

International labor mobility

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**International Labor Mobility:
Roots, Returns and
Consequences**

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International Labor Mobility: Roots, Returns and Consequences

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To obtain the degree of Doctor at Maastricht University,
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in accordance with the decision of the Board of Deans,
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by

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1

Introduction

Cross-border labor mobility is a politically sensitive issue (Dustmann & Preston, 2019) and is central to public debate in many countries. It is associated with concerns about job displacement, pressure on public services, and fears over national identity and social integration (Alesina & Tabellini, 2024). At the same time, labor mobility reflects deeper inequalities between and within countries (Dustmann & Preston, 2019). In this context, governments must balance the economic benefits of migration — such as alleviating labor shortages and mitigating the effects of population aging, including shrinking workforces and rising pressure on welfare systems — against growing public concerns about labor market competition, housing affordability, integration, and cultural identity. Against this backdrop, this dissertation contributes to our understanding of the drivers of cross-border labor mobility and its impacts on host societies.

Rawls (1971) proposes a thought experiment in which we design society's rules from behind a "*veil of ignorance*" unaware of our future

status, talents, or circumstances of birth. The goal is to identify fair and impartial principles of justice—rules that anyone would accept, regardless of their position in society. While Rawls' framework lies beyond the scope of this dissertation, it invites us to reflect on one fundamental reality: the place where an individual is born determines their future opportunities and wealth they can accumulate through their life cycle (Dustmann & Preston, 2019). In this context, geographic mobility emerges as a key mechanism for individuals to improve their prospects when faced with limited opportunities relative to potential alternatives.

Individuals choose to migrate when the expected benefits exceed the costs. Economists have traditionally framed these benefits and costs in monetary terms. For instance, Hicks, (1932) observed that “*differences in net economic advantages, chiefly differences in wages, are the main causes of migration*”. In other words, people tend to move from low-wage or high-unemployment areas to places with better job prospects, higher pay, and overall improved economic conditions. Building on these ideas, standard migration models developed by Sjaastad, (1962) and Mincer, (1978) formalized this cost–benefit approach, and provided a clear economic explanation for the reason why people move, which is to improve their economic well-being.¹ More recently, however, the economic literature has expanded this notion to also include non-economic factors, including family and social ties (Huttunen, Møen, & Salvanes, 2018), cultural proximity (Belot & Ederveen, 2012; Falck, Heblich, Lameli, & Südekum, 2012), shared language (Adsera & Pytlikova, 2015), and overall quality of life considerations (Piyapromdee, 2021) as drivers of mobility.

Beyond individual-level implications, cross-border labor mobility also affects a wide range of economic, political, and social outcomes in host countries (Alesina & Tabellini, 2024). Economically, the literature has documented that immigration has a null or slightly positive impact on

¹ While Sjaastad (1962) formalized the model from the perspective of an individual, Mincer (1978) did it from the perspective of the family, where a dual-career couple will move only if the overall gain to the household outweighs the potential loss one partner might experience in their own job.

average native wages and employment, although sudden large inflows can temporarily depress wages and jobs in the short run (see Edo, (2019) for a review of the literature). Nonetheless, immigrants' skill composition matters, as native workers whose skills compete with immigrants tend to see wage losses, while those in complementary occupations see gains, creating distinct "winners" and "losers" in the labor market (Edo, 2019; Lewis, 2011; Ottaviano & Peri, 2012). Politically, immigration can lead to increased support for anti-immigrant parties and lower demand for redistribution among natives (see Alesina & Tabellini, (2024) for a review). Socially, immigration may reshape attitudes toward diversity and national identity, and in doing so, affect social cohesion. When cultural differences are salient, it can heighten perceptions of in-group and out-group distinctions, potentially leading to polarization or reduced solidarity within the host society (see Alesina & Tabellini, (2024) for a review).

Despite growing interest in international labor mobility, important gaps remain in the literature regarding the barriers that constrain cross-border mobility, the role of policy in shaping these flows, and how destination economies respond to immigration under varying cultural conditions. Globalization has significantly reduced the costs of international labor mobility, and Europe has gone further through regional integration and the free movement of goods, services, and labor. Yet, removing institutional barriers has not led to fully integrated labor markets,² suggesting that other factors, such as cultural differences, can also deter mobility across borders (Dorn & Zweimüller, 2021). At the same time, governments can use national policies to actively influence individuals' mobility decisions, as is the case of special tax regimes to attract highly skilled foreign workers (Kleven, Landais, Muñoz, & Stantcheva, 2020). Understanding how these factors shape mobility patterns is essential, both to explain who moves and why, and to assess the labor market consequences for destination countries. In this context, this dissertation focusses on three interrelated aspects of international

² For instance, the European labor market remains considerably less integrated and more heterogeneous than the US labor market.

labor mobility: the role of cultural differences in cross-border commuting, migrants' responses to the removal of a tax incentive, and the labor market effects of a large immigration shock under conditions of cultural proximity. Together, these themes answer the broader, overarching, research question: how do economic and non-economic factors shape the patterns and consequences of international labor mobility?

This thesis consists of three chapters, each of them addressing a distinct gap in the economics literature on international mobility. **Chapter 2** asks the question of how cultural distance between regions affects cross-border commuting? This chapter contributes to the literature by relying on a novel use of granular and spatial linguistic variation to capture cultural distance between regions, a factor often overlooked in commuting studies but increasingly recognized as crucial in shaping immigration outcomes. In **Chapter 3**, we ask how financial incentives affect migration decisions? We study the impact of a temporary tax incentive for skilled migrants on their decisions to remain in the host country once the incentive expires. While prior studies document large mobility responses to preferential tax schemes among top earners,³ far less is known about emigration responses down the income distribution. By focusing on the post-expiration behavior of a broad set of migrants, this chapter fills this gap in the migration literature. Finally, in **Chapter 4**, we ask to what extent natives' labor market outcomes in the destination country are affected by immigration? This chapter studies the local labor market effects of a sudden and unexpected Surinamese immigration wave to the Netherlands in the mid-1970s. Differently than previously explored in the literature, immigrants arrived in the host country with full labor market rights and relatively strong cultural and linguistic proximity, offering new insights into how local economies absorb unexpected migration waves in the short run.

The Netherlands is the case study for this dissertation for several reasons. First, with a population of roughly 17.5 million in 2022, more

³ See Kleven, Landais, Muñoz, & Stantcheva, (2020) for a review.

than one in four Dutch residents had a migration background,⁴ and approximately 14% were born outside the country.⁵ Second, the Netherlands provides access to unique and high-quality migration administrative data, which combined with tax administrative data and historical dialect, census and labor force data, offers empirical opportunities rarely available elsewhere. Third, migration has been a central and politically sensitive topic in the Netherlands, regularly dominating electoral campaigns and policy debates. Over the years, public discourse has been shaped by concerns over labor market impacts, housing prices, integration, and cultural identity, leading to policy adjustments such as the recent changes to the 30% tax ruling,⁶ as well as discussions to tighten asylum and family reunification rules, to introduce stricter integration requirements, and to impose new limits on international student inflows. Thus, exploring the drivers of international labor mobility and its labor market effects is essential for generating academic knowledge, guiding evidence-based policymaking, and contributing to public and political discourse. The remainder of this introduction provides a detailed account of each chapter.

Chapter 2 investigates the role of cultural distance in shaping cross-border commuting flows from Belgium to the Netherlands. To this

⁴ Migration background is defined as the first- and second-generation immigrants. First generation immigrants are those born outside the Netherlands while second generation immigrants are people born in the Netherlands but with one or both parents born elsewhere.

⁵ Among the first-generation immigrants, about one-third of migrants were born in Europe and two-thirds were born outside Europe. Of those born outside Europe, most have a Turkish (12.2%), Surinamese (9.6%) and Moroccan (9.5%) background. Among the second generation, the distribution of countries of origin differs according to the parents' country of birth. Of those with one parent born outside the Netherlands, just over half are of non-European origin with around one-sixth of them having a parent who was born in Indonesia. Of those with both parents born outside the Netherlands, 86.4% are of non-European origin. They often have a Moroccan (20%) or Turkish (17%) background (Statistics Netherlands (CBS), 2023)

⁶ The 30% tax ruling has undergone several changes over the years, with the most recent one taking place in 2025. **Chapter 3** of this thesis focuses on the reform implemented in 2012.

end, we use unique administrative data on commuting flows from Belgium to the Netherlands and historical dialect data covering the Netherlands and the Dutch-speaking part of Belgium. By explicitly integrating cultural proximity into commuting models, this chapter introduces a novel perspective, extending previously migration-focused frameworks to commuting behavior.⁷ We use historical dialects to measure cultural distance between regions based on the identifying assumption that they serve as a proxy for contemporaneous culture while affecting commuting only through the cultural channel (Falck et al., 2012). Given that “dialect borders” do not align perfectly with national borders, we introduced meaningful variation in cultural distance between municipalities on opposite sides of the Belgium–Netherlands border. In our basic regression, an increase in cultural distance between municipalities by one standard deviation leads to a robust decrease ranging from 7% to 22% in cross-border commuting flows from Belgium to the Netherlands. Our findings also suggest that at short distances, economic factors—such as lower commuting costs—are the dominant influence, whereas at longer distances (around 55 to 60 kilometers), the impact of cultural dissimilarity becomes more pronounced. Finally, we show that within the Netherlands, cultural dissimilarity plays a smaller role in deterring commuting flows and that the presence of national borders magnifies the effect of cultural dissimilarity, adding layers of institutional and administrative barriers that further hinder cross-border labor mobility.

In **Chapter 3**, we investigate the workers’ mobility response to a loss in preferential tax treatment, exploiting a sharp and unexpected policy change that shortened the duration of the tax break available to high-skilled migrants to the Netherlands. The policy change only affected migrants who arrived from specific countries within a certain period. Since no wage threshold was used to define eligibility, this creates a unique opportunity to study emigration responses down the income

⁷ For more on the migration-focused frameworks please refer to Adsera & Pytlikova, (2015); Belot & Ederveen, (2012); Falck, Heblich, Lameli, & Südekum, (2012); Huttunen, Møen, & Salvanes, (2018); Piyapromdee, (2021).

distribution—an unexplored area in the existing literature. We relied on tax and population-wide administrative data and a difference-in-differences approach to uncover the causal effect of tax-induced emigration. Consistent with theoretical models on tax-induced mobility,⁸ our findings show that taking away tax breaks from migrants strongly increases their likelihood of leaving the country. Crucially, our findings show that this effect is almost entirely driven by out-migration from the top 1% of the income distribution and no migration response was detected for those below the 95th earnings percentile. Individuals in the top 1% of the income distribution decrease their length of stay by 11.1% and are 18.3% less likely to remain beyond the end of the five years that they receive the tax break, while ‘highly mobile’ individuals, those who have arrived from a country that is not their country of origin/nationality, also react in terms of both intensive and extensive duration margins when among top 5% of earners. We estimate that the elasticity of migration with respect to the net-of-tax rate for the top 1% of earners is 1.48 to 1.74. Regarding the distortionary effect of national policies on international tax competition, we show that the most mobile tax earners become much more likely to leave for a tax-friendly country post reform, and we conclude that, at least in terms of the change in taxable receipts from labor in the Netherlands, this policy was cost neutral.

Chapter 4 analyzes the labor market effects of a large, unexpected migration wave: the arrival of Surinamese immigrants following Suriname’s independence in 1975. This event generated a sudden and exogenous labor supply shock, as immigrants had full labor market rights and relatively strong Dutch language skills. Using historical census data, labor force surveys, and population counts, we apply a difference-in-differences strategy exploiting variation in local immigrant exposure while controlling for initial foreign-born shares. The results align with the canonical immigration model, which predicts that immigration reduces employment or wages for natives with similar skills, while effects on

⁸ See, for instance, (Agrawal & Foremny, 2019; Akcigit, Baslandze, & Stantcheva, 2016; H. J. Kleven, Landais, & Saez, 2013; H. J. Kleven, Landais, Saez, & Schultz, 2014)

other groups may be limited or even positive.⁹ We show an almost 0.9% decline in employment among low-skilled natives, with no significant impact on high-skilled employment or occupational scores. While we do not directly observe immigrants' language proficiency, we find circumstantial evidence that municipalities receiving more Creole-background immigrants—presumed to have stronger Dutch proficiency—experienced slightly larger negative employment effects than those receiving more Hindustani or Javanese immigrants. These findings are supported by the theoretical frameworks suggesting that the complementarity or substitutability of immigrant skills shapes natives' labor market outcomes, and they highlight that language proficiency may be an important factor influencing these outcomes when it does not create comparative advantages for natives.¹⁰ However, further research is needed to unpack the mechanisms behind these findings.

Chapter 5 concludes the dissertation by reflecting on the broader implications of the findings and offering policy recommendations. In addition, it acknowledges the study's limitations, such as data constraints and methodological considerations, and outlines potential directions for future research to build on the results.

⁹ See Dustmann, Schönberg, & Stuhler, (2016) for a thorough review on the topic.

¹⁰ See Edo, (2019); Lewis, (2011); Ottaviano & Peri, (2012) for a discussion

2

Culture and Cross-Border Commuting

This Chapter is based on a joint work with Frank Cörvers and Hans Schmeets and has been accepted for publication in the Journal of Economic Geography. We would like to thank Wilbert Jan Heeringa for sharing the historical dialect differences data used in this Chapter. We thank the editor of the Journal of Economic Geography, Jorge De la Roca, and three anonymous referees for the helpful comments and suggestions. We would also like to thank participants at multiple seminars, workshops, and conferences for their valuable suggestions.

Abstract

Using unique administrative data and historical dialects data for the Netherlands and the Dutch-speaking part of Belgium, we estimate the effect of cultural dissimilarity on cross-border commuting flows from the Dutch-speaking regions of Belgium (i.e. Flanders and Brussels) to the Netherlands. The results show that economic drivers, specifically the cost of commuting measured by the geographical distance remains the most important factor defining the distribution of workers across the national borders of Belgium and the Netherlands. Furthermore, we showed that an increase in cultural difference between municipalities by one standard deviation leads to a robust decrease in cross-border commuting flows from Belgium to the Netherlands ranging from 7% to 22%.

2.1 Introduction

The European economic and political integration that started shortly after World War II is grounded in the neoclassical and neoliberal belief that removing institutional barriers will foster the economic development of all participating countries (Bartz & Fuchs-Schündeln, 2012; Knotter, 2014). This ongoing process is based on two cornerstones: free movement of goods and services and free movement of labor. Labor mobility takes two distinct forms, namely migration and commuting across regions. Migration is mainly a one-time movement of an individual from one place to another, while commuting is periodic travel between an individual's place of residence and work. This paper will focus on the latter.

Labor mobility is an important adjustment mechanism in the labor market to prevent inefficient allocation of labor resources and, as a consequence, economic distortions. Although the impacts of regional labor mobility can be translated into aggregate labor market outcomes—participation rate, unemployment rate, and wage level (Cörvers et al. 2009)—it is ultimately a family decision whether or not to move.¹ This decision is based on economic and non-economic drivers within or across national borders and such drivers are the subject of an extensive and expanding literature. The standard migration models developed by Sjaastad, (1962) and Mincer, (1978) explore the economic drivers of migration, but the returns on mobility were measured only in terms of economic gains. More recently, the literature has incorporated non-economic drivers of individual and family decisions to migrate, such as language, culture, and family ties (Adsera & Pytlikova, 2015; Belot & Ederveen, 2012; Falck et al., 2012; Huttunen et al., 2018).

Commuting studies mainly focus on economic drivers, such as labor supply, wages, transportation infrastructure, and regional borders (Clark et al. 2003; Gutiérrez-i-Puigarnau and van Ommeren 2010;

¹ We are aware that public policies such as investments in (cross-border) infrastructure or public transport play a role as well for (cross-border) travel, although these are (partly) endogenously determined.

Heuermann and Schmieder 2019; Mulalic et al. 2014; Persyn and Torfs 2016; Shirley 2018). Nonetheless, there is scant evidence of the impact of non-economic factors on commuting.² Moreover, unlike the migration literature, the literature on commuting has mainly focused on movements within national borders. Cross-border travel opportunities often seem to be underutilized compared to the use of within-country infrastructure and public transport (Plat & Raux, 1998; Rietveld, 2012; Vickerman, 2016).

This paper explores the effects of economic and non-economic drivers and impediments on labor mobility between municipalities in the European Union. Specifically, we study how commuting flows from the Dutch-speaking Belgium municipalities (i.e. Flanders and Brussels) to the Netherlands are shaped by the cultural dissimilarities between municipalities. In other words, how do cultural distances between two municipalities—municipalities of residence and workplace—influence individuals' decisions to commute across the border? Cultural distance³ defines to what degree a particular workplace region is culturally unfamiliar to an individual's region of residence.

The majority of commuting studies have focused on internal mobility with a few exceptions focusing on cross-border labor mobility at the NUTS-2 level (Bouwens 2004; Broersma et al. 2022; Buch et al. 2009). To the best of our knowledge, this paucity in empirical evidence is due to the lack of (1) suitable data to study international cross-border movements and (2) measures of culture that provide variation at a fine spatial scale and that are independent of people's mobility decisions. This paper addresses these problems by using administrative Dutch data from Statistics Netherlands (CBS) and by introducing a dialect database from the Reeks Nederlandse Dialectatlassen that was collected between 1922 and 1975.

² One exception is Persyn (2015).

³ Cultural distance and dissimilarity will be used as synonyms as will cultural proximity and similarity.

A major issue when assessing the impacts of non-economic drivers on mobility and, specifically, cultural factors is reverse causality. To overcome this problem, we rely on the definitions of culture and cultural differences between the regions provided by the cultural economic literature. As proposed by Guiso et al. (2006), culture is the intergenerational transmission of values and beliefs that barely change over time. Moreover, Fernández and Fogli (2009) defined cultural differences as the systematic distinction in values and beliefs across either socially or geographically different groups. Our research strategy, therefore, relies on these definitions and we argue, following Tabellini (2010), that cultural traits from earlier generations are a natural proxy for the current culture.

Notwithstanding the definitions, if our past cultural variable is to be a valid proxy for contemporaneous culture, it needs to affect commuting through the cultural channel alone. The historical dialect is a good candidate to fulfill this requirement for two reasons: 1) past barriers to communicating in different dialects do not form communication barriers that would hinder mobility today (Falck et al. 2012); and 2) language is correlated with the evolution of ideas from earlier generations (Tabellini 2008a). Thus, historical dialects are uncorrelated with current mobility except through contemporaneous culture. The strength of the identification strategy relies on the fact that the “borders” of historical dialects do not perfectly overlap with national borders, thus creating variation within and across countries that can be used to measure the cultural distance between two municipalities of different countries.

We use administrative cross-border labor mobility data and a dialect database for the Netherlands and Flanders/Brussels in Belgium to estimate the causal effect of cultural distance on cross-border commuting flows. The data on international commuting are collected by Statistics Netherlands (CBS) and cover the year 2017, while the dialect database is based on the Reeks Nederlandse Dialectatlassen and covers the period 1922–1975. These data are complemented with regional data from Statistics Netherlands, the national statistical office of Belgium (StatBel), and the National Bank of Belgium. Precise information on an individual’s

place of residence and work is available, allowing the municipalities of origin and destination pairs to be accurately inferred.

Our contribution is threefold. First, to our knowledge, this is the first paper to explore microdata from an administrative database on international cross-border mobility. We use information, such as place of residence and workplace, age, and gender, at the individual level. These data are enriched by using information at the municipality level on population, sector composition, average workers' compensation, and average housing prices or at NUTS-3 when the more disaggregated information is not available. Second, in line with Belot and Ederveen (2012), this paper explores explicit measures of culture, although at a finer scale. Moreover, we go beyond Falck et al. (2012) by exploring movements across national borders and focusing on commuting instead of internal migration. Third, we incorporate insights into non-economic drivers from migration models and develop a commuting framework to assess the impact of cultural traits on commuting.

Our main finding is that for any given geographical distance between places on either side of the border, the distribution of workers across the border is negatively affected by the cultural distance between these places. In our basic regression, an increase in cultural inequality between municipalities by one standard deviation leads to a robust 11.59% decrease in cross-border commuting flows from the Dutch-speaking part of Belgium to the Netherlands. Although at first glance the effect is rather small—10.76% of that of geographical distance—it is still meaningful, especially given that our study focuses on commuting behavior for which distance is expected to matter the most. We argue that our results can be interpreted as the causal estimation of cultural distance on commuting. To corroborate this, we perform several robustness checks for our baseline specification to rule out the effects of omitted variables.

Finally, we extend our analysis to better understand the interplay between geographical distance and cultural dissimilarity in shaping cross-border commuting flows. Our heterogeneity analysis reveals that cultural dissimilarity remains a barrier to commuting, even at shorter

distances where economic factors like reduced commuting costs tend to dominate. As the distance threshold increases to around 55-60 kilometers, the influence of cultural dissimilarity becomes more pronounced, compounding with higher economic costs and further curtailing cross-border mobility. Additionally, by including both intra-country commuting within the Netherlands and cross-border commuting from Belgium to the Netherlands, we explore the mechanisms driving these effects. Our findings indicate that within the Netherlands, cultural dissimilarity plays a smaller role due to shared institutional structures, whereas crossing national borders magnifies the impact of cultural dissimilarity, adding layers of institutional and administrative barriers that significantly reduce cross-border labor mobility.

The rest of the paper is organized as follows. Section 2 includes the literature review in which related papers are briefly revisited and important definitions are clarified. The third section describes the methodology and research strategy, in which a gravity model is developed by incorporating non-economic drivers in the commuting flows equation. Section 4 presents the administrative cross-border commuting data, as well as the historical dialect database. The baseline results are presented in Section 5 and with the respective robustness checks. Section 6 and 7 explore the heterogeneity analysis and mechanisms of our results, respectively. Section 8 concludes and discusses some international mobility issues.

2.2 Literature Review

It is only recently that the economic literature has begun to explore differences in cultural traits and their influence on a variety of economic choices (Alesina and Giuliano 2015). One challenge researchers face when studying cultural economics relates to the definition of culture, which is not a straightforward concept. Nonetheless, it is possible to distinguish the empirical and theoretical definitions (Alesina and Giuliano 2015). On the theoretical side, authors usually define culture using the distinction between values and beliefs. Some authors have defined culture as a set of beliefs about the consequences of one's actions, but these beliefs can be

manipulated by earlier generations. One example is provided by Guiso et al. (2008) who used an overlapping generation model to study how cultural transmission forms individual beliefs. In their model, children absorb their beliefs from their parents, update them based on their own experiences, and transmit their own beliefs to their offspring. In contrast, Akerlof and Kranton (2000) developed a model in which individuals acquire a set of values as a form of prescriptions on how they should behave in accordance to some people and in contrast with others.⁴ It is noteworthy, however, that the two definitions are not incompatible as shown by the models developed by Bénabou (2008) and Bénabou and Tirole (2011), according to which individuals systematically update beliefs when processing and internalizing their own values.

On the empirical side, most papers rely on the definition of Guiso et al. (2006). The authors define culture as the intergenerational transmission of values and beliefs that barely change over time. Contrary to the theoretical definitions, no distinction is made between values and beliefs. Following Fernández and Fogli (2009) we define cultural differences as the systematic distinction in values and beliefs across either socially or geographically different groups. Moreover, values and beliefs, which are intrinsic to each individual of a certain group or region, are transmitted across generations forming the cultural traits of individuals.⁵

The empirical research in the field of cultural economics is in its infancy and aims to establish the causal relation between culture and economic and social outcomes. To assess the causal impact of cultural traits on economic and social outcomes, the literature has explored three different methods,⁶ namely those based on survey data, on an

⁴ Akerlof and Kranton (2000) introduced the concept of “identity economics” in which economic outcomes were analyzed by establishing individuals’ identities as primary motivations for choice.

⁵ In the rest of this paper values and beliefs as synonyms for the concept of cultural traits.

⁶ There is also a small body of experimental work that this paper will not review.

epidemiological approach,⁷ and on historical case studies (see Alesina and Giuliano 2015 for a review of the literature and its challenges).

Traditionally, when measuring cultural traits through survey questions, the following variables have been explored: generalized trust, individualism vs. collectivism, family ties, and generalized morality (Alesina et al. 2015; Alesina and Giuliano 2010; Guiso et al. 2006; Guiso et al. 2009; Tabellini 2008a, 2010).⁸ Here, the endogeneity problem arises from the fact that differences in values or beliefs may be a consequence of the diverse institutional and economic environments across countries or regions. By making use of instrumental variables, this approach attempts to overcome the problems of reverse causality and omitted variables. The second way to estimate the causal effect of culture on economic outcomes is to rely on the epidemiological approach that examines how immigrants of different origins, usually of the second generation, behave in the same destination country (Alesina et al., 2015; Fernández, 2007; Fernández & Fogli, 2009). This approach exploits the fact that all second-generation immigrants are subject to the same markets and institutions in the destination country but potentially differ with regard to the cultural traits inherited from their parent's country of origin.

Putnam et al. (1994) was the first historical case study to link the long-term persistence of economic development and culture. They exploited a natural experiment in which an institutional reform in the 1970s in Italy functioned differently by region. Putnam et al. (1994) hypothesized that the discrepancy in economic performance between regions reflects a persistent difference in the level of cooperation, participation, social interaction, and trust, which the authors labeled as "social capital."⁹ These regional differences in social capital were linked

⁷ Evidence based on immigrants and their descendants are referred to as the "epidemiological approach" by Fernández (2008).

⁸ The literature has explored these variables as referring to cultural traits without distinguishing between values and beliefs to define one's culture.

⁹ The field of cultural economics classifies this as cultural traits. This paper will follow the empirical literature and refers to the set of values and beliefs intergenerationally transmitted as cultural traits that define one's culture.

to whether or not the region experienced the institution of free cities in the twelfth century. This historical institution arrangement created a deep civic sense and cooperative behavior, cultural traits that were transmitted from generation to generation. Guiso et al. (2016) revisited Putnam et al.'s (1994) hypothesis and used the presence of free cities in the past to explain how self-efficacy beliefs—a cultural trait transmitted across generations—still impacts current levels of civic capital.

Our paper explores a cultural measure that is closely related to the use of historical case studies to measure culture. Guiso et al. (2016) and Putnam et al. (1994) made use of a historical event to document the long-term persistence of cultural traits on different economic and social outcomes. These studies, however, do not explore the transmission mechanism behind the persistence of cultural differences over time. We argue that language is correlated with the evolution of ideas from earlier generations (Tabellini, 2008a) and, thus, with the transmission mechanism. For instance, regarding values and beliefs, Dohmen et al. (2012) empirically tested three channels of intergenerational transmission of risk preferences and belief in trustworthiness of other people, namely 1) the transmission from parents to children, 2) the impact of prevailing attitudes in the local environment and 3) the impact of positive assortative mating patterns of parents. Dohmen et al. (2012) documented that children and parents have similar patterns of choices due to the intergenerational transmission of attitudes, which in turn may culminate in similar outcomes regarding, for example, social interactions and behavior, choice of occupation, investments in assets, and education.

This paper also relates to the urban economic literature, specifically to cross-border commuting. Broersma et al. (2022) explored the economic drivers of cross-border commuting movements in regions of the Netherlands, Belgium, and Germany at the NUTS-2 level. They found, in line with the theoretical expectations, that the higher the wage and the lower the unemployment in the residency region, the lower the out-commuting. Other studies also look at the issues related to cross-border commuting and find that cross-border immobility, rather than mobility, is the norm in Europe, despite the regional economic differences

(Bouwens 2004; Broersma et al. 2022; Buch et al. 2009). Importantly, although these studies explored the non-economic drivers of cross-border commuting, they have mainly done so using a simple dummy variable indicating whether a common (national) language is spoken across border regions.

Finally, our paper also relates to a growing literature on the effects of local amenities on location decision choices and (internal) mobility (Cresswell 2014; Falck et al. 2012; Frenkel et al. 2013; Hooijen et al. 2017; Hooijen et al. 2020; Huttunen et al. 2018; Imeraj et al. 2018; Moretti 2011; Rijnks et al. 2018; Venhorst 2013; Venhorst and Cörvers 2018). For instance, Huttunen et al. (2018) treated the presence of extended family members as a location-specific amenity that affects costs and expected gains of mobility. The authors used administrative data to identify if individuals had parents and siblings living in the same labor market region and, therefore, whether non-economic factors may affect mobility. Huttunen et al. (2018) found that individuals are less likely to move away from regions where their extended family lives given that, in general, individuals enjoy living in the company of their family. Huttunen et al.'s (2018) contribution, therefore, is not related to the subjective cultural trait defined by the strength of family ties' but rather an objective measure of extended family presence in the labor market.

Falck et al. (2012) also explored the effects of culture on mobility by studying how individuals in Germany base their mobility decisions on their preference for living in culturally familiar environments. The authors defined cultural proximity across German regions as the degree of similarity between the dialects spoken in each region more than 120 years ago. Falck et al. (2012) mitigate the confounding factor of culture and institutions by exploiting within-country variation of the degree of similarity between the historical dialects. Their main finding is that, although not as important as geographical distance, cultural distance plays an important role in internal mobility decisions.

In sum, the literature has explored the effects of different local amenities and cultural traits on economic and social outcomes. However,

when it comes to the study of culture and mobility, empirical research has mainly focused on internal movements. Turning to international mobility, as emphasized by Belot and Ederveen (2012), most empirical studies have only used rough proxies of cultural differences, such as a dummy for common language. One of their main contributions is, therefore, to propose explicit measures of culture that allow testing of cultural differences as barriers to migration. Belot and Ederveen (2012) made use of cross-country variation in cultural variables, although, as pointed out by Falck et al. (2012), cross-country analyses may hide substantial heterogeneity in cultural traits within a country.

2.3 Methodology and Research Strategy

The standard migration models developed by Sjaastad (1962) and Mincer (1978) analyze the perspectives of an individual or a family that maximize the utility of migration depending on the net economic gains. The costs associated with the movement depend on economic factors, family structure, and unobservable components. The main idea of the models based on these frameworks is that mobility is driven by economic opportunities. More recently, the economic literature has also explored non-economic drivers of migration decisions, such as local-specific amenities. Nonetheless, commuting models such as those developed by Heuermann and Schmieder (2019) and Persyn and Torfs (2016) focus mainly on the economic drivers of mobility decisions.

Moretti (2011) studied the equilibrium in local labor markets and suggested that individuals' indirect utility depends on the nominal wage, cost of housing, local amenities, and a random term capturing individual idiosyncratic preferences. These idiosyncratic preferences relate to how strongly an individual is attached to a municipality, keeping constant the nominal wage and specific local amenities. That is, a municipality may become more attractive to someone if the individual was born there, by having a family member present, or by a more culturally familiar place. Moreover, Venhorst, (2013) and Huttunen et al. (2018) argue that, in general, individuals enjoy living in the company of their family members and the costs to move away from them are high. Furthermore, these

authors suggested that having family members in a specific municipality can be thought of as one location-specific amenity. It is noteworthy that these models were developed to explain which workers select into regional mobility after a local shock. In our framework discussed below, we rely on the same reasoning as Huttunen et al. (2018), and we treat cultural familiarity as a local amenity that decreases the costs of commuting from one municipality to another.

Falck et al. (2012) also study migration decisions and present evidence that, conditional on economic factors, individuals base their migration decisions within Germany on their preference for living in culturally familiar places. Here, we explore how cultural dissimilarity influences the distribution of commuters across the border. The dissimilarity degree is treated as a local amenity and the hypothesis is that individuals prefer to commute to places where cultural differences are small, conditional on geographical distance and economic factors. Our framework is based on the commuting models of Heuermann and Schmieder (2019) and Persyn and Torfs (2016). Consider the following gravity equation where C_{od} is the commuting flow from the origin municipality, o , to the destination municipality, d .

$$C_{(od)} = M_{(o)}N_{(d)}\delta_{od}^{-\theta} \quad (1)$$

where M_o are the push factors of the origin municipality, o , with respect to all possible destination municipalities, while N_d are the pull factors of the destination municipality, d , with respect to all origin destinations. The term $\delta_{od}^{-\theta}$ refers to specific commuting costs that hamper commuting flows from o to d , with $\theta > 0$. Now, assume that commuting costs δ_{od} are a function¹⁰ of the geographical distance in kilometers and a continuous variable CD_{od} that represents the cultural differences such that:

$$\delta_{od} = Dist_{od}^{\alpha_1} CD_{od}^{\alpha_2} \quad (2)$$

¹⁰ Persyn and Torfs (2016) assume the commuting costs to be a log-linear function of the distance and use a dummy variable representing the effects of a regional border.

$Dist_{od}$ is then calculated as the Euclidian distance in kilometers from the origin municipality centroid, o , to the destination municipality centroid, d . The term CD_{od} captures the cultural differences between the two regions, o and d , such that $0 \leq CD_{od} \leq 100$.¹¹ Now, suppose that M_o and N_d can be written as a log-linear function¹²:

$$M_o = \exp^{\beta_3 X_o} P_o^{\beta_4} \quad (3)$$

$$N_d = \exp^{\beta_5 Z_d} W_d^{\beta_6} \quad (4)$$

where X_o is the origin municipality-specific characteristics, such as sectoral composition, housing prices, and average paid wage, that ease out-commuting from that region, while P_o is the population in the origin region and can be seen as the potential number of out-commuters. Z_d is the destination municipality counterpart of X_o , while W_d is the number of workers in the destination municipality. Substituting Equations 2, 3, and 4 into 1 and then taking the natural logarithm results in the following final empirical equation:

$$C_{od} = \exp^{\beta_3 X_o} P_o^{\beta_4} \exp^{\beta_5 Z_d} W_d^{\beta_6} [Dist_{od}^{\alpha_1} CD_{od}^{\alpha_2}]^{-\theta}$$

$$\begin{aligned} \ln(C_{od}) = & -\beta_1 \ln(Dist_{od}) - \beta_2 \ln(CD_{od}) + \beta_3 X_o + \beta_4 \ln(P_o) \\ & + \beta_5 Z_d + \beta_6 \ln(W_d) + \rho_o + \gamma_d + \varepsilon_{od} \end{aligned} \quad (5)$$

Equation 5 is the final equation to be estimated, where $-\beta_1 = -\theta\alpha_1$ and $-\beta_2 = -\theta\alpha_2$ and an error term, ε_{od} , as well as origin-NUTS2 and destination-NUTS2 fixed effects, ρ_o and γ_d , respectively, are added.

¹¹ Even though the construction of the variable allows values from 0 to 100, in our setting it varies between 10.68 and 51.46. See Section 4.2 for further explanation and Figure 2.A.1 for the cultural dissimilarity distribution.

¹² In contrast with Heuermann and Schmieder (2019), here we assume a log-linear function to be able to easily include the regions' characteristics and not only the regions' fixed effects.

However, there are two problems when estimating a model of workers' distribution across the border depending on cultural distance such as how to measure cultural dissimilarity and endogeneity problems. The first problem arises when trying to measure contemporaneous culture on a fine scale. Most papers have relied on survey data to measure cultural values. However, due to the fact that surveys are often based on small samples, regional information on cultural values is only available and representative at the country level or at most at NUTS-2. Since most commuters tend to travel small distances, meaning that most movements would occur from a few NUTS-2 regions on one side of the border to a few other NUTS-2 regions on the other, this results in a low variance in cultural differences. The second problem refers to the endogeneity problem. This occurs since commuting across the border may depend on the contemporaneous cultural proximity while the current cultural proximity might be affected by the number of commuters between the two regions.

To circumvent these problems, it is important to understand how contemporaneous culture is determined in our paper. As proposed by Guiso et al. (2006) culture can be defined as the intergenerational transmission of values and beliefs that hardly change over time. Hence, by using this definition, it is possible to rely on a historical measure of culture as a proxy for contemporaneous culture. The requirement for this proxy to be valid for the purposes of this paper is that this variable serves as a proxy for contemporaneous culture and, at the same time, does not affect commuting other than through the cultural channel. Therefore, we rely on historical dialects, given that this variable correlates with the intergenerational transmission of values and beliefs and is uncorrelated with current mobility.

In other words, the advantage of using the historical dialect is that it does not impose communication barriers that would prevent mobility today, given that the use of dialects is less common nowadays than it was in the past (Falck et al., 2012).¹³ Moreover, Tabellini (2008a) argued that

¹³ For the Netherlands and Flanders, see Rys et al. (2021) and Schmeets and Cornips (2021).

language is correlated with the evolution of ideas from earlier generations and hence the intergenerational transmission of values and beliefs. Hence, historical dialects are uncorrelated with current mobility except through current culture and are correlated with contemporaneous culture. It is noteworthy that the strength of the identification strategy relies on the fact that the “borders” of historical dialects do not perfectly overlap with national borders, creating variation within and across countries in cultural traits that will be used to measure the cultural similarities between two regions.

2.4 Data, Variable Definitions, and Descriptive Statistics

2.4.1 Cross-Border Database

The analyses rely on a cross-border commuting administrative database from Statistics Netherlands (CBS) that covers the year 2017. For this year, **Table 2.1** shows that 34,530 individuals crossed the borders of the Dutch-speaking part of Belgium (i.e. Flanders and Brussels) to work in the Netherlands. Cross-border commuters travel, on average, 56 km from the (Belgium) municipality of origin to reach the (Dutch) municipality of destination and a median distance of 34.5 km. The majority of commuters are male (59%) and 43.1 years old on average, while females were slightly younger, averaging 42 years old. Of all commuters, most are employed in the sectors of Government, care, culture, recreation and other services (28%). The aggregation into only six different sectors is due to the lack of more disaggregated data for Belgium municipalities. In **Table 2.2** these are, together with the Dutch data, used to construct a sectoral composition similarity index between pairs of origin and destination regions.

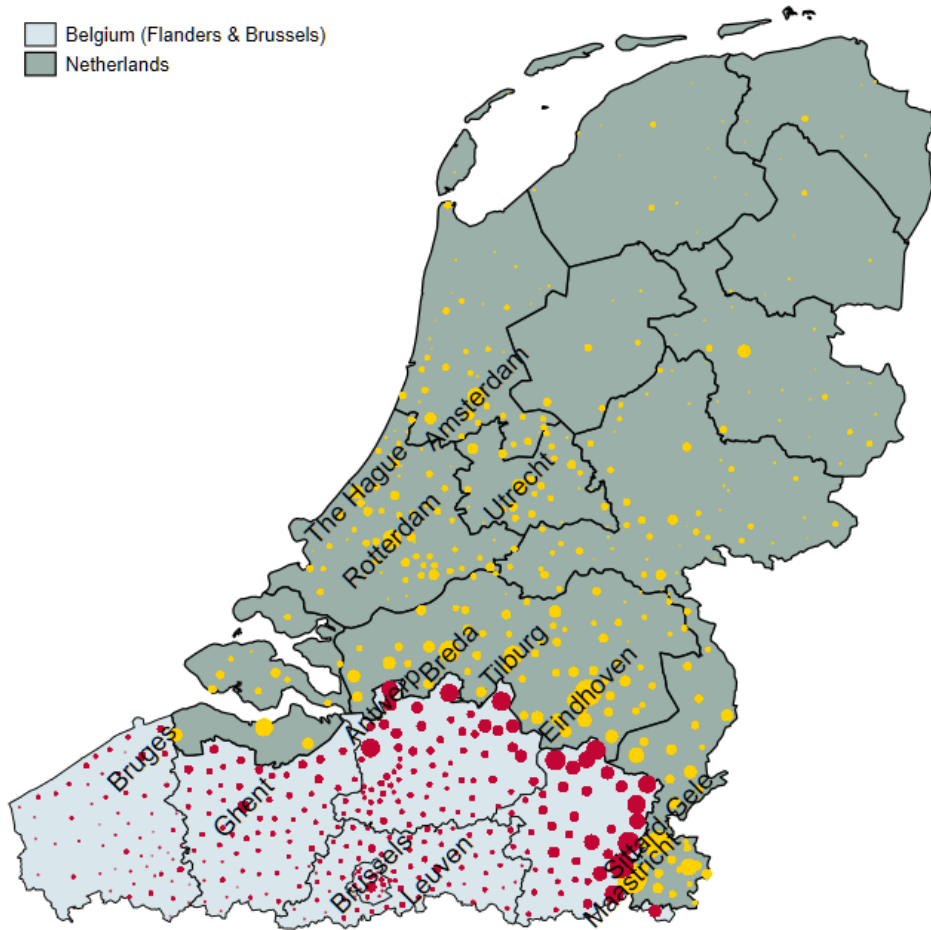
Table 2.1: Descriptive Statistics: Commuters

	Commuters	
	Mean	Std. Dev.
Distance (km)	56.26	50.41
Age (years)	42.73	12.12
Gender (male)	0.59	0.49
Agriculture, forestry, and fishing	1%	7%
Manufacturing and other industries	21%	41%
Construction	3%	17%
Wholesale, retail, transportation, accommodation, and food services	26%	44%
Financial, business services, and real estate	21%	41%
Government, care, culture, recreation, and other services	28%	45%
Observations	34,530	

Source: Own calculations using administrative data on individual commuters from Statistics Netherlands (CBS).

Around 56% of the total commuters are of Dutch nationality while 40% are Belgians and 4% are other nationalities. **Figure 2.1** depicts these movements; red points represent the commuters from each origin region and yellow points represent the mass of commuters to each destination region. The thicker the points, the higher the mass of individuals commuting from or to that region. Note that individuals do not necessarily commute to the closest point across the border and, hence, the points should not be viewed as a pair of origin and destination municipalities but solely as one origin municipality and one destination municipality.

Figure 2.1: Cross-Border Commuter Distribution



Notes: Red points represent the mass of commuters from each origin municipality in Belgium, whereas yellow points represent the mass of commuters in each destination municipality in the Netherlands. The regions depicted in the graph correspond to the NUTS 2 regions in Netherlands and Belgium

Source: Own calculations based on administrative data from Statistics Netherlands (CBS).

Table 2.2 shows that there are in total 133,860 pairs of municipalities (388 municipalities in the Netherlands and 345 in Belgium from which 7,949 unique pairs of origin and destination municipalities have positive commuting records from Belgium to the Netherlands. The average number of individuals commuting between all pairs of municipalities is 0.26 and, when considering only the municipalities with

positive records, the average is 4.34. Moreover, the positive commuting records show that roughly 23 individuals per 100,000 inhabitants commute from each region in Belgium.

Table 2.2: Descriptive Statistics: Municipalities

	All Municipalities		Positive Commuting Municipalities	
	Mean	Std. Dev.	Mean	Std. Dev.
Number of commuters per pair of municipalities	0.26	5.05	4.34	20.28
Number of commuters per 100,000 inhabitants	1.357	26.91	22.85	108.18
Distance (km)	165.65	71.65	96.60	51.29
Dialect distance	34.66	5.50	30.16	5.83
Working Pop Dutch destination municipalities	22,537.11	35,182.10	58,414.31	86,186.63
Population in Belgium origin municipalities	23,782.40	42,927.40	45,685.82	94,390.37
Sectoral composition similarity	0.29	0.11	0.26	0.09
Unemployment NL (%)	4.82%	1.11%	4.58%	0.95%
Unemployment BE (%)	5.04%	2.55%	4.98%	1.88%
Workers' compensation Netherlands (million €)	12,676.98	11,335.32	16,000.30	12,421.16
Gross investment Netherlands (million €)	5,410.59	5,205.39	6,981.31	5,760.63
Workers' compensation Belgium (million €)	10,100.51	9,319.58	10,346.72	8,815.16
Gross investment Belgium (million €)	4,891.26	3,613.67	5,058.90	3,716.70
Observations	133,860		7,949	

Notes: This table is divided into two parts: 1) all pairs of municipalities and 2) all pairs of municipalities that recorded positive commuting.

Sources: Commuter information (numbers and distances) comes from Statistics Netherlands (own calculations), information on the dialect distances comes from Reeks Nederlandse Dialectatlassen (see, Heeringa, 2004), and information on regional characteristics comes from Statistics Netherlands, StatBel, and the National Bank of Belgium (Available on Cross-border Open Data (Statistics Netherlands (CBS))).

The administrative cross-border data of **Table 2.2** are complemented by regional data from Statistics Netherlands, StatBel, and the National Bank of Belgium. These institutes provide information on the population and average housing prices at the municipality level, as well as sectoral composition, gross product, gross investments, workers' compensation, age, and Dutch unemployment rate at the NUTS-3 level, as well as the Belgian unemployment rate at the NUTS-2 level. Additionally, the information is complemented by specific municipality geo-coordinates that are used to link the coordinates of each individual living in the origin and destination municipalities. The geo-coordinates are then matched to Eurostat geo-data to properly assign the origin and destination locations to their respective NUTS-2/3 regions and municipalities.

2.4.2 Dialect Distance Database

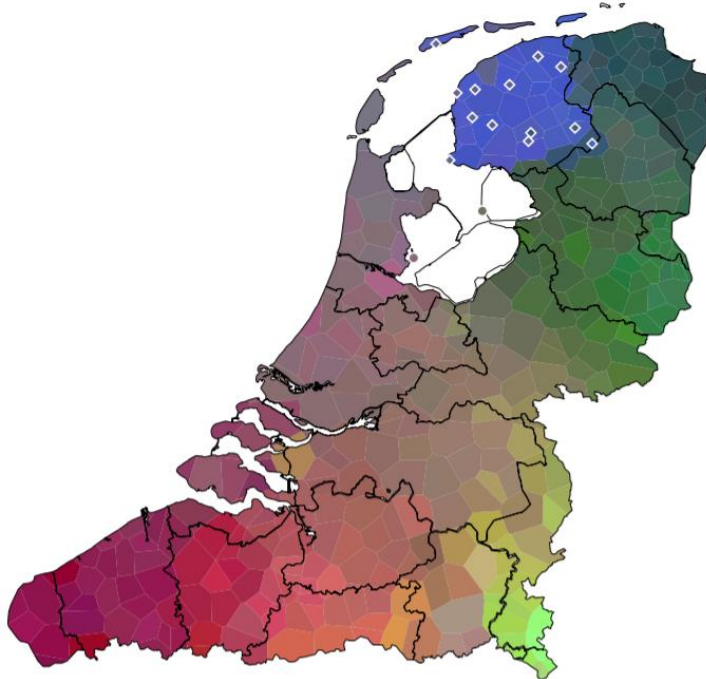
In order to fully understand how the cultural dissimilarity between municipalities affects mobility, we rely on the Reeks Nederlandse Dialectatlassen that was collected between 1922 and 1975, in which 361 dialects and 125 words were selected to calculate the dialect distance between regions, which is our approximation for cultural distance. The matrix construction relies on distances in the sound components measured with the Levenshtein distance. The construction and motivation behind the use of the Levenshtein distance are discussed by Heeringa (2004).¹⁴ **Figure 2.2** depicts dialect distances based on a multidimensional scaling technique and offers a visualization of these differences between regions.¹⁵ Based on the data, which is visually presented in **Figure 2.2**, we calculated the shortest distance from the

¹⁴ We are thankful to Wilbert Heeringa who provided us with the dialect distance matrices as well as **Figure 2.2** and **Figure 2.A.2** shows the distribution of cultural dissimilarity distances between pairs of Belgium and Dutch municipalities based on Heeringa (2004).

¹⁵ For a better understanding of how the multidimensional scaling technique works, see **Figure 2.A.2** in the Appendix.

dialect centroid within each country's border to the centroid of the respective municipality in the same country.¹⁶

Figure 2.2: Dialect Differences



Notes: Geographic coordinates enable the determination of distances between locations. Conversely, known distances can be used to establish an optimal coordinate system containing the locations' coordinates. This process is achieved through multidimensional scaling (MDS) technique. In an MDS plot, closely related dialects are positioned near each other, while distinctly different dialects are placed farther apart. The variation seen in Figure 2 is represented by color using multidimensional scaling (MDS) in which each variety is represented by a color. That is, each dialect point gets a color according to its MDS values. The first MDS dimension is mapped to red, the second inversely to blue, and the third inversely to green. This approach is used to create a map in which each variety gets its own unique color. The regions depicted in the graph correspond to the NUTS 2 regions in Netherlands and Belgium. Source: Heeringa (2004).

¹⁶ Dialect boundaries are diffuse, and a municipality may be exposed to multiple dialects. To assign a single value, we calculated the shortest distance to the centroid of each dialect region, giving each municipality the score of its closest dialect.

As previously discussed, culture is defined as the intergenerational transmission of values and beliefs that hardly change over time. Cultural differences are associated with a systematic distinction in values and beliefs across socially different groups, though there are also geographically different groups, meaning that geographical distance¹⁷ may play a role in shaping different cultural traits. Hence, by studying commuting behavior, and unlike Falck et al. (2012) who explored domestic removals, this paper faces a problem with regard to individuals traveling only a short distance. Indeed, this might also imply that individuals are commuting to more culturally familiar places. This may be particularly true for Dutch people living in Belgium and commuting to the Netherlands. **Figure 2.2**, depicting a visualization of the cultural differences measured by the Levenshtein distance, shows that two geographically equally distant destination regions might be, to some extent, more (or less) culturally distant from the origin region. Thus, if individuals choose a destination where cultural differences are smaller, this would corroborate the idea that culture is a barrier to mobility.

We would like to add that the identification strategy partly relies on the fact that historical dialects do not impose communication barriers that would prevent migration today and thus only influence mobility through the current culture. However, one could argue that although using a historical proxy for current culture, in the past, culture was shaped by mobility, and historical mobility, in turn, affects current mobility. Mobility in the past was not as widespread as it is nowadays given the advances in transport infrastructure, as well as communication and information technologies. Nonetheless, it is important to rule out that the effect on current mobility from current culture is confounded with the effects of past mobility. Hence, as well as the controls mentioned in the previous section, we also control for the share of immigrants in the 1930s and 1970s—the beginning and end of the dialect data collection period—per municipality in the Netherlands. These data are retrieved from the

¹⁷ The correlation between dialectal difference measured with Levenshtein distance and geographical distance is 0.55.

Dutch census of 1930 and 1971.¹⁸ In the robustness check section, we further discuss the possible confounding effects of past mobility.

2.5 Results

2.5.1 Baseline Results

Table 2.3 depicts the estimated results of the gravity equation defined by Equation 5 by means of the Poisson pseudo-maximum-likelihood (PPML) method in order to deal with heteroscedasticity (Santos Silva and Tenreyro 2006) and the high amount of zero flows between municipalities, which represents 94.1% of the sample. The first column shows the most basic result when commuting flows are regressed on the natural logarithm of dialect differences, i.e., our measure of cultural dissimilarity. This specification results in the elasticity of commuting with respect to the cultural difference, which is -5.19 . Specifications 2, 3, and 4 subsequently introduce the regional controls previously discussed,¹⁹ as well as NUTS-2 fixed effects and NUTS-2-pair fixed effects; for instance, specification 2 includes regional controls, specification 3 includes NUTS-2 fixed effects and specification 4 introduces NUTS-2-pair fixed effects. Finally, specification 5 includes origin and destination municipality fixed effects.²⁰ The elasticity of commuting with respect to cultural dissimilarity drops significantly after the introduction of regional controls and fixed effects and, specifically, by the introduction of geographical distance,

¹⁸ Over time, municipalities have split into more than one or merged into one. Special care was taken to properly assign past shares of immigrants to current municipality boundaries.

¹⁹ Regional controls include the differences in population size, unemployment, gross product, gross investments and workers' compensation.

²⁰ Given the cross-sectional nature of data, once includes origin and destination municipality fixed effects are introduced, regional controls are dropped due to perfect collinearity.

although it remains negative and significant.²¹ The elasticities now vary between -0.50 and -0.98 .

From **Table 2.2**, the mean of the cultural difference measure between municipalities (i.e. the dialect distance) is 34.66 with a standard deviation of 5.5 (see **Figure 2.A.1** for the distribution). If we consider specification 3 as the baseline describing elasticity, then a one-standard-deviation increase in cultural difference between municipalities leads to a 11.59% decrease in the respective commuting flow ($5.5/34.66 \times -0.73$). Likewise, an increase of 71.65 km in geographical distance between municipalities, which is the standard deviation of the distance between municipalities at the mean of 165.65 km (**Table 2.1**), would lead to a 107% decrease in the commuting flow between two municipalities ($71.65/165.65 \times -2.49$). Hence, in this case, although the effect of cultural distance is only 10.76% of that of geographical distance, it is still a sizable effect given that geographical distance should indeed be the main hurdle for commuting due to the impracticability of traveling long distances daily. To better comprehend the size of the effect and put these into perspective, we compare this result with the baseline impact of cultural dissimilarity on gross internal migration flows in Falck et al. (2012). They found a decrease of 6% of internal migrants for an increase of one standard deviation in their cultural dissimilarity measure. Although our estimated elasticity of 11.59% is larger, possibly because it concerns cross-border commuting instead of internal migration, our baseline result is in line with this empirical estimation.

²¹ Throughout the thesis, significant, statistically significant, significance statistical significance will be used interchangeably

Table 2.3: Basic Regressions for out-commuters from Belgium to the Netherlands

VARIABLES	(1)	(2)	(3)	(4)	(5)
	C_{od}	C_{od}	C_{od}	C_{od}	C_{od}
$Ln(CD_{od})$	-5.19*** (0.23)	-0.98*** (0.16)	-0.73*** (0.17)	-0.67*** (0.17)	-0.50*** (0.12)
$Ln(Dist_{od})$		-2.68*** (0.05)	-2.49*** (0.07)	-2.50*** (0.08)	-2.13*** (0.05)
Constant	16.53*** (0.74)	2.65*** (0.50)	-6.80*** (0.75)	-6.70*** (1.51)	11.80*** (0.32)
Observations	133,860	133,860	133,860	132,228	114,219
Controls	NO	YES	YES	YES	NO
NUTS-2 fixed effects	NO	NO	YES	NO	NO
NUTS-2-pair fixed effects	NO	NO	NO	YES	NO
Origin and Destination Municipality fixed effects	NO	NO	NO	NO	YES
Sample	Full	Full	Full	Full	Full
Impact at Mean	-82.38%	-15.56%	-11.59%	-10.63%	-7.93%

Notes: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Column 1 is the basic specification including only the logarithms of the dialect distance between municipalities. Column 2 includes the logarithm of the linear geographical distance in km and regional controls while columns 3 and 4 include NUTS-2 fixed effects and NUTS-2-pair fixed effects, respectively. Impact at mean is calculated based on a one-standard-deviation increase in cultural difference when evaluated at the cultural difference mean

2.5.2 Robustness Checks

2.5.2.1 *Other Distance Measures*

One potential threat to this study is the way distance is measured, as the choice of distance metric can influence the interpretation of cross-border commuting flows. While Euclidean distance, used in the previous section, offers a straightforward measure of spatial separation, it may not fully capture the practical costs of commuting, such as travel time or road distance. Additionally, the potential non-linear relationship between distance and commuting flows necessitates a more flexible modeling approach to ensure robustness.

Table 2.4: Other Distance Measures & Regional Characteristics

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	C_{od}	C_{od}	C_{od}	C_{od}	C_{od}	C_{od}	C_{od}	C_{od}
$Ln(CD_{od})$	-0.50*** (0.14)	-1.03*** (0.16)	-1.09*** (0.17)	-0.78*** (0.17)	-0.65*** (0.18)	-0.74*** (0.17)	-0.69*** (0.17)	-0.64*** (0.18)
$Ln(Dist_{od})$	-0.46* (0.27)			-2.49*** (0.07)	-2.46*** (0.07)	-2.49*** (0.07)	-2.49*** (0.07)	-2.62*** (0.08)
$Ln(Dist_{od})^2$	-0.32*** (0.04)							
$Ln(Shortest Travel Dist_{od})$		-2.48*** (0.07)						
$Ln(Travel Time_{od})$			-3.18*** (0.10)					
Presence of Railway				0.23*** (0.07)				
Border					0.33*** (0.13)			
Sectoral Composition						-0.18 (0.35)		
Housing Price Difference							1.19*** (0.16)	1.30*** (0.21)
Constant	-10.60*** (0.93)	-4.39*** (0.72)	-14.73*** (0.76)	-6.27*** (0.78)	-7.39*** (0.76)	-6.74*** (0.75)	-7.08 (4.35)	-6.51 (5.75)
Observations	133,860	133,860	133,860	133,860	133,860	133,860	128,428	128,428
Controls	YES	YES	YES	YES	YES	YES	YES	YES
NUTS-2 fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Sample	Full	Full	Full	Full	Full	Full	Full	Dutch
Impact at the Mean	-7.94%	-16.35%	-17.30%	-12.38%	-10.32%	-11.75%	-10.95%	-10.16%

Notes: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Table 2.B.1 presents same estimations with NUTS-2 pair Fixed Effects (FE). Column 1 includes polynomial specifications of distance. Column 2 includes the shortest travel distance while Column 3 includes the time distance between municipalities. Column 4 includes control for the presence of railway stations in both the municipality of origin and destination. Columns 5 includes a dummy indicating that the region lies on a border region. Columns 6 includes the sectoral composition difference index. Column 7 includes a measure of housing price differences between destination and origin municipality. Columns 8 restrict the sample to Dutch national commuters and include a measure of housing price differences between origin and destination municipality. Impact at mean is calculated based on a one-standard-deviation increase in cultural difference when evaluated at the cultural difference mean

To address this, we extend the analysis by incorporating alternative measures of geographical distance. First, we include a polynomial specification of Euclidean distance (**Table 2.4**, column 1) to account for potential non-linear effects. Next, we incorporate the shortest travel distance (Column 2) and travel time (Column 3) between

municipalities to better reflect practical commuting costs. Finally, we control for the presence of railway stations in both the municipality of origin and destination (Columns 4). Across all specifications, our cultural dissimilarity variable remains consistently negative and significant, with its magnitude stable regardless of the distance measure or fixed effects used.²² For instance, the impact of cultural dissimilarity ranges from a 7.9% to 17.3% decrease in commuting flows when evaluated at the mean, depending on the specification. This robustness highlights the persistent role of cultural barriers in shaping cross-border commuting flows, independent of how distance is measured.

2.5.2.2 *Regional Characteristics*

This section addresses the robustness of the baseline results, specifically with respect to regional confounding factors that may still affect the results despite the inclusion of regional fixed effects. Column 5 of **Table 2.4** introduces a dummy variable that indicates if the NUTS-3 regions lie in the border region of countries. Border regions are naturally closer to each other and, as mentioned above, geographical distance plays a role in determining cultural similarity between municipalities. Hence, commuting flows between these municipalities could be higher because of being a border region rather than having cultural similarities. The results remain robust to adding the border control variable.

Specification 6 from **Table 2.4** introduces a sectoral composition difference index by considering six sectors²³ and their relative importance in the total employment of each NUTS-3 region. The absolute difference in sector composition is calculated for each origin and destination pair and by each sector, which are then summed up. The lower the value, the more similar the two municipalities are with respect to their sectoral

²² Please refer to Table 2.B.1 in the Appendix for the robustness estimations with NUTS-2-pair fixed effects instead.

²³ 1) Agriculture, forestry, and fishing; 2) manufacturing and other industries; 3) construction; 4) wholesale, retail, transportation, accommodation, and food services; 5) financial, business services, and real estate; 6) government and care, culture, recreation, and other services.

composition. The reason for including this variable is that one might be concerned that the more alike two municipalities are with respect to their social and economic activities, the more culturally similar they are and, therefore, the commuting flows would be driven solely by the effect of the former rather than the latter. The results are robust to adding this variable for sectoral employment similarity.²⁴

Another concern to our study is urbanity and the fact that geographical vicinity to big centers drive the results. We dealt with this by excluding big destination Dutch municipalities close to the border. The results are presented in **Table 2.B.2** in the Appendix, where Tilburg, Breda, Sittard-Geleen, Eindhoven and Maastricht are excluded, one at the time, from the estimations. The results remain robust.

2.5.2.3 *Housing Effects*

Another plausible explanation for cross-border commuting flows is that they are connected to the living costs. Although our framework does not explicitly include housing prices, this might still play a role in determining mobility decisions. Columns 7 of **Table 2.4** introduces controls for the average purchasing price of houses within each municipality on both sides of the border. As mentioned above, many cross-border workers are Dutch and it might be that the effect of housing prices is mainly driven by Dutch nationals enjoying lower living costs across the border, translating to small travel distances and more similarity between municipalities for geographical reasons. Hence, column 8 only consider the positive commuting flows if the majority of commuting individuals from the origin region are Dutch. The coefficients of cultural dissimilarity remain robust to the inclusion of these new controls and vary from -0.64 to -0.69 , translating into an impact at the mean of -10.16% to -10.95% given a one-standard-deviation increase.²⁵

²⁴ Please refer to Table 2.B.1 in the Appendix for the robustness estimations with NUTS-2-pair fixed effects instead.

²⁵ Please refer to Table 2.B.1 in the Appendix for the robustness estimations with NUTS-2-pair fixed effects instead.

2.5.2.4 *Cultural History & Network: Migration, Religion and Network*

As previously discussed, endogeneity due to reverse causality is one of the main issues when regressing commuting flows on cultural differences, which motivates the use of a historical cultural variable. Nonetheless, there is still some concern that past migration shaped historical cultural traits and, thus, cultural similarity due to network effects may influence the current flow of commuters.

Table 2.5 addresses this issue.²⁶ Column 1 includes controls on the proportion of Belgians that lived in each municipality of the Netherlands in 1930 and 1971. The coefficient of cultural distance remains in the same order of magnitude and significance, corroborating the idea that the effect of cultural distance on commuting flows does not confound with omitted variables. However, the network effect might be more pronounced for Belgians than Dutch. Hence, column 2 addresses this issue by considering only the flows from municipalities in which the majority of commuters are non-Dutch nationals.

Culture of regions in the Netherlands and Belgium might have been shaped by other historic reasons, such as religion. Column 3 of Table 2.5 deals with this by including a measure of religious difference between the municipalities. The results remain robust to the inclusion of this control. Finally, even though we have controlled for the proportion of Belgians living in Dutch municipalities in the past, another potential threat to our identification is that commuting flows nowadays are influenced by network of Belgians living in Dutch municipalities rather than cultural differences. Thus, we included the share of Belgians living in Dutch municipalities in 2017 in column 4. Again, the coefficients remain in the same order of magnitude and are fairly stable, corroborating that cultural distance plays a role in determining commuting flows across European municipalities and that the result is not driven by omitted variables.

²⁶ Please refer to Table 2.B.3 in the Appendix for the robustness estimations with NUTS-2-pair fixed effects instead.

Table 2.5: Basic Regressions with Belgians in the Past & Religious Composition & Networks

VARIABLES	(1) C_{od}	(2) C_{od}	(3) C_{od}	(4) C_{od}
$Ln(CD_{od})$	-0.63*** (0.17)	-0.74*** (0.18)	-0.70*** (0.17)	-0.69*** (0.17)
$Ln(Dist_{od})$	-2.45*** (0.07)	-2.23*** (0.08)	-2.47*** (0.07)	-2.41*** (0.08)
<i>Prop Belgian Pop in 1930</i>	-11.70* (6.98)	-1.98 (8.63)		
<i>Prop Belgian Pop in 1971</i>	14.66*** (2.33)	20.63*** (2.00)		
<i>Religious Difference</i>			-0.33*** (0.11)	
<i>Prop Belgian Pop in 2017</i>				8.56*** (0.96)
Constant	-13.62*** (1.24)	-16.03*** (1.25)	-11.52*** (1.05)	-8.19*** (0.77)
Observations	130,755	130,410	133,515	133,860
Controls	YES	YES	YES	YES
NUTS-2 fixed effects	YES	YES	YES	YES
Sample	Full	Non-Dutch	Full	Full
Impact at the Mean	-10.00%	-11.74%	-11.11%	-10.95%

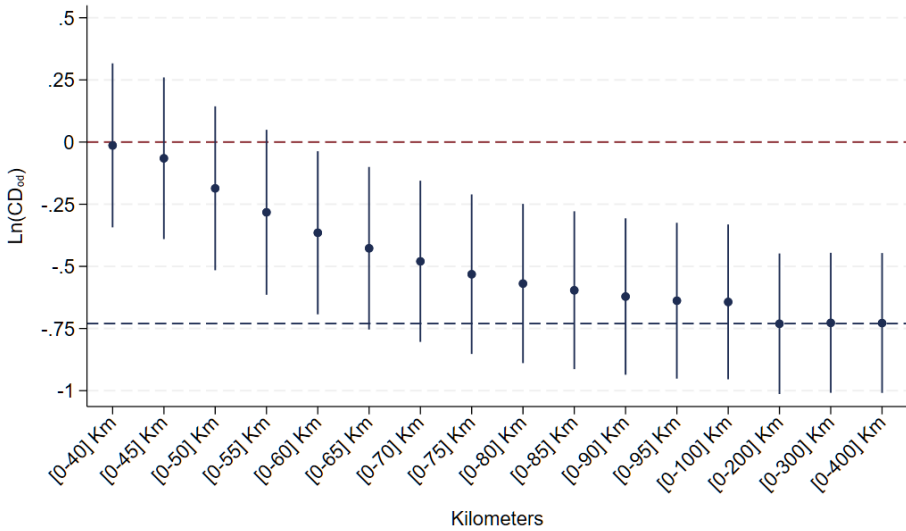
Notes: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Table 2.B.3 presents same estimations with NUTS-2 pair Fixed Effects (FE). Column 1 includes a measure of the Belgium population living in the municipality of destination in 1930 and 1971. Column 2 restricts the sample to non-Dutch commuters and include a measure of the Belgium population living in the municipality of destination in 1930 and 1971. Column 3 includes a measure of religious difference between the municipalities. Column 4 includes the share of Belgians living in Dutch municipalities in 2017. Impact at mean is calculated based on a one-standard-deviation increase in cultural difference when evaluated at the cultural difference mean

2.6 Heterogeneity Analysis

In this section, we extend our analysis to better understand the interplay between geographical distance and cultural dissimilarity in shaping cross-border commuting flows. Specifically, we investigate whether there are threshold effects of cultural dissimilarity when looking at short geographical distances. **Figure 2.3** plots the $Ln(CD_{od})$ coefficients

resulting from estimating the regressions based on Equation 5 (see the baseline result in Column 3 of **Table 2.3**) that systematically adjust the sample by excluding municipalities beyond specific bins of 5 kilometers of distance.²⁷ This approach allows us to assess how the importance of cultural dissimilarity evolves across different commuting distances.

Figure 2.3: Cultural Dissimilarity Effect on Commuting Flows at Different Distance Thresholds



Notes: Figure 2.3 estimates the effect of cultural dissimilarity on commuting flows across municipalities by excluding all municipalities above bins of 5 kilometers of distance. Thus, as the distance threshold increases, the sample size of regressions also increases. The x-axis displays the distance range between origin and destination municipality pairs that were considered in the estimation. The Figure plots the coefficients of cultural dissimilarity with a 90% confidence interval displayed.

The findings suggest that cultural dissimilarity remains a barrier to commuting flows, even at shorter distances. However, the results are not statistically significant in these cases because origin and destination

²⁷ We start restricting the sample at the 8th bin of 5 kilometers, or 40 kilometers, because below that threshold the sample of origin destination pairs of municipalities represents less than 1% of our total sample of 133,860. Moreover, the average cultural dissimilarity between origin and destination pairs of municipalities is three quarters of the full sample.

municipalities pairs tend to be more culturally similar and the sample size is considerably smaller. At very short distances, economic factors—such as lower commuting costs—become the dominant influence. As the commuting distance increases, particularly around 55 to 60 kilometers, the impact of cultural dissimilarity becomes more pronounced, compounding with the higher economic costs of longer commutes. This suggests that the combined burden of economic and cultural barriers increasingly curtails cross-border labor mobility over longer distances.

2.7 Mechanism

To explore the mechanisms driving the effect of cultural dissimilarity on commuting flows, we extend our analysis to include both intra-country commuting within the Netherlands and cross-border commuting from Belgium to the Netherlands. This broader approach allows us to disentangle the role of cultural dissimilarity in contexts with and without national borders, providing insights into the interplay between cultural and institutional barriers. Again we use data from Statistics Netherlands (CBS) on commuting flows between Dutch municipalities. It is important to note that this intra-country data includes bidirectional flows, whereas the cross-border sample only observes inflows from Belgium to the Netherlands. In order to mimic the data used so far, we perform the analysis on two different sets of Dutch municipalities. First, we examine all pairs of municipalities and designate a municipality 'a' as the origin if more residents of 'a' commute to municipality 'b' than vice versa. Consequently, municipality 'b', which receives more commuters from 'a', is considered the destination municipality relative to 'a'. Second, we looked at whether a municipality is a net receiver or a net sender with respect to all municipalities. Those that are net receivers are then considered to be the destination municipalities and the net senders the origin municipalities.

In Columns 1 and 2 of **Table 2.6**, we analyze commuting flows between Dutch municipalities, excluding cross-border movements and focusing on movements crossing “imaginary” administrative NUTS 3 boundaries within the Netherlands that have been shown to be also

functionally relevant (Cörvers et al. 2009). The coefficient for cultural dissimilarity is negative and significant, although it is smaller in absolute terms and more aligned to the internal migration behavior as presented in Falck et al. (2012). This suggests that within a shared institutional framework and national context, cultural dissimilarity may not act as a strong barrier to labor mobility as in cross-border settings. Column 2 expands the analysis to include all commuting flows, combining within-country commuting in the Netherlands with cross-border commuting from Belgium to the Netherlands. In Column 2, the effect of cultural dissimilarity remains negative and significant, consistent with the results for intra-country flows in Column 1.

Table 2.6: Intra-country & Cross-border Mobility

VARIABLES	(1) C_{od}	(2) C_{od}	(3) C_{od}
$Ln(CD_{od})$	-0.50*** (0.13)	-0.46*** (0.13)	-0.45*** (0.13)
$Ln(CD_{od}) \times$ Cross Border			-0.65*** (0.22)
$Ln(Dist_{od})$	-1.91*** (0.07)	-1.92*** (0.07)	-1.92*** (0.07)
Constant	14.38*** (0.51)	14.38*** (0.49)	14.33*** (0.50)
Observations	24,464	158,324	158,324
Controls	YES	YES	YES
NUTS-2 fixed effects	YES	YES	YES
Sample	Dutch NUTS 3	NL & BE NUTS 3	NL & BE NUTS 3
	Net Commuting	Net Commuting	Net Commuting
Impact at the Mean	-13.14%	-8.37%	-8.19%

Notes: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Table A4 presents same estimations with origin and destination municipalities defined relatively to the pair of municipalities. Column 1 analyzes commuting flows between Dutch municipalities, excluding cross-border movements. Column 2 includes all commuting flows, combining within-country movements in the Netherlands and cross-border flows from Belgium to the Netherlands. Column 3 introduces an interaction term between cultural dissimilarity and a dummy for cross-border movements to assess the incremental effect of crossing the Belgium-Netherlands border. Impact at mean is calculated based on a one-standard-deviation increase in cultural difference when evaluated at the cultural difference mean

To estimate the incremental impact of cultural distance, we include an interaction term between cultural dissimilarity and a cross-border dummy variable in Columns 3. The interaction term is negative and significant, while culture dissimilarity remains negative and significant, highlighting the compounding influence of crossing a national border as both cultural and institutional barriers. These findings underscore the mechanisms behind the observed patterns. Within the Netherlands, cultural dissimilarity plays a smaller role in deterring commuting flows, likely due to shared institutional structures and cultural integration. However, the presence of national borders magnifies the effect of cultural dissimilarity, adding layers of institutional and administrative barriers that exacerbate cultural differences and significantly reduce cross-border labor mobility.

2.8 Conclusions and Discussion

In this paper we contribute to the literature on cross-border commuting behavior by using historical dialect difference matrices to identify cultural dissimilarities between municipalities of Belgium and the Netherlands on a fine scale. As expected, the main driver explaining the distribution of commuters from Belgium to the Netherlands is geographical distance. Nevertheless, cultural dissimilarity also plays an important role in shaping the cross-border labor mobility behavior of workers. The effect of cultural dissimilarity is 5.19% of the size of the effect of geographical distance. This amounts to an 11.59% decrease in commuting flows from (Dutch-speaking) Belgian municipalities to Dutch municipalities when the distances in historical dialects between them, i.e. the measure of cultural differences, increase by one standard deviation. Our results are robust to including other variables as indicators of alternative explanations for the impact of cultural dissimilarity on cross-border commuting flows, such as border effects, sectoral dissimilarity, past mobility, long-distance commuters, and housing prices.

European integration is centered on the idea of the free movement of goods, services, and labor. Scholars and politicians once believed that removing institutional barriers would stimulate labor mobility across

European borders and, in the presence of regional disparities, generate efficiency gains. However, among the 220 million employed individuals in Europe aged 20–64, only 0.6% worked across a border in 2018, while 8.3% of the employed population commuted across NUTS-2 regions within a country (Eurostat, 2019). It is well known that European countries are culturally different to each other. To our knowledge, this paper is the first attempt to quantify the effects of cultural differences on commuting flows between two neighboring countries.

However, our study has some limitations. The first relates to the fact that the administrative data on international cross-border commuting only contains information about individuals living in Belgium and working in the Netherlands and not the other way around (which was less than half in 2018, Eurostat, 2019).²⁸ Although this is an important limitation, it does not discredit our first and important steps toward a better understanding of non-economic impediments to international cross-border commuting. Another limitation of our paper is related to our focus on the movements from the Dutch-speaking part of Belgium (i.e. Flanders and Brussels) to the Netherlands. Future research should also include other borders that are less culturally homogeneous across the sample.

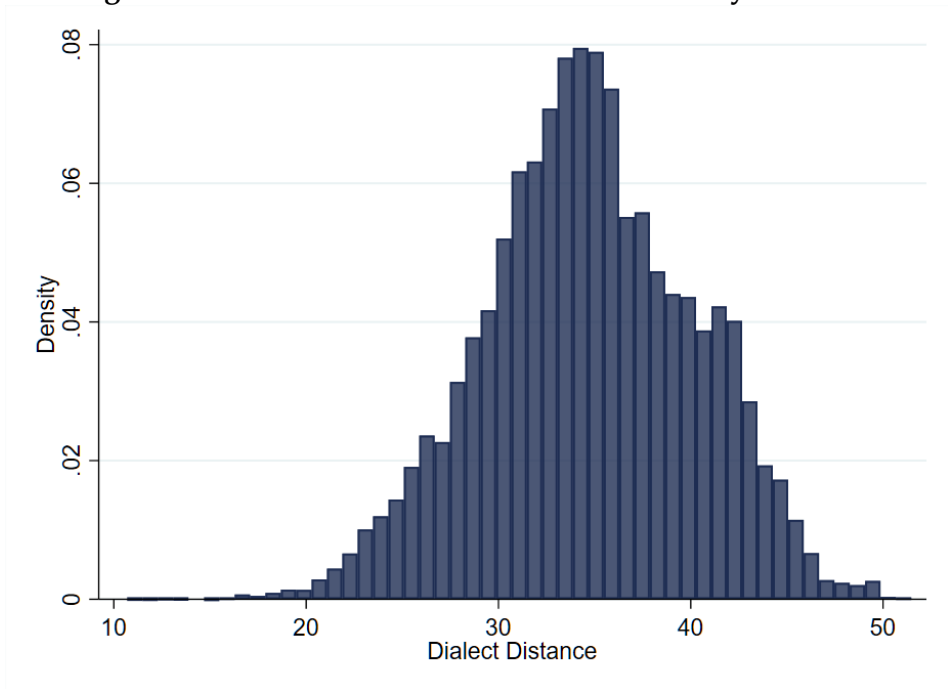
The results of our study suggest that cross-border labor mobility can be further enhanced by softening cultural borders between neighboring countries and municipalities. Policies aimed at fostering cross-border labor market integration should not only focus on improving economic and infrastructural conditions but also consider the role of cultural differences, which continue to matter even at short distances. For instance, EU bilateral Interreg projects aimed at learning each other's language and better understanding each other's cultures can soften cultural differences and promote cross-border commuting. This may have

²⁸ In 2019, a total of 34,865 individuals commuted from Belgium to the Netherlands, which is more than twice the number of individuals commuting in the opposite direction, 15,570 (Eurostat, 2020)

a positive impact on labor market matching, labor participation, unemployment, and regional economic growth in border municipalities.

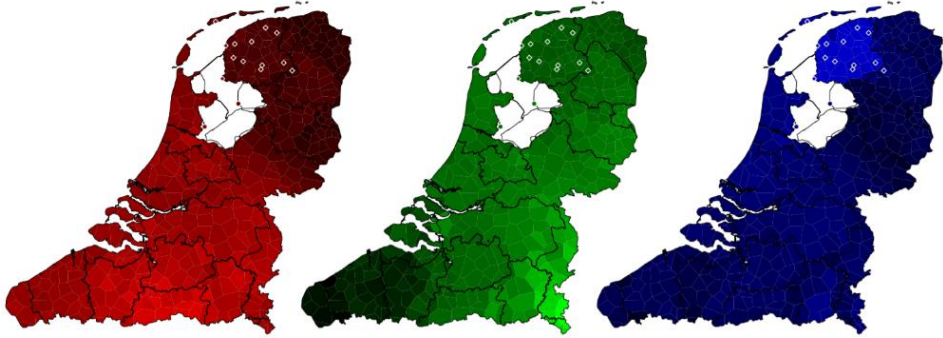
Appendix 2.A Additional Figures

Figure 2.A.1: Distribution of cultural dissimilarity distances



Source: Own calculations based on Heeringa (2004)

**Figure 2.A.2: Three color dimensions for the pronunciation
Levenshtein distances.**



Source: Heeringa (2004)

Notes: Red represents the x-axis, green represents the inverse z-axis, and blue represents the inverse y-axis.

Appendix 2.B Additional Tables

Table 2.B.1 Other Distance Measures & Regional Characteristics – NUTS 2 Pair FE

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	C_{od}	C_{od}	C_{od}	C_{od}	C_{od}	C_{od}	C_{od}	C_{od}
$\ln(CD_{od})$	-0.43*** (0.14)	-0.94*** (0.16)	-1.00*** (0.17)	-0.72*** (0.17)	-1.19*** (0.19)	-1.43*** (0.18)	-0.64*** (0.17)	-0.61*** (0.18)
$\ln(Dist_{od})$				-2.51*** (0.07)	-2.55*** (0.06)	-2.57*** (0.07)	-2.50*** (0.08)	-2.64*** (0.08)
$\ln(Dist_{od})^2$								
$\ln(Shortest\ Travel\ Dist_{od})$		-2.46*** (0.08)						
$\ln(Travel\ Time_{od})$			-3.16*** (0.10)					
Presence of Railway Station				0.23*** (0.07)				
Border					0.60*** (0.12)			
Sectoral Composition Index						-1.60*** (0.36)		
Housing Price Difference							1.18*** (0.16)	1.29*** (0.21)
Constant	-10.98*** (1.61)	-3.52*** (0.97)	-17.20*** (1.24)	-6.11*** (1.52)	0.78 (0.62)	2.81*** (0.57)	-11.43** (4.51)	-6.56 (5.87)
Observations	132,228	132,228	132,228	132,228	133,860	133,860	126,820	123,424
Controls	YES	YES	YES	YES	YES	YES	YES	YES
NUTS-2-pair fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Sample	Full	Full	Full	Full	YES	YES	Full	Dutch
Impact at the Mean	-6.83%	-14.92%	-15.87%	-11.43%	-18.89%	-22.70%	-10.16%	-9.68%

Notes: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Table 2.4 presents same estimations with NUTS-2 Non-pair Fixed Effects (FE). Column 1 includes polynomial specifications of distance. Column 2 includes the shortest travel distance while Column 3 includes the time distance between municipalities. Column 4 includes control for the presence of railway stations in both the municipality of origin and destination. Columns 5 includes a dummy indicating that the region lies on a border region. Columns 6 includes the sectoral composition difference index. Column 7 includes a measure of housing price differences between destination and origin municipality. Columns 8 restrict the sample to Dutch national commuters and include a measure of housing price differences between origin and destination municipality. Impact at mean is calculated based on a one-standard-deviation increase in cultural difference when evaluated at the cultural difference mean

Table 2.B.2: Excluding Big destination Dutch municipalities

VARIABLES	C_{od} (1)	C_{od} (2)	C_{od} (3)	C_{od} (4)	C_{od} (5)
$Ln(CD_{od})$	-0.75*** (0.17)	-0.70*** (0.18)	-0.79*** (0.17)	-0.76*** (0.17)	-0.50*** (0.18)
$Ln(Dist_{od})$	-2.47*** (0.07)	-2.48*** (0.07)	-2.51*** (0.07)	-2.48*** (0.07)	-2.63*** (0.07)
Constant	-6.68*** (0.77)	-11.16*** (1.01)	-6.56*** (0.76)	-5.70*** (0.76)	-12.25*** (1.04)
Observations	133,515	133,515	133,515	133,515	133,515
Controls	YES	YES	YES	YES	YES
NUTS-2 fixed effects	YES	YES	YES	YES	YES
Sample (Excluding)	Tilburg	Breda	Sittard- Geleen	Eindhoven	Maastricht
Impact at the Mean	-11.88%	-11.06%	-12.54%	-12.06%	-7.92%

Notes: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Column 1 excludes Tilburg from the estimation, Column 2 excludes Breda, Column 3 excludes Sittard-Geleen, Column 4 excludes Eindhoven and Column 5 excludes Maastricht. Impact at mean is calculated based on a one-standard-deviation increase in cultural difference when evaluated at the cultural difference mean

Table 2.B.3: Belgians in the Past & Religious Composition & Networks – NUTS 2 Pair FE

VARIABLES	(1) C_{od}	(2) C_{od}	(3) C_{od}	(4) C_{od}
$Ln(CD_{od})$	-0.57*** (0.17)	-0.65*** (0.19)	-0.60*** (0.17)	-0.63*** (0.17)
$Ln(Dist_{od})$	-2.46*** (0.08)	-2.20*** (0.08)	-2.49*** (0.08)	-2.42*** (0.08)
<i>Prop Belgian Pop in 1930</i>	-10.78 (6.95)	-1.04 (8.46)		
<i>Prop Belgian Pop in 1971</i>	13.30*** (2.50)	19.84*** (2.08)		
<i>Religious Difference</i>			-0.60*** (0.14)	
<i>Prop Belgian Pop in 2017</i>				7.45*** (1.02)
Constant	-11.12*** (1.39)	-14.57*** (1.47)	-8.75*** (1.23)	-10.14*** (1.24)
Observations	128,914	127,169	131,883	132,228
Controls	YES	YES	YES	YES
NUTS-2-pair fixed effects	YES	YES	YES	YES
Sample	Full	Non-Dutch	Full	Full
Impact at the Mean	-9.05%	-10.31%	-9.52%	-10.00%

Notes: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Table 2.5 presents same estimations with NUTS-2 Non-pair Fixed Effects (FE). Column 1 includes a measure of the Belgium population living in the municipality of destination in 1930 and 1971. Column 2 restricts the sample to non-Dutch commuters and includes a measure of the Belgium population living in the municipality of destination in 1930 and 1971. Column 3 includes a measure of religious difference between the municipalities. Column 4 includes the share of Belgians living in Dutch municipalities in 2017. Impact at mean is calculated based on a one-standard-deviation increase in cultural difference when evaluated at the cultural difference mean

Table 2.B.4: Intra-country & Cross-border Mobility: Robustness

VARIABLES	(1) C_{od}	(2) C_{od}	(3) C_{od}
$Ln(CD_{od})$	-0.30*** (0.10)	-0.26** (0.10)	-0.25** (0.10)
$Ln(CD_{od})$ X Cross Border			-1.08*** (0.24)
$Ln(Dist_{od})$	-1.82*** (0.05)	-1.84*** (0.05)	-1.84*** (0.05)
Constant	12.50*** (0.38)	12.40*** (0.38)	12.37*** (0.39)
Observations	72,680	206,540	206,540
Controls	YES	YES	YES
NUTS-2 fixed effects	YES	YES	YES
Sample	Dutch NUTS 3 Maximum	NL & BE NUTS 3 Maximum	NL & BE NUTS 3 Maximum
Impact at the Mean	-7.99%	-5.40%	-5.20%

Notes: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Table 2.6 presents same estimations with net receivers defined relatively to all municipalities. Column 1 analyzes commuting flows between Dutch municipalities, excluding cross-border movements. Column 2 includes all commuting flows, combining within-country movements in the Netherlands and cross-border flows from Belgium to the Netherlands. Column 3 introduces an interaction term between cultural dissimilarity and a dummy for cross-border movements to assess the incremental effect of crossing the Belgium-Netherlands border. Impact at mean is calculated based on a one-standard-deviation increase in cultural difference when evaluated at the cultural difference mean

3

Tax-Induced Emigration: Who Flees High Taxes? Evidence from the Netherlands

This Chapter is based on a joint work with Olivier Marie, Frank Cörvers and Hans Schmeets. We are grateful to David R. Agrawal, Kyra Hanemaaijer, Wouter Leenders, Gabriella Massenz, Marco Musumeci, Simon Rabaté, Marjon Weerepas, Zihan Ye and Gabriel Zucman for taking the time to give detailed comments on an earlier version of the paper. We also wish to thank participants at multiple seminars, workshops, and conferences for their valuable suggestions.

Abstract

We study a policy change in the Netherlands that unexpectedly reduced the duration of preferential tax treatment for high-skilled migrants arriving from specific countries in certain years. Utilizing comprehensive administrative data, we document substantial emigration responses, primarily driven by the top 1% of earners. Highly mobile individuals in the 95-99th percentile also emigrate sooner, particularly to countries offering tax-breaks to attract skilled workers. This challenge the often-assumed notion of universally high international tax-induced emigration elasticities, showing these apply mostly to top earners. Increased tax intake from remaining workers offsets the loss from emigrating high earners, making the policy fiscally cost-neutral.

3.1 Introduction

Globalization has reduced the monetary and psychological costs associated with international labor mobility. To benefit from this trend, national governments have increasingly turned to preferential tax schemes to attract specific groups of foreign workers into their labor market. There is now solid evidence of strong international mobility elasticities when these schemes are available to top earners or scientists and inventors (see H. Kleven et al., (2020) for a review).¹ Much less is known about how such temporary tax breaks may affect out-migration decisions after their expiration, and, especially, how it generally applies to migrants down the income distribution. This is an important question as the fiscal burden entailed would be politically tangible if the policies would induce these individuals to stay on to pay the high-income tax rates they are usually subject to once the preferential treatment has expired. The difficulty in obtaining credible causal estimates of tax-induced out-migration stems from policies in many other countries and time-specific factors that are difficult to account for affecting individuals' staying decisions. This paper aims to shed light on this issue by using administrative data from the Netherlands to investigate the impact of a policy that radically changed the length of time that migrants were afforded preferential tax rates based on their country of previous residence and time already spent in the country.

In 2001, the Netherlands introduced a preferential tax scheme aimed at skilled immigrants, in line with many European countries. The four main goals of the "30% ruling" as it is commonly known and which

¹ Kleven, Landais, and Saez (2013) made one of the first attempts to try to understand tax-induced international mobility by studying the labor market of professional football players in 14 European countries. Akcigit, Baslandze, and Stantcheva (2016) shift the focus to scientists and inventors and estimate their mobility responses to changes in tax rates of eight OECD countries. Finally, H. J. Kleven et al., (2014) focus on only one country to provide evidence on how immigrants that belong to the top 1% of the income distribution react to the introduction of a preferential tax scheme in Denmark. Muñoz (2023), studying changes in top-earner tax rates across Europe, reports migration elasticities of above one for foreigners.

are described in detail in the next section, were to: 1) attract workers from abroad that have a specific skill that is scarce in the Netherlands; 2) make the Dutch settling climate attractive and competitive for companies; 3) decrease the administrative pressure for employers and employees; and 4) create incentives for employees to stay in the Netherlands (Dialogic, 2017). As reflected in the policy's name, eligible immigrants would have 30% of their gross income exempted from tax for ten years. The resulting fiscal strain of the scheme on Dutch taxpayers led to a substantial revision of the eligibility criteria and its assessment process, and the maximum duration of the tax exemption in 2012.

Controversially, a key new criterion for non-eligibility—having relocated from a place less than 150 km from the Dutch border—was applied retroactively to beneficiaries who arrived after 2007, but it was not applicable to those who had arrived in the country prior to 2007. Individuals who did not meet this distance and arrival threshold would unexpectedly lose their right to the 30% tax break after five years, while all others would keep it for ten years. In practice, this meant that individuals who began benefiting from the 30% ruling before December 31, 2006, retained eligibility for the full ten-year benefit, while those who arrived between January 1, 2007, and December 31, 2011, were subject to the new criteria at their five-year reassessment; notably, the distance requirement became binding for individuals arriving from Belgium, Luxembourg and parts of Germany and France.² Crucially, therefore, this policy change was credibly orthogonal to any other observed and unobserved factors that may have affected past individual location decisions; given the retroactive nature of the policy implementation.

To study the effects of this policy change we gained access to administrative data from the Dutch Tax Authority (*Belastingdienst*) about recipients of the scheme and matched it to population-wide administrative data from Statistics Netherlands (CBS) for the years 2002 to 2019. The pool of potential individuals affected by this reform was large, as in 2012 almost 33,000 employees were enrolled in the preferential

² Please refer to Section 2.1 for a in-depth discussion on the institutional background.

tax scheme. Their labor market incomes were, as expected given the scarcity skill criteria, higher than the average Dutch population, but they were comparable to the top 50% of the distribution. We can thus look at a much more diverse group of affected migrants than in most previous contexts, which will be highly relevant when we consider policy response heterogeneities. Two crucial pieces of information are available in the administrative data: the start date and end date for each individual's eligibility for the preferential tax scheme and each individual's country of residence before migration. These data enable us to identify certain groups as treated individuals. We focus on workers who arrived from Belgium or Luxembourg—the two countries whose emigrants were all excluded from the scheme by the new 150 km criterion³—to compare their return rate to those arriving from non-affected countries before or after 2007.

The analysis from our difference-in-differences approach yields very consistent findings: workers from those countries had strong out-migration responses to their loss of eligibility for the tax break, even after they had spent substantial time in the host country. The treated beneficiaries stay, on average, 5.3 fewer months or 8% less time working in the country. Looking at this using a different margin—the probability of treated individuals spending more than five years in the Netherlands (i.e., staying beyond the loss of the tax break)—there is a decrease of 13% for the average tax break recipient. We clearly show that there are no pre-trends in the duration in the pre-policy period, and that our findings are robust to a battery of tests that change the samples used to define control or treatment individuals, including using only nearby Nordic countries as the control group. Notably, we discover that the policy effect is driven entirely by the behavioral responses of those workers belonging to the top 5%, and especially the top 1% of the income distribution.⁴ We do not

³ Our main analysis excludes individuals from partially treated countries (Germany and France) due to data limitations that prevent us from precisely identifying their previous municipality of residence (see Subsection 2.2). However, we show that our results remain robust when including these individuals in the analysis (see Subsection 4.2).

⁴ Individuals are compared to the Dutch wage distribution of the year that their wage is computed. For more information on the variable construction, please see Appendix B.

observe any out-migration timing responses among affected workers earning below the 95th percentile. This offers a novel and complementary perspective to the prevailing evidence of large tax-induced migration elasticities among top earners across countries (e.g., Akcigit, Baslandze, and Stantcheva 2016; Kleven et al. 2014; Kleven, Landais, and Saez 2013).⁵ While international studies have focused on the ultra-rich, some work has examined the migration behavior of other economically relevant but not necessarily top-income groups.⁶

While closely related in context, our study focuses on a distinct margin relative to Timm, Giuliodori, and Muller (2025), who examine a later reform introducing a wage threshold that applied only to new arrivals under the 30% ruling. Their analysis documents *in-migration* responses to a new positive incentive among mid-level earners just above the threshold. In contrast, we study *out-migration* following the retroactive reduction in duration of the earlier scheme, covering the entire affected income distribution among existing residents. This contrast is important: whereas Timm et al. capture entry decisions in response to enhanced incentives, we examine staying decisions following a negative policy shock among migrants who have already incurred substantial settlement costs. Comparing these two margins helps reveal whether preferential tax schemes primarily shape initial selection or long-term retention. We later draw on their findings in our cost–benefit analysis, combining inflow and outflow responses to assess the scheme’s overall fiscal implications.

⁵ Much more is known about within-country tax mobility. Agrawal and Foremny (2019) and Moretti and Wilson (2017) found strong mobility elasticities in response to tax changes affecting the top 1% of earners in Spain and the top 5% of U.S. inventors, respectively. Using Switzerland as a “laboratory” for tax-induced migration due to unrestricted movement between municipalities, Baselgia and Martínez (2024) documented sharp responses among the super-rich. Martínez (2022) and Schmidheiny and Slotwinski (2018) more generally show that top earners are highly mobile, while Köthenbürger et al. (2023) report very large migration elasticities for a specific migrant group.

⁶ For example, Akcigit et al. (2021) show that inventors are highly mobile within the US; Liebig, Puhani, and Sousa-Poza (2006) find that college graduates and foreigners relocate within Switzerland; and Rubolino and Giommoni (2023) document income-gradient responses within Italy, with greater responsiveness at the top.

We further show that the mobility response is not very sensitive to having created family roots (marrying or having children) while in the Netherlands, but those who have arrived from a country that is not their country of origin/nationality respond more strongly to the reform. These “highly mobile” individuals explain the drop in duration observed among those just below the top 1% of the income distribution. We consider three potential confounders—working from abroad, changes in capital wealth taxation, and wage compensation—and demonstrate why they cannot be behind the policy effect we uncover. Considering policy implications, we estimate the elasticity of out-migration responses to the average tax rate for the top 1% of earners of between 1.48 and 1.74 in our context, that are not shared with individuals right below in the income distribution, despite their treatment intensity in terms of tax rate change being very similar. These are in line with the literature on the elasticity of in-migration decisions, but they are surprisingly large perhaps given that these individuals would have already spent a significant amount of time in the host country. We document how the next location choice of leavers, and in particular that of highly mobile top earners, is influenced by ‘tax shopping’ consideration, indicating potential distortionary effects of international tax competition on the policy we study. Finally, we provide a simple cost–benefit analysis of the policy in terms of income tax receipts resulting from this reform and show that it was basically cost neutral.

The rest of the paper is organized as follows. Section 2 describes the institutional background, presenting the eligibility criteria for the Dutch preferential tax scheme and the changes implemented in 2012. It also presents the data and descriptive statistics. Section 3 explains our empirical specification and presents some graphical evidence of the policy effect. Section 4 reviews the results from our empirical analysis and their robustness. Section 5 is devoted to our heterogeneity analysis. Section 6 discusses potential confounders. Section 7 considers the policy implications of our results, and Section 8 concludes.

3.2 Institutional Background and Data

3.2.1 Institutional Background

3.2.1.1 *The Dutch 30% ruling preferential tax scheme*

It is now common for governments competing for foreign talent to offer temporary but substantial tax breaks to attract high-skill migrants and enjoy the positive externalities they can generate, as shown in the map of Europe reported in **Figure 3.A.1**. The design of such policies first aims to incentivize highly productive individuals to choose the country as their immigration destination of choice. A second important objective is to try to retain these high-earning individuals beyond the temporary discount period and, in doing so, collect the (usually) high levels of income taxes these workers would face thereafter. From an individual perspective, potential migrants base their location decisions on maximizing their income given their moving costs. Tax breaks that last for many years make a country relatively more attractive by raising an individual's potential net earnings. Longer duration tax breaks should increase workers' future moving costs: as migrants create deeper roots in the chosen country, they have a better chance of settling in the country permanently.

The Netherlands introduced a tax break scheme in the mid-1960s, making it the country with what is probably the oldest and longest running tax break for immigrant workers in the world.⁷ Until the 2012 change we study, the 2001 iteration of the law had become widely known as the 30% ruling⁸ The stated aim of the policy is to attract individuals with skills considered in short supply in the Dutch labor market by offering a tax break to compensate for the extra expenses the incoming employees

⁷ Article 31a, paragraph 2, part e, of the Wage Tax Act 1964 (Ministerie van Binnenlandse Zaken en Koninkrijksrelaties (1964). Subsequently, the arrangement was further elaborated in 11 articles of the Implementing Decree Wage Tax 1965 (Ministerie van Binnenlandse Zaken en Koninkrijksrelaties 1965).

⁸ The 30% ruling was actually the result of a merger of two older schemes (the "35% ruling" and Nedeco), which were in place in the Netherlands since the mid-1980s. See Weerepas, M. et al. (2013) for more information on the older schemes

might incur as they settle in the Netherlands. Once granted, 30% of the employee's gross labor income is free of tax for up to ten years.⁹

To be eligible for the 30% ruling before 2012, the incoming employee needed to fulfill certain requirements. First, the employee had to be hired from abroad or assigned to work in the Netherlands, meaning that the scheme is a special regime for expatriates.¹⁰ Second, the incoming employee must be employed by a Dutch resident employer or a foreign employer who is a wage tax withholding agent in the Netherlands. Third, the employee must have specific skills not readily available in the Dutch labor market. This scarcity requirement is verified against a specific expertise test. In practice, this last criterion was quite loosely defined, as it only required that an employee's occupation was one of seven broad categories, including one labeled as "other" to accommodate individuals on a case-by-case basis.¹¹

If the employee met these requirements, the employee would be granted a tax break for a maximum of ten years. The employee's job would be reevaluated to make sure it still met the scarcity requirement five years after the start of the tax benefit. Thus, in theory, the ten-year duration was divided into two five-year intervals. In practice, meeting the Dutch labor market specific skills requirement criterion was almost never an issue as almost all applicants automatically received the duration

⁹ There were two adjustments in the duration period. The first one, in 2012, decreased the maximum duration of the benefit to eight years; the second one, in 2019, decreased by another three years to five years.

¹⁰ This definition does not restrict eligibility on nationality but on place of residence before starting work in the Netherlands, meaning that hired employees did not have to be non-Dutch to be eligible. However, any work period in the Netherlands in the ten years pre-application would be deducted from the potential maximum duration of the tax break.

¹¹ These seven broad categories were: top managers, scientists, product specialists, teachers at international schools, employees of international institutions, and senior and middle management on mandatory company rotation, and an undefined final category where all applicants from other occupations considered high skilled could be accommodated. Author's own translation from the Dutch of the occupational categories described in Algemene Rekenkamer (2016).

extension if they had stayed up to that point.¹²

3.2.1.2 *The 2012 changes in eligibility criteria*

In the decade following its introduction, calls for the 30% ruling to be reformed began as it had grown in size and nature beyond its initial scope. The number of yearly applications had grown by 50% (from 8,000 to 12,000) between 2002 and 2009; the government admitted that “the scheme is applied more widely than was intended when it was introduced. In particular, the interpretation of the criterion that the employee must possess a ‘specific expertise’ is too broad in practice, partly due to jurisprudence on this subject.” (Kamerstuk, 2011d).

To solve this first issue, the government proposed replacing the expertise criterion with a wage threshold¹³ that would arguably more naturally capture areas that had a skills shortage in the Dutch labor market. Another contentious point was the potential application of the tax break to Dutch nationals returning to work in the Netherlands— after deduction of periods within ten years spent working abroad—as it was viewed as unfair that this growing number of individuals would be paid any extraterritorial costs. To avoid almost any Dutch citizen being able to benefit from the 30% ruling, the government proposed extending the deduction period from ten to 25 years.

Finally there were, as the Secretary of State for Finance put it at the time, “signals from the border regions that the 30% ruling can lead to a vacancy at a Dutch employer giving preference to foreign employees over Dutch employees, solely because the employee who comes from abroad, can apply the 30% ruling and the Dutch employee cannot.” (Kamerstuk, 2011a). In response, the secretary proposed the relatively radical solution of imposing a new distance criterion, which made any individual who lived less than 150 km from the border of the Netherlands

¹² This is statistically confirmed in the data as there is no discontinuity in taxable wage levels around the five-year threshold for recipients of the 30% ruling who were not affected by the policy change, we study.

¹³ This amount was € 35,000 (which is € 50,000 gross) for individuals older than 30 years and € 26,605 (which is € 38,007 gross) for individuals younger than 30 years old

(as the crow flies) in the period before moving would no longer be eligible for the tax break. All these proposed changes were eventually implemented as amendments to the Wage Tax Act 1964 (Kamerstuk, 2011c) and became effective on January 1, 2012.¹⁴

3.2.1.3 *Transitional rule: eligibility dependent on arrival time*

A transitional rule was also implemented along with the changes to the 30% ruling that differentially affected recipients depending on their original application date. Making use of the already existing five-year re-application extension requirement, the government decreed that starting January 1, 2012, if an individual “no longer qualified under the applicable regulations (i.e. does not meet the salary criterion at the time of assessment and/or before entering employment lived in the aforementioned border region), will no longer be entitled to the 30% ruling.” (Kamerstuk, 2011d).

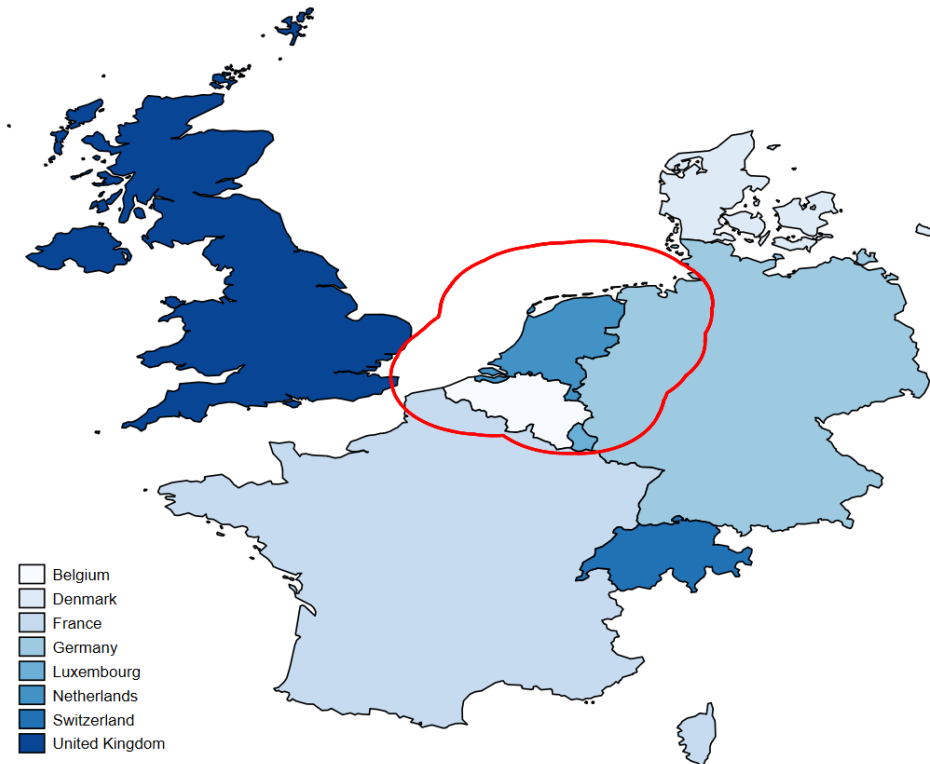
In practice this meant that all individuals who had started taking advantage of the 30% ruling before December 31, 2006, (i.e., those who had their five-year interim test scheduled *before* 2012) were entitled to ten years of tax breaks regardless of the new criteria. Individuals who arrived between January 1, 2007, and December 31, 2011, (i.e., those who had their five-year interim test scheduled *after* 2012) needed to meet both the new wage and distance criteria to obtain an extension of the tax break beyond five years. Since almost all claimants who reached this stage met the wage criterion—over 94% of them¹⁵—it is the distance threshold that really created an unexpected early loss of the tax break for those who had originally relocated from a place less than 150 km from the Dutch border. More specifically, as **Figure 3.1** illustrates, workers previously recruited from the whole of Belgium, the whole of Luxembourg, a big part of

¹⁴ The amendment was voted in parliament just 3 months before it was implemented which means there was very little scope for anticipation of the policy.

¹⁵ The value can be seen as the lower bound as we compute the average monthly wage for beneficiaries in their second year in the Netherlands, expressed in the equivalent of 2012 euros, and do not account for sectors that are exempted from the threshold (i.e., Research and Education). Breaking down by treatment status, the values are 94.88% and 94.62% for treated and control individuals, respectively.

western Germany, and a small part of northern France were no longer entitled to claim the benefits of the 30% ruling.

Figure 3.1: New Distance Tax Break Criterion: 150 km to the Dutch Border



Notes. The red line depicts the 150 km distance threshold from the closest point of the Dutch border. To be eligible for the 30% ruling after 2012, workers must be recruited from a place that lies outside the red delimitations. That is, incoming employees from Belgium, Luxembourg, a big part of western Germany, and a small part of northern France are no longer eligible for the Dutch preferential tax scheme. Note that the United Kingdom was not affected by the change, despite a small part of the southeast United Kingdom lying within the 150 km threshold.

To better understand the details of the 2012 law change and its transitional rule, **Figure 3.A.2** depicts the duration of the preferential scheme on a time-line diagram and divides the sample of beneficiaries into two different groups: 1) incoming employees who applied for the

scheme between 2002 and 2006, and 2) incoming employees who applied for the scheme between 2007 and 2011. We further split these groups into employees who met and those who did not meet the distance threshold criterion introduced in 2012. With this we illustrate the specific group of workers—those who arrived from 2007 to 2011 from specific locations—who were affected by the policy reform. These individuals will constitute the treated group that we use in a difference-in-differences approach to obtain causal estimates of the impact of drastically reducing the duration of the tax break on return migration decisions. All other claimants who were unaffected because they came earlier from the same regions or contemporaneously from other locations, will serve to control for other factors that are unrelated with the policy change that may have influenced worker movement out of the Dutch labor market. Before explaining in more detail our identification strategy, we first present the data used along with essential descriptive statistics of the workers covered by the 30% ruling tax break and its reform.

3.2.2 Data and Summary Statistics

3.2.2.1 Database: 30% ruling tax records and administrative data

We use data from the Dutch tax office (*Belastingdienst Nederland*) on the whole population of beneficiaries of the 30%-ruling from the period of 2002 to 2011. We have specific information on the start and end date of the individuals' tax break, the employer that made the request, and monthly taxable wage data. Through the analyzed period, 54,386 unique individuals¹⁶ used the preferential tax scheme.

We complement our database with administrative data from the Statistics Netherlands (CBS). The data allow us to identify specific immigration and emigration dates and a full set of background characteristics such as gender, age, municipality of residence, employment information, company sector, citizenship, and other household characteristics. We describe in detail each dataset used, the

¹⁶ There are 46,782 unique individuals if we do not consider individuals from the partially treated countries, Germany and France, which we will drop in our preferred specifications as explained below.

matching process, and the construction of all variables in Appendix 3.C. One limitation of our data is that we are only able to identify the previous country of residency and not the precise municipality of residence of migrants prior to their arrival in the Netherlands.¹⁷ As noted in **Figure 3.1**, Germany and France are partially affected by the 150 km distance threshold, but we are unable to precisely ascertain whether individuals fall into the treated category based on their residence. Consequently, as explained in the next section, we will focus on the policy response in terms of out-migration decisions of those who arrived from either Belgium or Luxembourg, excluding Germany and France from the main analysis.

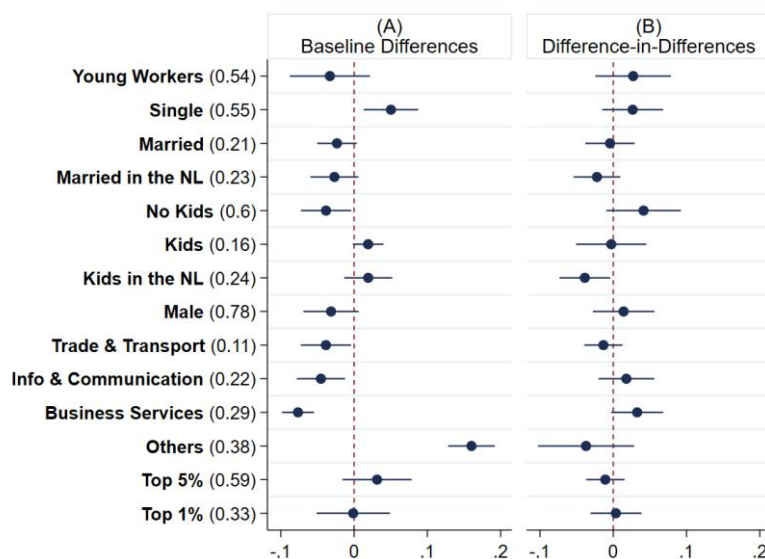
3.2.2.2 *Summary statistics on 30% ruling recipients*

As **Figure 3.A.3** illustrates, the country of origin of the tax break recipients is very international; for example, 18.4% come from either China, India, or Japan, and 16.3% from the United Kingdom or Ireland. Germany, France, and Belgium-Luxembourg account for 8.8%, 5.2%, and 2.9% of migrants respectively, meaning that 1,582 individuals are from the two countries that are entirely treated. As previously mentioned, Germany and France will be dropped from the analysis, meaning that in the final sample of analysis, Belgium and Luxembourg represent 3.4% of the individuals. **Figure A3.2** shows that recipients come to be employed in a wide variety of economic sectors but also that more than half work in either business services (28.5%) or trade, transport, and catering (20.6%). The map in **Figure 3.A.4** illustrates the ratio of beneficiaries per municipality population in the Netherlands. Only five municipalities have a ratio exceeding 1% beneficiaries per population, with a strong concentration in urban areas. Over a quarter of beneficiaries reside in and around Amsterdam, and this figure approaches 60% when including Amstelveen, Eindhoven, The Hague, and Rotterdam. This distribution aligns with expectations, as these large municipalities are home to companies seeking high-skilled workers. Notably, it also shows there is

¹⁷ The exact location of residence was requested during the registration for the tax exemption, but this information was not recorded in any administrative data, and the original forms containing it are no longer available.

no overconcentration of immigrants in the 'border regions'—an area of concern for some politicians advocating for the policy change.

Figure 3.2: Descriptive Characteristics of Recipients, by Treatment Status

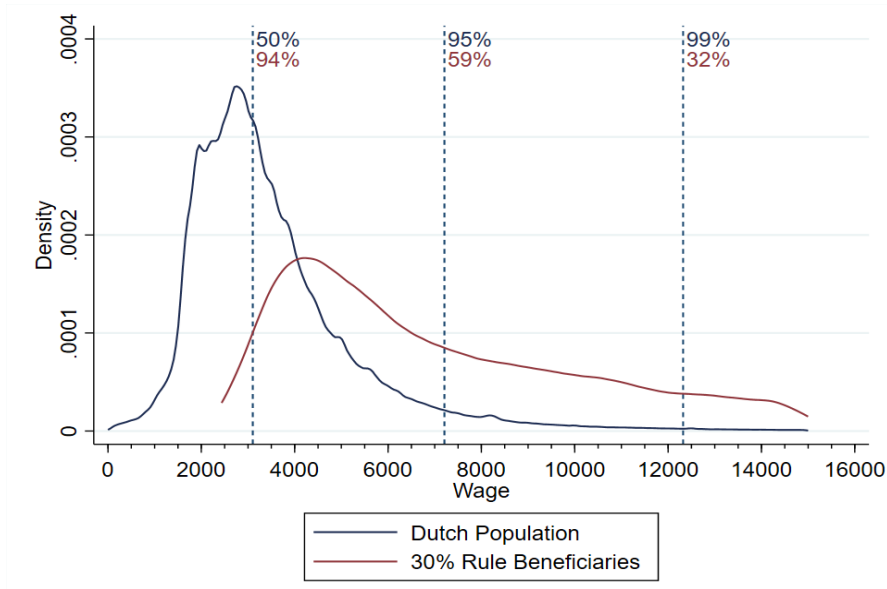


Notes. Panel A presents the summary statistics for the main characteristics of the beneficiaries along with the 95% confidence intervals. For each variable, we present the baseline average for the control years (values in parentheses) as well as the plotted difference in means between the control and treatment countries in the pre-policy period of 2002–2006. Panel B plots the difference-in-differences in means between the control and treatment groups, with no systematic overall differences between the two groups throughout the years.

Basic information on the origin, sector, and location of 30% ruling beneficiaries is relevant but our administrative data also enable us to describe them in terms of demographics, family structure, and earnings. Of interest for our identification strategy—the difference-in-differences presented formally in the next section—is how different these characteristics are between individuals from treated and nontreated countries and how this changes before and after the policy was introduced. We present all this information in **Figure 3.2**, which reports pre-policy averages of all recipients (in brackets), estimated baseline differences relative to those from Belgium-Luxembourg in the left part of the graph, and estimated differences pre and post policy between these

two groups in the right part of the graph.

Figure 3.3: Wage Distribution: Dutch Population and 30% Ruling Beneficiaries



Notes. For the Dutch population, we construct their earnings distribution using all Dutch earners in 2012, correcting for the full-time equivalent. For the beneficiaries, we compute their average monthly wage for each year that they were in the Netherlands, and we select the highest average monthly earnings out of their first two years with positive earnings. To construct their earnings distribution, we select all the beneficiaries from the control years (2002–2006) and express their pre-tax monthly wages in the equivalent of 2012 euros.

Figure 3.2 also reveals that tax break beneficiaries are mostly males (78%), are relatively young (54% are younger than age 35), and a majority are single (55%) and without children (60%). As expected, they are high earners, with three-fifth having wages that put them in the top 5% of the Dutch wage distribution and one-third having wages that put them in the top 1%. Recipients from treated countries are less likely to be

married and to work in very different sectors,¹⁸ but otherwise, they have similar high earnings profiles. Finally, when we examine how these differences across origin countries change over time, we find that almost none remain significant. This finding reassures us about the validity of our identification strategy, as it confirms there are no observable compositional changes among migrants in the treatment and control groups before and after the policy implementation.

An interesting feature of the setting we are studying is that the changes introduced in 2012 affected individuals at all levels of the earnings distribution, not only top earners. **Figure 3.3** depicts this distribution, along with that of all other wage earners in the Netherlands for 2012.¹⁹ As expected from a scheme aimed at attracting high-skilled migrants, the median pre-tax earnings of beneficiaries is high at €8.3k a month (in 2012 €), which was about 2.7 times more than the national median wage in 2012.²⁰ There are a lot of very high earners among 30% ruling recipients, with nearly 60% of them in the top 5% (monthly wage above €7.2k), and almost a third in the top 1% (monthly wage above €12.3k), of the income distribution of the Netherlands. While almost all tax break beneficiaries are above the national median, many of them are high—but not top—earners, with the mode of the distribution receiving net wages around €4.5k a month, and a few even receiving wages below

¹⁸ Others refer to Agriculture, forestry, and fishing (0.05%), Construction (0.52%), Industry and energy (13.73%), Financial sectors (8.24%), Renting and trading of real estate (0.2%), Government and care (10.58%) Culture, recreation, and other services (3%) and Unknown (1.2%).

¹⁹ Figure 3.A.5 plots the same distributions, further split by recipients from treated and non-treated countries, showing that they are relatively evenly distributed across all income groups.

²⁰ This is about twice the size of the income threshold of €50,000 a year (or €4,166 a month) implemented for eligibility in 2012. Only around 5% of workers had lower earnings when we measured their income, and over 45% of them worked in exempted sectors such as Research and Education, making the new eligibility threshold non-binding for most beneficiaries already in the country (see Timm et al., (2025) for a thorough analysis of the impact of these policy changes on migration inflows).

the 50th percentile of the Dutch distribution.²¹ This last observation will enable us to uniquely consider the migration response to reducing fiscal advantages for high-skilled migrant workers who have very different earnings profiles, not only for those at the very top of the distribution as in most of the previous literature.

Two final summary statistics we report in Figure 3.A.6 relate to the magnitude of the tax break in terms of net earnings, and especially of its loss. We first present, in Figure 3.A.6.1, the relationship between taxable and net wages, when the worker benefits or does not benefit from the 30% ruling. The graph also reports the mean tax rate corresponding to each wage level. The relative loss in earnings due to the 30% ruling is large at all wage levels, ranging from 16% for those below the 95th percentile of the distribution, to about 21% and 24% for those between the 95th and 99th percentiles and for the top 1%, respectively.²² We also show, in Figure 3.A.6.2, the earnings dynamics around the time when the 30% ruling benefit expires for each recipient in our sample who reaches the end of the tax break. When the tax break ends, we see that the average monthly net wage drops substantially from around €7k to €5k, which is a 28% decrease.²³ The loss in earnings from losing the benefit of the 30% ruling is thus very large, but slightly less than expected given the change in net wage it entails (Figure 3.A.6.1). This could be the result of some wage compensation from employers given to tax-break losers, something we explore later.

3.3 Identification Strategy and Graphical Evidence

3.3.1 Difference-in-Differences Approach

Our identification strategy exploits the 2012 policy change in the 30%

²¹ These represent about 6% of the sample who, for practical reasons, are excluded from the analysis. None of our results are altered by including these individuals.

²² A person with a taxable income of €10,000 per month would have approximately €7,000 and €5,700 net income with and without the 30% ruling, respectively.

²³ Note that the average taxable wage follows an inverse path when the 30% ruling expires as it increases from around €6.8k to almost €9k.

ruling as a quasi-experiment to causally evaluate the impact of losing preferential tax rate status on the out-migration decision of high-skilled migrants. The fact that it unexpectedly increased the tax rate after five years, rather than ten, for a specific group of individuals— depending on arrival time in the Netherlands and on country of previous residence— makes it ideal for a difference-in-differences approach. Estimates obtained will reflect the difference in migrants' decisions to leave the Netherlands pre and post policy (i.e., arrived before or after 2007) between those who originally relocated from a treated or control country (i.e., from a country closer or further than 150 km). Thus, the treated sample is composed of individuals who immigrated to the Netherlands from Belgium and Luxembourg, excluding individuals that previously lived in Germany and France from the analysis as we are unable to identify the precise previous location.²⁴

The difference-in-differences specification we estimate in equation (1) is its most basic form, where the main outcome of interest, Y , is either the length of stay in the Netherlands, measured in months²⁵, or a dummy variable that indicates whether the incoming employee stayed in the country for more than five years.

$$Y_{it} = \beta_1 \text{Treat Country}_i \times \text{Post 2012}_t + \gamma_t + \zeta_i + X_i' \Gamma + \varepsilon_{it} . \quad (1)$$

Subscripts i and t denote beneficiary i and start year t . *Treat* is a dummy variable equal to 1 if the previous place of residence of individual i , lies within 150 km of the Dutch border, and 0 otherwise. *Post* is a dummy variable equal to 1 if individual i started benefiting from the 30% ruling between 2007 and 2011, and 0 if they started benefitting from 2002 to 2006. Therefore, β_1 is our coefficient of interest that reflects the causal policy

²⁴ The robustness section will show that the results are not sensitive to the inclusion of these countries in the control group.

²⁵ We truncate the maximum length of stay to 108 months to account for the fact that beneficiaries that arrived at earlier cohorts naturally would have a higher length of stay and the fact that everyone loses the benefit after 10 years.

impact. The main effect of *Treat Country* (previous country of residence) is absorbed into the country-of-origin fixed effects ζ_i and the main effect of *Post 2012* is captured in start year fixed effects (γ_t). Finally, X'_i is a vector of individual level controls such as age, gender, marital status, presence of children and sector²⁶ and ε_{it} is the standard error, clustered at the level of treatment (country-of-origin by start year). We will estimate multiple versions of equation (1), most often augmented with country of origin and start year fixed effects that include all available individual controls from our administrative data.

Of paramount importance to the validity of our identification strategy is that the policy change was credibly orthogonal to any other observed and unobserved factors that may have affected past individual location decisions; given the retroactive nature of the policy implementation, that condition is likely met. This is also statistically confirmed by the exercise we carry out in Panel B of **Figure 3.2**, which shows that there were no significant differences in characteristics between treated and control individuals in the pre- and post-policy period (i.e., estimates of equation (1) using characteristics as outcomes). Also crucial to any difference-in-differences approach is that the pre-policy trends between the treatment and control groups are similar. In our case that would mean that the difference in time spent in the Netherlands between individuals from treated countries and control countries was evolving similarly for those arrived up to 2007. We check (and validate) for common pre-trends in Section 3.3., which graphically presents pre- and post-treatment dynamics.²⁷

3.3.2 Distribution of Out-Migration Timing

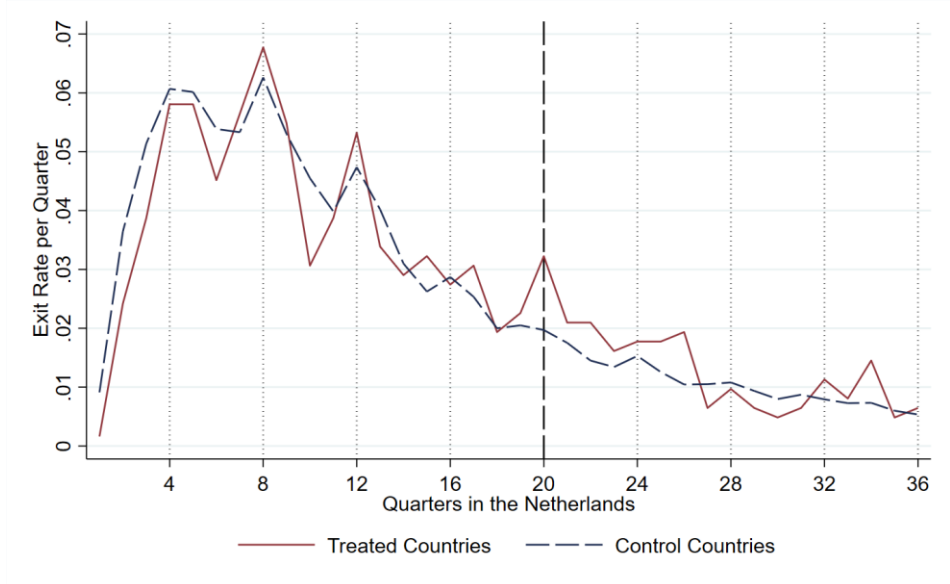
We begin our analysis of the policy effect by documenting the probability

²⁶ Given the dynamic nature of some of the controls, we opted to measure it at the same time as their income.

²⁷ Another key assumption for the validity of our difference-in-differences approach is that there were no contemporaneous tax policy shocks in Belgium and Luxembourg relative to other control countries. Using available data, we find no evidence of significant tax policy changes in either country during the study period that could plausibly drive our results.

of out-migration by duration, depending on treatment status. Our administrative data allow us to precisely identify immigration and emigration dates, which we use to construct exit rates by quarters spent in the Netherlands for those previously located in a treated or control country. **Figure 3.4** shows this distribution for the post-policy period (i.e., for those who arrived in the Netherlands between 2007 and 2011).

Figure 3.4: Exit Rate Dynamics: Post-Policy Period



Notes. The figure depicts the quarterly exit rate by treatment status. Treatment and control refer to the country of previous residence, meaning that the treatment group contains the individuals who left the Netherlands after immigrating from Belgium or Luxembourg while the control group contains individuals who immigrated from elsewhere. The series is constructed considering only individuals who arrived between 2007 and 2011. The series is delimited by quarter 36 because not all individuals who arrived between 2007 and 2011 would have reached more than 36 quarters in the Netherlands by the end of 2019. Moreover, all individuals from our control sample would lose the benefit after ten years in the country.

The figure first reveals that there are peaks of exits at 4, 8, and 12 quarters which are most likely linked due to full year contract durations that are negotiated by migrant workers. This bunching in the density of the distribution is very similar between 30% ruling beneficiaries from both treated and control countries. What is clearly different, however, is the much higher exit rate for treated expats—50% higher than for the

control group—which coincides with the fifth year of stay in the Netherlands when they lose their tax break. **Figure 3.A.7** presents the same figures for the pre-policy period, and while the distribution is somewhat noisier, it does not show any significant difference in exit probability around the twentieth quarter between arrivals from treatment and control countries during 2002 to 2006. To formally assess whether the sharp increase in exits observed in **Figure 3.4** reflects a true policy effect, we test whether the gap of 50% in the post-policy period differs significantly from the absence of such a gap in **Figure 3.A.7**. We begin by examining this dynamically, checking for pre-trends before the policy change.

3.3.3 Pre- and Post-Treatment Dynamics

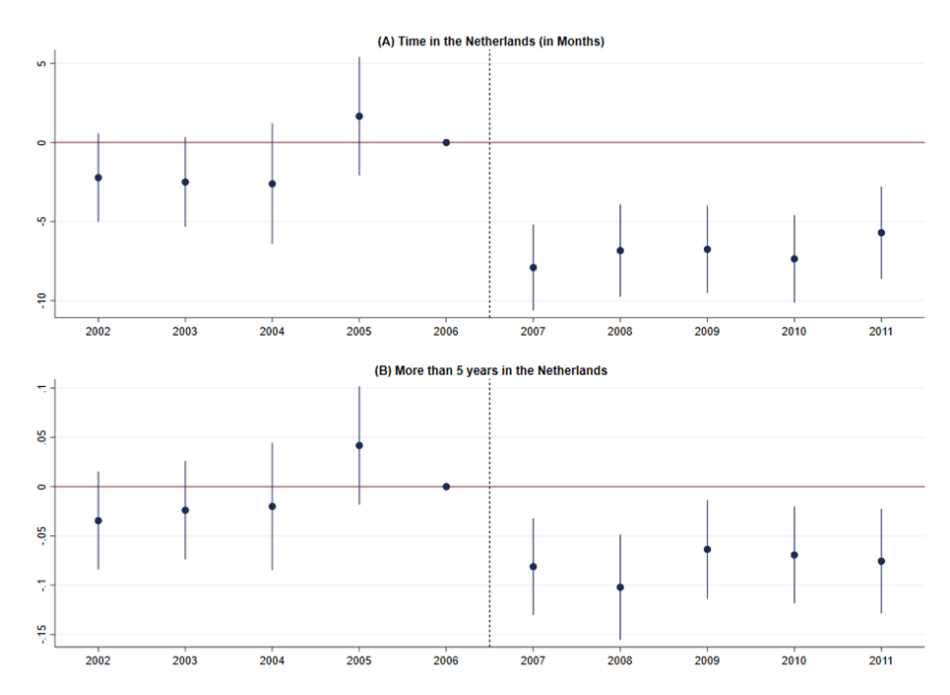
The main regression estimates we will rely on to evaluate the policy impact in this paper will stem from the difference-in-differences specification defined by equation (1). Before turning to these average effects, we inspect the dynamics of the treatment effect pre and post treatment. We do this by estimating separate coefficients $\hat{\gamma}$ for each start year t using equation (2), which is a dynamic version of equation (1) anchored around 2006, the year prior to the treatment start (i.e., akin to an event study difference-in-differences specification).

$$Y_{it} = \alpha + \text{Treat Country}_i \times \sum_{\substack{t=2002 \\ t \neq 2006}}^{2011} \gamma_t \mathbb{1}(\text{Start Year} = t) + \zeta_i + X_i' \Gamma + \varepsilon_{it}. \quad (2)$$

Figure 3.5 displays coefficients produced from equation (2) and their 95% confidence intervals with the outcome Y being either months in the Netherlands in Panel A or staying longer than five years in Panel B and the horizontal red line representing no difference in duration. What stands out is the visually striking contrast with all estimates pre policy being small and statistically nondifferentiable from zero and all post-policy estimates being strongly negative, relatively constant, and statistically significant. The first finding of a non-effect pre-policy is reassuring for the validity of our difference-in-differences approach as it

confirms that the common pre-trend assumption is not violated. The second finding of a negative and stable post-policy treatment effect suggests that the policy had a homogenous effect of significantly reducing time spent in the Netherlands, independent of time already spent in the country²⁸. This implies that the average effect generated by standard difference-in-differences specifications presented in the next section does not hide large heterogeneities of the out-migration response by the remaining tax break duration, which is a highly relevant policy finding.

Figure 3.5: Effect of the 30% Ruling Loss on Time Spent in the Netherlands



Notes. Depicts the event study based on equation 2. Panel A considers the first outcome of interest, time in the country measured in months, while Panel B considers the second outcome of interest, the probability of staying in the Netherlands for more than five years. The vertical dashed line between 2006 and 2007 split the sample between the cohorts that were affected by the 2012 change in the 30% ruling from the sample that was not affected.

²⁸ The effect size across cohorts is also similar in terms of policy response, as they all have relatively similar baselines in both outcomes.

3.4 Average Policy Impact Estimates and Robustness Checks

3.4.1 Average Policy Impact Estimates

Table 3.1 reports estimates of how the policy change affected the mobility behavior of highly skilled immigrants. Panels A and B show our outcomes of interest, the length of stay in the Netherlands measured in months and the probability of staying more than five years in the country, respectively. Column (1) presents the most basic specification stemming from equation (1); then we successively add control variables up to our full and preferred, specification in column (6). All regressions include country-of-origin and start year fixed effects. The coefficients of interest, the interaction between *Treat Country* and *Post 2012*, are always negative, statistically significant, and hardly vary across specifications. These results clearly show that individuals affected by the shortening of the preferential tax scheme from ten to five years spent less time working in the Netherlands. Our results from Panel A (months spent in the country) show that treated beneficiaries out-migrated on average 5.3 months earlier, representing an 8.1% decrease from a baseline of 65.5 months. Looking at our extensive margin measure in Panel B (the probability that individuals stayed beyond the end of the five-year preferential tax treatment), we reach the same conclusion. We estimate that the mean beneficiary affected by the policy change was 13% less likely to spend at least five years working in the Netherlands, a duration that is almost half the time that all high-skilled migrant workers spent before the duration of the tax break was shortened.

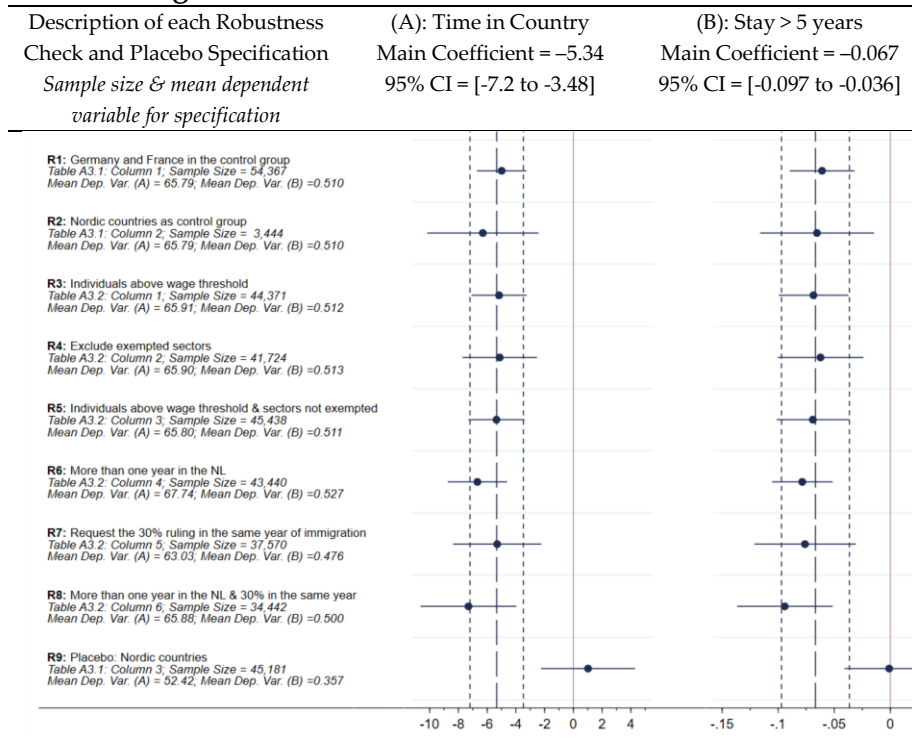
Table 3.1: Baseline Results: Time Spent in the Netherlands

Dependent Variable:	Time in Country					
	in Months (Panel A) and Over 5 Years (Panel B)					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Months in Country						
Treat Country * Post 2012	-5.78*** (0.88)	-5.38*** (0.87)	-5.38*** (0.87)	-5.36*** (0.91)	-5.34*** (0.91)	-5.34*** (0.93)
Mean of dependent variable	65.79	65.79	65.79	65.79	65.79	65.79
Impact at mean	-8.8%	-8.2%	-8.2%	-8.1%	-8.1%	-8.1%
Panel B: Probability > 5 Years						
Treat Country * Post 2012	-.071*** (.015)	-.067*** (.014)	-.067*** (.014)	-.067*** (.015)	-.067*** (.015)	-.067*** (.015)
Mean of dependent variable	.510	.510	.510	.510	.510	.510
Impact at mean	-13.9%	-13.1%	-13.1%	-13.1%	-13.1%	-13.1%
Country of Origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Start Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector	No	Yes	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	Yes	Yes
Gender	No	No	No	Yes	Yes	Yes
Married	No	No	No	No	Yes	Yes
Child	No	No	No	No	No	Yes
Observations	46,763	46,763	46,763	46,763	46,763	46,763

Notes. Panel A considers the first outcome of interest, time in country, while Panel B considers the second outcome of interest, the probability of staying in the Netherlands for more than five years. All regressions include country of origin and start year fixed effects. Clustered standard error at the level of treatment (country-of-origin by start year) in parentheses *** p<0.01, ** p<0.05, * p<0.1.

3.4.2 Robustness Checks

We carefully check the robustness of our main findings by changing how we define the treatment and control group and graphically plot the resulting coefficients and 95% confidence intervals in **Figure 3.6** (R1 to R8); **Tables A3.1 and A3.2** present the detailed estimates presented. The left graph (A) shows the length of stay in the Netherlands measured in months, and the right graph (B) shows the probability of staying in the Netherlands beyond five years. All coefficients in **Figure 3.6** are put in perspective with our preferred estimates from **Table 3.1**, column (6): a vertical blue line shows the coefficient, and dashed lines show the 95% confidence interval. Hence, when the estimated robustness check's coefficient overlaps with these lines, we can reject the hypothesis that it is significantly different from our main policy estimate.

Figure 3.6: Robustness Checks and Placebo Test

Note. The main coefficient is from our preferred specifications estimating the impact of losing the tax break on the time length in the Netherlands measured in months and on the dummy indicator of staying more than five years in the Netherlands as in column (6) of Table 3.1, Panels A and B, respectively. The long-dashed line is the coefficient estimate, dotted lines are lower and upper bounds of the confidence interval, and the solid line marks the zero effect. The coefficients for each regression are displayed as a blue circle with a line representing ± 2 standard errors. R1 includes Germany and France as control countries; R2 only takes Nordic countries as controls; R3 excludes individuals with wages below the threshold introduced in 2012 (5.1% of the sample); R4 excludes individuals who work in sectors exempted from the wage threshold (e.g., Research and Education, 10.8% of the sample); R5 restricts the sample on both R3 and R4 (97.16% of the sample); R6 uses only individuals who stayed more than one year in the Netherlands (92.9% of the sample); R7 uses only individuals who requested the 30% ruling in the same year as they immigrated to the Netherlands (80% of the sample); R8 restricts on both R6 and R7 (73.7% of the sample), R9 is a placebo test in the country, considering the Nordic countries as our treated sample and dropping individuals who immigrated from Belgium and Luxembourg.

Including French and German migrants, whose countries are partially treated, in the control group (R1) does not change the results. Using only individuals from Nordic countries as controls (R2)²⁹ – (the rich and nearby countries, so they are most similar to Belgium and

Luxembourg) makes the estimates somewhat larger, but more imprecise. Excluding individuals who have wages below the threshold introduced in 2012 (R3), workers from exempted sectors after the reform (R4), or both (R5), do not affect the findings. Finally, if we use only individuals who stayed at least 12 months in the Netherlands (R6), those who applied for the 30% ruling the same year as they immigrated to the Netherlands (R7), or both (R8), the estimates are again statistically equivalent to those in the main specification. These checks confirm that our findings do not depend on any specificities of the sample used to evaluate the policy.

In the last row of **Figure 3.6** (R9), we report results from running a placebo specification in which individuals arriving from Nordic countries are taken as the treated group and those from Belgium and Luxembourg are dropped from the sample. The estimates on both outcomes are very close and statistically indistinguishable from zero (the vertical red line).

3.5 Heterogeneity in Policy Response

Our baseline results quite clearly show that beneficiaries who had their tax break revoked after five years shortened their stay in the Netherlands. To get a more comprehensive picture of the policy effect, we analyze the heterogeneity in the out-migration response of subgroups of the population. We first focus on individuals with different earnings levels to check whether our results are driven by those belonging to the top of the distribution, which is the most studied group in the literature (see H. Kleven et al., (2020) for a review). We also explore whether behavioral responses are different for individuals who might be more mobile as they arrived in the Netherlands from a country that is not their country of origin/nationality. Lastly, we look to see whether creating family roots by marrying or having children while benefiting from the scheme influences the timing of the out-migration after the shortening of the tax break duration.

²⁹ The Nordic countries are Sweden (40.1%), Denmark (25.9%), Finland (19.4%), Norway (12.5%), and Iceland (2.1%).

3.5.1 Top Earners versus Other Beneficiaries

The literature on taxation and migration has mainly focused on the behavioral responses of top earners primarily because those individuals are the ones targeted by preferential schemes, thereby allowing researchers to study the causal effect of taxes on migration. Our setting provides a unique opportunity of studying the mobility response of a much broader population as the policy change applied to all levels of the distribution, with beneficiaries quite well represented at most level of earnings above the median (see Figure 3.3). We thus estimate the policy response in out-migration behavior separately for four subgroups of the earnings distribution: 50–95, 95–99, and 99–100.³⁰

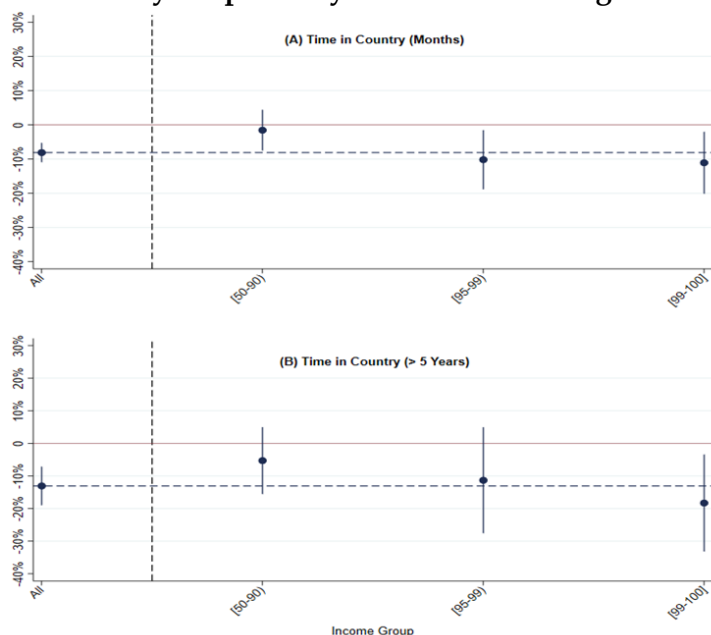
Figure 3.7 graphically presents the resulting coefficients in terms of policy effects (i.e., estimated impact relative to baseline) with details about each specification reported in **Table 3.B.5**. For comparison, our baseline mean policy impact estimates are shown and extended by the horizontal blue dashed line: at –8% for time in months in Panel (A) and at –13% for staying longer than five years in Panel (B). What this exercise reveals is that top earners are the most responsive individuals following a loss in their preferential tax status and that, surprisingly, there is no detectable change in the duration of stay for those below the 95th percentile of the distribution.

More strikingly, in terms of staying longer than five years, only the estimate for the top 1% of earners is statistically significant. These individuals are, post reform, 18% less likely to still be in the Netherlands after 30% tax break expires for them; this means they do not start paying the high tax rates they would pay if they stayed in the country. These are large effects that are very much in line with the rest of the literature on tax-induced migration responses of the very rich. What is a very new and

³⁰ We have also estimated coefficients for these subgroups using interactions of the difference-in-differences effects with dummy indicators of our earning percentile groups. Adding the main policy coefficient to the extra subgroup effect estimated gave us statistically similar estimates as when using this split-sample approach. Since we have enough statistical power in these split regressions, we decided to report these results instead of the triple interaction variant as they are much easier to interpret.

policy-relevant finding is that, below these high earnings thresholds, there seems to be very little mobility reaction among highly skilled migrants to the radical change in the preferential tax status. Given its importance, we will further explore the reasons, and consequences of this large difference in out-migration response of top earners.

Figure 3.7: Policy Response by Position in Earnings Distribution



Notes. The figure reports the policy impact evaluated at the mean \pm 2 standard errors based on equation (1), by position in the earnings distribution: 50–95, 95–99, and 99–100 percentile. Panel A shows time in country in months and Panel B shows the probability of staying more than five years. The first coefficient and the horizontal blue dashed line relate to the baseline results from Table 3.1. The horizontal dashed red line indicates zero effect. Table 3.B.5 presents detailed results. All regressions include age, gender, marital status, presence of children and sector controls.

3.5.2 Mobility Potential of Migrants

Migrant workers originally attracted by tax incentives to a country are not a random sample of the population. They are all, by definition, mobile, but some may be especially so and react more strongly to the loss of preferential tax treatment. In addition to the individuals' earnings level, which we just saw changes response behavior, we explore the potential impact of other migrant characteristics that make someone more or less likely to leave the Netherlands after the unexpected shortening by five

years of the 30% ruling. First, we consider the effect of having established stronger family roots, specifically, marrying or having a child in the Netherlands before the loss of the tax break. Either of these events should increase the cost of migration and could in turn reduce the response to treatment. Second, we consider how migrants who arrived in the Netherlands from a country that is not their country of origin/nationality may react differently from others. These individuals are probably “highly mobile,” as living in the Netherlands is not their first migration experience, and they may be especially sensitive to tax rates when choosing a location.

3.5.2.1 *Family roots: marriage and children*

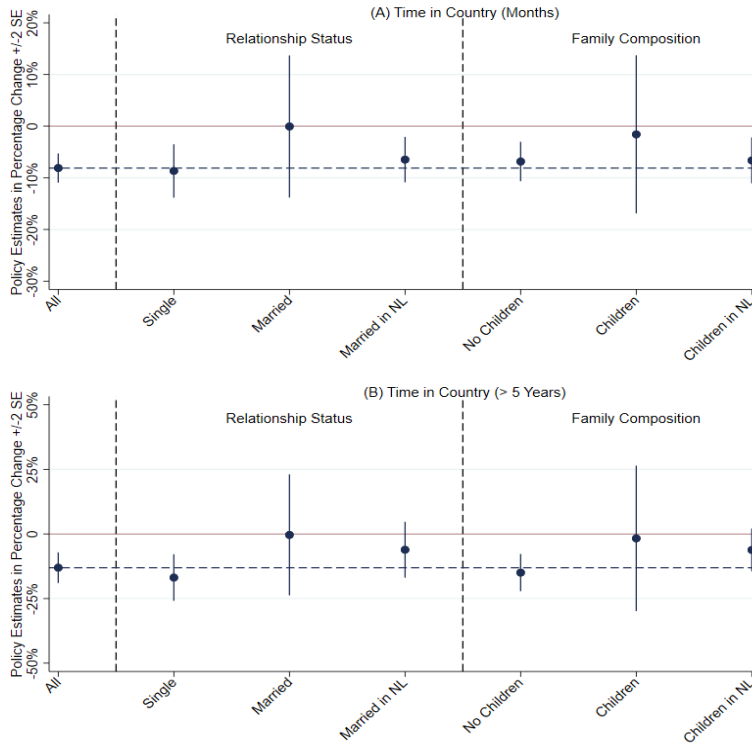
To explore the importance of having created family roots while in the Netherlands on the response to the new policy, we split the individuals in our sample depending on their marital status—single, married, or married in the Netherlands—and on the presence of a child in the household—no children, with children, and children born in the Netherlands.

We graphically present the coefficients in **Figure 3.8** in terms of percentage change; details are reported in **Table 3.B.6**. Despite very different baseline durations, single individuals (average stay 58.5 months) and those married in the Netherlands (average stay 85.9 months) react proportionally as strongly to losing the benefit, at about the mean level (Figure 3.8, Panel A). Those already married when they migrated do not change the timing of their out-migration at all.

A very similar picture emerges when looking at heterogeneity depending on the presence or non-presence of children in the household. Households without a child (average stay 55.1 months) and those who had a child in the Netherlands (average stay 85.6 months) react at the intensive margin, decreasing their length of stay in the Netherlands. Turning to stays longer than five years in Panel B, the only statistically significant estimates are for single individuals or individuals without children who reduce their probability of staying beyond their qualification for the 30% ruling by almost 15%. Those who married or who had children in the Netherlands are not affected; this may stem from

the fact that almost three-quarters of them already passed the 5 year threshold pre policy change. Family roots thus do seem to matter for mobility behavior, but perhaps not as much as one would have expected.

Figure 3.8: Policy Response by Family Roots Status



Notes. The figure reports the policy impact evaluated at the mean \pm 2 standard errors based on equation (1) by roots status, specifically, their marriage status and family composition. Panel A shows time in country in months and Panel B shows the probability of staying more than five years. The first coefficient and the horizontal blue dashed line relate to the baseline results from Table 3.1. The horizontal dashed red line indicates zero effect. Table 3.B.6 presents detailed results. All regressions include age, gender and sector controls.

3.5.2.2 Highly mobile: third-country migrants

Migrants who have arrived from a country that is not their country of origin/nationality may have low mobility costs and thus be especially reactive to changes in tax status when choosing their living location. We are uniquely able to study their policy response in our setting as a very

large share of individuals—41.6%— relocated from treated countries (Belgium and Luxembourg) to the Netherlands.³¹

Table 3.2 presents the result from a specification that adds an interaction of our difference-in-differences estimator with a dummy indicating that the individual has arrived from a country that is not their country of origin/nationality. This triple interaction will reveal any additional effects that being a potentially highly mobile individual may have on out-migration behavior post policy. We do this for the whole sample and then depending on level of earnings relative to the Dutch distribution. The main sample regression results reported in column (1) show that being highly mobile does not seem to be a source of sensitivity in either of our outcomes of interest. Running the same specification on distinct samples of the income distribution (i.e. 50–95, 95–99, 99–100) reveals that this average result hides strong response heterogeneities.

While being a highly mobile individual has no additional effect on the out-migration decision for the bottom 95% (no response) or top 1% (strong response), it is important for top earners between these two groups. These high-income, highly mobile migrants react much more strongly in terms of both intensive and extensive duration margins. They stay on average 13.6 fewer months, and 16.5% of them stay less than five years after the policy implementation which, when evaluated at the mean, translates into decreases of 18.1% and 27.4% respectively. Two important takeaways from these results are that highly mobile individuals react at lower thresholds of earnings than other workers to tax incentives, but that this only occurs at very high incomes; all those below the 95th percentile, on average, do not change their migration behavior after losing the 30% ruling.

³¹ Individuals who have arrived from a country that is not their country of origin/nationality represent 28% of individuals who migrated from control countries.

Table 3.2: Highly Mobile Individuals

Dependent Variable	Time in Country			
	in Months (Panel A) and Over 5 Years (Panel B)			
	(1)	(2)	(3)	(4)
<u>Panel A: Months in Country</u>				
	All	[50-95]	[95-99]	[99-100]
Treat Country * Post 2012	-5.38*** (1.60)	-3.77 (3.58)	-1.51 (4.05)	-7.98** (3.51)
Treat Country * Post 2012 * Highly Mobile	0.25 (4.10)	7.49 (7.47)	-13.69** (5.31)	1.86 (4.67)
Mean of Dependent Variable	63.00	59.84	75.26	56.59
Impact at Mean	0.4%	12.5%	-18.2%	3.3%
<u>Panel B: Probability > 5 Years</u>				
Treat Country * Post 2012	-0.071*** (0.018)	-0.074 (0.049)	0.003 (0.050)	-0.087** (0.035)
Treat Country * Post 2012 * Highly Mobile	0.010 (0.051)	0.125 (0.107)	-0.165*** (0.061)	-0.005 (0.062)
Mean of Dependent Variable	0.475	0.435	0.603	0.415
Impact at Mean	2.1%	28.7%	-27.4%	-1.2%
Controls	YES	YES	YES	YES
Country of Origin FE	YES	YES	YES	YES
Start Year FE	YES	YES	YES	YES
Observations	46,763	24,628	10,658	11,424

Notes: Panel A considers the first outcome of interest, time in country measured in months, while Panel B considers the second outcome of interest, the probability of staying in the Netherlands for more than five years. . All regressions include country of origin and start year fixed effects. Highly mobile individuals are defined as the individuals who are not nationals of or were not born in the previous country of residence. Clustered standard error at the level of treatment (country-of-origin by start year) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

3.6 Potential Confounders

Our results so far clearly point to a strong policy effect on the migration behavior of certain individuals but before making definite causal claims, we must first consider three potential cofounders that may bias our

results. First, the 30% ruling never required claimants to physically move to the Netherlands, and thus our out-migration measure may not be the appropriate outcome of interest. Second, the preferential tax scheme studied also extends to wealth accumulated in the Netherlands, which potentially threatens the interpretation of our findings, particularly the earnings heterogeneity interpretation. Finally, certain migrants may be better at negotiating wage compensation than others after the loss of the tax break, which could also affect our interpretation of heterogeneity in the responses.

3.6.1 Commuting, Not Migrating?

We have so far measured migration duration response in terms of time between a physical move in and out of the Netherlands for work. Commuting, or even working from abroad, does not preclude an individual from benefitting from the 30% ruling as long as she was recruited while living in another country.³² Since job and physical tax-induced mobility do not have to be aligned, something demonstrated by Agrawal and Hoyt (2018) for the United States, one might worry that our results so far may not reflect the true policy response in terms of employment (and tax payment) duration.

To check this, we first document, in **Figure 3.A.8**, changes in the number of beneficiaries who worked from abroad over our whole sample period, for treated and control countries. Apart from a downward trend for both groups, there is no clear difference throughout between the two groups, suggesting no differential policy response in the probability of working from abroad. These results are confirmed by the nonsignificant estimates presented in Table 3.B.7, Panel A, which runs our difference-in-differences specification using a dummy indicating whether an individual has ever commuted as an outcome. Panel B considers commuting duration and again returns small and nonsignificant estimates. There is also no heterogeneity on these two margins by income

³² About one in four individuals from treated countries commuted at some point pre-policy, and they do so for about four out of the average 18 quarters they spent working in the Netherlands (see baselines in Tables A7 and A8).

level, as indicated by the results presented in columns (2) to (4). Finally, we also show, in **Table 3.B.8** and **Table 3.B.9**, that our main and heterogeneity policy response results are unaffected by considering employment time instead of residence time in the Netherlands as the outcome of interest.³³

3.6.2 A Capital Tax Migration Response?

Individuals in the Netherlands with even low levels of net savings and investments are required to pay an effective tax rate of 1.2% on such capital. In addition to 30% of their wage being free of tax, beneficiaries of the Dutch high-skill migrant scheme are also exempt from this tax. It is important to note that, unlike a wealth tax which typically applies only at very high levels of wealth accumulation, the Dutch capital taxation is imposed at a low threshold (€21,139 in 2012), affecting a large proportion of those benefiting from the 30% Ruling. We therefore explore whether the out-migration responses we have observed could be (partially) driven by the loss of these capital tax breaks, rather than solely by the reduced income following the change in the duration of preferential treatment for high-skilled migrants.

To investigate this issue, we obtained administrative capital data from Statistics Netherlands and matched it to our sample of beneficiaries.³⁴ We use this information to generate new estimates that account for relative capital level to check whether this affects the nature of our findings. We do so by including in our specification an indicator as to which decile of the capital wealth distribution beneficiaries belong to and interacting this indicator with our main difference-in-differences estimator. The resulting triple interaction should capture the additional impact on timing of out-migration by capital level (and the taxes it

³³ Employment time is defined as the duration for which an individual spent working for a Dutch employer, in the Netherlands or abroad, something we can precisely estimate in the tax receipt data.

³⁴ One limitation of these data is that they are only available from 2006 to 2019, meaning that individuals who out-migrated from the Netherlands before this period are not covered, nor are individuals who stayed less than one year in the Netherlands. Detailed information on the wealth data construction can be found in Appendix B

implies). Of particular interest is how this breaks down across individuals from different earnings levels, as it is possible that our finding of a strong policy response only at the top of the wage distribution is influenced by underlying differences in capital wealth among the groups. **Table 3.B.10** presents the results from this analysis and first reports, in column (1), estimates of the baseline difference-in-differences coefficient for the subsample of beneficiaries on the availability of the capital wealth data. These are, in comparison to our main findings from **Table 3.1**, still negative and significant if somewhat larger for both the intensive and extensive duration margins. Adding the triple interaction with the capital decile, column (2), slightly reduces the size of the main coefficient but also reveals a positive and significant coefficient for our capital wealth measure. This positive correlation between duration and capital is probably endogenous—length of stay increases capital accumulation—so it should not be causally interpreted. More importantly, the triple interactions reported in column (2) are small and insignificant, suggesting that capital wealth and its taxation do not differentially affect out-migration in our setting. This is consistent with the relatively low correlation between income and capital wealth in our sample, implying that migration responses are primarily driven by earnings taxation rather than capital taxation. as shown in the final three columns of **Table 3.B.10**, this finding is driven by the change in the behavior of the top 1% of earners, independently of their capital level. We thus conclude that in our context, the effect we estimate is an earnings tax response, not a capital tax response.³⁵

3.6.3 Wage Compensation?

A final potential confounder relates to a possible difference in bargaining power: the more rent an individual can extract from her employer when losing her preferential tax status, the longer she is likely to stay. If this variation is large between individuals with higher or lower wages, this could explain the striking difference in out-migration responses we have

³⁵ This is in line with the modest emigration responses with respect to the net of wealth taxation that Advani, Burgherr, & Summers, (2022) find for the very rich in the United Kingdom.

found at various levels of the earnings distribution. To investigate this potential threat, we perform a simple exercise that compares the growth in taxable wages for individuals from treated and control countries, depending on their earnings level, around the time when their tax exemption status ended (four quarters after compared to four quarters before).³⁶ That is, after five years for treated individuals and ten years for all nontreated ones four quarters before).³⁷ **Table 3.B.11** presents the estimates of the change in log-wage by position in the earnings distribution—50–95, 95–99, and 99–100.³⁸ The coefficient of the dummy for quarters after the expiration of the tax break is almost exactly equivalent to the increase in taxable wages expected by the loss of the 30% ruling benefit for all groups. However, the difference in wage growth by treatment status is small and non-significant, especially for those in the top 5% and top 1% of the earnings distribution. This suggests that the strong out-migration response we uncovered for the very top earners is not driven by large differences in the bargaining power of these workers.

3.7 Policy Implications

3.7.1 Elasticity Estimates

The quasi-experimental institutional setting used in this paper allows us to uncover the individuals' intensive and extensive margin duration responses to a sudden and unexpected increase in tax rates. However, a key parameter to the design of optimal tax policy is the elasticity of

³⁶ We have shown that the policy has a strong effect on out-migration duration beyond the expiration of the tax break. Despite the important selection issues as to which individuals we can observe post-period wages for, we think that comparing across earnings groups is still be informative about underlying differences in bargaining power potential.

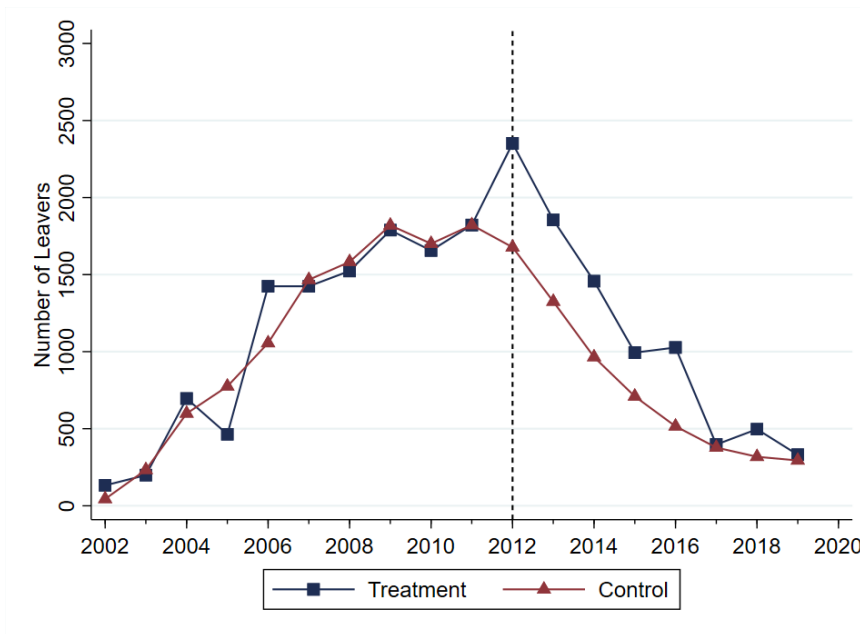
³⁷ Since we have already shown that the policy has a strong effect on out-migration duration beyond the expiration of the tax break, there are obviously important selection issues as to which individuals we can observe post-period wages for. However, we think that comparing across earnings groups can still be informative about underlying differences in bargaining power potential.

³⁸ Because our data end in 2019, we use the subsample of recipients who arrived up to 2008 in the Netherlands for this analysis to have at least one year of wage data for those who stayed beyond the expiration of the 30% ruling.

mobility responses with respect to the net tax rate.

We turn to estimating these in our context and start by graphically documenting, in **Figure 3.9**, the evolution of the number of 30% ruling recipients belonging to the top 5% of the earnings distribution in our sample who left the Netherlands between 2002 and 2019. The series is labeled as treatment and control, in reference to the country of previous residence and normalized in 2011, the pre-policy year. Up to that point, both series followed very similar paths, but after there is a clear increase in the number of leavers among treated recipients but not those in the control group. The difference persists until 2016, the year by which the tax exemption was over for all treated individuals in our sample.

Figure 3.9: Out-Migration of 30% Recipients, Treated vs. Control Countries



Notes. The figure reports the number of leavers by treatment status. Treatment and control refer solely to the country of previous residence, meaning that treatment group contains individuals who left the Netherlands after immigrating from Belgium or Luxembourg, while the control group contains individuals who immigrated from elsewhere. The treatment series is normalized to match the control series in 2011, and the vertical dashed line marks the year in which the change in the 30% ruling became effective. To construct these series we only considered individuals from the top 5% and excluded all individuals whose previous country of resident was Germany or France.

Next, to obtain elasticity of mobility estimates with respect to the net tax rate, we follow the literature and run the following two-stage least squares (2SLS) equation:

$$\ln(\text{Leavers}_{it}) = \alpha_0 + \beta_1 \text{Treat Country} + \beta_2 \text{Post 2012}_t \quad (3) \\ + e \ln(1 - \tau_{it}) + \varepsilon_{it} .$$

The units of analysis $i=\{1,0\}$ indicate that individuals belong to either treatment or control group, based on *Treat Country*, country of previous residence and t denotes year of emigration. Leavers_{it} is the number of leavers in each $t=\{2001,2019\}$, and τ_{it} is the average tax rate, for each group i in year t . We instrument our variable of interest, the net of tax rate $\ln(1 - \tau_{it})$, by interacting *Treat Country* \times *Post 2012*.

Table 3.3 column (1) presents the elasticity of mobility with respect to the net tax rate for the whole sample; columns (2) to (4) show the elasticity of mobility by subsamples with respect to the net tax rate based on individual's position in the earnings distribution. Long-run elasticities in Panel A are estimated using a specification that defines 2012–2019 as the post-policy period, thereby capturing longer-run emigration responses while avoiding estimates driven by very early exits. Short-run elasticities in Panel B restrict the post-policy period to 2012–2016 and focus on individuals who stayed five years or less in the Netherlands, ensuring that estimates are not influenced by departures occurring after the completion of the full statutory duration of the 30% ruling.³⁹ The mean long-run elasticity is -1.05 and significant, but, as in our duration results, it is mostly driven by the out-migration reaction of

³⁹ The long-run elasticities include all individuals from treated and control countries who emigrated between 2003 and 2019. The short-run elasticities restrict the sample to individuals who stayed five years or less in the Netherlands and emigrated between 2002 and 2016, the period during which the preferential tax treatment had expired for all treated recipients in our sample. This distinction allows the long-run estimates to abstract from very early exits, while the short-run estimates avoid behavior driven by departures following the completion of the full ten-year benefit period of individuals from the control years.

those at the top of the earnings distribution. The elasticity of mobility with respect to the net the tax rate is not significant for the bottom 95% of the income distribution, while for the 95–99 category and the top 1%, they are large and precisely estimated between -1.26 and -1.74 , respectively.⁴⁰

Table 3.3: Elasticity Estimates

Dependent Variable	$\ln(\text{Leavers})$			
	(1)	(2)	(3)	(4)
Panel A: Long Run				
	All	50-95	95-99	Top 1
$\ln(1 - \tau)$	-1.05** (0.47)	-0.78 (1.06)	-1.26** (0.58)	-1.74*** (0.51)
Observations	34	34	34	34
Panel B: Short Run				
$\ln(1 - \tau)$	-0.86 (1.07)	-0.53 (1.85)	-1.28 (1.21)	-1.48** (0.65)
Treat Country FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Observations	30	30	30	30

Note.: Standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All regressions include country of origin and year fixed effects. The long-run elasticities consider all individuals from treated and control countries who emigrated between the years 2003 and 2019. Short-run elasticities consider all individuals who stayed five years or less in the Netherlands who emigrated between the years of 2002 and 2016, the period when the preferential tax treatment has expired for all treated recipients in our sample.

⁴⁰ Following Moretti & Wilson (2017), we convert our flow elasticity to a stock elasticity by multiplying it by the gross flow-to-stock ratio, which is 0.668 in our sample. The stock elasticity is naturally smaller, as it uses a much larger base (the total stock of migrants rather than annual leavers), and could also be seen as policy relevant because it captures the long-run impact of tax changes on the overall presence of high-skilled migrants.

The gradient in elasticity is even more marked when looking at short-run estimates, as only the coefficient of those above the 99th percentile of earnings is precisely estimated at -1.48 . These results first serve to confirm our novel finding that tax-induced migration seems to only be an important issue for very top earners.⁴¹ The results are large but in line with what has been found for migration inflows of this population in other contexts, such as Denmark, for which Kleven et al., (2014) estimate elasticities with respect to tax rates of between 1.5 and 2.

3.7.2 Leaving for Tax Friendly Countries?

Many of the migrants leaving the Netherlands because of the reform were probably originally attracted to the country because of the tax break it offered. A relevant policy question concerns the next location of those who left early: are they more likely to move to (other) countries who also offer preferential tax schemes to high-skilled migrants? This would speak to the problem of international tax competition, which can reduce the effectiveness of a global impact of tax reforms enacted in isolation by one country. Using information on the exact destination reported by all 30% ruling recipients who left the country and our quasi-experimental setting, we investigate this issue.

We use the list produced in **Table 2** of Kleven et al. (2020) to identify countries that offer tax breaks to high-skilled migrants and in which year these were introduced to create a “tax break destination” dummy that takes a value 1 if a departing migrant moves to one of those countries, and 0 otherwise.⁴² We then estimate our standard difference-in-differences specification, using this dummy as the outcome of interest. Table 3.B.12 presents the results for all beneficiaries, for the split by income group, and for “mobility” for the top 1%. The reform does not, on

⁴¹ As already highlighted in Section 2.2.2, the net tax rate for the 50–95, 95–99, and top 1% income groups decreases respectively by 16%, 22%, and 24% with the loss of the tax break. The very small difference between the last two groups suggests that differences in the intensity of the treatment received cannot fully explain why the top 1% behave differently in terms of their migration responses.

⁴² This includes all departures to the individual’s previous country of residence—the destination of most leavers—and those who decided to stay in the Netherlands.

average, affect the likelihood of an individual moving to a tax break country. However, it appears to increase the probability that the highest earners leave for places with preferential migrant tax schemes, although this is not precisely estimated. This effect is driven by the location choice of the 1% who are highly mobile—defined in Section 5.2.2 as those arriving in the Netherlands from a country that is not their country of origin/nationality — who are post policy more than twice as likely to leave for other tax break countries. These results do point to a distortionary effect on national policies from international tax competition, especially among the most mobile high earners.

3.7.3 Cost–Benefit Analysis

To get an overall view of the policy we study, we provide a very basic tax revenue cost–benefit analysis of reducing tax breaks for all high-skilled migrants from ten to five years. We use the out-migration response estimates reported for the various earning groups in Table 3.B.5. That is, we assign a decrease in the length of stay in the Netherlands of 0 months for the bottom 95%, 7.2 months for the 95–99 group, and 6.9 months for the top 1%. Table 3.B.13 reports some key values needed for our cost-benefit estimations: the average monthly wage, the length of stay in the Netherlands, and the average tax rate with and without the 30% ruling. The table does so for each income group and separately for those who stay more or less than five years.

All individuals who stay beyond the new tax exemption threshold pay higher taxes, even if they react by leaving early, which is beneficial for the country’s coffers. However, there is a loss to the public purse from those who react to the reform by out-migrating before reaching five years in the Netherlands. That is what happens following the early flight of those at the top of the income distribution, which is costly given the tax receipt potential they represent. These two effects together, through the years, yield lower tax returns to the Dutch tax office. That is, collecting higher taxes from individuals after their initial five-year stay would turn into €497.3 million in extra revenue. However, given the decrease in the length of stay for all individuals, revenues would decrease by €516.9 million, translating into a net loss in tax revenue of €1.63 million per year. Given that the income tax receipts were almost €67 billion in the

Netherlands for 2019,⁴³ we can conclude that, at least in terms of a change in taxable receipts from labor, this radical policy was in the end cost neutral.⁴⁴

3.8 Conclusions

In this paper, we investigate the mobility response of migrants to a loss in preferential tax treatment using a radical change in the duration of the 30% ruling in the Netherlands, which provides certain migrants with a tax break of 30%. Only workers who arrived from certain countries during a certain period were affected by this reform, which enables us to produce a credible causal estimate of tax-induced out-migration resulting from this reform. We find strong mobility responses: the durations of stay in the Netherlands shorten significantly, especially for those at the top of the income distribution, who were post policy 18% less likely to stay beyond the expiration of their tax break. The result also reveals that for most typical workers treated—those below the 95th percentile of earnings—there is no change in emigration behavior. This is an important finding as most previous evidence of large international mobility elasticities to tax rates has been based on the reaction of top earners, which we show to be unique.

Taken together with the evidence of Timm et al. (2025), our findings suggest that the 30% ruling has succeeded in fulfilling its two core objectives, at least for individuals down the income distribution. While their study shows that the scheme effectively attracts highly skilled workers, particularly those just above the wage threshold, our results indicate that, on average, these workers do not leave once the benefit expires. In this sense, the 30% ruling not only attracts highly skilled

⁴³ This is according to the public finance overview report for the country from Statistics Netherlands (CBS), available [here](#).

⁴⁴ Our cost-benefit analysis is partial equilibrium and considers only emigration responses. The net fiscal effect could differ if changes in inflows due to revised rules for new arrivals were also taken into account. Timm et al. (2025) show that the 30% ruling significantly increased inflows of mid-to-high-income migrants near the eligibility threshold, implying a fiscal cost as more individuals qualified for the tax break.

individuals but also retains them beyond the preferential period, with only the most mobile top earners displaying significant tax-induced out-migration.

The fact that top earners are nearly as responsive to changes in tax rates when making out-migration decisions as they are when initially selecting a location in other contexts is striking, especially given that many have already spent considerable time in the country before leaving. These reactions may be particularly pronounced in the Netherlands, given its relatively small size and the ease with which high-skilled migrants can access preferential treatment in nearby countries. Therefore, these findings should be considered as policy-relevant upper bounds for larger countries. We also address the critical issue of international tax competition, showing that the most mobile top earners are significantly more likely to relocate to a tax-friendly country following the reform. This highlights the idea that to be truly effective, substantial tax reforms should ideally be implemented at the level of larger entities (e.g., the EU or OECD) rather than unilaterally by individual countries.

Appendix 3.A Additional Figures

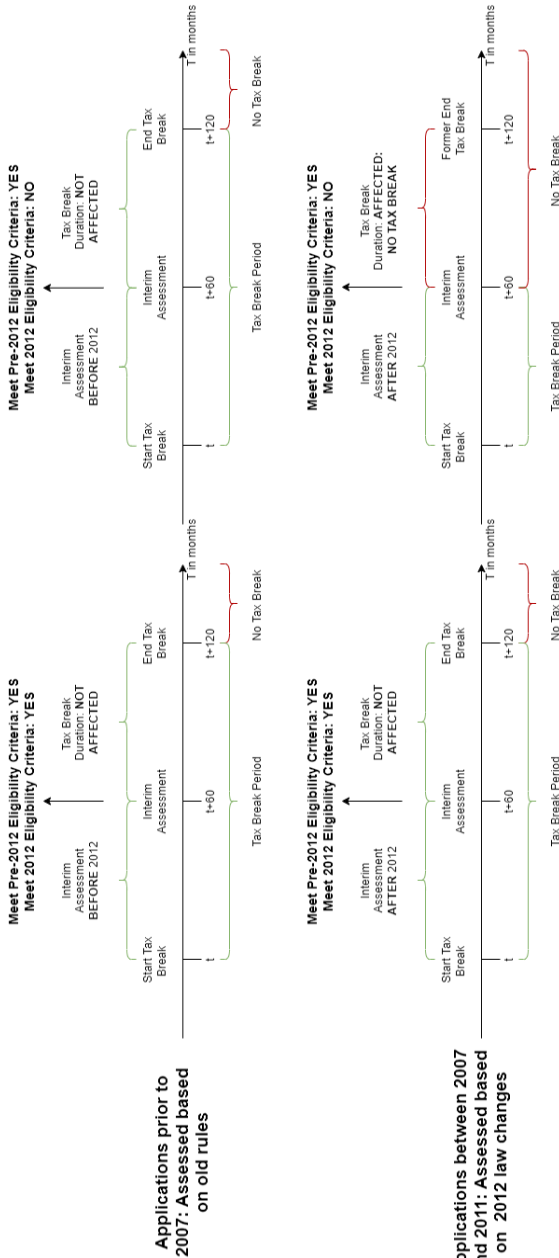
Figure 3.A.1: Preferential Tax Scheme Countries for High-Skilled Migrants



Notes. Figure based on tax break countries identified in Table 2 of Kleven et al. (2020).

Figure 3.A.2: Preferential Tax Scheme Time Diagram

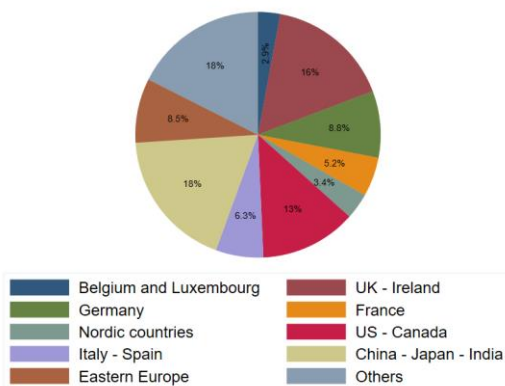
2012 LAW CHANGE:
OUTLOOK OF OLD APPLICATIONS



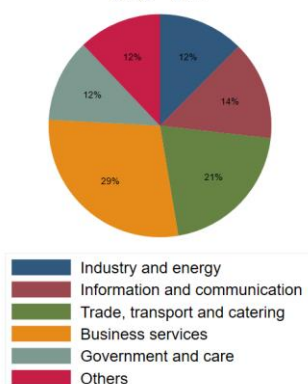
Notes. The time diagram divides the sample of beneficiaries into three different groups presented in two different rows: incoming employees who applied to the scheme in the period prior to 2007 and therefore were assessed by the five-year interim check before January 1, 2012; incoming employees who applied to the scheme between 2007 and the end of 2011 and therefore were susceptible to the 2012 law change due to the format of the transitional rule; and, incoming employees who applied from 2012 onward. We further divide the groups into two other subgroups, those who meet and those who do not meet the 2012 eligibility criteria (diagrams in the left and right, respectively).

Figure 3.A.3: Origin Country of Beneficiaries and Sectoral Composition of Employers

A3.1: Country of previous residence
2002 - 2011



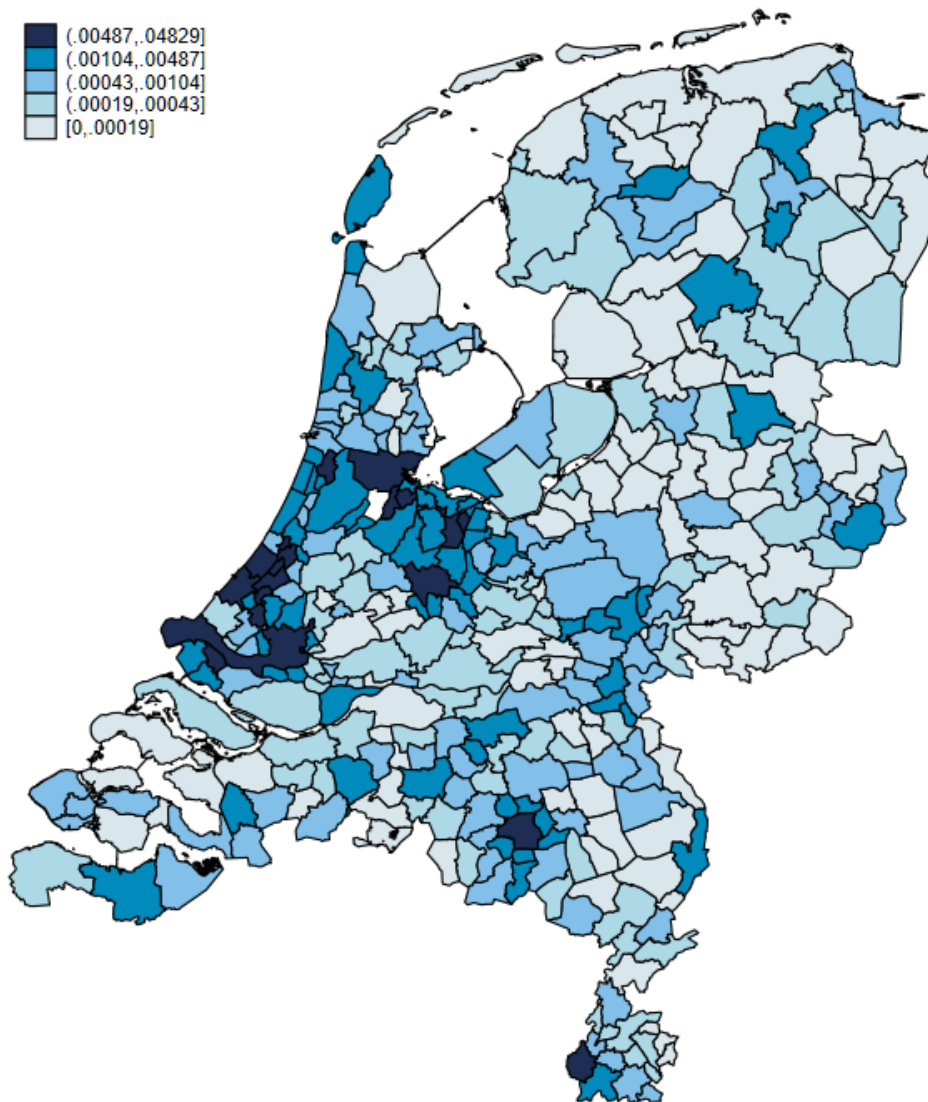
A3.2: Sectoral Composition
2002 - 2011



Notes. Figure A3.1 reports the country of previous residence of beneficiaries as a percentage of the total sample (2002–2011). Figure A3.2 reports the sectors in which beneficiaries work as a percentage of the total sample (2002–2011).

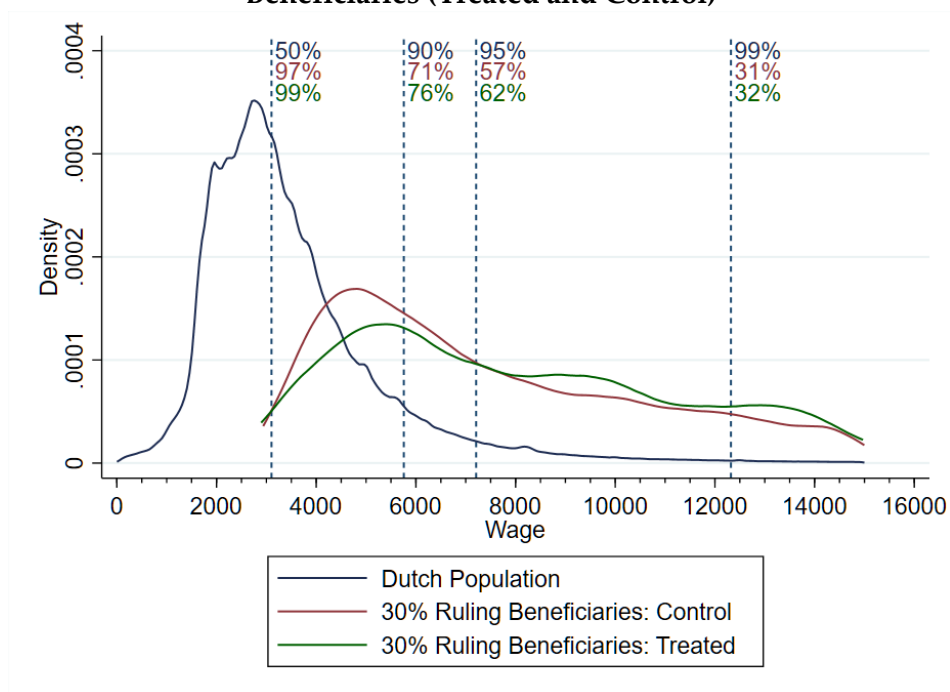
Others are composed of Agriculture, forestry, and fishing (0.1%), Construction (0.63%), Financial sectors (7.39%), Renting and trading of real estate (0.21%), Culture, recreation, and other services (3.16%) and unknown (0.59%).

Figure 3.A.4: Location Ratio of Beneficiaries per Municipality Population



Notes. Author's own calculations of the ratio of beneficiaries per population: Only 5 municipalities have a higher than 1% beneficiaries per population: Amstelveen, Amsterdam, Leiden, Wassenaar and Eindhoven. In general, 30% of the beneficiaries live in Amsterdam. The Hague, Amstelveen, Rotterdam, and Eindhoven complete the top five municipalities in the Netherlands, which account for the place of residence of more than 50% of all incoming employees.

Figure 3.A.5: Wage Distribution: Dutch Population and 30% Ruling Beneficiaries (Treated and Control)



Notes. For the Dutch population, we construct their earnings distribution using all Dutch earners in 2012, correcting for the full-time equivalent. For the beneficiaries, we compute their average monthly wage for each year that they were in the Netherlands, and we select the highest average monthly earnings out of their first two years with positive earnings. To construct their earnings distribution, we select beneficiaries from the control and treatment groups in the control years (2002–2006) and express their pre-tax monthly wages in the equivalent of 2012 euros.

Figure 3.A.6: Static & Dynamic Impact on Earnings of Losing the 30% Ruling

Figure 3.A.6.1: Tax rate & net wage, with and without 30% ruling

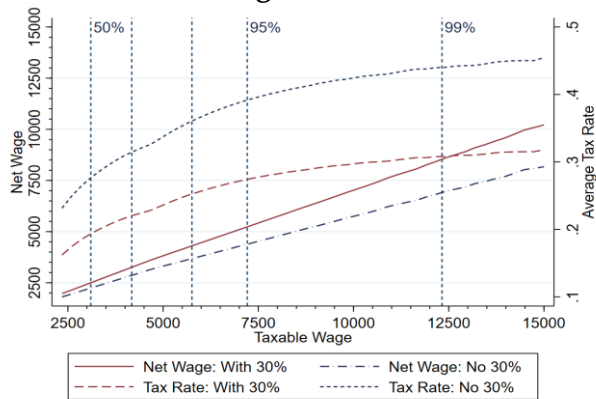
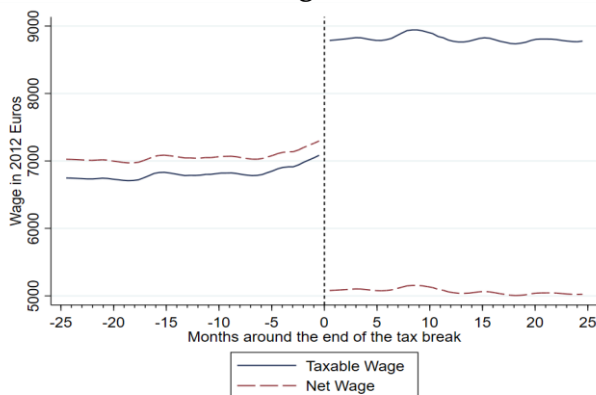
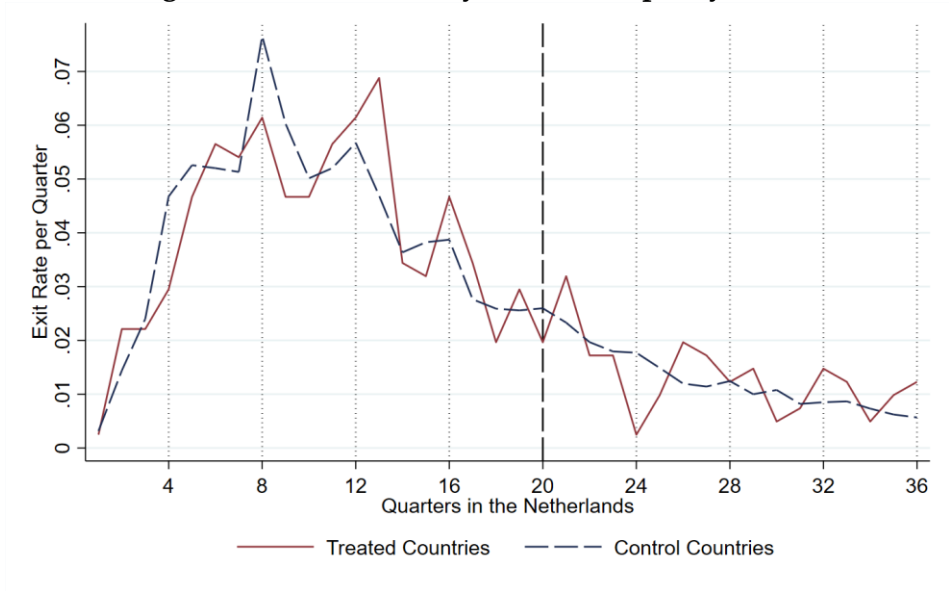


Figure 3.A.6.2: Change in taxable & net wage around timing of 30% ruling loss



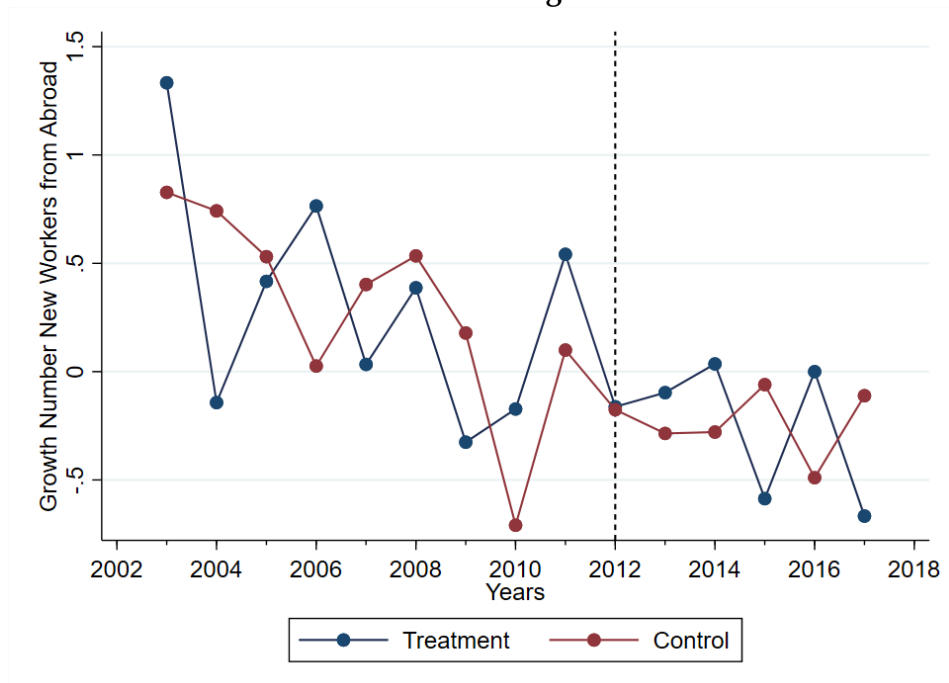
Notes. In Figure 3.A.6.1 the net wage and the tax rate are constructed based on the Dutch sample of earners in 2012, excluding beneficiaries of the 30% ruling to avoid potential behavioral responses to the tax changes related to the preferential tax scheme. In Figure 3.A.6.2 the wage dynamics are constructed by performing a local polynomial regression of the taxable and net wage in the months around the end of the tax break for the whole sample of beneficiaries that stayed at least ten years in the Netherlands. Note that the net wage is higher than the taxable wage before the end of the tax break: for example, if the average individual earns €100,000.00 per year and faces an average tax of 40%, her taxable wage is €70,000.00 while the net wage is €30,000.00 + €70,000.00*60% = €72,000.00.

Figure 3.A.7: Exit Rate Dynamics: Pre-policy Period



Notes. The figure depicts the quarterly exit rate by treatment status in control years. Treatment and control refer to the country of previous residence, meaning that the treatment group comprises the individuals who left the Netherlands after immigrating from Belgium or Luxembourg, while the control group comprises individuals who immigrated from elsewhere. The series is constructed only considering individuals who arrived between 2002 and 2006.

Figure 3.A.8: Growth Rates on the Number of New Workers Commuting



Notes. The figure shows the growth rates in the number of workers who do not reside in the Netherlands but continue to work for a Dutch employer. The series is divided by treatment status. Treatment and control refer solely to the country of previous residence, meaning that treatment group comprises the individuals who left the Netherlands after immigrating from Belgium or Luxembourg, while the control group comprises individuals who immigrated from elsewhere. The vertical dashed line marks the year in which the change in the 30% ruling became effective.

Appendix 3.B Additional Tables

Table 3.B.1: Descriptive Statistics of Tax Break Recipients by Timing of Arrival

	Treat Country				Control Country			
	2002–2006		2007–2011		2002–2006		2007–2011	
	Mean	S.D	Mean	S.D	Mean	S.D	Mean	S.D
Panel A: Migration								
Time in Job (Months)	72.58	37.19	68.35	37.57	60.08	37.11	59.80	39.03
Fraction ≥ 5 years	0.51	0.50	0.47	0.50	0.41	0.49	0.43	0.50
Fraction ≥ 10 years	0.41	0.49	0.23	0.42	0.27	0.44	0.20	0.40
Panel B: Employment								
Top 5%	0.63	0.48	0.49	0.50	0.59	0.49	0.41	0.49
Top 1%	0.34	0.47	0.25	0.43	0.32	0.47	0.20	0.40
Number of Jobs	1.65	1.00	1.90	1.09	1.52	0.97	1.82	1.04
Industry & Energy	0.13	0.33	0.11	0.32	0.14	0.35	0.11	0.32
Trade & Transport	0.07	0.26	0.07	0.26	0.11	0.31	0.17	0.37
Info & Communication	0.18	0.38	0.20	0.40	0.22	0.41	0.20	0.40
Business Services	0.21	0.41	0.25	0.43	0.28	0.45	0.28	0.45
Government & Care	0.23	0.42	0.17	0.38	0.11	0.32	0.12	0.32
Others	0.19	0.39	0.19	0.39	0.13	0.34	0.12	0.33
Panel C: Background								
Age	36.13	7.97	35.82	8.56	35.69	8.15	34.45	8.16
Gender	0.75	0.43	0.74	0.44	0.78	0.42	0.76	0.43
With Children	0.18	0.38	0.14	0.35	0.16	0.36	0.12	0.32
Without Children	0.57	0.50	0.63	0.48	0.61	0.49	0.62	0.49
Children in NL	0.25	0.44	0.23	0.42	0.24	0.42	0.26	0.44
Single	0.60	0.49	0.65	0.48	0.56	0.50	0.58	0.49
Married	0.20	0.40	0.14	0.35	0.21	0.41	0.16	0.37
Married in NL	0.21	0.40	0.21	0.41	0.23	0.42	0.26	0.44
Observations	629		964		19,072		34157	

Notes. Appendix B contains the descriptions of the variables.

Table 3.B.2: Pre- and Post-Treatment Dynamics Estimates

Dependent Variable	Time in Country	Time in Country
	(Months)	(> 5 years)
	(1)	(2)
Cohort 2002	-2.22 (1.42)	-0.035 (0.025)
Cohort 2003	-2.50* (1.45)	-0.024 (0.025)
Cohort 2004	-2.61 (1.95)	-0.020 (0.033)
Cohort 2005	1.66 (1.91)	0.042 (0.031)
Cohort 2006, omitted	- (-)	- (-)
Cohort 2007	-7.91*** (1.38)	-0.081*** (0.025)
Cohort 2008	-6.83*** (1.49)	-0.102*** (0.027)
Cohort 2009	-6.76*** (1.41)	-0.064** (0.026)
Cohort 2010	-7.36*** (1.41)	-0.069*** (0.025)
Cohort 2011	-5.71*** (1.49)	-0.076*** (0.027)
Constant	58.49*** (0.20)	0.430*** (0.003)
Country of Origin FE	YES	YES
Start Year FE	YES	YES
Observations	46,763	46,763

Notes. Clustered standard error at the level of treatment (country-of-origin by start year) in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include country of origin and start year fixed effects. The event study results are based on equation 2. Column (1) considers the first outcome of interest, time in country measured in months, while column (2) considers the second outcome of interest, the probability of staying in the Netherlands for more than five years. We omit the year 2006 as it was the last year in which the 30% ruling change did not affect individuals.

Table 3.B.3: Robustness Checks: Alternative Control Groups & Placebo Tests

Dependent Variable	Time in Country		
	in Months (Panel A) and Over 5 Years (Panel B)		
	(1)	(2)	(3)
Panel A: Months in Country			
	DE & FR	Nordic Countries	Placebo
Treat Country * Post 2012	-4.99*** (0.86)	-6.30*** (1.93)	1.02 (1.64)
Mean of Dependent Variable	65.79	65.79	52.42
Impact at Mean	-7.6%	-9.6%	1.9%
Panel B: Probability > 5 Years			
Treat Country * Post 2012	-0.061*** (0.014)	-0.07** (0.03)	-0.001 (0.020)
Mean of Dependent Variable	0.510	0.510	0.357
Impact at Mean	-12.0%	-13.7%	-0.3%
Controls	YES	YES	YES
Country of Origin FE	YES	YES	YES
Start Year FE	YES	YES	YES
Observations	54,367	3,444	45,181

Notes. Clustered standard error at the level of treatment (country-of-origin by start year) in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include country of origin and start year fixed effects. Panel A considers the first outcome of interest, time in country measured in months, while Panel B considers the second outcome of interest, the probability of staying in the Netherlands for more than five years. Column (1) consider Germany and France as control countries along with all other countries. Column (2) considers an alternative control group comprising the Nordic Countries. Column (3) presents a placebo test in the country, considering the Nordic countries as our treated sample and dropping individuals who immigrated from Belgium and Luxembourg.

Table 3.B.4: Robustness Checks: Wage, Sectors, Migration Time & Rule Request

Dependent Variable	Time in Country					
	in Months (Panel A) and Over 5 Years (Panel B)					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Months in Country						
	Above Wage	Exempted Sectors	Wage & Exempted Sectors	> 1 Year	Same Year	> 1 year & Same Year
Treat Country * Post 2012	-5.17*** (0.96)	-5.13*** (1.30)	-5.35*** (0.97)	-6.68*** (1.03)	-5.30*** (1.54)	-7.30*** (1.66)
Mean of Dependent Variable	65.91	65.90	65.80	67.74	63.03	65.88
Impact at Mean	-7.8%	-7.8%	-8.1%	-9.9%	-8.4%	-11.1%
Panel B: Probability > 5 Years						
Treat Country * Post 2012	-0.068*** (0.016)	-0.062*** (0.019)	-0.069*** (0.016)	-0.078*** (0.014)	-0.076*** (0.023)	-0.094*** (0.021)
Mean of Dependent Variable	0.512	0.513	0.511	0.527	0.476	0.500
Impact at Mean	-13.3%	-12.1%	-13.5%	-14.8%	-16.0%	-18.8%
Controls	YES	YES	YES	YES	YES	YES
Country of Origin FE	YES	YES	YES	YES	YES	YES
Start Year FE	YES	YES	YES	YES	YES	YES
Observations	44,371	41,724	45,438	43,440	37,570	34,442

Notes. Clustered standard error at the level of treatment (country-of-origin by start year) in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All regressions include country of origin and start year fixed effects. Panel A considers the first outcome of interest, time in country measured in months, while Panel B considers the second outcome of interest, the probability of staying in the Netherlands for more than five years. The robustness tests are divided into six groups: 1) excluding individuals who in the base year have a wage (in 2012 euros) below the threshold introduced in 2012 (5.1% of the sample); 2) excluding individuals who work in sectors that are exempted from the wage threshold (e.g., Research and education) (10.8% of the sample); 3) considering only individuals with wages above the threshold or who work in exempted sectors (97.16% of the sample); 4) considering only individuals who stayed more than one year in the Netherlands (92.9% of the sample); 5) considering only individuals who the firm requested application of the 30% ruling for in the same year that they immigrated to the Netherlands (80% of the sample); 6) considering individuals who stayed more than one year in the Netherlands and the firm requested application of the 30% ruling for in the same year of the immigration (73.7% of the sample).

Table 3.B.5: Time in NL: Income Heterogeneity

Dependent Variable	Time in Country in Months (Panel A) and Over 5 Years (Panel B)		
	(1)	(2)	(3)
<u>Panel A: Months in Country</u>			
	[50–95]	[95–99]	[99–100]
Treat Country * Post 2012	-1.01 (1.93)	-7.21** (3.06)	-6.93** (2.84)
Mean of Dependent Variable	64.79	70.68	62.53
Impact at Mean	-1.6%	-10.2%	-11.1%
<u>Panel B: Probability > 5 Years</u>			
Treat Country * Post 2012	-0.027 (0.026)	-0.063 (0.045)	-0.087** (0.035)
Mean of Dependent Variable	0.504	0.557	0.476
Impact at Mean	-5.4%	-11.3%	-18.3%
Controls	YES	YES	YES
Country of Origin FE	YES	YES	YES
Start Year FE	YES	YES	YES
Observations	24,628	10,658	11,424

Notes. Clustered standard error at the level of treatment (country-of-origin by start year) in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All regressions include country of origin and start year fixed effects. Panel A considers the first outcome of interest, time in country measured in months, while Panel B considers the second outcome of interest, the probability of staying in the Netherlands for more than five years. All regressions are conditional on being part of a certain income distribution group: 50–95, 95–99, and 99–100.

Table 3.B.6: Time in NL: Family Root Heterogeneity

Dependent Variable	Time in Country					
	in Months (Panel A) and Over 5 Years (Panel B)					
	(1)	(3)	(3)	(4)	(5)	(6)
Panel A: Months in Country						
	Single	Married	Married NL	No Kids	Kids	Kids NL
Treat Country * Post 2012	-5.09*** (1.52)	-0.04 (4.55)	-5.56*** (1.89)	-3.78*** (1.05)	-1.12 (5.42)	-5.67*** (1.90)
Mean of Dependent Variable	58.46	65.89	85.87	55.08	70.26	85.56
Impact at Mean	-8.7%	-0.1%	-6.5%	-6.9%	-1.6%	-6.6%
Panel B: Probability > 5 Years						
Treat Country * Post 2012	-0.073*** (0.019)	-0.002 (0.060)	-0.045 (0.040)	-0.058*** (0.014)	-0.009 (0.078)	-0.046 (0.031)
Mean of Dependent Variable	0.433	0.508	0.736	0.388	0.555	0.750
Impact at Mean	-16.9%	-0.4%	-6.1%	-14.9%	-1.6%	-6.1%
Controls	YES	YES	YES	YES	YES	YES
Country of Origin FE	YES	YES	YES	YES	YES	YES
Start Year FE	YES	YES	YES	YES	YES	YES
Observations	26,223	8,597	11,887	28,623	6,190	11,886

Notes. Clustered standard error at the level of treatment (country-of-origin by start year) in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include country of origin and start year fixed effects. Panel A considers the first outcome of interest, time in country measured in months, while Panel B considers the second outcome of interest, the probability of staying in the Netherlands for more than five years.

Table 3.B.7: Working from Abroad

Dependent Variable	Commuting			
	(1)	(2)	(3)	(4)
<u>Panel A: Ever Commute</u>				
	All	[50-95]	[95-99]	[99-100]
Treat Country * Post 2012	0.034 (0.022)	0.039 (0.024)	0.017 (0.042)	0.046 (0.046)
Mean of Dependent Variable	0.262	0.233	0.232	0.320
Impact at Mean	13.0%	16.7%	7.3%	14.4%
<u>Panel B: Months Commuting</u>				
Treat Country * Post 2012	-0.059 (1.265)	0.848 (2.263)	-0.467 (3.362)	-0.917 (1.535)
Mean of Dependent Variable	11.43	9.82	11.48	13.21
Impact at Mean	-0.5%	8.6%	-4.1%	-6.9%
Controls	YES	YES	YES	YES
Country of Origin FE	YES	YES	YES	YES
Start Year FE	YES	YES	YES	YES
Observations	46,763	24,628	10,658	11,424

Notes. Clustered standard error at the level of treatment (country-of-origin by start year) in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include country of origin and start year fixed effects. The two outcomes of interest are employment abroad (measured in months) and whether the individual has ever worked abroad. Employment abroad is measured by the number of months an individual spent working abroad for a Dutch company and who continues to pay taxes in the Netherlands on an average monthly wage of at least €1,000.00. Work from abroad is a dummy variable that equals 1 if employment abroad is higher than zero.

Table 3.B.8: Employment Time in NL: Heterogeneities – Income

Dependent Variable	Time in Employment			
	in Months (Panel A) and Over 5 Years (Panel B)			
	(2)	(3)	(4)	(5)
Panel A: Months in Employment				
	All	[50-95]	[95-99]	[99-100]
Treat Country * Post 2012	-4.57*** (1.32)	-0.67 (2.18)	-4.54 (3.13)	-7.19** (3.39)
Mean of Dependent Variable	72.91	71.43	76.69	71.17
Impact at Mean	-6.3%	-0.9%	-5.9%	-10.1%
Panel B: Probability > 5 Years				
Treat Country * Post 2012	-0.054** (0.021)	-0.013 (0.023)	-0.028 (0.039)	-0.093* (0.050)
Mean of Dependent Variable	0.600	0.582	0.632	0.592
Impact at Mean	-9.0%	-2.2%	-4.4%	-15.7%
Controls	YES	YES	YES	YES
Country of Origin FE	YES	YES	YES	YES
Start Year FE	YES	YES	YES	YES
Observations	46,763	24,628	10,658	11,424

Notes: Clustered standard error at the level of treatment (country-of-origin by start year) in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include country of origin and start year fixed effects. Panel A considers the first outcome of interest, time in employment measured in months, while Panel B considers the second outcome of interest, the probability of being employed by a Dutch firm for more than five years.

Table 3.B.9: Employment Time in NL: Family Root Heterogeneity

Dependent Variable	Time in Employment					
	(1)	(3)	(3)	(4)	(5)	(6)
Panel A: Months in Employment						
	Single	Married	Married NL	No Kids	Kids	Kids NL
Treat Country * Post 2012	-5.30** (2.14)	1.24 (5.44)	-3.21 (2.13)	-4.08*** (1.56)	3.60 (4.98)	-5.42*** (1.85)
Mean of Dependent Variable	68.05	71.97	87.85	64.33	76.78	89.16
Impact at Mean	-7.8%	1.7%	-3.7%	-6.3%	4.7%	-6.1%
Panel B: Probability > 5 Years						
Treat Country * Post 2012	-0.076*** (0.028)	0.012 (0.061)	0.000 (0.039)	-0.062*** (0.023)	0.069 (0.065)	-0.042 (0.026)
Mean of Dependent Variable	0.551	0.592	0.752	0.504	0.627	0.794
Impact at Mean	-13.8%	2.0%	0.0%	-12.3%	11.0%	-5.3%
Controls	YES	YES	YES	YES	YES	YES
Country of Origin FE	YES	YES	YES	YES	YES	YES
Start Year FE	YES	YES	YES	YES	YES	YES
Observations	26,223	8,597	11,887	28,623	6,190	11,886

Notes. Clustered standard error at the level of treatment (country-of-origin by start year) in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include country of origin and start year fixed effects. Panel A considers the first outcome of interest, time in employment measured in months, while Panel B considers the second outcome of interest, the probability of being employed by a Dutch firm for more than five years.

Table 3.B.10: Time in Country: Wealth Heterogeneity

Dependent Variable	Time in Country				
	in Months (Panel A)		and Over 5 Years (Panel B)		
	(1)	(2)	(4)	(4)	(5)
Panel A: Time in Country (in Months)					
	All	[50-95]	[95-99]	[99-100]	
Treat Country * Post 2012	-6.95*** (1.39)	-11.10** (5.26)	-8.23 (9.11)	-1.29 (4.67)	-18.45** (8.86)
Capital Wealth Decile		1.82*** (0.21)	2.55*** (0.37)	1.78*** (0.34)	1.32*** (0.25)
Treat Country * Post 2012 * Capital Wealth Decile		0.89 (0.67)	0.62 (1.30)	-0.14 (0.90)	1.48 (0.92)
Mean of Dependent Variable		66.87	72.83	66.73	64.36
Impact at Mean		1.3%	0.9%	-0.2%	2.3%
Panel B: Time in Country (> 5 years)					
Treat Country * Post 2012	-0.101*** (0.023)	-0.122 (0.091)	-0.102 (0.116)	0.012 (0.128)	-0.210** (0.105)
Capital Wealth Decile		0.020*** (0.003)	0.029*** (0.004)	0.022*** (0.005)	0.011*** (0.004)
Treat Country * Post 2012 * Capital Wealth Decile		0.006 (0.012)	0.004 (0.017)	-0.004 (0.021)	0.009 (0.011)
Mean of Dependent Variable		0.528	0.600	0.511	0.500
Impact at Mean		1.1%	0.7%	-0.8%	1.8%
Controls	YES	YES	YES	YES	YES
Country of Origin FE	YES	YES	YES	YES	YES
Start Year FE	YES	YES	YES	YES	YES
Observations	30,529	30,529	16,529	6,718	7,218

Notes. Clustered standard error at the level of treatment (country-of-origin by start year) in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All regressions include country of origin and start year fixed effects. Panel A considers the first outcome of interest, time in country, while Panel B considers the probability of staying in the Netherlands for more than five years. Wealth Quintiles are constructed with respect to the wealth of other beneficiaries to avoid a distribution that is skewed to the left and low variation. Appendix B presents more information about the wealth data.

Table 3.B.11: Wage Compensation

Dependent Variable	Ln(Wage) (1)	Ln(Wage) (2)	Ln(Wage) (3)
	[50-95]	[95-99]	Top 1
Treat Quarters	0.283*** (0.014)	0.254*** (0.025)	0.281*** (0.044)
Treat Country * Treat Quarters	-0.102* (0.055)	-0.088 (0.068)	-0.029 (0.091)
Quarter in NL FE	YES	YES	YES
Country of Origin FE	YES	YES	YES
Observations	26,824	10,543	6,456

Notes. Clustered standard error at the level of treatment (country-of-origin by start year) in parentheses *** p<0.01, ** p<0.05, * p<0.1. The logarithm of the taxable wage is our outcome of interest; it is regressed on treated quarters and the interaction between treated quarters and treated countries. The sample is restricted to individuals who arrived between 2002 and 2008 to account for the fact that individuals from control countries who arrived in treated years had completed their ten years before 2019. Treated quarters are defined in two different ways: 1) for the treated country from treated years, treated quarters are defined from the 20th quarter onward; 2) for the rest of the sample, the treated quarters are defined from the 40th quarter onward. The analysis is done locally around the end of the tax break, minus four quarters before, and four quarters after. All regressions include years in the Netherlands and country of origin fixed effects.

Table 3.B.12: Tax Break Countries

Dependent Variable	Time in Country in Months (Panel A) and Over 5 Years (Panel B)					
	(1)	(2)	(3)	(4)	(5)	(6)
	All	50-95	95-99	Top1	Top 1 Low Mobile	Top 1 High Mobile
Treat Country * Post 2012	0.014 (0.014)	-0.002 (0.026)	0.014 (0.016)	0.035* (0.021)	-0.002 (0.013)	0.081** (0.041)
Mean of Det Variable	0.045	0.056	0.038	0.039	0.018	0.064
Impact at Mean	31.1%	-3.6%	36.8%	89.7%	-11.1%	126.6%
Controls	YES	YES	YES	YES	YES	YES
Country of Origin FE	YES	YES	YES	YES	YES	YES
Start Year FE	YES	YES	YES	YES	YES	YES
Observations	46,763	24,629	10,659	11,422	7,841	3,559

Notes. Clustered standard error at the level of treatment (country-of-origin by start year) in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include country of origin and year fixed effects. The outcome of interest takes a value of 1 if the destination country offers a tax break, and 0 otherwise, including if an individual decided to stay in the Netherlands. If an individual is returning to their country of previous residence, then we assign 0 to the dependent variable.

Table 3.B.13: Cost–Benefit Analysis

	[50–95]		[95–99]		Top 1%	
	< 5 years	> 5 years	< 5 years	> 5 years	< 5 years	> 5 years
Monthly Wage (€ of 2012)	€3,548.53	€3,667.90	€6,682.96	€6,687.97	€18,921.72	€17,785.02
Time in Country (Months)	26.71	100.91	27.62	99.04	29.74	95.68
Δ Time in Country (Months)	0	0	7.21	7.21	6.93	6.93
Tax Burden - With 30%		24.3%		29.1%		36.4%
Tax Burden - Without 30%		37%		44%		52%
Observations	14,686	13,533	7,306	5,263	8,868	4,202
<u>Millions of €</u>						
10 Years Tax Break	€334.98	€1,216.76	€385.43	€1,013.67	€1,786.38	€2,599.64
5 Years Tax Break	€ 334.98	€1,474.57	€ 284.81	€1,106.68	€1,370.12	€2,746.12
Δ Tax Revenue	€ 0	€ 257.82	€ -100.62	€ 93.01	€ -416.26	€ 146.48
Δ Total Tax Revenue	€-19.57					
Δ Total Tax Revenue/Year	€-1.63					

Notes. The table presents the cost–benefit analysis based on estimated numbers obtained from Table 3.B.5. The Table displays the average monthly wage, length of stay in the Netherlands, average tax rate with and without the 30% ruling by each group of income, and separating individuals who stayed less than five years and more than five years. We assume that the decrease in the length of stay in the Netherlands is equal for individuals who stayed less or more than five years in the country.

Appendix 3.C Dataset, Sample, and Variables Descriptions

Construction of sample

We use administrative data from the Dutch Tax Office (Belastingdienst) and from Statistics Netherlands (CBS). The Dutch Tax Office provides specific and unique data on all the beneficiaries of the 30% ruling, 200,885 individuals, for 2001–2019. The information details the specific starting and end date of the tax break for each individual as well as their respective employer.

We start our sample construction by merging these individuals with their specific migration information from the CBS files GBAMIGRATIEBUS and VRLGBAADRESBUITENLANDBUS. GBAMIGRATIEBUS contains information from the Municipal Personal Records Database – Registration of Residents – (GBA), and VRLGBAADRESBUITENLANDBUS contains the Registration of Non-Residents (RNI).

The GBAMIGRATIEBUS provides information on immigration, emigration, and country of previous residence of all individuals who once lived in the Netherlands. The VRLGBAADRESBUITENLANDBUS mainly provides information on the start and end dates of a foreign address, when individuals emigrated from the Netherlands. For some countries, mainly neighboring countries, the following information is also available: 1) the place where the person officially lives; and 2) the distance to the Dutch border in kilometers. It is noteworthy that an individual who has never lived in the Netherlands but has any relationship with any Dutch authority will appear in the VRLGBAADRESBUITENLANDBUS but not in the GBAMIGRATIEBUS.⁴⁵

⁴⁵ Statistics Netherlands acknowledges that individuals are not obliged to report a move to the RNI (unlike the GBA), so the information quality may not be as good as that in the GBA.

Information on monthly wage comes from the CBS files SECMWERKNDGAMNDBEDRAGBUS (2001–2019) and is complemented with the file POLISBUS/SPOLISBUS (2006–2019). When the monthly wage is not available in either of these CBS files, we fill the gaps with unique data from the Dutch Tax Office on annual earnings that we transform to a monthly basis.

Information on background characteristics such as age, gender, and birth country come from the file GBAPERSOONTAB, while information on household characteristics such as marital status and number of children comes from GBAHUISHOUDENSBUS. Both files are available for 2001–2019.

Our analysis mainly focuses on the individuals who arrived in the Netherlands from 2002–2011 and participated in a valid 30% ruling during this period; they are observed from 2002–2019 (73,400 individuals). There are 48,334 individuals in both the VRLGBAADRESBUITENLANDBUS and the GBAMIGRATIEBUS files. There are 11,612 individuals who immigrated to the Netherlands and never left and are only in GBAMIGRATIEBUS; 13,454 individuals have never lived in the Netherlands and thus, they are only in VRLGBAADRESBUITENLANDBUS.

Construction of variables

Using GBAMIGRATIEBUS and VRLGBAADRESBUITENLANDBUS we construct a longitudinal panel with the individuals' start and end dates (immigration and emigration) in the Netherlands as well as the start and end dates at a foreign address. We defined cross-border workers as workers who have at least €1,000.00 of average monthly income in a certain period covered by the VRLGBAADRESBUITENLANDBUS.

We divide our variables of interest in two groups: 1) time in country, which is the number of months an individual stayed in the Netherlands, taking into account the first immigration date and the last emigration date from GBAMIGRATIEBUS; and 2) time in contact, which is the number of months an individual stayed in contact with the Netherlands, which takes into account the first contact date and the last contact date from the GBAMIGRATIEBUS and the VRLGBAADRESBUITENLANDBUS. That

is, time in contact also considers the period an individual spent commuting, before and/or after they immigrated/emigrated to the Netherlands. From these two groups we extract two other variables of interest namely two dummy variables indicating whether the individual stayed in the country for more than 60 months (five years) and whether the individual stayed in contact with the Netherlands for more than 60 months (five years).

Our difference-in-differences strategy is constructed by interacting a year dummy variable with a country of previous residence dummy. The year dummy equals 1 if the start year of the tax break is between 2007 and 2011, and 0 if the start year of the tax break is between 2002 and 2006. The country dummy equals 1 if the country of previous residence was Belgium or Luxembourg, and 0 otherwise. Information on country of previous residence is extracted from GBAMIGRATIEBUS. Some individuals immigrated and emigrated more than once to the Netherlands in the observed period.⁴⁶ As one of the eligibility criteria to be eligible for the 30% ruling before 2012 was that the individual had not lived in the Netherlands in the past ten years, then only the country of previous residence when they first immigrated to the Netherlands is sufficient for our purposes.

We have monthly wage information from SECMWERKNDGAMNDBEDRAGBUS and POLISBUS/SPOLISBUS that we deflated to 2012 euros. SECMWERKNDGAMNDBEDRAGBUS is the average wage, and POLISBUS/SPOLISBUS is the monthly wage. We use POLISBUS/SPOLISBUS to develop the wage dynamic graph around the end date of the tax break. To construct the wage percentiles we select we use both SECMWERKNDGAMNDBEDRAGBUS and POLISBUS/SPOLISBUS. We compute the average monthly wage for each year, and we select the highest average monthly wage out of an individual's first two years with positive earnings. Then, we compare it to the Dutch wage distribution of that specific year, excluding all the beneficiaries. As we are using the average monthly wage of beneficiaries,

⁴⁶ Around 86% of the 61,450 individuals either immigrated permanently or emigrated permanently.

we rely on the SECMWERKNDGAMNDBEDRAGBUS to define the Dutch distribution.

We use the GBAHUISHOUDENSBUS for our family composition heterogeneity analysis. We look at each beneficiary household characteristic when they arrived in the Netherlands and the last household characteristic observed in the data. The data allow us to split individuals into two different groups with three subgroups of interest. The first group relates to the marital status and the subgroups are always single, always married, and married in the Netherlands. We could also add a fourth group for the individuals who divorced in the NL, but the sample size would be too small, so we decided to include them in the always single group. The second group relates to the kids in the household; the subgroups are with children, no children, and, children in the Netherlands.

VRLGBAADRESBUITENLANDBUS contains information on the foreign country in which individuals live after they left the Netherlands.⁴⁷ Using this file we construct the variable country of destination, which we use for our German analysis.

The last variable of interest that we use is a dummy variable that equals 1 if an individual's country of birth is different than the country of previous residence, and 0 otherwise. We use the GBAPERSOONTAB files to define the country of birth and the GBAMIGRATIEBUS to define the country of previous residence.

Wealth Data

We use administrative data from Statistics Netherlands (CBS) on household wealth (VEHTAB), which is available from 2006 to 2020, and which contains asset and debt information from January 1 of the reporting year, recorded at the end of the previous year (e.g., 2006 data reports 2005 household assets and debts). We define wealth as the net assets of a

⁴⁷ Some case also contains the period and country where the individual lived before moving for the first time to the Netherlands. This is the case if and only if this individual has any relationship with the Netherlands (e.g. work relation).

household as in the variable VEHW1000VERH.

The wealth of an individual is defined as the household level in the same year as their income level was calculated (see section above). If an individual's household did not report its assets and debts in the same year as the recorded income data, then we consider the wealth recorded one year after the base year as the individual's wealth. Wealth quintiles are constructed with respect to the wealth distribution of other beneficiaries given that the distribution of wealth of beneficiaries is not comparable to the wealth distribution of the Dutch population.

Only individuals living in the Netherlands by January 1, 2006, have data on household wealth. The sample of individuals who benefit from the 30% ruling from 2005 to 2011 is 37,868, while the sample of individuals who have wealth data recorded over the same period is 30,529, which is the sample we use to run our analysis.

4

The Labor Market Impact of Post-Colonial Immigration: The Surinamese Case in the Netherlands

I am grateful to my supervisors Frank Cörvers and Hans Schmeets for the for taking the time to give detailed comments on an earlier version of this Chapter. I would also like to thank Olivier Marie, Nico Pestel, Bart de Koning and Gedeão Locks for helpful comments and suggestions.

Abstract

This paper examines the labor market effects of a large and unexpected migration wave: the Surinamese migration to the Netherlands following Suriname's independence in 1975. The migration constituted a sudden, exogenous labor supply shock, as immigrants arrived en masse with right to work. Leveraging variation in local exposure to Surinamese immigrants, we employ a difference-in-differences strategy to estimate the impact on native employment and occupational outcomes. We find that the migration shock led to a decline in employment among low-skilled natives, while high-skilled natives experienced no adverse effects. Although our data do not directly capture language proficiency of immigrants, our results indicate that municipalities receiving Surinamese immigrants with a Creole background—who presumably have higher Dutch proficiency—experienced somewhat larger negative impacts on low-skilled native employment compared to municipalities with more Hindustani and Javanese immigrants. Effects on occupational scores remained muted. These findings suggest that immigrants' language proficiency might be an important factor to consider when studying immigration effects on native labor market outcomes, though further research is needed to unpack the underlying mechanisms.

4.1 Introduction

More than 280 million people reside in countries different than their birth country in 2020, which represents about 3.6% of the world population. This represents an increase of 128 million since 1990 and is more than three times the number estimated in 1970.^{1,2} Given the effects of recent intensifications in conflicts around the world and climate change on migration and mobility patterns, sudden and unexpected waves of immigrants became more likely to happen.³ Thus, it is crucial for public policy design to better understand how receiving countries absorb these abrupt and unexpected immigration shocks, their short-run effects on wages and employment of natives and the duration of these effects.

This paper studies the local economic effects of a sudden and unexpected immigration shock in the Netherlands in 1975, following the independence of Suriname. By the time of Suriname's Independence Day, all Surinamese living in the Netherlands would be regarded as Dutch nationals, while those living in Suriname would thereafter be regarded as Surinamese nationals. This measure prompted an exodus from Suriname of people wanting to secure Dutch nationality, resulting in almost 60,000 Surinamese immigrating to the Netherlands in 1974-1975, and another 40,000 of them immigrating between 1979 and 1980.^{4,5} Because there have been few of such drastic inflow of immigrants in the Dutch history, and immigrants arrived at the Netherlands with the right to work, this episode offers a good opportunity to study how local labor markets

¹ McAuliffe & Triandafyllidou (2021).

² There were around 566,900 immigrants (first generation) living in the Netherlands in 1970 (4.37% of the total population), 1,054,000 in 1990 (7.08% of the total population) and 2,262,000 in 2020 (12.99% of the total population), Statistics Netherlands (CBS)

³ (UNHCR, 2020; World Bank, 2018)

⁴ In the period from 1976 to 1978 yearly around 7,700 Surinamese immigrants arrived in the Netherlands.

⁵ A special transitional period from 1975 until 1980 was phased in to control immigration after independence, during which all Surinamese nationals were allowed to apply for a three-month residence permit in the Netherlands that, subject to the requirement of self-support, would open the possibility of obtaining Dutch citizenship.

adapted. Remind that, contrary to the recent huge inflow of Syrian and Ukraine people, the Surinamese inflow consisted of previous Dutch nationals who were far better adapted to Dutch culture and who had good Dutch language skills.

Our identification strategy relies on classifying local labor markets, defined as the municipalities in the Netherlands, as more or less exposed to the Surinamese immigration prior to the 1975 event, controlling for the initial foreign-born share.⁶ We start our analysis documenting that municipalities that were more exposed to the Surinamese immigration prior to the 1975 immigration indeed received more Surinamese immigrants after the Suriname's Independence. Important to our research design is that municipalities with high and low exposure to previous Surinamese migration did not systematically differ in their reception of immigrants from other countries of origin. Although the Netherlands experienced immigration from other countries of origin during the period from 1972 to 1985, settlement patterns for these other immigrant groups were similar across municipalities regardless of their exposure to the previous Surinamese inflow.

We used a difference-in-difference (DID) design, controlling for the initial share of foreign-born in each municipality, in order to estimate the effects of immigrants on labor market outcomes of natives employment and occupational score.⁷ We show that natives' employment levels were negatively affected in municipalities that were more exposed to the shock of Surinamese immigration flows vis-a-vis municipalities that were less exposed. The effect was entirely driven by the impact of Surinamese immigration flows on low-skilled natives' employment levels while we found no effect on high-skilled natives' employment. The

⁶ We follow Abramitzky, Ager, Boustan, Cohen, & Hansen (2023) and include this to control for differential trends by initial foreign-born share.

⁷ In this paper we use the International Socio-Economic Index of occupational status (ISEI) (Ganzeboom, De Graaf, & Treiman, 1992), which captures the socio-economic status of occupations by leveraging survey data on the earnings and educational attainment of individuals in specific occupations.

findings are in line with the canonical spatial equilibrium model of immigration as a local labor supply shock, in which adjustment may occur through changes in labor demand, occupational allocation, and native mobility across municipalities.⁸ In this framework, our estimates based on regional exposure capture the total local labor market effect of immigration, rather than isolating a single adjustment margin, which is particularly relevant given that the new Surinamese immigrants were on average less educated than natives. Looking at the other margin of response, native's occupational score, we found no evidence of a decline in the occupational scores' outcomes of low-skilled natives due to the substantial increase in Surinamese immigrants.

The central identifying assumption of our approach is that, conditional on the initial foreign-born share, municipality, and province by year fixed effects, municipalities with a higher Surinamese exposure prior to 1975 would have followed similar economic trends in the absence of the unexpected Surinamese inflow. We provide graphical evidence using event study graphs to illustrate that trends in the outcome variable (e.g., native employment and occupational score) were parallel between high- and low-exposure municipalities prior to 1975. Moreover, our analysis shares similarities with shift-share instruments as it relies on the initial distribution of Surinamese settlements to assess labor market exposure to the wave of Surinamese immigrants following the independence of Suriname in 1975 (Bartik, 1991; Card, 2001). While Jaeger, Ruist, and Stuhler (2018) caution that serial correlation in immigrant inflows can bias shift-share instruments in immigration studies, the sudden and dramatic nature of the 1975 through 1980 Surinamese independence shock likely mitigates these concerns in our context. Gielen and Webbink (2023) describe this group as "beat-the-ban immigrants" distinguishing them from the "economic immigrants" who arrived in the Netherlands before the policy announcement, further underscoring the distinct nature of this migration wave.

⁸ Please refer to Dustmann et al. (2016) for a in-depth discussion.

This paper contributes to this literature by analyzing the effects of a large and unexpected migration wave: the Surinamese migration to the Netherlands following Suriname's independence in 1975. Unlike gradual migration flows or those driven by labor demand, the Surinamese migration represents a sudden, exogenous labor supply shock, as immigrants arrived en masse with full labor market rights and relatively good host-country language skills. This is different from the previous literature (Angrist and Kugler 2003; Borjas 2017; Borjas and Monras 2017; Card 1990; Peri and Yasenov 2019; Tumen 2016).⁹ In this respect, it parallels studies such as Edo (2020) that focuses on the return of French nationals from Algeria, emphasizing how the scale and speed of migration shocks can amplify their impact on local labor markets,¹⁰ Glitz (2012) who studied the return of Germans living in eastern Europe and the former Soviet Union to Germany after the fall of the Berlin Wall and Mäkelä (2017) who studied the returnees from Mozambique and Angola to Portugal in the mid-1970s.¹¹ However, unlike the studies of Edo (2020) and Mäkelä (2017) on repatriates returning to their country of citizenship, our analysis examines an inflow of immigrants who, despite historical ties, were "foreign" nationals entering the Netherlands. This distinction allows us to uniquely explore the integration dynamics and labor market effects associated with immigration from former colonial territories rather than repatriation of nationals.

An important aspect of our study and to our knowledge previously unexplored in the literature, is the assumed heterogeneity in language proficiency among Surinamese migrants due to colonial educational

⁹ There is an extensive literature examining the labor market impacts of immigration (see, for instance, Dustmann et al. (2016); Edo (2019), for comprehensive reviews). In this paper, we selectively highlight only those studies exploiting natural experiments, as these are most directly comparable to our empirical approach.

¹⁰ Hunt (1992) also explores this event finding negative employment and wage effects.

¹¹ Additional natural experiments in the literature include Dustmann, Schönberg, & Stuhler (2017) and Beerli, Ruffner, Siegenthaler, & Peri (2021). These studies, however, exploit policy-driven cross-border commuting shocks rather than migration episodes, thus examining slightly different contexts compared to those discussed here.

policies.¹² Specifically, colonial policies of "Dutchification" predominantly targeted the Creole population in urban areas, which is believed to have resulted in greater Dutch language proficiency compared to Hindustani and Javanese populations from rural regions (see subsection 2.1.1). We leverage this variation in language skills to examine how proficiency influences labor market integration following sudden migration shocks. Unlike other migration episodes studied in the literature, in which limited immigrant language proficiency or cultural familiarity typically creates comparative advantages for natives in certain (communication-intensive) tasks (Ottaviano & Peri, 2012), this context presents a natural contrast between immigrants with high and low Dutch language proficiency. This variation allows us to examine whether language skills influenced labor market outcomes, providing new insights into the role of language in the absorption of sudden migration shocks.

The rest of the paper is organized as follows. Section 4.2 provides historical background on Suriname's colonial past, the process of Dutchification, and the migration dynamics leading up to the 1975 independence. Section 4.3 describes the data sources and summary statistics as well as it details the empirical strategy, including the construction of the exposure measure and the difference-in-differences framework used to estimate the impact of the migration shock. Section 4.4 details how the higher exposure led to higher influx of Surinamese immigrants to the treated municipalities. Section 4.5 presents the main results, focusing on the employment and occupational score outcomes of native workers, and the robustness checks. Section 4.6 explores heterogeneity effects by migrant diversity and native population

¹² While our analysis does not directly measure immigrants' language proficiency, we rely on previous literature suggesting that colonial educational policies (specifically "Dutchification" policies) contributed to higher Dutch language proficiency among Creole Surinamese populations relative to Hindustani and Javanese groups. This assumption is based on historical accounts and educational policy studies rather than direct measurement in our data.

characteristics. Finally, Section 4.7 concludes by summarizing the key findings and their policy implications.

4.2 Historical Background

4.2.1 Historical Background

4.2.1.1 Colonial Period, Abolition of Slavery and “Dutchification” of the Colony

The Breda Treaty, which was signed by England and the Netherlands in 1667, included the exchange of Manhattan for Suriname and marked the starting point of more than 300 years of Dutch political dominance over Suriname (Hoefté 2014). During the colonial era, Suriname's economy was founded on plantations and primarily dependent on the labor force of enslaved people from Africa. It was centered on the production of sugar and other tropical products to be sold in the European Market (van Amersfoort, 2011).

Slavery was abolished in 1863 and owners received compensation for every enslaved person they manumitted (Hoefté, 2014; van Amersfoort, 2011). Although formerly enslaved people were now legally free, they were obliged to hire themselves out as plantation workers for a period of 10 years in order to make the transition from slavery to wage labor as smooth as possible (Stipriaan, 1998). However, after this 10 years period many formerly enslaved people left the countryside in search of employment elsewhere, preferably in the colony's capital, Paramaribo, or in the recent gold and balata industries (Hoefté, 2014). The failed attempts to tie former enslaved people to the plantations had profound ethnic composition and social consequences to Suriname.

Given that the colonial government considered that large-scale agriculture was to be the only viable economic option, tens of thousands of Asian indentured workers, the Hindustani, were hired to keep the plantations operating (Hoefté, 2014). Additionally, between 1890 and 1940, the Dutch government hired small groups of Javanese workers in the Dutch East Indies (Indonesia) to completely replace those who left the plantations (van Amersfoort, 2011). The arrival of these indentured

workers in Suriname caused a change in the nation's ethnic composition. Due to the growth of this Asian community and the Javanese population, who currently make up 35% and 14% of the population of Suriname respectively, the percentage of people of African heritage (Creoles) fell from 95% in 1863 to 47% in the next century (Oostindie 2008).

Moreover, following this high influx of former enslaved people to the capital, the colonial authorities and the white elite realized that education and Christianization were the most effective ways to maintain order in the country, slowly replacing violence as a mean of control (Stipriaan, 1998). One direct consequence of these assimilation policies culminated in the universal compulsory education for children between 7 and 12 years old, introduced in 1876/1877 (Stipriaan, 1998). It is noteworthy to mention that, although most indentured workers remained in Suriname after completing their contracts, which typically lasted five years, they were merely the colonial state's temporary legal responsibility (Stipriaan 1998). Therefore, due to the reduced imposition of Western culture on Hindustani and Javanese in the rural areas at the time, these assimilation tactics actually meant the "Dutchification" of the formerly enslaved Creoles in urban areas (Oostindie 2008; Stipriaan 1998; Vezzoli 2014). Notwithstanding the influence of racial and geographic divisions on the assimilation policy's reach, the Dutch became the only language of instruction in the entire nation in 1947, beginning the de facto "Dutchification" of Suriname (Meel, 2001).

The Dutch political sovereignty over Suriname came to an end after the proclamation of the Charter of the Kingdom in 1954, which established the Netherlands, Suriname, and the Netherlands Antilles as three independent entities that would collectively make up the Kingdom of the Netherlands. Defense and foreign affairs would be shared by the Kingdom's members, while each region would be in charge of its own internal affairs. Most crucially, starting in 1954, every resident of the Kingdom was regarded as a Dutch citizen (van Amersfoort, van Niekerk 2006).

4.2.1.2 *Independence and Emigration to the Netherlands*

The first movements of people from Suriname to the Netherlands date back to the eighteenth century, when enslaved people were brought to the Netherlands as domestic servants (Oostindie 1986). For the majority of Suriname's history, education was the main factor influencing migration from the colony to the Netherlands. According to van Amersfoort and van Niekerk (2006), student migration was common throughout the entire nineteenth century, starting with the Dutch colonial elite who sent their children to study in the Netherlands and then followed by the Creole elite. For the middle-class people in Suriname that aspired a job in the colony administration, a diploma in the Netherlands was fundamental and seen as a way of social-economic progress (Stipriaan 1998; van Amersfoort and van Niekerk 2006).

During this period and up until World War II, immigration to the Netherlands was highly selected on gender, ethnicity, age and destination, being overrepresented by male Creole students clustering in university municipalities. However, over the course of the second half of the 20th century, this picture gradually changed with regard to the selection on gender and socioeconomic status due to more affordable transportation and Suriname's increased prosperity, though selectivity on the ethnic background was still present, with immigrants predominately being Creoles (van Amersfoort 2011; van Amersfoort and van Niekerk 2006). At the beginning of the 1970s, the Surinamese population in the Netherlands started to better reflect the composition of the population in the colony, given the participation of the Hindustani descendants in the migratory movements during the 1960s (Van Amersfoort & Van Niekerk, 2006). Importantly, migration from the colony to the colonizing country was influenced by past migration patterns, as we will show in Section 4.4, and intensified by the communication between immigrants in the Netherlands and their family members and friends in Suriname, the network effect (van Amersfoort 2011).

Nearly 30,000 Surinamese people lived in the Netherlands in 1971, more than doubling the amount from 1960.¹³ Despite the fact that the total number of Surinamese immigrants was not alarming, especially when compared to the Dutch population at the time, this rapid growth and the regional concentration, along with a negative stereotypical view of the Surinamese held by the Dutch population and based on media reports rather than personal experiences, were causing concern among the government and the general public (van Amersfoort 2011; van Amersfoort and van Niekerk 2006). Nonetheless, the Dutch government was hesitant to begin the process of decolonization. To their surprise, in 1974, following the 1973 elections in Suriname, Henck Arron—leader of the Creole party, Nationale Partij-kombinatie (NPK), and the newly elected Surinamese prime minister—suddenly announced that the colony wished to become independent before the end of 1975 (van Amersfoort and van Niekerk 2006; Vezzoli 2014).

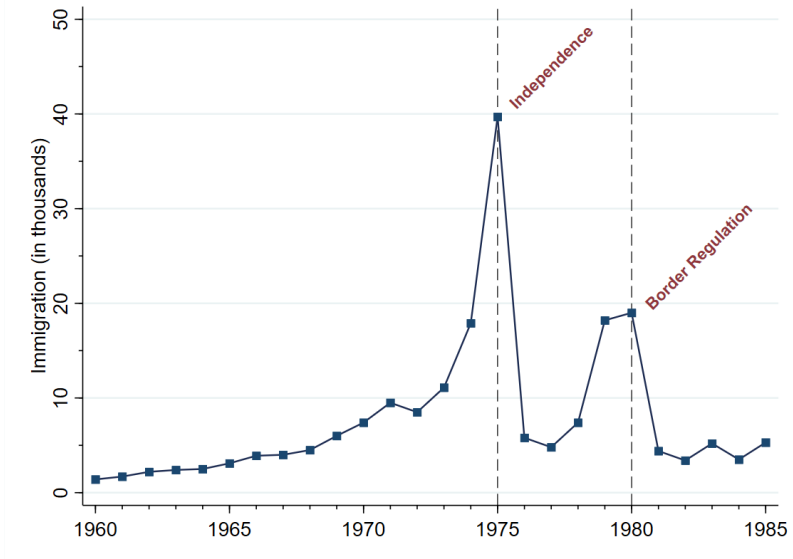
The Hindustani opposition party, Vooruitstrevende Hervormingspartij (VHP), in particular, did not support independence because they believed it would lead to insecurity, potential ethnic violence, and discrimination (Vezzoli 2014). The Hindustani party concentrated its efforts on attempting to guarantee dual citizenship for the Surinamese once independence became irreversible; however, this was later rejected by the Dutch government (van Amersfoort and van Niekerk 2006; Vezzoli 2014). The Dutch government was keen to quickly grant Suriname independence due to domestic pressure, which gave the Surinamese administration the advantage of negotiating migration opportunities and economic perks (Vezzoli 2014).

By the time of Suriname's Independence Day, all Surinamese living in the Netherlands would be regarded as Dutch nationals, while those living in Suriname would thereafter be regarded as Surinamese nationals (Van Amersfoort, 2011). This measure prompted an exodus from Suriname of people wanting to secure Dutch nationality, resulting in almost 60,000

¹³ This paper uses data at the municipality level, which is not openly available, but one can see these numbers for the country level in Statline (Statistics Netherlands (CBS), n.d.-b).

Surinamese immigrating to the Netherlands in 1974-1975 as shown in **Figure 4.1**. Additionally, a special transitional period till 1980 that was phased in to control immigration after independence allowed all Surinamese nationals to apply for a three-month residence permit in the Netherlands that, subject to the requirement of self-support, would open the possibility of obtaining Dutch citizenship (van Amersfoort and van Niekerk 2006; Vezzoli 2014). This sparked a second wave of immigration in the years 1979–1980, with roughly 40,000 Surinamese moving to the Netherlands also shown in **Figure 4.1**.¹⁴

Figure 4.1: Immigration Inflow of Surinamese in the Netherlands



Notes: The figure plots the national inflow of Surinamese Immigrants in the Netherlands though 1960-1985.

¹⁴ Figure 4.A.1 in the Appendix 4.A shows the stock of immigrants from Suriname in the Netherlands from the years 1972 until 1985 and the stock of immigrants from other countries. The stock of immigrants' series is normalized so that it matches the stock of Surinamese series in 1972.

4.3 Data and Empirical Strategy

4.3.1 Data

Our empirical analyses are based on three datasets. During the period 1973-1985, Statistics Netherlands (CBS) conducted a so-called labor force census (Arbeidskrachtentelling (AKT))¹⁵ every other year to collect data regarding employment. This will be the main data sources for the labor market analysis. The data includes detailed information about workers, their occupation, their demographic characteristics, and their place of residence and work. We focus on individuals aged 18–65, with non-missing information for nationality, place of residence,¹⁶ education, and occupation.

The second data source is the 1971 full count population census, which is an administrative data including more than 13 million individuals living in the Netherlands in 1971. We use this data to collect detailed information about workers, their occupation, their demographic characteristics, and their place of residence and work. Importantly, this data allows us to precisely assert the share of immigrants, by their birth country, living in each of the municipalities prior to the high influx of Surinamese in 1975, which will be vital information for our empirical strategy described in the following sections.

Both the AKT data and the full-count population census enable us to compute our dependent variables of interest: native employment and the International Socio-Economic Index of Occupational Status (ISEI), hereafter referred to as the occupational score. The ISEI is an internationally standardized measure that captures the socioeconomic status of occupations based on survey information about the earnings and education of individuals in particular occupations. Specifically, the ISEI is derived by assigning scores to occupations in a way that maximizes the

¹⁵ These were large-scale surveys (sample size of 3%)

¹⁶ The 1979 AKT does not provide information on individual's residence and work municipalities, that is the reason why this year is excluded from the analysis.

indirect effect of education on income, thereby reflecting how education translates into income through occupational pathways (H. B. Ganzeboom, De Graaf, & Treiman, 1992). To implement this measure in our context, we merged the ISEI with the CBS data using the conversion tables provided by the International Stratification and Mobility File (ISMF) project.¹⁷

The third data source is the population statistics of 1972-1985. The Population Statistics provides information about the size and composition of the population on January 1 of each year. The observations are based on information that CBS receives from the population registers of municipalities. This data relates to all persons included in the population register. This data allows us to determine the net inflow of immigrants per municipality¹⁸ for each year during 1972 and 1985. Thus, the data provides information about immigrants' settlement patterns—crucial for our identification strategy—demonstrating that municipalities with greater initial exposure to Surinamese immigration, as described in the next section, continued to receive more Surinamese immigrants in subsequent years (see in section 4.4).

4.3.2 Local Area Exposure

The first step to study the economic effects of the sudden inflow of Surinamese immigrants in the Netherlands is to measure the municipality labor market's exposure to the unexpected inflow in 1975, 1979 and 1980. We construct our exposure measure based on the share of Surinamese population residing in each municipality prior to 1975, using the 1971 Census. We computed municipality exposure by dividing the Surinamese share in 1971 by the municipality population in the same year. In other words, this measure captures the treatment intensity of municipality labor markets, which is consistent with the measure used by Clemens, Lewis, and Postel (2018) to study the ending of the Bracero guest worker

¹⁷ One can access the conversion files [here](#).

¹⁸ The population statistics, census and the AKT data are harmonized to the municipal boundaries of 2019.

program and Abramitzky, Ager, Boustan, Cohen, & Hansen, (2023) who studied the effect of immigration restrictions in 1920s on the US labor market. Moreover, we have also calculated the initial foreign-born exposure measure that will be used throughout the analysis. The resulting exposure measures can be described as following:

$$Suriname\ Exp_{i,1971} = \frac{Total\ Surinamese\ Population_{i,1971}}{Total\ Population_{i,1971}} \quad (1)$$

$$FB\ Exp_{i,1971} = \frac{Total\ Foreign\ Born\ Population_{i,1971}}{Total\ Population_{i,1971}} \quad (2)$$

Our empirical strategy exploits the considerable settlement variation across municipalities of Surinamese people in 1971. This historical settlement pattern, combined with the sudden nature of Suriname's independence in 1975, generated plausibly exogenous variation in the local intensity of the immigration shock. Specifically, municipalities with a higher pre-existing Surinamese population were more likely to receive new arrivals due to social networks. Figure 4.2 illustrates the geographical variation in the exposure measure with darker shading reflecting higher exposure to the Surinamese migration. Exposure to Surinamese immigrants is apparent across municipalities even within the same province, features that will help to study the effects of immigrants on municipality's labor markets.

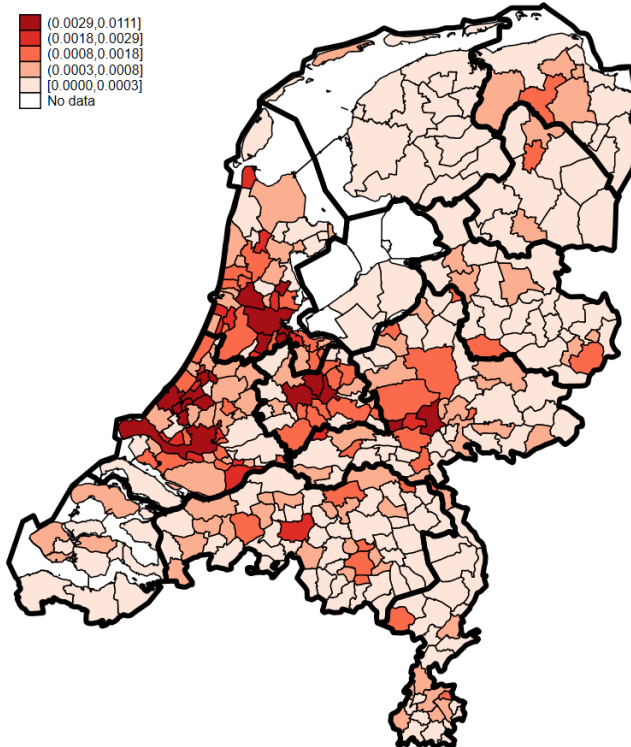
For the analysis, we consider a balanced panel of 347 municipalities and split them into two groups according to the Surinamese exposure measure percentiles: (0-90]; and (90-100].¹⁹ For the analysis, we only consider the 25% biggest municipalities in the Netherlands, according to their population in 1971,²⁰ resulting in panel of

¹⁹ We have also performed robustness checks using a continuous treatment variable.

²⁰ From the total inflow of Surinamese immigrants to the Netherlands between 1972-1985, 88% of them settle in the 25% biggest municipalities of which 81% of them settled in municipalities from the top 10%.

88 municipalities.²¹ The main reason to consider only the top 25% municipalities in the Netherlands is that the settlement patterns of other immigrants is similar across treatment and control municipalities at the top 25%, differing only with respect to the Surinamese immigrants, as showed in Section 4.4. As our analysis will show, the treatment and control groups within the big municipalities have very close pre-reform parallel trends.

Figure 4.2: Distribution of Surinamese in 1971



Notes: This figure plots the Surinamese Exposure measure of municipalities in the Netherlands in 1971.

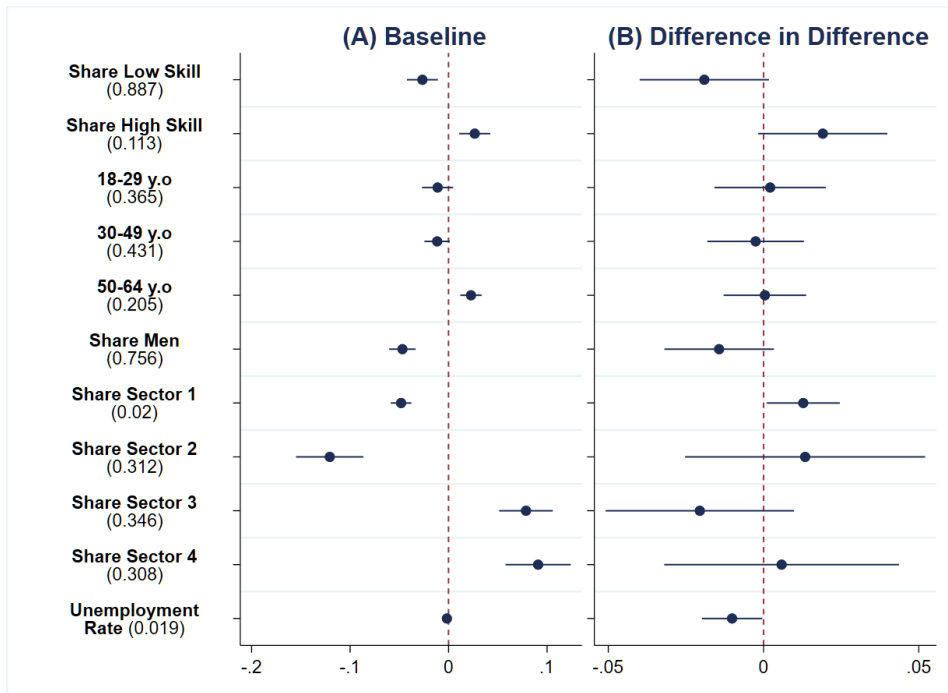
²¹ We perform robustness checks using various population-size thresholds for control municipalities, specifically those with populations exceeding 10,000, 20,000, 30,000, and 40,000 individuals, as well as municipalities with populations above the median.

4.3.2.1 *Summary Statistics*

Table 4.B.1 in Appendix 4.B presents the pre-1975 shock descriptive statistics for the two groups of municipalities that we compare in our Difference in Differences estimations: municipalities with high and low Surinamese exposure. The table reveals that, before the 1975 Surinamese immigration, highly exposed municipalities differed significantly from less exposed ones. These municipalities had smaller populations and a lower share of immigrants from other countries. Additionally, highly exposed municipalities tended to have, on average, a lower (higher) share of low (high) skilled natives, as well as a lower share of employed males. However, there were no significant differences in employment rates or age composition between high- and low-exposure municipalities.

Of particular interest to our identification strategy—specifically the difference-in-differences approach outlined in the next section—is how these characteristics differ between high- and low-exposure municipalities, and more importantly, how these differences change before and after the unexpected 1975 Surinamese immigration flow. We present this information in **Figure 4.3**, which shows the pre-immigration averages for highly exposed municipalities (in brackets), the estimated baseline differences between high- and low-exposure municipalities on the left side of the graph, and the estimated pre- and post-1975 immigration shock differences between these two groups on the right side. Despite the observed differences in means, when we examine how these differences evolve over time, we find that almost none remain statistically significant. This finding reassures us about the validity of our identification strategy, confirming that there are no observable compositional changes between high and low exposed municipalities before and after the 1975 Surinamese immigration.

Figure 4.3: Descriptive Characteristics of Municipalities, by Treatment Status



Notes. Figure 4.3 Panel A presents the summary statistics for the main characteristics of the municipalities along with the 95% confidence intervals. For each variable, we present the baseline average for the treated municipalities in control years (values in parentheses) as well as the plotted difference in means between the control and treatment countries in the pre-immigration period of 1971 and 1973. Figure 2 Panel B plots the difference-in-differences in means between the high and low exposed municipalities from the top quartile according to their population in 1971, with no systematic overall differences between the two groups throughout the years.

4.3.3 Difference-in-Differences Approach

Formally, we estimate the effects of the high influx of Surinamese immigrants on municipality labor markets based on the following difference-in-differences model:

$$y_{i,t} = \beta_1 Treated Municipality_i \times Post_t + \beta_2 FBExp_{i,1971} \times Post_t + \alpha_i + \gamma_{i,t} + \varepsilon_{i,t} \quad (3)$$

Where $y_{i,t}$ is our outcome of interest (native employment²² and occupational score) for unit i (municipality), in year $t = 1971, 1973, 1975, 1977, 1981, 1983$ and 1985 . *Treated city* equals 1 for municipalities that belong to the top 10% of the Surinamese exposure measure and 0 otherwise. *Post* equals 1 for years equal of or after 1975. $FBExposure_{i,1971} \times Post_t$ is included in our preferred specification in order to account for different trends by initial foreign-born exposure measure and identify the impact of immigrants solely from differences in the intensity of treatment captured by the size of the Surinamese share of each municipality. The main effect of the *Treated city* and *FBExposure* is absorbed into municipality fixed effects (α_i) and the main effect of *Post* is included in province-by-year fixed effects ($\gamma_{i,t}$). Controlling for municipality and province-by-year fixed effects implies that β_1 is estimated from changes in the number of Surinamese within the same municipality over the years and captured by the exposure measure of high inflow municipalities as compared to municipalities in the same region that were less likely to receive a high inflow of immigrants.

The central identifying assumption of our approach is that, conditional on the controls, municipality, and province by year fixed effects, municipalities with a high Surinamese exposure prior in 1971 would have followed similar economic trends absent of the unexpected Surinamese inflow. Thus, through the analysis we also examine how the outcomes changed prior to the inflow across treatment status by extending equation 3 to the event study model as depicted in equation 4.

²² In a spatial equilibrium framework as the one in the present Chapter, using native employment levels recovers the total effect of the immigration shock. This estimate inherently captures adjustments along the endogenous labor supply margin, including the regional migration of workers and changes in local labor demand. Consequently, this measure is considered the most policy-relevant parameter. In contrast, specifications using relative outcomes, such as wage or employment shares or rates, often identify a relative or partial effect of immigration, typically excluding the overall labor market impact that stems from aggregate population and capital adjustments.

$$\begin{aligned}
Y_{it} = & \alpha + Treated\ City_i \times \sum_{\substack{t=1971 \\ t \neq 1973}}^{1985} \beta_{1,t} \mathbb{1}(Year = t) \\
& + FBExp_{i,1971} \times \sum_{\substack{t=1971 \\ t \neq 1973}}^{1985} \beta_{2,t} \mathbb{1}(Year = t) \\
& + \alpha_i + \gamma_{i,t} + \varepsilon_{it}.
\end{aligned} \tag{4}$$

This event-study equation allows us to plot the coefficients of interest over time. As mentioned previously, for the analysis of Surinamese immigration inflows, the data covers the years from 1972 to 1985 continuously. In contrast, for the labor market analysis, the data points are available for the years 1971, 1973, 1975, 1977, 1981, 1983, and 1985.²³

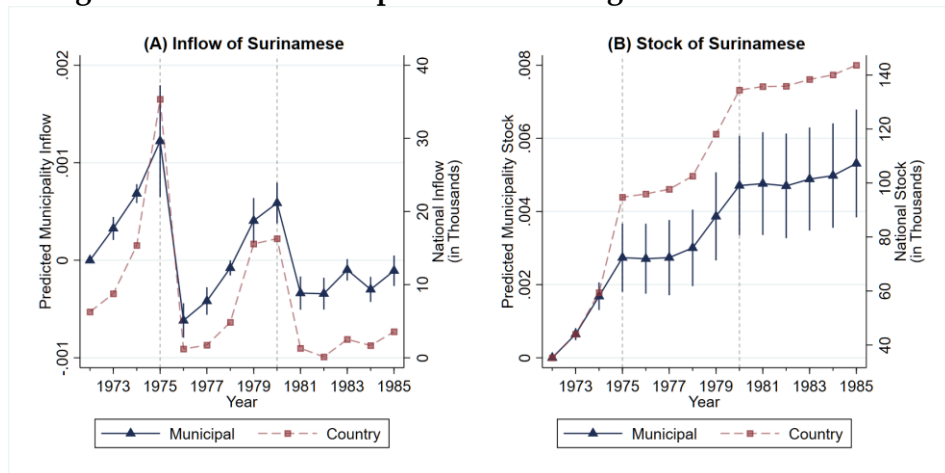
4.4 Effects of Local Exposure on Surinamese Immigration

Before we proceeding to estimate the effects of the 1975 Surinamese migration shock on the labor market outcomes of natives, it is essential to establish that our exposure measure—the pre-1975 Surinamese exposure based on 1971 settlement pattern—actually predicts subsequent immigration inflows, ensuring that the exposure measure is a valid predictor of treatment intensity. That is, the cornerstone of our analysis is that municipalities with higher exposure before 1975, as determined by our 1971 exposure measure, received more immigrants from Suriname during the period from 1975 to 1985 compared to less-exposed municipalities. **Figure 4.4** provides graphical evidence that this is the case. Panel A of **Figure 4.4** plots the coefficients $\beta_{1,t}$ from equation 4 and their 95% confidence interval, for the full sample of municipalities in the Netherlands. The outcome of interest is the inflow of Surinamese in municipality i at year t , scaled by the municipality's i population in 1971, based on equation 4. Panel A depicts an important feature: The inflow of Surinamese immigrants to highly exposed municipalities closely

²³ As previously mentioned, we don't have data from AKT in 1979.

mirrored the national inflow depicted by the red line and also shown in **Figure 4.1**.

Figure 4.4: Local Area Exposure and Immigration from Suriname



Notes: The figure shows the effect of the post-1975 independence of Suriname on Surinamese immigration to the Netherlands. It plots the estimated inflow of Surinamese immigrants and associated 95 percent confidence intervals for highly treated municipalities (panel A) based on the event study model (equation (4)). It also plots the national inflow of Surinamese by year. Panel B plots the estimated stock of Surinamese immigrants and associated 95 percent confidence intervals for highly treated municipalities together with the national stock of Surinamese population in the Netherlands across the years.

Important to the analysis, however, is that Surinamese immigrants not only arrived in certain municipalities but that they settled there. Panel B of Figure 3 depicts this result when the dependent variable is the stock of Surinamese in each municipality i at year t scaled by the municipality's i population in 1971. We do this for the sample of municipalities that we will analyze through most of the paper, that is, including only municipalities in the top quartile in terms of population size. We estimated an increase in the number of Surinamese as a share of the municipality's population by 0.326 percentage points (see Column 1 of **Table 4.B.2**, which is based on equation 3). Although the value may not appear to be significantly high at first glance, the mean Surinamese exposure measure of the treated group is 0.394 percentage points,

compared to 0.063 of those in control municipalities, thus nearly doubling the value.

Arguably, there are two potential threats to the causal interpretation of our results. First, there could be unobserved factors that affected more or less exposed municipalities differently such as unobserved region-specific shocks to prices, demand, or productivity. We partially account for it by including municipality and province by year fixed effects. After showing the main results in the next section, we carry out a number of robustness tests that indicate our findings are not confounded by unobserved industry- or region-specific shocks. Second, highly exposed municipalities could also attract more immigrants from other sending countries and, thus, the labor market effects studied in the next section are a result of high inflow of immigrants in general rather than due to the Surinamese inflow. We dealt with this problem in two ways: First, we include, in all specifications, the foreign-born exposure measure defined in equation 2. Second, we provide graphical evidence that this is not the case by plotting the results when the outcome of interest is the stock of immigrants from other countries of origin, shown in Figure 4.A.3. The results are not statistically significantly different from 0 at the 95% confidence interval. In other words, although immigrants from other origin countries were arriving in the Netherlands through the period of 1972-1985, there were no differences in settlement patterns across treatment status (see Column 2 of Table 4.B.2). We further corroborate this finding by showing the results for specific groups of immigrants that represented a big portion of immigrants in the Netherlands in 1971: Indonesia, Dutch Antilles, Turkey, Morocco, and Eastern Europe. Graphical evidence is shown in Figure 4.A.4 and precise estimates are shown in Table 4.B.2, Columns 2-7.

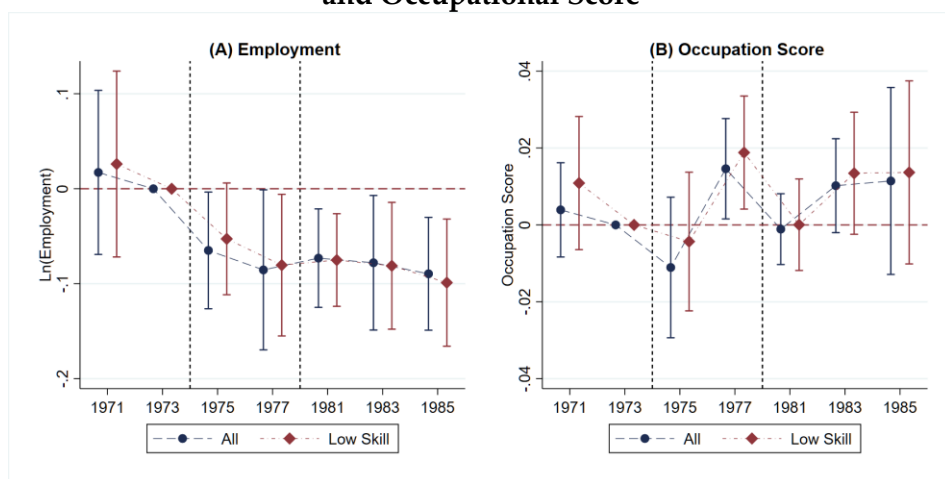
4.5 Labor Market Results and Robustness

4.5.1 Main Results

This section investigates whether native's employment options were negatively impacted by the large influx of Surinamese immigrants. We start the analysis with graphical evidence based on equation 4 and shown

in **Figure 4.5**, where the point estimates based on equation 3 are shown **Table 4.1**. The outcomes of interest are the log total native workers and log occupational score in order to obtain a complete picture of both the relative and total effects of immigration. Figure 4.5 shows, the estimated coefficients for both employment and occupational score are statistically not significantly different from zero before 1975, meaning that there were no significant employment and occupational score trajectories differences in treated and control municipalities prior to 1975.

Figure 4.5: Impact of Surinamese Immigrants on Native’s Employment and Occupational Score



Notes: The figure shows the effect of the Surinamese inflow on native worker’s employment and occupational score. It plots the event study coefficients and associated 95 percent confidence intervals for the highly treated municipalities based on equation (4). The dependent variables are the log total workers (panels A) and average log occupational score (panels B) for the whole population and for the low-skilled population. Control variables are municipality share of foreign born and municipality and year fixed effects. Standard errors are clustered at the NUTS 2 level.

However, after the sudden inflow of Surinamese immigrants to the Netherlands, the level of employment of natives in highly exposed municipalities is depressed (point estimates are shown in **Table 4.1** panel A). Panel A of **Table 4.1** also shows the impact of immigration by skill level (high vs low skill) separately to capture the important complementarity and substitutability impacts of immigrants on natives (Ottaviano & Peri, 2012). Interestingly, the effects come solely by the

depression of employment opportunities of low skilled natives while the effects on employment for highly educated workers are not statistically significant, although it does not rule out negative employment effects (graphical evidence for low skill natives shown in Panel A of Figure 4.5). In the post migration period, there was an 8.7% decrease in the level of native employment in the highly treated municipalities, which translates into a 0.83% decrease in native's employment level when evaluated at the pre-1975 mean.

The canonical partial equilibrium model of immigration predicts that an influx of low-skilled immigrants can depress employment and wage of low educated natives while potentially benefiting high-skilled natives due to complementarity (Ottaviano & Peri, 2012).²⁴ In our analysis, while we observe that low-skilled natives experienced employment declines in municipalities with higher Surinamese exposure, we do not find similar negative effects when looking at the occupational score. Specifically, Panel B of **Table 4.1** shows no evidence of statistically significant declines in occupational score for low-skilled and high-skilled natives. These results suggest that although employment opportunities for low-skilled natives contracted, this did not translate into measurable declines in their relative occupational standing during this period.²⁵

Finally, the results show that the employment effects persist through 1980s, after the immigration shocks in 1975 and 1980. In a spatial equilibrium framework such as the one adopted in this Chapter, such persistence can arise for two non-exclusive reasons. First, labor demand may adjust only slowly if capital accumulation or firm entry responds with delay to the sudden labor supply shock (Edo, 2020). Second, part of

²⁴ The literature indicates that Surinamese who immigrated to the Netherlands from the early 1970s onwards, including the significant influx around and after the 1975 independence, were, on average, less educated and from lower social classes compared to previous Surinamese migrants and the native Dutch population (Gielen & Webbink, 2023; van Niekerk, 2004).

²⁵ It is important to note that the occupational score, while correlated with wages in many contexts, does not capture actual earnings and may not fully reflect wage dynamics in these local labor markets.

the adjustment may occur through native out-migration from more exposed municipalities, leading to persistent differences in local employment even in the absence of long-run national effects (Dustmann, Schönberg, & Stuhler, 2016). The reduced-form nature of the analysis captures the combined effect of these adjustment margins and does not allow disentangling them empirically.

Table 4.1: Employment and Occupational Score Baseline Results

	(1)	(2)	(3)
Panel A: Ln (Employment)			
	All	High Skill	Low Skill
Suriname Share X Post	-0.087*** (0.026)	-0.042 (0.045)	-0.091*** (0.030)
Panel B: Ln (Occupational Score)			
	All	High Skill	Low Skill
Suriname Share X Post	0.003 (0.005)	0.001 (0.009)	0.003 (0.006)
Panel A: Mean Dep. Var.	10.49	8.24	10.37
Panel A: Impact at Mean	-0.83%	-0.51%	-0.88%
Panel B: Mean Dep. Var.	3.73	4.10	3.68
Panel B: Impact at Mean	0.08%	0.02%	0.08%
Municipality FE	Yes	Yes	Yes
Province by Year FE	Yes	Yes	Yes
Observations	616	616	616

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. This table presents the coefficient of the interaction between Surinamese exposure and the post-migration flow. The Estimations control for share of foreign born in each municipality with municipality and province by year fixed effects. Panel A look at the first outcome of interest, native employment, while panel B focuses on the second outcome of interest, occupational score. Column 1 presents the results of native employment and occupational score for the whole sample of natives living in each of the 82 municipalities. Column 2 performs the analysis on the sample to the low skilled natives for each municipality and Column 3 for the high skilled natives.

4.5.2 Robustness Checks

As in every difference-in-differences analysis, the main concern is that the effects are caused by a failure of the common trends' assumption in outcomes municipalities or by unobserved confounders. The first of these threats we dealt with by providing compelling graphical evidence that the trajectories of outcomes of interest were the same across treatment status. **Table 4.B.3** provided further evidence that the employment effects for low skilled natives remain robust when including linear time trends for each province in the vector of control variables together with municipality and year fixed effects, while the occupational score effects for highly skilled natives dissipated. Also, **Table 4.B.4** includes a Bartik, (1991) variable in the control to accounts for region-specific, sector-driven demand trends or shocks. The intuition is to control for employment and occupational score changes at the regional level that might be more exposed to external shocks affecting employment and occupational score given their industry composition. The results remain robust.

As previously mentioned, the vast majority of Surinamese were concentrated in four municipalities which are also the biggest municipalities in the Netherlands: Amsterdam, Rotterdam, The Hague and Utrecht. Thus, a natural concern is that the results might be, at least in part, driven by outliers. We dealt with this threat by dropping each municipality in our sample at a time, ranked by 1970 population and plotted the results in **Figure 4.A.5** and **Figure 4.A.6**, for employment and occupational score respectively. Reassuringly, the results remain robust and the point estimates when excluding one of the five biggest municipalities in the Netherlands are displayed in **Table 4.B.5**. We extend this exercise to COROP regions (NUTS III regions) and we dropped each NUTS III region at a time, ranked by the 1970 population and plotted the results in **Figure 4.A.7** and **Figure 4.A.8**. The results remain robust even to the exclusion of bigger regions of the country and, thus, the estimates do not rely on any municipality nor region to retain its sign and statistical significance. Finally, we consider the continuous measure of exposure as our intensity to treat. The results, shown in **Table 4.B.6**, are robust.

We also carefully checked the robustness of the main findings by changing how we define the treatment and control group, excluding potential outliers and running placebo regressions. We start by limiting the treatment group to only municipalities in the 95th percentile of the Surinamese exposure measure. We also vary the population-size thresholds used to define the control group, running regressions with municipalities larger than 10,000, 20,000, 30,000, and 40,000 inhabitants, as well as those above the median population size. The results are shown in **Table 4.B.7** and **Table 4.B.8** respectively and, despite smaller employment effects when the threshold of population increases, they remain very robust to these alternative samples.

Finally, source of concern is that the results are driven not by the fact that municipalities were highly exposed to Surinamese immigrants but rather that treated, and control municipalities were in differential trends given their initial foreign-born share. Although we control for this possibility by including the foreign-born exposure share interacted with post 1975 dummy - and have demonstrated that it does not affect our results (see Figure 4.A.3) - , we provide additional evidence. To further confirm that our findings reflect differences solely due to exposure to Surinamese immigration, we conduct placebo regressions. In these placebo tests, the treated municipalities are defined as those in the top 10% of exposure to Turkish and Moroccan immigration (calculated following Equation 1).²⁶ The results are shown in **Table 4.B.9** and, reassuringly, are not statistically different from zero.

4.6 Heterogeneity Analysis: Immigrants Diversity

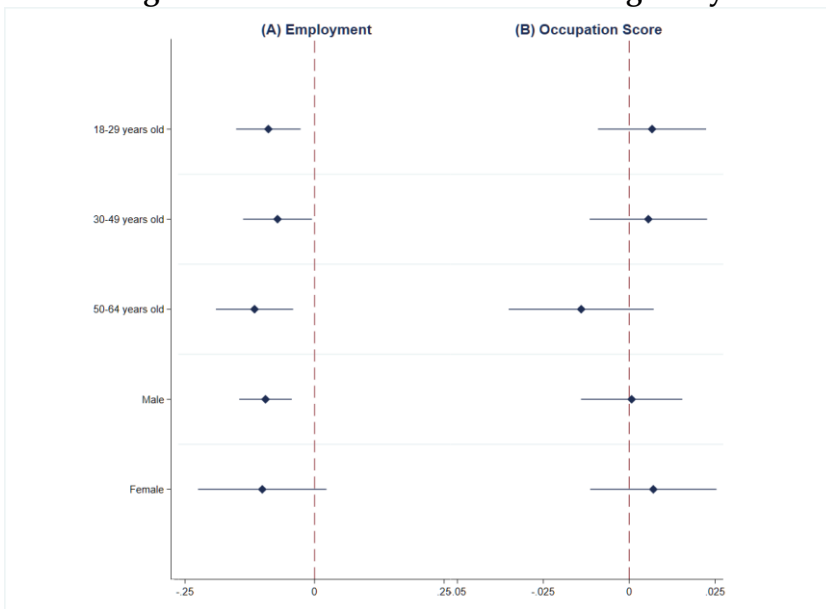
4.6.1 Natives Heterogeneity

This subsection discusses the heterogeneous impact of Surinamese migration on different groups within the native population. The different groups are split by age, 18-29 years old, 30-49 years old and 50-64 years

²⁶ The Turkish and Moroccan population were, and continues to be, one of the largest migrant groups in the Netherlands

old, and gender, male and female. Given the employment effects on low skill natives and the muted effects on occupational score across the native’s skills, we focus solely on heterogenous effects within the low skill native population. Figure 4.6 shows that, within the low skill natives, employment effects are negative in all groups, apart from the female population, although the high confidence intervals point to an unprecise estimate. The point estimate results are shown in **Table 4.B.10** and the impact at the mean varies from -0.75% to -1.32%. We also show that the effects on occupation scores are muted for every group as a result of the Surinamese migration.

Figure 4.6: Low-skilled natives Heterogeneity



Notes: The figure shows the effect of the post-1975 independence of Suriname on native worker’s employment and occupational score. It plots the estimated effect and the associated 95 percent confidence intervals for the highly treated municipalities based on equation (3) for different groups of low-skilled native population, by age groups [18-29]; [30-49] and [50-64] and breaking down by male and female population.

4.6.2 Immigrants Diversity

One important feature of our study is that, differently than previously explored in the literature (Borjas, 2017; Card, 1990; Foged & Peri, 2016;

Peri & Yassenov, 2019), immigrants arrive to the Netherlands with the right to work and, at least for part of the Surinamese migrant population (see subsection 4.2.1.1), with the local language knowledge.²⁷ While the 1971 census does not provide a direct breakdown of language skills or ethnic groups within the Surinamese population, we draw on historical context and the literature to differentiate between groups. Specifically, Dutch colonial educational policies ("Dutchification") were predominantly targeted at the Creole population, which was largely Christian, while the Hindustani and Javanese groups—primarily Hindu and Muslim, respectively—were less exposed to these language policies. This allows us to calculate the Surinamese exposure measure for each group, 1) Creole and 2) Hindustani & Javanese, and define treated municipalities as the top 5% of municipalities in terms of their exposure measure.

We present in **Table 4.B.11** the result from a triple interaction specification, of the dummy indicating the municipalities that belongs to the top 5% of the Creole exposure measure with a dummy indicating municipalities that belong to the top 5% of the Hindustani & Javanese exposure measure with a post 1975 dummy. We explore these differences in Table 4.B.11, which presents the results from this triple-interaction model including the post-1975 indicator, and in Figure 4.7, which shows the estimated impacts graphically for low-skilled natives. Focusing on the low skilled native, we present graphically in Figure 4.7 the point estimates of our triple interaction model. The results indicate that the effects of Surinamese immigrants on low skilled natives' employment is mainly driven by effects on municipalities that were more exposed to the Surinamese Creole and municipalities that were exposed to both groups, Surinamese Creole and Hindustani & Javanese. The results also show that

²⁷ Some papers have also explored the repatriation of French from Algeria to France in after the end of the Algerian war of independence in 1962 (Edo, 2020); the wave of Portuguese repatriates from Angola and Mozambique in the mid-1970s (Carrington & De Lima, 1996; Mäkelä, 2017). However, unlike these studies on repatriates returning to their country of citizenship, our analysis examines an inflow of immigrants who, despite historical ties, were "foreign" nationals entering the Netherlands

there is no impact on the occupation score of native workers in municipalities that are highly exposed to the Surinamese Hindustani & Javanese migration, while close to zero effects on municipalities highly exposed to Surinamese Creole and both groups, Surinamese Creole and Hindustani & Javanese.

Although our data do not allow us to directly measure immigrants' language proficiency, this historical and institutional context suggests that Creole Surinamese likely had higher Dutch language proficiency than the Hindustani and Javanese populations. Language proficiency plays a central role in determining the extent to which immigrants act as substitutes or complements to native workers. Immigrants with limited command of the host-country language typically avoid communication-intensive tasks and thus fill different roles than natives.²⁸ This segmentation of the labor market reduces direct competition and enhances complementarity.²⁹

Our results are consistent with this framework. Municipalities receiving Surinamese population that presumably have higher Dutch proficiency, the Creole population,³⁰ show greater displacement of low-skilled natives, likely because these immigrants were better equipped to compete for the same jobs. Conversely, immigrants with lower Dutch proficiency likely entered distinct labor market segments, minimizing

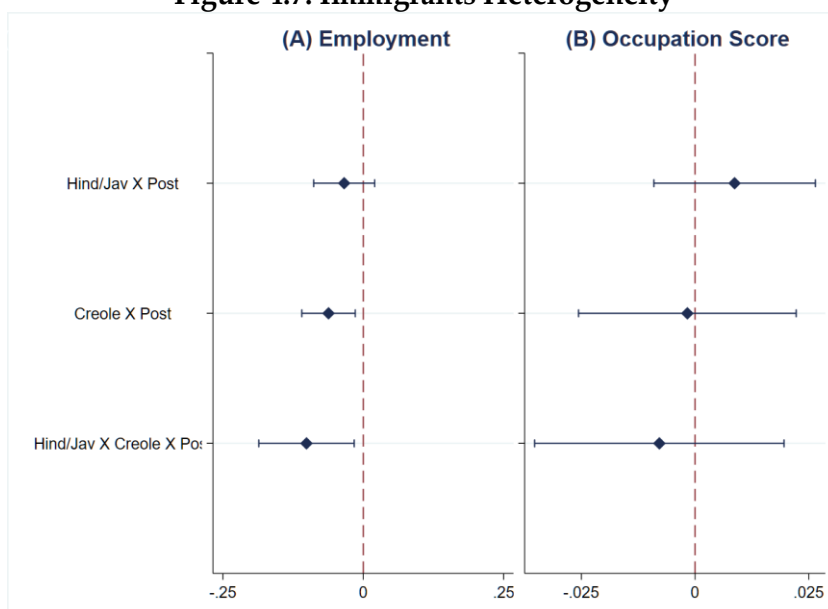
²⁸ For instance, Lewis, (2011) finds that wage competition from immigration in the U.S. is mainly borne by immigrants with poor English skills, whereas those who arrived at younger ages (and thus speak better English) are much closer substitutes for native workers.

²⁹ Indeed, immigrants with weaker language ability often specialize in manual or non-language-intensive work, while native workers shift toward communication-heavy tasks (Peri & Sparber, 2009). Consistent with this mechanism, Foged & Peri, (2016) studied a refugee inflow in Denmark and showed that less-educated natives moved into less manual (more language-intensive) occupations in response, ultimately raising their wages and employment.

³⁰ Using surveys on migrants in the Netherlands. SPVA, 1998 and 2002. SIM, 2006 and 2011, Gielen, Moroni, & Webbink (2022) show that Creoles speak Dutch better and are more likely to speak Dutch with their partner and with their children compared to Hindustani.

direct substitutability with native workers. This aligns with existing literature showing that immigrants and natives are often complements to natives, especially when language barriers are present (Ottaviano & Peri, 2012), and that language ability is a key determinant of the degree of substitutability between immigrant and native labor (Lewis, 2011). While we cannot directly confirm this mechanism in our data, our findings highlight that immigrants' language skills may be an important dimension to consider when evaluating the impacts of immigration on native labor market outcomes.

Figure 4.7: Immigrants Heterogeneity



Notes: The figure shows the effect of the post-1975 independence of Suriname on native worker's employment and occupational score for the low-skilled population. It plots the estimated effect and the associated 95 percent confidence intervals for the highly treated municipalities based on equation (3) for different groups of Surinamese immigrants, Creole and Hindustani & Javanese.

4.7 Conclusions

This paper examines the labor market impact of a sudden and unexpected migration shock in the Netherlands following Suriname's independence in 1975. Unlike many migration episodes studied in the literature,

Surinamese immigrants arrived with full labor market rights, creating an exogenous labor supply shock. Using a difference-in-differences approach, we document that municipalities with higher pre-existing Surinamese communities received a disproportionately large share of immigrants. Our findings indicate that this migration wave significantly reduced employment among low-skilled native workers, while high-skilled natives were largely unaffected. Despite concerns about wage suppression, we find no robust evidence that native occupational score was negatively impacted.

An important contribution of this study is its focus on a migration episode where immigrants arrived with relatively strong cultural and linguistic proximity to the destination country. In general, Surinamese immigrants possessed a certain level of Dutch proficiency due to the colonial history and education system shared between the Netherlands and Suriname. Although our data do not directly measure language skills, we explore potential heterogeneity in this regard by examining the composition of arriving migrants. Historical and institutional context suggests that Creole Surinamese immigrants likely had higher Dutch proficiency due to colonial education policies, while Hindustani and Javanese groups had lower proficiency. Our results provide circumstantial evidence consistent with this interpretation: municipalities receiving a higher share of Creole Surinamese experienced somewhat larger employment declines among low-skilled natives. These findings suggest that language proficiency may influence the degree of labor market competition between immigrants and native workers.

Our findings have important policy implications and highlight potential avenues for further research. They suggest that the economic impact of migration is shaped not only by the size of the inflow but also by the skill composition and language proficiency of immigrants. In the case of sudden and unexpected migration shocks, short-run competition with natives may intensify, particularly if immigrants possess relatively similar skills and native and immigrant workers have comparable levels of language proficiency. That is, higher language proficiency—typically expected to ease integration—may in the short run increase labor market

competition by enabling immigrants to compete more directly with natives who have similar skill profiles. This contrasts with evidence from more gradual or policy-managed migration contexts, or when immigrants possess relatively weaker cultural and linguistic proximity, where migrants often complement, rather than substitute for, native workers.

This highlights the importance of anticipating and managing large migration flows to mitigate adverse short-run effects on vulnerable groups. Over the longer term, policies that support immigrant integration remain essential. Initiatives such as job-specific training, language acquisition programs, and active labor market policies can help steer immigrants into roles that complement the native workforce, reduce labor market mismatches, and promote economic integration. Although our results suggest that language proficiency may initially heighten competition in certain segments of the labor market, facilitating language acquisition is still critical for promoting upward mobility and expanding opportunities for immigrants, while also supporting broader social and economic cohesion.

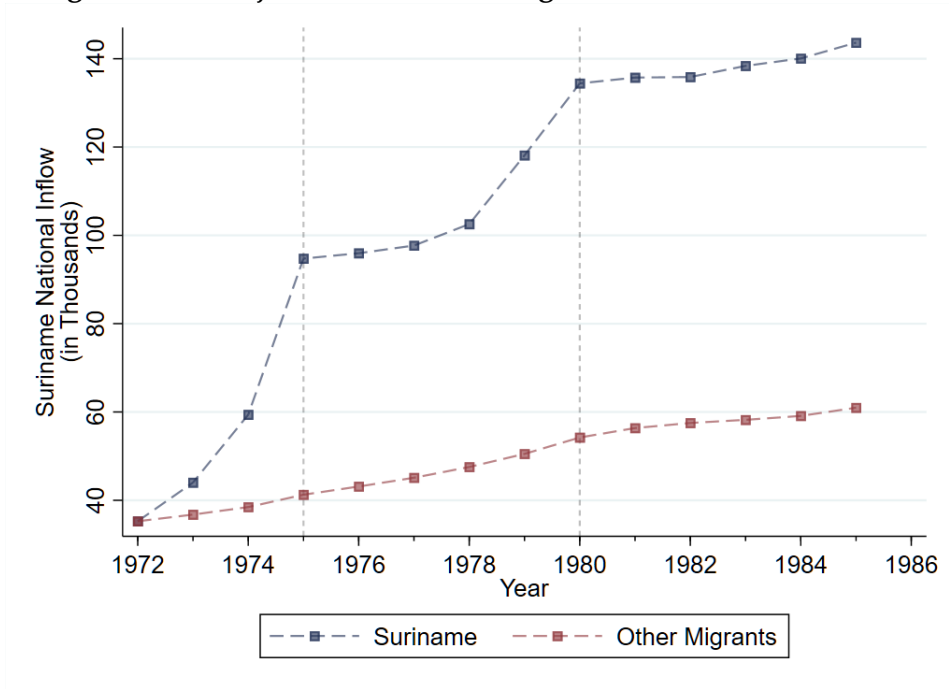
For instance, evidence from Gielen & Webbink (2023) shows that the multi-generational integration of Surinamese migrants facilitated convergence in long-term economic outcomes for immigrant families in the Netherlands. However, it remains unclear whether these intergenerational positive effects extend to native workers after experiencing sudden migration shocks. Understanding how migration shocks interact with local labor market structures and shape not only immigrant integration but also native adjustment is essential for designing more effective policies. Future research should examine whether displacement effects on natives persist or diminish over time and explore potential spillover effects on intergenerational mobility and long-term earnings for native populations.

While these findings provide valuable insights, some limitations remain that point to important avenues for future research. Although we document the effects of the migration shock on native employment, we do not fully disentangle the mechanisms driving these outcomes. The

observed employment level decline could stem from different channels, such as native workers exiting the labor force, moving to less-affected municipalities, or experiencing increased unemployment spells. Further research should explore these adjustment mechanisms in greater detail by analyzing internal migration patterns, unemployment insurance claims, or sectoral reallocation among native workers. Understanding these dynamics would provide a more comprehensive picture of how labor markets absorb large migration shocks.

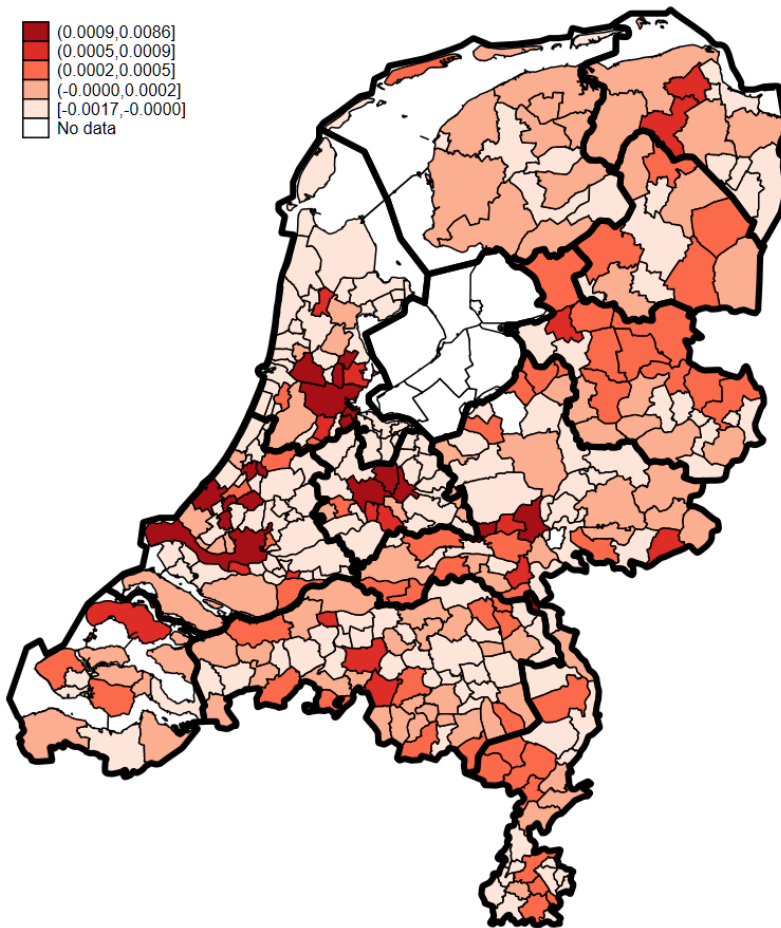
Appendix 4.A Additional Figures

Figure 4.A.1: Adjusted Stock of Immigrants: Suriname Vs. Others



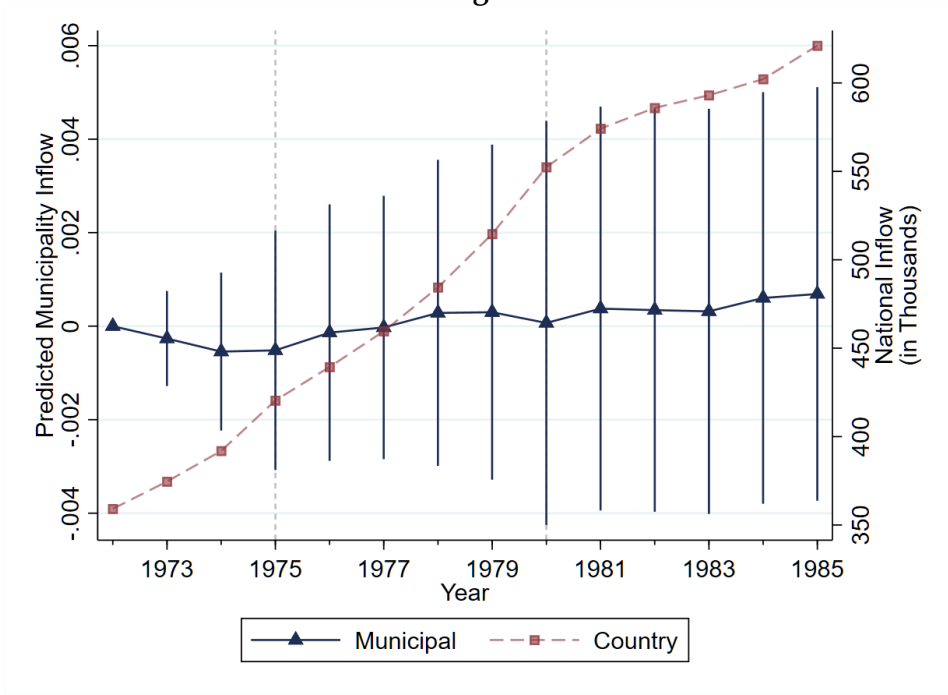
Notes: The figure shows the stock of immigrants from Suriname in the Netherlands from the years 1972 until 1985 and the stock of immigrants from other countries. The stock of immigrants' series is normalized so that it matches the stock of Surinamese series in 1972.

Figure 4.A.2: Distribution of Surinamese Conditional on Foreign Borns



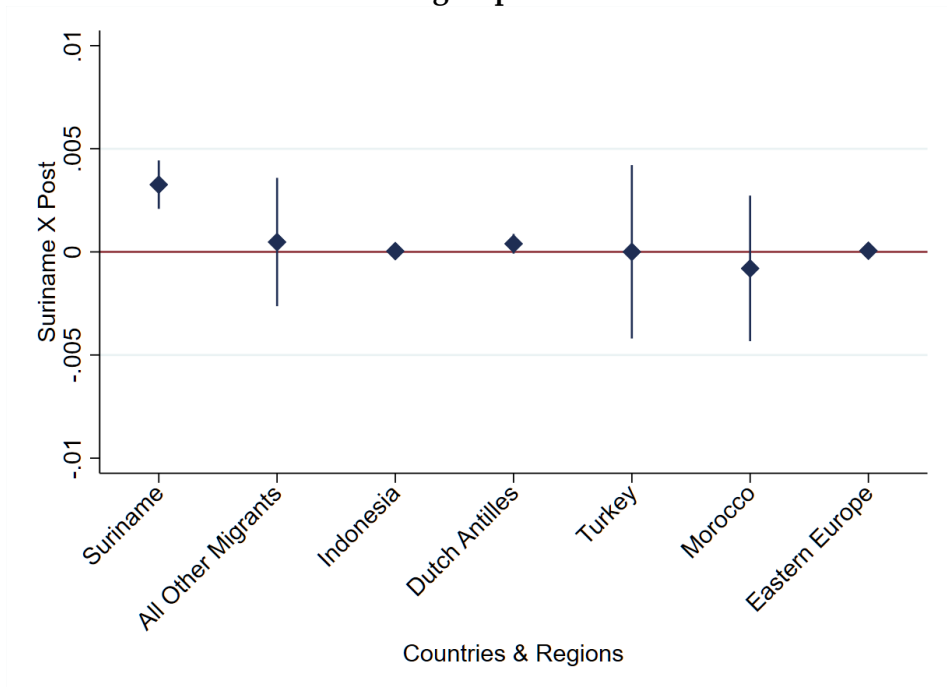
Notes: The Figure shows the variation of the exposure to Surinamese immigrants by plotting the residuals from a regression of Surinamese exposure measure on foreign born exposure measure and province fixed effects

Figure 4.A.3: Treat Municipalities and the Arrival of Other Immigrants



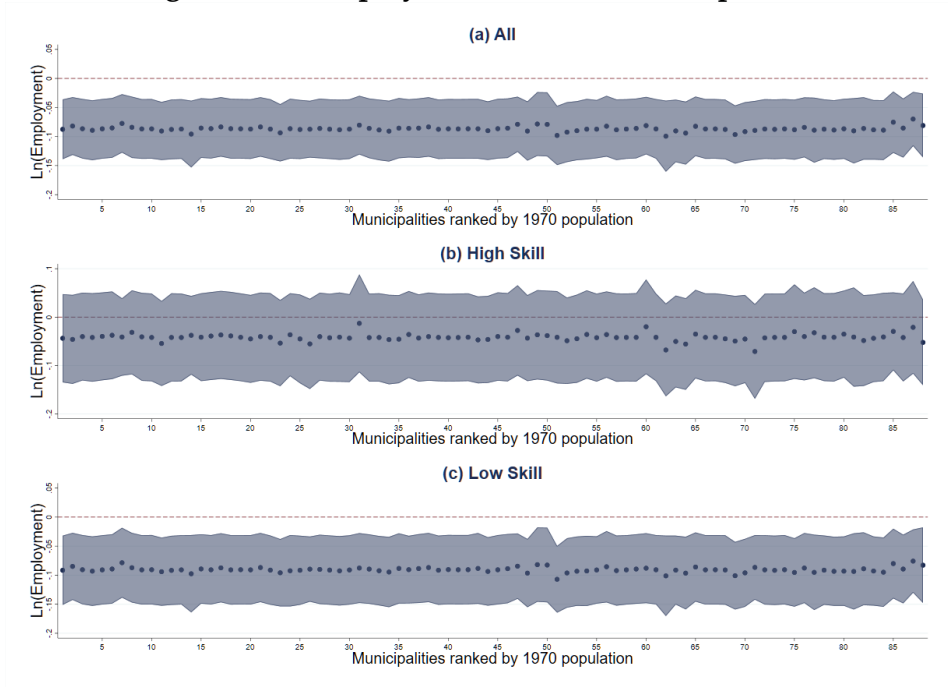
Notes: The figure shows the effect of the post-1975 independence of Suriname on immigration to the Netherlands, excluding the Suriname inflows. It plots the estimated inflow of all other immigrants and associated 95 percent confidence intervals for highly treated municipalities (panel A) based on the event study model (equation (4)). It also plots the national inflow of all other immigrants by year.

Figure 4.A.4: Difference-in-differences and arrival of other migrant groups



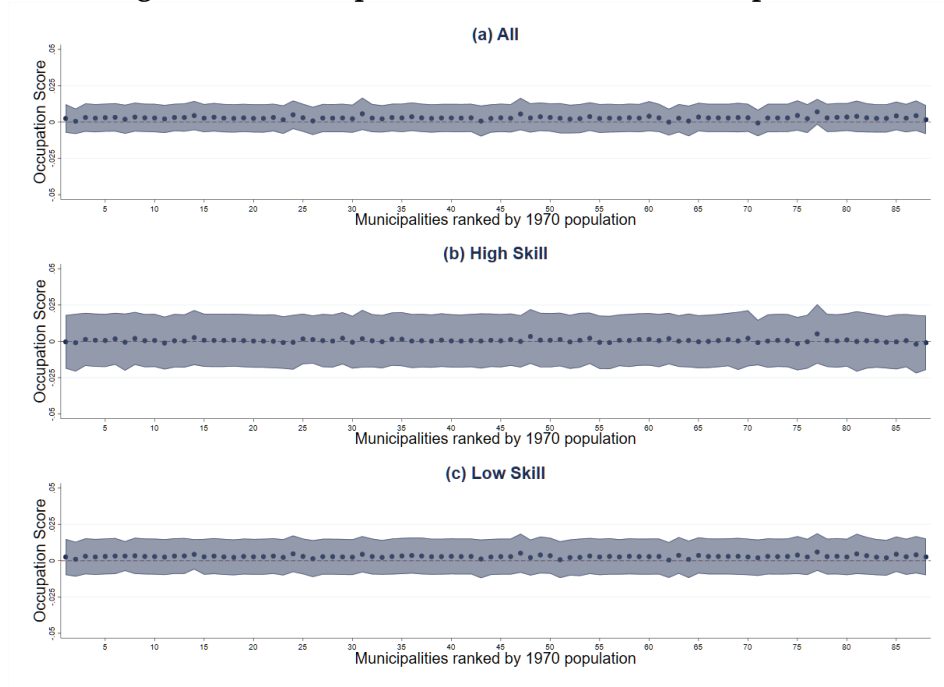
Notes: The figure shows the effect of the post-1975 independence of Suriname on immigration to the Netherlands. It plots the estimated inflow of Surinamese immigrants and associated 95 percent confidence intervals for highly treated municipalities based on the equation 3. It also plots the inflow of all other immigrants, immigrants from Indonesia, Dutch Antilles, Turkey, Morocco and Eastern Europe.

Figure 4.A.5: Employment Outliers: Municipalities



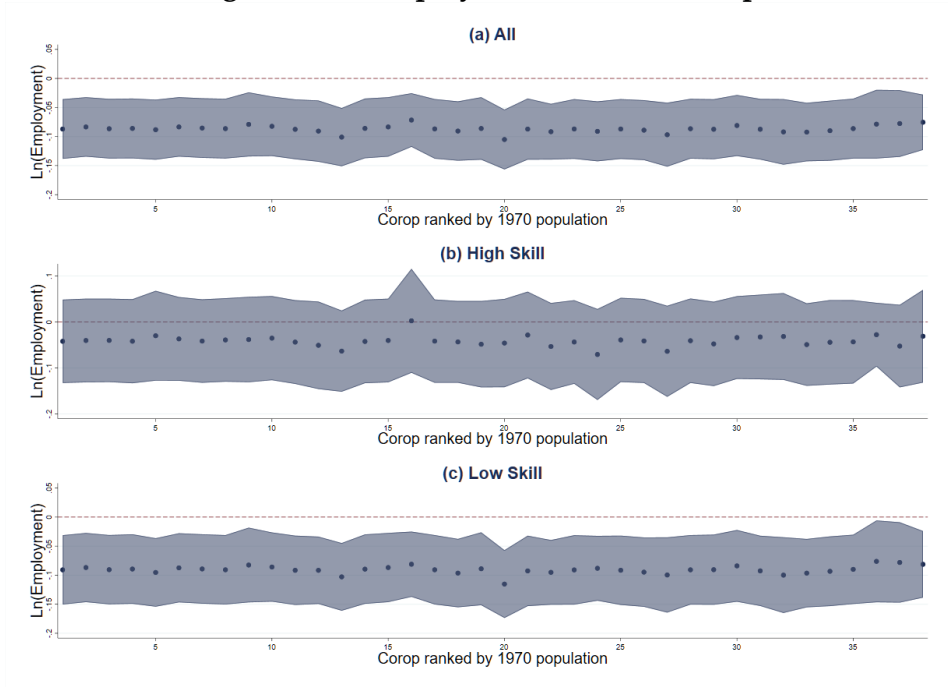
Notes: This figure presents the coefficient of the interaction between Surinamese exposure and the post-migration year excluding one by one each of the municipalities in the Netherlands associated 95 percent confidence intervals for highly treated municipalities based on the equation 3

Figure 4.A.6: Occupation Score Outliers: Municipalities



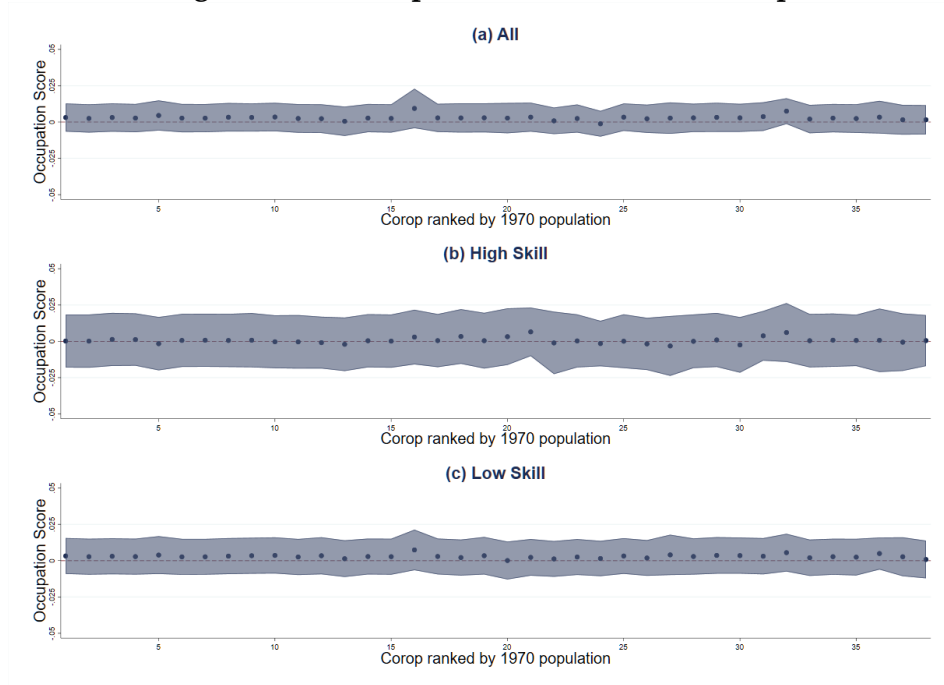
Notes: This figure presents the coefficient of the interaction between Surinamese exposure and the post-migration year excluding one by one each of the municipalities in the Netherlands and the associated 95 percent confidence intervals for highly treated municipalities based on the equation 3

Figure 4.A.7: Employment Outliers: Corop



Notes: This figure presents the coefficient of the interaction between Surinamese exposure and the post-migration year excluding one by one each of the NUTS-3 regions in the Netherlands and the associated 95 percent confidence intervals for highly treated municipalities based on the equation 3.

Figure 4.A.8: Occupation Score Outliers: Corop



Notes: This figure presents the coefficient of the interaction between Surinamese exposure and the post-migration year excluding one by one each of the NUTS-3 regions in the Netherlands and the associated 95 percent confidence intervals for highly treated municipalities based on the equation 3.

Appendix 4.B Additional Tables

Table 4.B.1: Descriptive Statistics & Balance Tests

Municipality Characteristics	Highly Exposed Mean (SD)	Low Exposed (Full) Mean (SD)	P-value (Full)	Low Exposed (Top 25%) Mean (SD)	P-value (Top 25%)
Share Low Skilled Workers	0.8868 (0.0456)	0.9151 (0.0496)	0.0008	0.9133 (0.0314)	0.0001
Share High Skilled Workers	0.1132 (0.0456)	0.0849 (0.0496)	0.0008	0.0867 (0.0314)	0.0001
Share Age Group (18-29 years old]	0.3647 (0.0458)	0.3765 (0.0492)	0.1595	0.3759 (0.0294)	0.0752
Share Age Group (30-49 years old]	0.4306 (0.0370)	0.4512 (0.0474)	0.0105	0.4422 (0.0278)	0.0393
Share Age Group (50-64 years old]	0.2047 (0.0295)	0.1722 (0.0431)	0.0000	0.1819 (0.0294)	0.0001
Share Men (Working)	0.7560 (0.0366)	0.8155 (0.0412)	0.0000	0.8028 (0.0368)	0.0000
Share Sector 1	0.0202 (0.0120)	0.0983 (0.0706)	0.0000	0.0684 (0.0586)	0.0000
Share Sector 2	0.3123 (0.0961)	0.4283 (0.0999)	0.0000	0.4330 (0.0820)	0.0000
Share Sector 3	0.3463 (0.0791)	0.2604 (0.0700)	0.0000	0.2677 (0.0521)	0.0000
Share Sector 4	0.3075 (0.0961)	0.1996 (0.0687)	0.0000	0.2166 (0.0642)	0.0000
Ln(Population)	11.7624 (0.9009)	9.9489 (0.7618)	0.0000	10.9605 (0.4505)	0.0000
Employment Rate	0.9808 (0.0126)	0.9810 (0.0207)	0.9497	0.9791 (0.0138)	0.5039
Occupational Score	42.2603 (1.9435)	39.8131 (2.7005)	0.0000	40.1460 (1.8229)	0.0000
Share Other Immigrants	0.0427 (0.0117)	0.0173 (0.0126)	0.0000	0.0217 (0.0117)	0.0000
Share Surinamese Immigrants	0.0039 (0.0023)	0.0006 (0.0007)	0.0000	0.0006 (0.0004)	0.0000

Notes: This table presents the pre-1975 shock descriptive statistics for the two groups of municipalities that we compare in our Difference in Differences estimations: municipalities with high and low Surinamese exposure. The table is divided on high exposed municipalities and low exposed municipalities, composed of all low exposed municipalities and those from the top 25% in terms of their population in 1971. The table presents the descriptive statistics for the main characteristics of the municipalities as well as the balance tests

Table 4.B.2: Stock of Immigrants

Dependent Variable	Stock of Immigrants as Share of 1970 population						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Suriname	All Other Immigrants	Indonesia	Dutch Antilles	Turkey	Morocco	Eastern Europe
Suriname Share X Post	0.00326*** (0.000527)	0.000478 (0.00140)	3.79e-05 (8.30e-05)	0.000394* (0.000215)	3.11e-06 (0.00189)	-0.000802 (0.00158)	5.72e-05 (0.000104)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province by Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,232	1,232	1,232	1,232	1,232	1,232	1,232

Notes: Robust standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table presents the coefficient of the interaction between Surinamese exposure and the post-migration flow. The Estimations control for share of foreign born in each municipality with municipality and province by year fixed effects. Column 1 looks at the inflow of Surinamese population while Columns 2, 3, 4, 5, 6 and 7 look at the inflow of all other immigrants, immigrants from Indonesia, Dutch Antilles, Turkey, Morocco and Eastern Europe, respectively.

Table 4.B.3: Robustness Check: NUTS 2 Trend Fixed Effect Model

	(1)	(2)	(3)
Panel A: Ln (Employment)			
	All	High Skill	Low Skill
Suriname Share X Post	-0.062** (0.025)	-0.001 (0.042)	-0.066** (0.030)
Panel B: Ln (Occupational Score)			
	All	High Skill	Low Skill
Suriname Share X Post	0.004 (0.005)	-0.002 (0.009)	0.003 (0.006)
Panel A: Mean Dep. Var.	10.49	8.24	10.37
Panel A: Impact at Mean	-0.59%	-0.01%	-0.64%
Panel B: Mean Dep. Var.	3.73	4.10	3.68
Panel B: Impact at Mean	0.11%	-0.05%	0.08%
Municipality FE	Yes	Yes	Yes
Province by Year FE	Yes	Yes	Yes
Observations	616	616	616

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. This table presents the coefficient of the interaction between Surinamese exposure and the post-migration year. The Estimations control for share of foreign born in each municipality with year and NUTS-2 trend by year fixed effects. Panel A look at the first outcome of interest, native employment, while panel B focuses on the second outcome of interest, occupational score. Column 1 presents the results of native employment and occupational score for the whole sample of natives living in each of the 82 municipalities. Column 2 performs the analysis on the sample to the low skilled natives for each municipality and Column 3 performs the analysis on the sample to the high skilled natives for each municipality.

Table 4.B.4: Robustness Check: Including Bartik

	(1)	(2)	(3)
Panel A: Ln (Employment)			
	All	High Skill	Low Skill
Suriname Share X Post	-0.080*** (0.025)	-0.037 (0.043)	-0.078*** (0.028)
Panel B: Ln (Occupational Score)			
	All	High Skill	Low Skill
Suriname Share X Post	0.003 (0.005)	-0.004 (0.010)	0.005 (0.006)
Panel A: Mean Dep. Var.	10.49	8.24	10.37
Panel A: Impact at Mean	-0.76%	-0.45%	-0.75%
Panel B: Mean Dep. Var.	3.73	4.10	3.68
Panel B: Impact at Mean	0.08%	-0.10%	0.14%
Municipality FE	Yes	Yes	Yes
Province by Year FE	Yes	Yes	Yes
Observations	616	616	616

Notes: Robust standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table presents the coefficient of the interaction between Surinamese exposure and the post-migration year. The Estimations control for share of foreign born in each municipality and a Bartik variable to account for region-specific, sector-driven demand trends or shocks with municipality and province by year fixed effects. Panel A look at the first outcome of interest, native employment, while panel B focuses on the second outcome of interest, occupational score. Column 1 presents the results of native employment and occupational score for the whole sample of natives living in each of the 82 municipalities. Column 2 performs the analysis on the sample to the low skilled natives for each municipality and Column 3 performs the analysis on the sample to the high skilled natives for each municipality.

Table 4.B.5: Excluding Specific Municipalities

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Groningen						
	Ln (Employment)			Ln (Occupational Score)		
	All	High Skill	Low Skill	All	High Skill	Low Skill
Suriname Share X Post	-0.089*** (0.026)	-0.041 (0.046)	-0.095*** (0.030)	0.003 (0.005)	-0.001 (0.009)	0.002 (0.006)
Panel B: Utrecht						
	All	High Skill	Low Skill	All	High Skill	Low Skill
Suriname Share X Post	-0.075*** (0.026)	-0.029 (0.040)	-0.080*** (0.030)	0.004 (0.005)	-0.000 (0.009)	0.004 (0.006)
Panel C: The Hague						
	All	High Skill	Low Skill	All	High Skill	Low Skill
Suriname Share X Post	-0.085*** (0.025)	-0.042 (0.045)	-0.089*** (0.030)	0.003 (0.005)	0.001 (0.009)	0.003 (0.006)
Panel D: Rotterdam						
	All	High Skill	Low Skill	All	High Skill	Low Skill
Suriname Share X Post	-0.070*** (0.023)	-0.021 (0.048)	-0.076*** (0.027)	0.004 (0.005)	-0.002 (0.010)	0.004 (0.006)
Panel E: Amsterdam						
	All	High Skill	Low Skill	All	High Skill	Low Skill
Suriname Share X Post	-0.081*** (0.027)	-0.052 (0.044)	-0.083** (0.033)	0.002 (0.005)	-0.001 (0.009)	0.003 (0.006)
Panel A: Mean Dep. Var.	10.49	8.24	10.37	3.73	4.10	3.68
Panel A: Impact at Mean	-0.85%	-0.50%	-0.92%	0.08%	-0.02%	0.05%
Panel B: Mean Dep. Var.	10.44	8.20	10.32	3.73	4.10	3.68
Panel B: Impact at Mean	-0.71%	-0.35%	-0.77%	0.11%	0.00%	0.11%
Panel C: Mean Dep. Var.	10.41	8.17	10.29	3.73	4.10	3.68
Panel C: Impact at Mean	-0.81%	-0.51%	-0.86%	0.08%	0.02%	0.08%
Panel D: Mean Dep. Var.	10.39	8.17	10.27	3.73	4.10	3.68
Panel D: Impact at Mean	-0.67%	-0.25%	-0.73%	0.11%	-0.05%	0.11%
Panel E: Mean Dep. Var.	10.38	8.15	10.26	3.73	4.10	3.68
Panel E: Impact at Mean	-0.77%	-0.63%	-0.80%	0.05%	-0.02%	0.08%
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Province by Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	609	609	609	609	609	609

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. This table presents the coefficient of the interaction between Surinamese exposure and the post-migration year excluding one by one the top 5 biggest municipalities in the Netherlands. The Estimations control for share of foreign born in each municipality with municipality and province by year fixed effects. Panel A excludes Groningen from the estimation while Panels B, C, D and E exclude Utrecht, The Hague, Rotterdam and Amsterdam, respectively.

Table 4.B.6: Continuous Treatment

	(1)	(2)	(3)
Panel A: Ln (Employment)			
	All	High Skill	Low Skill
Suriname Share X Post	-31.31*** (8.82)	-0.02 (12.78)	-35.14*** (9.17)
Panel B: Ln (Occupational Score)			
	All	High Skill	Low Skill
Suriname Share X Post	1.00 (1.14)	3.22** (1.58)	-0.70 (1.07)
Panel A: Mean Dep. Var.	9.85	7.41	9.75
Panel A: Impact at Mean	-0.55%	0.00%	-0.63%
Panel B: Mean Dep. Var.	3.66	4.09	3.61
Panel B: Impact at Mean	0.05%	0.13%	0.03%
Municipality FE	Yes	Yes	Yes
Province by Year FE	Yes	Yes	Yes
Observations	616	616	616

Notes: Robust standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table presents the coefficient of the interaction between a continuous measure of Surinamese exposure and the post-migration year. The Estimations control for share of foreign born in each municipality with municipality and province by year fixed effects. Panel A look at the first outcome of interest, native employment, while panel B focuses on the second outcome of interest, occupational score. Column 1 presents the results of native employment and occupational score for the whole sample of natives living in each of the 82 municipalities. Column 2 performs the analysis on the sample to the low skilled natives for each municipality and Column 3 performs the analysis on the sample to the high skilled natives for each municipality.

Table 4.B.7: Top 5% as treatment group

	(1)	(2)	(3)
Panel A: Ln (Employment)			
	All	High Skill	Low Skill
Suriname Share X Post	-0.093*** (0.023)	-0.004 (0.060)	-0.111*** (0.028)
Panel B: Ln (Occupational Score)			
	All	High Skill	Low Skill
Suriname Share X Post	0.013* (0.007)	0.016 (0.014)	0.008 (0.008)
Panel A: Mean Dep. Var.	10.59	8.41	10.45
Panel A: Impact at Mean	-0.89%	-0.05%	-1.07%
Panel B: Mean Dep. Var.	3.75	4.10	3.70
Panel B: Impact at Mean	0.35%	0.39%	0.22%
Municipality FE	Yes	Yes	Yes
Province by Year FE	Yes	Yes	Yes
Observations	616	616	616

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. This table presents the coefficient of the interaction between Surinamese exposure and the post-migration year. Treatment is defined as the municipalities in the top 10% of the Surinamese exposure measure. The Estimations control for share of foreign born in each municipality with municipality and province by year fixed effects. Panel A looks at the first outcome of interest, native employment, while panel B focuses on the second outcome of interest, occupational score. Column 1 presents the results of native employment and occupational score for the whole sample of natives living in each of the 88 municipalities. Column 2 performs the analysis on the sample to the low skilled natives for each municipality and Column 3 performs the analysis on the sample to the high skilled natives for each municipality.

Table 4.B.8: Different Employees Thresholds for Control Cities

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Above 10 thousand	Ln (Employment)			Ln (Occupational Score)		
	All	High Skill	Low Skill	All	High Skill	Low Skill
Suriname Share X Post	-0.149*** (0.036)	-0.072* (0.040)	-0.144*** (0.035)	-0.000 (0.005)	-0.003 (0.005)	0.003 (0.006)
Observations	2,163	2,161	2,163	2,163	2,161	2,163
Panel B: Above 20 thousand	Ln (Employment)			Ln (Occupational Score)		
	All	High Skill	Low Skill	All	High Skill	Low Skill
Suriname Share X Post	-0.122*** (0.027)	-0.058* (0.035)	-0.119*** (0.028)	-0.001 (0.005)	-0.002 (0.005)	0.002 (0.006)
Observations	1,323	1,323	1,323	1,323	1,323	1,323
Panel C: Above Median	Ln (Employment)			Ln (Occupational Score)		
	All	High Skill	Low Skill	All	High Skill	Low Skill
Suriname Share X Post	-0.111*** (0.027)	-0.044 (0.037)	-0.110*** (0.030)	0.000 (0.006)	0.001 (0.005)	0.002 (0.006)
Observations	1,218	1,218	1,218	1,218	1,218	1,218
Panel D: Above 30 thousand	Ln (Employment)			Ln (Occupational Score)		
	All	High Skill	Low Skill	All	High Skill	Low Skill
Suriname Share X Post	-0.109*** (0.031)	-0.062 (0.043)	-0.113*** (0.036)	0.002 (0.004)	0.001 (0.010)	0.002 (0.006)
Observations	784	784	784	784	784	784
Panel E: Above 40 thousand	Ln (Employment)			Ln (Occupational Score)		
	All	High Skill	Low Skill	All	High Skill	Low Skill
Suriname Share X Post	-0.075*** (0.025)	-0.023 (0.035)	-0.074** (0.030)	0.001 (0.005)	0.000 (0.010)	0.001 (0.005)
Observations	469	469	469	469	469	469
Panel A: Mean Dep. Var.	10.49	8.24	10.37	3.73	4.10	3.68
Panel A: Impact at Mean	-1.42%	-0.87%	-1.39%	0.00%	-0.07%	0.08%
Panel B: Mean Dep. Var.	10.49	8.24	10.37	3.73	4.10	3.68
Panel B: Impact at Mean	-1.16%	-0.70%	-1.15%	-0.03%	-0.05%	0.05%
Panel C: Mean Dep. Var.	10.49	8.24	10.37	3.73	4.10	3.68
Panel C: Impact at Mean	-1.06%	-0.53%	-1.06%	0.00%	0.02%	0.05%
Panel D: Mean Dep. Var.	10.49	8.24	10.37	3.73	4.10	3.68
Panel D: Impact at Mean	-1.04%	-0.75%	-1.09%	0.05%	0.02%	0.05%
Panel E: Mean Dep. Var.	10.57	8.29	10.45	3.73	4.10	3.68
Panel E: Impact at Mean	-0.71%	-0.28%	-0.71%	0.03%	0.00%	0.03%
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Province by Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table presents the coefficient of the interaction between Surinamese exposure and the post-migration year. Treatment is defined as the municipalities in the top 10% of the Surinamese exposure measure while control are the municipalities above the median size. The Estimations control for share of foreign born in each municipality with municipality and province by year fixed effects. Panel A uses municipalities with above 10 thousand employees as control municipalities, while Panels B, C, D and E look at municipalities with above 20 thousand, above median, above 30 thousand and above 40 thousand employees as control municipalities.

Table 4.B.9: Placebo in Country of Origin

	(1)	(2)	(3)
Panel A: Ln (Employment)			
	All	High Skill	Low Skill
Turkish Share X Post	-0.020 (0.020)	-0.024 (0.033)	-0.005 (0.023)
Panel B: Ln (Occupational Score)			
	All	High Skill	Low Skill
Turkish Share X Post	-0.001 (0.003)	0.026*** (0.009)	-0.000 (0.004)
Panel A: Mean Dep. Var.	9.87	7.55	9.76
Panel A: Impact at Mean	0.25%	-0.87%	0.52%
Panel B: Mean Dep. Var.	3.68	4.01	3.65
Panel B: Impact at Mean	-0.27%	0.12%	-0.05%
Municipality FE	Yes	Yes	Yes
Province by Year FE	Yes	Yes	Yes
Observations	616	616	616

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. This table presents the coefficient of the interaction between Surinamese exposure and the post-migration year. Treatment is defined as the municipalities in the top 5% of the Turkish exposure measure. The Estimations control for share of foreign born in each municipality with municipality and province by year fixed effects. Panel A looks at the first outcome of interest, native employment, while panel B focuses on the second outcome of interest, occupational score. Column 1 presents the results of native employment and occupational score for the whole sample of natives living in each of the 189 municipalities. Column 2 performs the analysis on the sample to the low skilled natives for each municipality and Column 3 performs the analysis on the sample to the high skilled natives for each municipality.

Table 4.B.10: Natives Heterogeneity

	(1)	(2)	(3)	(4)	(6)
Panel A: Ln (Employment)					
	18-29	30-49	50-64	Male	Female
Suriname Share X Post	-0.089*** (0.031)	-0.071** (0.033)	-0.116*** (0.037)	-0.095*** (0.025)	-0.101 (0.062)
Panel B: Ln (Occupational Score)					
	18-29	30-49	50-64	Male	Female
Suriname Share X Post	0.007 (0.008)	0.006 (0.009)	-0.014 (0.011)	0.001 (0.007)	0.007 (0.009)
Panel A: Mean Dep. Var.	9.39	9.49	8.79	10.08	8.97
Panel A: Impact at Mean	-0.95%	-0.75%	-1.32%	-0.94%	-1.13%
Panel B: Mean Dep. Var.	3.67	3.70	3.67	3.69	3.66
Panel B: Impact at Mean	0.19%	0.16%	-0.38%	0.03%	0.19%
Municipality FE	Yes	Yes	Yes	Yes	Yes
Province by Year FE	Yes	Yes	Yes	Yes	Yes
Observations	616	616	616	616	616

Notes: Robust standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table presents the coefficient of the interaction between Surinamese exposure and the post-migration year. The Estimations control for share of foreign born in each municipality with municipality and province by year fixed effects. Panel A looks at the first outcome of interest, native employment, while panel B focuses on the second outcome of interest, occupational score. The focus on this table is on the outcomes of the low-skilled natives. Column 1 looks at the age group 18-29, Column 2 at the age group 30-49 and Column 3 at the age group 50-64. Columns 5 and 6 break down the analysis by gender.

Table 4.B.11: Migrant Diversity

	(1)	(2)	(3)
Panel A: Ln (Employment)			
	All	High Skill	Low Skill
Hindustani & Javanese Share X Post	-0.039 (0.024)	-0.007 (0.041)	-0.034 (0.027)
Creole Share X Post	-0.035 (0.024)	0.006 (0.068)	-0.062** (0.024)
Hindustani & Javanese Share X Creole Share X Post	-0.108** (0.043)	-0.011 (0.062)	-0.101** (0.043)
Panel B: Ln (Occupational Score)			
	All	High Skill	Low Skill
Hindustani & Javanese Share X Post	0.007 (0.006)	0.007 (0.015)	0.009 (0.009)
Creole Share X Post	0.002 (0.010)	-0.008 (0.012)	-0.002 (0.012)
Hindustani & Javanese Share X Creole Share X Post	-0.000 (0.010)	0.029* (0.017)	-0.008 (0.014)
Panel A: Mean Dep. Var.	10.59	8.38	10.46
Panel A: Impact at Mean Hind/Jav	-0.37%	-0.08%	-0.33%
Panel A: Impact at Mean Creole	-0.33%	0.07%	-0.59%
Panel A: Impact at Mean Hind/Jav & Creole	-1.02%	-0.13%	-0.97%
Panel B: Mean Dep. Var.	3.74	4.10	3.69
Panel B: Impact at Mean Hind/Jav	0.19%	0.17%	0.24%
Panel B: Impact at Mean Creole	0.05%	-0.20%	-0.05%
Panel B: Impact at Mean Hind/Jav & Creole	0.00%	0.71%	-0.22%
Municipality FE	Yes	Yes	Yes
Province by Year FE	Yes	Yes	Yes
Observations	616	616	616

Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. This table presents the coefficients of the interaction between Hindustani & Javanese Share and Creole Share and the post-migration year and the triple interaction between Hindustani & Javanese Share, Creole Share and the post-migration year. The Estimations control for share of foreign born in each municipality with municipality and province by year fixed effects. Panel A look at the first outcome of interest, native employment, while panel B focuses on occupational score. Column 1 presents the results of native employment and occupational score for the whole sample of natives. Column 2 performs the analysis on the sample to the low skilled natives for each municipality and Column 3 performs the analysis on the sample to the high skilled natives for each municipality.

5

Conclusions and Recommendations

This dissertation comprises three empirical studies and examines the drivers that shape patterns of international labor mobility, as well as its effects on the labor market outcomes of natives in destination countries. It addresses the overarching research question: how do economic and non-economic factors shape the patterns and consequences of international labor mobility? I showed that both economic and non-economic factors significantly influence individuals' decisions to move internationally. Moreover, I demonstrated that under certain circumstances, such as a sudden and unexpected labor supply shock resulting from a wave of immigration, native workers whose skills competed with those of newly arrived immigrants may experience a decline in employment. The results from this dissertation also speak to existing migration theories. The findings confirm key predictions of traditional economic models of migration, such as those by Sjaastad, (1962) and Mincer, (1978), which emphasize that individuals respond to

economic incentives. At the same time, they support more recent theoretical extensions that incorporate non-economic drivers, including cultural proximity and social integration barriers (Adsera & Pytlikova, 2015; Belot & Ederveen, 2012; Falck et al., 2012). Finally, the findings from these studies highlight several policy-relevant implications and suggest avenues for future research, while also acknowledging the limitations discussed throughout the dissertation.

Main findings

Chapter 2 provides evidence that non-economic factors are important drivers of individuals' decisions to move across borders, by asking how cultural distance between regions affects cross-border commuting. In particular, this chapter examines the role of cultural distance, measured by differences in historical dialects, in influencing cross-border commuting between Belgium and the Netherlands. We find that geographical distance is the main factor explaining commuting patterns across the border, as one would expect. Nevertheless, cultural dissimilarity also plays a significant role. Specifically, a one-standard-deviation increase in cultural distance between municipalities leads to a robust 11.6% decrease in commuting flows from Belgium to the Netherlands. Our results further suggest that at short distances, economic factors—such as lower commuting costs—are the dominant influence. In contrast, at longer distances (approximately 55 to 60 kilometers), cultural dissimilarity becomes more pronounced. This aligns with theoretical frameworks emphasizing the role of cultural proximity in mobility decisions and highlights the limitations of purely economic explanations for cross-border commuting behavior. These findings constitute an empirical contribution to migration theories by demonstrating that both economic incentives and cultural barriers jointly shape the dynamics of cross-border mobility. These findings demonstrate that even in contexts of European integration, non-economic barriers, such as cultural distance, continue to constrain mobility.

Building on Chapter 2's focus on non-economic factors, **Chapter 3** shifts attention to economic influences by asking how financial incentives

affect migration decisions. Specifically, we study how a sudden and unexpected policy change that shortened the duration of the tax break available to high-skilled migrants to the Netherlands affects their decisions to remain in the host country. We show that migrants strongly increase their likelihood of leaving the Netherlands. However, our findings show that this effect is almost entirely driven by out-migration from the top 1% of the income distribution, and no migration response was detected for those below the 95th percentile of earnings. Individuals in the top 1% of the income distribution decrease their length of stay by 11.1% and are 18.3% less likely to remain beyond the end of the five years that they receive the tax break. 'Highly mobile' individuals, those who have arrived from a country that is not their country of origin/nationality, also react in terms of both intensive and extensive duration margins when among the top 5% of earners. Together, the results from Chapter 2 and Chapter 3 highlight that both cultural and economic factors shape international mobility patterns. They influence not only the decision to move but also how long individuals remain in their destination.

Having established the importance of both economic and non-economic factors in shaping individuals' decisions to move across borders, **Chapter 4** turns to the consequences of migration by asking to what extent natives' labor market outcomes in the destination country are affected by immigration. Specifically, we analyze the labor market effects of a large, unexpected migration wave to the Netherlands following Suriname's independence in 1975. Immigrants arrived in the Netherlands with full labor market rights and relatively strong Dutch language skills. We show that municipalities more exposed to this labor supply shock experienced a decline in employment levels among low-skilled natives, while there was no significant impact on high-skilled employment or occupational scores. Among low-skilled natives, we find that the adverse employment effects are present across all age groups examined, as well as among men, with no detectable impact on employment levels for women. The effects on occupational scores, which capture the socio-economic standing of occupations based on the earnings and educational

attainment of individuals in those roles,¹ within low-skilled natives are muted across all groups studied. This evidence is aligned with theoretical models of labor market competition, where immigration can generate displacement effects among native workers with similar skills, particularly in the short term. It also suggests that non-economic factors such as language and cultural proximity may reinforce competition, depending on the context.

Even though, in general, Surinamese immigrants arrived in the Netherlands with relatively strong cultural and linguistic proximity, we also find circumstantial evidence that municipalities receiving more Creole-background immigrants, who presumably have stronger Dutch proficiency, experienced slightly larger adverse employment effects than those receiving more Hindustani or Javanese immigrants. Altogether, the findings from this Chapter demonstrate that while international mobility offers opportunities for individuals, it can also have consequences in destination labor markets, particularly for native workers whose skills compete with those of the newly arrived immigrants.

A key strength of this dissertation is its focus on identifying causal effects, which enhances the policy relevance of the findings. While randomized controlled trials (RCTs) represent the gold standard for causal inference, such experiments are often infeasible in the context of international labor mobility. Instead, each chapter leverages plausibly exogenous variation or natural experiments as sources of identification, combined with empirical strategies such as quasi-experimental designs, to isolate causal impacts and address the overarching research question of how economic and non-economic factors shape the patterns and consequences of international labor mobility. **Chapter 2** exploits spatial variation in historical dialects to proxy for cultural distance across municipalities located on either side of the Belgium–Netherlands border, allowing for the causal estimation of non-economic barriers to cross-border commuting, conditional on geographical proximity. **Chapter 3**

¹ We used the International Socio-Economic Index of occupational status (ISEI) as a measure (Ganzeboom et al., 1992)

takes advantage of a sudden and unanticipated policy reform that shortened the duration of a preferential tax break for high-skilled migrants from specific countries and cohorts and applies a difference-in-differences approach to causally estimate the impact of taxation on emigration decisions. **Chapter 4** analyzes a large, unexpected migration shock to the Netherlands following Suriname's independence in 1975 and uses variation in immigrant exposure across municipalities to causally identify local labor market impacts, again employing a difference-in-differences framework. In all three chapters, extensive robustness checks are conducted to validate the empirical findings, including alternative specifications, alternative control groups, sensitivity analysis and placebo tests. By establishing causal relationships rather than mere correlations, the findings presented in this dissertation offer a sound empirical basis for drawing policy implications.

Policy Implications

Building on this foundation, the policy implications derived from the three studies offer a comprehensive perspective on the drivers and consequences of international labor mobility. Although all chapters focus on the Netherlands, the contexts explored differ substantially, from cross-border commuting within the EU, to tax-induced emigration of highly skilled individuals, to the labor market effects of a large migration wave. Nevertheless, across these diverse settings, the findings demonstrate that both economic and non-economic factors influence mobility patterns, and that these patterns, in turn, can have significant effects in destination countries. Together, these insights provide comprehensive lessons with several policy-relevant implications.

For instance, while the findings from **Chapter 2** demonstrate that non-economic factors matter for cross-border commuting, we also show that within the Netherlands, cultural dissimilarity between municipalities has a weaker effect on commuting flows. That is, when a national border is present, these cultural differences become more pronounced, as institutional and administrative barriers amplify their impact and continue to hinder cross-border labor mobility. Despite European

integration and the free movement of goods, services, and labor, our findings provide a detailed picture of how cultural differences continue to shape mobility patterns across borders, even at short distances. The results in this chapter underscore the need for policies that extend beyond economic or infrastructural improvements and actively address the role of cultural proximity in promoting cross-border labor market integration. For instance, initiatives such as EU Interreg projects, which promote language learning and mutual cultural understanding, can help mitigate these barriers and, in turn, enhance labor market matching and regional economic performance in border areas.

The findings of **Chapter 3** have several policy-relevant implications. They reveal that tax-induced migration responses are concentrated among top earners, with no detectable effects for individuals lower in the income distribution. This highlights that highly mobile, high-income individuals are particularly responsive to changes in tax rates, making them a key group to consider when designing tax policies aimed at attracting or retaining skilled workers. At the same time, the results point to the distortionary effects of international tax competition, as the most mobile top earners become more likely to relocate to countries offering similar tax incentives following the shortening of the tax break. These patterns highlight the difficulties individual countries face in retaining top talent when implementing unilateral tax changes. To mitigate these challenges, substantial tax reforms would ideally be coordinated at the level of larger entities, such as the European Union or the OECD, rather than introduced in isolation by individual countries. While current efforts, such as the OECD's Inclusive Framework on Base Erosion and Profit Shifting (BEPS), are focused on corporate taxation, they reflect a broader recognition of the need for coordinated approaches to reduce harmful tax competition. This logic should potentially be extended to areas such as preferential tax regimes for highly mobile individuals.

Lastly, the findings from **Chapter 4** suggest that the economic impact of migration depends not only on the size of the inflow but also on the skill composition and language proficiency of immigrants. In

particular, short-run competition with natives may intensify if immigrants possess relatively similar skills and high levels of proficiency in the local language. That is, higher language proficiency—which is typically seen as easing integration—may, in the short run, increase labor market competition by enabling immigrants to compete more directly with natives who have similar skill profiles. These results highlight the importance of anticipating and managing large migration flows to mitigate adverse short-run effects on vulnerable groups. Over the longer term, policies that support immigrant integration remain essential. Initiatives such as job-specific training, language acquisition programs, and active labor market policies can help steer immigrants into roles that complement the native workforce, reduce labor market mismatches, and promote economic integration. Although our results suggest that language proficiency may initially heighten competition in specific segments of the labor market, facilitating language acquisition is still critical for supporting broader social cohesion.

Limitations & Avenues for Future Research

While the three chapters of this thesis provide new insights into the drivers and consequences of international labor mobility and offer several policy-relevant implications, they also face certain limitations that point to important directions for future research. Some of these limitations stem from data constraints or the specific scope of each chapter. In contrast, others reflect the broader challenges of studying how individuals make mobility decisions and how destination labor markets respond to migration. Addressing these limitations in future work will be essential to deepen our understanding of these dynamics and to provide an even stronger evidence base to inform policy. The nature of these limitations differs across chapters, as discussed below.

As discussed in **Chapter 2**, our administrative data only cover individuals living in Belgium and working in the Netherlands, excluding

those who commute in the opposite direction.² While this is a significant limitation, it does not diminish the contribution of our study as a first step towards a better understanding non-economic barriers to cross-border commuting. Moreover, our analysis focuses on flows from the Dutch-speaking part of Belgium (i.e., Flanders and Brussels) to the Netherlands. Future research should expand this work to other border regions where cultural heterogeneity is greater, allowing for a broader assessment of how cultural distance interacts with language, history, and other non-economic factors in shaping mobility. In addition, exploring how these cultural barriers evolve over time or respond to targeted policy interventions, such as language-learning and mutual cultural understanding programs, would further deepen our understanding of the mechanisms at play.

Turning to **Chapter 3**, while it provides robust evidence on how the withdrawal of preferential tax benefits prompts emigration among top earners, some limitations remain that open avenues for future research. First, the analysis focuses on emigration responses but does not examine how the same policy change may have affected immigration flows from the countries targeted by the reform. Existing studies indicate that preferential tax schemes can significantly attract highly skilled migrants. For instance, evidence from Denmark suggests that such policies attract individuals at the very top of the earnings distribution (Kleven, Landais, Saez, & Schultz, 2014), whereas recent research on the Netherlands (Timm, Giuliadori, & Muller, 2025) highlights that these schemes can also influence migration decisions around a new income threshold implemented. This highlights the importance of examining how reducing such benefits impacts immigration inflows across different income segments.

Second, there is evidence documenting the crucial role of top earners and highly skilled individuals in driving innovation,

² In 2019, a total of 34,865 individuals commuted from Belgium to the Netherlands, which is more than twice the number of individuals commuting in the opposite direction, 15,570 (Eurostat, 2020).

productivity, and learning spillovers (see, for instance, Bender, Bloom, Card, Van Reenen, & Wolter, 2018; Bloom et al., 2019). While the relationship between individual taxation and innovation has been explored for selective groups, such as inventors in the United States (Akcigit, Grigsby, Nicholas, & Stantcheva, 2021), less is known about a broader population of high-income individuals. Our study does not directly examine how policy-induced out-migration among this group affects broader economic outcomes in the host country. Understanding whether and how tax-induced mobility of top earners translates into long-term consequences for productivity and innovation remains a crucial area for future research. Finally, recent changes to the Dutch 30% ruling, including the 2025 introduction of a cap on eligible income (€233,000 per annum), speak directly to our findings and provide a natural experiment to assess whether restricting the generosity of tax breaks along other dimensions similarly affects mobility patterns.

In **Chapter 4**, while we document the effects of the migration shock on native employment, we do not fully disentangle the mechanisms driving these outcomes. The observed decline in employment levels could stem from various channels, such as native workers exiting the labor force, relocating to less-affected municipalities, or experiencing prolonged unemployment spells. Further research should explore these adjustment mechanisms in greater detail by analyzing internal migration patterns, unemployment insurance claims, or sectoral reallocation among native workers. Understanding these dynamics would provide a more comprehensive picture of how labor markets absorb large migration shocks.

Finally, although existing research documents the multi-generational integration of Surinamese migrants and how it facilitated convergence in long-term economic outcomes for immigrant families in the Netherlands (Gielen & Webbink, 2023), it remains unclear whether these long-term positive effects extend to native workers after experiencing sudden migration shocks, particularly when immigrants possess relatively strong cultural and linguistic proximity to the destination country. Understanding how migration shocks interact with local labor market

structures and shape not only immigrant integration but also native adjustment in the long run is essential for designing more effective policies. Future research should examine whether displacement effects on natives persist or diminish over time and explore potential spillover effects on intergenerational mobility and long-term earnings for native populations.

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Impact

The research presented in this dissertation had as its primary objective to advance our understanding of the drivers that shape the patterns of international labor mobility, as well as its effects on the labor market outcomes of natives in destination countries. In today's world, where globalization has significantly reduced the costs of international labor mobility and conflict- and climate change-induced migration are increasingly plausible.¹ Therefore, rigorously building an empirical evidence base on the causes and consequences of international labor mobility is crucial for offering a sound basis for drawing informed policy implications and improving societal well-being.

While EU border regions cover 40% of the EU's territory, account for 30% of the EU population, and produce 30% of the EU's GDP,² they often function as peripheral areas within their respective countries, experiencing lower economic growth in terms of GDP and employment, higher unemployment rates and lower population growth.³ Cross-border labor mobility is, therefore, viewed as an important adjustment mechanism in the labor market to enhance economic performance, stimulate innovation, promote regional integration, and foster economic convergence among regions. Despite Europe's effort for regional integration and the free movement of goods, services, and labor, in 2019, only 3.5% of European citizens working in EU countries were citizens of another Member State and around 1% lived in one EU country and crossed the border to work in another.⁴ These numbers suggest that

¹ UNHCR, (2020); World Bank, (2018)

² European Commission, (2017)

³ Brakman, Garretsen, Van Marrewijk, & Oumer, (2012); Broersma, Edzes, & Dijk, (2020); van Gemeren, Verstraten, Tichelbäcker, & Zwaneveld, (2016)

⁴ Eurostat (2020).

despite Europe's efforts toward regional integration by the removal of institutional barriers, labor markets have not yet fully integrated.

Using granular, spatial variation in historical dialects to capture cultural proximity and integrate this measure into cross-border commuting models, **Chapter 2** introduces a novel scientific perspective on the reasons behind this phenomenon. Although EU citizens are, in principle, free to work across national borders, we show that an increase in cultural distance between Belgium and Dutch municipalities leads to a robust decrease in cross-border commuting flows between them. This highlights that cultural proximity might be a crucial determinant of low mobility in terms of relocation or work, even if there are better economic prospects elsewhere. These results have notable implications for policymaking to realize the potential of cross-border labor mobility as an economic adjustment mechanism for improving individuals' well-being and enhancing regional economic performance. Our findings suggests that policies may need to go beyond economic or infrastructural improvements and actively address the role of cultural proximity in Europe. For instance, initiatives such as EU Interreg projects, which promote language learning and mutual cultural understanding, can help mitigate these barriers and, in turn, enhance labor market matching and regional economic performance in border areas.

The findings from **Chapter 2** also highlight another important aspect of international labor mobility: at short distances, economic factors—such as lower commuting costs—are the dominant factor influencing cross-border mobility, whereas at longer distances (around 55 to 60 kilometers), the impact of cultural dissimilarity becomes more pronounced. Consequently, both economic and non-economic factors influence decisions regarding international mobility.

Chapter 3 complements these findings by demonstrating that economic factors are not confined to short-distance movements only. It highlights that even after individuals have spent a substantial amount of time in the host country and are likely to be integrated, economic incentives continue to play a significant role in their decisions to move

abroad. In this chapter, we demonstrate that immigrants significantly increase their likelihood of leaving the Netherlands after a policy change reduced the duration of the tax break available of some high-skilled immigrants. These findings have direct implications for governments and policymakers designing tax schemes. While such tax incentives may be used by governments to attract high-skilled immigrants, for instance, to increase local productivity or alleviate labor shortages in the short term, they risk being ineffective in retaining immigrants beyond the temporary tax break period if benefits initially promised are later withdrawn or altered. Immigrants continue to reassess the value of staying, even after spending a substantial amount of time in the host country. This is specifically the case for high-income, highly mobile individuals whose location decisions are sensitive to changes in tax conditions, warranting policy credibility and predictability.

While **Chapter 2** and **Chapter 3** focused on the reasons behind “why” individuals move internationally, it is also important to understand the consequences of these movements for receiving countries, especially as the scale of international mobility continues to rise globally. In 2020, more than 280 million people resided in countries different than their birth country, which represents about 3.6% of the world population. This represents a rapid increase from the estimated 84 million in 1970 and 153 million in 1990.⁵ The Netherlands is no exception to the global trend, although in 1970 around 4.4% or 566,900 immigrants (first generation) already lived in the country, this number rose to 7.1% of the population by 1990 (1,054,000 individuals). As of 2024, the Netherlands had a population of around 17.9 million, with around 16.2% (2,915,000

⁵ McAuliffe & Triandafyllidou (2021).

individuals) of them being first generation migrants.^{6,7} This constitutes a substantial increase over the past five decades and highlights the growing importance of understanding the economic and social impacts of immigration in the destination countries.

Against this backdrop, **Chapter 4** examines the labor market impact of a large, sudden migration wave from Suriname to the Netherlands in the mid-1970s, where immigrants arrived in the host country with full labor market rights and relatively strong cultural and linguistic proximity, offering new insights into the consequence to local economies of an unexpected migration wave in the short run. We show that municipalities more exposed to this labor supply shock experienced a decline in employment levels among low-skilled natives. Altogether, the findings from this thesis demonstrate that while international mobility offers opportunities for individuals, under certain circumstances it can also have consequences in destination labor markets, particularly for native workers whose skills compete with those of the newly arrived immigrants.

The results from **Chapter 4**, thus, highlight the importance of anticipating and managing large migration flows to mitigate adverse short-run effects on vulnerable native groups. Over the longer term, policies that support immigrant integration remain essential. Initiatives such as job-specific training, language acquisition programs, and active labor market policies can help steer immigrants into roles that complement the native workforce, reduce labor market mismatches, and promote economic integration. Although our results suggest that

⁶ Migration background is defined as the first- and second-generation immigrants. First generation immigrants are those born outside the Netherlands while second generation immigrants are people born in the Netherlands but with one or both parents born elsewhere.

⁷ Among the first-generation immigrants, about one-third of migrants were born in Europe and two-thirds were born outside Europe. Of those born outside Europe, most have a Turkish (12.2%), Surinamese (9.6%) and Moroccan (9.5%) background. Please check Statistics Netherlands (CBS), (2023) for more details on the second generation migrant numbers.

language proficiency may initially heighten competition in specific segments of the labor market, facilitating language acquisition is still critical for supporting broader social cohesion and enhancing the societal well-being (Foged, Hasager, Peri, Arendt, & Bolvig, 2023, 2024).

In summary, this dissertation advances our understanding of the drivers that shape patterns of international labor mobility, as well as its effects on the labor market outcomes of natives in destination countries. From cultural distance in cross-border commuting, to tax policy for attracting and retaining immigrants, and the labor market consequences of sudden immigration shocks, the findings in this thesis underscore the complex trade-offs that policymakers face in managing international mobility. Governments must balance the economic benefits of immigration—such as alleviating labor shortages and mitigating the effects of population aging—against public concerns over labor market competition, housing affordability and integration.⁸ It also highlights the critical role of bridging the statistical department with universities to foster high-quality research, leveraging the Netherlands' uniquely rich data infrastructure to generate evidence that better informs policy. Taken together, this research offers insights that can help policymakers and educators design more effective immigration, taxation, and labor market policies that not only attract and integrate immigrants, but also anticipate and mitigate short-term impacts on native workers, ultimately maximizing the long-term economic and social benefits of immigration.

⁸ Please refer to Alesina & Tabellini (2024) for an in-depth review on the political economy effects of immigration.

Summary

This dissertation contributes to our understanding of the drivers of cross-border labor mobility and its impacts on host societies. It focuses on three interrelated aspects of international labor mobility: the role of cultural differences in cross-border commuting, migrants' responses to the removal of a tax incentive, and the labor market effects of a large immigration shock under conditions of cultural proximity. Together, these themes address the overarching question of how economic and non-economic factors shape the patterns and consequences of international labor mobility.

Chapter 2 investigates the role of cultural distance in shaping cross-border commuting flows from Belgium to the Netherlands. To this end, we use unique administrative data on commuting flows from Belgium to the Netherlands and historical dialect data covering the Netherlands and the Dutch-speaking part of Belgium. We use historical dialects to measure cultural distance between regions based on the identifying assumption that they serve as a proxy for contemporaneous culture while affecting commuting only through the cultural channel. Given that “dialect borders” do not align perfectly with national borders, we introduced meaningful variation in cultural distance between municipalities on opposite sides of the Belgium–Netherlands border. In our basic regression, an increase in cultural distance between municipalities by one standard deviation leads to a robust decrease ranging from 7% to 22% in cross-border commuting flows from Belgium to the Netherlands. Our findings also suggest that at short distances, economic factors—such as lower commuting costs—are the dominant influence, whereas at longer distances (around 55 to 60 kilometers), the impact of cultural dissimilarity becomes more pronounced. Finally, we show that within the Netherlands, cultural dissimilarity plays a smaller role in deterring commuting flows and that the presence of national

borders magnifies the effect of cultural dissimilarity, adding layers of institutional and administrative barriers that further hinder cross-border labor mobility.

In **Chapter 3**, we investigate the workers' mobility response to a loss in preferential tax treatment, exploiting a sharp and unexpected policy change that shortened the duration of the tax break available to high-skilled migrants to the Netherlands. The policy change only affected migrants who arrived from specific countries within a certain period. Since no wage threshold was used to define eligibility, this creates a unique opportunity to study emigration responses down the income distribution—an unexplored area in the existing literature. We relied on tax and population-wide administrative data and a difference-in-differences approach to uncover the causal effect of tax-induced emigration. Our findings show that taking away tax breaks from migrants strongly increases their likelihood of leaving the country. Crucially, our findings show that this effect is almost entirely driven by out-migration from the top 1% of the income distribution and no migration response was detected for those below the 95th earnings percentile. Individuals in the top 1% of the income distribution decrease their length of stay by 11.1% and are 18.3% less likely to remain beyond the end of the five years that they receive the tax break, while 'highly mobile' individuals, those who have arrived from a country that is not their country of origin/nationality, also react in terms of both intensive and extensive duration margins when among top 5% of earners. We estimate that the elasticity of migration with respect to the net-of-tax rate for the top 1% of earners is 1.48 to 1.74. Regarding the distortionary effect of national policies on international tax competition, we show that the most mobile tax earners become much more likely to leave for a tax-friendly country post reform, and we conclude that, at least in terms of the change in taxable receipts from labor in the Netherlands, this policy was cost neutral.

Chapter 4 analyzes the labor market effects of a large, unexpected migration wave: the arrival of Surinamese immigrants following Suriname's independence in 1975. This event generated a sudden and

exogenous labor supply shock, as immigrants had full labor market rights and relatively strong Dutch language skills. Using historical census data, labor force surveys, and population counts, we apply a difference-in-differences strategy exploiting variation in local immigrant exposure while controlling for initial foreign-born shares. We show an almost 0.9% decline in employment among low-skilled natives, with no significant impact on high-skilled employment or occupational scores. While we do not directly observe immigrants' language proficiency, we find circumstantial evidence that municipalities receiving more Creole-background immigrants—presumed to have stronger Dutch proficiency—experienced slightly larger negative employment effects than those receiving more Hindustani or Javanese immigrants. These findings are supported by the theoretical frameworks suggesting that the complementarity or substitutability of immigrant skills shapes natives' labor market outcomes, and they highlight that language proficiency may be an important factor influencing these outcomes when it does not create comparative advantages for natives.

Together, the results highlight that both economic and non-economic factors significantly influence individuals' decisions to move internationally and it demonstrated that, under certain circumstances, immigration may affect native workers employment.

Samenvatting

Dit proefschrift draagt bij aan de kennis over de drijfveren van grensoverschrijdende arbeidsmobiliteit en de effecten daarvan op de landen waar de werknemers naartoe gaan. Het richt zich op drie onderling samenhangende aspecten van internationale arbeidsmobiliteit: de rol van culturele verschillen bij grensoverschrijdend woon-werkverkeer, de reactie van werkzame migranten ('expats') op het afschaffen van een fiscale stimulans, en de arbeidsmarkteffecten van een grote immigratieschok op de nationale beroepsbevolking. Samen behandelen deze thema's de overkoepelende vraag hoe zowel economische als niet-economische factoren de patronen en gevolgen van internationale arbeidsmobiliteit vormgeven.

Hoofdstuk 2 onderzoekt de rol van culturele afstand bij het verklaren van grensoverschrijdende pendelstromen van België naar Nederland. Hiervoor maken we gebruik van unieke administratieve gegevens over pendelstromen van België naar Nederland en historische dialectgegevens die betrekking hebben op Nederland en het Nederlandstalige deel van België (Vlaanderen). Historische dialecten worden gebruikt om culturele afstand tussen regio's te meten, op basis van de aanname dat zij dienen als een proxy voor hedendaagse cultuur, en het pendelgedrag uitsluitend via dit culturele aspect beïnvloeden. Aangezien 'dialectgrenzen' niet perfect samenvallen met nationale grenzen, kunnen we de variatie in culturele afstand tussen gemeenten aan weerszijden van de Belgisch-Nederlandse grens gebruiken om het causale effect te schatten van culturele afstand op pendel. In onze basisregressie leidt een toename van de culturele afstand tussen gemeenten met één standaarddeviatie tot een daling van 7% tot 22% in grensoverschrijdende pendelstromen van België naar Nederland. Onze bevindingen tonen bovendien aan dat op korte afstanden economische factoren —zoals lagere reiskosten— dominant zijn, terwijl op grotere afstanden (vanaf ongeveer 55 kilometer) het effect van culturele verschillen sterker wordt. Tot slot laten we zien dat binnen

Nederland culturele verschillen een kleinere rol spelen bij de verklaring van pendelstromen en dat de aanwezigheid van nationale grenzen het effect van culturele verschillen versterkt, doordat extra institutionele en administratieve barrières worden toegevoegd die grensoverschrijdende pendelstromen verder belemmeren.

In Hoofdstuk 3 onderzoeken we voor werkzame migranten de gevolgen van een sterke en onverwachte aanpassing in de belastingheffing die de duur van een belastingvoordeel (de 30%-regeling voor expats) voor hoogopgeleide migranten in Nederland verkortte. Deze aanpassing had uitsluitend betrekking op migranten die in een bepaalde periode uit specifieke landen naar Nederland waren gekomen. Aangezien geen loondrempel werd gehanteerd om te bepalen wie daar gebruik van kon maken, biedt dit een unieke mogelijkheid om de effecten van alle inkomensgroepen te bestuderen – een tot nu toe onontgonnen terrein in de bestaande literatuur. We maken gebruik van fiscale en andere register gegevens van de hele bevolking en een difference-in-differences-benadering om het causale effect van de belastingaanpassing op het vertrek uit Nederland vast te stellen. Onze resultaten tonen aan dat het afschaffen van belastingvoordelen voor werkzame migranten de kans dat ze het land verlaten sterk vergroot. Cruciaal is dat dit effect vrijwel volledig wordt verklaard door het vertrek uit Nederland van de groep die bestaat uit de top 1% van de inkomensverdeling; dit effect wordt kleiner bij een lager topinkomen, en voor personen onder het 95e inkomenspercentiel wordt helemaal geen effect meer waargenomen. Personen in de top 1% verkorten hun verblijfsduur gemiddeld met 11,1% en zijn gemiddeld 18,3% minder geneigd om langer dan vijf jaar te blijven –de periode waarin zij van de belastingkorting profiteren. Bij de ‘zeer mobiele’ personen, dat wil zeggen degenen die zijn aangekomen vanuit een ander land dan hun land van herkomst of een andere nationaliteit hebben dan hun herkomstland, zijn dergelijke effecten van de belastingaanpassing op het vertrek uit Nederland te zien wanneer zij tot de top 5% van verdieners behoren. Wij schatten dat de elasticiteit van migratie ten opzichte van het netto-belastingtarief voor de top 1% van de inkomensverdeling tussen 1,48 en 1,74 in ligt. Met betrekking tot het verstorende effect van dit nationale beleid op de internationale fiscale

concurrentie laten we zien dat de meest mobiele belastingbetalers na de hervorming veel vaker geneigd zijn te vertrekken naar fiscaal gunstige landen. Wij concluderen verder dat dit beleid, in termen van de verandering in belastbare arbeidsinkomsten in Nederland, budgettair neutraal was.

Hoofdstuk 4 analyseert de arbeidsmarkteffecten van een grote, onverwachte migratiegolf: de komst van Surinaamse immigranten na de onafhankelijkheid van Suriname in 1975. Deze gebeurtenis veroorzaakte een plotselinge en exogene schok in het concurrerende arbeidsaanbod voor werkenden in Nederland, aangezien deze immigranten volledige toegang tot de arbeidsmarkt hadden en over relatief sterke kennis van de Nederlandse taal beschikten. Met behulp van historische volkstellingsgegevens, arbeidskrachtenenquêtes en bevolkingsstatistieken passen we een ‘difference-in-differences’-strategie toe, waarbij we de variatie tussen gemeentes wat betreft de instroom van immigranten benutten. Daarbij controleren we onder meer voor het aanvankelijke aandeel van immigranten die zelf afkomstig zijn uit een ander land in de desbetreffende gemeenten. We vinden een daling van bijna 0,9% in de werkgelegenheid onder laaggeschoolde werknemers van Nederlandse herkomst, zonder significante effecten op de werkgelegenheid van hooggeschoolden of op het soort werk. Hoewel we de taalvaardigheid van immigranten niet direct observeren, vinden we wel indicaties dat gemeenten die relatief meer immigranten met een Creoolse achtergrond – met een veronderstelde sterkere Nederlandse taalvaardigheid dan de overige migrantengroepen uit Suriname – ontvingen, iets grotere negatieve werkgelegenheidseffecten ondervonden dan gemeenten die meer Hindostaanse of Javaanse immigranten ontvingen. Deze bevindingen sluiten aan bij theoretische perspectieven die benadrukken dat complementariteit of substitutie van vaardigheden van immigranten bepalend zijn voor de arbeidsmarkttuitkomsten van werknemers in het land van bestemming, en dat taalvaardigheid hierbij een belangrijke factor kan zijn.

Samenvattend laten de resultaten in dit proefschrift zien dat zowel economische als niet-economische factoren een belangrijke rol spelen in

internationale migratiebeslissingen en dat immigratie onder bepaalde omstandigheden gevolgen kan hebben voor de werkgelegenheid van werknemers in het land van bestemming.

About the Author



José Victor Cremonesi Giarola was born on October 16, 1991, in Jundiaí, São Paulo, Brazil. He received his Bachelor's (2013) in Economics from the Universidade Federal do Paraná, Brazil and Master's (2017) in Development Economics from Université Paris 1 Panthéon-Sorbonne, France. He worked for a year as research assistant at the Centre d'économie de la Sorbonne (CES) before starting his doctoral studies at Maastricht University's School of Business and Economics in 2019 under the supervision of Frank Cörvers and Hans Schmeets.

At Maastricht University, José Victor taught a number of undergraduate courses on international economics and served as a co-coordinator of the International Economic Relations course for Economics and Business Economics B.Sc. 1st year students. José Victor presented his work at several conferences and had the Chapter of this thesis entitled "*Culture and Cross-Border Commuting*" accepted for publication in the Journal of Economic Geography. Through his Ph.D. journey, José Victor worked for five months as a trainee at the European Commission, Directorate-General for Employment, Social Affairs & Inclusion.

Currently, he serves as a Social Policy Advisor at the Economic Policy Research Institute (EPRI), working on numerous projects related to social protection.

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