcome tests if selection bias in either the first or the second stage is problematic. Figure 4 plots p-values from placebo outcome tests where I apply the same specifications as for the main results. There is no statistical evidence for discontinuities at the cutoff in any of the background characteristics. That is, Figure 4 displays a distribution of p-values that approximates the uniform distribution as expected given local randomization at the cutoff. (Article C discusses other traditional identification tests and provides further evidence on the robustness of the design-based identification). Altogether, the above arguments and empirical justifications strongly suggest that the RD design is able to separate the independent causal effect of forced placement from the myriad of potential confounders.

Figure 4. Forced placement RD design: Placebo outcome tests



Note: Distribution of p-values from the placebo outcome tests where each placebo outcome is regressed on the treatment, the residency variable, and the interaction between the two. Bandwidth is ± 6 months from the cutoff. Vertical dashed line indicates a p-value of 0.05. There is no statistical evidence for discontinuities in background characteristics that potentially confound the comparison at the cutoff.

Exploiting Randomization: Survey Experiments

Article D, the final article of the dissertation, is based on a series of randomized survey experiments. The randomized experiment is often considered a golden standard for causal inference because full randomization into treatment and control groups ensures that the groups, in expectation, are similar on all confounding characteristics. By design, this rules out the selection problem.

We embedded our experiment in a survey administered to a nationally representative sample of Danes. Responses were collected in June 2017 by Survey Sampling International (n = 1,747). The sample was drawn to match the broader population of Denmark with respect to age, gender, income, and education. We only include participants who have at least one parent born in Denmark and with Danish citizenship (n = 1,638). The median age in the sample was 30 years (SD = 15 years), and 50 percent were female. 10 percent of our participants had not graduated high school, 21 percent had vocational training, 13 percent were high school graduates, 11 percent had some college education or were currently enrolled in college, 27 percent were college graduates, 16 percent had a post-college degree, while 2 percent did not answer the question. Median income was "between $60,000 and $74,999" (DKK 400,000-499,999). On a 10-point political ideology self-identification measure, 1 denoting the left-wing extreme and 10 denoting the right-wing extreme, the median value was 5.

In article D, our design follows a three-step sequence where participants first reported their "best estimates" (*prior beliefs*) of three types of facts about non-Western immigrants. Second, we randomly assigned participants to receive or not receive correct information about the same facts. Third, we asked participants to report on our outcomes: first, they reported their best estimates of the immigration facts (*posterior beliefs*), second their immigration *policy opinions*, and finally how they *interpreted* the immigration facts.

In the first step of the sequence, we included a diverse set of immigrant-related concerns to capture as much of real-life politics as possible. In particular, participants were asked to state their prior beliefs about non-Western immigrants' crime rate, their welfare dependency rate, and the size of the non-Western population living in Denmark. Regarding crime, we asked: "In 2016, out of 100 crimes in Denmark, how many do you think were committed by immigrants or descendants from non-Western countries?" Regarding welfare, we asked: "In 2016, out of 100 people receiving social benefits in Denmark, how many do you think were immigrants or descendants from non-Western countries?" Regarding size, we asked: "In 2016, out of 100 people living in Denmark, how many do you think were immigrants or descendants from non-Western countries?"[13] We randomized the question order and for each question, participants used a sliding cursor to choose a number between 0 and 100.

[13] We focus on "immigrants and descendants" because in the Danish context the two groups are almost always discussed together. We use absolute rather than relative immigration numbers to mirror the information stems in Hopkins et al. (2019).

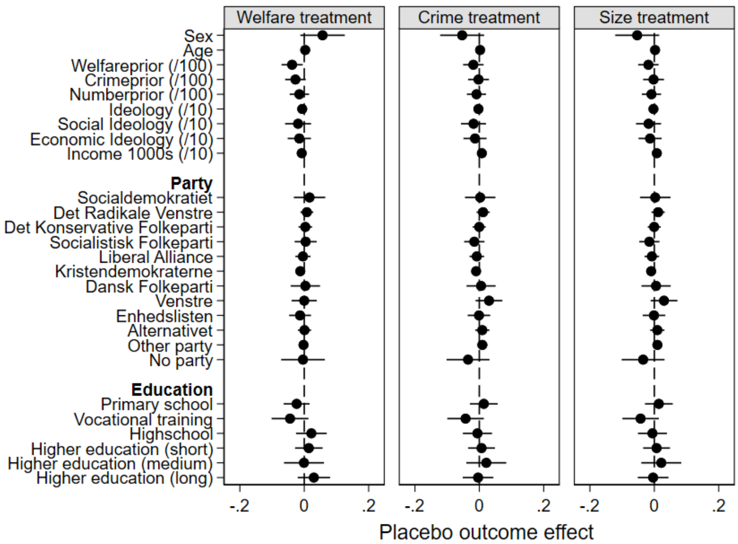
In the second step of the sequence—following a series of filler questions—we then assigned participants to either a "no-information" control condition (n = 410) or one of three treatment conditions. In the three treatment conditions, participants received information about either the number of crimes committed by non-Western immigrants in Denmark (*crime condition*, n = 409), the number of non-Western welfare recipients in Denmark (*welfare condition*, n = 410), or the share of non-Western immigrants (*size condition*, n = 409). The three information treatments included the same preamble: "We are interested in whether you have heard about a story that has recently been in the news. The story is …" For the crime condition, participants subsequently read, "[a] new report from Statistics Denmark shows that 21 out of 100 people who were convicted of committing a criminal offense in Denmark in 2016 were immigrants or descendants from non-Western countries." The welfare condition stated that 14 out of 100 welfare beneficiaries were non-Western immigrants, and the size condition stated that 8 out of 100 people living in Denmark were non-Western immigrants. All statistics were based on true information from the official government agency Statistics Denmark, which is generally regarded as a trustworthy source.

In the third and final step of the sequence, we asked participants to report on our three outcome measures. First, in the context of an eight-item battery tapping a number of policy preferences, participants saw two items related to immigrants and crime (e.g., "Politicians should make it easier to expel criminal immigrants"), two items related to immigrants and welfare (e.g., "Refugees and immigrants who live in Denmark should have the same right to economic support as ethnic Danes"), and two items related to the preferred number of immigrants in Denmark (e.g., "Denmark should receive more refugees than is the case today"). All answers to the immigration-related questions were summed into an index, scaled to range from 0 to 1, with higher values indicating greater support for anti-immigration policies. (I refer the reader to appendix A of article D for all question wordings).

Second, as our measure for posterior beliefs, we relied on the prior beliefs measures from the pre-treatment questionnaire and asked participants to report again their best guess on each of the three issues. Third and finally, we asked participants to indicate on five-point scales how they interpreted the immigration information presented to them. For example, participants in the *crime* condition were asked: "Thinking back on the report from Statistics Denmark, which showed that 21 of 100 crimes in Denmark were committed by immigrants or descendants from non-Western countries, do you think that number is very low, low, neither/nor, high, or very high?" We asked participants in the welfare and size conditions similar questions. Participants in the control condition received three questions, one for each of the immigration quantities, and were asked to "imagine that a report from Statistics Denmark showed ...", followed by the same correct statistics used in the treatment conditions.

An implication of the randomization logic—that the treatment and control groups are similar on all confounding characteristics—is that we should observe no differences between the groups in the observed covariates. In Figure 5, we utilize this logic to validate the design-based identification of our information treatments. In particular, we plot the point estimates and their corresponding 95 percent confidence intervals from placebo outcome tests where we regress the covariates on each treatment, respectively. The tests show that the pre-treatment covariates are well balanced across the control and treatment groups and thus support the identifying assumption that the potential outcomes are independent of the assignment into the treatment and control groups.

Figure 5. Randomized information treatments: Placebo outcome tests



Note: Each filled black circle shows an estimated placebo effect for each pre-treatment covariate. Estimations compare the mean of the treated group to the mean of the control group. Black lines show 95 % confidence intervals based on robust standard errors against heteroscedasticity. The tests show that the pre-treatment covariates are well balanced on average and thus support the key identifying assumption of random assignment.

A Note on Generalizability

While the research designs provide high internal validity, the designs are more limited in terms of the external validity of their results. The scope of the external validity is limited on three dimension. First, the choice of Denmark as the "case" under study. Second, the choice reforms (treatments) studied. Third, the selection of outcomes.

The first and second choice are motivated in the same overall consideration: all questions asked in this dissertation are causal and I therefore need (exogenous) variation in my treatments that enables causal claims. The Danish case is unique in that it provides relevant reforms that yield this (exogenous) variation while it simultaneously gives the possibility of combining micro data about the treatments with micro data on relevant integration outcomes in the national Danish registers. Moreover, I argue that the focus on the specific reforms is warranted because they have formed the backbone of Danish integration policy for the past two decades.

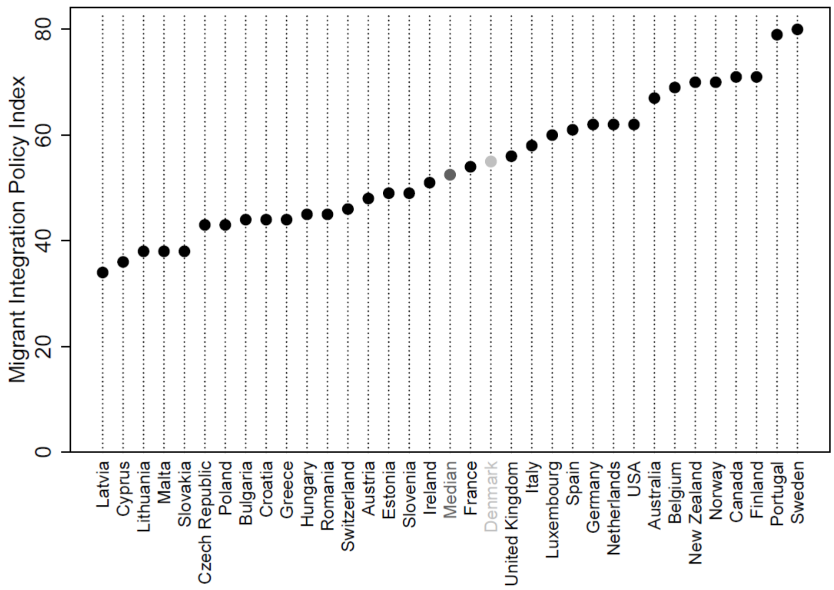
One might contend that focusing so strongly on quasi-experiments unnecessarily limits the dissertation's scope of inference. In one sense, this is true. External validity is best studied by replicating the results of studies with high internal validity in other countries and other periods. It would therefore have been better to study more quasi-experiments across other contexts. However, it is questionable if the dissertation would have benefitted from a less stringent focus on causal identification and more focus on cross-country comparisons.

First, the immigrant integration literature, which makes up the main frame of this dissertation, has been dominated by studies based on limited research design and data—including cross-country comparisons—that prevent them from isolating the independent treatment effects from the myriad of factors that simultaneously determine treatment and affect the integration outcomes. This should be clear from the discussions in chapter 2. Second, Samii (2016) argues that it is not true that data from a variety of sources, i.e., a dataset that includes different contexts (e.g., different countries) increase the generalizability of our results. Rather, statistical control in standard observational studies limits the variation on the independent variable of interest in a way that the estimated effect most of the time will be just as 'localized' as its experimental or quasi-experimental counterparts. That is, conventional statistical control limits the set of *comparable control cases*—used to make the comparisons from which the effect estimate is derived—so that the resulting effect estimate cannot be generalized to *non-comparable cases*.

Taken together, it is unclear whether the dissertation would gain generalizability from a well-controlled cross-country study compared to the quasi-experimental strategy that it employs. However, it seems clear that the loss of internal validity by employing a cross-country strategy would be significant and that the clear contributions from the quasi-experimental strategy would be jeopardized.

Overall, generalizability beyond Denmark remains an open question. One guide for assessing the generalizability of the findings is to compare the strictness of Danish integration policies to regimes in other Western countries, for instance based on the migrant policy integration index (MIPEX). On the MIPEX, Danish policies are about as restrictive as the sample median. This may suggest that the effect estimates might be representative more broadly. At this point, one can only speculate how the results below might generalize to other countries. On the one hand, the results may provide an upper bound for the effects in countries with more restrictive regimes than Denmark because there would less room for policy restrictions to affect integration. In a similar logic, the Danish results could provide lower bounds for the effects in more liberal regimes because restrictions would have more room to affect integration. On the other hand, there might exist a threshold in terms of restrictiveness where the effects of policy tightenings become very different. If this is the case, then results may be different in contexts that are either more or less liberal than Denmark.

Figure 6. Migrant policy integration index for European and North American countries



Note: MIPEX measures a country's integration policies based on 167 integration policy indicators where 0 represents as unequal policies as possible and 100 as equal as possible.

Returning to the limitation in terms of choice of outcomes: my outcome selection serves its own particular end as each individual article contributes to specific parts of literature. For example, in articles A and B I select outcomes to devise tests that target each of discussed mechanisms separately. Whether the causal claims I make generalize beyond the studied outcomes is an open questions. However, there is not one agreed upon measure of refugee integration that is applied consistently in the literature. Rather, integration is a multifaceted concept and different studies have used different measures to capture the various dimensions of integration. Building on Kymlicka's (1995, 2012) definition of integration[14], Harder et al. (2018), for example, discuss six dimensions of integration including, social, economic, political, psychological, linguistic, and navigational integration. Moreover, they show that these dimensions tend to be highly correlated, such that a high level on one dimension is associated with a high level on another dimension. This suggests that my findings might very well generalize beyond the studied outcomes.

Overview of Research Questions, Research Designs, and Data

Table 3 summarizes the discussion above and provides an overview of the individual articles, including research questions, outcomes, identification strategies, and data sources.

[14] Who sees success as the degree to which immigrants have the knowledge and capacity to build a successful, fulfilling life in the host society. Integration means that all barriers to full participation in the society have been removed.

Table 3. Overview of research questions, data, and research designs employed in the dissertation

Research Question	Outcomes	Identification Strategy	Data Source
Do reductions in refugees' welfare benefits constrain their integration?	• Residential integration, i.e. the concentration of non-Western immigrants in the individual's residential area. • Neighborhood welfare dependency, i.e. the share of unemployed in the individual's neighborhood.	• Comparison of refugees who are eligible for regular social assistance benefits to refugees who are only eligible for sharply reduced benefits (start help). • Employing an RD design that utilizes that the start help reform abruptly discontinued refugees' benefits.	Register data from Statistics Denmark that leverages a unique personal identifier to combine data on each refugee's date of residency and information on the integration outcomes (n = 2,324).
Do reductions in refugees' welfare benefits incentivize their integration?	• Citizenship acquisition. • Economic integration, i.e. employment.	• Comparison of refugees who are eligible for regular social assistance benefits to refugees who are only eligible for sharply reduced benefits (start help). • Employing an RD design that utilizes that the start help reform abruptly discontinued refugees' benefits.	Register data from Statistics Denmark that leverages a unique personal identifier to combine data on each refugee's date of residency and information on the integration outcomes (n = 2,324).
Does forced placement promote or hamper refugee integration?	• Citizenship acquisition. • Secondary migration, i.e. likelihood of staying in the assigned municipality. • Local neighborhood concentrations, i.e. share of co-ethnics and share of immigrants.	• Comparison of refugees under forced placement who face economic sanctions if they relocate to refugees who can freely relocate without sanctions. • Employing an RD design that utilizes that the dispersal policy reform abruptly discontinued refugees' placement.	Register data from Statistics Denmark that leverages a unique personal identifier to combine data on each refugee's date of residency and information on the integration outcomes (n = 1,650).
Would citizens hold more favorable immigration policy opinions had they been better informed?	• Posterior beliefs, i.e. best estimates about immigration facts. • Immigration policy opinions. • Interpretations of immigration facts.	• Comparison of individuals who receive one of three relevant facts about non-Western immigrants. • Employing survey experiments that utilize randomization.	Experiment embedded in a survey administered to a nationally representative sample of Danes collected in June 2017 by Survey Sampling International (n, control group = 410; n, crime treatment = 409, n, welfare treatment = 410; n, size treatment = 409).

Note: Sample sizes refer to total effective sample used in the analyses (i.e. excluding missing data). For more detail, see the sample selection in the individual articles.

Chapter 4.
Key Findings and Discussions

This chapter provides an overview of the dissertation's key findings. I start by discussing how reductions in refugees' benefits affect their integration outcomes. I then turn to the question of forced placement and its effects on refugee integration. Finally, I discuss whether citizens would hold more favorable immigration policy opinions had they been better informed about refugees' actual integration success or failure. Note that the reader should consult the individual articles and their supplementary information for more elaborate and detailed analyses, robustness tests, and so forth.

For the most part, the discussions below build on the same results that are included in the individual research articles. One exception is the first section, where I discuss how reductions in refugees' benefits affect their integration outcomes. In article B, I examine the impact of the start help reform on citizenship acquisition and *short-term* economic integration. In this report, I also include the reform's impact on *medium-* and *long-term* economic integration.

Low Benefits and Refugee Integration

Below, I estimate the effect of start help on refugee integration by regressing the respective integration outcomes on the start help indicator, the residency variable, and the interaction between the two. All estimations in this result section are without covariates and within ±6 months of the July 1 2002 cutoff.[15] In articles A and B, I display results both with and without covariates. The inclusion of covariates checks the design-based identification: if refugees who obtain residency just before and after the cutoff are similar in all confounding respects, then including or excluding covariates should not substantively alter the effect estimates. Consistent with the placebo outcome tests above, the effect estimates are relatively stable across models with and without covariates (please consult the result sections of article A and B for details on results with covariates). Moreover, in the appendices of article A and B, I show that results are relatively insensitive to varying the width of the estimation windows. Overall, this proves the robustness of my findings.

[15] Note that in article A, I use a triangular kernel—upon request by a reviewer—that weights refugees at the cutoff more than refugees away from the cutoff. To align the results across articles, I weight observations equally across all estimations in the results section of this dissertation. Note that the use of kernels that weight observations differently does not make a substantive difference for the results.

Core Effects of Lower Benefits on Refugee Integration

In article A and B, I examine how benefit reductions influence refugee integration. As discussed in the theory section, I expect that reductions can influence refugees via two distinct mechanisms. On the one hand, reductions may be seen as a barrier to integration because they mainly create a large underprivileged group who lives on a subsistence minimum and is denied an equal share of society's goods. On the other hand, reductions may be perceived as an incentive to get off welfare and find employment as a stepping-stone toward broader integration into the host society.

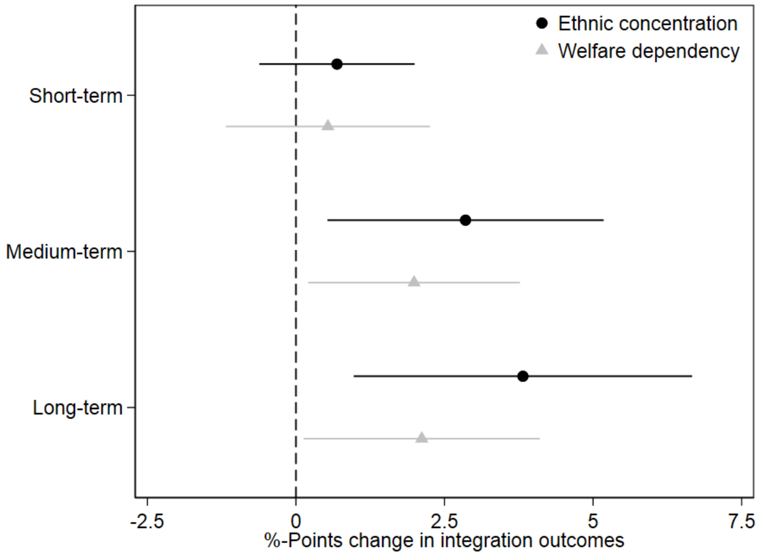
Figure 7 tests the expectations derived from the deprivation mechanism. The filled circles first give the main results from article A on the effect of lower benefits on residential integration. The assumption behind the expectation that refugees on start help settle in areas of higher ethnic concentration compared to refugees on regular assistance is that housing is less expensive in residential areas with high concentrations of non-western immigrants. This assumption yields another testable implication, namely that refugees on start help live in neighborhoods with higher welfare dependency rates. The filled triangles test this expectation and indicate the secondary results from article A on the effect of lower benefits on neighborhood welfare dependency.

The figure shows a clear empirical pattern that is consistent with the theoretical expectations. In the short term, there is no effect of lower benefits on residential integration ($\alpha_{\text{ethnic concentration}} = 0.69$, $P < 0.300$). The short-term null effect reflects the Danish dispersion rules that affect refugees equally across the start help cutoff and ensure that they stay in their assigned municipality for the first three years after residency. In contrast, there is a sharp increase in the effect estimate in the medium term ($\alpha_{\text{ethnic concentration}} = 2.85$, $P < 0.017$). This means that refugees on start help settle in neighborhoods with about 3 percentage points higher ethnic concentration compared to refugees on regular assistance. Relative to the counterfactual mean at the cutoff of about 10 percent, this corresponds to a 30 percent increase in the medium term. It demonstrates that once refugees can move freely—without facing economic—the expected effect of lower benefits on residential integration materializes. This effect estimate increases further in the long term as refugees on start help live in neighborhoods that are about 4 percentage points more segregated ($\alpha_{\text{ethnic concentration}} = 3.82$, $P < 0.010$).

Moreover, the figure clearly shows that the RD effects on neighborhood welfare dependency closely mirror the effects on ethnic concentration ($\alpha_{\text{short-term welfare dependency}} = 0.54$, $P < 0.540$; $\alpha_{\text{medium-term welfare dependency}} = 1.99$, $P < 0.030$;

$\alpha_{\text{long-term welfare dependency}} = 2.11$, $P < 0.038$). Overall, this corroborates the assumption that housing prices and ethnic concentration go hand-in-hand and supports the interpretation that economic deprivation rather than an alternative mechanism drives the results.

Figure 7. Effects of lower benefits on residential outcomes

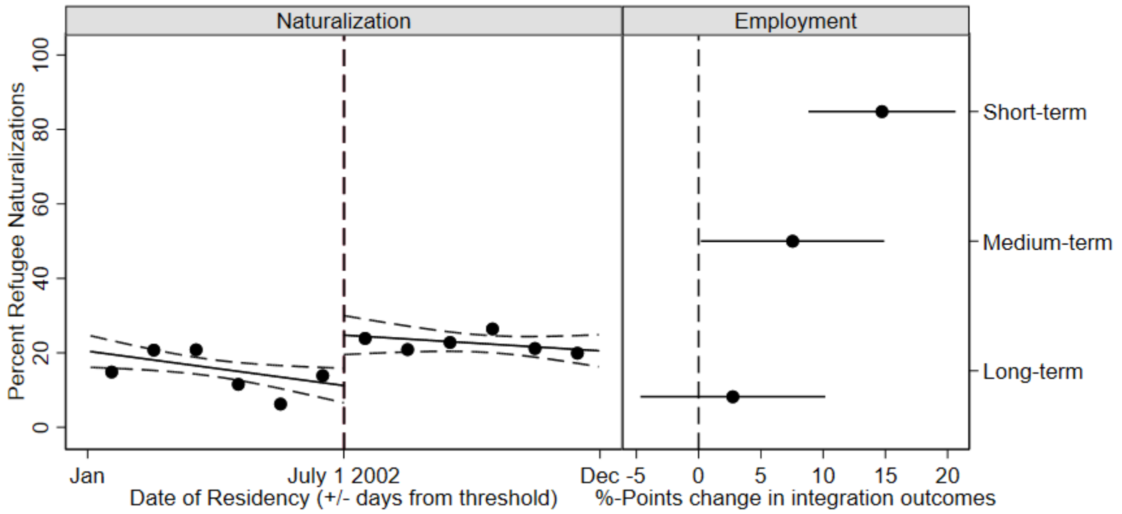


Note: RD effect estimates with 95 % confidence intervals. Standard errors are clustered by municipality. Bandwidth ±6 months. N = 2,324.

Figure 8 tests the expectations derived from the incentives mechanism. The left panel graphically gives the main results from article A regarding the impacts of lower benefits on citizenship acquisition (as of December 31 2015). Building on a rational choice perspective, I expect that the start help reductions have a positive impact on citizenship acquisition because they increase the benefits of citizenship (or raise the costs of remaining non-citizen). There is a close connection between "historical" employment and citizenship acquisition. Thus, to acquire citizenship, refugees must be self-sufficient. Specifically, refugees cannot have received social assistance for more than an aggregate period of four months within five years of the naturalization date. Given this connection, I expect a positive effect on employment. Moreover, this measure taps directly into the underlying incentives mechanism given that a reduction in assistance increases the benefits of getting off welfare and finding employment. The filled circles in the right panel test this prediction.

The figure shows an effect of lower benefits on citizenship acquisition of about 13.5 percentage points ($\alpha_{\text{citizenship acquisition}} = 13.55$, $P < 0.002$). Compared to the counterfactual mean at the cutoff of about 11 percent, this corresponds to approx. 120 percent increase in the naturalization rate. Looking at the employment outcome, the results tell a different story: benefit reductions have a large positive short-term effect, but the impact dissipates over time and become statistically indistinguishable from zero in the long term ($\alpha_{\text{short-term employment}} = 14.72$, $P < 0.0001$; $\alpha_{\text{medium-term employment}} = 7.74$, $P < 0.046$; $\alpha_{\text{long-term employment}} = 2.75$, $P < 0.468$). Although I can only speculate at this point—because I do not have the updated data to test the following conjecture—the findings suggest that the naturalization effect could also dissipate over time given its close link to the employment outcome.

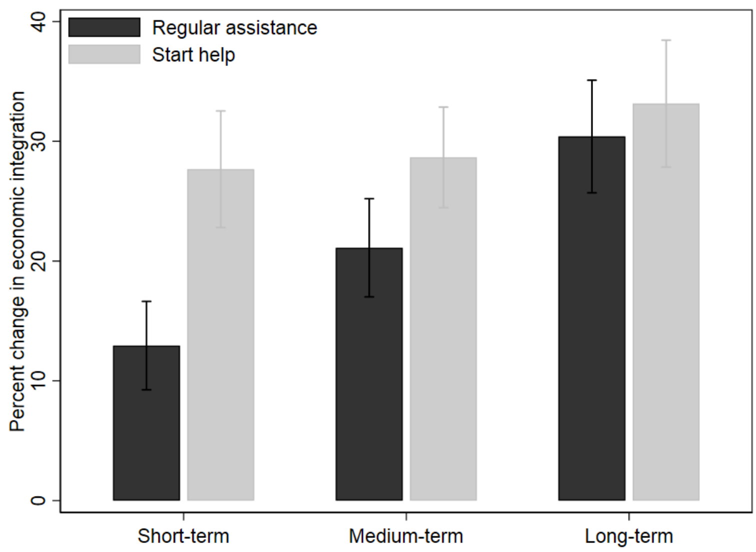
Figure 8. Effects of lower benefits on citizenship acquisition and employment



Note: Left panel graphically displays the result of applying the RD design. Lines are average naturalization rates (with 95 % confidence intervals) on each side of the cutoff. Right panel gives the RD effect estimates on employment with 95 % confidence intervals. Standard errors are clustered by first municipality. Bandwidth ±6 months. N = 2,324.

The above findings suggest that benefit reductions of the magnitude that put refugees on a subsistence minimum have enduring negative effects that in the long term hinder integration of refugees. The findings suggest that reductions in benefits provide an incentive for some refugees to get off welfare and find employment, but this effect is rather short-lived and offset in the long term. This empirical pattern can be explained in two ways. Either it reflects a *catch-up* where regular assistance refugees over time close the gap, in terms of economic integration, to start help refugees. Alternatively, it reflects a *reversion*, where start help refugees over time are more likely to fall back into unemployment. To distinguish between the catch-up and reversion mechanism, Figure 9 plots the mean employment rates of refugees on regular assistance and start help, respectively, at the cutoff.[16] The figure demonstrates that the mean employment rate does not change much over time in the start help group, whereas there is a clear upward trend over time in the regular assistance group. These findings support that the regular assistance group catches up in terms of economic integration. This explains why the effect of the incentive mechanism dissipates over time. However, it remains an open question why the economic deprivation mechanism contrastingly has enduring effects. One explanation might be that the mechanisms are activated among different refugees. This raises the important question of effect heterogeneity.

Figure 9. Catch-up or reversion?



Note: Means at the cutoff with 95 % confidence intervals. Standard errors are clustered by municipality. Bandwidth ±6 months. N = 2,324.

Effect Heterogeneity

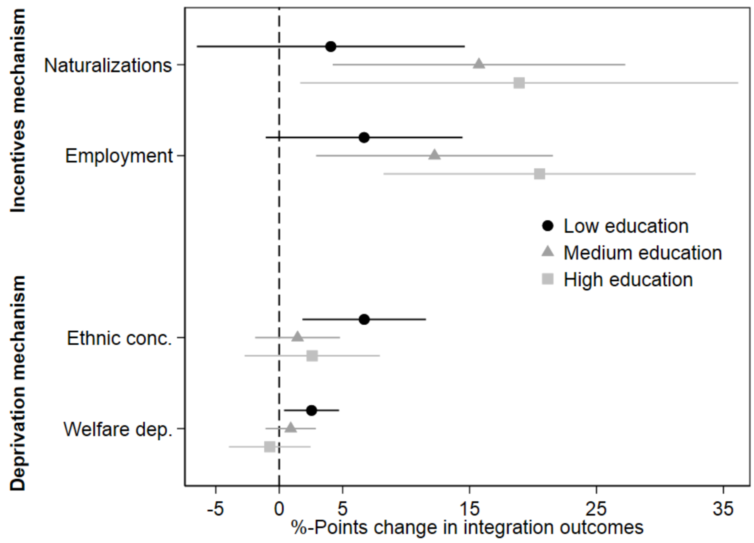
As discussed in the theory section, it is very likely that effects are contingent rather than uniform across refugee characteristics. In particular, I expect the positive incentive effects to be driven by better educated refugees who face fewer resource constraints and less marginalization, and I expect that the negative deprivation effects are concentrated among the less educated who face larger resource constraints and more marginalization. Such heterogeneity in the effects would explain the decline of the incentive effect and the simultaneous rise of the deprivation effect over time.

To test these propositions, I replicate the above models while splitting the sample according to education level. In particular, I split education at arrival

[16] To obtain the correct standard errors, I regress the employment outcome on the (centered) residency variable for the regular assistance and start help group separately and extract the respective constants.

into low (black circles), medium (grey triangles), and high (grey squares) educated[17]. Figure 10 displays the results of these subgroup analyses. Consistent with the theoretical predictions, the figure clearly demonstrates that the positive incentive effects are concentrated among the better educated. While the effects are relatively small and statistically insignificant among the low educated ($\alpha_{\text{citizenship acquisition}} = 4.07$, $P < 0.450$; $\alpha_{\text{employment}} = 6.69$, $P < 0.091$), the estimates increase and are statistically significant among the medium educated ($\alpha_{\text{citizenship acquisition}} = 15.74$, $P < 0.009$; $\alpha_{\text{employment}} = 12.24$, $P < 0.011$) as well as high educated refugees ($\alpha_{\text{citizenship acquisition}} = 18.91$, $P < 0.033$; $\alpha_{\text{employment}} = 20.52$, $P < 0.002$). Consistent with the expectations, the figure displays the contrasting pattern for the deprivation outcomes. The estimated effects are small and statistically insignificant among the high educated ($\alpha_{\text{ethnic concentration}} = 2.60$, $P < 0.338$; $\alpha_{\text{welfare dependency}} = -0.75$, $P < 0.648$) as well as medium educated ($\alpha_{\text{ethnic concentration}} = 1.45$, $P < 0.395$; $\alpha_{\text{welfare dependency}} = 0.91$, $P < 0.369$). Negative deprivation are clearly driven by the low educated ($\alpha_{\text{ethnic concentration}} = 6.70$, $P < 0.008$; $\alpha_{\text{welfare dependency}} = 2.55$, $P < 0.022$).

Figure 10. Are the effects of lower benefits heterogeneous?



Note: RD effect estimates with 95 % confidence intervals. Standard errors are clustered by municipality. Bandwidth ±6 months. N (low education) = 914; N (medium education) = 944; N (high education) = 466. Outcomes: Naturalization, (short-term) employment, (long-term) ethnic concentration, and (long-term) neighborhood welfare dependency.

[17] I define low education as less than or equal to 120 months. This corresponds to 10th grade in the Danish primary school. To this group I add refugees with unknown education level. Medium education is defined as more than 120 months and less than 180 months, which corresponds to a bachelor's degree. Finally, high education is more than 180 months.

Overall, the findings show that lower benefits influence refugee integration in both positive and negative directions. On average, it has a positive impact on short-term economic integration. However, there is a catch-up effect, where refugees on regular benefits slowly close the employment gap over time. In essence, this means that the short-term economic effect dissipates over time. In comparison, lower benefits have negative impacts on residential integration that persist in the long-term. Moreover, it is clear from the analyses above that while the short-term positive incentive effects are driven by the better educated; the long-term negative deprivation effects are concentrated among the less educated. This gives a more nuanced understanding of how benefit reductions affect refugee integration. In fact, the reductions push the better educated off welfare and into employment more quickly than if they had been eligible for regular assistance. However, as this effect fades over time, the negative deprivation effect rises among the low educated. Altogether, this suggests that lower benefits create barriers rather than providing incentives for the integration of refugees in the long term. Moreover, it seems that these barriers materialize among the low educated, whereas the better educated remain seemingly unaffected. From a policy standpoint, these findings are especially concerning because they show that low benefits marginalize those who have few chances of integrating into the host society at the outset and push them further towards the margins of society.

Forced Placement and Refugee Integration

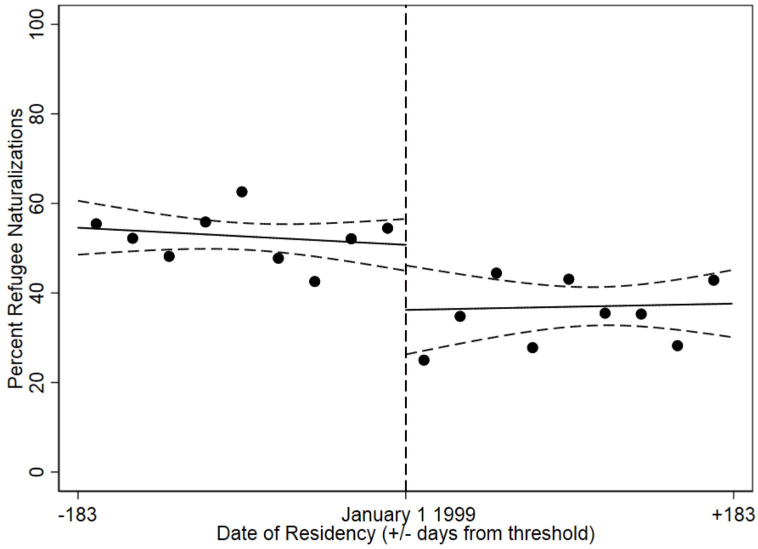
Below, I estimate the effect of forced placement on refugee integration by regressing the respective integration outcomes on the placement indicator, the residency variable, and the interaction between the two. All estimations in this result section are without covariates and within ±6 months of the January 1 1999 cutoff. In the main manuscript of article C, I display results both with and without covariates. Consistent with the placebo outcome tests above, the effect estimates are relatively stable across these models (I refer the reader to the result section of article C for details on the estimates with covariates). Moreover, in the appendices of article C, I show that results are relatively insensitive to varying the width of the estimation windows. Overall, this proves the robustness of my findings.

Core Effects of Forced Placement on Refugee Integration

Article C delves into the question: does placement matter for the integration of refugees? In particular, I exploit that the Integration Act changed the dispersion of refugees from voluntary to forced placement, where refugees who obtained residency after January 1 1999 were subject to economic sanctions if they did not stay in their assigned municipality for the first three years after residency. Theoretically, two opposing views structure the debates about forced placement and its impacts on integration. One view that builds on the hypothesis that ethnic concentration slows down the rate of host country-specific skill acquisition sees forced placement as a tool to promote refugee integration by securing a better geographical distribution of new refugees and forcibly immersing them into ethnically Danish local communities. I label this the *skill acquisition mechanism*. The opposing view builds on the idea that there are synergies between placement characteristics and characteristics of the individual. Consequently, forced placement is seen as an impediment to integration as it limits individuals' ability to select into places with characteristics that match their own characteristics. I label this the *synergy mechanism*.

To test these two contrasting theoretical predictions, I use citizenship acquisition as a key indicator for overall integration into the host society (for a detailed discussion of the citizenship indicator, I refer the reader to article C). Figure 11 illustrates the result graphically and reports the main finding of article C: the percentage of refugees who acquire citizenship dropped by about 13 percentage points at the forced placement cutoff (α = -12.79 points, $P <$ 0.033). Relative to the counterfactual mean, this is equivalent to a drop of about 26 percent in the naturalization rate. In stark contrast to the *skill acquisition hypothesis*, this finding suggests that forced placement has a substantial negative impact on refugee naturalization. The results are striking as I compare refugees who are identical in terms of background characteristics but differ by only a few days with regard to residency. Moreover, given that I compare forcibly placed refugees to voluntarily placed refugees, this estimated effect may be interpreted as a lower bound on the effect of dispersal relative to non-dispersal.

Figure 11. Effects of forced placement on citizenship acquisition



Note: The figure displays the result of applying the RD design graphically. Lines are average naturalization rates (with 95 % confidence intervals) on each side of the cutoff. Standard errors are clustered by first municipality. Bandwidth ±6 months. N = 1,650.

While this finding suggests that forced placement *does not* affect refugee naturalization through the skill acquisition mechanism, it remains an open question whether the negative effect estimates predominantly reflect synergies between individual characteristics and placement locations *or* some alternative mechanism. While forcibly placed refugees faced economic sanctions if they moved from their assigned municipality within the first three years of residency, voluntarily placed refugees could move freely. Against this backdrop, one implication of the *synergy mechanism* is that voluntarily placed refugees, in their first years of residency, can be expected to relocate across municipalities at higher rates than forcibly placed refugees do. Another implication is that the relocation gap should slowly close over time as forcibly placed refugees become able to relocate freely.

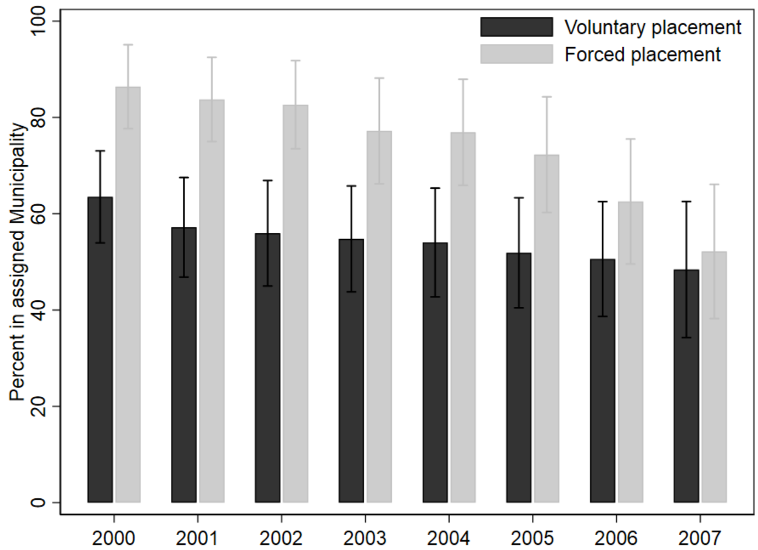
To test these propositions, Figure 12 plots the mean inter-municipality migration rates[18] of the forcibly and voluntarily placed groups, respectively, at the cutoff. In particular, this outcome tracks refugees' likelihood of staying in their assigned municipality (it takes the value 100 if a refugee stayed in her assigned municipality and 0 if she moved away). The figure shows a large initial difference between forcibly and voluntarily placed refugees' likelihood of remaining in their assigned municipality: after one year, about 37 percent of

[18] As I do not observe refugees' assigned municipality, I, instead, use the first observed municipality as a proxy for assignment municipality. For each year, refugees are assigned the value 100 if they stay in their first observed municipality and zero if they have moved away.

the voluntarily placed refugees have relocated compared to only 14 percent of the forcibly placed refugees. As expected, this difference in migration rates slowly begins to narrow after 2002, where the forced placement group no longer faces relocation sanctions. From 2006, there is no longer a statistically significant difference between the two groups' migration rates. Although the results indicate that there is a mismatch between the initial placement of refugees and their individual characteristics, it remains unclear whether this reflects a *general* location effect that some places are unconducive to refugee integration *or* the more refined *synergy* story that certain characteristics make a refugee a better match for a particular location. Separating these two mechanisms would have important consequences for the design of refugee allocation policies.

To separate the synergy mechanism from any general location effects, I first trim the sample to include only forcibly placed refugees who remained in their assigned municipality for the first three years. I match this group of refugees with voluntarily placed refugees who after three years live one of the same municipalities as the forcibly placed refugees. In this matched sample, I replicate the RD models with naturalization as the outcome. This means that I compare refugees who live in the same municipalities, but while the latter group lives there voluntarily, the first group is forced to. The logic of this comparison is that I net out any general location effects from the effect estimates. Any remaining difference between the groups should reflect the synergy mechanism, i.e., a mismatch between location and individual refugee characteristics.

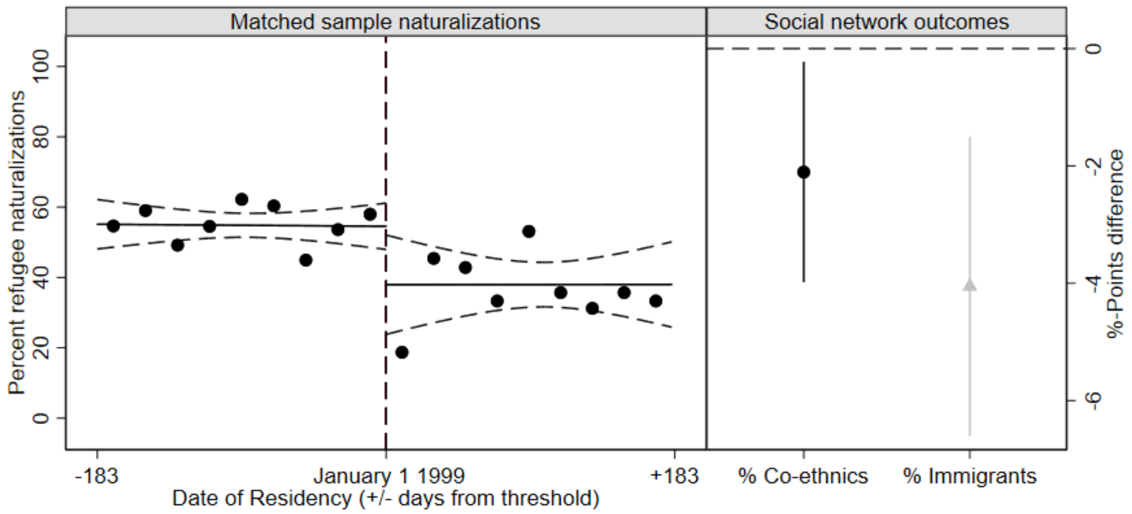
Figure 12. The synergy hypothesis: Effects of forced placement on inter-municipality migration



Note: Means at the cutoff with 95 % confidence intervals. Standard errors are clustered by first municipality. Bandwidth ±6 months. N = 1,650.

The left panel of Figure 13 gives the result of the estimated effect of forced placement on citizenship acquisition in the matched sample. The panel shows a clear drop in naturalization rates at the cutoff. In fact, the percentage of refugees who acquired citizenship dropped by about 16.5 percentage points (α = -16.61 points, $P < 0.034$), which is somewhat larger than the full sample estimate.[19] Following the logic from above that this test nets out any general location effects, the evidence suggests that the forced placement effect is driven predominantly by the synergy mechanism. This means that two refugees of similar backgrounds (i.e., similar ethnic background, of similar age, sex, family situation, and educational levels) follow very different integration paths when settled in different placement locations. While one thrives, the other struggles to integrate into the host society. Forced placement affects refugee integration negatively because it eliminates refugees' ability to select into places that match their characteristics. However, until now, I have been unconcerned about the factors that shape these synergies.

Figure 13. Synergy or general location effect?



Note: Left panel displays the result of applying the RD design graphically. Lines are average naturalization rates (with 95 % confidence intervals) on each side of the cutoff. N = 1,055. Right panel displays the marginal RD effect estimates (with 95 % confidence intervals) on social network outcomes. N = 1,650. Standard errors are clustered by first municipality. Bandwidth ±6 months.

As discussed in the theory section, one crucial aspect of the synergies may be social networks that potentially form a positive adaptive function of support.

[19] This increase reflects that for a small proportion of the forcibly placed refugees who move out of their assigned municipality—which would happen only if they found a job in another municipality—there is a positive, albeit insignificant, reform effect that factors into the overall effect estimate.

Social networks offer assistance in practical matters and may improve labor market integration by reducing search costs and problems of asymmetric information (Portes 1987; Laezar 1999; Munshi 2003; Bayer et al. 2008). They may positively influence integration by transferring knowledge about social norms (Coleman et al. 1966; Wilson 1987; Case and Katz 1991; Bertrand, Luttmer, and Mullainathan 2000). Finally, they may help develop confidence and self-esteem by offering emotional and financial support, which guards against feelings of isolation and depression (Bertrand, Luttmer, and Mullainathan 2000; Chiswick and Miller 2005; Spicer 2008; Boswell 2001). Research suggests that co-ethnics form these social networks (Habyarimana et al. 2007; Algan et al. 2016; Damm 2009a; Åslund 2005; Damm and Rosholm 2010).

To get at the proposition that deterioration of social networks is an important driver of the effect of forced placement, I follow previous research and measure neighborhoods' conduciveness to the formation of social networks using as indicators local concentrations of co-ethnics and immigrants. I replicate the RD models with initial local concentrations as outcomes. If social networks are an important factor in the synergies between place and individual, one should expect a negative forced placement effect on these alternative outcomes. The right panel of Figure 13 shows the results: forced placement markedly lowers the initial local concentrations by about ⅓ on each measure compared to the counterfactual means ($\alpha_{\text{co-ethnics}}$ = -2.10 percentage points, $P <$ 0.029; $\alpha_{\text{immigrants}}$ = -4.05 percentage points, $P <$ 0.003). Overall, this indicates that social networks are one important factor that shapes synergies between places and individuals. It also raises the important question of effect heterogeneity because one might expect that certain groups rely more on social networks than others do.

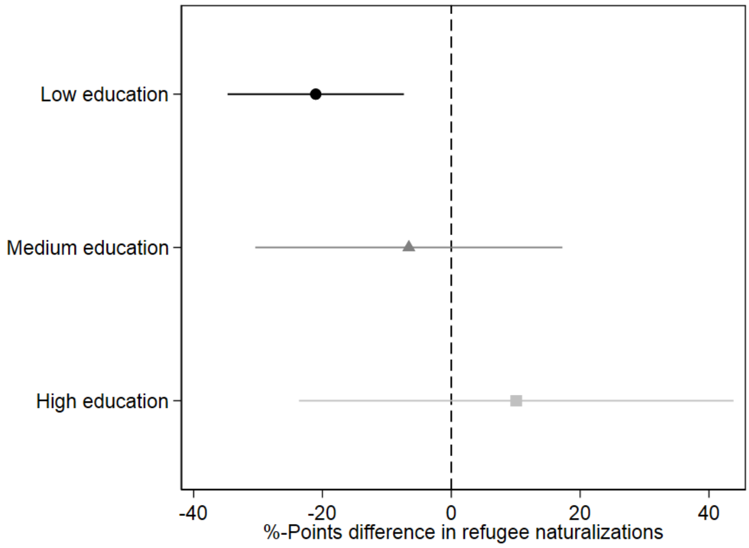
Effect Heterogeneity

As discussed in the theory section, the negative forced placement effect can be expected to be driven by the less educated, who face the largest resource constraints and can be expected to benefit most from living in enclaves with more advantaged members of the same ethnic group. I test these expectations by separately estimating the forced placement effect within the subgroups low, medium, and high education.[20] Figure 14 displays the results of these subgroup analyses and shows an empirical pattern that is consistent with the theoretical prediction. Thus, the negative effect is concentrated in the group with low education ($\alpha_{\text{low educated}}$ = -21.04, $P <$ 0.004), the estimate decreases and is

[20] I follow the same definition as above. Low education is less than or equal to 120 months; medium education is more than 120 months, but less than 180 months; and high education is more than 180 months.

insignificant in the medium group ($\alpha_{\text{medium educated}}$ = -6.59, P < 0.587), and turns positive in the group with high education, who experience an insignificant increase in their likelihood of naturalization ($\alpha_{\text{high educated}}$ = 10.08 percentage points, P < 0.555).

Figure 14. Are the forced placement effects heterogeneous?



Note: RD effect estimates with 95 % confidence intervals. Standard errors are clustered by municipality. Bandwidth ±6 months. N (low education) = 725; N (medium education) = 666; N (high education) = 259.

Taken together, these findings are crucial. First, they contribute to our theoretical understanding of the importance of social networks and ethnic clusters on refugee integration and have important implications for refugee allocation policies. The results do not square with the *skill acquisition hypothesis*, according to which forced placement has a positive effect on refugee integration because it dilutes ethnic concentrations that would otherwise slow down the rate of host country-specific skill acquisition. Instead, the results support the view that initial access to ethnic clusters can promote refugee integration. While forced placement achieves its immediate goal of placing new refugees in less ethnically concentrated areas it has a negative impact on refugee integration. Moreover, it is clear that this effect is driven predominantly by the less educated, who face the largest resource constraints and difficulties in integrating into the host society at the outset.

In terms of policy design, I believe that the results have clear implications for the way allocation policies—which have been adopted by several European governments to specifically disperse and balance the distribution of refugees geographically—should be designed. By adopting allocation policies with forced placement elements, governments make it difficult for new refugees to select into places that match their own characteristics and, in particular, tap into social networks of co-ethnics. This means that host countries are lowering refugees' chances of getting a good start on their new life and creating barriers for their long-term integration. The results suggest that governments should consider redesigning allocation policies to maximize synergies between location and refugee characteristics (Bansak et al. 2018) including a focus on existing immigrant networks (Martén et al. 2019). This could benefit individual refugees as well as local communities.

Perceptual Biases and Preferences Regarding Policy

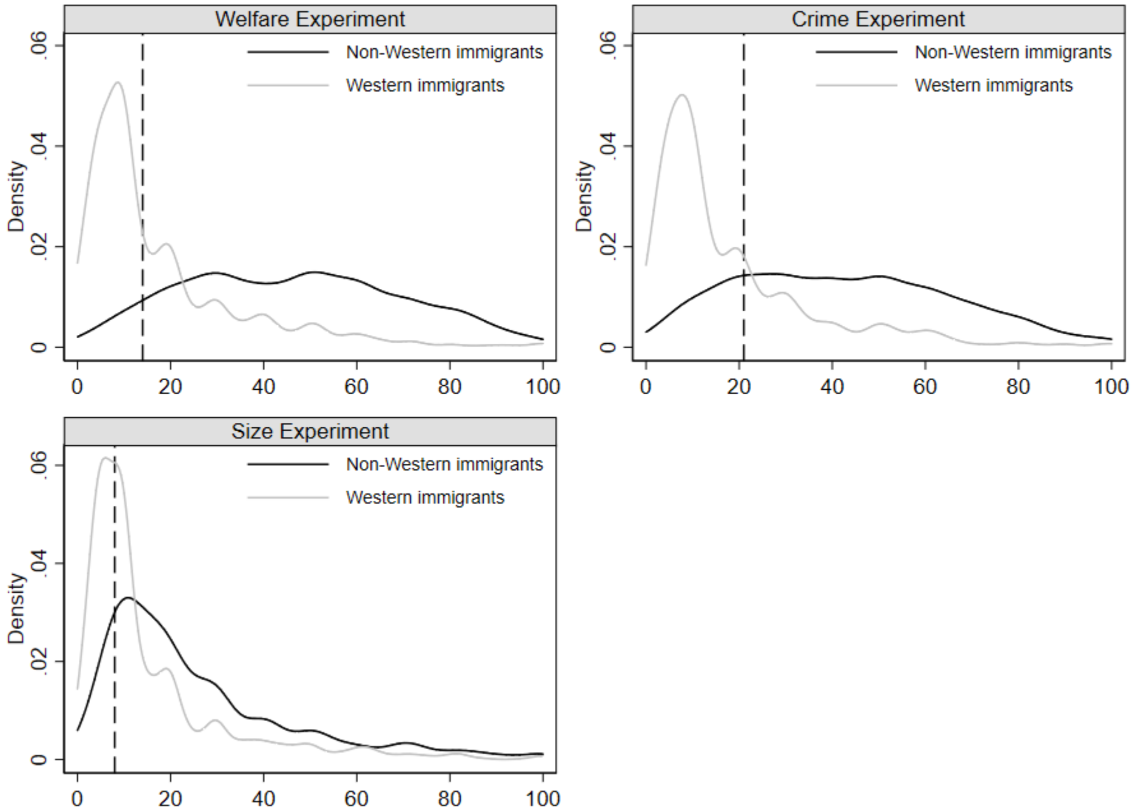
The analyses above demonstrate that existing policies are too restrictive if their aim is to maximize refugee integration. This raises the crucial question: why do policy makers adopt these types of policies? As discussed in the theory section, one reason might be that citizens often exaggerate the prevalence of immigrants in their surroundings just as they tend to hold inaccurate beliefs about the broader social and economic impacts of immigration (Wong et al. 2012). Moreover, people who overestimate problems with immigration are more likely to support anti-immigration policies (Sides and Citrin 2007; Nadeau et al. 1993). Figure 15 plots participants' prior beliefs about non-Western and Western immigrants, respectively. The solid black lines represent beliefs about non-Western immigrants, the solid grey lines beliefs about Western immigrant. The dashed vertical lines show the respective true non-Western quantities. The figure demonstrates that citizens indeed are very skeptical about non-Western immigrants and markedly exaggerate problems related to non-Western immigration: prior beliefs about non-Western immigrants are skewed widely to the right of the true quantities.[21] Moreover, the correlations between priors and support for anti-immigration policies are quite strong (across the different priors, correlations vary between 0.39 and 0.48) such that people who exaggerate problems of non-Western immigration are more likely to support restrictive policies.[22] Given the premise that public opinion have some impact on public policy, this offers an explanation as to why we see these restrictive integration policies despite their seemingly negative impacts

[21] In comparison, participants are much more positive about Western immigration. This shows that respondents were numerate and able to distinguish between immigrant groups.

[22] These correlations reflect the association between priors and preferences regarding policy in the control group, who was not exposed to corrective information that can be expected to distort this relationship.

on refugee integration. It is a disheartening conclusion, and it raises the question whether citizens might hold more favorable immigration policy opinions if they were better informed. Article D examines this important research question.

Figure 15. Prior beliefs by immigrant group



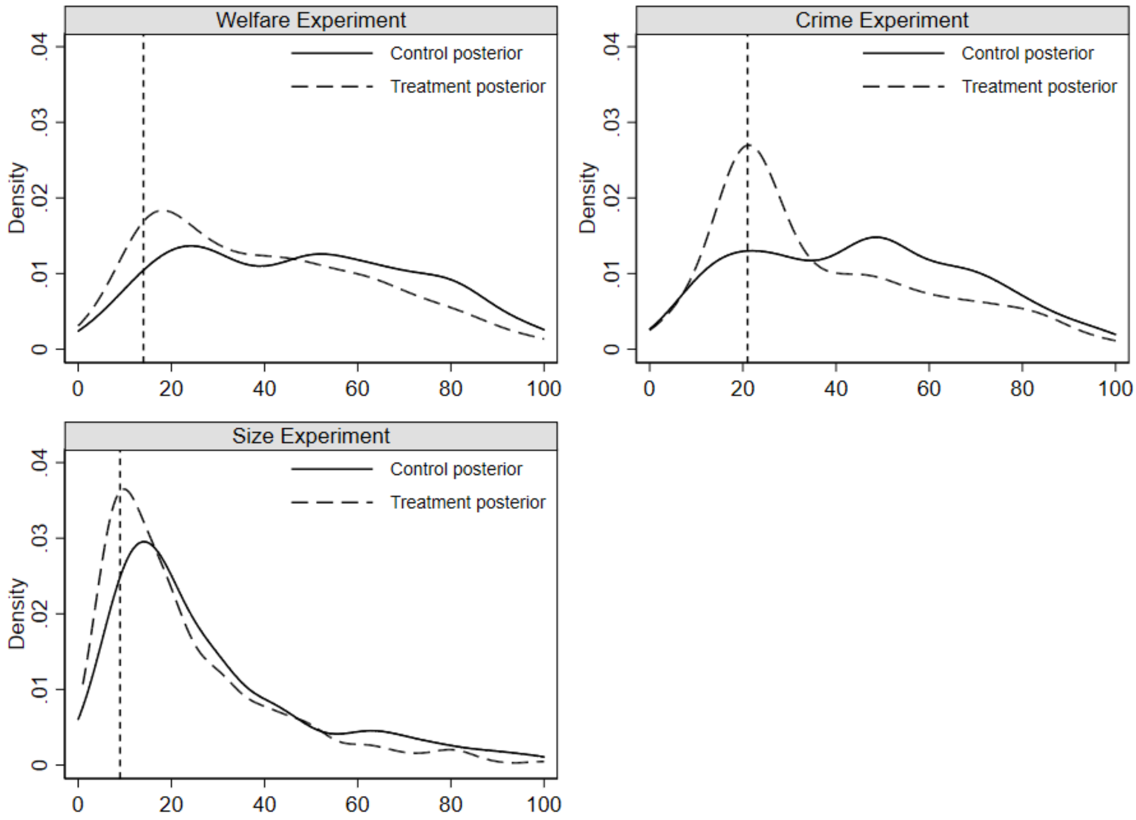
Note: Each panel displays the distribution of the respective prior beliefs about non-Western (solid black line) and Western (solid grey line) immigrants. The vertical dashed lines show the respective true quantities of non-Western immigrants.

The design of article D followed a three-step sequence where participants first reported their "best estimates" (*prior beliefs*) of the three types of immigrant facts. These facts are described in the research design section and their distributions are plotted in Figure 15. Second, we randomly assigned participants to receive or not receive correct information about the same facts. Third, participants reported again their best estimates of the immigration facts (*posterior beliefs*), their immigration *policy opinions* as well as how they *interpreted* the immigration facts. One expectation that draws on Bayesian learning models stipulates that citizens use new information to update their posterior evaluations of immigrants' integration performance into the host society and, correspondingly, adjust the preferences regarding policy. Another expectation holds that while people might acknowledge correct information and update their posterior beliefs, they reinterpret the information in a selective fashion that justifies their existing opinions and keep their policy preferences unchanged (Gaines et al. 2007).

To get at these contrasting expectations, I first test whether citizens update their factual beliefs about non-Western immigrants in light of corrective information. Figure 16 gives the results. The figure compares the *posterior beliefs* of participants in the treatment groups and participants in the no-information control group. The three panels show that the treatments moved posterior beliefs downward toward the true proportions (i.e., the vertical dashed lines) relative to participants in the control group. In particular, the crime and welfare treatments both had an average effect of about 8 percentage points ($P < 0.0001$) while the size treatment had an effect of about 5.5 percentage points ($P < 0.0001$). This shows that that people do not simply reject disconfirming information. This claim is reinforced by the fact that the treatment effects are largest among participants who initially overestimated problems with non-Western immigrants the most. Thus, in all three experiments, moderate or large overestimators[23] updated their posterior beliefs the most (about 8-15 percentage points each) (for details on the latter set of results, please consult Figure 2 of article D).

[23] We define four categories of prior biases based on participants' prior beliefs. Underestimators are participants who hold prior beliefs below the true value. Small overestimators are participants with prior beliefs above or equal to the true value, but within the 33.33 percentile of the distribution. Moderate overestimators are participants with prior beliefs within the 33.33-66.67 percentiles. Finally, large overestimators are participants above the 66.67 percentile.

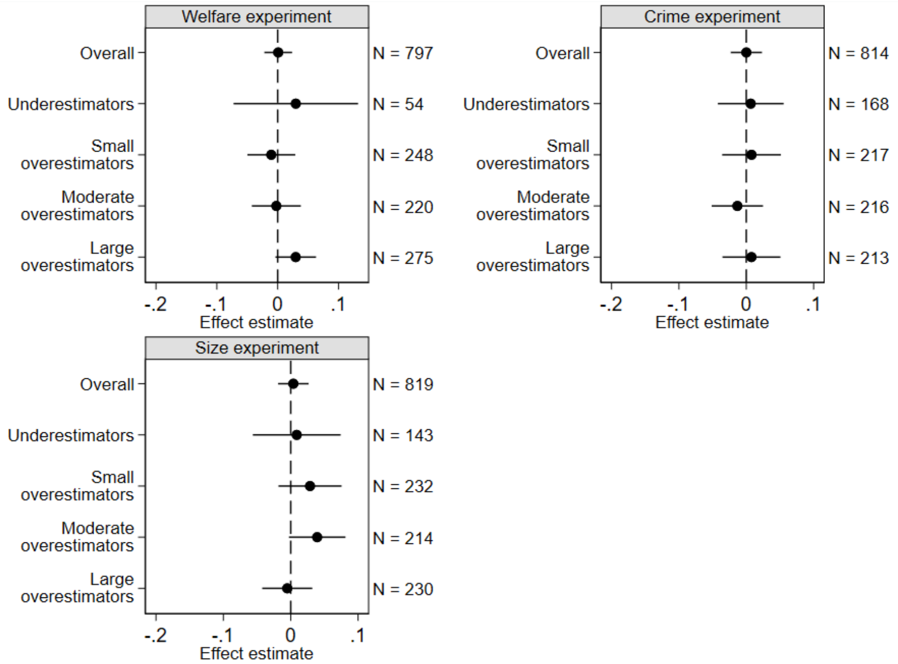
Figure 16. Posterior Beliefs by Treatment Status



Note*:* Differences in posterior beliefs between control group and treatment groups in the three experiments. Vertical dotted lines give the true proportions of non-Western immigrants.

While the corrective information made participants update their factual beliefs, the next question is whether it also caused them to change their policy opinions. To examine this, Figure 17 presents estimated coefficients from models where we regress immigration-related policy opinions on our treatment conditions. In stark contrast to the previous findings, Figure 17 shows that none of the information treatments affected participants' immigration policy opinions. In all three panels, the coefficient for the average treatment effect (labeled "Overall") is statistically insignificant and close to zero. The same results emerge when we subset the analysis based on participants' prior belief: the effect estimates are insignificant, irrespective of participants' initial beliefs about the problems associated with immigration. Taken together, these results replicate earlier studies (e.g., Alesina et al. 2018; Hopkins et al. 2019; Lawrence and Sides 2014): correct information about non-Western immigrants causes people to hold more accurate factual beliefs but fails to affect immigration policy opinions.

Figure 17. Effects of corrective information on policy preferences

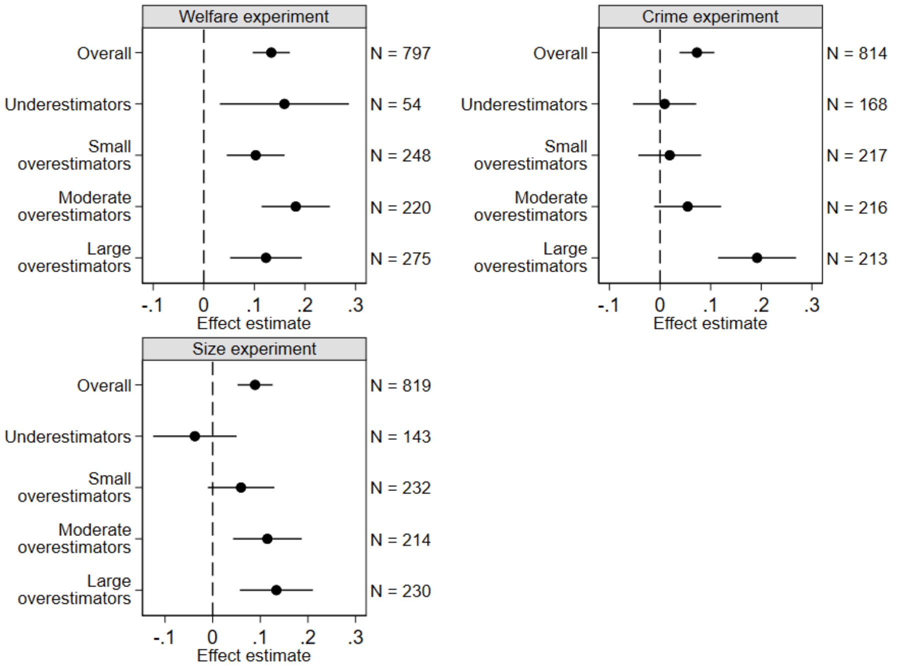


Note: Effects of information treatments on immigration policy preferences for the three experiments (95 % confidence intervals). "Overall" gives the average treatment effects while the other coefficients give effects broken down by participants' prior beliefs. Outcomes are scaled from 0-1 where higher values indicate anti-immigration preferences.

Why did our information treatments fail to change policy opinions? One possibility is that people acknowledge new facts but choose to interpret them in a highly selective fashion. For example, consider a "treated" participant who at the outset held unfavorable views on immigration. She may feel forced to accept the corrective information, but may nevertheless justify her negative opinions by interpreting the *new* number as still being too high (i.e., "the number is smaller than I thought, but still much too high"). Consider a comparable participant in the control condition. She would have no need to adjust her factual beliefs and thus no reason to reinterpret the numbers. This leads to the observable implication that participants who initially overestimated the immigration-related problems *and* received correct information, interpret the treatment information as more worrisome compared to control participants who did not receive corrective information.

Figure 18 tests this proposition by regressing the interpretation outcome on the treatment conditions. The "overall" estimates show that across all conditions, participants who received correct information interpreted the numbers as higher and more problematic than comparable "control" participants who had no reason to adjust their interpretations ($\alpha_{welfare} = 0.14$, $P < 0.0001$; $\alpha_{crime} = 0.07$, $P < 0.0001$; $\alpha_{size} = 0.08$, $P < 0.0001$). While the effects are similar across subgroups in the welfare experiment, we see that the effects increase as prior biases grow larger in both the crime and size experiments. Overall, the results suggest that people use interpretations to justify their existing attitudes. This helps explain why getting the facts right in itself may be insufficient to change people's policy views.

Figure 18. Effects on interpretations



Note: Effects of information treatments on interpretations for the three experiments (95 % confidence intervals). "Overall" gives the average treatments effects while the other coefficients give effects broken down by participants' prior beliefs. Outcomes are scaled from 0-1, where higher values indicate that participants interpret the number as high.

The findings support the conclusions from earlier work that people update their factual beliefs in light of correct information but fail to change their policy views. As a novel finding, we demonstrate that the link between facts and policy beliefs breaks down because people interpret information in a belief-consistent manner. In this way, the results contribute to our understanding of how people evaluate information with political consequences and how they use (or *avoid* using) information to guide their policy preferences. Overall, this provides the following answer to the question raised in article D: it does not seem that provision of correct information matters for citizens' policy preferences. This is disheartening because it means that policy-makers cannot rely on "explaining the facts" as a tool to promote more favorable integration policy views and thereby soften the electoral constraints they face when trying to design policies that foster integration, while simultaneously being supported by domestic voters.

Of course, the study has its limitations. First, exposure to information over an extended period might yield larger effects. Second, other information types that speak directly to natives' cultural concerns about immigrants (e.g., their willingness to learn the Danish language) may matter more. Third, had we instead presented the information differently (e.g., relative comparisons of non-Western immigrants versus ethnic Danes) *or* linked the corrections to other types of information like party cues (e.g. Barrero Rodriguez et al. 2019), participants might have viewed non-Western immigrants more favorably.

Chapter 5. Conclusion

Integration of refugees is a fundamental challenge that all refugee-receiving countries face. Many refugees arrive in their host countries very vulnerable, with few resources and face many challenges integrating into the host societies. Moreover, many refugee-receiving countries in Europe and the Americas have experienced marked increases in the size and diversity of their refugee populations. At the same time, policy makers are constrained by public demands for policies that limit the inflow of new refugees and restrict existing refugees' access to the same rights that apply to natives. In this tension between developing policies that both foster integration and are supported by voters, governments have been prompted to reassess and, most commonly, tighten their integration policies. Much is at stake in this process. *First*, however, we know relatively little about the possible impacts of stricter integration policies on refugee integration. *Second*, we know even less about how policy makers can remove the electoral constraints they face when developing integration policies.

This dissertation contributes to our understanding of the research problem that follows from the tension between policy goals and electoral constraints: *how to develop policies that foster integration and are supported by voters*. As discussed in the introduction, I split this problem into two *research questions* and for each question separate sets of *researchable questions*. The study of research question 1 provides an answer to the first part of the research problem: how to develop policies that foster integration. The examination of research question 2 provides an answer to the second part of the research problem: how to develop policies that are supported by voters.

Research Question 1

I examined research question 1—how integration policies influence refugee integration—in three individual research articles (articles A-C), where I studied the impacts of two policies that have formed the backbone of Danish integration policy for the past two decades: the start help policy that significantly lowered refugees' benefits and the dispersal reform that changed refugees' initial place assignment from voluntary to forced placement.

The results on the start help policy show that lower benefits create barriers rather than incentives for integration of refugees in the long term. While the reductions seem to raise refugees' incentive to get off welfare and find employment in the short term, this effect dissipates over time as refugees entitled to regular benefits slowly close the employment gap. Instead, it is clear that benefit reductions have persistent and long-term negative impacts on residential integration. This suggests that the deprivation effect, on average, dominates the incentives effect over time.

Moreover, the analyses show that the short-term positive incentive effects are driven by the better educated, whereas the positive effects remain small and insignificant among the low educated. This means that what the benefit reductions in fact do is to push the better educated off welfare and into employment more quickly than if they had been eligible for regular assistance. From one viewpoint, one might argue that the reductions work as intended as they give the better educated that "kick in the ass" they need to get off welfare. From another viewpoint, one could argue that the reform did not have the desired effects as the positive incentive effects disappear over time. In addition, as the positive effects are concentrated among the better educated, the low educated are left behind facing larger integration barriers that follow from economic deprivation.

This shows up in the estimates on the deprivation outcomes that demonstrate that the negative deprivation effect comes to dominate over time as the incentives effect fades. Moreover, these negative effects are driven by the low educated. Overall, this means that lower benefits create barriers rather than incentives for integration of refugees in the long term and suggests that these barriers materialize among the low educated, whereas the better educated remain seemingly unaffected. These findings have clear implications for policy design: if the aim is to promote integration, governments should not limit refugees' access to the host country's benefit system.

The results on the forced placement policy do not square with the *skill acquisition hypothesis*, which contends that forced placement has a positive effect on refugee integration because it dilutes ethnic concentrations that would slow down the rate of host country specific skill acquisition. Instead, the results support the view that initial access to ethnic clusters can promote refugee integration. The forced placement achieves its immediate goal of placing new refugees in less ethnically concentrated areas, but this actually has a negative impact on refugee integration. Moreover, it is clear that this effect is driven predominantly by the less educated, who, from the outset, face the largest resource constraints and difficulties integrating into the host society.

These findings have clear implications for refugee allocation policies. With these policies, governments make it difficult for new refugees to select into places that match their own characteristics and, in particular, tap into social networks of co-ethnics. This means that host countries are lowering refugees' chances of getting a good start on their new life and creating barriers for their long-term integration. The results suggest that governments should consider redesigning their allocation policies to maximize synergies between location and refugee characteristics (Bansak et al. 2018) including a focus on existing immigrant networks (Martén et al. 2019). This could benefit individual refugees as well as local communities.

Returning to research question 1—how integration policies influence refugee integration—the findings show that existing policies, i.e. low benefits and forced placement, which have formed the backbone of Danish integration policies for the past two decades, have detrimental impacts on refugee integration. It is especially disheartening that these effects are predominantly driven by the least educated. From a policy standpoint, these findings are concerning because they show that strict policies marginalize those who have few chances of integrating into the host society further and push them to the margins of society. Theoretically, the findings align with recent work (e.g. Marbach et al. 2018, Hainmueller et al. 2016) that points to the existence of an influential early integration window that affect refugees' subsequent integration trajectory disproportionately.

Although the evidence does not provide a guide for the optimal level of policy strictness, it demonstrates that the current Danish policies are too strict if the aim is to maximize integration. This provides policy makers with a direction in the process of developing policies that promote integration.

Research Question 2

I examine research question 2—does refugees' integration success or failure affect public support for policy—in research article C, where I study whether citizens would hold more favorable preferences regarding integration and immigration policy had they been better informed about refugees' actual integration success or failure. Given that the findings above indicate that future policies should be less restrictive, the answer to this question is important because it potentially provides policy makers with tools to remove or soften the electoral constraints they face when designing integration policies and thereby open up space to develop less strict policies.

Specifically, we conduct a large-scale survey experiment that isolates the effects of correct information—about the integration success/failure of the non-Western immigrant population in Denmark—on native-born Danes' preferences regarding integration policy. The findings demonstrate that while participants update their factual beliefs in light of correct information, they remain unwilling to change their policy preferences. These findings support conclusions from earlier work (Lawrence and Sides 2014; Hopkins et al. 2019). As a novel finding, we show that the link between facts and policy beliefs breaks down because people interpret the correct information in a belief-consistent manner that allows them to avoid using the new information to guide their policy preferences.

Overall, this gives the following answer to the question raised in article D: it does not seem that the provision of correct information affects citizens' policy preferences. Returning to research question 2, this is disheartening because it means that policy makers seemingly cannot rely on "explaining the facts" as a tool to promote more favorable integration policy views and thereby remove the electoral constraints they face when trying to design policies that foster integration. However, these findings have limitations that might give policy makers leeway to design less strict policies that foster integration. First, exposure to information over an extended period might yield larger effects. Second, other information types that speak directly to natives' cultural concerns about immigrants (e.g., their willingness to learn the Danish language) may matter more. Third, had we presented the information differently (e.g., relative comparisons of non-Western immigrants versus ethnic Danes) *or* linked the corrections to other types of information like party cues (e.g. Barrero Rodriguez et al. 2019), participants might have viewed non-Western immigrants more favorably.

Bringing It Together: The Research Problem

The findings of this dissertation offer empirical answers to its research problem: how to develop policies that foster integration while at the same time being supported by voters. On the one hand, the findings clearly demonstrate that the current Danish policies are too strict if the aim is to maximize integration. This contributes to the ongoing debates about integration policy. For policy design, this means that policy makers should reassess current policies: they should provide refugees with equal benefits to prevent negative effects from economic deprivation and remove restrictions on relocation to leverage synergy effects between individual characteristics and place characteristics. Theoretically, the debates about integration policy are structured by two contrasting paradigms. One paradigm, often supported by parties on the right, argues that strict policies promote integration. The contrasting paradigm, often supported by parties on the left, holds that more lenient policies catalyze social mobility and integration. The dissertation's findings clearly support this latter paradigm.

The findings align with recent work that shows that less restrictive policies—e.g., fewer restrictions on citizenship acquisition (Hainmueller et al.

2015; 2017a; 2019), faster processing of asylum applications (Hainmueller et al. 2016; Hvidtfeldt et al. 2018), protection of unauthorized immigrants (Orrenius and Zavodny 2012; Hainmueller et al. 2017b), and fewer restrictions on asylum seekers' possibility of employment (Marbach et al. 2018)—act as catalysts of integration. In spite of this evidence, we continuously experience that policy makers tighten integration policies and thereby decrease refugees' chances of successful integration. One plausible reason for the mismatch between the supply of policies and the aim of maximizing integration might be that domestic voters demand strict policies. This constrains policy makers' ability to deliver policies that achieve the goal of promoting integration. In line with previous work, the findings in paper D show that this is the case: citizens' are very skeptical of immigrants and markedly exaggerate problems related to immigration. In addition, there is a strong correlation between skepticism and support for anti-immigration policies.

One way of escaping the electoral constraints would be if it were possible to promote citizens' preferences regarding policy by providing information about refugees' actual integration into the host society. However, as shown above, this does not seem to be a straightforward endeavor as citizens seem to distort the relationship between corrective information and their policy views by interpreting the correct information in a belief-consistent manner that makes it possible to rationalize away the new information as a guide to their policy preferences. Consequently, it seems that "explaining the facts" cannot be used as a straightforward tool for policy makers to create leeway to develop less strict policies that would promote integration.

Although the findings in paper D have their limitations (cf. above) that might give policy makers leeway to design less strict policies by using more focused information, alternative routes for developing policies that foster integration might be more viable solutions in terms of gaining support by the public. For example, policy makers might camouflage policies that predominantly affect immigrants and refugees as social policies rather than integration policies. One example of this framing strategy is the recent Danish election, where the Social democrats promised that the strict immigration and integration policies would remain unchanged. While the new social democratic government has not made any major revisions of the integration policy regime, they established a new "benefit commission" whose job is to give recommendations about how to alleviate child poverty. Moreover, until the commission gives its recommendations, the government has introduced a monthly allowance of DKK 1700 (~ USD 250) for especially vulnerable families. Even though these changes mainly affect refugee families, the adjustments have not received major media attention or public opposition. While this speaks for the usefulness of utilizing a camouflaging strategy, this strategy obviously has its limitations when it comes to making fundamental revisions to the overall integration policy regime.

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English Summary

This dissertation contributes to our understanding of a fundamental policy challenge that refugee-receiving countries face: how to develop policies that foster integration and are supported by voters. It splits this challenge into two. On the one hand, there is the policy goal of promoting integration. This leads to research question 1: how does integration policies affect refugee integration. On the other hand, policy makers face the electoral constraint that policies need to be supported by voters. This leads to research question 2: does refugees' integration success or failure affect public support for policy.

The dissertation takes its theoretical point of departure in two contrasting theoretical paradigms that structure the debates about integration policy. One paradigm, argues that strict policies—such as limited benefits or forced placement—promote integration. The contrasting paradigm, holds that lenient policies—like equal benefits or voluntary placement—catalyze social mobility and integration.

I study these contrasting expectations in the context of two Danish policy reforms: the start help policy and the forced placement policy. Combined, these policies have formed the backbone of Danish integration policy for the past two decades. The start help policy lowered refugees' social assistance benefits by up to 50 percent for new refugees who obtained residency after July 1 2002. The forced placement policy fundamentally changed the Danish dispersal system as of January 1 1999: new refugees who obtained residency after this date were subject to forced placement, whereas refugees who arrived earlier were placed on a voluntary basis. I exploit these cutoffs in regression discontinuity designs that just like controlled randomized experiments control for *all* confounding factors *by design*. The reforms provide rigorous research designs (i.e., natural experiments) for causal identification. My data are based on the Danish national registers and combine information about the *treatments* (i.e., the cutoffs) with information on relevant integration outcomes.

Overall, the findings show that the start help and forced placement policy are too strict if the aim is to maximize integration. For policy design, this means that policy makers should reassess current policies: they should provide refugees with equal benefits to prevent negative effects from economic deprivation and remove restrictions on relocation to leverage synergy effects between individual characteristics and place characteristics. Theoretically, the findings support the paradigm, which argues that equal benefits and voluntary placement catalyze social mobility and integration. These results align with recent studies, which show that less restrictive policies—i.e., fewer restrictions

on citizenship acquisition (Hainmueller et al. 2015; 2017a; 2019), faster processing of asylum applications (Hainmueller et al. 2016; Hvidtfeldt et al. 2018), protection of unauthorized immigrants (Orrenius and Zavodny 2012; Hainmueller et al. 2017b), and fewer restrictions on asylum seekers' possibility of employment (Marbach et al. 2018)—are catalysts of integration.

In spite of this evidence, we continuously experience that policy makers tighten integration policies and thereby decrease refugees' chances of successful integration. One plausible reason for the mismatch between the supply of policies and the aim of maximizing integration is that domestic voters demand strict policies (Lawrence and Sides 2014; Hopkins et al. 2019). This constrains policy makers' ability to deliver policies that achieve the goal of promoting integration.

The last part of the dissertation moves on to study this policy constraint and explores strategies that can potentially create leeway to develop less strict policies that would promote integration. This part of the dissertation examines whether it is possible to promote citizens preferences regarding integration policy by providing them with information about refugees' actual integration success or failure. In particular, we conduct a large-scale survey experiment that isolates the effects of correct information about non-Western immigrants' welfare dependency rates, their crime rates, and their overall size in relation to the total population.

Two opposing views structure the theoretical expectations to the impacts of this type of information. One view that draws on Bayesian learning models argues that citizens use information to update their evaluations of immigrants' integration performance into the host society. In this logic, the provision of information may be expected to promote more positive preferences regarding policy (Sides and Citrin 2007; Nadeau et al. 1993). Another view holds that people acknowledge correct information and update their factual beliefs, but reinterpret the information in a selective fashion that justifies their existing opinions (Gaines et al. 2007). In this logic, the provision of information has little, if any, influence on citizens' policy preferences.

In line with previous work, the findings *first* show that citizens' are very skeptical of non-Western immigrants and markedly exaggerate problems related to immigration. In addition, there is a strong correlation between skepticism and support for anti-immigration policies. This demonstrates that policy makers indeed face pronounced electoral constraints when designing integration policy. *Second*, the results demonstrate that while participants update their factual beliefs in light of correct information, they remain unwilling to change their policy preferences. These findings support conclusions from earlier work (Lawrence and Sides 2014; Hopkins et al. 2019). As a novel finding, we show that the link between facts and policy beliefs breaks down because people interpret the correct information in a belief-consistent manner that allows them to avoid using the new information to guide their policy preferences. Overall, this means policy makers seemingly cannot rely on “explaining the facts” as a strategy to promote more favorable integration policy views and thereby create leeway to develop less strict policies that would foster integration.

Dansk resumé

Lande der modtager flygtninge står over for den fundamentale politiske udfordring: hvordan udvikles politikker der fremmer integrationen og som samtidig bakkes op af vælgerne. Afhandlingen bidrager til forståelsen af problemstilling, og inddeler udfordringen i to forskningsspørgsmål. På den ene side er der målet om at udvikle politikker, der fremmer integrationen, hvilket fører til forskningsspørgsmål 1: hvordan påvirker integrationspolitikker flygtninges integration. På den anden side begrænses politiske beslutningstagere af, at det er nødvendigt at politikkerne møder opbakning i befolkningen. Dette fører til forskningsspørgsmål 2: påvirker flygtninges integrationssucces eller -fiasko befolkningens opbakningen til policy.

Afhandlingen tager sit teoretiske afsæt i to modsatrettede teoretiske paradigmer, som ofte strukturerer debatten omhandlende integrationspolitik. Det første paradigme argumenterer for, at strengere politikker, såsom begrænset adgang til overførselsindkomster eller tvungen placering, fremmer integrationen. Det andet paradigme argumenterer modsat for, at mindre strenge politikker, såsom lige adgang til overførselsindkomster eller frivillig placering, fremskynder social mobilitet og integration.

Til at studere disse modsatrettede forventninger anvender jeg henholdsvis den danske starthjælpsreform og reformen af den danske placeringspolitik, der tilsammen har udgjort rygraden af dansk integrationspolitik de seneste to årtier. Starthjælpspolitikken nedsatte flygtninges overførselsindkomst med op til 50 procent for nye flygtninge, der opnåede opholdstilladelse efter 1. juli 2002. Den danske placeringspolitik blev fundamentalt ændret fra 1. januar 1999, hvor spredningen af flygtninge overgik fra et frivilligt til tvunget regime. Jeg udnytter disse tærskler i regressionsdiskontinuitetsdesigns, der ligesom randomiserede eksperimenter *per konstruktion* kontrollerer for alternative forklaringer. Reformerne udgør dermed naturlige eksperimenter og stringente forskningsdesigns for kausal inferens. Mit data er baseret på de national danske registre og kombinerer information omkring reformernes tærskelværdier med information omkring relevante integrationsvariable.

Overordnet viser resultaterne, at starthjælpen og tvungen placering er for stramme, såfremt målet er, at fremme integrationen. For policy betyder det, at de politiske beslutningstagere bør genoverveje disse politikker. Konkret bør de give flygtninge ret til regulære overførselsindkomster for at forhindre de negative konsekvenser der følger af økonomiske afsavn. Endvidere bør de fjerne den tvungne placering, der forhindrer udnyttelsen af potentielle positive synergieffekter, der måtte være mellem flygtninges og deres placerings

karakteristika. Teoretisk støtter resultaterne det andet paradigme, der argumenterer for, at lige overførselsindkomster og frivillig placering fremskynder social mobilitet og integration. Resultaterne flugter ned den seneste forskning, der viser, at færre begrænsninger på erhvervelsen af statsborgerskab (Hainmueller et al. 2015; 2017a; 2019), hurtigere behandling af asylansøgninger (Hainmueller et al. 2016; Hvidtfeldt et al. 2018), beskyttelse af illegale indvandrer (Orrenius and Zavodny 2012; Hainmueller et al. 2017b), samt færre begrænsninger af asylansøgeres muligheder for at arbejde (Marbach et al. 2018) fremmer integrationen.

På trods af disse resultater oplever vi en stadig stigende tendens til, at de politiske beslutningstagere strammer forskellige integrationspolitikker. Dermed besværliggør de faktisk flygtninges integration fremfor at hjælpe den på vej. En potentiel årsag til dette misforhold mellem målet om at fremme integrationen og politikudbuddet er, at vælgerne rent faktisk efterspørger disse politikker (Lawrence and Sides 2014; Hopkins et al. 2019), og dermed begrænser beslutningstagernes muligheder for at udvikle alternativer, der kan levere på målet om at fremskynde integrationen.

Den sidste del af afhandlingen beskæftiger sig med disse vælgermæssige begrænsninger, og udforsker strategier, beslutningstagerne potentielt kan anvende til at skabe sig selv spillerum til at udvikle politikker, der fremmer integrationen. Denne del af afhandlingen undersøger, om det er muligt at fremme vælgernes præferencer for integrationspolitik ved at præsentere dem for information om flygtninges faktiske integration. Konkret anvender vi et surveyeksperiment til at isolere effekterne af at give vores respondenter korrekt information om ikke-vestlige indvandreres afhængighed af overførselsindkomster, deres kriminalitetsrater samt størrelsen af den ikke-vestlige indvandrerbefolkning relativt til den samlede befolkning.

To modsatrette perspektiver strukturerer hvordan denne type information kan forventes at påvirke respondenterne. Det første perspektiv, der baserer sig på bayesianske læringsmodeller, argumenterer for, at vælgere anvender information til at opdatere deres evalueringer af immigranternes faktiske integration. Følgeligt forventes det, at de justerer deres policy præferencer i en mere positiv retning (Sides and Citrin 2007; Nadeau et al. 1993). Det andet perspektiv anerkender at vælgerne måske anvender information til at opdatere deres faktiske overbevisninger, men argumenterer modsat for, at de fortolker information på selektiv vis således de er i stand til at retfærdiggøre deres eksisterende meninger (Gaines et al. 2007). Dermed kan det ikke forventes, at information har nogen effekt på deres policy præferencer.

På linje med den eksisterende litteratur viser mine resultater for det *første*, at vælgerne er meget skeptiske overfor ikke-vestlige indvandrere, og markant overestimerer problemer relaterer til denne immigration. Derudover viser resultaterne, at der er en stærk korrelation mellem at være skeptisk og foretrække stramme politikker. Samlet viser det, at beslutningstager står overfor markante elektorale begrænsninger i deres overvejelser om udformningen af integrationspolitikker. For det *andet* viser resultaterne, at vores respondenter er parate til at opdatere deres faktiske overbevisninger i lyset af ny information, men de forbliver modvillige i forhold til at justere deres policy præferencer. Dette underbygger konklusionerne fra tidligere studier i andre kontekster (Lawrence and Sides 2014; Hopkins et al. 2019). Vi viser samtidig, at linket mellem fakta og policy bryder sammen, fordi vores respondenter fortolker den nye information på en måde, der er i overensstemmelse med deres eksisterende meninger, hvilket retfærdiggør at de undgår at anvende den nye information til at guide deres policy præference. Samlet set betyder det, at beslutningstagerne tilsyneladende ikke kan regne med, at det er tilstrækkeligt at forklare de faktiske forhold, som en strategi til at fremme mere favorable integrationspolitiske holdninger og dermed skabe sig selv et spillerum til at udvikle politikker, der fremskynder integrationen.