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Essays in Labor Mobility and Workplace Technological Change

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Abstract

Globalization significantly transforms labor markets. Advances in production technologies, transportation, and political integration reshape *how* and *where* goods and services are produced. Local economic conditions and diverse policy responses create varying speeds of change, affecting regions' attractiveness for living and working – and promoting mobility. Competition for talent necessitates a deep understanding of why individuals choose specific destinations, how to ensure their effective labor market integration, and what workplace factors affect workers' well-being. This thesis focuses on two crucial aspects of labor market change – Migration and workplace technological change. It contributes to our understanding of the determinants of labor mobility, the factors facilitating migrant integration, and the role of workplace automation for worker well-being.

Chapter 2 investigates the relationship between minimum wages (MWs) and regional worker mobility in the EU. EU citizens are free to work anywhere in the common market, which allows them to take advantage of the significant variation in MWs across the EU. However, although MWs are set at the national level, it is also their local relevance that varies substantially – depending on factors such as the share of affected workers or the extent to which they shift local compensation levels. These variations may attract workers from elsewhere, from within a country or from abroad. Analyzing regional variations in the Kaitz index, a measure of local MW impact, reveals that higher MWs can significantly increase inflows of low-skilled EU workers, particularly in central Europe.

Chapter 3 examines the inequality in returns to skills experienced by immigrants, focusing on the role of linguistic proximity between migrants' origin and destination countries. Harmonized individual-level data from nine linguistically diverse migrant-hosting economies allows for an analysis of the wage gaps faced by immigrants from various origins, implicitly indicating how well they and their skills are integrated into the local labor markets. The analysis reveals that greater linguistic distance is associated with a higher wage penalty for highly skilled immigrants and a lower position in the wage distribution for those without tertiary education.

Chapter 4 investigates an institutional factor potentially relevant for the integration of immigrants – the labor market impact of Confucius Institutes (CIs), Chinese government-sponsored institutions that promote Chinese language and culture abroad. CIs have been found to foster trade and cultural exchange, indicating their potential relevance in shaping attitudes and trust of natives towards China and Chinese individuals. Examining the relationship between local CI presence and the wages of Chinese immigrants in local labor markets of the United States, the analysis reveals that CIs associate with significantly reduced wages for nearby residing Chinese immigrants. An event study demonstrates that the mere announcement of a new CI negatively impacts local wages for Chinese immigrants, independent of the CI's actual opening.

Chapter 5 explores how working in automatable jobs affects life satisfaction in Germany. Following earlier literature, we classify occupations by potential for automation, and define the top third of occupations in this metric as *automatable jobs*. We find workers in highly automatable jobs reporting a lower life satisfaction. Moreover, we detect a non-linearity, where workers in moderately automatable jobs (the second third of the distribution) experience a positive association with life satisfaction. Overall, the negative relationship of automation is most pronounced among younger and blue-collar workers, irrespective of the non-linearity.

Deutsche Kurzfassung

(German Abstract)

Die Globalisierung verändert Arbeitsmärkte nachhaltig. Fortschritte in Produktionstechnologien, im Transportwesen und in der politischen Integration prägen entscheidend *wie* und *wo* Güter und Dienstleistungen hergestellt werden. Lokale wirtschaftliche Gegebenheiten sowie unterschiedliche politische Maßnahmen führen dabei zu regional variierenden Geschwindigkeiten des Wandels, was die Attraktivität von Regionen als Lebens- und Arbeitsort beeinflusst – und somit auch die Mobilität der Menschen erheblich verändert. Der zunehmende Wettbewerb um Fachkräfte erfordert ein tiefgreifendes Verständnis dafür, warum Individuen in ihren Migrationsentscheidungen bestimmte Zielregionen für sich wählen, wie ihre erfolgreiche Integration in den dortigen Arbeitsmarkt sichergestellt werden kann und welche Arbeitsplatzfaktoren das Wohlbefinden der Beschäftigten allgemein beeinflussen. Die vorliegende Dissertation befasst sich mit zwei zentralen Aspekten des Wandels auf den Arbeitsmärkten - Migration und technologische Veränderungen am Arbeitsplatz. Sie trägt zum besseren Verständnis der Determinanten von Arbeitskräftemobilität bei, identifiziert Faktoren, welche die Integration von Migranten in den Zielarbeitsmarkt fördern (oder auch behindern), und untersucht den Zusammenhang zwischen Automatisierung am Arbeitsplatz und dem Wohlbefinden der Beschäftigten.

Kapitel 2 analysiert Mindestlöhne als Determinante regionaler Arbeitskräftemobilität in der EU. EU-Bürger genießen die Freiheit, überall im gemeinsamen Binnenmarkt zu denselben Bedingungen wie Einheimische zu arbeiten. Dieser Umstand ermöglicht es ihnen, die teils erheblich unterschiedlichen Mindestlohnniveaus in den verschiedenen Mitgliedstaaten zu berücksichtigen und gegebenenfalls für ihre eigenen Beschäftigungschancen zu nutzen. Obwohl Mindestlöhne typischerweise für die nationale Ebene festgelegt werden, variiert ihre lokale Relevanz innerhalb eines Staats jedoch deutlich. Ihr Einfluss wird durch Faktoren wie das lokale Preisniveau, die absolute und relative Anzahl der vom Mindestlohn betroffenen Personen sowie den Grad, in dem der Mindestlohn die lokale Lohnstruktur beeinflusst, bestimmt. Diese Unterschiede können erheblich sein und Wanderungsbewegungen innerhalb und zwischen Ländern begünstigen. Die Analyse regionaler Variationen des Kaitz-Index, einem Maß für die lokale Wirkung von Mindestlöhnen, zeigt, dass höhere Mindestlöhne signifikant die Zuwanderung geringqualifizierter Arbeitnehmer, insbesondere in Zentraleuropa, fördern können.

Kapitel 3 befasst sich mit den unterschiedlichen Bildungsrenditen von Immigranten verschiedener Herkunftsländer und untersucht die Relevanz der sprachlichen Nähe zwischen Herkunfts- und Zielländern als Erklärungsansatz. Mithilfe harmonisierter Individualdaten aus neun sprachlich diversen Zielländern wird eine Analyse der Lohnunterschiede von Migranten unterschiedlicher Herkunft durchgeführt. Implizit wird somit untersucht, wie gut oder schlecht Immigranten bestimmter Herkunftsländer ihre Qualifikationen in den jeweiligen lokalen Arbeitsmarkt einbringen können. Die Ergebnisse zeigen, dass eine größere sprachliche Distanz mit einem höheren Lohnabschlag für hochqualifizierte Mi-

granten sowie mit einer niedrigeren Position in der Lohnverteilung für Migranten ohne tertiären Bildungsabschluss einhergeht.

Kapitel 4 analysiert, ob Konfuzius-Institute—staatlich geförderte Einrichtungen der chinesischen Regierung, die in den Zielländern Kenntnisse um die chinesische Sprache und Kultur fördern sollen—relevant für die Integration chinesischstämmiger Immigranten in den lokalen Arbeitsmarkt sind. Frühere Studien haben gezeigt, dass Konfuzius-Institute einen positiven Einfluss auf Handel und kulturellen Austausch mit China haben, was darauf hindeutet, dass die Institute potenziell Einstellungen und Vertrauen gegenüber China und chinesischen Staatsangehörigen beeinflussen können. Die Untersuchung des Zusammenhangs zwischen der lokalen Präsenz von Konfuzius-Instituten in den USA und den Löhnen chinesischer Migranten in der näheren Umgebung zeigt, dass Konfuzius-Institute signifikant mit niedrigeren Löhnen für chinesische Immigranten korrelieren. Eine ergänzende Analyse mittels Ereignisstudie zeigt zudem, dass bereits die Ankündigung eines neuen Konfuzius-Instituts das Lohnniveau chinesischer Immigranten in einer Region negativ beeinflusst – unabhängig von der tatsächlichen Eröffnung des Instituts.

Kapitel 5 untersucht den Zusammenhang zwischen der Tätigkeit in potenziell automatisierbaren Berufen und der Lebenszufriedenheit von Arbeitnehmern in Deutschland. Aufbauend auf früheren Studien werden Berufe anhand ihres Automatisierungspotenzials klassifiziert, wobei das oberste Drittel dieser Verteilung als besonders stark von Automatisierung betroffen gilt. Die Analyse zeigt, dass Beschäftigte in diesen Berufen eine geringere Lebenszufriedenheit aufweisen. Eine ergänzende Untersuchung belegt jedoch einen nicht-linearen Zusammenhang: Beschäftigte in Berufen mit moderatem Automatisierungspotenzial (das zweite Drittel der Verteilung) berichten eine signifikant höhere Lebenszufriedenheit. Heterogenitätsanalysen zeigen, dass der negative Zusammenhang zwischen hohem Automatisierungspotenzial und Lebenszufriedenheit besonders bei jüngeren und sogenannten *Blue-collar*-Beschäftigten (also vor allem bei manuell arbeitenden Industriearbeitern und Handwerkern) stark ausgeprägt ist, ungeachtet der festgestellten Nichtlinearität.

Acknowledgments

As an optimist, I always believed this dissertation would be completed, but as a perfectionist, I spent countless hours refining it – I hope the attention to detail will be evident and valuable to readers. As an economist, I sometimes questioned whether the effort was worth it, but now, finalizing the thesis, I am convinced it was. Nearing the end of this long journey feels both surreal and bittersweet. The unwavering support, feedback, and friendship of many kept me going. I am deeply grateful to everyone who contributed to my research; each discussion with friends and colleagues offered new insights and helped me grow.

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Trier, September 2024 *Jonas Benjamin Feld*

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Chapter 1

Introduction

Globalization significantly transforms societies and economies worldwide. It continuously influences what products are made, how production processes are carried out, where production takes place, and the extent to which labor or machines are used. These shifts challenge traditional working methods and profoundly impact labor markets in various ways. Central to globalization are advancements in transportation and communication technologies, which have drastically reduced the costs of transporting information, goods, and people. These developments facilitate the growth of global financial markets, trade, and easier travel over ever larger distances, and lead to an integration of markets worldwide (IMF, 2000). Two factors especially relevant to the labor market effects of globalization will be central to this thesis.

The first is migration. The processes underlying globalization have a spatially varying impact, affecting places differently and shaping the relative attractiveness of regions for business, work, and living. In response to these changes, parts of the global workforce relocate, accelerating worldwide migration counts in recent decades. Labor mobility is therefore an integral part of the worldwide integration processes, being both a driver and a consequence of globalization (Freeman, 2006). Yet, due to the number and complexity of individual decisions, significant gaps remain in understanding when and where workers move, and what influences their integration into local markets (Bansak et al., 2021).

The second factor is workplace technological change. The automation of many work tasks has led to the disappearance of certain jobs, the evolution of job profiles, and the creation of new professions. While fostering innovation and efficiency, which can enhance job opportunities, these changes also create uncertainty and necessitate continuous skill development. Their influence on workers' overall well-being is therefore profound. Additionally, well-being considerations are important aspects of migration decisions (Nowok et al., 2013, Stillman et al., 2015). Assessing workplace determinants of well-being is therefore key not only to understanding immediate job-related factors but also the broader labor market implications.

The aim of this thesis is to provide new insights to this debate. It contributes in particular to four areas: (1) the determinants of labor mobility, (2) factors relevant to migrant labor market integration, (3) factors interfering with integration, and (4) the association between workplace automation and individual worker well-being. The following paragraphs of this chapter introduce the reader to the wider literature and explain each study's contribution.[1]

[1]Note that whenever the male form is used throughout the text—either implicitly or explicitly—it is solely for readability purposes. The text is intended to be inclusive of all genders.

Determinants of labor mobility

Migration, in general, refers to the change in one's place of living from one location to another, which can be internal (within a country) or external (crossing international borders). Globally, most migration occurs internally, with only a relatively small share being cross-border (IOM, 2019).[2] Economic theory distinguishes push and pull factors of migration. Push factors compel people to leave their current place of residence. Pull factors attract people to certain destinations (Bodvarsson and Van den Berg, 2013, Bansak et al., 2021).

Scholars have long noted that economic considerations drive many migration decisions. Smith (1776) and Hicks (1963) emphasized wage differences, Ravenstein (1889) and Zipf (1946) migration costs as determinants of mobility decisions. Underlying these assumptions are utility considerations. Migration occurs whenever the destination offers higher utility than the place of origin and the benefits outweigh the costs (Sjaastad, 1962). Empirical research has particularly applied gravity models of migration, as these can easily be extended (see, for instance, significant contributions by Borjas, 1987, 1991).[3]

For internal migration, work-related factors have been identified as decisive determinants, in particular mean wages, unemployment figures, and local employment growth (Greenwood, 1975, 1985, Kennan and Walker, 2011).[4] Most literature addresses international migration, though. The local wage level (respectively, GDP per capita) was identified as the most important pull factor, while being rather irrelevant as a push factor. Smaller geographical distance between countries increases bilateral migration (e.g. Mayda, 2010, Grogger and Hanson, 2011). Contiguity, colonial relationships, shared languages, and linguistic proximity likewise increase bilateral migration numbers (Artuç et al., 2015, Adsera and Pytlikova, 2015), while visa regulations deter migrant inflows from visa-affected origin countries (Clark et al., 2007, Czaika and De Haas, 2017). Moreover, low unemployment rates have been found to attract international mobility (e.g. Geis et al., 2013), as do higher unemployment benefits and social spending (Pedersen et al., 2008, De Giorgi and Pellizzari, 2009).[5] High income taxes, in contrast, discourage immigration (Czaika and Parsons, 2017). Interestingly, bilateral migration between EU countries is substantially more responsive to economic conditions than elsewhere, likely because EU citizens can freely move and work across the bloc (Ortega and Peri, 2013).

[2]For the purposes of this thesis, I follow the United Nations (1998)'s recommendation, and consider in the following as international migrants those living outside their country of birth for more than 12 months, and as mobile workers those who have moved for labor-related reasons within the last 12 months.

[3]An excellent explanation of gravity models in migration is provided by Beine et al. (2016), who also highlight random utility maximization as the theoretical basis of the model, while Ramos (2016) offers a non-technical summary with empirical evidence.

[4]This literature refers to the United States. Similar findings have been reported for several European (e.g. Hunt, 2006, Etzo, 2011, Détang-Dessendre et al., 2016) and developing countries (e.g. Lall et al., 2006, Bao et al., 2009).

[5]The *welfare magnet hypothesis* (Borjas, 1999) implies that migrants may prefer destinations with more generous welfare regimes. This is based on observations that immigrants are over-represented in receiving welfare benefits (Giulietti and Wahba, 2013). However, the empirical evidence is mixed on this topic. Early literature includes Levine and Zimmerman (1999), Brueckner (2000) and Gelbach (2004). For more recent findings using advanced methods, refer to Geis et al. (2013), Cigagna and Sulis (2015), Razin and Wahba (2015) and Kahanec and Guzi (2022). For a literature review, see Czaika and Reinprecht (2022).

Analytically, most literature identifies migrants by country of birth, which can overlook people born outside their home country or moving in from a third place. Visa regulations typically consider citizenship, adding to complexity in migration analyses (Ramos, 2016). Moreover, studies examining migration movements often rely on changes in population stocks as their main measure of mobility (due to data availability), thereby implicitly neglecting issues like return migration (Dustmann and Görlach, 2016). Additionally, many studies suffer from omitted variable bias and multilateral resistance to migration (where alternative locations are not irrelevant), leading to potential over- or underestimation of effects (Bertoli and Moraga, 2013).

In chapter 2, I contribute to the literature on migration determinants by investigating the impact of minimum wages (MWs) on internal and external immigration of low-skilled EU workers into 103 NUTS-2 regions in six EU countries. Earlier research indicated that among OECD countries, the *existence* of MWs (the level has not been studied) leads to higher immigration (Cigagna and Sulis, 2015). Studies on state-level MW variations in the United States provide mixed results, finding higher MWs either attracting (e.g. Cushing, 2003, Boffy-Ramirez, 2013, Giulietti, 2014) or deterring mobility (e.g. Cadena, 2014, Martin and Termos, 2015, Monras, 2019).[6]

To my knowledge, my study is the first to examine the unique European context. The common EU market offers an exceptionally well-suited case study for verifying the importance of labor market institutions for worker mobility in general. MWs vary significantly across members, and the freedom of movement for workers ensures equal labor market access anywhere in the EU for all EU citizens. From cross-country harmonized individual-level data, I examine actual internal and external incoming worker mobility (not population stocks) at the regional level. The Kaitz index—a measure of local MW impact—is the main independent variable, facilitating effective cross-regional comparisons even across country borders. My methodological approach includes a multi-level structure on regional-level panel data, estimation of region-fixed effects, and adjustment for country-time trends. Moreover, I address potential sources of endogeneity, such as simultaneity, reverse causality, and dynamic processes, by employing alternative methodological specifications including the Arellano-Bond dynamic generalized method of moments (GMM) instrumental variable estimator for panel data (Arellano and Bond, 1991).[7] My findings indicate that more impactful MWs lead to higher local labor inflows, with my main outcome estimation showing that a one standard deviation increase in the local Kaitz index is associated with a 10 percent rise in the regional labor inflow rate.

Factors relevant to migrant labor market integration

The study presented in the second chapter of this thesis offers valuable insights on the impact MWs have for spatial labor mobility, although it does face some limitations dsince the underlying data does not provide detailed information on the exact migrant origin. Migrants often select their destination considering country-pair specific issues such as cultural proximity and networks of fellow migrants at destination (e.g. Pedersen et al., 2008, Beine et al., 2011), which indicates that migrant integration at the destination is an important factor in many migration decisions. Moreover, from a destination coun-

[6] Chapter 2.2 provides a literature review on research related to MWs as determinant of migration.

[7] However, due to insufficient data on the exact origin of incoming EU workers, I cannot directly address multilateral resistance to migration. The common correlated effects estimator is unsuitable in this case, as the panel is unbalanced and the sample size is small across both panel and time dimensions (Chudik and Pesaran, 2015, Ditzen, 2018).

try perspective, it is beneficial if migrants are well integrated into their labor market (Kancs and Lecca, 2018). Understanding the factors relevant to migrant integration and assimilation in host economies is therefore crucial.

Most immigrants initially have worse labor market outcomes than natives, all else equal (e.g. Chiswick, 1978, LaLonde and Topel, 1992, Kee, 1995, Borjas, 2015). Several strands of literature have examined this pattern. One strand of research focuses on issues of discrimination and segmented labor markets. Employers' willingness to hire certain immigrants, and on what terms, can be a matter of taste (Becker, 1957). Wages often reflect social status, and employers may not offer the same wage to immigrants as to natives. In some cases, citizenship-based restrictions even prevent migrants from taking certain positions (Piore, 1972, Massey et al., 1993, Altonji and Blank, 1999, Constant and Massey, 2005, Hellerstein and Neumark, 2008). Measuring labor market discrimination is challenging, though, as it is often concealed and attributed to other seemingly legitimate reasons. Consequently, empirical evidence is limited (see e.g. Lang and Lehmann, 2012).

Another strand of literature follows the assimilation hypothesis (Chiswick, 1986), which suggests that immigrants face initial labor market frictions due to a lack of local expertise on the functioning of the destination area's labor market, specific local labor market features, and effective job search strategies for their hosting market. Over time, though, immigrants adapt. Their labor market outcomes improve as they become more accustomed to the local culture and customs and gain valuable local skills. Cultural proximity, which serves as a proxy for individuals' knowledge of the destination country's labor market, positively influences migration returns (Docquier et al., 2007). Different arrival cohorts, with varying immigrant origin compositions, therefore perform differently over time (Borjas, 1985, Lubotsky, 2007).

A final strand of literature examines the role of skill portability in migrant integration. Education and labor market experience obtained abroad often have a lower initial value than those obtained domestically (Friedberg, 2000, Fortin et al., 2016), and the returns to foreign education over time are also lower (Ferrer and Riddell, 2008). This can be due to labor market mismatch and segregation (Peri and Sparber, 2009), non-transferable credentials (Chiswick and Miller, 2009), and varying education quality (Bratsberg and Terrell, 2002).

An important form of country-specific human capital is immigrants' language skills. Proficiency in the destination country's dominant language significantly boosts immigrants' initial earnings (Chiswick, 1991, Dustmann, 1994), and higher educated migrants particularly benefit from faster wage growth due to their language skills (Chiswick and Miller, 1995, Bleakley and Chin, 2010). Additionally, language proficiency facilitates the transferability of other skills (Imai et al., 2019). Moreover, linguistic proximity eases the process of learning the host country's language (Bleakley and Chin, 2004, 2010), and learning a new language is simpler when an immigrant already speaks a linguistically similar language (Beenstock et al., 2001, Isphording and Otten, 2014a). Consequently, linguistic proximity can play a crucial role in both skill transferability and overall migrant integration.

In chapter 3, my co-author and I contribute to the literature by examining the impact of linguistic proximity on the returns to immigrant skills. We propose that not just individual language proficiency, but the general linguistic abilities of immigrants from a specific origin in a certain destination facilitate integration and returns to skills. Building on existing literature, we argue that employers may find it easier to assess the skills of migrants from linguistically closer origins (objective costs). And additionally, subjective costs related to stereotyping may be perceived more significant for migrants from linguistically distant countries, potentially leading to wage penalties for these immigrants.

To test our hypotheses, we harmonized individual-level data from nine linguistically diverse immigration destinations, collectively hosting over one-third of the global immigrant population. From these rich data, we estimate the systematic variation in returns to skills that immigrants from different origin countries experience in each host country. We then empirically assess the role of linguistic proximity in explaining these differences. Our analysis accounts for multiple languages in the destination, bilateral relationships (including several measures of cultural proximity), time-varying origin-country factors, and employs destination- and origin country-fixed effects – ensuring to isolate the impact of linguistic proximity. Our findings indicate that linguistic proximity reduces the average wage penalty for high-skilled migrants and improves the wage distribution position for non-highly educated immigrants.

These results align with previous work by Chiswick and Miller (2012), who found that immigrants to the United States experience larger earnings gaps, the longer it takes Americans to learn the migrant's origin country language (i.e. a measure of linguistic proximity to English). Our study also builds on Adserà and Ferrer (2021), who showed that Canadian immigrants earn less and are more likely to work in lower-skilled positions if their native language is lexically distant from English. With our study, we particularly complement these earlier research by including the perspective of destination countries where the dominant language is not English, and by utilizing multi-dimensional dyadic data, i.e. using multiple data points for each origin and destination country. This approach allows us to apply fixed effects for both origin and destination, thereby eliminating country-specific constant influences on skill transferability and overall migrant integration in our data.

Factors interfering migrant labor market integration

Linguistic proximity often serves as a proxy for cultural proximity, a broader and perhaps more subjective measure of the relative closeness of countries and societies (e.g. Ginsburgh, 2005). Perceived cultural distance influences the attitudes (O'Rourke and Sinnott, 2006) and the level of trust natives have towards immigrants from different origin countries (Cettolin and Suetens, 2019). This reflects how employers' hiring decisions and contract terms may be shaped by personal norms, biases, and individual preferences towards different societal sub-groups, as theorized by Becker (1957). Empirically, Keita and Valette (2019) demonstrate that immigrants' labor market outcomes are affected by natives' attitudes towards the immigrants' countries of origin. Consequently, any factor that alters attitudes and perceptions of cultural distance may potentially impact the labor market outcomes of immigrants.

A potential factor influencing perceived cultural distance is cultural and language institute programs such as the German Goethe-Institutes, the French Alliance Française centers, the Portuguese Instituto Camões, or the Chinese Confucius Institutes (CIs). These organizations, set up abroad and mostly financed by their respective national governments, aim to promote and teach language, culture, and general knowledge of their origin countries. They also seek to encourage bilateral cooperation, such as facilitating trade relationships. Such institutes have been found to increase bilateral trade and foreign direct investments (Lien and Miao, 2018). Moreover, access to these institutes offers potential migrants an affordable means to acquire destination-specific skills. Jaschke and Keita (2021) show that Goethe institutes improve language skills upon arrival in Germany, and lead to a self-selection of migrants with characteristics in demand by the local destination labor market.

In chapter 4, my co-author and I argue that cultural institutes can have a significant impact on the institute-hosting societies. We investigate the role of Chinese CIs in the United States, which have only emerged relatively recently, within the last 20 years. These institutes aim to enhance local awareness and understanding of China by offering cultural and language education. We hypothesize that if these institutes succeed in their mission, they could effectively reduce the perceived cultural distance between the United States and China, thereby improving labor market opportunities for Chinese immigrants on the domestic American labor market.

To investigate this, we integrate individual-level data from the American Community Survey (ACS) with spatial data on the locations of CIs in the United States. We analyze the labor market outcomes of Chinese immigrants residing near CIs, adjusting for local region- and year-fixed effects, as well as state-time trends to capture changes in labor market-relevant state legislation. Using ordinary least squares (OLS) estimation techniques, we find that the presence of these institutes is associated with larger wage gaps for Chinese immigrants. Additionally, analyzing specifically the number of local CIs reveals a U-shaped relationship: While additional CIs exacerbate the wage gap, the rate of decline diminishes as the number of institutes increases.

To verify that our findings are directly attributable to the presence of CIs, we conduct a placebo test with other immigrant groups. This test shows that while fellow East Asian immigrants (excluding Chinese) also experience a negative relationship between labor market outcomes and CI presence (which we interpret as a halo effect), other immigrant groups do not. Immigrants from Europe even experience a slightly positive relationship. This may indicate that CIs influence the ranking of employers' tastes and the respective relative positions of certain immigrant groups in these rankings. Additionally, to strengthen the causal interpretation of our results, we conduct an event study, which reveals that the mere announcement of a new CI, even before its opening, is associated with lower local wages for Chinese immigrants. We speculate that this negative impact may stem from increased media attention on Chinese politics when CIs enter the local agenda, which often conflicts with interests of the United States. This potentially leads to more adverse attitudes towards Chinese immigrants in an area and consequently affects their chances on the local labor market.

Workplace automation and well-being

The final paper addresses the relationship between workplace technological change and worker well-being. Automation technology, by targeting specific workplace processes, often reduces the human element in various tasks, enhancing efficiency in the production processes. Technological advancement not only streamline operations, but can notably aid the workforce by simplifying tasks and boosting worker productivity, especially for low-skilled workers through reducing the need for extensive education and experience (Agrawal et al., 2023).

Research has explored the implications of workplace automation, revealing its pervasive impact across nearly all types of jobs. Automation has been identified as the primary driver of labor market polarization in Western countries (e.g. Autor and Dorn, 2013).

Projections suggest that OECD countries could see between 13-47 percent of current employment being replaced by automation technologies in the future (notable studies include Autor, 2015, Arntz et al., 2016, Frey and Osborne, 2017, Josten and Lordan, 2020). Workers most affected by automation often find themselves relatively disadvantaged over time, experiencing minimal wage growth and limited upward social mobility (Acemoglu and Restrepo, 2020). Given these profound changes in the labor market, it is plausible that workplace automation also significantly influences the well-being of workers, as job security, income stability, and career progression are often considered integral components of an individual's subjective well-being.

The literature on the role of job automation for individual worker well-being can be broadly classified into two strands. The first strand posits that automation induces fear of job loss and difficulties in adaptation, which can lead to a range of negative consequences, including reduced health and well-being metrics (e.g. Giuntella et al., 2023, Abeliansky et al., 2021). The second strand highlights the complementary benefits of automation, such as increased workplace safety, less stressful and physically demanding job profiles, the emergence of new job opportunities, and overall productivity gains, which can even secure many existing jobs (e.g. Gihleb et al., 2022, Jacobs et al., 2023).

Various outcomes have been investigated within these research strands, including job satisfaction and diverse measures of physical and mental health. However, the influence of workplace changes due to automation extends beyond these specific domains. Life satisfaction has been described as a comprehensive well-being measure that encompasses both direct socio-economic factors, such as income and education, and indirect elements, including psychosocial well-being, physical and mental health, environmental conditions, workplace satisfaction, and social relationships, among others (Fernández-Ballesteros et al., 2001, Medvedev and Landhuis, 2018). Therefore, life satisfaction serves as a broad indicator suitable for detecting the overall impact of automation on individual worker well-being.

In Chapter 5, my co-author and I explore how job automation influences overall worker well-being in Germany, the most automation-affected country in Europe (Lordan, 2018), offering insights into the potential outcomes technological changes in the workplace has on life satisfaction. We seek to contribute to a more nuanced understanding of how automation shapes not only job roles but also the holistic experience of workers in an evolving labor market.

Notably, two studies have specifically examined the impact of workplace technological change on life satisfaction before. Giuntella et al. (2023) analyzed workers' exposure to artificial intelligence in the workplace, finding that since 2015, affected workers report lower job and life satisfaction, increased concerns about job security and the own economic situation, but no decrease in health-related measures. Lordan and Stringer (2022) studied the Australian labor market from 2001 to 2018 using a broad automation measure and found modest negative associations between working in automatable jobs and life satisfaction in certain industries, with little heterogeneity based on socio-demographic characteristics.

In our study, we closely follow Lordan and Stringer's methodology by applying (Autor and Dorn, 2013)'s broad occupation-level susceptibility to automation measure. The measure classifies the top third of employment-weighted occupations in terms of relative routine task intensity as *automatable*. Using panel data from a representative sample of the German workforce over the years 1981-2020, we estimate the individual-level association between working in automatable occupations and self-reported overall life satisfaction. We apply individual-level fixed effects, and adjust our estimates for time-varying individual characteristics, job-related covariates, and 1-digit occupation fixed effects. Our baseline model reveals a negative association between working in an automatable job and life satisfaction in Germany, consistent with earlier findings from the first strand of literature.

However, when we modify Autor and Dorn's binary measure into a three-category interval measure, we reveal a non-linear relationship. Workers in automatable jobs do not differ significantly in reported life satisfaction from those in least-automatable jobs. Instead, workers in semi-automatable jobs—those experiencing moderate levels of automation—exhibit a positive association with life satisfaction relative to the others. We speculate that this result may be due to adaptability: Workers who are gradually affected by automation may experience less fear of job displacement compared to those who are more severely affected, while still benefiting substantially from the productivity-enhancing aspects of new technologies. This finding aligns well with the second strand of literature discussed above. Additionally, our heterogeneity analysis indicates that the negative association observed in the baseline is primarily driven by blue-collar and younger workers, with the result remaining significant for blue-collar workers even with the amended measure.

Taken together, the four empirical papers examine the determinants and implications of two key aspects of globalization: Migration and workplace technological change. Each paper approaches this broader topic from distinct perspectives, utilizing individual-level data alongside spatially disaggregated information from Europe, the United States, and beyond. The following chapters will present these empirical analyses in detail.

Chapter 2

Minimum Wages and Labor Mobility in the European Union*

The EU boasts the largest single labor market globally; EU citizens enjoy the freedom to take up work anywhere within the common market. And despite considerably diverse labor market regimes across the EU, little is known about how local labor market settings influence spatial labor mobility within the bloc. By integrating cross-country harmonized local labor mobility data from the EU Labor Force Survey with the Kaitz index, a standardized measure of local minimum wage (MW) impact, I investigate the relevance of MWs for regional (i.e. sub-national) low-skilled labor mobility. Utilizing both a fixed effects model and the Arellano-Bond dynamic panel instrumental variable estimator on a sample of 103 NUTS-2 regions across six EU countries from 2003 to 2019, my analysis reveals that more substantial MWs correspond to elevated local labor inflows: On average, a one percent increase in the Kaitz index associates with a 0.03 percentage point higher worker inflow rate to the given region, indicating a Kaitz index elasticity of low-skilled labor inflow of about 0.18. This result holds for several alternative model specifications and robustness tests. Moreover, I observe substantial cross-country heterogeneity, and find particularly pronounced mobility responses among younger workers.

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2.1 Introduction

Minimum wages (MWs) define a lower legal limit of remuneration for labor. Their simple mode of operation and easy implementation makes MWs one of the most widely discussed labor market institutions, often used as flagship policy during election campaigns.[8] About 150 countries around the world have some kind of statutory national MW, and, as of 2023, only 9 out of 49 European countries (including 5 out of 27 EU member states) have no legally binding national wage floor.[9] EU directive 2022/2041 demands all EU member states to either set *adequate* statutory MWs (at least 60 percent of a country's gross median wage or 50 percent of its gross average wage) by the end of 2024 or, alternatively, to ensure a minimum collective bargaining (CB) coverage rate of 80 percent. At present, less than half the EU countries meet any of these criteria, and most are relatively far from reaching the required standard.[10] Accordingly, applicable statutory MWs will most likely see a significant increase across many EU countries in the near future.

Standard economic theory suggests that higher MWs increase labor supply but decrease labor demand (Boeri and van Ours, 2008). The net impact of MWs on an area's *expected earnings*—wage level adjusted for the likelihood of finding employment—is contingent upon the prevailing dominant effect and can either be positive or negative (Harris and Todaro, 1970). Additionally, studies on worker mobility underscore the significance of expected earnings in mobility decisions (e.g. Sjaastad, 1962, Becker, 1964, Zavodny, 1999, Jaeger, 2007, Kennan and Walker, 2011). Therefore, I argue that MWs potentially influence labor mobility patterns.[11] As a result, EU directive 2022/2041 may have an unintended effect; potentially impacting labor mobility decisions and labor flows across the EU.

Earlier studies on the impact of MWs for spatial worker mobility in the United States yielded mixed results: Cushing (2003) found that poor Americans were drawn to domestic areas with relatively higher MWs, while Martin and Termos (2015) showed an outflow of low-skilled workers linked to higher MW levels. Monras (2019) provided a similar result but stressed the significance of specific local labor market characteristics in determining this outcome. Across the EU, applicable MWs vary substantially more than across US states.[12] Despite this variation, suprisingly, no research has yet explored the relationship between MWs and labor mobility within the EU – a gap this study seeks to address.

In general, EU countries exhibit significant variability in several factors that potentially impact spatial labor mobility, including economic, political, and social conditions, infrastructure, and cultural aspects. This diversity is evident not only at the national level but also within countries at the regional level.[13] Research has shown that both na-

[8]For instance, MWs were the central topics of the (successful) election campaigns of the UK Labour Party (in 1997) and the Social Democratic Party of Germany (in 2013 & 2021). Olaf Scholz, now German chancellor, acknowledged in 2021 that raising the German MW to 12 EUR (an increase of about 15 percent from the previous level) would be the 'most important law' if he would get elected (Der Spiegel, 2021).

[9]European countries without statutory national MW include the Nordic countries, Italy, Austria, Switzerland and Liechtenstein. See ILO (2020) for a comprehensive global report.

[10]Refer to section 2.3 for more details.

[11]Throughout the text, when using terms such as labor mobility, worker mobility, labor migration, and similar, I specifically refer to *spatial* worker mobility.

[12]In 2022, nominal MWs in the EU varied between 2 EUR per hour in Bulgaria and 13 EUR per hour in Luxembourg, while in the United States, they varied from 7.25 USD per hour (the federal MW applicable nationwide) to 15 USD in California and Washington, D.C. (WSI, 2023).

[13]Throughout the text, 'regions' and 'regional' refer to regions within countries.

tional and regional variations in economic fundamentals influence labor mobility patterns in the EU (e.g., Beyer and Smets, 2015). Additionally, the effects of MWs are closely tied to the characteristics of local labor markets (e.g., Harris and Todaro, 1970, Dube et al., 2010). Consequently, a comprehensive analysis of the European context requires a focus on the regional (i.e. local) level. Moreover, since all EU citizens have equal access to any of the EU's national labor markets, variations in MWs potentially affect the mobility of any EU citizens, not just natives. I therefore examine the mobility of the entire EU workforce in light of these variations.[14]

Due to the absence of harmonized EU labor mobility figures, my analysis starts by establishing a measure of regional labor flows. I rely on the EU Labor Force Survey (LFS) as data source, a cross-country harmonized and regionally representative survey of workers across the EU. I quantify the regional influx of low-skilled individuals (those most likely affected by MWs) relative to the respective local population across all surveyed EU NUTS-2 regions.[15] Subsequently, I combine these inflow rates with Hamermesh's (1981) conceptualization of the Kaitz index, a measure indicating the relevance, or *bite*, of the applicable national MW for a given region. My sample comprises 103 NUTS-2 regions across six countries, all of which were EU members with statutory MWs in place during my entire observation period from 2003 to 2019.[16]

I evaluate the relationship between MWs and regional labor mobility by mainly employing two types of panel regression models: a region-fixed effects model and the Arellano-Bond (AB) generalized method of moments (GMM) estimator, a dynamic panel data model. My analysis reveals a strong correlation between changes in the Kaitz index and labor mobility in the EU. Specifically, a one percent increase in the local Kaitz index is associated with a roughly 0.03 percentage point higher inflow rate of low-skilled individuals into a region, all else equal, reflecting an elasticity of 0.18. A one standard deviation increase in the index corresponds to a 10 percent higher inflow rate. I run several robustness checks and causality tests; the outcomes of these specifications enhance the validity of my baseline result and provide support for the assumed causal direction of the relationship. Furthermore, a heterogeneity analysis reveals distinct relationships across countries and suggests some varying outcomes for natives, domestic mobility, and younger workers. There is no indication of a heterogeneous relation across sexes.

In the upcoming sections, I conduct a detailed examination of the intricate relationship between MWs and labor mobility in the EU. Section 2.2 delves into the theoretical underpinnings of the presumed relationship and reviews relevant literature. Section 2.3 offers an overview of the current landscape of MWs across the EU, coupled with some essential insights regarding intra-EU mobility. In section 2.4, I outline the data and empirical strategy adopted in this study. My baseline finding is presented in section 2.5, alongside several causality assessments, robustness tests and a thorough heterogeneity analysis.

[14] I refrain from analyzing third-country nationals due to their unequal work rights, dependence on (frequently changing) visa regulations, and other potential limitations in labor mobility.

[15] The *Nomenclature of Territorial Units for Statistics* (NUTS) is an EU geocode standard, referencing administrative divisions within EU countries. Currently, the EU comprises 240 NUTS-2 regions (excluding 37 regions from the former EU member UK).

[16] My sample includes all EU countries with nationwide statutory MWs in place throughout the sampling period, and for which there exists adequate data on regional labor inflows. This selection includes Belgium, France, Greece, Portugal, Spain, and the UK. Note that throughout the sampling period, the UK was a member of the EU. For simplicity, I categorize it as an EU country in the text, recognizing that this classification may not hold at present.

2.2 Literature

"[D]ifferences in net economic advantages, chiefly differences in wages, are the main causes of migration" (Hicks, 1963, 76-77)

According to Sjaastad (1962) and Becker (1964), moving from one place to another can be described as a rational (investment) decision where individuals compare the expected costs and benefits of a specific move. Mobility takes place whenever the expected benefits exceed the associated expected costs. Likewise, the selection of one destination over another hinges on the anticipated comparative benefits or gains. Harris and Todaro (1970) formalized these considerations in what is known as the *expected income hypothesis*: Individuals contemplating labor migration consider not only the potential income achievable in a particular area (local wage levels) but also the probability of finding employment there, which involves taking into account the local unemployment rate.

Labor market effects of minimum wages

With wages being important drivers for labor mobility, the question whether and how wages are regulated becomes particularly salient for understanding labor migration dynamics. MWs by design have an influence on an area's entrance wages. Moreover, they potentially shift the average and median wages of a region, especially if they also trigger labor supply and mobility responses (Grossman, 1983). If a new, elevated MW exceeds the reservation wage of previously inactive members of a population, the increased assured compensation encourages these individuals to join the local workforce. On the other hand, firms may be resistant to increasing employment given the higher labor costs per hour. Therefore, according to conventional economic theory, MWs in competitive markets are believed to stimulate increased labor supply but to reduce labor demand, leading to unemployment (Boeri and van Ours, 2008). Alternative theories propose that in some cases, rather than reducing employment, MWs could boost employment – depending on, for instance, the degree of local market concentration, local market imperfections, and other features of specific (local) labor markets (Card and Krueger, 1993, Manning, 2003). Despite abundant empirical evidence, a universally accepted conclusion on the labor market effects of MWs remains elusive.[17]

Given these theoretical considerations and specifically the argumentation by Harris and Todaro (1970), highlighting local context factors, MWs appear to shape the attractiveness of local areas to outsiders by influencing income prospects. However, the impact of MWs may vary across segments and sectors within labor markets, and typically depends on the relative intensity of the specific local income and substitution effects catalyzed by the MW. Research particularly points towards different labor market effects among various demographic and occupational groups: For instance, Dolton and Bondibene (2012) found that during economic downturns, it is particularly those at the margin of the workforce that are negatively affected by MWs – young workers are laid off first. Clemens and Wither (2019) similarly show that low-skilled employment declines more than general employment when MWs 'bite'. Episodes of economic unrest expose certain groups to increased vulnerability, and MWs particularly contribute to this effect.

[17] An encompassing review of the literature goes beyond the scope of this paper. Neumark and Wascher (2008), Dube and Lindner (2021) and Neumark and Shirley (2022) provide extensive summaries of the existing literature.

In general, Dube and Lindner (2021) demonstrate that MWs typically impact workers at approximately the bottom 30 percentiles of wages, contingent upon the proportional magnitude of the MW relative to local compensation levels. The overall effect of MWs on expected income thus depends on various factors, including local market structures like relative wage levels and industrial layout, the composition of the local workforce, and demand and supply elasticities in the local labor market (Neumark and Shirley, 2022).

Moreover, several authors argue that MWs are disproportionally more relevant for newcomers compared to established residents: MWs serve as a reference wage value of an area , given that they provide a (worst-case) minimum remuneration of *any* job available at the destination labor market (e.g., Sum et al., 2002, Cortes, 2004, Neumark et al., 2014). MWs also appear more relevant for mobile workers since these tend to be younger than average workers, are typically less experienced and tenured, have lower average education levels, and initially lack the social capital that could help them in the local labor market. These characteristics make them lean towards working in low-pay jobs and workplaces with higher job turnover rates (Chiswick, 1986). Orrenius and Zavodny (2008) compare the labor market effects of MWs on low-skilled natives and international immigrants in the United States, finding no substantial differences in wages or employment. However, they suggest that migrants' selectivity in mobility decisions, including avoiding regions with high MWs and intense competition with natives, may influence this outcome.

Minimum Wages and Labor Mobility

As a result, MW can have a significant impact on labor mobility decisions. However, empirical evidence regarding MWs' impact on labor mobility in the United States, the only country studied in this context so far, presents a mixed picture. Cushing (2003) investigates how spatial variations in the level and in the coverage of applicable MWs affects cross-state labor mobility between 1985-1990. He finds that state-level MWs above the federal MW[18] attract Americans from the lower end of the wage distribution; the absolute difference in MW levels between two states impacts the size of inter-state migration flows. Moreover, he finds a higher percentage of employment being covered by MWs positively correlates with the likelihood of low-income Americans deciding to move to the state in question. Martin and Termos (2015) investigate the outward mobility response of low-skilled Americans to local MW changes. They find that increases in local MWs lead to more low-skilled emigration away from that area. A similar finding is presented by Monras (2019), who investigates the correlation between state-level applicable MW changes and the inter-state mobility of prime-age (25-35 years old) low-skilled workers in the United States. He shows that, on average, MWs positively affect wages but negatively impact the employment likelihood of affected workers, and that the substitution effect of MWs typically outweighs the income effect in most areas in his sample. However, he observes that this does not significantly increase local unemployment, as numerous low-skilled workers move away from the affected regions, effectively clearing the local market.

Other studies have investigated the influence of MWs on the labor mobility of *international migrants*. Castillo-Freeman and Freeman (1992) find the implementation of the United States federal MW in Puerto Rico (which was significantly higher than the

[18] The federal MW applies to all workers in the United States. States are free to set their own applicable MWs, which can exceed (but not fall below) the federal standard.

local MW at that time) led to increased out-migration of low-skilled Puerto Ricans to the United States (potentially avoiding unemployment on the home market). Cigagna and Sulis (2015) find for a sample of 15 OECD countries (including nine EU countries) that the existence of MWs (no matter their level) positively influences immigrant counts into a country.

Research on the mobility of international migrants *within* the United States (a group arguably more mobile than natives due to weaker local ties) presents a similarly mixed picture: Von Scheven and Light (2012) show that Latin American immigrants tend not to settle in states that have recently increased their MWs to above the federal MW in the United States. They argue that states with relatively low MWs maintain larger low-wage sectors, which makes them attractive to immigrants on occupational grounds. Similarly, Cadena (2014) finds that low-skilled immigrants arbitrage labor markets by deviating away from high-MW states towards settling in US states with rather stagnant MWs. MW-induced job losses of teens are substantially larger in states with historically low migrant shares, a finding he claims supports the proposed mechanism. Somewhat contrastingly, Boffy-Ramirez (2013) reveals that some groups of migrants exhibit increased attraction to state-level MW changes: Specifically, migrants who have been residing in the United States for 2-4 years (i.e., those less settled and more eager to explore opportunities in the United States labor market) tend to be drawn towards regions with higher MWs. Meanwhile, he finds no significant response to MW changes among more established migrants. Giulietti (2014) assesses the impact of state-level variations in expected wages stemming from increases in the federal MW level (arguing that the income effect is equivalent everywhere, but not necessarily the substitution effect, i.e., the local employment response). He finds that MWs are a sizeable pull factor for recently arrived low-skilled migrants and a significant predictor of inter-state mobility among more established low-skilled migrants (residing in the United States for five years or longer).

Key insights and implications for this study

Although the overall perspective on the relationship between MWs and labor mobility seems rather inconclusive, certain key aspects emerge from the literature: First, MWs seem to influence labor mobility. In the United States, they have been observed to attract mobile workers from other states and international migrants alike. However, certain groups of workers have also been found to steer clear of higher MW areas, either relocating or opting for different regions from the outset. The influence of MWs on the overall number of incoming low-skilled mobile workers has been less studied. Second, and unsurprisingly, it is the individuals at the lower end of the income distribution that have been found to respond to shifts in MWs. There appear to be few, if any, spillover effects on higher skill levels in the United States. Accordingly, the local bite of the MW may be decisive for the overall MW effect in an area. Third, several studies point out the high relevance of local labor market characteristics for actual mobility responses. In particular, local features affecting labor demand and labor supply have been found to matter for the local mobility response to MWs.[19] Hence, if feasible, any analysis of the impact of MWs on labor mobility should focus on the sub-national level, ideally the lowest geographical level available.

[19] This discovery aligns with similar findings in literature exploring commuting patterns influenced by MWs (e.g. Kuehn, 2016, McKinnish, 2017).

With this study, I seek to contribute to the literature a regional-level assessment of the EU. Europe's labor market context is distinct, shaped by the diversity of its member countries and the intricate dynamics within them. This diversity sets it apart from the relatively homogeneous conditions in the United States. The notable variation in labor market institutions, such as MW regimes, underscores this distinction, reflecting diverse economic landscapes and policy frameworks across and within EU countries (see section 2.3). Inherent heterogeneity not only enhances the potential for mobility responses to changes in labor market settings but also provides rich variation for regression analysis, making Europe an exceptionally compelling case study. Moreover, the EU's principle of free movement of workers facilitates skill transfer and competition across country borders, thereby extending the pool of workers attuned to MW considerations. However, challenges such as cultural disparities, language barriers, and varying levels of employment protection may also hinder cross-border worker mobility. In some settings, fixed mobility costs may outweigh the benefits of mobility. Compounding this complexity, the prevalence of overeducation and down-skilling among migrant workers in Europe (Nieto et al., 2015) increases the potential pool of workers responsive to MW changes (Gregory and Zierahn, 2022). Additionally, higher MWs are associated with heightened recruitment selectivity (Butschek, 2022), potentially exacerbating labor market discrimination against external candidates, particularly in cross-border mobility.

Given the larger variability in MW levels across Europe and the substantial heterogeneity in the 'bite' of MWs within and across countries (see also sections 2.3 and 2.4), and considering that spatial mobility in Europe has been found generally more responsive to economic incentives than in the United States (Ortega and Peri, 2013), I anticipate MWs to play a more significant role as a location factor in Europe than in the United States. Specifically, I assume the impact of MWs on *expected income* (Harris and Todaro, 1970) be more nuanced in Europe compared to the United States labor market. In addition to the mere income effect, this expectation also arises from the relatively lower number of mobile workers in Europe: This factor potentially limits negative impacts of labor supply changes resulting from incoming EU workers. Hence, the substitution effect of MWs, hypothesized to specifically deter mobile workers (Orrenius and Zavodny, 2008, Monras, 2019), is likely less relevant for mobility within and across EU countries. Impactful MW increases should therefore increase the expected income for a locality relatively more than in the United States. Consequently, I expect MW changes to relate positively to incoming worker mobility counts in Europe.

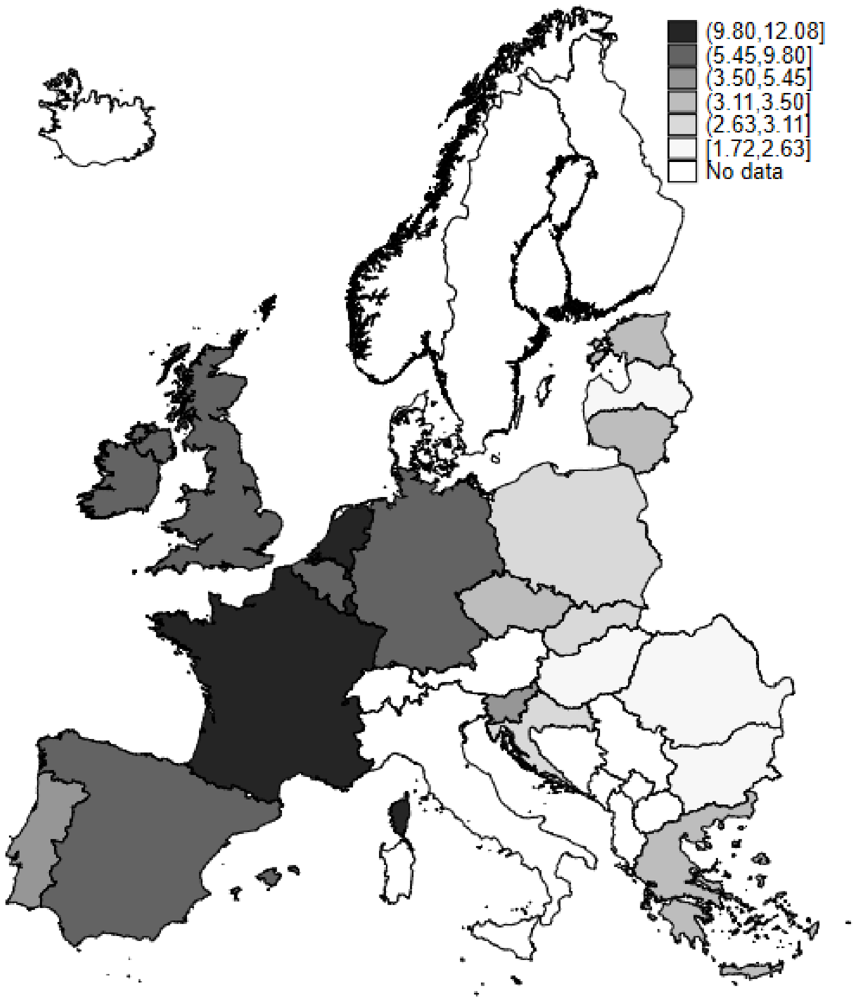
2.3 Minimum wages and labor mobility in the EU

In the following sections, I provide essential background information on MWs and labor mobility in the EU context. Section 2.3.1 examines which countries have MWs in place in Europe and the levels of these wages. It also considers their relationship with broader local labor market developments and the potential future impact of EU Directive 2022/2041 in this context. Section 2.3.2 offers an overview of mobility patterns within the EU, comparing them with labor mobility trends in the United States to better understand the intricate dynamics of worker mobility in Europe.

2.3.1 Minimum wages in the EU

MW policies vary widely across EU member states and are subject to ongoing debates and reform efforts. Some countries (Austria, Cyprus, Finland, Italy, and the Scandinavian countries) have hesitated to introduce statutory national MW policies, citing features of their labor market that would make MWs redundant. Other countries have had MWs in place for decades (France since 1950, Spain since the 1960s). Figure 2.1 visualizes EUR-denominated statutory MW levels in place across the EU in 2019 (the final year of my empirical analysis below).

Figure 2.1: EUR-denominated statutory minimum wages in Europe (2019)

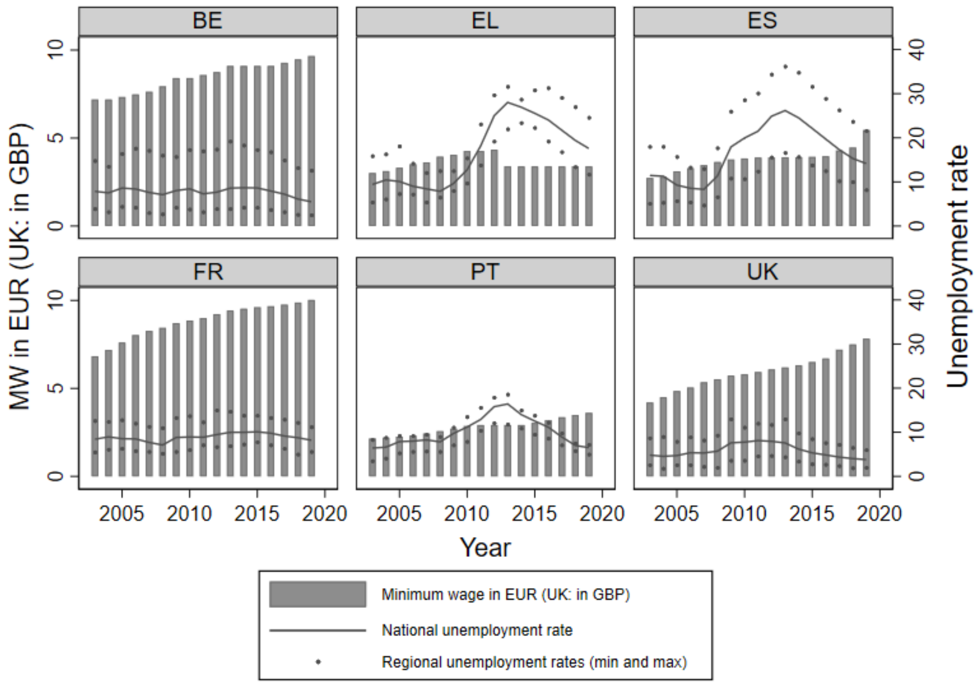


Source: Own elaboration, based on data from WSI (2023). All values denominated in EUR. "No data" indicates no statutory national MW in place in 2019. Cyprus is not shown on the map. The MWs of non-EU countries, i.e., EUR-denominated MW levels of Albania (1.21 EUR), North Macedonia (1.63 EUR) and the Republic of Serbia (1.78 EUR) are not shown on the map.

In 2019, statutory nominal MWs in the EU ranged between 1.72 EUR in Bulgaria and 12.08 EUR per hour in Luxembourg. In general, Eastern European countries' MWs are lower than the Western European ones, and MWs in Northern Europe tend to be higher than those in Southern Europe. Despite reflecting diverse economic developments and distinct levels of productivity, these disparities also show divergent (social) policy regimes and contrasting views on the level of the optimal MW (Eurofound, 2020). Figure

2.2 provides the development of applicable statutory MWs for the countries empirically investigated in this study[20], and contrasts these with national unemployment rates, and with the within-country variation of regional (NUTS-2 level) unemployment rates (reported are the respective national minimum and maximum values).[21]

Figure 2.2: Nominal minimum wages and unemployment rates



Source: Own elaboration, based on data from WSI (2023) and Eurostat data series `lfst_r_lfu3rt` and `une_rt_a_h`. UK's statutory MW is expressed in GBP (suppressing exchange rate fluctuations). Figure A.1 (appendix) shows EUR-denominated MWs.

Among the six sampled countries, (EUR-denominated) nominal MWs varied from 2.14 EUR in Portugal in 2004 to 10.03 EUR in France in 2019. Belgium, France, and the UK consistently maintained MWs exceeding 5 EUR per hour throughout the entire sampling period. Greek and Portuguese MWs never reached this threshold, and the Spanish MW did so only in 2019. Except for Greece, MWs never decreased in nominal terms in my sample.[22] During economic upswings (2006-2009 and post-2015), most countries adjusted MWs more significantly. But especially during phases of high unemployment, governments in Greece, Spain, and Portugal have had had limited scope for generous MW adjustments (Clemens and Wither, 2019).

Regional unemployment also varied widely within countries during my sampling period, with higher national rates often linked to greater regional disparities. Moreover, labor markets displayed notable imbalances in areas such as employment, participation rates, and youth unemployment, often affecting similar sets of regions and countries (e.g. OECD, 2012, Eurofound, 2014).

[20] See section 2.4 for the exact data sample.

[21] A similar graph for the group of EU-15 countries is available in the appendix (figure A.2).

[22] In 2013, as part of a comprehensive economic reform, Greece underwent a one-time reduction in the statutory hourly MW of roughly 1 EUR, representing a decrease of around 22 percent.

EU legislation affecting domestic minimum wages

In 2020, the European Commission initiated a legislative process to enhance the sufficiency and reach of MWs, and to enforce CB as the primary means to guarantee equitable wages and working conditions throughout all EU countries.[23] MWs and CB legislation in the EU are governed by domestic laws. The *Directive on adequate Minimum Wages in the European Union* (EU Directive 2022/2041), formally adopted in 2022, mandates member states to align their national regulations with EU-wide minimum standards by the end of 2024. MWs are to be set at a minimum of 60 percent of the gross median wage, 50 percent of the gross average wage, or, alternatively, a minimum of 80 percent of workers should be covered by some form of binding CB agreement. Table 2.1 reports each EU country's current status with respect to these standards.

Table 2.1: MW levels and CB coverage in the EU

Country	MW relative to median wage (2021)	MW relative to mean wage (2021)	Collective bargaining coverage rate (latest available)	Directive's target met?
Directive's target:	**60**	**50**	**80**	
Austria	(no MW)	(no MW)	98.0 (2019)	x
Belgium	44.7	40.9	96.0 (2019)	x
Bulgaria	62.7 (2018)	42.8 (2018)	10.8 (2022)	x
Croatia	45.8 (2020)	40.0 (2020)	46.7 (2022)	
Cyprus	(no MW)	(no MW)	43.3 (2022)	
Czech Republic	43.2	37.2	34.7 (2019)	
Denmark	(no MW)	(no MW)	82.0 (2018)	x
Estonia	42.6	36.3	6.1 (2018)	
Finland	(no MW)	(no MW)	88.8 (2017)	x
France	60.9	49.2	98.0 (2018)	x
Germany	51.1	45.1	54.0 (2018)	
Greece	49.8	39.8	14.2 (2017)	
Hungary	45.2	35.3	21.8 (2019)	
Ireland	46.1	35.8	34.0 (2017)	
Italy	(no MW)	(no MW)	100.0 (2019)	x
Latvia	42.3	34.3	27.1 (2018)	
Lithuania	46.7	38.7	7.9 (2019)	
Luxembourg	54.8	43.4	56.9 (2018)	
Malta	43.3 (2018)	35.4 (2018)	41.8 (2022)	
Netherlands	46.3	38.9	75.6 (2019)	
Poland	55.0	45.0	13.4 (2019)	
Portugal	66.2	46.6	73.6 (2018)	x
Romania	54.8	40.1	15.0 (2022)	
Slovak Republic	52.4	39.3	24.4 (2015)	
Slovenia	60.4	50.5	78.6 (2017)	x
Spain	48.4	40.5	80.1 (2018)	x
Sweden	(no MW)	(no MW)	90.0 (2022)	x
United Kingdom	56.9	47.5	26.9 (2019)	

Note: Own elaboration, based on most recent indicators available. If at least one value reaches its proclaimed norm, the target is met. Information on MW ratios (mean and median) by the OECD (data series `MIN2AVE`) or, in the case of Bulgaria, Croatia, and Malta, calculated using Eurostat data series `earn_ses18_19` and `earn_ses_pub2s`. Information on CB coverage comes from the `OECD/AIAS ICTWSS` database.

[23] The initiative is part of the *European Pillar of Social Rights* action plan, aiming to establish common living standards across all member states. Its objective is to ensure that EU citizens, irrespective of their place of work within the EU, can enjoy a decent living from their labor income.

As of December 2023, only 11 of the 27 EU member states meet any of the directive's standards. All countries without a statutory MW in place, plus Belgium, France, and Spain meet the CB coverage rate (an exception is Cyprus, which introduced a MW in 2023). France, Bulgaria, Portugal, and Slovenia meet the 60 percent minimum-to-median wage ratio target, with Slovenia also being the only country meeting the 50 percent minimum-to-mean wage ratio target. Most other countries are still relatively far from reaching any of the targets. Between 2015 and 2021, many countries even moved away from reaching the targets (not detailed here). Several EU countries have announced plans to meet certain targets within the next 2-3 years (Eurofound, 2023).

2.3.2 Labor mobility in the EU

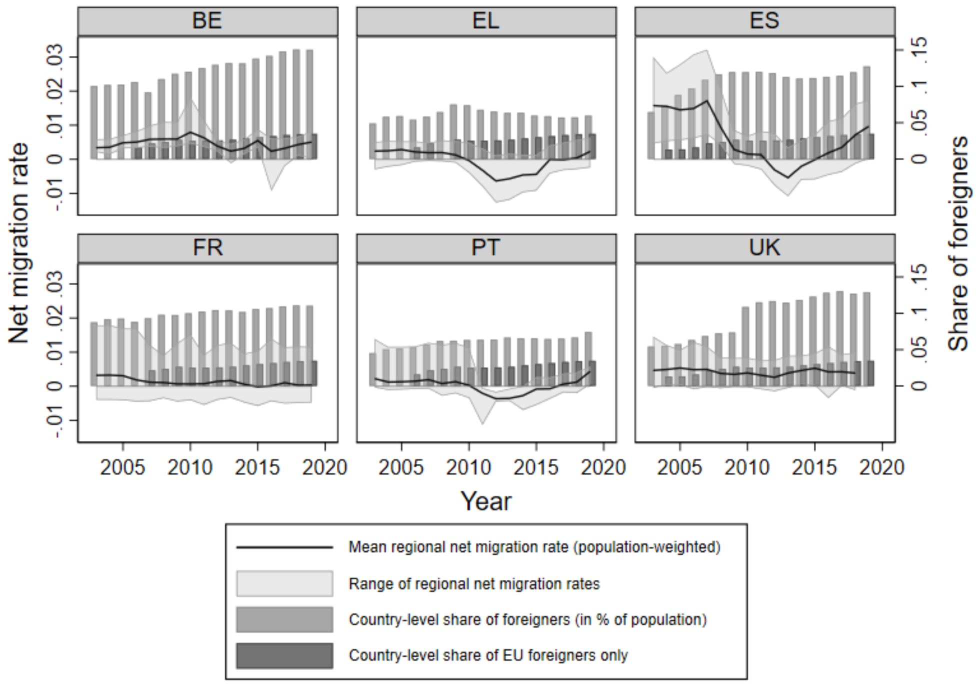
Labor mobility is a potential means to facilitate the adjustment of regional labor markets and to offset imbalances caused by economic shocks and regulatory changes, such as MW regulations (Blanchard and Katz, 1992, Kahanec et al., 2016, Cadena and Kovak, 2016, Dustmann and Preston, 2019). Yet a crucial aspect of the unified EU market is the freedom of movement for workers. Article 45 of the *Treaty on the Functioning of the European Union* (TFEU) guarantees EU citizens' equal treatment across the common EU labor market, i.e., EU citizens possess the right to work anywhere inside the EU under the same principles and regulations as the host country's nationals. This provision enhances EU citizens' job prospects and encourages labor mobility throughout the EU (Ortega and Peri, 2013). Accordingly, it is not only that regional labor market adjustments are influenced by internal worker mobility within a country, but cross-border mobility responses by EU citizens could be another significant factor in this process.

Notwithstanding this consideration, most labor mobility in the EU occurs within countries: A comparative study by the OECD (2012) found inter-regional mobility (NUTS-1) of the working age population *within* EU countries was 1 percent and cross-border mobility 0.3 percent in 2010, i.e., a total of 1.3 percent of the population changed their NUTS-1 region of residence within the EU that year. Cross-state mobility in the United States, in contrast, was reported at 2.4 percent of the population (and even higher in other studies, see for instance Molloy et al., 2011).[24] Moreover, internal mobility in Europe is heterogeneous both across and within countries. Specifically, countries with higher per capita income (most EU15 countries) and northern European countries experience higher per capita internal mobility. These countries also tend to attract more inbound mobility from abroad. In contrast, countries with relatively lower income levels and those in the south of Europe exhibit comparatively lower levels of internal and cross-border mobility: Arpaia et al. (2014) and Liu (2018) find relatively the highest internal mobility figures for the UK, Denmark, France, and Belgium, and the lowest mobility rates in Spain, Portugal, Greece, and Poland. Overall, internal and cross-border labor mobility in the EU have increased substantially over the last two decades (Kahanec, 2013, Liu, 2018, European Commission, 2022).

[24] Similar results on EU labor mobility for other periods can be found, e.g., in Gáková and Dijkstra (2008), Bonin et al. (2008), and Dorn and Zweimüller (2021). For a global comparison of inter-regional mobility figures, see Bell and Charles-Edwards (2013).

Figure 2.3 provides an overview of NUTS-2 level net migration rates, i.e. the local change of the resident population not attributable to births and deaths. As such, these figures do not imply absolute mobility counts, but portray whether a country's regions, on average, attract more individuals than they lose.[25] Figure A.3 in the appendix shows the respective figure for the full set of EU-15 member countries.

Figure 2.3: Mean regional net migration rates and share of foreigners



Source: Own elaboration, based on Eurostat dataset series `lfst_r_lfsd2pwc`, `demo_r_gind3`, and `migr_pop1ctz`. Note: There exist significant gaps in the data on the stock of EU foreigners in early years for several countries, including Belgium, Greece, and France until 2008.

The mean net migration rate varies substantially between and within countries, and also over the business cycle. Greece, Portugal, and Spain display significant variability over time (even experiencing net out-migration in the years after the financial crisis), while the mean net migration rates of Belgium, France, and the UK appear relatively more stable. Within-country variability is particularly pronounced in Spain and France.

The diversity in the proportion of foreigners across the countries is considerable as well. With the exception of Greece, the foreign population as a share of the total population increased in all sampled countries over the observed period. Belgium, Spain, France, and the UK have relatively high proportions of foreigners, constituting roughly 10-15 percent of their populations. Conversely, Greece and Portugal show notably lower proportions of their population originating from abroad. However, among the foreign population in these countries latter countries, there is a higher representation of EU citizens. The share of EU migrants relative to the share of all foreigners increases over time in all the countries shown here, indicating the growing importance of intra-EU labor

[25]A country might experience substantial mobility while maintaining a relatively low net migration rate if the number of incoming and outgoing individuals effectively offsets each other.

mobility across the EU, a trend noted in other studies as well (see, e.g., Gáková and Dijkstra, 2008, Eurofound, 2014, European Commission, 2022).[26]

2.4 Data and empirical strategy

To investigate the impact of MWs on labor mobility in the EU, this section first introduces the data I use. In particular, I highlight my approach to measuring cross-country harmonized regional labor inflow figures across regions in the EU, and describe Hamermesh's version of the so-called Kaitz index which I use to measure the regional 'bite' of a MW. I then continue to motivate my covariates, explain my empirical model, and lay out my final data sample.

2.4.1 Measuring labor mobility

One of the main difficulties in analyzing labor mobility within the EU is the lack of appropriate data on actual worker flows (Raymer et al., 2013, Willekens et al., 2016, Wisniowski, 2017, Willekens, 2019, Fenwick, 2022). All EU countries record harmonized population stock data down to the regional level (also of various subgroups, differentiated, for instance, by working age), but they do not track *movements* of workers in standardized, comparable ways.[27] To overcome this well-known data limitation problem, I calculate regional labor inflow figures from EU LFS microdata.[28] The EU LFS is an individual-level representative household sample survey conducted by all EU member states on a quarterly basis. It offers a consistent methodology and questionnaire, and it has a substantial sample size across all surveyed countries and regions.[29] It is therefore suitable for cross-country comparisons down to regional levels, which makes it Eurostat's primary source for the EU's regional labor market statistics.

I leverage a feature of the EU LFS to derive regional mobility rates: The questionnaire requires individuals to provide their country and region of residence one year before the survey date. Together with other recorded individual-level characteristics, such as differentiating between nationals, EU citizens and third country nationals, it is possible to extract specific macro-level indicators at the regional level. For my purpose, I derive worker inflow rates at the regional level for all EU NUTS-2 regions in the following manner:

$$inflow\_rate_{i,t}^{s} = \frac{m_{i,t}^{s}}{N_{i,t}}, \tag{2.1}$$

where $m_{i,t}^{s}$ is the count of 'recent movers' interviewed in region i in year t with characteristic s, who reported living in a different region or country 365 days before the survey date. The subscript s represents individual-level characteristics like nationality, sex, age, and so on. The denominator $N_{i,t}$ signifies the total count of individuals interviewed in region i in year t. To assess actual worker mobility, I limit the data to

[26]Refer to figure A.4 in the appendix for a broader EU-wide perspective.

[27]Mainly, challenges arise in defining consistent criteria identifying individuals as mobile. Fassmann et al. (2009) review problems associated with measuring mobility in Europe and beyond.

[28]An approach employed previously by Antolin and Bover (1997) to measure inter-regional mobility within Spain, by Bonin et al. (2008) to quantify cross-border mobility in the EU, and by Bloomfield et al. (2017) to assess the cross-border mobility of accounting professionals in Europe, for instance.

[29]Typically, the annual EU LFS sample size ranges between 1-2 percent of the local population.

individuals aged 15-65 and focus on low-skilled workers, the group most affected by MW legislation. Moreover, I consider only individuals possessing EU citizenship (which inherently includes nationals of the respective country), as these have equal rights to work in the domestic labor market and move freely between regions across the EU.[30]

However, calculating the regional inflow rate is not equally feasible for all EU countries and regions. Over the years, several countries have changed their regional NUTS breakdown. In my sample, this affects certain regions of France, Greece, and the UK.[31] Adapting these changes is not always viable, sometimes necessitating the exclusion of specific regions from the analysis, resulting in an unbalanced sample.[32] Moreover, in some countries (in my sample this pertains to the UK), the lowest surveyed regional breakdown is not at the NUTS-2 level (the UK's county and district level) but only at the NUTS-1 level (the UK's former government office regions). To ensure data availability at my primary analytical level (NUTS-2) for the UK, I adjust the calculated inflow rates from the next higher available level to correspond to the lower level.[33] Therefore, in section 2.5.3, I assess the robustness of my baseline regression results against a sample excluding the UK.

Typical problems associated with survey data are non-response, imperfect coverage of subgroups of the population in the sampling frame (and in the post-stratification criteria determining design- and survey weights), and measurement errors related to self-reported data. The extent of these problems in the EU LFS is unknown, making them difficult to address (Bell et al., 2015, Galgóczi et al., 2016, Wisniowski, 2017, Fenwick, 2022).[34] In all the countries of my sample, with the exception of the UK, participation in the EU LFS is mandatory. This design feature reduces non-response and helps ensure adequate subgroup coverage. Nevertheless, the EU LFS only considers those who have registered as permanent residents to be part of the population. Accordingly, short-term mobility (for instance, the presence of seasonal workers) is not covered under my concept of labor inflows. Some authors claim this design feature negatively affects the survey's

[30]Not all individuals from the new EU member states gained immediate access to EU labor markets after 2004 due to transitional arrangements. The EU LFS suppresses detailed nationality information, identifying individuals only within aggregated groups (domestic nationals, EU15, EU10, EU3, and others). This aggregation also limits the construction of more advanced models (for instance, a gravity-type mobility model as proposed by Beine et al. (2011)). To assess the impact of time-varying labor market access, I ran alternative regressions with different definitions of EU nationals: A restrictive version including only individuals from groups where all nationalities had full access, a less restrictive version where at least half the population had access, and a least restrictive version where at least one country had access. All these regressions closely mirror my baseline estimates, likely due to the small number of affected individuals. The respective regression estimates are available upon request.

[31]For historical NUTS breakdowns see `https://ec.europa.eu/eurostat/web/nuts/history` (last accessed: 08.12.2023).

[32]For instance, the greater London area (UKI) changed its delineation from NUTS 2010 version to NUTS 2013 version, creating completely new regions (increasing the number of NUTS-2 regions from two to five, with no boundary overlap). Consequently, it was impossible to incorporate the data in or before 2012. I include the London area data only from 2013 onward. Refer to table 2.7 in the appendix for details on my full data sample.

[33]For instance, I assign regions *Tees Valley and Durham* (NUTS-2 region UKC1) and *Northumberland and Tyne and Wear* (NUTS-2 region UKC2) the inflow rate calculated for NUTS-1 region *North East England* (NUTS-1 region UKC). See table A.1 for the full crosswalk used.

[34]See Heeringa et al. (2017) for an extensive overview of typical problems associated with survey data and potential remedies.

coverage of international migrants (Rendall et al., 2003, Martí and Ródenas, 2007).[35] In my sample about 12.5 percent of identified mobile workers are moving across borders. I test the relevance of international mobility for my outcomes in section 2.5.3.

A more problematic feature of the EU LFS may be what Martí and Ródenas (2007) labelled as the *Problem of Answer Impossible*: In certain countries, the survey design replaces only a fraction of the interviewed individuals each quarter. Consequently, some individuals are surveyed in multiple quarters before being replaced, and this time span is sometimes longer than a year.[36] An individual being in the survey sample for longer than a year cannot report having moved into the region in the last 365 days *for mechanical reasons*. The size of the bias introduced is determined by the national survey design and in particular by the specific survey's replacement rate. Although I cannot directly test the significance of this issue, I expect it to downward-bias the mobility rates derived from the LFS (incoming mobility is less likely to be surveyed than established residents). Since this concern is region-specific and likely constant over time (assuming no changes in survey design), a fixed effects panel model should adequately capture the bias (see the discussion of my econometric specification below).

2.4.2 Regional 'bite' of the minimum wage

My data on the level of statutory national MWs originates from the *WSI Minimum Wage Database International*, which is based on reports by the respective national statistical offices, and supplemented with information from various government agencies and the ILO. This dataset provides standardized average MW figures for each country, accounting for country-specific MW policies based on age, occupation, industry, place of residence, etc., and factoring in domestic features like the length of the average work week and the average number of hours worked per month. The data is presented in hourly and monthly formats, denominated in national currency and euros, and is consistently reported as of January 1st annually.[37]

Nominal MW figures lack information regarding their local market relevance, though. To compare MWs' relevance spatially, i.e. across regions and countries, a local reference value is needed. Kaitz (1970) proposed an index to measure the local 'bite' of the MW: The MW in relation to the typical wage paid in an area. This so called *Kaitz index* is often expressed as the gross hourly MW relative to the gross mean or median hourly earnings in an area. As the MW increases or average earnings decrease, the Kaitz index rises. Accordingly, a relatively high Kaitz index typically indicates relatively high *real*[38] MWs in an area (and typically more workers being affected), while a low index score implies less relevance of the MW for the local labor market (Boeri and van Ours, 2008).

[35] The empirical evidence from Rendall et al. (2003) and Martí and Ródenas (2007) relies on LFS samples and methods from before my sample period, and predates significant methodological changes in survey design (see, for instance, European Commission, 2003). Larger sample sizes have since improved subgroup coverage (Eurostat, 2021). Concurrently, increased EU mobility has enhanced the representation of mobile workers in my sample *per se* (see section 2.3.2).

[36] In my sample, this pattern affects all countries, albeit with varying intensity. Yet, the approach chosen by each country typically remains constant over time. An exception is Belgium, where significant changes to the LFS design were implemented in 2006 and 2017. I test my results against a sample excluding Belgium (section 2.5.3).

[37] See WSI (2023) for the dataset and information on country-specific calculations.

[38] The Kaitz index implicitly states the MW in real terms, as it is not susceptible to inflation (provided that the local overall wage level adjusts for inflation).

It is widely acknowledged that MWs not only impact local wage levels but also influence the probability of securing employment, affecting both employment and unemployment rates (see section 2.2). An essential factor influencing the attractiveness of MWs is the concept of *expected earnings*, which is contingent upon the regional interplay between labor supply and demand (Harris and Todaro, 1970). However, capturing the exact mechanisms of income and substitution effects presents a considerable challenge. Recent studies have highlighted that employers respond to MW adjustments not only by altering their labor demand but also through the modulation of employer-provided benefit schemes, which can constitute up to 30 percent of employers' compensation costs (Clemens et al., 2018, Clemens, 2021). Traditional metrics like average hourly earnings, utilized in most appliances of the Kaitz index, therefore may lack some precision when comparing MW levels across diverse legislatures and different work cultures. Employers potentially consider various costs to adapt to changes in MWs, including payroll taxes, social contributions, and bargained earnings components such as paid vacations, bonus payments, and other allowances.

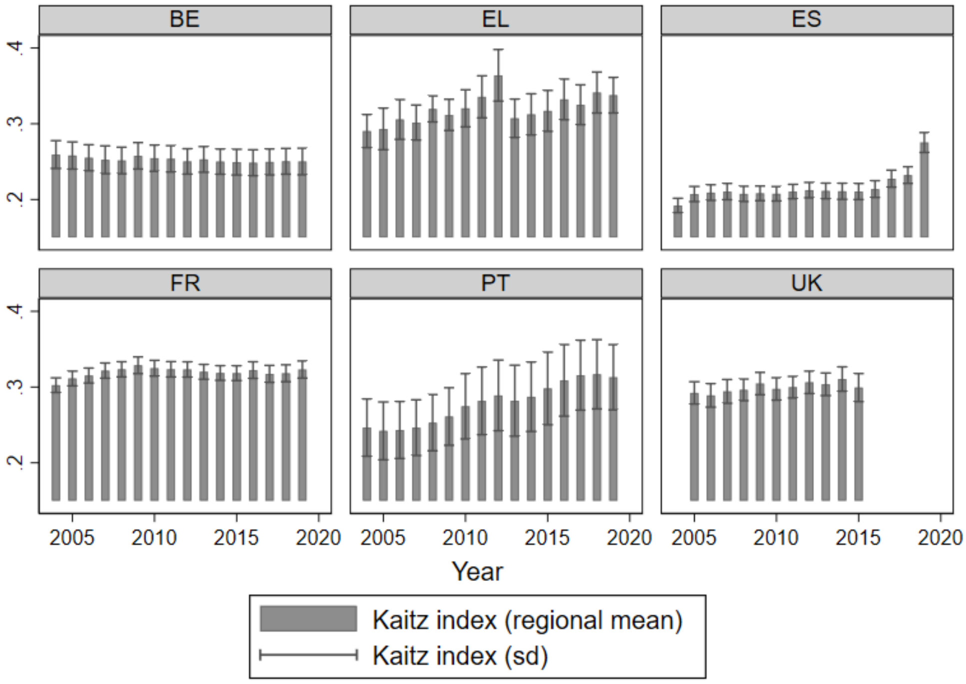
Hamermesh (1981) introduced a Kaitz index variant that takes this argumentation into account. His index considers total compensations instead of solely some measure of paid wages, allowing for comprehensive comparisons across jurisdictions. The measure encompasses nominal local wage levels and adapts to diverse local compensation structures: For instance, it addresses employer-covered tax and social security payments not reflected in gross wages, and it accommodates regional variations in fringe benefits. In regions with higher non-wage costs, the MW impact is relatively less intense in Hamermesh's Kaitz index variant, and it rises if employers cut these costs.

Building on Hamermesh's Kaitz index concept, I combine the nominal MW figures from the WSI with Eurostat's and the UK's Office for National Statistics (ONS) data on the mean regional compensation of employees per hour. Figure 2.4 provides the development of this regional-level Kaitz index over time for the countries of interest in this study. Regional-level descriptive statistics on this measure are available from the appendix, table A.2.

The regional Kaitz index, i.e., the applicable statutory MW relative to the mean compensation of employees in the NUTS-2 regions in my sample, ranges from around 11 percent to 45 percent.[39] The regional mean Kaitz index value is highest in Greece and France, and lowest in Spain. Heterogeneity also exists across time. Belgium, France, and the UK maintained relatively steady Kaitz index values over the sampling period, contrasting with Portugal and Greece, which exhibited significant fluctuations. Substantial increases in Spain's statutory MW toward the end of the sample period led to significantly higher Kaitz values. Internally, Portugal and Greece also exhibit the most notable regional variations, with Belgium also demonstrating a relatively higher degree of regional variation compared to the remainder countries. Notably (but not shown in the graph), Kaitz values in major population centers (primarily the capital regions) tend to be relatively low, while they appear higher in more rural regions. This reflects higher labor compensation costs (and higher costs of living) in cities. In the results section, I test whether my outcomes are heterogeneous with respect to local population density.

[39] The lowest Kaitz index value is reported for region UKI3, i.e. inner London area, in 2015. The largest MW bite was detected in 2012 in EL54, the Greek region of Epirus.

Figure 2.4: Variations in regional Kaitz index



Source: Own elaboration based on WSI (2023) and Eurostat data `nama_10r_2coe`. Data on UK employee compensation missing for 2004 and 2016-2019.

2.4.3 Covariates

My set of covariates aims to capture the factors influencing labor mobility into a region beyond the potential impact of local MWs. These factors encompass changes in a region's attractiveness for workers over time. Following earlier mobility literature (e.g. Boffy-Ramirez, 2013, Cadena, 2014, Giulietti, 2014), my model specifications include indicators capturing regional employment prospects, indicators of local economic development, and historical trends in attracting foreign labor.

The *total population* of an area is indicative of the size of the local labor market and the associated job opportunities.[40] Moreover, it proxies for general infrastructure, such as the availability of public goods and services (education, healthcare, transportation, and the like), and potential network sizes in an area. I include the regional *gross domestic product per capita* in EUR terms to account for differences in economic development, productivity, and income. It proxies for individual's standard of living within a region. The inclusion of the local *unemployment rate* aims to capture the likelihood of actually finding a job in an area, and serves as a proxy for longer-term shifts in labor demand. Its value also signifies the local labor market's health and its resilience to labor market shocks. However, the unemployment rate only reflects the proportion

[40]NUTS delineations, which range from 800,000 to 3,000,000 inhabitants, adhere to population size regulations. While utilizing total regional population as a covariate is a viable option, an alternative approach involves using constant average regional population figures for weighting in the regression. However, the unbalanced nature of my panel data is partly attributed to population changes affecting NUTS delineations (as seen in areas like London), influenced by regional mobility patterns. This necessitates careful control in my estimation strategy. Moreover, employing average population figures for weighting poses challenges in unbalanced panel data due to variations in underlying base years.

of the labor force actively seeking but unable to find employment, excluding those not actively seeking work, unavailable for immediate employment, or who have left the labor force. Consequently, changes in the unemployment rate may not accurately represent shifts in employment opportunities for low-wage workers. To address this limitation, I also consider the *youth employment rate* in an area. Literature, such as Neumark and Wascher (2008), identifies teenagers—due to their typically low qualifications—as the group most affected by MW laws. Therefore, changes in the youth employment rate serve as a crucial indicator of regional labor market trends for low-skilled workers. The relative homogeneity of this group across countries further strengthens its role as an effective measure of the local low-skill labor market (Neumark and Wascher, 2004). Finally, I include the region's *share of foreigners* (individuals born outside the country of interest), to proxy for factors such as immigration history, community networks, and social integration dynamics (Beine et al., 2011). All my covariates are sourced from Eurostat.[41]

2.4.4 Econometric specification

To assess the relationship between MWs and regional labor inflow rates for my sample of EU countries, I broadly adopt methodologies previously used in assessing the United States labor market (Boffy-Ramirez, 2013, Cadena, 2014, Giulietti, 2014, Monras, 2019). In my baseline specification, I apply a fixed effects panel data model at the regional level, nested within the country level. This model is described by the following equation:

$$inflow\_rate^{s}_{i(c),t} = \alpha_0 + \beta_1 lnKaitzIndex_{i,t-1} + \gamma \mathbf{X}'_{i,t} + \lambda_{c,t} + \lambda_t + \epsilon_{i(c),t}, \quad (2.2)$$

where the dependent variable, $inflow\_rate^{s}_{i(c),t}$, is the relative inflow rate of recently arrived low-skilled individuals of characteristic s into region i (located in country c) in year t. In my baseline specification, characteristic s exclusively denotes inflows of individuals holding EU citizenship (i.e., natives and EU mobile citizens). My main variable of interest in the specification is $KaitzIndex_{i,t-1}$ (in logarithmic form), which denotes the value of the local Kaitz index in region i in year $t-1$. Using the lagged value ensures that MW changes always precede potential mobility responses.[42] $\mathbf{X}'_{i,t}$ denotes a vector of time-varying covariates at the regional level. It includes the covariates presented earlier. All covariates are likewise expressed in logarithmic terms and lagged by one year. Nevertheless, I lag the covariate on the share of foreigners by three years to minimize potential noise resulting from simultaneous movements with the dependent variable, the inflow rate. Moreover, λ_t captures time fixed effects, and $\lambda_{c,t}$ adjusts for country-specific linear time trends.[43] $\epsilon_{i,t}$ denotes the error term. In my estimations of

[41] The respective variables are derived from Eurostat data series `demo_r_gind3`, `nama_10r_2gdp`, `lfst_r_lfu3rt`, `lfst_r_lfe2en1`, `lfst_r_lfe2en2`, and `lfst_r_lfsd2pwc`.

[42] When an individual reports a move within the last 365 days, this person may have relocated in the previous calendar year: Imagine the survey interview took place on January 1st in year t, then the actual movement date may have been any day back in time until January 1st in $t-1$. The Kaitz index takes into account the MW on January 1st of each year. Accordingly, under my approach MW changes precede any mobility responses recorded from the data. Moreover, this approach alleviates potential endogeneity concerns.

[43] Region-time trends would capture all the degrees of freedom in my model, see section 2.4.5. Nevertheless, I test the robustness of my results against such a specification in section 2.5.3.

the mentioned model, I modify the standard errors by clustering them at the regional level. This adjustment is made to accommodate for intragroup correlation, i.e. any interdependence observed within regions.

Note that employing fixed effects in estimating the aforementioned model offers significant advantages – most notably, it effectively eliminates cross-regional differences that are constant over time. For instance, certain regions might be especially attractive to outsiders due to their level of urbanization, easy accessibility, specific cultural appeal, extensive networks of foreign residents, or other (rather) time-invariant factors. Permanently elevated (or decreased) labor inflows into a region have no influence on the estimated regression coefficients under my setting.[44] In other words, what I am examining with my model specification is how variations in the regional Kaitz index (or any of my other covariates) correspond to the regional labor inflow rate, irrespective of regional specifics such as region-specific labor market responses. This aspect is also crucial in tackling the data constraints I previously outlined: Most of the identified limitations arise from country- and region-specific characteristics, especially reliant on the national survey design and its regional implementation. Employing a fixed effects model setup can aid in mitigating, and ideally eradicating, any systematic biases in the data, provided these biases remain consistent over time.

Furthermore, all the recognized potential limitations of the data lean towards underestimating the actual labor mobility figures. Technically, in a regression analysis, this makes it more difficult to identify relationship estimates that deviate from zero. Hence, significant coefficient estimates discovered in my analysis are likely to signify the actual direction of the underlying relationship, but may be less precisely estimated (for a detailed examination of this technical aspect, refer to Cohen, 1977). National survey design changes could also influence my model setup and possibly introduce bias into my estimates. I include country-time trends to address changes in the national survey design over time, and also to capture developments in the relative attractiveness of certain countries over others (for instance, due to developments in terms of a country's legal framework, economic developments, etc.).[45]

Potential Sources of Endogeneity

My model inherently encompasses several potential origins of endogeneity. The presented approach establishes statistical correlation, for instance, but it lacks the capability to eliminate the possibility of reverse causality. To tackle this issue, I employ various strategies. One method involves using lag analysis, where I consider lags of two and three years on the Kaitz index rather than just one year. This examination of temporal sequences helps determine whether higher lags consistently show certain patterns, adding robustness to the identification strategy and providing insights into the probable direction of causation. Each additional lag successfully introduced makes a reverse causal relationship less probable. Moreover, in the robustness section of this paper, I perform

[44] The same applies to systematic region-specific deviations among the covariates. Furthermore, since regions are nested within countries, this also includes time-invariant country-specific elements, encompassing legislation, customs, and similar factors.

[45] Underlying country-time trends may exhibit non-linear characteristics, for instance, due to multiple amendments in survey designs. Therefore, I also test first-order non-linear country-time trends in the robustness section of this paper. Moreover, I test the possibility of region-time trends. However, given that my degrees of freedom are essentially zero in such a model setup, I abstain from using it as my main model – also bearing in mind the high risk of overidentifying the model (Wooldridge, 2010).

a reverse causality test to explore whether the labor inflow rates, with all other features held constant, can predict changes in the local Kaitz index (i.e. swapping the dependent variable with the primary independent variable in my model).

Endogeneity can also arise from the correlation between regressors and the error term, due to factors like omitted variables or measurement errors in the data. Additionally, current labor mobility may be influenced by past mobility, following trends such as the business cycle. The examination of EU mobility in section 2.3 suggested potential correlations in the mean regional net migration rates across consecutive periods, particularly in Spain and Greece. Technically, the underlying structure of my panel data may then be dynamic in the sense that it is first-order serially (auto-) correlated. It is plausible that my data even contends with a combination of such issues. For instance, the current unemployment rate might be influenced by the previous period's labor supply, which in turn could be influenced by the preceding period's labor inflows – and the magnitude of these effects may vary across diverse regions and across time.

My primary approach to addressing these concerns involves testing the outcomes of the fixed effects model against the Arellano-Bond estimator (henceforth referred to as the AB model).[46] The AB model is suitable for addressing several endogeneity issues as well as problems associated with autocorrelation and potential heteroscedasticity of data. In essence, it is a dynamic panel data estimator, incorporating lags of the dependent variable as a predictor – a departure that violates the strict exogeneity assumption necessary for fixed effects models (Nickell, 1981). Though basically a random effects model, the AB model applies first differencing to the regression equation. This process effectively removes time-invariant region-specific factors, methodologically akin to the baseline fixed effects model I employ. Furthermore, it tackles endogeneity using an instrumental variable (IV) approach by employing longer lags of the dependent variable as instruments for lags of higher order. As a GMM estimator, it is more efficient than standard IV estimators and other models in the class of dynamic panel data estimators. And despite the endogenous nature of having the dependent variable (lagged) on both sides of the equation it can be shown to be consistent, also in terms of heteroscedasticity (Roodman, 2009). However, this class of models is very sensitive, even with regard to the smallest changes in the methodological setup. I therefore use the model to verify only the results of my fixed effects estimates. As an alternative strategy, I test the robustness of my main results against the heteroskedasticity- and autocorrelation-consistent Newey-West estimator (Newey and West, 1987).

2.4.5 Data sample

My data sample includes all EU15 regions that maintain statutory MWs throughout the entire 2003-2019 sample period, and for which I have adequate data on my mobility rates sourced from the EU LFS. This leaves me with the NUTS-2 regions of Belgium, Greece, Spain, France, Portugal and the UK. As of 2019, the final year of my sample, the six countries account for slightly more than 52 percent of the entire EU15 population. Unfortunately, however, in some countries across various years, the EU LFS lacks information on an individual's residence 365 days ago. In my sample, this pertains to France (no such information has been reported for the years 2003-2005) and the UK (in 2004 and 2008). Additionally, this issue extends to specific regions in France, Greece, and Spain during certain years, contributing to the unbalanced nature of my

[46] See Arellano and Bond (1991), Blundell and Bond (1998), and Roodman (2009).

panel dataset for analysis. Moreover, I refrain from including the French overseas territories, the Greek island regions (except for Crete), the Spanish exclaves of Ceuta and Melilla, and the Portuguese island regions of Acores and Madeira in my analysis. All these regions possess distinctive territorial statuses within their respective countries' legislative frameworks. These statuses result in unique attributes of the local labor markets, including constrained labor mobility and exemptions from statutory MW laws. Finally, there is missing data on the mean compensation of employees (needed to consturct the Kaitz index) and for some covariates for specific regions and years (mainly affecting the UK).

My dependent variable is derived from EU LFS data, which is based on a survey conducted on a population sample. Consequently, potential measurement issues associated with the LFS methodology, coupled with my specific calculation approach, add layers of complexity to this variable. Overall, my derived labor inflow figures resemble previous findings in the literature (refer to section 2.3 and the summary statistics reported in table A.3 in the appendix). Yet certain regions, particularly in Belgium and Greece, exhibit remarkably high labor inflow rates for specific years, a pattern unexpected and unexplained.[47] I follow Aggarwal (2017) and explore common outlier analysis methods to assess the severity of outliers and potential remedies.

First, I seek to verify the estimated values of the abnormal observations with other data sources. Specifically, I compare the labor inflow estimates with Eurostat-published net migration rates (refer to section 2.3). However, I encounter difficulty in validating the accuracy of the extreme percentiles in my sample – the top and bottom 1 percent of values. The correlation with Eurostat's net migration rates is notably weak for these values, yielding a calculated correlation coefficient of around 0.19. Following up, an extreme-value analysis using Tukey fences reveals several *far out* outliers, i.e. values significantly distant from the range statistically to be expected given the overall sample distribution (see figure A.5 in the appendix). The Kurtosis measure on the full dataset exhibits a highly leptokurtic value exceeding 14.[48] To address these abnormal outliers, I trim my sample by excluding both the highest and lowest 1 percent of values, aiming to preserve the distribution's skewness as much as possible. The trimmed sample demonstrates a value range that is less than half the original. Its Kurtosis measure is approximately 4.6, marking a reduction to less than one third of the previous value. For a visual representation of the samples, my outlier analysis and the adopted measure to adjust, refer to the two *Tukey* box-plots in the appendix (figures A.5 and A.6).

Table 2.2 outlines my final (*trimmed*) data sample. For each country, the table lists the number of regions a country consists of, the potential number of observations (derived by multiplying the number of regions by 17, representing the number of years covered in my sampling strategy), and the actual number of observations. Moreover, the table details the reasons for missing data entries, whether due to missing information in the underlying LFS data, missing covariates, or as a result of my trimming approach.

The final data sample comprises 103 NUTS-2 regions, totaling 1,220 observations. The data is unbalanced, with the percentage of missings varying significantly across countries. Portugal has the lowest missing quota at 1.2 percent, while the UK exhibits

[47] Notably, the Athens metropolitan area (Attica, EL30) and Belgium's Flemish and Walloon Brabant (BE24, BE31) regions (surrounding the Brussels capital region), account for 7 of the top 10 highest labor inflow rates in my dataset, including the top three.

[48] In such instances, Dixon's Q test would be preferable for outlier detection and as a criterion for their removal. However, it is unsuitable for unbalanced panel data (Aggarwal, 2017).

Table 2.2: Final data sample

	# of Regions	# of potential observations	# of actual observations	# of missings (LFS)	# of missings (covariates)	# of missings (trimming)	# of missings (total)	missing quota (total)
Belgium	11	187	167	0	11	9	20	0.107
Greece	10	170	114	44	0	12	56	0.329
Spain	17	289	245	42	0	2	44	0.152
France	22	374	271	77	26	0	103	0.275
Portugal	5	85	84	0	1	0	1	0.012
UK	38	646	339	83	224	0	307	0.475
Total	103	1,751	1,220	246	262	23	531	0.303

Note: Potential observations equal the number of regions times the sampling period (2003-2019, i.e. 17 years). LFS missing data refers to suppressed information on an individual's region 365 days ago. Covariate missings reflect absent data in the respective datasets.

the highest at 47.5 percent. France and Greece also show relatively high missing quotas, surpassing 25 percent of potential observations. Notably, missing data patterns differ among countries. Belgium and Greece are particularly impacted by the trimming procedure. The availability of observations in Greece, Spain, France, and the UK is affected by suppressed information within the LFS data. France lacks data for the complete LFS years 2003-2005, while the UK has complete data gaps for 2004 and 2008. In the case of the UK, moreover, the high missing data rate is also due to the complete absence of compensation of employees data (required for constructing the Kaitz index) for the country's 152 observations in the last four years of the sampling period (coinciding with the period after the Brexit vote, which would have presented analytical challenges anyhow). Additionally, alike in Belgium and France, 6-7 percent of missing data is due to gaps in other covariates series. Appendix tables A.2 and A.3 present summary statistics at the regional and country levels, respectively, for my final data sample.

2.5 Results

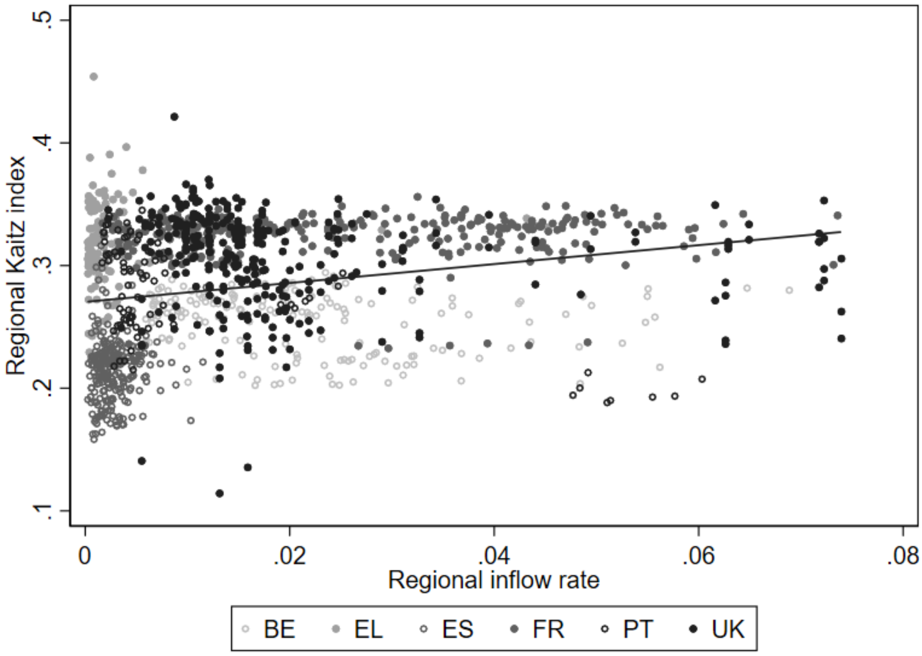
The results section begins with descriptive observations from the data, providing an initial overview. It then proceeds to outline the main findings derived from both the fixed effects and AB models. Following this, the analysis tests the resilience of these results against alternative model specifications. Finally, it explores heterogeneous effects among various subgroups, enhancing the depth of the conclusions drawn.

2.5.1 Descriptive evidence

The introduction of my analysis involves presenting general observations drawn from the data, specifically focusing on the correlation between regional labor inflows and their corresponding Kaitz index scores. Figure 2.5 presents a simple scatter plot that visualizes the relationship between these two measures.

The graph displays a positive relationship between a region's inflow rate and its Kaitz index score, which is accentuated by the added linear fit regression line. However, although a visual correlation is evident for the full sample of countries, it notably disappears when analyzed on a country-by-country basis. The graph distinctly emphasizes diverse patterns among countries. France demonstrates relatively high variability in inflow rates but comparatively lower variability in the Kaitz index. Conversely, Greece and Spain exhibit more consistent inflow rates but greater fluctuations in Kaitz index scores.

Figure 2.5: Scatter plot of regional labor inflow rates versus Kaitz index



Note: The graph plots regional labor inflow rates against the local Kaitz index, with distinct colors and designs for each country. The line represents a linear fit.

Portugal stands out with a seemingly downward-sloping relationship between the Kaitz index value and labor inflow rate. Notably, Portugal displays three distinct observation clusters: one with high inflow rates and Kaitz index values around 20 percent, another with moderate inflow rates aligned with sample-mean Kaitz values, and a third exhibiting low inflow rates along with the highest relative variation in Kaitz scores. Belgium's observations, and even more so, the UK's, appear dispersed across all aspects (yet still implying a positive correlation between the variables).

The overall pattern of a positive relationship between a region's labor inflow rate and its Kaitz index score is also evident when examining a simple correlation table (see in the appendix, table A.4). Alongside the positive correlation with the Kaitz index, the labor inflow rate demonstrates positive correlations with GDP per capita and the youth employment rate, while displaying a negative correlation with the general unemployment rate. Interestingly, the Kaitz index exhibits negative correlations with all covariates except for the youth employment rate, to which it correlates positively. Considering that the Kaitz index is typically lower in urban areas—where population, GDP per capita, unemployment rates, and the proportion of foreigners tend to be higher, and youth employment tends to be lower—all the indicated correlations are comprehensible.

2.5.2 Main result

Section 2.4 detailed the rationale behind my empirical approach and emphasized the baseline model, defined by equation 2.2. Table 2.3 showcases the results of this fixed effects-estimated model. The initial model variant in the table features the baseline model without any covariates. The subsequent model represents the baseline specification with an integration of the comprehensive set of covariates. Additionally, the table demonstrates variations of the baseline specification where the primary variable of interest, the Kaitz index, is lagged by two or three years, deviating from the one-year lag featured in the baseline model.

Table 2.3: Main results – Fixed effects model

VARIABLES	(1) FE without covariates	(2) FE with covariates	(3) (2) with Kaitz (lag 2y.)	(4) (2) with Kaitz (lag 3y.)
ln Kaitz (lag 1y.)	0.015**	0.029***		
	(0.006)	(0.006)		
ln Kaitz (lag 2y.)			0.030***	
			(0.006)	
ln Kaitz (lag 3y.)				0.036**
				(0.014)
Constant	-1.155***	-0.695	1.564***	-0.798
	(0.261)	(0.482)	(0.496)	(0.531)
Model specifications				
Covariates	NO	YES	YES	YES
Region FE	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES
Country-time trend	YES	YES	YES	YES
Observations	1220	1220	1125	1093
Within R2	0.489	0.517	0.500	0.547
Between R2	0.271	0.188	0.120	0.079

Note: Fixed effects (FE) regressions with regions as panel units. The dependent variable is the regional inflow rate of low-skilled individuals. Covariates are the population count, GDP per capita, unemployment rate, youth employment rate, and share of foreigners. The full estimation table is available from the appendix (table A.5). The Kaitz index and all covariates are transformed to logarithm and lagged by one year (the share of foreigners by three years). All model specifications include year fixed effects and country-time trends. Standard errors clustered at the regional level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The outcome of the region-fixed effects model without any covariates, detailed in column (1) of table 2.3, replicates the result from the visual analysis of the scatter plot in figure 2.5: It suggests a generally positive relationship between the Kaitz index and the regional labor inflow rate of low-skilled workers in my sample, significantly estimated at the 5 percent significance level. The model in column (2), which I will refer to as my baseline model from here on, utilizes the full set of covariates and confirms this overall pattern in the data: The coefficient estimate for the Kaitz index is 0.029, highly significant at the 1 percent level. This primary finding suggests that, on average, a 1 percent increase in the Kaitz index is associated with a 0.029 percentage point higher labor inflow rate to a given region, keeping all else equal. Put differently, a one standard deviation increase in the local Kaitz index corresponds to a roughly 10 percent higher labor inflow rate in my sample – indicating an estimated elasticity of roughly 0.18.[49] Notably, the estimated coefficient for the Kaitz index is about 70 percent of the

[49]Remember the mean Kaitz index is around 28.2 percent in my sample, and the average regional labor inflow rate for low-skilled workers is about 1.6 percent. Therefore, at the mean, a 1 percent increase in the Kaitz index (equivalent to a 0.282 percentage point rise) leads to an increase in the local labor inflow rate for the low-skilled from 1.6 percent to 1.629 percent.

estimated magnitude GDP per capita has on labor mobility in my sample, underscoring its substantial role as a determinant of low-skilled labor mobility.[50]

To put this result into perspective, I examine the 4.1 percent increase in the national MW in Portugal in 2015, when the nominal MW changed from 2.92 EUR to 3.04 EUR per hour. This example is illustrative, as the mean MW change in my sample is roughly 4.3 percent, and since Portugal's mean Kaitz index score aligns closely with the overall sample mean Kaitz score. Portugal's MW increase corresponded to an elevation in the country's mean regional Kaitz index from about 28.7 to 29.8, i.e. a mean local Kaitz index increase of 3.8 percent.[51] As per the identified relationship between the Kaitz index and local labor inflow rates, the baseline model predicts that this increase should have led to an approximately 0.11 percentage point higher average labor inflow rate across the sampled Portuguese regions. Consistent with the model prediction (and neglecting any other potential influences on local labor mobility in Portugal), the average regional labor inflow rate in Portugal increased from 0.40 percent in 2014 to 0.54 percent in 2015.

Table 2.3 additionally includes model variations where the Kaitz index is lagged by two and three years, respectively, aimed at bolstering the proposed direction of causality and verifying the credibility of my results. In both models, the coefficient estimates for the Kaitz index remain consistent with the baseline specification, reinforcing its reliability. However, the coefficient estimate for the three-year lag is relatively less precise, likely due to increased noise in the estimation process, compromising its accuracy. Nonetheless, the overall findings from both lagged models lend support to the suggested direction of causality.

2.5.3 Robustness

As outlined in the empirical section, it is crucial to consider potential methodological limitations that may restrict my findings in several ways. Endogeneity, such as the potential impact of reverse causality on result interpretation, cannot be disregarded, and perfect exogeneity of regressors cannot be guaranteed. Moreover, the region-specific nature of labor market responses to changes in MWs and the Kaitz index may result in correlated residuals within regions. Additionally, labor mobility patterns might exhibit trends, leading to a dynamic setting with underlying autocorrelation. A fixed effects model could potentially be susceptible to bias and inefficiencies stemming from the identified methodological issues. The following tests are intended to verify the robustness of my baseline results.

Arellano-Bond dynamic panel estimator

My main approach to test the robustness of the baseline results is utilizing the AB model. It addresses several potential sources of endogeneity in my setting – adjusting for potential underlying dynamic processes (autocorrelation), and integrating instrumental variables to mitigate biases from the correlation of regressors with the error terms, thereby establishing a quasi-causal relationship. However, the AB model's complexity

[50]Refer to the full estimation results detailed in table A.5 in the appendix.

[51]Note that spillover effects of MW amendments to the Kaitz index are typically constrained to be $\leqslant 1$, as MW amendments also affect the Kaitz index' denominator (the mean employee compensation). See also an evaluation of the robustness of the Kaitz index measure in accurately identifying MW amendments in the next section.

and sensitivity emphasize the critical need for accurate model specification: I essentially replicate the fixed effects model configuration by employing an identical set of variables (including time dummies and Country-time trends). In my AB model estimations, the majority of covariates are considered strictly exogenous.[52] Only the Kaitz index and the lag of the labor inflow rate are categorized as (potentially) endogenous. For my purpose, I apply the one-step system GMM estimation, assuming that my orthogonality-adjusted instruments are uncorrelated with the fixed effects (Arellano and Bover, 1995, Blundell and Bond, 1998).[53] To address instrument proliferation (which is particularly pertinent in system GMM-estimated AB models), I restrict the number of lags used as instruments for the endogenous regressors to be strictly between 2 and 5. This approach avoids problems associated with overfitting the endogenous variables.[54] As with the fixed effects model, I cluster standard errors at the regional level (Arellano and Bond, 1991, Blundell and Bond, 1998, Roodman, 2009). The first column of table 2.4 replicates the results from my baseline fixed effects model (as reference). The second column provides the outcome from the AB model.

Table 2.4: Comparing baseline, Arellano-Bond, and Newey-West estimations

VARIABLES	(1) FE model (baseline)	(2) AB model	(3) Newey-West estimator
ln Kaitz (lag 1y.)	0.029***	0.026**	0.029***
	(0.006)	(0.013)	(0.008)
Labor inflow rate (lag 1y.)		0.157***	
		(0.053)	
Constant	-0.695	-0.044	-6.151***
	(0.482)	(0.037)	(0.620)
Observations	1220	1220	1220
R^2	0.517		0.710
# of instruments		165	
Sargan statistic		620.906	
Sargan p-value		0.000	
Hansen J statistic		101.529	
Hansen p-value		0.988	
AR1		-5.095	
AR1 p-value		0.000	
AR2		-0.558	
AR2 p-value		0.577	

Note: The dependent variable is the regional inflow rate of low-skilled individuals. Column (1) reports the results of the baseline fixed effects (FE) model, column (2) of the AB model, column (3) of a Newey-West method estimated OLS model (using region dummies instead of region fixed effects). Standard errors clustered at the regional level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[52]This contrasts somewhat with my earlier discussion regarding control variables as a potential source of endogeneity; however, it substantially simplifies the model (which is crucial for demonstrating my fixed effects model's robustness) and limits the number of applied instruments.

[53]Rather than employing first-differencing by subtracting the previous observation from the contemporaneous one, I apply forward orthogonal deviations (as recommended by Roodman (2009) when dealing with unbalanced panel data). This involves subtracting the average of all future available observations of a variable.

[54]The discussion in Roodman (2009, pp. 98) aids determining an appropriate number of lags utilized as instruments. Upon reducing the number of lags further, I find my results remain largely unchanged.

The AB model confirms the outcomes observed in the baseline fixed effects model. It bolsters confidence in the presumed causal link, suggesting that rises in MWs lead to increased labor inflow rates of low-skilled workers in a region. The magnitude of the estimated coefficient for the Kaitz index is in line with my baseline estimate. However, it is less precisely estimated. Both the Sargan and the Hansen J statistics indicate no overidentification by the number of 165 instruments used in the model.[55] Including the lagged dependent variable as a regressor further exposes an inherent dynamic relationship within the data. The respective coefficient is estimated as highly significant, demonstrating a relatively large positive magnitude (which is consistent across several alternative model specifications I tested). The observed autocorrelation is primarily of first-order; the test for second-order autocorrelation is rejected. Overall, the AB model provides strong support for the estimated relationship being significant and causal.

Incorporating dynamic processes, as identified with the AB model, into fixed effects models presents methodological challenges. In particular, lagged values of the dependent variable are correlated with the error term in such a setting, which violates the Gauss-Markov theorem (Nickell, 1981). To gauge the potential impact of omitted dynamic processes in my baseline fixed effects model, I draw on the Newey-West estimator (Newey and West, 1987), which adjusts estimates for autocorrelation and heteroscedasticity in the error terms. I estimate an OLS model with region-fixed effects using the Newey-West method, everything else alike my baseline fixed effects model. The outcome of this analysis is detailed in column (3), aligning with the earlier models presented in the table and thus affirming the consistency of my overall findings (even in the potential presence of dynamic processes underlying in the data). From this exercise I conclude that the induced bias by dynamic processes, if indeed existing and relevant, is not severely affecting my baseline estimates.

Reverse causality and placebo tests

The analysis of both the lag structure (as depicted in table 2.3) and the findings from the AB model (as presented in table 2.4) concurred in supporting the existence of the proposed causal direction within the identified relationship between Kaitz index changes and labor mobility. To reinforce the causality argument further, I proceeded with several supplementary tests. First, I examine the potential presence of reverse causality within my context by conducting two additional model variations where I reverse the roles of the dependent and main independent variable. I explore two distinct lag structures of the regional labor inflow rate to clarify its influence on current realizations of the Kaitz index: one with no lag and another with a one-year lagged inflow rate. The respective outcomes of these analyses are detailed in table A.6 in the appendix. Notably, the labor inflow rate does not demonstrate statistical significance in explaining the regional Kaitz index in either of the proposed settings – indicating further support for the presumed direction of causality.

Alternatively, I run a placebo test. The MW is likely pertinent primarily to those directly affected by it, or earning wages relatively close to the MW (see also section 2.2). MW amendments should be rather irrelevant for mobility decisions of high earners. Unfortunately, the EU LFS lacks specific information on the surveyed workers' actual income levels. Nevertheless, higher skill levels typically align with higher wages, and vice

[55] The Hansen J statistic comes with a very high p-value, though. Upon reducing the number of lags further, I find my results remain largely consistent while decreasing the Hansen p-value substantially.

versa. The LFS does allow for accurate screening of individuals' skill levels, a feature I use for the following exercise. Table 2.5 presents the results of a placebo test, of a model using the regional inflow rate of *high-skilled* workers as the dependent variable, contrasting with the usual focus on low-skilled workers. Due to its theoretical irrelevance, the estimated coefficient of the Kaitz index on high-skilled workers' mobility rates should be negligibly small or insignificant.

Table 2.5: Placebo test: High-skilled individuals

VARIABLES	(1) FE (baseline)	(2) (1) for high-skilled	(3) (1) using sample of (2)
ln Kaitz (lag 1y.)	0.029***	0.095	0.038**
	(0.006)	(0.112)	(0.016)
Constant	-0.695	2.286	-1.433
	(0.482)	(3.059)	(0.944)
Model specifications			
Covariates	YES	YES	YES
Region FE	YES	YES	YES
Year dummies	YES	YES	YES
Country-time trend	YES	YES	YES
Observations	1220	545	545
Within R2	0.517	0.105	0.682
Between R2	0.188	0.001	0.300

Note: The dependent variable is the regional inflow rate of low-skilled individuals in columns (1) and (3). In column (2), the dependent variable is the regional inflow rate of high-skilled individuals. The model in column (3) differs from (1) in the way that it applies the main model only to the sub-sample of observations of model (2). All other model specifications as in table 2.3. Standard errors clustered at the regional level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

As evident from column (2) in the table, the coefficient on the Kaitz index is, as expected, insignificant: I fail to detect a statistically significant relationship between the local Kaitz index score and the labor inflow rate of high-skilled workers to a region. The standard error of the estimated coefficient is even larger than the coefficient itself. Note, however, that the number of observations is substantially lower for this sample compared to my baseline result, attributable to a considerable reduction in the domain size when focusing solely on high-skilled workers with recent mobility backgrounds (of which only relatively few exist in the data). Accordingly, the results from this model cannot be compared directly to the reference model presented in column (1). In order to rule out the possibility that there are sample composition effects that drive this result, I re-run my baseline model, but restrict it to the sample underlying the model in column (2). This model variation is reported in column (3). I still find a robust result of somewhat comparable magnitude to the original baseline model in column (1), but with a slightly increased p-value (which is plausible given the massively reduced sample size). In essence, the placebo test lends further support for the proposed causal mechanism behind my baseline result.

Other robustness tests

I run several further robustness tests to fortify the reliability of the findings and to check the credibility of my estimates. First, I check my model against the importance of single countries for my overall outcome. While the sample sizes of the individual countries are too small to be estimated separately, I estimate my model by excluding each country one-at-a-time (leave-one-out – L1O). I report the results in table A.7 in the appendix; none of the countries is dominant in explaining my overall result, and any country of my sample may be dropped from the sample without significantly changing the basic result – that increases in the Kaitz index relate to higher inward low-skilled labor mobility at the regional level.

Moreover, and methodologically somewhat similar, I run a Jackknife resampling estimation (testing the importance of single regions for my outcome), and also use bootstrapping (employing 1000 replications) to test the stability of my results. Both these exercises (detailed in table A.8 in the appendix) yield outcomes in line with my baseline finding. Expanding the scope of assessment, I further evaluate my baseline findings by adjusting for *region-time trends* rather than country-time trends. While this approach might seem preferable initially, it also takes away all the degrees of freedom in my model, excluding the possibility of various statistical testing (such as assessing the overall significance of the model). Notwithstanding this drawback, the model including region-time trends yields similar coefficient estimates—both in terms of magnitude and significance—to those in my baseline model (refer to column (4) in table A.8). Furthermore, I scrutinize the nature of the country-time trends utilized in my analysis. Given variations in the LFS sampling design across different periods within certain countries, there is a possibility of time-varying impacts on the estimates. To address this, I implement a model incorporating first-order non-linear country-time trends in column (5) of table A.8. This adjustment aims to account for any potential time-varying effects at the country level, impacting all regions within a country. Again, this test results in a robust estimate, akin to the findings derived from the baseline model.

Finally, I assess the credibility of my MW measure. One may be skeptical that MW increases are the actual drivers of the variation in the Kaitz index in my data. It could also be changes in employee compensation (the other Kaitz index component), for reasons unrelated to the MW, that mainly affect the development of local Kaitz index scores over time. In the worst case, my specification would capture a relationship of mobility responses due to variations in local compensation levels, not due to alterations in MWs. To address this concern, I first calculate the Pearson correlation coefficient between the Kaitz index and its two components: the nominal MW level and the mean compensation of employees in an area. The results show that in my sample, the Kaitz index is substantially more correlated with the MW ($\rho \approx +0.45$) than with the compensation measure ($\rho \approx -0.03$). This indicates that MW amendments drive Kaitz index fluctuations more significantly in my sample than local compensation levels do.

To further validate my findings, I re-evaluate my baseline results using two modified versions of the Kaitz index, both of which keep the denominator (local compensation level) constant over time. First, I use each region's initial observation of the local compensation level to construct the area's Kaitz index scores over the entire sampling period. Second, I calculate the regional mean value of local compensation levels over time and use this time-invariant value as denominator in the Kaitz index throughout all observations of a specific region. Both these amended measures yield estimation outcomes in line with my baseline estimate, showing slightly higher magnitudes but comparable standard errors and p-values. The estimated coefficients are statistically in line with my baseline estimate. This test demonstrates that variations in applicable MWs drive my overall results, not variations in local compensations. This also lends some interpretation for my overall result: Higher MWs do attract incoming worker mobility, and in particular to those regions with relatively low compensation levels. All the robustness tests mentioned are accessible in the appendix, specifically detailed in table A.8.

2.5.4 Heterogeneity analysis

There are several potential sources of heterogeneity across spatially diverse regions, as well as at the micro level (as indicated in sections 2.2 and 2.3). For instance, natives may exhibit distinct mobility patterns compared to other EU citizens, despite equal access to the labor market. Additionally, the assessment of MW changes in the home country labor market may differ from evaluations in other EU countries' labor markets. Consequently, mobility patterns may vary between domestically relocating workers and those engaging in cross-border labor mobility. Moreover, gender and age disparities may also be influential factors. These forms of heterogeneity are considered in the following.

Spatial heterogeneity

Figure 2.5 visually illustrates potential variations in the relationship between the Kaitz index and regional inflow rates across countries. To explore this aspect further, I enhance my model by introducing an interaction term between the primary variable of interest, the Kaitz index, and a categorical variable identifying the country in which a region is located. I conduct two variations: First, I use Greece, the smallest country (in terms of population) with the lowest mean regional labor inflow rates in my sample, as the reference category. Alternatively, I utilize the UK as the reference category, being the largest country in my sample with the highest number of observations. The corresponding results are detailed in the appendix, table A.9 (columns 2 and 3). I detect little heterogeneity across the sampled countries under these specifications, with the overall relationship between Kaitz index scores and labor inflow rates remaining largely consistent. Notably, only France exhibits a significant positive deviation in the point estimate (at the 10 percent level of significance), but this is observed only when Greece serves as the reference. Region-fixed effects and country-time trends may capture most cross-country variations, and the rather limited number of observations per country may further contribute to imprecision in estimating point estimates. Moreover, my outcomes may also indicate more substantial within-country variability (as also the reported R-squared values suggest), an aspect I investigate further below.

However, while I find little heterogeneity among countries overall, my results show some distinct (but mostly insignificant) positive point estimates for Belgium, France and the UK, while the remaining southern European countries of my sample deviate negatively. Therefore, alternative to the previous approach, I employ a grouping strategy to analyze location-specific heterogeneity. I categorize the sampled countries into two groups: Into southern European countries (Greece, Portugal, Spain) and central European countries (Belgium, France, and the UK). This grouping reflects geographic, economic, and, to some extent, cultural similarities, as well as relative levels of internal worker mobility (see section 2.3, figure 2.5, and table A.3). Similar to the previous analyses, I interact the categorical variable with the Kaitz index measure. As reference category I define the group of southern European countries. The results are detailed in column 4 of table A.9. Under this specification, the overall outcome for the influence of Kaitz level variations on incoming worker mobility is consistent with previous outcomes, though slightly smaller in magnitude and less precisely estimated. Central European countries deviate notably from this reference outcome, showing a significantly larger coefficient magnitude. Accordingly, there seems to be a distinct relationship for the estimated relationship between the southern European countries and the central European ones – MWs appear more relevant for worker inflows in the central European countries compared to the group of southern European countries. A potential rationale behind this finding is the higher cultural value Mediterranean societies place on family ties: Even in the presence of financial incentives, southern European individuals have been found to be less inclined to relocate from their local communities (Alesina et al., 2015).

Another explanation for these findings may be the more substantial economic differences across regions within the middle European countries, particularly between the urban agglomeration regions (especially the capital regions) and other regions in the countries. The scatter plot in figure 2.5 visually supports this interpretation. For instance, the mean Kaitz index value for the Île-de-France (the Paris metro region – NUTS-2 region code FR10), is about 10 percentage points lower than in all other regions of France, i.e. the 'bite' of the MW is substantially lower than elsewhere in France (refer to table A.2 in the appendix). Similar large differences between major urban centers and more rural areas can be observed in Belgium and the UK, and to some extent also in Portugal. Provided the 'bite' of MWs is stronger in rural areas, increases in national MWs should draw more individuals to these areas, my results suggest. On the other hand, considering the appeal of urban centers to many individuals, even slight alterations in relative affordability can result in significantly increased labor inflows (Harris and Todaro, 1970). Though my specification adjusts for region-fixed effects, it does not necessarily adjust for any such potential systematic differences the MW might have between urban and rural areas in my data. I investigate this aspect by introducing a categorical variable identifying urban areas to my baseline estimation. Urban areas are defined as regions with higher than median population density within my sample.[56] However, the results on this exercise (reported in column 5 of table A.9) show that urban regions do not yield significantly different results from the baseline specification.[57]

Heterogenous effects in citizenship and domestic mobility

In section 2.3, I demonstrated the historical diversity of migration legacies within the countries comprising my sample, reflected in varying stocks of foreigners, intensities of worker inflows from abroad, and net migration rates. At the regional level, overall in-

[56]Other thresholds (top 25 and top 10 percent of regions in terms of population density) do not yield substantially different results.

[57]An alternative strategy to investigate the impact of urban areas is to split the sample into the relatively more urban and rural areas and investigate the subsets separately. Observation numbers are still reasonably large for both groups in such a setting, and the mean Kaitz index values and the mean labor inflow rates in both urban and rural subsamples closely align with those in the baseline setting. The results of this exercise are depicted in columns (2) and (3) of table A.10. The estimated influence of the Kaitz index in urban regions surpasses the one estimated for rural areas by a magnitude of almost twice. However, the confidence intervals of both estimated coefficients overlap, and a corresponding t-test shows no statistically different results.

coming mobility is notably characterized by internal mobility, and significantly influenced by the mobility decisions of nationals. Roughly 90 percent of mobile workers in my sample are citizens of the country in which their destination region is located, and about 12.5 percent of mobility counts involve cross-border movements. Dustmann and Preston (2019) argue that monetary incentives may influence mobility decisions differently, depending on whether mobility is internal or external, and whether individuals are nationals or international migrants. I assess the significance of these patterns in my data using two analytical approaches: I assess the significance of potentially varying patterns in my data by employing two analytical approaches: First, I explore the distinct mobility response exclusively among natives. Second, I evaluate whether restricting my mobility measure to internal mobility yields different results. The respective outcomes are presented in table A.10 in the appendix.

In the native-only sample, I find a significant coefficient estimate of 0.025, roughly similar to the baseline scenario, with similar interpretation. Given the slightly higher baseline estimate reflecting the average response of both natives and EU citizens, this suggests natives may be relatively less responsive to Kaitz index variations – though this inference cannot be empirically verified due domain size limiations.[58] However, this interpretation gain further support by a significant positive estimate of 0.020 when examining the Kaitz index's relationship with internal labor mobility (of both natives and EU workers). As before, the estimated coefficient is smaller than the baseline estimate (though not statistically different), indicating domestic mobility may be relatively less responsive than cross-border mobility. Again, domain size limitations prevent a deeper analysis using my data sample. Nevertheless, both these findings align with literature suggesting that migrant workers are more mobile, less restricted in location choice, and more responsive to monetary incentives (see section 2.2).

Heterogenous effects across sex and age

Another potential source of heterogeneity at the individual level is respondents' sex. The influence of MWs on labor inflow rates may vary between males and females. This may be due to distinct average characteristics of male versus female workers (for instance, in terms of average education) or to heterogeneous mobility patterns exhibited by each sex. Similarly, age may play a significant role in shaping the examined relationship: It has been observed that both inter-regional and cross-border mobility tend to be more prevalent among younger individuals (e.g., Eurofound, 2014, European Commission, 2022). Furthermore, a considerable body of literature investigates the labor market impacts of MWs specifically among young workers – presumed to be most affected by MWs due to their inherently lower levels of education (see also section 2.4). I explore potential heterogeneity across these dimensions in table A.11 by amending the dependent variable – specifically focusing on labor inflow rates of females, males, or individuals aged 27 years or younger.

I observe only marginal differences between low-skilled male and female workers. Outcomes closely align the baseline model estimates. Young low-skilled workers, in contrast, appear to have a higher responsiveness to MWs: A 1 percent increase in the Kaitz index corresponds to a 0.47 percentage point increase in the respective labor inflow rate – a coefficient magnitude approximately 50 percent higher than the one derived

[58] Unfortunately, the LFS's limited domain size precludes independent regional-level analysis of low-skilled working-age EU citizens (i.e. investigating region's populations of EU citizens except for natives).

from the baseline model. However, given the elevated standard error and the lower number of observations under this specification, the coefficient estimate is statistically not significantly different to the baseline outcome.[59]

2.6 Conclusion

Several studies have investigated the effects of MWs on spatial labor mobility patterns within the United States labor market. However, our understanding of how MWs influence mobility patterns in other parts of the world remains limited. The EU presents a particularly compelling case study due to its freedom of movement of workers, a significant diversity in institutional settings across countries, and substantial regional variations in economic fundamentals, customs, and numerous other regional features potentially influencing the attractiveness of regions to outsiders. This study contributes to the broader literature by exploring MWs' impact on regional incoming labor mobility counts within the EU.

Specifically, I examined the mobility impact of changes in the Kaitz index, defined as the MW relative to the mean local compensation of employees, acknowledging the varying local relevance of MWs across countries and regions. For my analysis, I utilized cross-country harmonized regional mobility data across NUTS-2 regions derived from the EU LFS, with my dependent variable being the local share of low-skilled workers holding EU citizenship (which includes domestic nationals), who relocated to a region in the past 365 days. My baseline fixed effects model revealed a significant positive association between the Kaitz index and incoming regional labor mobility in Europe. A one percent increase in the Kaitz index corresponds to a 0.03 percentage point higher local inflow rate of low-skilled workers, all else equal. While this correlation may appear modest, it holds significance – implying an elasticity of approximately 0.18. At the mean, a one percent increase in the Kaitz index is associated with a 0.18 percent higher regional labor inflow rate, or, put differently, a one standard deviation Kaitz index increase associates with a 10 percent higher inflow rate. This primary finding has proven robust across various alternative model specifications and robustness tests. Additionally, an AB dynamic GMM panel estimator, alongside multiple other tests, has provided support for the identified relationship being causal.

Moreover, a heterogeneity analysis revealed several significant variations. While little variability was observed among countries when examined individually, notable differences emerged when grouping my sample into the central European countries versus the southern European countries. Specifically, I found that Kaitz index scores are significantly more relevant for incoming mobility counts of low-skilled workers in the central-European country group encompassing Belgium, France, and the UK, relative to the Kaitz impact observed among the Mediterranean countries of Greece, Spain, and Portugal. Furthermore, when limiting my mobility measure to either native workers only or to domestic mobility, I found a similar responsiveness to Kaitz index changes as under my baseline, though the effect was slightly smaller (but not significantly distinct). Additionally, I observed no statistically distinct variations due to individual characteristics like sex and age, although the estimate for younger workers was 50 percent higher than the baseline.

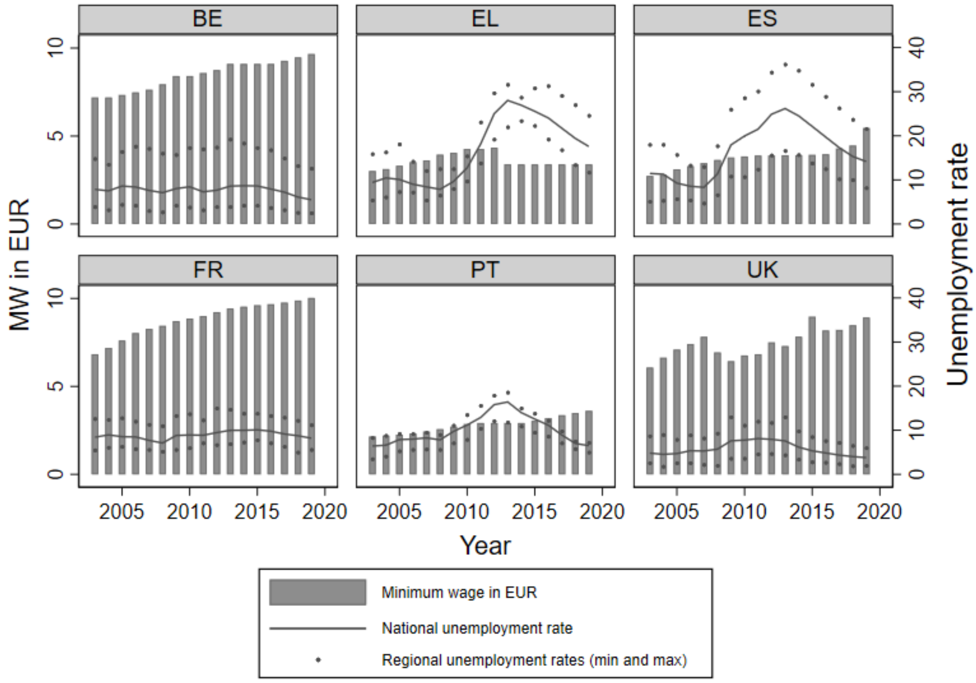
[59] It is important to reiterate that estimating subgroups of the population with my data may be susceptible to domain size problems. In practical terms, this leads to a reduction in the number of observations relative to the baseline model in this instance.

My study reveals several key implications for labor mobility and wage policy in Europe. Firstly, the local impact of MWs is significant for worker mobility in Europe. Higher MWs attract incoming low-skilled worker flows, particularly to regions where the MW 'bites' the most – typically rural areas where overall compensation levels are lower. This outcome aligns well with earlier research on the United States labor market. Secondly, the relationship identified is spatially heterogeneous across Europe, with a stronger impact observed in the central European countries compared to the sampled group of southern European countries. This finding likely reflects greater economic disparities between regions and the generally higher internal mobility in central Europe, as well as cultural factors that influence responsiveness to changing economic conditions. Thirdly, there is some indications that different groups of workers may react differently to Kaitz index fluctuations. For example, young workers showed by far the highest coefficient estimate for MW impact among all the tested models. Understanding spatial and demographic-specific variations can inform targeted policy interventions and labor market strategies that address specific demographic or occupational needs. Implementing region-specific or age-adjusted MWs, for instance, could help address demographic changes in certain areas. These implications are especially relevant in light of EU directive 2022/2041, which mandates that member states maintain adequate MWs from 2025 onwards. Countries like Belgium, Greece, and Spain will need to make substantial adjustments to their MW legislations. Significantly increased, but locally varying incoming worker mobility counts will most likely be a consequence. Tailoring MW legislation to account for regional and demographic factors could help achieve policy objectives beyond merely setting entry-level wages and adjusting for EU requirements.

MWs are just one mechanism that can influence worker mobility. The EU, with its diverse range of domestic labor market regimes, offers a compelling case for studying how different institutional settings affect worker mobility. Future research could particularly explore the complementary and substitution effects of various labor market instruments, investigate heterogeneous worker responses both within and across countries, and examine the role of cultural factors in determining the effectiveness of instruments. Additionally, addressing current shortcomings in mobility data is essential. For instance, individual-level data that includes detailed information on both the origin and destination of mobile workers could significantly enhance EU labor market policy analyses in the future.

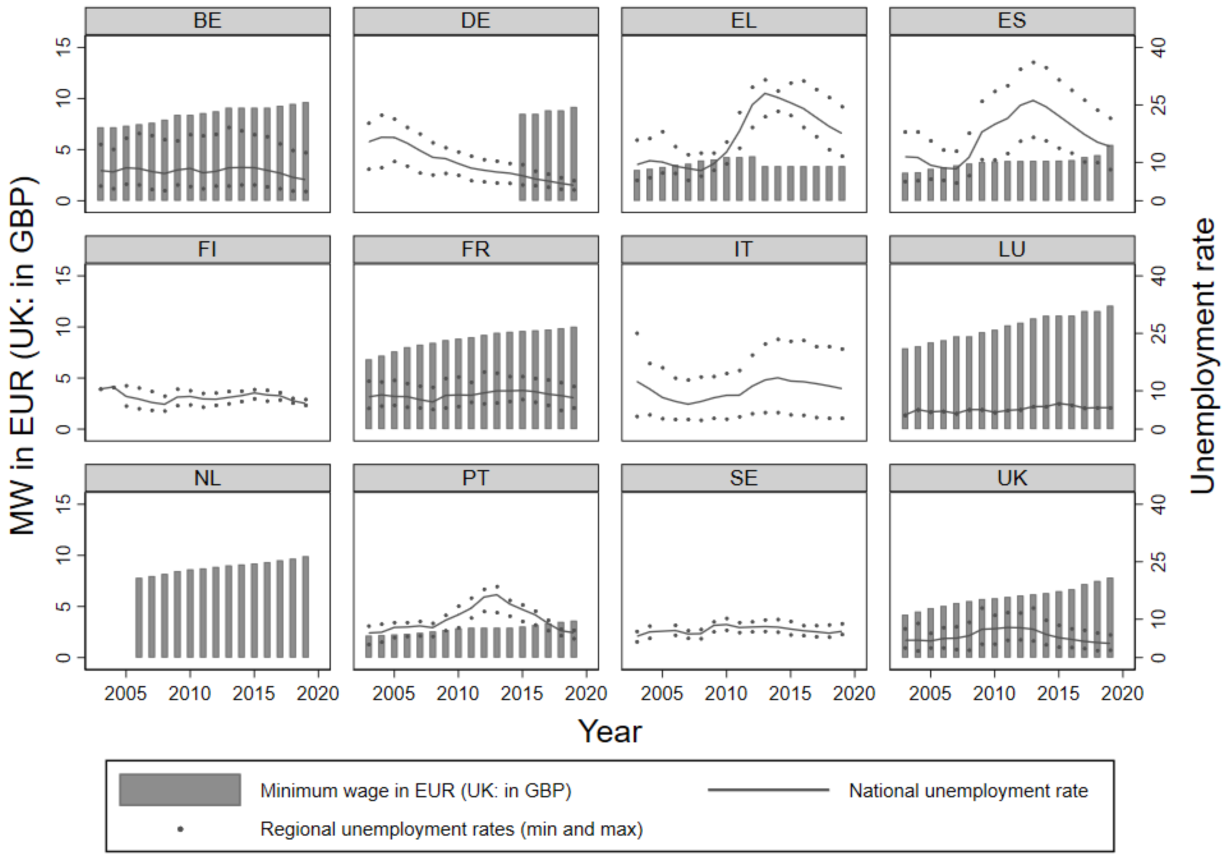
2.7 Appendix A

Figure A.1: EUR-denominated nominal MWs and unemployment rates



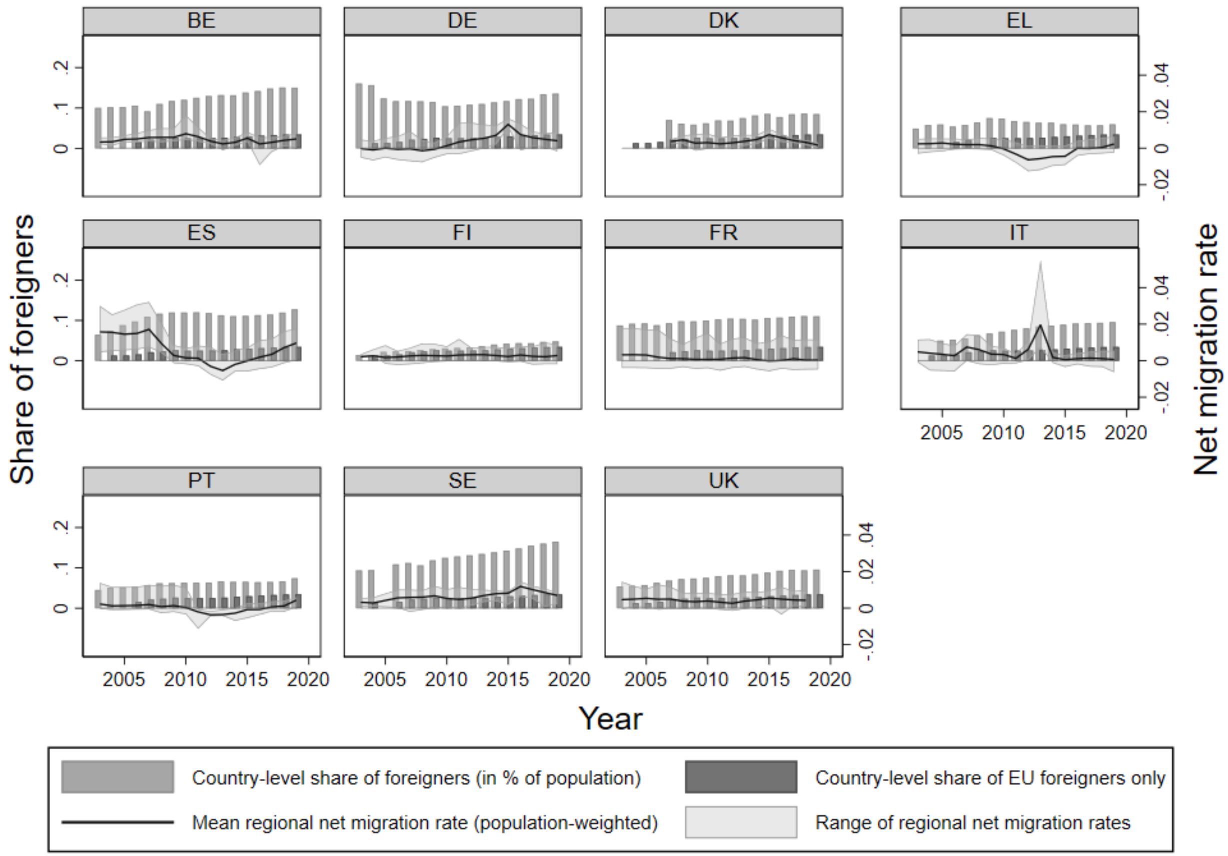
Note: Same graph as in figure 2.2, except the UK MW is shown in EUR-denominated terms, reflecting exchange rate fluctuations.

Figure A.2: EU-15: MWs and unemployment rates



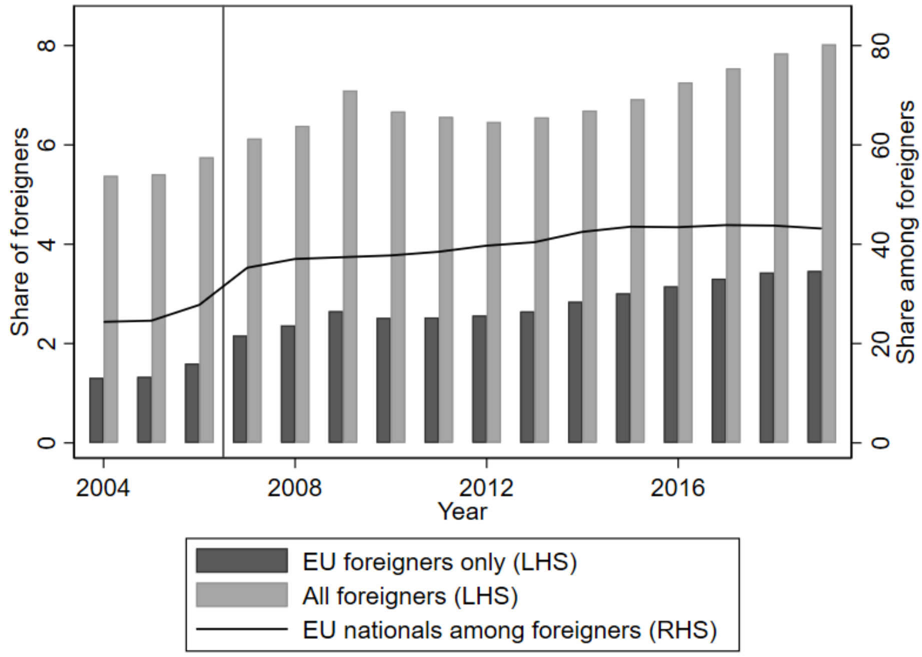
Source: Own elaboration, based on data from WSI (2023) and Eurostat data sets `lfst_r_lfu3rt` and `une_rt_a_h`.
Note: MW levels are indicated by the bars. A missing bar means no statutory nationwide MW existed that year. Blank spaces on unemployment rates indicate missing data. Graphs for Austria, Denmark, and Ireland excluded due substantial amounts of missing information.

Figure A.3: Share of foreigners and net migration rates



Source: Own elaboration, based on Eurostat dataset series `lfst_r_lfsd2pwc`, `demo_r_gind3`, and `migr_pop1ctz`.
Note: Data for Luxembourg not included in the chart (extreme outlier in terms of share of foreigners, which is roughly between 30-40 percent during my entire sampling period). Data on Austria and the Netherlands not included due missing data. For all countries contained in the chart: Blank spaces indicate missing data.

Figure A.4: EU-wide share of foreigners



Source: Own elaboration, based on Eurostat data series `migr_pop1ctz`.
Note: Bars represent the proportion of foreigners in the EU, either "All Foreigners" or the share of "EU foreigners", i.e. locally residing EU citizens with citizenship other than the hosting country's (depicted on the left-hand scale, LHS). The line shows the ratio of EU foreigners to all foreigners, depicted on the right-hand scale (RHS). Due to missing data, Estonia, Ireland, Greece, France, and Portugal are included in the data series only from 2007, marked by the vertical line. Malta is included in the data series only from 2009 and Romania from 2012 onwards. Bulgaria is absent in the data in 2007, Greece in 2008, and Poland in 2009.

Table A.1: UK – NUTS-2 to NUTS-1 regions crosswalk

	NUTS-1		NUTS-2	
Country	Region	Code	Region	Code
England	North East	UKC	Tees Valley and Durham	UKC1
			Northumberland and Tyne and Wear	UKC2
	North West	UKD	Cumbria	UKD1
			Cheshire	UKD6
			Greater Manchester	UKD3
			Lancashire	UKD4
			Merseyside	UKD7
	Yorkshire and the Humber	UKE	East Riding and North Lincolnshire	UKE1
			North Yorkshire	UKE2
			South Yorkshire	UKE3
			West Yorkshire	UKE4
	East Midlands	UKF	Derbyshire and Nottinghamshire	UKF1
			Leicestershire, Rutland and Northamptonshire	UKF2
			Lincolnshire	UKF3
	West Midlands	UKG	Herefordshire, Worcestershire and Warwickshire	UKG1
			Shropshire and Staffordshire	UKG2
			West Midlands	UKG3
	East of England	UKH	East Anglia	UKH1
			Bedfordshire and Hertfordshire	UKH2
			Essex	UKH3
	London	UKI	Inner London - West	UKI3
			Inner London - East	UKI4
			Outer London - East and North East	UKI5
			Outer London - South	UKI6
			Outer London - West and North West	UKI7
	South East	UKJ	Berkshire, Buckinghamshire, and Oxfordshire	UKJ1
			Surrey, East and West Sussex	UKJ2
			Hampshire and Isle of Wight	UKJ3
			Kent	UKJ4
	South West	UKK	Gloucestershire, Wiltshire and Bristol/Bath area	UKK1
			Dorset and Somerset	UKK2
			Cornwall and Isles of Scilly	UKK3
			Devon	UKK4
Wales	Wales	UKL	West Wales and The Valleys	UKL1
			East Wales	UKL2
Scotland	Scotland	UKM	North Eastern Scotland	UKM5
			Highlands and Islands	UKM6
			Eastern Scotland	UKM7
			West Central Scotland	UKM8
			Southern Scotland	UKM9
Northern Ireland	Northern Ireland	UKN	Northern Ireland	UKN0

Note: Table provides the crosswalk used between NUTS-1 and NUTS-2 regions in the UK (NUTS version 2016), based on historical NUTS data by Eurostat (see `https://ec.europa.eu/eurostat/web/nuts/history`, last accessed: 27.07.2024).

Table A.2: Regional-level summary statistics

Region	# of observations	mean N	mean inflow rate	s.d. inflow rate	mean Kaitz Index	s.d. Kaitz Index	mean Population	mean GDPpc	mean UR	mean Youth Empl. Rate	mean Share Foreigners
BE10	16	7716	0.0385	0.0130	0.2059	0.0028	1118409	64135	0.1627	0.1737	0.3696
BE21	16	6444	0.0171	0.0123	0.2383	0.0024	1767956	40964	0.0550	0.2891	0.1066
BE22	16	5051	0.0176	0.0106	0.2708	0.0029	844192	28301	0.0514	0.3176	0.1063
BE23	16	6416	0.0225	0.0149	0.2569	0.0033	1445809	31481	0.0408	0.3102	0.0591
BE24	14	5991	0.0318	0.0095	0.2216	0.0030	1082315	36260	0.0445	0.2459	0.0886
BE25	16	6059	0.0169	0.0104	0.2788	0.0046	1166554	33245	0.0358	0.3306	0.0464
BE31	13	3537	0.0392	0.0050	0.2279	0.0045	379996	37132	0.0743	0.1760	0.1226
BE32	15	5788	0.0191	0.0097	0.2697	0.0039	1316609	22057	0.1251	0.1948	0.1261
BE33	16	6518	0.0161	0.0101	0.2629	0.0041	1076170	25035	0.1094	0.2181	0.1432
BE34	15	3842	0.0300	0.0149	0.2849	0.0042	271330	22122	0.0735	0.2578	0.0972
BE35	14	3858	0.0302	0.0112	0.2657	0.0056	474758	23478	0.0909	0.2235	0.0742
EL30	7	40478	0.0015	0.0013	0.3088	0.0094	3857174	23873	0.1983	0.1570	0.0882
EL43	10	12021	0.0021	0.0016	0.3571	0.0285	622246	15318	0.1640	0.2068	0.0563
EL51	13	11210	0.0059	0.0030	0.3197	0.0174	602368	13165	0.1425	0.2178	0.0504
EL52	16	24176	0.0089	0.0109	0.3411	0.0153	1895851	14286	0.1739	0.1589	0.0586
EL53	12	5084	0.0035	0.0017	0.2444	0.0223	283334	17221	0.2157	0.1284	0.0273
EL54	11	10938	0.0042	0.0020	0.3586	0.0354	344005	13458	0.1405	0.1761	0.0299
EL61	14	9337	0.0033	0.0020	0.3074	0.0146	737927	13724	0.1599	0.1876	0.0323
EL63	13	10885	0.0030	0.0016	0.3235	0.0223	684775	13851	0.1745	0.1653	0.0294
EL64	10	10248	0.0026	0.0014	0.2953	0.0175	556977	16661	0.1661	0.2235	0.0459
EL65	8	10453	0.0015	0.0009	0.3023	0.0338	587188	14641	0.1329	0.2031	0.0491
ES11	17	8630	0.0041	0.0012	0.2250	0.0170	2736848	20056	0.1455	0.2389	0.0612
ES12	11	3068	0.0041	0.0021	0.2062	0.0222	1054064	20417	0.1247	0.2299	0.0482
ES13	13	2503	0.0042	0.0027	0.2133	0.0176	579072	21509	0.1305	0.2265	0.0644
ES21	14	4722	0.0036	0.0021	0.1753	0.0165	2151930	29077	0.1086	0.2435	0.0593
ES22	8	2720	0.0042	0.0020	0.1902	0.0208	616426	28326	0.0894	0.2895	0.0973
ES23	12	1638	0.0093	0.0051	0.2229	0.0214	311531	24748	0.1263	0.2584	0.1154
ES24	15	4219	0.0043	0.0022	0.2032	0.0108	1305787	24728	0.1176	0.2895	0.1037
ES30	16	5198	0.0048	0.0019	0.1810	0.0161	6249384	31157	0.1252	0.2816	0.1473
ES41	15	9446	0.0051	0.0022	0.2124	0.0194	2500150	21043	0.1404	0.2488	0.0591
ES42	17	6905	0.0061	0.0019	0.2289	0.0190	2010048	18293	0.1838	0.2637	0.0833
ES43	17	3695	0.0044	0.0027	0.2371	0.0203	1084095	16055	0.2268	0.2205	0.0320
ES51	14	9574	0.0032	0.0011	0.1966	0.0184	7298320	27630	0.1340	0.3219	0.1361
ES52	17	7641	0.0039	0.0015	0.2248	0.0191	4846678	20464	0.1763	0.2817	0.1424
ES53	16	2396	0.0055	0.0022	0.2229	0.0183	1065672	25086	0.1448	0.3204	0.1969
ES61	17	16810	0.0032	0.0009	0.2312	0.0208	8165873	17360	0.2418	0.2293	0.0757
ES62	11	3334	0.0038	0.0009	0.2464	0.0245	1412914	19256	0.1685	0.3009	0.1383
ES70	15	5323	0.0043	0.0020	0.2274	0.0219	2012625	19826	0.2186	0.2407	0.1474
FR10	13	47325	0.0335	0.0220	0.2321	0.0053	11950142	54194	0.0851	0.2601	0.1941
FR21	13	9217	0.0407	0.0202	0.3371	0.0040	1334275	27854	0.0989	0.2998	0.0672
FR22	12	9414	0.0330	0.0163	0.3285	0.0034	1921481	24463	0.1025	0.2967	0.0609
FR23	12	9454	0.0374	0.0212	0.3140	0.0026	1844453	28274	0.1023	0.3039	0.0551
FR24	13	10668	0.0353	0.0175	0.3334	0.0039	2560665	27162	0.0845	0.3113	0.0670
FR25	13	7843	0.0380	0.0176	0.3392	0.0036	1473434	25901	0.0795	0.3360	0.0338
FR26	13	7823	0.0425	0.0172	0.3337	0.0034	1637748	27106	0.0879	0.3137	0.0681
FR30	13	20994	0.0290	0.0195	0.3204	0.0028	4054731	26417	0.1272	0.2591	0.0485
FR41	12	10515	0.0334	0.0201	0.3311	0.0037	2341000	24462	0.1022	0.3210	0.0876
FR42	12	9268	0.0318	0.0184	0.3092	0.0036	1863162	30007	0.0840	0.3379	0.1102
FR43	13	7205	0.0345	0.0205	0.3319	0.0052	1174028	25109	0.0825	0.3213	0.0717
FR51	13	16652	0.0369	0.0171	0.3312	0.0038	3652633	28768	0.0792	0.3458	0.0412
FR52	13	13948	0.0439	0.0193	0.3387	0.0037	3248918	27269	0.0709	0.3096	0.0353
FR53	13	8237	0.0387	0.0176	0.3457	0.0046	1785626	26150	0.0858	0.3329	0.0499
FR61	13	13305	0.0424	0.0147	0.3253	0.0080	3310871	28727	0.0885	0.2976	0.0905
FR62	13	11374	0.0521	0.0262	0.3142	0.0056	2948783	29215	0.0795	0.2961	0.0965
FR63	13	6392	0.0429	0.0211	0.3344	0.0051	737412	24267	0.0708	0.3570	0.0574
FR71	13	26330	0.0357	0.0199	0.3046	0.0038	6385541	32662	0.0776	0.3251	0.1035
FR72	11	6053	0.0439	0.0215	0.3321	0.0049	1355103	25786	0.0775	0.3368	0.0574
FR81	13	10807	0.0437	0.0185	0.3294	0.0056	2715057	24808	0.1215	0.2314	0.1356
FR82	13	19195	0.0348	0.0179	0.3094	0.0055	4961786	30430	0.0970	0.2735	0.1599
FR83	4	988	0.0255	0.0177	0.2984	0.0075	328973	26413	0.0863	0.3853	0.1143

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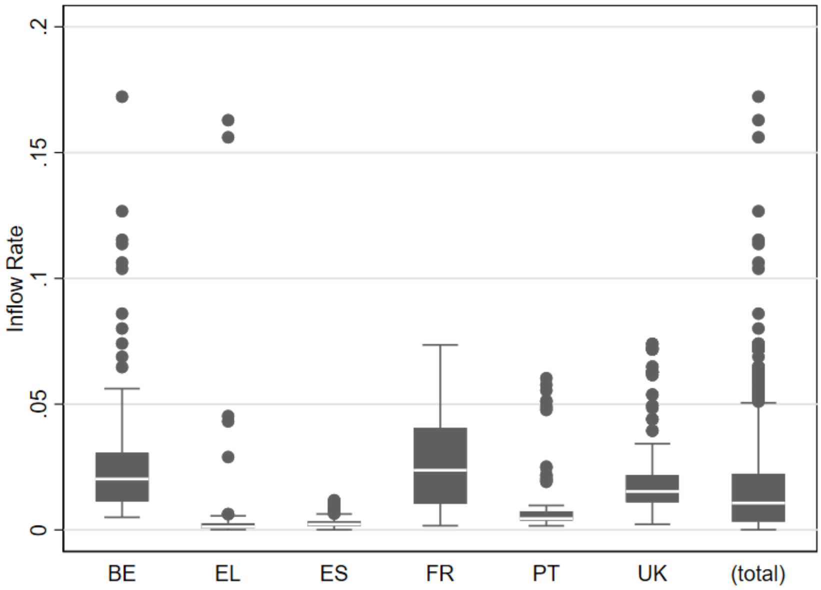
Region	# of observations	mean N	mean inflow rate	s.d. inflow rate	mean Kaitz Index	s.d. Kaitz Index	mean Population	mean GDPpc	mean UR	mean Youth Empl. Rate	mean Share Foreigners
PT11	17	27792	0.0114	0.0091	0.2974	0.0255	3668549	13835	0.1093	0.3145	0.0355
PT15	17	9532	0.0073	0.0022	0.2969	0.0325	435324	17839	0.1001	0.2838	0.1093
PT16	17	14515	0.0063	0.0020	0.2876	0.0270	2307453	14404	0.0742	0.2907	0.0508
PT17	17	15839	0.0280	0.0232	0.2168	0.0223	2790236	23374	0.1094	0.2647	0.1045
PT18	16	11468	0.0062	0.0022	0.2875	0.0302	748765	15672	0.1097	0.2632	0.0315
UKC1	10	2722	0.0238	0.0177	0.3291	0.0131	1171475	23417	0.0881	0.4432	0.0354
UKC2	10	2722	0.0238	0.0177	0.3264	0.0129	1417876	26524	0.0796	0.4899	0.0456
UKD1	7	6146	0.0169	0.0037	0.3181	0.0093	499221	29358	0.0544	0.5478	0.0313
UKD3	10	6790	0.0233	0.0158	0.3211	0.0104	2665176	29375	0.0754	0.4790	0.1027
UKD4	10	6790	0.0233	0.0158	0.3502	0.0130	1458246	26459	0.0577	0.5126	0.0609
UKD6	7	6146	0.0169	0.0037	0.2969	0.0123	908416	39349	0.0471	0.5228	0.0527
UKD7	7	6146	0.0169	0.0037	0.3294	0.0061	1514138	26042	0.0819	0.4405	0.0504
UKE1	10	5609	0.0293	0.0186	0.3367	0.0176	913797	26528	0.0747	0.5051	0.0479
UKE2	10	5609	0.0293	0.0186	0.2966	0.0109	793621	29974	0.0435	0.5410	0.0486
UKE3	10	5609	0.0293	0.0186	0.3370	0.0193	1335743	23543	0.0839	0.4977	0.0628
UKE4	10	5609	0.0293	0.0186	0.3257	0.0118	2212462	28706	0.0739	0.4671	0.0962
UKF1	10	4558	0.0334	0.0155	0.3232	0.0130	2099994	26583	0.0643	0.5044	0.0644
UKF2	10	4558	0.0334	0.0155	0.3145	0.0103	1700083	29629	0.0571	0.5219	0.1169
UKF3	10	4558	0.0334	0.0155	0.3347	0.0174	709523	23701	0.0539	0.5473	0.0641
UKG1	10	5207	0.0247	0.0143	0.2978	0.0147	1290560	30480	0.0453	0.5170	0.0571
UKG2	10	5207	0.0247	0.0143	0.3259	0.0086	1562153	25175	0.0564	0.5453	0.0438
UKG3	10	5207	0.0247	0.0143	0.3282	0.0146	2724822	27410	0.0957	0.4016	0.1416
UKH1	10	5673	0.0327	0.0168	0.3101	0.0116	2372425	30169	0.0512	0.5589	0.0851
UKH2	10	5673	0.0327	0.0168	0.2507	0.0117	1727318	34905	0.0503	0.4997	0.1261
UKH3	10	5673	0.0327	0.0168	0.2757	0.0103	1723156	27342	0.0562	0.5362	0.0700
UKI3	4	5713	0.0287	0.0040	0.1304	0.0140	1130366	201589	0.0580	0.4026	0.4064
UKI4	4	5713	0.0287	0.0040	0.2438	0.0138	2271574	57429	0.0803	0.4015	0.3766
UKI5	4	5713	0.0287	0.0040	0.2680	0.0071	1840011	26072	0.0752	0.4040	0.2887
UKI6	4	5713	0.0287	0.0040	0.2293	0.0104	1269209	32295	0.0552	0.4610	0.2648
UKI7	4	5713	0.0287	0.0040	0.2247	0.0144	2029546	44520	0.0628	0.4034	0.3864
UKJ1	10	8063	0.0359	0.0176	0.2498	0.0081	2258467	46575	0.0441	0.5374	0.1360
UKJ2	10	8063	0.0359	0.0176	0.2410	0.0112	2728488	35472	0.0448	0.5454	0.0988
UKJ3	10	8063	0.0359	0.0176	0.2854	0.0081	1889946	34259	0.0492	0.5515	0.0781
UKJ4	10	8063	0.0359	0.0176	0.2741	0.0137	1717896	27791	0.0627	0.5224	0.0763
UKK1	10	4945	0.0356	0.0209	0.2920	0.0091	2340991	34834	0.0468	0.5570	0.0790
UKK2	10	4945	0.0356	0.0209	0.3100	0.0123	1268802	26923	0.0457	0.5878	0.0658
UKK3	10	4945	0.0356	0.0209	0.3410	0.0182	534630	22036	0.0512	0.5270	0.0393
UKK4	10	4945	0.0356	0.0209	0.3344	0.0084	1134160	25510	0.0515	0.5258	0.0504
UKL1	10	2905	0.0250	0.0173	0.3409	0.0118	1924125	21255	0.0701	0.4760	0.0344
UKL2	10	2905	0.0250	0.0173	0.3221	0.0122	1119931	29216	0.0574	0.4747	0.0628
UKM5	9	5044	0.0195	0.0175	0.2615	0.0122	475084	48511	0.0400	0.6651	0.0834
UKM6	9	5044	0.0195	0.0175	0.3448	0.0339	463179	29200	0.0467	0.5622	0.0492
UKN0	10	2456	0.0223	0.0181	0.3249	0.0345	1798184	26149	0.0609	0.4205	0.0533

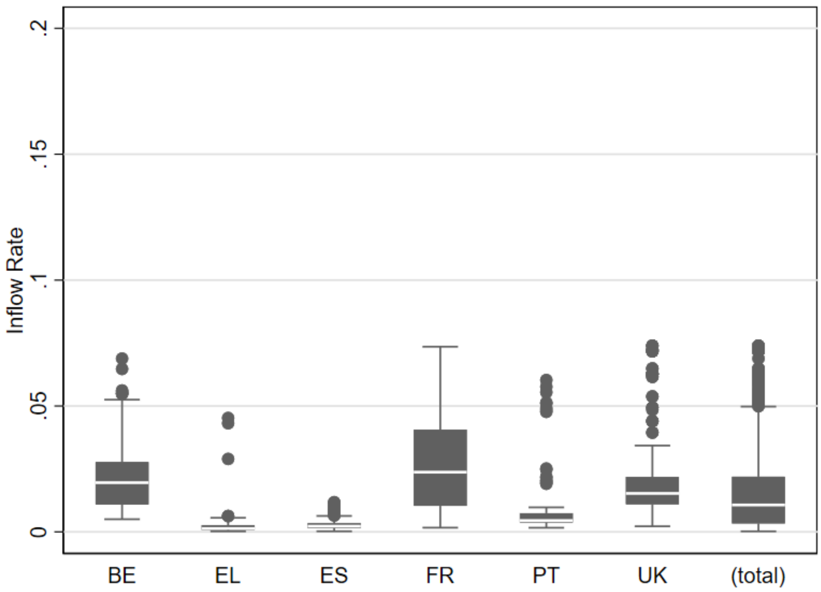
Note: Table provides regional-level summary statistics of the final sample used in analysis. N denotes the annual number of individuals surveyed in a region, GDPpc the regional GDP per capita, and UR the regional unemployment rate.

Figure A.5: Boxplot of inflow rates – *raw* sample



Note: The graph provides Tukey boxplots on the derived regional inflow rates by country (raw sample, non-trimmed). The data's skewness is 2.5719, the kurtosis is 14.4322.

Figure A.6: Boxplot of inflow rates – final (*trimmed*) sample



Note: The graph provides Tukey boxplots on the derived regional inflow rates by country for the trimmed data sample (the raw sample without the highest and lowest 1 percent of values). The skewness for the trimmed sample is 1.4409, the kurtosis is 4.5945.

Table A.3: Country-level summary statistics

Country	indicator	# of obs.	mean	std.dev.	min	max
Belgium	Labor inflow rate	167	0.0217	0.0134	0.0050	0.0688
	Kaitz index	167	0.2544	0.0250	0.2025	0.2945
	Population	167	1,007,138	439,208	252,295	1,849,523
	GDP per capita	167	32,401	12,131	17,445	69,873
	Unemployment rate	167	0.0797	0.0406	0.0260	0.1920
	Youth employment rate	167	0.2519	0.0581	0.1425	0.3994
	Share of foreigners	167	0.1149	0.0829	0.0208	0.4087
Greece	Labor inflow rate	114	0.0027	0.0063	0.0002	0.0452
	Kaitz index	114	0.3157	0.0383	0.2217	0.4543
	Population	114	949,341	894,701	273,843	3,999,457
	GDP per capita	114	15,165	3,163	11,189	29,247
	Unemployment rate	114	0.1602	0.0742	0.0540	0.3160
	Empl. Rate of Youth	114	0.1896	0.0643	0.0623	0.3454
	Share of foreigners	114	0.0408	0.0190	0.0131	0.0961
Spain	Labor inflow rate	245	0.0027	0.0019	0.0002	0.0118
	Kaitz index	245	0.2104	0.0211	0.1585	0.2646
	Population	245	2,836,513	2,399,517	277,989	8,410,095
	GDP per capita	245	21,886	4,673	11,594	35,241
	Unemployment rate	245	0.1558	0.0728	0.0510	0.3620
	Empl. Rate of Youth	245	0.2697	0.0906	0.1274	0.4679
	Share of foreigners	245	0.0868	0.0504	0.0055	0.2130
France	Labor inflow rate	271	0.0261	0.0173	0.0017	0.0736
	Kaitz index	271	0.3216	0.0235	0.2222	0.3563
	Population	271	2,986,787	2,417,840	309,693	12,213,447
	GDP per capita	271	28,137	6,130	22,662	59,749
	Unemployment rate	271	0.0895	0.0193	0.0500	0.1500
	Empl. Rate of Youth	271	0.3079	0.0401	0.1997	0.5103
	Share of foreigners	271	0.0772	0.0413	0.0216	0.1986
Portugal	Labor inflow rate	84	0.0107	0.0147	0.0016	0.0603
	Kaitz index	84	0.2733	0.0403	0.1885	0.3454
	Population	84	2,006,317	1,234,606	400,937	3,719,898
	GDP per capita	84	16,618	3,833	11,001	25,974
	Unemployment rate	84	0.0995	0.0377	0.0290	0.1860
	Empl. Rate of Youth	84	0.2902	0.0621	0.1867	0.4618
	Share of foreigners	84	0.0607	0.0347	0.0156	0.1260
UK	Labor inflow rate	339	0.0199	0.0151	0.0022	0.0739
	Kaitz index	339	0.3064	0.0393	0.1145	0.4216
	Population	339	1,549,525	639,713	444,381	2,827,820
	GDP per capita	339	30,910	19,351	16,876	222,201
	Unemployment rate	339	0.0624	0.0208	0.0260	0.1300
	Empl. Rate of Youth	339	0.5090	0.0705	0.3430	0.6920
	Share of foreigners	339	0.0782	0.0702	0.0153	0.3970
Total	Labor inflow rate	1220	0.0158	0.0161	0.0002	0.0739
	Kaitz index	1220	0.2820	0.0523	0.1145	0.4543
	Population	1220	2,028,362	1,848,263	252,295	12,213,447
	GDP per capita	1220	26,231	13,123	11,001	222,201
	Unemployment rate	1220	0.1012	0.0594	0.0260	0.3620
	Empl. Rate of Youth	1220	0.3361	0.1303	0.0623	0.6920
	Share of foreigners	1220	0.0800	0.0604	0.0055	0.4087

Note: Country-level summary statistics based on final data sample.

Table A.4: Variable correlations

	inflow rate	Kaitz index	population	GDP per capita	unemployment rate	youth empl. rate
Kaitz index	0.257***	1				
population	-0.0316	-0.207***	1			
GDP per capita	0.192***	-0.237***	0.119***	1		
unemployment rate	-0.376***	-0.220***	0.163***	-0.259***	1	
youth empl. rate	0.258***	0.249***	-0.0966***	0.181***	-0.675***	1
share of foreigners	0.0203	-0.368***	0.212***	0.598***	0.154***	-0.175***

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Correlation statistics based on final sample used.

Table A.5: Main results – Fixed effects model (showing covariates estimates)

VARIABLES	(1) FE without covariates	(2) FE with covariates	(3) (2) with Kaitz (lag 2y.)	(4) (2) with Kaitz (lag 3y.)
ln Kaitz (lag 1y.)	0.015**	0.029***		
	(0.006)	(0.006)		
ln Kaitz (lag 2y.)			0.030***	
			(0.006)	
ln Kaitz (lag 3y.)				0.036**
				(0.014)
ln population (lag 1y.)		0.003	0.001	-0.010
		(0.018)	(0.017)	(0.019)
ln GDP per capita (lag 1y.)		0.041***	0.037***	0.043***
		(0.007)	(0.008)	(0.009)
ln unemployment rate (lag 1y.)		0.004	0.010***	0.008**
		(0.003)	(0.003)	(0.003)
ln youth employment rate (lag 1y.)		-0.012***	-0.006*	-0.013***
		(0.003)	(0.003)	(0.004)
ln share of foreigners (lag 3y.)		-0.002	-0.001	-0.006**
		(0.002)	(0.002)	(0.003)
Constant	-1.155***	-0.695	1.564***	-0.798
	(0.261)	(0.482)	(0.496)	(0.531)
Model specifications				
Covariates	NO	YES	YES	YES
Region FE	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES
Country-time trend	YES	YES	YES	YES
Observations	1220	1220	1125	1093
Within R2	0.489	0.517	0.500	0.547
Between R2	0.271	0.188	0.120	0.079

Note: Dependent variable is the regional inflow rate of low-skilled individuals. The Kaitz index and all covariates in logarithm. All covariates lagged by one year, the share of foreigners by three years. All models include year fixed effects and country-time trends. Standard errors clustered at the regional level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Robustness – Reverse causality test

VARIABLES	(1) FE (baseline)	(2) Reverse causality (no lag)	(3) Reverse causality (lag 1y.)
ln Kaitz (lag 1y.)	0.029***		
	(0.006)		
labor inflow rate		0.047	
		(0.092)	
labor inflow rate (lag 1y.)			0.047
			(0.090)
Constant	-0.695	2.001	1.989
	(0.482)	(1.703)	(1.709)
Model specifications			
Covariates	YES	YES	YES
Region FE	YES	YES	YES
Year dummies	YES	YES	YES
Country-time trend	YES	YES	YES
Observations	1220	1182	1182
Within R2	0.517	0.662	0.662
Between R2	0.188	0.144	0.144

Note: The dependent variable in column (1) is the regional inflow rate of low-skilled individuals. The dependent variable in columns (2) and (3) is the regional Kaitz index expressed in logarithmic terms. All other model specifications as in table 2.3. Standard errors clustered at the regional level are depicted in parentheses:
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Robustness – Leave-1-Out country-by-country

VARIABLES	(1) FE (baseline)	(2) (1) w/o Belgium	(3) (1) w/o Greece	(4) (1) w/o Spain	(5) (1) w/o France	(6) (1) w/o Portugal	(7) (1) w/o UK
ln Kaitz (lag 1y.)	0.029***	0.022***	0.039***	0.031***	0.025***	0.029***	0.023***
	(0.006)	(0.006)	(0.009)	(0.007)	(0.007)	(0.007)	(0.007)
Constant	-0.695	0.436	-0.377	-0.289	-2.181***	-1.077***	-1.433
	(0.482)	(0.545)	(0.492)	(0.910)	(0.564)	(0.310)	(1.472)
Model specifications							
Covariates	YES	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES	YES	YES	YES
Country-time trend	YES	YES	YES	YES	YES	YES	YES
Observations	1,220	1,053	1,106	975	949	1,136	881
Within R2	0.517	0.554	0.550	0.566	0.496	0.547	0.500
Between R2	0.188	0.563	0.094	0.180	0.000	0.228	0.152

Note: The dependent variable is the regional inflow rate of low-skilled individuals. Column (1) provides results from my baseline model, the other specification always take the full sample but without regions from the country indicated. All other model specifications as in table 2.3. Standard errors clustered at the regional level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Robustness – Other approaches

VARIABLES	(1) Main model	(2) Jacknife approach	(3) Bootstrap approach	(4) Region-trends (linear)	(5) Non-linear country-trends	(6) COE fixed (initial obs.)	(7) COE fixed (mean value)
ln Kaitz (lag 1y.)	0.029***	0.029***	0.029***	0.034***	0.028**		
	(0.006)	(0.007)	(0.007)	(0.008)	(0.012)		
ln Kaitz (lag 1y.)						0.042***	
(using initial local COE value)						(0.010)	
ln Kaitz (lag 1y.)							0.042***
(using mean local COE value)							(0.010)
Constant	-0.695	-0.695	-0.695	-1.206*	469.224	0.151	0.161
	(0.482)	(0.708)	(0.659)	(0.712)	(2300.249)	(0.444)	(0.444)
Model specifications							
Covariates	YES	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES	YES	YES	YES
Country-time trend	YES	YES	YES	YES	YES	YES	YES
Observations	1220	1220	1220	1220	1220	1220	1220
Within R2	0.517	0.517	0.517	0.584	0.526	0.522	0.522
Between R2	0.188	0.188	0.188	0.160	0.058	0.170	0.170

Note: The dependent variable is the regional inflow rate of low-skilled individuals. The models in columns 2-7 are estimated using jacknife resampling technique, bootstrapping (with 1000 replications), region time-trends (rather than the country time-trends utilized in the baseline model), non-linear country time-trends, amended Kaitz index (initial regional observation used for local compensation measure), and amended Kaitz index (mean regional observation value used for local compensation measure), respectively (COE denotes compensation of employees). All other model specifications as in table 2.3. Standard errors clustered at the regional level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Spatial heterogeneity

VARIABLES	(1) FE with covariates	(2) Country-specific (reference: Greece)	(3) Country-specific (reference: UK)	(4) Country-group (reference: south Europ. countries)	(5) Urban regions (reference: rural areas)
ln Kaitz (lag 1y.)	0.029***	0.022***	0.050***	0.016*	0.033***
	(0.006)	(0.008)	(0.018)	(0.009)	(0.009)
BE # ln Kaitz (lag 1y.)		0.034	0.006		
		(0.056)	(0.058)		
EL # ln Kaitz (lag 1y.)		*(reference)*	-0.028		
			(0.019)		
ES # ln Kaitz (lag 1y.)		-0.016	-0.045		
		(0.025)	(0.028)		
FR # ln Kaitz (lag 1y.)		0.102*	0.074		
		(0.053)	0.054		
PT # ln Kaitz (lag 1y.)		-0.015	-0.043		
		(0.029)	(0.035)		
UK # ln Kaitz (lag 1y.)		0.028	*(reference)*		
		(0.019)			
Central European				0.043**	
# ln Kaitz (lag 1y.)				(0.020)	
Urban regions					0.006
# ln Kaitz (lag 1y.)					(0.009)
Constant	-0.695	-2.183**	-2.183**	-2.444**	-1.195
	(0.482)	(1.033)	(1.033)	(1.035)	(0.720)
Model specifications					
Covariates	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES	YES
Country-time trend	YES	YES	YES	YES	YES
Observations	1220	1220	1220	1220	1220
Within R2	0.517	0.587	0.587	0.585	0.584
Between R2	0.188	0.192	0.192	0.182	0.157

Note: The dependent variable is the regional inflow rate of low-skilled individuals. All model specifications as in table 2.3, except for the additionally added interaction terms when depicted in columns (2)-(5). Standard errors clustered at the regional level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Urban vs. rural areas, nationality, within-country mobility

VARIABLES	(1) FE (baseline)	(2) (1) only for urban areas	(3) (1) only for rural areas	(4) (1) considering natives only	(5) (1) restricted to domestic mobility
ln Kaitz (lag 1y.)	0.029***	0.034***	0.018**	0.025***	0.020***
	(0.006)	(0.013)	(0.007)	(0.006)	(0.006)
Constant	-0.695	-0.791	-0.662	-0.076*	-1.448***
	(0.482)	(0.374)	(0.737)	(0.433)	(0.501)
Model specifications					
Covariates	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES	YES
Country-time trend	YES	YES	YES	YES	YES
Observations	1220	617	603	1220	1220
Within R2	0.517	0.677	0.483	0.530	0.363
Between R2	0.188	0.010	0.432	0.293	0.002

Note: The dependent variable is the regional inflow rate of low-skilled individuals, but with the subgroup restrictions indicated at the top. All model specifications as in table 2.3. Standard errors clustered at the regional level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Heterogeneity across gender and age

VARIABLES	(1) FE (baseline)	(2) (1) for females only	(3) (1) for males only	(4) (1) for young (<28y.) only
ln Kaitz (lag 1y.)	0.029***	0.032***	0.033***	0.047***
	(0.006)	(0.009)	(0.008)	(0.016)
Constant	-0.695	-0.405	-0.822	-2.336***
	(0.482)	(0.528)	(0.553)	(0.654)
Model specifications				
Covariates	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES
Country-time trend	YES	YES	YES	YES
Observations	1220	1136	1161	1095
Within R2	0.517	0.465	0.486	0.498
Between R2	0.188	0.173	0.223	0.347

Note: The dependent variable is the regional inflow rate of low-skilled individuals, but with the subgroup restrictions indicated at the top. All other model specifications as in table 2.3. Standard errors clustered at the regional level are depicted in parentheses:
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Chapter 3

Linguistic Proximity and the Inequality in Returns to Migrants' Skills*

We provide novel evidence on the inequality of returns to immigrant skills in hosting economies. While migrant wage gaps are well established in the literature, less is known about the origins of their heterogeneity. We propose one potential rationale for this gap, related to linguistic proximity between the destination and the origin countries. We exploit individual-level data from nine different hosting economies across diverse countries of origin, for both recent and long-term migrants. We find that greater linguistic distance between origin and destination is associated with a higher average wage penalty for highly skilled immigrants, and a substantially lower position in the wage distribution for immigrants without tertiary education.

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3.1 Introduction

We study the inequality in returns to tertiary education for immigrants from various origin countries in nine destination countries. A rich array of studies documents immigrant wage gaps that remain even after adjusting for socioeconomic characteristics. In line with the human capital framework, numerous studies have emphasized the role of skill portability from the origin to the host economy for immigrant wage assimilation (e.g. Friedberg, 2000, Bazzi et al., 2016, both these studies also provide a thorough overview of the empirical literature in the field).[60] Compared to this rich literature, considerably less attention has been devoted to the *inequality* of the immigrant-native wage gap across destinations and origins. We propose to move the focus from migrants to employers, recognizing that their ability to assess the quality of migrants' education is lower if the migrant candidates come from linguistically more distant countries.

Our study stems from two theoretical grounds. First, we leverage information asymmetry in employment contracts to hypothesize that employers find it more costly to inspect the skills of job candidates in the case of linguistically distant countries of origin. As typically assumed in the labor literature, prior to employment, employers cannot assert job candidates' qualifications due to information asymmetry, and they typically rely on references, diplomas, and certificates to reduce that asymmetry. However, certification and references from a linguistically distant country of origin may impose too high costs on the employer: Pure understanding of the credentials is challenging when they are written in a distant language. Naturally, different occupations demand different skill levels, which is why we expect linguistic proximity to be more important, the more important certifications are, i.e. the higher the demanded skill level (e.g. Peri and Sparber, 2009). Second, stereotyping and mental accounting lead to a form of a "halo effect", where immigrants from a given country of origin are lumped together to represent one stereotype – distinct from natives as well as from immigrants from another origin (e.g. Guiso et al., 2009, Rydgren and Ruth, 2013). Immigrants from linguistically distant origins are more distinctly identifiable and thus face a higher risk of stereotyping, which homogenizes wages within that group regardless of individual skills, thus driving the wage penalty to human capital for the highly educated individuals in the group relative to natives.[61]

Both theoretical mechanisms give rise to a testable empirical hypothesis: *linguistic distance inflates immigrant wage gaps experienced by high-skilled immigrants*. We address this hypothesis by providing a comprehensive set of measures of returns to human capital (tertiary education) across multiple countries of origin and destination. Note that

[60] Speaking the language of the host economy is a prerequisite for being able to communicate one's knowledge and thus transfer the existing human capital, which is why language skills have been at the core of empirical research on the immigrant wage gap. Empirical research on immigrant wage gap relies on assimilation hypothesis (Chiswick, 1986), skill portability hypothesis (e.g. Rivera-Batiz, 1990, Chiswick, 1991, Dustmann, 1994, Chiswick and Miller, 1995, Berman et al., 2003, Bleakley and Chin, 2004, 2010), as well as labor market segmentation/discrimination (Piore, 1972, Reich et al., 1973, Doeringer and Piore, 1985). The existing literature typically finds that across various hosting economies, competence in the hosting economy language mitigates the wage penalty, but it does not eradicate it (e.g. McManus et al., 1983, McManus, 1985, Kossoudji, 1988, Rivera-Batiz, 1990, Chiswick, 1991, Dustmann, 1994, Chiswick and Miller, 1995, Dustmann and Fabbri, 2003, Ferrer et al., 2006, Goldmann et al., 2015). Also, learning the language of the host economy appears to reduce the wage penalties (Berman et al., 2003).

[61] Guiso et al. (2009) finds that differences in linguistic proximity, along with other factors, explain remarkably well the average bilateral trust between individuals from different countries.

there are important reasons why the testable empirical hypothesis does not have to hold in the data. First, it has been demonstrated empirically that cultural and linguistic proximity drive both migration flows (e.g. Chiswick and Miller, 1994, Caragliu et al., 2013, Adsera and Pytlikova, 2015) and returns to migration (e.g. Docquier et al., 2007). This implies that across hosting economies, immigrants from linguistically distant economies may be missing in observational data, thus making it difficult to capture and quantify the effects of linguistic proximity on returns to skills among migrants. Second, both of the presented theoretical mechanisms may be too weak to show up in the data. Finally, the two theory-based mechanisms may be dominated by mechanisms stemming from other theories of migration, which are unrelated to linguistic proximity, such as network effects.

To verify the hypothesis of this study, we harmonize an extensive collection of individual-level data from nine important immigrant destinations in order to estimate origin and destination-specific returns to tertiary education and thus quantify returns to (foreign) human capital. The destination countries in our study are located on four continents and cover six different language families. Together, they host about 36 percent of the total international migrant population worldwide.[62] All microdatasets include both native and immigrant individuals. From this collected microdata we estimate how much the average wage for tertiary-educated immigrants differs from the average wage of tertiary-educated natives in each destination country. These estimates are obtained across different origin countries and for each destination country separately, and they are adjusted for individual characteristics such as age, gender, marital status, and time since arrival in the destination country – as well as for job-related characteristics, such as occupation and industry. Accordingly, we exploit heterogeneity not only for the range of countries of origin, but also across destinations. We then relate these deviations to bilateral measures of linguistic proximity for origin and destination country pairs.

In the existing empirical literature, papers typically study a single destination country. Less attention is devoted to the linguistic abilities of employers to inspect the qualifications of immigrant job candidates. Indeed, most existing studies on immigrant-native wage assimilation provide evidence from destinations where the native language is one of the main global languages. This might be problematic given that highly skilled immigrants often speak some of the most important global languages as second languages (such as English, Spanish, and French; as documented by e.g. LaLonde and Topel, 1992, Dustmann, 1994, Dustmann and Fabbri, 2003).[63] We study the role of linguistic proximity between destination and origin countries in the dispersion of wage gaps among highly skilled migrants. First, we document remarkable inequality in the returns to tertiary education among migrants across countries of origin. Subsequently, we study the role of general linguistic proximity between origin and destination countries – rather than individual language skills – in explaining these systematic differences.

We find that, *ceteris paribus*, a higher linguistic proximity between origin and destination countries is associated with a higher position of immigrants in the destination country wage distribution. We also find that the (adjusted) gap in wages for highly skilled immigrants is higher for linguistically distant origin countries, *ceteris paribus*. Our results are thus an addition to the existing literature: while in the past *individual* language

[62]To obtain this number, we rely on UN migrant stock data: `https://www.un.org/development/desa/pd/content/international-migrant-stock` (last accessed: 27.07.2024).

[63]Incidentally, the majority of popular destinations for skilled migrants have as a native language one of these few main global second languages.

skills were believed to facilitate skills transmission in the context of migration, we show that generally, employers experience difficulties in acquiring information about the skills of immigrants from linguistically distant origins, even if the individual language skills of these immigrants are not an obstacle. We therefore point to linguistic distance as a *country-level* factor that explains a fair share of the inequality in immigrants' skill returns across hosting economies. In addition, we contribute to the literature by systematically evaluating returns to human capital across multiple origin and destination countries.

Our paper is structured as follows: Section 3.2 summarizes the literature on immigrant-native wage gaps in the context of language proximity. In section 3.3 we discuss our methodology and in section 3.4 we cover data and stylized facts emerging from this study. Section 3.5 presents the results. Finally, the last section offers a summary of the results together with policy implications, and reviews avenues for further research.

3.2 Related literature

There is a consensus in the existing literature that even after adjusting for individual characteristics, wages remain unequal between natives and immigrants (Chiswick, 1978, Borjas, 1985, Chiswick, 1986, Jasso and Rosenzweig, 1988, Borjas, 1992, 1994). The origins of this gap have been traced to the shortcomings of immigrants as job seekers (i.e., limited knowledge of the local labor market) and the transferability of skills: assimilation is costly and takes a long time.[64] Chiswick and Miller (2013) argue that what matters is not the knowledge of the host economy language per se, but the match between an immigrant's own language proficiency and the language requirements of the job. Imai et al. (2019) find for Canada that immigrants with limited language proficiency self-select into occupations that require more manual skills, even when they worked in cognitive skill-demanding occupations in their origin countries before. Studying the case of a rare language, which is seldom acquired by immigrants prior to the arrival in a hosting economy – Dutch among immigrants to the Netherlands – Yao and van Ours (2015) show that wage gaps prevail particularly among disfavored groups.

In addition, Bleakley and Chin (2004, 2010) demonstrate that the easier it is for an immigrant to learn and understand the language in the destination country, the more successful he or she is in the labor market. In fact, Beenstock et al. (2001) document that knowledge of languages that are similar to the ones spoken in the destination country correlates with an increased learning ability for those languages, and therefore with economic and social success. Extant literature argues that it is not simply knowledge of the host economy language, but also linguistic proximity that plays a role in the successful labor market performance of immigrant workers (e.g. Crystal, 1987, Espenshade and Fu, 1997, Chiswick and Miller, 1998, 2001, Isphording and Otten, 2013, Isphording, 2014).

Measuring language proximity is a challenge. Historically, the literature has started from studies of language proximity to English. For example, Chiswick and Miller (2005) proposed to categorize immigrants by how easy it is for Americans to learn their foreign language. They group immigrants into two main categories: those from English-speaking countries of origin, and non-native English speakers. The latter category is further divided

[64] The early studies on the United States as a hosting economy include Borjas (1987), LaLonde and Topel (1992), Borjas and Friedberg (2009), Borjas (2015). The wage gaps were documented for many hosting economies: Kee (1995) for the Netherlands, Friedberg (2000) for Israel, Schaafsma and Sweetman (2001), Fortin et al. (2016) for Canada among others.

into people from countries whose dominant spoken language is close to English, far from English, or in-between.

This admittedly crude measurement has been refined with the use of advances in linguistic sciences. In ethnology, language proximity is measured at six levels, where full proximity denotes identical languages in both origin and destination country, and zero denotes the most "different languages" in relative terms (e.g. Adsera and Pytlikova, 2015, Adserà and Ferrer, 2021).[65] Dyen's lexicostatistical proximity measure is based on similarities across 200 commonly used words in each language (Dyen et al., 1992).[66] Finally, literature uses the Levenshtein distance measurement applied to the Automatic Similarity Judgment Program (ASJP), an expert judgment dataset, comparing two languages using wording, length of words, and phonetic features of 40 basic words of daily life.

Table B.1 reports an overview of measures used in the economic literature. This summary reveals that many studies exploit heterogeneity across origin countries, but in the context of a single destination country. For instance, studies analyze in isolation immigrants to the US, Canada, or Israel – thus confounding the role of language proximity with the role of characteristics of the labor market, and cultural factors in a given destination country with factors for a given origin-destination country pair. To obtain meaningful information about the role of linguistic proximity in isolation from those other factors, it is imperative to exploit the heterogeneity of destination countries in addition to the heterogeneity of origin countries.

Note that difficulty in learning the language of the hosting economy may have substantially lower relevance for highly skilled migrants, i.e. immigrants with a tertiary degree earned in the origin country, for a number of reasons: First, good command of the languages of most major host economies prior to immigration is more prevalent among highly skilled individuals.[67] Second, many highly skilled migrants have had exposure to learning more than one foreign language during their education, so even if they do not speak the language of their destination country, acquiring it is much easier due to the knowledge of other languages (Beenstock et al., 2001). Finally, highly educated migrants self-select into destination countries with either a common spoken language (Clark et al., 2007, Pedersen et al., 2008, Beine et al., 2011, Grogger and Hanson, 2011) or a similar language (Belot and Ederveen, 2012, Belot and Hatton, 2012, Adsera and Pytlikova, 2015) to a much higher degree than low-educated migrants.

Despite these distinctive features of high-skilled migration and prevailing immigrant wage gaps among highly skilled migrants, little is known about the systematic drivers of those wage gaps. Indeed, in relation to highly skilled migrants, cultural distance or insufficient command of the host economy language are not likely to be exclusive or dominant explanations for the existence of migrant wage gaps. One potentially relevant and previously unstudied channel is related to the linguistic abilities of the employer rather than the employee: for highly skilled migrants, communicating in the language of the destination country does not need to be a challenge, but for an employer who

[65]The Ethnologue database catalogs 7,111 living languages around the globe. It provides characteristic features that describe each language, including classifications on the language family it belongs to (linguistic lineage). This information is used to construct measures of similarity across languages (Pedersen et al., 2008, Belot and Hatton, 2012, Adsera and Pytlikova, 2015).

[66]This measure is restricted to Indo-European languages, naturally censoring its usage (Belot and Ederveen, 2012).

[67]E.g. in many countries entry exams to the university or exit exams out of high school explicitly involve knowledge of one of the major global second languages.

is potentially interested in hiring an highly skilled immigrant worker, it may be costly to acquire information about the curriculum or the quality of education in the case of linguistically distant countries of origin.

This mechanism is particularly relevant for occupations exempt from general certification in most destination countries. Note that in the case of those occupations, skills portability might not be an issue.[68] In many occupations there is no standardized international certification, even though skills themselves are highly portable (e.g. analysts, engineers in many specializations, the so-called creative occupations, etc.). If an employer needs to verify the content of university curricula to check if a potential candidate has the desired skills, linguistic proximity may become an issue if the university curriculum or portfolio is available in a linguistically distant tongue. In addition, it is difficult to evaluate the quality of received education in those cases, as it may be challenging to acquire information about within-country rankings, regional recognition of a tertiary institution, etc. In those cases, the cost of acquiring information about the candidate's skills may be simply excessive, thus imposing a wage penalty for highly skilled migrants coming from linguistically distant countries.

To sum up, it has been shown in the empirical literature that linguistic proximity shapes migration flows. We build on this literature and hypothesize that linguistic proximity between the origin and destination economies amplifies the cost of screening candidates due to the objective costs of acquiring information in a distant language and due to subjective costs exemplified by a "halo" effect and stereotyping. Based on this hypothesis we assess empirically whether linguistic proximity explains the inequality in the wages of highly skilled migrants. To isolate the role of linguistic proximity, we utilize multiple languages in destination countries and destination-by-origin fixed effects specifications. To separate the effect of linguistic proximity from simple language proficiency, we focus on highly skilled migrants, who typically speak one or more foreign languages.

We contribute to the literature by verifying whether immigrants from linguistically distant countries observe greater deviation of returns to their skills, relative to immigrants from linguistically closer countries. In contrast to the achievements of the prior literature, our aim is to hypothesize about the drivers and empirically study the systematic inequality of high-skill migrants' wages, *holding constant* the conditions of a given country of origin and a given destination economy. We exploit a vast collection of individual-level data from nine popular destination countries: Argentina, Brazil, Canada, France, Germany, Israel, Mexico, the United Kingdom, and the United States. These destination countries represent a diverse range of languages and experience migration from many countries of origin. Working with nine destination countries and diverse origin countries allows us to exploit the role of heterogeneity in language proximity in the inequality of returns to tertiary skills among migrants.

To study the role of language in explaining immigrant wage gaps, we utilize a measure of linguistic proximity based on *ease of communication*. This measure has previously been used in trade literature (Melitz and Toubal, 2014), but to the best of our knowledge, it has been absent in the migration literature. This measure is suitable for capturing the proposed mechanism: the costs to an employer of assessing the quality of a job

[68]For example, medical doctors are subject to strict certification, which makes skills portability an issue, but linguistic proximity is less relevant given the high degree of standardization of certification in this occupation. As another example, IT specialists, also subject to strict certification, have no national certificates as internationally recognized certificates dominate the profession. Thus, in this profession, neither skill portability nor linguistic proximity constitutes an obstacle to mobility.

seeker's credentials. Our measure of the *ease of communication* begins with the *Automated Similarity Judgment Program* , which indexes language similarities between 200 common words across country pairs, based on an assessment by ethnologists and ethnostatisticians. In subsequent steps, this measure is adjusted for common spoken and official languages and standardized to deliver a common language index (CLI), which has a bounded distribution within our sample: between 0 for the most linguistically distant country-pair and 1 for the highest linguistic proximity (see section 3.4 in this paper as well as the original paper of Melitz and Toubal, 2014, for a detailed description on how to construct the measure). [69]

The key hypothesis of our paper states that immigrants with tertiary education from linguistically distant countries experience lower wages than immigrants with a tertiary degree from less linguistically distant countries, *ceteris paribus*. Command of the hosting economy language among immigrants has been extensively studied. However, university graduates typically speak one of the major global languages. Meanwhile, the ability of the employers to verify the credentials of the immigrant job applicants may largely depend on linguistic proximity. We use a measure of language proximity, which reflects the "ease of communication" between the origin and destination countries (Melitz and Toubal, 2014). We expect that linguistic proximity is more relevant in origin countries with less recognized educational systems or that are known for low-quality educational systems. We also hypothesize that language proximity is more relevant when no other credentials are available to the migrant, i.e., shortly after arrival.

3.3 Methods

To investigate determinants of immigrant wage gaps, we pursue two empirical strategies. First, we work directly with the individual-level data as explained in the section 3.3.1. We refer to this approach as the *one-step approach*. We exploit the richness of several million individual-level observations, but this choice poses some econometric challenges. Therefore, we present a second empirical strategy, using what we call the *two-step approach*: we obtain measures of the immigrant wage gaps across countries of origin and destination from individual-level data (the first step), which we then relate to linguistic proximity for origin-destination country-pairs (the second step). The first step, the process of obtaining the immigrant wage gaps, is described in section 3.3.2, whereas the second step, the method for linking wage gaps and linguistic proximity, is described in section 3.3.3.

3.3.1 Individual-level analysis

This section outlines our one-step approach. The benefit of this procedure is that it takes direct advantage of the richness of our data at the individual-level. We convert the individual measure of wages into a percentile measure (denoted by $\hat{w}$) within each destination country to maintain comparability of the estimates across the samples. We

[69]The online appendix to their study also distributes the data on the Automated Similarity Judgment Program, as well as on common spoken and official languages around the world.

then estimate the contribution of language proximity measured by common language index (CLI) from:

$$\hat{w}_i = \alpha + \beta \mathbf{X}_i + \gamma HE_i \times \mathbf{M}_i \times CLI_{d,o} + \delta_Z \mathbf{Z}_o + \delta_{d,o}\mathbf{Z}_{d,o} + \delta_d + \delta_o + \epsilon_i, \quad (3.1)$$
$$\hat{w}_i = \alpha + \beta \mathbf{X}_i + \gamma HE_i \times \mathbf{M}_i \times CLI_{d,o} \times \text{schooling}_o + \delta_Z \mathbf{Z}_o + \delta_{d,o}\mathbf{Z}_{d,o} + \delta_d + \delta_o + \epsilon_i \quad (3.2)$$

where the superscript d denotes destination country, o superscript denotes origin country, index i denotes individual. HE_i identifies the education status of individual i: it is a dummy variable taking on a value of 1 if an individual is highly educated (has a tertiary degree) and 0 otherwise. $\mathbf{M}_i$ identifies the migrant status of individual i: it is a dummy variable taking on the value of 1 if an individual is an immigrant in a given destination country, and 0 if an individual is a native.[70] $\mathbf{X}_i$ is a vector of individual demographic control variables (personal, job, and household characteristics, including information on years since immigration). Country-level controls included in $\mathbf{Z}_o$ consist of population size, GDP per capita, fertility, and mortality rates. Country-pair controls included in $\mathbf{Z}_{d,o}$ consist of geographical variables (distance, contiguity) and historical variables (years at war, common colonizer, common religion, common legal system). The key variable of interest is language proximity measured by our common language index, $CLI_{d,o}$. We observe the own effect of this variable as well as its interaction with the average educational attainment in the country of origin, $schooling_o$. Language proximity is strictly 1 in all cases where $\mathbf{M}_i = 0$ (or, otherwise phrased, when $d = o$).

The one-step procedure portrayed by equation (3.1) estimates a well-known Mincerian wage regression, jointly with the role of linguistic proximity in determining wage inequality. The specification in equation (3.1) allows the returns to tertiary education to vary by country of origin, but they are constrained to be common across destination countries. Hence, the term $CLI_{d,o}$ is the only variable specific to a country of origin *jointly* with a country of destination, exploiting the inequality in returns to tertiary skills across the pairs of origin and destination countries. Our main research hypothesis implies a positive estimate of γ: relative to natives with tertiary education, returns to tertiary education among immigrants should increase in language proximity.

The strategy proposed in equation (3.1) comes with a significant advantage: the entire variation in our sample is exploited in one step. But it also has two limitations. First, unless a multilevel regression is employed, there is no way to obtain correct estimates of the standard errors for characteristics related to several levels of variation: individual, country of destination, country of origin, and origin-destination country pairs. However, our sample contains data for about forty million individuals, prohibiting the use of non-linear estimation methods such as multilevel regressions. Given that the coefficient of interest γ varies across origin-destination country pairs, we pursue clustering the standard errors at country-pair level to obtain correct standard errors for estimations of the γ parameter. Note that this modeling choice limits the interpretation of the significance for the variables related to e.g. individual-level variation.

Second, the destination countries differ in the level of development and structure of the labor market, and the destination countries' data sets differ in sampling design and sample sizes. Consequently, combining the analysis into a one-step procedure introduces several hazards. We introduce destination country fixed effects (δ_d) to account for country-level characteristics, and we introduce sample weights from the original sampling

[70]We consider individuals who obtained education in the destination economy as natives, regardless of their ancestry. See section 3.4 for detailed coverage of the variable definitions and data sources.

design in the estimation. The estimates thus reflect the relative size of populations in the destination countries, not the sample sizes for the data utilized. Note that given the specification of the explained variable – percentile of within destination country wage distribution – the interpretation of the γ estimate refers to immigrant wage gaps in relative terms (position in income distribution) rather than in absolute terms (percent of the wage).

Given the limitations of the one-step approach, we also propose a two-step approach. In the first step, we estimate the returns to tertiary education by country of origin, separately for each destination country. Specifically, we obtain deviations of returns to tertiary education specific to a given country of origin. This step is described in section 3.3.2. In the second step, we relate these estimated deviations to linguistic proximity at the country-pair level. This step is described in section 3.3.3.

3.3.2 Estimating the returns to skills by country of origin

We begin our two-step approach by estimating the country-of-origin-specific returns to skills, separately for each destination country. In other words, for each destination country, we estimate the systematic wage deviation by education level that results from an individual's origin in a given country. The goal is to find out, for instance, whether the returns to skills for Norwegian immigrants in Germany differ from those experienced by Swedes in Germany. We seek to obtain a dataset that lists the returns for skills for all destination countries *and* all origin countries that are sufficiently covered in our micro-level data (i.e., we seek to obtain an m $\times$ n matrix in which migrants' destinations are in the rows and their places of origin are in the columns, and the entries in the matrix represent the estimated wage variances for each of these combinations).

For this purpose, we first estimate destination-specific Mincerian wage regressions using individual-level data. We denote by $log(w_i)$ the log of the hourly wage of individual i:

$$log(w_i) = \alpha + \boldsymbol{\beta X}_i + \eta HE_i + \boldsymbol{\mu M\_COO_i} + \boldsymbol{\rho HE_i} \times \boldsymbol{M\_COO_i} + \epsilon_i, \qquad (3.3)$$

where $\mathbf{X}_i$ is a vector of individual demographic control variables (personal, job, and household characteristics, including information on migrant status and years since immigration). In this notation, HE_i is a dummy variable for the tertiary education status of individual i (taking on the value of 1 if completed, and 0 otherwise), and $\boldsymbol{M\_COO_i}$ is a sequence of dummy variables taking on the value of 1 if an individual i is an immigrant arriving from the given country of origin (COO) and zero otherwise. Accordingly, parameter η measures the average returns to tertiary education for natives in that destination country. Analogously, the vector of parameters μ captures the average effect of a immigrant from a given origin in a given destination for individuals without tertiary education. Finally, the vector of parameters $\boldsymbol{\rho}$ captures the additional effect (positive or negative) of tertiary education for immigrants migrating from a particular COO to a particular destination.[71]

We estimate equation (3.3) with two samples of immigrants: recent migrants with less than five years since immigration (the first sample), and non-recent migrants, i.e. those who have resided in the destination country for at least five years (the second

[71] We explain in detail in section 3.4 that for tertiary-educated individuals if the age at arrival in the destination country is below the customary graduation age, we re-code those individuals to be natives in a sense that their highest achieved education had been obtained in the destination country.

sample). We identify samples using subscript t. This modeling choice has two advantages. Splitting the estimates for recent migrants and non-recent migrants allows us to tackle the potential bias stemming from selectivity in return migration: we cannot capture the size of the bias, but when estimated among individuals with similar duration of stay in the hosting economy, the bias should be homogeneous (Dustmann and Görlach, 2016). Second, estimating the two subsamples allows us to control for general differences of recent immigrants relative to those who had the chance to fully explore the local conditions, as described by Chiswick and Miller (2012).[72]

Using the estimates of $\hat{\mu}$ and $\hat{\rho}$ we create a database of estimates for each country of origin o in each destination country d, and for every available sample t. In analogy to the approach in section 3.3.1 and equations (3.4)-(3.5), we construct a vector $\hat{\gamma}_{o,d,t}$. This vector contains the estimates for individuals without tertiary education from the vector of $\hat{\mu}$ and with tertiary education from the vector of $\hat{\rho}$. This set of estimates becomes a vector $\hat{\boldsymbol{\gamma}}_{\boldsymbol{d,o,t}}$ used as dependent variable in the second stage, in section 3.3.3 below.

To assure that each $\hat{\boldsymbol{\gamma}}_{\boldsymbol{d,o,t}}$ element can be reliably estimated, sufficient degrees of freedom are needed. Given the number of variables in equation (3.3), we impose a sample restriction that in each destination country, at least ten individuals from a given origin country are available for each level of education and duration of stay in the destination country.

3.3.3 Immigrant wage gaps and language proximity

Section 3.3.2 described the first step of our two-step approach, i.e. the methodology for determining average returns to tertiary education by destination and country of origin. The second step in this approach, laid out in the following, tests the relevance of linguistic proximity in explaining differences in these returns. For this purpose, we estimate the following equation:

$$\hat{\gamma}_{d,o,t} = \boldsymbol{\lambda} HE_{d,o} \times CLI_{d,o} + \delta_Z \mathbf{Z}_{d,o} + \delta_d + \delta_o + \delta_t + \epsilon_{d,o,t}, \tag{3.4}$$
$$\hat{\gamma}_{d,o,t} = \boldsymbol{\lambda} HE_{d,o} \times CLI_{d,o} \times schooling_o + \delta_Z \mathbf{Z}_{d,o} + \delta_d + \delta_o + \delta_t + \epsilon_{d,o,t}, \tag{3.5}$$

where the dependent variable $\hat{\gamma}_{d,o,t}$ is the estimated systematic dispersion in returns to tertiary education for immigrants from equation (3.3), for a given destination (d) and origin (o) country and time since migration (t, a dummy variable identifying first-step estimates concerning recent migrants); HE dummy identifies whether a given estimate concerns immigrants with a tertiary degree from a given country of origin in a given destination country; $\mathbf{Z}$ is a vector of controls for both the country of origin (population size and GDP per capita) and country-pair unique factors (geographical distance, contiguity, common legal system, religion, and historic past), and δ_d, δ_o and δ_t capture fixed effects for destination, origin and migrant sample type, respectively.

The key variable of interest is linguistic proximity ($CLI_{d,o}$). We observe the own effect of this variable as well as its interaction with the average educational attainment in the country of origin, $schooling_o$. The $\boldsymbol{\lambda}$ vector of parameters thus captures: (i) the pure effect of language proximity, (ii) the "halo" effect of the country of origin quality of the educational system, and (iii) the interaction between these two variables.

We estimate equation (3.4) with several specifications. First, we account for the sample size used to estimate a given $\hat{\gamma}_{d,o,t}$ estimate. Not only do larger immigrant

[72] Also Lubotsky (2007) offers several arguments for why to estimate immigrant-native wage gaps for recent and non-recent immigrants separately.

groups in a given destination country give more confidence in the robustness of the $\hat{\gamma}_{d,o,t}$ estimate, but the size of the migration flows is also typically proportional to the sample size of a population of migrants from a given country of origin in a given destination country. In our case, the sample sizes differ between destination countries for methodological reasons (for instance, the sampling size of the United States data is much larger than that of Germany), so the sample weights are inappropriate. We thus use migrant stocks for country pairs from the United Nations as weights.

Second, since $\hat{\gamma}_{d,o,t}$ is an estimated parameter, we bootstrap standard errors in estimating equation (3.4). An alternative way to address the issue that $\hat{\gamma}_{d,o,t}$ is an estimate with a measurement error is to re-weight the regressions with e.g. $t - statistic$ or the inverse of standard errors obtained in estimating the equation (3.3). However, such re-weighting for precision excludes re-weighting for migration flows. We adopt both re-weighting and bootstrapping to test the robustness of our results.

3.4 Data

We acquire individual-level data from around the world, applying the following criteria: First, the data source reports wages and individual characteristics relevant to estimating the Mincerian regression. Second, the data source has to refer to a popular migrant destination country. Third, the data sources should be linguistically diversified, so that it is possible to exploit variation in linguistic proximity across country pairs, holding constant the destination country fixed effects and the origin country fixed effects. Data sources for nine migration destination countries meet these criteria: Encuesta Permanente de Hogares (*EPH*) for Argentina, IPUMS-published census data for Brazil, Canada, Israel, and Mexico (IPUMS-I, 2020), Labor Force Surveys for France and the UK, Socio-Economic Panel (SOEP) for Germany and American Community Survey (ACS) data from the United States. These destinations collectively host about 36 percent of the total migrant population worldwide.[73] We report details of data coverage in table B.2.

We harmonize these data sets. For each destination country, we pool together waves of data spanning several periods. We adjust wages for inflation using the consumer price index (CPI) from the World Development Indicators database by the World Bank. We harmonize occupations into five levels: jobs not requiring skills, jobs requiring primary skills, specialists, high-skilled jobs, and managers. We harmonize industry into four levels: agriculture, manufacturing, construction, and services. In the case of both industry and occupation, it would not be possible to obtain more detailed classifications in a harmonized way. We standardize data on education using ISCED categories. We harmonize measures of age to a continuous measure and marital status to four levels: single, married, in an informal relationship, and widowed/divorced.

We restrict our full sample to salaried workers aged 18 to 64. We drop observations with negative incomes, hours information missing, strictly 0 or above 100 hours per week (similar to e.g., Mishel et al., 2012). We use hourly wages, calculated from weekly/monthly wage data and average hours when available. For cases with income or hours reported in intervals, such as Israel, we use the midpoint. For Brazil and the USA, the midpoint was applied only to hours when reported in intervals.

[73]We obtain this estimate using UN migrant stock data: `https://www.un.org/development/desa/pd/content/international-migrant-stock` (last accessed: 27.07.2024).

As controls for country of origin, we include population, mortality, and fertility as well as GDP per capita (adjusted for purchasing power parity), which we take from the World Development Indicators database by the World Bank, and educational attainment from the dataset developed by Barro and Lee (2013). We merge country-level variables with the individual-level data using the year relevant for the individual immigrant. For example, if an individual originates from Nigeria and is identified as a resident in the UK in the 2005 Labor Force Survey, with (self-reported) eleven years since migration, we merge the individual-level observation with the 1994 (= 2005 - 11) World Bank data on Nigeria, and the five-year interval 1990-1995 Barro and Lee (2013) data.

Controls unique to a given country pair do not vary over time. These controls include distance between the two countries, contiguity, common legal system, common religion, and measures for common historic past (colonizer, war, etc.). Data on the distance between two countries and their contiguity originally comes from the CEPII database. Common colonization history data is taken from Head et al. (2010). Data on common legal systems comes from *JuriGlobe*, the University of Ottawa's world legal systems database. The measure on common religion is based on the *CIA World Factbook*, enhanced with information obtained from the International Religious Freedom Report[74], The WorldChristianDatabase.org, and the Pew Research Center[75]. Finally, the number of years at war comes from the *CorrelatesOfWar.org* archive. Bilateral migration flows and stocks are taken from the United Nations. We use the same variables as suggested by Head et al. (2010).

We examine the importance of linguistic proximity using the CLI, a measure of the *ease of communication* between individuals from two different countries. Conceptually similar to Melitz and Toubal (2014), we set up our study using the raw data on the ASJP linguistic proximity measure from Bakker et al. (2009) and combine it with measures of common official language (COL), common spoken language (CSL), and common native language (CNL) from *CIA World Factbook* and (European Commission, 2006) data.[76] The logic behind the approach is intuitive: If there exists a COL for a given pair of countries, people from the two countries will most likely be able to communicate with one another fairly easily (and be able to judge on someone's credentials), making actual linguistic proximity less important. Similarly, suppose there is some positive probability that two randomly selected individuals, one from each country, are able to communicate in a CNL (for instance, many people in the United States report Spanish as their native language) or in another CSL (e.g. via a *lingua franca*). In that case, the importance of linguistic proximity between the predominant languages of the two countries is less relevant. Some degree of *ease of communication* exists, and our CLI indicator captures this.[77]

[74] See `https://www.state.gov/j/drl/rls/irf/` (last accessed: 27.07.2024).

[75] The data comes from "Mapping the Global Muslim Population. A Report on the Size and Distribution of the World's Muslim Population" available at `https://www.pewforum.org/2009/10/07/mapping-the-global-muslim-population/` (last accessed: 27.07.2024).

[76] The missing data on CNL and CSL fractions were hand collected by Melitz and Toubal (2014) and can be found in the online appendix of their paper.

[77] The exact methodology behind constructing the CLI measure is rather complex. Keeping it simple, CNL and CSL can roughly be interpreted as measuring the likelihood that two randomly selected individuals, one from each country, can communicate in their native language (CNL) or another language they both speak (CSL). See Melitz and Toubal (2014) for a detailed description of the underlying methodology.

To obtain our CLI indicator, we standardize the original language proximity index (*ease of communication*) by our sample, to be bounded by the values of 0 and 1 for the lowest and the highest linguistic proximity in our data, respectively.[78]

3.4.1 Summary statistics

Table 3.1 reports summary statistics of our data. Note that we use several sources of data, each with multiple variables. In the interest of brevity, we report in table 3.1 key variables of interest for three groups of variables: data which we are able to recover from individual data in our sample, data on migration from the United Nations which are matched at a country level, and economic data matched at country or country-pair level.

Our sample consists of a large number of observations from Brazil (32% of observations) and the United States (48% of observations). This is because these two data sources are essentially censuses. Meanwhile, data for Israel, Germany, France, and the UK come from representative population surveys. To adjust for these effects in the individual-level regressions, the observations are weighed by the inverse of the destination country sample size.

The sample of destination countries allows us to study quite diverse cases in terms of immigration prevalence, years since migration, and structure in terms of education and age. We have countries with long and short traditions of immigration (high average value of years since immigration indicator), attracting mostly highly skilled immigrants and quite the opposite. Even in terms of age, there are countries where migrants are younger than native salaried workers as well as the opposite. Perhaps more relevant for this study, the sample of destination countries is characterized by quite diverse migration patterns in terms of geographic proximity (e.g. contiguity or distance) and common culture (proxied here by religion and common colonial past).

[78]Note that our measure of linguistic proximity departs from Melitz and Toubal (2014) in one minor detail, namely we standardize the ASJP measures by common spoken language rather than common native language, see figure B.1 for comparison. This form of standardization is closer to the mechanisms we propose in our model: the ability of an employer to inquire about the qualifications of a candidate employee with a foreign diploma. In the interest of transparency, we estimate our model also with the original common language index by Melitz and Toubal (2014), as reported in Appendix B.4.

Table 3.1: Summary statistics

	Total	Argentina	Brazil	Canada	France	Germany	Israel	Mexico	UK	USA
	Individual-level data									
# of obs.	42,862,635	1,117,908	13,555,036	1,076,098	402,225	301,969	197,929	5,095,568	539,941	20,575,961
# of natives	39,322,727	1,075,475	13,523,179	910,651	369,750	253,229	109,488	5,081,264	502,009	17,497,682
# of immigrants	3,539,908	42,433	31,857	165,447	32,475	48,740	88,441	14,304	37,932	3,078,279
# of recent immigrants	540,013	38,523	11,278	28,820	2,976	3,352	16,317	10,884	9,206	418,657
% of recent immigrants	0.153	0.908	0.354	0.174	0.092	0.069	0.184	0.761	0.243	0.136
Years since immigration	19.62	3.50	9.31	18.90	28.25	19.96	22.92	3.20	20.00	19.87
% of HE natives	0.368	0.333	0.145	0.619	0.308	0.313	0.329	0.195	0.167	0.588
% of HE immigrants	0.507	0.196	0.446	0.657	0.237	0.160	0.369	0.449	0.233	0.519
Age of natives	37.25	38.72	34.19	37.49	39.42	40.45	32.96	33.53	39.59	40.46
Age of immigrants	40.95	43.90	42.24	41.88	43.88	40.31	43.08	35.30	38.71	40.82
	Country-level data: migration (from United Nations)									
# of immigrants in DC	-	1,749,013	781,583	4,487,372	5,969,137	7,058,662	1,632,704	516,352	3,766,946	27,571,091
(s.d. of OCs in DC)	-	(136,592)	(48,342)	(89,099)	(282,479)	(225,947)	(50,543)	(6,751)	(96,272)	(242,630)
	Country-level data (World Bank, Barro and Lee (2013) and Melitz and Toubal (2014)									
common language index (CLI)	-	0.686	0.476	0.555	0.629	0.416	0.348	0.688	0.541	0.462
	-	(0.351)	(0.331)	(0.300)	(0.206)	(0.267)	(0.169)	(0.343)	(0.299)	(0.284)
common official language (COL)	-	0.462	0.071	0.391	0.316	0.044	0.056	0.516	0.338	0.291
	-	(0.503)	(0.259)	(0.493)	(0.471)	(0.206)	(0.232)	(0.504)	(0.475)	(0.455)
common spoken language (CSL)	-	0.458	0.076	0.392	0.429	0.239	0.138	0.520	0.372	0.302
	-	(0.454)	(0.197)	(0.299)	(0.236)	(0.251)	(0.073)	(0.454)	(0.339)	(0.324)
common native language (CNL)	-	0.371	0.043	0.100	0.047	0.025	0.054	0.423	0.101	0.112
	-	(0.429)	(0.175)	(0.183)	(0.106)	(0.124)	(0.074)	(0.412)	(0.257)	(0.233)
log of distance	-	8.704	8.891	8.831	6.998	7.920	7.448	8.547	8.257	8.858
	-	(1.052)	(0.653)	(0.653)	(0.952)	(1.031)	(0.773)	(0.782)	(0.982)	(0.562)
% of DC-contiguous OCs	-	0.192	0.190	0.043	0.316	0.103	0.111	0.065	0.014	0.015
% of common colonizer OCs	-	0.000	0.000	0.000	0.000	0.000	0.056	0.000	0.000	0.000
common religion	-	0.499	0.373	0.199	0.349	0.147	0.057	0.447	0.122	0.151
GDP per capita in OC (as % of DC)	-	267%	269%	43%	76%	31%	33%	357%	44%	29%

Note: Individual-level descriptive statistics obtained for sample of salaried workers aged 18-64 years old. Migrants defined as recent if resident in hosting economy for less than 5 years. The linguistic proximity index following Melitz and Toubal (2014), except that we standardize the distribution by common spoken language rather than common native language indicators. All estimations were obtained also for the original Melitz and Toubal (2014) specification standardized by common native language, these robustness results are reported in tables B.4 and B.5 in the appendix. Standard errors in parentheses (where applicable). OC and DC denote origin country and destination country, respectively.

3.4.2 $\hat{\gamma}_{d,o,t}$ estimates – the two-step approach

The first step in our two-step approach, i.e. the estimation of equation (3.3), yields estimates of returns to skills for individuals with tertiary education, across origin countries, relative to natives in the respective destination countries. Since we estimate the wages in logs, the coefficients have a clear interpretation: It is the percentage wage deviation of migrants from the average wage of native university graduates, broken down by country of origin. Tertiary education per se is estimated relative to individuals without tertiary education. For example, consider that a tertiary-educated native earns 20% more than a native without a tertiary degree. Further, consider that for a specific country of origin in the given destination country, the estimate $\hat{\gamma} = -10\%$ is equivalent to stating that an immigrant with earns 10% less than a native with the same education. We do not interpret the estimates of $\gamma_{d,o,t}$ as causal measures of wage discrimination against particular groups of migrants - we treat them as what they are analytically: measures of the dispersion of wages across the migrants' origin countries. Our objective is to check how much of that dispersion, i.e. the variance in origin-country-specific wages, can be explained by CLI.

Table 3.2: Number of $\hat{\gamma}_{d,o,t}$ estimates by country of destination

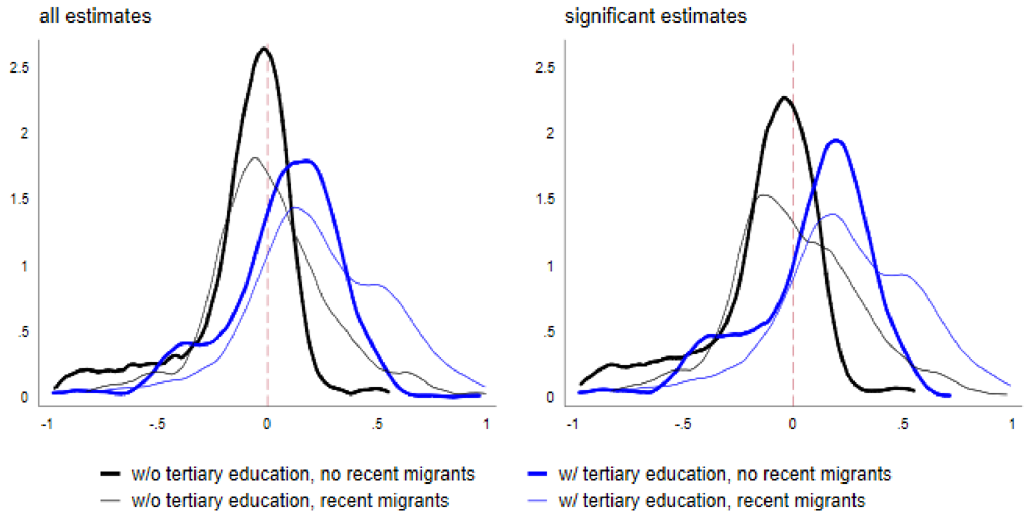
Destination	# of estimates			# of significant estimates			# of countries
	Total	Positive	Negative	Total	Positive	Negative	of origin
Argentina	151	36	115	96	11	85	28
Brazil	245	121	124	227	112	115	44
Canada	146	70	76	141	68	73	25
France	117	46	71	92	30	62	20
Germany	396	85	311	255	34	221	72
Israel	118	55	63	106	50	56	22
Mexico	164	24	140	143	17	126	33
UK	454	159	295	437	150	287	77
USA	839	143	696	831	138	693	140
Total	2630	739	1891	2328	610	1718	153

Note: This table reports the number of estimates of the $\hat{\gamma}_{d,o,t}$ for all the countries of destination listed in table B.2. The reference group is a native worker with a tertiary degree, thus a positive coefficient signifies that a migrant worker with a tertiary degree earns more than an otherwise identical native worker. We report jointly the estimates for recent immigrants and settled immigrants. In principle, the total number of estimates should be six times the number of the countries of origin (with and without a tertiary degree, for recent migrants, settled migrants, and for both groups together). For example, in the case of Brazil, 44 countries of origin imply 264 estimates in total. For some countries of origin and education levels, the coefficient $\hat{\gamma}_{d,o,t}$ could not be identified (insufficient degrees of freedom), which reduces the final sample to 245 estimated coefficients.

Table 3.2 reports the basic features of the obtained $\hat{\gamma}_{d,o,t}$ estimates from equation (3.3) on nine utilized individual-level datasets. We obtain in total 2,630 estimates of $\hat{\gamma}_{d,o,t}$ for 153 different countries of origin. Overall, roughly 89% of these $\hat{\gamma}_{d,o,t}$ estimates are statistically significant, i.e. the estimated $\hat{\gamma}_{d,o,t}$ coefficient has a p-value lower than 10%. Note that there are two reasons for an insignificant estimate: it is either insufficiently precisely estimated (true $\gamma_{d,o,t} \neq 0$, but we fail to reject the null hypothesis due to insufficient power relative to the variation in the data) or it is actually zero (true $\gamma_{d,o,t} = 0$). The latter implies that there is no wage premium/penalty for workers (of certain education level) from a given country of origin in a given destination country. The former implies that estimations with significant $\hat{\gamma}_{d,o,t}$ will be missing distribution mass around 0. This bias may not be large, though, as indicated by the overwhelming majority of estimates that are statistically significant. The outcome of the first step in our two-step procedure thus suggests that indeed for the vast majority of origins and destinations, there seem to be systematic deviations of migrant wages relative to the wages of native workers.

The deviations we find are not only statistically significant, but also economically relevant. Figure 3.1 visualizes the raw distribution of $\hat{\gamma}_{d,o,t}$ estimates obtained from equation (3.3). In line with the results reported in table 3.2, the distributions are remarkably dispersed and skewed towards negative values. We report two distributions: the one on the left-hand side provides the distribution of all estimates we obtained, while the graph on the right-hand side visualizes the significant estimates of $\hat{\gamma}_{d,o,t}$ only. Consequently, the estimates in the graph on the right-hand side are more dispersed since there are fewer estimates to be found in the range close around zero. Our results confirm that the vast dispersion in terms of wages across origin countries is not a unique feature of the United States (Butcher, 1994), but is a fairly general phenomenon.

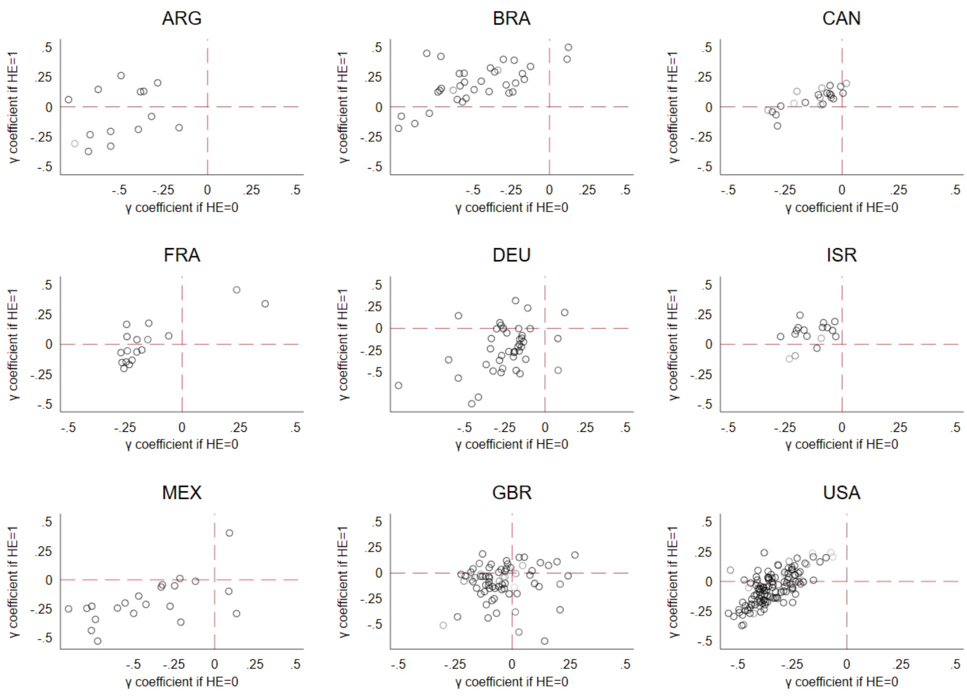
Figure 3.1: Dispersion of $\hat{\gamma}_{d,o,t}$ estimates



Note: The figures report the kernel density estimates for the distribution of $\hat{\gamma}_{d,o,t}$ from equation (3.3) for the full sample reported in table B.2. Blue lines show the distribution of estimates for highly (i.e. tertiary) educated migrants, and black lines for non-tertiary educated migrants. Bold lines represent the estimates for non-recent migrants, thin lines the ones for recent migrants. Recent immigrants are individuals who report residing in the country of destination for less than 5 years. Settled (non-recent) migrants denote individuals residing for 5 years or longer in the given destination country.

In figure 3.2 we further document the link between migrant gap and deviation of return to tertiary education among migrants. We portray on the horizontal axis the coefficient on the M_COO migrant dummy (i.e. the estimated effect for non-tertiary educated migrants from a certain origin country) and on the vertical axis the estimated $\hat{\gamma}_{d,o,t}$ effects for tertiary-educated migrants from the same origin. If there is a "halo" effect, one would expect a positive correlation between those two estimates, i.e. immigrants with a tertiary degree should experience a similar deviation from the average return to tertiary education for native workers as the immigrants without a tertiary degree observe from native workers without a tertiary degree. Indeed, we find a positive correlation of approximately 0.32 (with a standard error of 0.025).[79] This correlation, however, is heterogeneous across the nine studied destination countries: stronger in some countries (e.g. Canada and the USA) and weaker in others (e.g. the UK, Argentina, and Brazil). This heterogeneity implies that the strength of the "halo" effect may indeed differ not only across origin countries (some groups of migrants being more distinct than others), but also across destination countries.

Figure 3.2: Link between migrant wage gap and tertiary education gap among migrants



Note: The figures report the significant estimates $\hat{\gamma}_{d,o,t}$ from equation (3.3) for the full sample reported in table B.2, obtained jointly for recent and settled immigrants. The reference level for the two reported dummy coefficients is a tertiary-educated native. The shade of the circles denotes linguistic proximity (darker shade akin to more similar languages).

Incidentally, figure 3.1 also documents that the dispersion of $\hat{\gamma}_{d,o,t}$ is smaller for migrants without tertiary education than for migrants with tertiary education: kernel density estimates for immigrants without tertiary education (marked in black) are thinner than those for immigrants with tertiary education (marked in blue). We quantify this observation with the use of ANOVA analysis. We perform two independent variance decompositions: on the coefficient of the migrant dummy (M_COO) and on the $\hat{\gamma}_{d,o,t}$ estimates. In both cases, we use the same set of components: origin country fixed effects, destination country fixed effects, and migrant sample (settled versus recent versus jointly estimated for all migrants). We report those results in table B.3, illustrating the following observations: The variance of immigrant dummy estimates is considerably lower than for the migrants with tertiary education, but it is also more idiosyncratic. In fact, there is a high fraction in the variation of $\hat{\gamma}_{d,o,t}$ estimates that stems from destination countries, nearly 56% (compared to 34% on a lower total variation for the immigrant dummy). We also show that destination-by-origin variation essentially trumps the effects attributable

[79]This is an unconditional correlation coefficient, standard errors clustered for country pairs.

to destination countries, but not due to the origin countries. We interpret this analysis of variance as an indication that even if some hosting economies have particular tastes for immigrants from some origin countries (a flip side of the "halo" effects between migrants with and without tertiary degrees), this preference is certainly not enough to explain the dispersion of the obtained estimates. Indeed, there is more to be explored in the dispersion of returns to tertiary education among migrants. We formalize this analysis in the subsequent section.

3.5 Results

We present results in two substantive parts: the estimates at individual-level, which reflect the link between linguistic proximity and the position of migrants with tertiary education in the wage distribution, and the estimates at country-level, which reflect the link between linguistic proximity and the magnitude of deviation of return to tertiary education among migrants (enhanced with a robustness check for effect sizes across the wage distribution). These analyses look only at the population of salaried workers, aged 18-64. Note that individuals who obtained their education in the hosting economy are considered native workers, regardless of their country of birth. Our estimates therefore purposefully identify the foreign origin of a tertiary degree.

3.5.1 Individual-level analysis

We estimate equation (3.1) with a linear model, clustering the standard errors at the level of origin-by-destination country pairs.[80] This modeling choice is motivated by the fact that the variable of interest in our paper is $HE_i \times \mathbf{M}_i \times$ linguistic proximity$_{d,o}$ (the interaction of being a tertiary-educated migrant with certain linguistic proximity constraints). While variation in HE_i as well as $\mathbf{M}_i$ comes at the individual level, CLI$_{d,o}$ has only variation at country-pair level. With this choice, we can trust the standard errors of the parameter estimates for CLI$_{d,o}$, but it overstates the standard errors for all the parameters that are characterized by variation at the level of the individual, the origin country, or the destination country. Note that this modeling choice does not affect the estimates of the parameters, but only their standard errors. Table 3.3 reports our results.

We find that among native workers, tertiary education is associated with a wage percentile roughly 12.5 points higher than with secondary education or less, after adjusting for occupation and sector of employment. This result is roughly consistent with standard estimates of Mincerian wage regressions, which typically report about 20% wage gain due to tertiary education. Immigrants are on average 4.5 percentage points lower in the wage distribution. Column (1) of table 3.3 also reveals that relative to natives without tertiary education, immigrants with tertiary education are roughly 3.1 percentage points lower in the wage distribution. Note that these estimates adjust for industry and occupation, which implies that the interpretation of these two estimates is *within* the same occupation and industry rather than on average. We treat column (1) as reference points for subsequent estimations which explicitly account for linguistic proximity with

[80]Note that the sample size of more than 40 million individuals prevents the use of nonlinear estimators, such as maximum likelihood, which otherwise would be suitable for censored data (percentiles of wage distribution) and multi-level data (variation coming from the individual, the origin-country and the country-pair levels).

Table 3.3: One-step individual level regression

VARIABLES	(1)	(2) (1) + $CLI_{d,o}$	(3) (2) + $\mathbf{Z}_o$	(4) (3) + $\mathbf{Z}_{d,o}$	(5) (4) + $schooling_o$
HE for natives	12.548***	10.938***	10.190***	10.774***	8.957***
	(0.010)	(2.733)	(2.738)	(2.623)	(2.683)
Immigrants	-4.543***	0.364	0.866	-0.405	1.580
	(0.026)	(1.706)	(4.646)	(4.268)	(5.454)
HE for immigrants	-3.118***	-2.633	-1.726	-2.004	-1.865
	(0.038)	(2.144)	(3.171)	(3.060)	(3.161)
Linguistic proximity (CLI)					
for immigrants w/o HE		8.021***	6.568***	5.438**	12.972***
		(1.880)	(2.536)	(2.224)	(4.882)
for immigrants w/ HE		1.645	2.465	1.853	3.908*
		(2.438)	(2.552)	(2.342)	(2.367)
Model specifications					
COD FE	YES	YES	YES	YES	YES
COO FE	YES	YES	YES	YES	YES
Individual $\mathbf{X}$'s	YES	YES	YES	YES	YES
Origin $\mathbf{Z}$'s	NO	NO	YES	YES	YES
Pair $\mathbf{Z}$'s	NO	NO	NO	YES	YES
S.E. clustered for country pairs	NO	YES	YES	YES	YES
Observations	41,864,392	41,787,776	41,397,055	41,351,265	41,351,265
R^2	0.235	0.236	0.237	0.237	0.238

Note: Linguistic proximity measured as common language index, following Melitz and Toubal (2014), with the exception that we standardize the final measure by common spoken language rather than common native language. Sensitivity analysis with common native language as standardizing variable reported in table B.4 in the appendix. Standard errors clustered at the level of destination-by-origin country pairs in parentheses. $\hat{w}$ is the dependent variable, which is the destination country-specific wage percentile for individual i as per equation (3.1). Individual controls (available upon request) include age, age squared, gender, marital status, no. of children, occupation, industry, and years since immigration. Origin country controls (available upon request) include GDP per capita (adjusted for purchasing power, i.e. PPP-adjusted), fertility, mortality, and population size, merged by the year of arrival at a destination country for each individual i. Country-pair controls (available upon request) include the geographical distance between origin and destination countries, as well as contiguity dummy, common colonizer dummy, years at war measure, common religion, and common legal system (constant over time). Conventional levels of statistical significance are denoted by asterisk: ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.10$, respectively.

the inclusion of the CLI. Note that as of column (2) we are forced to cluster standard errors at the country-pair level (origin-by-destination). For example, comparing the standard error on the HE dummy between columns (1) and (2) we see that the estimate of the standard error goes up from 0.01 to 2.7. The clustering procedure is excessive for variables with variation at e.g. individual or origin-country level, but it is necessary to adequately infer the statistical significance of CLI.

The results in columns (2)-(5) report the estimates for the key variable of interest in our study: linguistic proximity. Note that this variable is only relevant for immigrants. We find that greater linguistic proximity between the origin and destination country is associated with a significantly higher percentile position in the wage distribution, in particular for immigrants without a tertiary degree. The effects are positive for immigrants with a tertiary degree as well, but they are smaller and less precisely estimated. Once we adjust for the quality of schooling in the origin country, as reported by column (5), we find a stronger positive correlation between linguistic proximity (as measured by CLI) and wage percentile in the destination country for both groups of migrants. At the same time, the coefficient for returns to tertiary education for natives is somewhat smaller: 9 percentiles rather than 12.5 percentiles.

To interpret the magnitude of 13 percentiles for the linguistic proximity (as measured by CLI) from our preferred specification in column (5), we perform the following exercise: With a wave of migration from Russia to Israel in the 1990s, Russian has become a significant CSL for the country-pair: an estimated 12.5% of the population in Israel can communicate effectively in Russian as a spoken language.[81] The CLI index value we observe for the country pair is 0.138. We evaluate a counterfactual value of CLI in our sample, as if this wave of immigration had not occurred, i.e. as if the share of Russian speakers in Israel would be essentially close to zero. In other words, we calculate the country-pair CLI value for a scenario in which Russian would be a rather irrelevant language in Israel – not being as relevant for any of the CLI index components CNL, CSL or ASJP linguistic proximity anymore. We obtain the value of $CLI = 0.018$. Our estimates in table 3.3 on the influence of CLI then imply that, on average, immigrants from Russia with a tertiary degree can expect to be about 1.6 percentiles higher in Israel's wage distribution in Israel nowadays, once one in eight citizens in Israel can easily communicate with them. Note that in this regard, our estimates are in line with the study of Eckstein and Weiss (2004).

Another intuitive example is provided by the differences among Nordic languages: we compare Norwegian, Swedish, and Danish relative to contemporaneous German. The ASJP linguistic proximity measure reports a relative linguistic proximity of 0.43 for Norwegian, and roughly 0.36 for Swedish and Danish. If Swedish or Danish were linguistically as similar to German as Norwegian is, then the average wages of immigrants with a university degree from those two countries would rank roughly 1 percentile higher in the relative wage distribution in Germany, *ceteris paribus*.

The migrant dummy continues to be negative in the more comprehensive specifications, but in terms of magnitude, it declines by roughly a factor of two. The same holds for the relative wages of migrants with a tertiary degree, relative to natives without a tertiary degree. Naturally, these estimates should be evaluated against standard errors of the magnitudes similar to those reported in column (1), because these are individual-level dummy variables, whereas columns (2)-(5) cluster standard errors at origin-by-destination country level.

3.5.2 Country-level analysis

The country-level analysis is based on a two-step procedure: we first obtain estimates of returns to tertiary education in each destination country for each observed origin country and subsequently, we utilize these obtained measures of dispersion in rewards to human capital as explained variables. Whereas in table 3.3 the explained variable was in percentiles of (within destination country) wage distribution, in our two-step country analysis the explained variable is the immigrant wage gap, expressed in percent of average wages. Since the explained variable itself is estimated from individual-level data, we apply bootstrapping to adjust the estimators, these bootstrapped specifications are denoted by a in table 3.4. The procedure of bootstrapping addresses the random nature of the explained variable, but still treats all observations equally. Meanwhile, highly statistically significant estimates of $\gamma_{d,o,t}$ are naturally more reliable than the insignificant ones. In a similar spirit, the estimates of $\gamma_{d,o,t}$ which refer to large migration flows may be of more policy relevance than those that refer to rare and quantitatively negligible migration flows. We thus introduce two separate weighting mechanisms: by

[81] The Russian diaspora in Israel accounts for almost all of the reported CSL value of the country pair.

$t-statistic$ of the obtained $\gamma_{d,o,t}$ estimator and by the size of the bilateral migration flow (scaled by the size of the sending country). While weighting by $t-statistic$ leverages our certainty about our estimates, weighting by migration flows yields a picture of reality that adjusts for the importance of migration flows: it weights our estimates by the relative number of migrants from a certain COO in a COD, i.e. mirroring the actual migrant counts reported by the United Nations. This should further minimize any potential biases due to differences in the sampling techniques used in the underlying datasets – further increasing overall reliability. Our preferred specification is reported in column (4a) of table 3.4. The other specifications permit identifying the role of weighing and bootstrapping against the raw estimation in column (1). All specifications include control factors for the origin countries, fixed effects for destination countries, and control factors for origin-by-destination country pairs. Finally, in all specifications, we cluster standard errors by origin country.

We find that on average, having tertiary education is associated with relative wages roughly 18 percentage points higher than the relative wage of individuals without any such degree, as reported by the specification (4a) in the first row of table 3.4, our preferred specification. This effect is consistent with other literature on returns to higher education.

Table 3.4: Two-step country-level regression

	(1)	(1a)	(2)	(3)	(3a)	(4)	(4a)
VARIABLES			$p-value<0.15$	$w=[t-stat]$		$w=[migr_{o,d}]$	
HE	0.153***	0.153***	0.175***	0.227***	0.227***	0.181***	0.181***
	(0.024)	(0.028)	(0.026)	(0.016)	(0.034)	(0.019)	(0.044)
CLI for immigrants							
w/o HE	-0.061	-0.061	-0.042	-0.037	-0.037	0.008	0.008
	(0.051)	(0.059)	(0.056)	(0.069)	(0.093)	(0.082)	(0.190)
w/ HE	0.137***	0.137***	0.128***	0.096***	0.096*	0.154***	0.154***
	(0.041)	(0.047)	(0.045)	(0.031)	(0.059)	(0.032)	(0.077)
Model specifications							
Bootstrapping	NO	YES	NO	NO	YES	NO	YES
DC FE	YES	YES	YES	YES	YES	YES	YES
OC $\mathbf{Z}$'s	YES	YES	YES	YES	YES	YES	YES
Pair $\mathbf{Z}$'s	YES	YES	YES	YES	YES	YES	YES
Clustered S.E.	YES	YES	YES	YES	YES	YES	YES
Observations	1,445	1,445	1,255	1,445	1,445	1,445	1,445
R^2	0.417	0.417	0.457	0.689	0.689	0.541	0.541

Note: Linguistic proximity measured as CLI, following Melitz and Toubal (2014), with the exception that we standardize the final measure by common spoken language rather than common native language, a robustness check with common native language standardization reported in table B.5 in the Appendices. Standard errors clustered at the level of the destination country. $\hat{\gamma}_{o,d,t}$ is the dependent variable, which is the destination-by-origin country-specific wage gap for immigrants, with and without tertiary education, relative to natives without tertiary education, as per equation (3.4). Origin country controls (available upon request) include GDP per capita (PPP adjusted), and population size, merged by the year of arrival at a destination country for each individual i. Country-pair controls (available upon request) include the geographical distance between origin and destination countries, as well as contiguity dummy, common colonizer dummy, years at war measure, common religion, and common legal system (constant over time). Conventional levels of statistical significance are denoted by asterisk: ***, **, and * denote $p<0.05$, $p<0.10$, and $p<0.15$, respectively. Standard errors in parentheses.

Recall that our main hypothesis relates to migrants *with tertiary education*. Immigrants without such a degree show no effect to linguistic proximity. Our results on tertiary-educated migrants, though, are robust across specifications and consistent: greater linguistic distance lowers migrants' returns on a university degree obtained in their home country. The effect is sizable: roughly 10-15 percentage points of relative wages. To put this number in perspective, an immigrant with a tertiary degree from a linguistically close country (CLI close to 1) will earn 15% more than an immigrant with an equal educational achievement obtained in a linguistically distant country (CLI close to 0), according to our preferred specification 4a.

We again utilize the examples from section 3.5.1 to illustrate this finding: Our estimates on the influence of CLI imply that highly skilled immigrants from Russia to Israel, who obtained their tertiary degree in their home country, may expect 1.8% higher wages than if there were essentially no population in Israel speaking any Russian – i.e. if there had been no migration wave in the 1990s establishing Russian as a relatively common language of communication in Israel. Conforming with our results, Eckstein and Weiss (2004) find an increase in returns to education for immigrants from Russia to Israel between 1990 and 2000. Similarly, immigrants from Sweden or Denmark to Germany could expect to earn roughly 1% more, if their languages were as similar to German as Norwegian is.

Overall, the results from the country-level two-step approach yield a strong conclusion that the higher the linguistic proximity between a given pair of origin and destination countries, the lower the average immigrant wage gap for individuals with (foreign) tertiary education. While the gap prevails even after adjusting for linguistic proximity, the CLI measure can account for a sizable part of dispersion in returns to a university degree.

3.5.3 Robustness of country-level results across the wage distribution

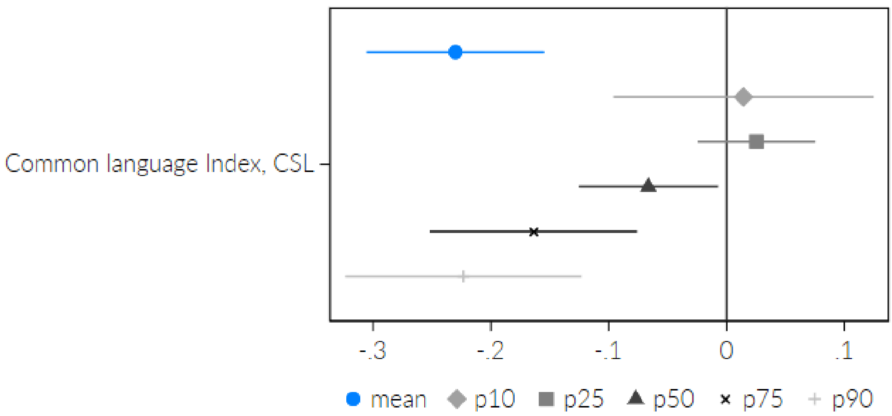
The Mincer earnings regressions function we propose for our first step regression in the country-level analysis provides us with estimated coefficients for values at the *mean* of the wage distribution. Similarly, the individual-level analysis provides us estimates of deviation at the *mean* of the wage distribution. However, tertiary-educated migrants tend to work in higher-paid occupations, presumably earning higher wages than the host country *mean* wages and also higher wages than the mean wages among all fellow immigrants from their home country. Effect size (and respective significance) may therefore differ at wage levels other than at the mean of the wage distribution. Indications for this phenomenon are already visible from our analysis so far (sections 3.5.1 & 3.5.2).

Similarly, the more unequal the distribution of wages in the hosting country, and the more skewed this distribution, the higher the probability that a coefficient estimated at the mean wage level offers a rather distorted picture of reality. Indeed, Butcher and DiNardo (2002) report that immigrant-native wage gaps can be affected by differences in the wage structure. Since the wage structure might differ over time both *within* and *across* the sampled destination countries of this study, it is imperative for us to see whether our results hold for different levels of the wage distribution.

Perhaps the most intuitive approach, a quantile regression would be viable in order to see whether the estimated coefficients are constant across the wage distribution. Such an approach would take into account the full variation available from the individual level. Unfortunately, the massive amount of data makes such an approach impractical due to high computing time. Instead, we follow a similar, but less computing-intensive approach: We apply the popular decomposition methods by Oaxaca (1973) and Blinder (1973) on our country-level analysis to obtain immigrant-native wage gap estimates at the median (rather than only at the mean), and complement this with the decomposition method of Juhn et al. (1993) to get estimates at other percentiles of the wage distribution – we estimate gaps at the mean, median, and at the 10th, 25th, 75th, and 90th percentile. Subsequently, we verify the importance of our measure of *ease of communication* (CLI) in assessing the part of the estimated wage gaps that other observed factors cannot explain away, i.e. we extract the unexplained component of the estimated gaps and survey how well CLI explains it. This second step follows our preferred model specification from section 3.5.2 and weights observations by migration flows.

Note that these decomposition techniques are more dependent on sufficient numbers of observations for immigrant groups along the wage distribution than quantile regressions: Instead of estimating effect sizes with one regression per destination country, Oaxaca-Blinder, and Juhn-Murphy-Pierce decompositions are applied to find the contribution of differences in observables and unobservables separately per country pair and per skill level *simultaneously*. Each group of immigrants from a certain origin country with a certain education level is compared with the respective groups of natives. Accordingly, this approach increases the number of distinct immigrant groups under consideration, lowering the number of observations available per group. This makes individual migrant observations relatively more important, and therefore potentially increases standard errors relative to the findings assessed in tables 3.3 & 3.4. On the other hand, robust results on this exercise then provide substantial support for the overall picture we assess here. Since we are interested in the returns to higher education, and given the findings in the previous sections, we report the results for highly skilled immigrants only.

Figure 3.3: CLI effect for highly skilled immigrants at different levels of the wage distribution



Note: Shown is the effect of *ease of communication* (CLI) at the mean and at different percentiles of the wage distribution. The dependent variable is the unexplained component of the estimated deviations from the average wage of natives. The estimated model follows our preferred model specification from section 3.5.2, with observations weighted by migration flows. Confidence intervals at 90% level. Detailed estimates are available upon request.

As seen in figure 3.3, the greater the linguistic proximity between origin and destination countries (as measured by CLI), the smaller the size of the adjusted migrant wage gap. At the mean, the results are similar to our simple two-step approach, which hints that the composition effects are not that large when one accounts for all the covariates included in the first-step regression. The estimate at the mean appears to be driven by origin-destination pairs of estimates of the adjusted migrant wage gap above the median wage. Accordingly, in the lower range of wages, we do not find much explanatory power of the CLI index. This finding can stem from two processes. First, at the bottom of the wage distributions within each destination country, the wages may be so low that the distribution is relatively compressed, thus leaving little room for variation in the adjusted migrant wage gap. Clearly, above median wages, there is more room for differentiating wages, *ceteris paribus*. Second, higher wages are typically associated with a higher skill intensity, which may be related to more detailed vetting of candidates' documents. Candidates from origin countries with relatively lower CLI index score face a greater challenge in demonstrating transparently which skills they possess before starting a job. This would imply why both adjusted migrant wage gaps have higher variations, but closer correlations with linguistic proximity.

In summary, linguistic proximity allows immigrants to reduce the immigrant wage gap. We find this result in our two-step approach, where the interpretation of the coefficient is simply the percentile of the wage distribution (greater linguistic proximity is correlated with a higher percentile of the wage distribution, *ceteris paribus*). We confirm this result in an analysis where instead of an immigrant dummy, we employ a two-step strategy with parametric and semi-parametric decomposition methods to obtain the immigrant wage gap adjusted for individual characteristics. The semi-parametric distributional analysis shows that linguistic proximity is associated with lower adjusted migrant wage gaps at and above the median of the wage distribution in destination countries.

3.5.4 Discussion and possible extensions

Our results demonstrate that linguistic proximity partially explains the dispersion of the returns to foreign tertiary education among immigrants. We propose a novel mechanism behind this empirical regularity: employers may find it more challenging to inspect the qualifications of job candidates who obtained their education in a foreign country. Some earlier literature has argued that immigrants' employment prospects depend on natives' trust towards origin countries (Keita and Valette, 2019), which is typically lower the greater the geographic distance (Cettolin and Suetens, 2019). Earlier literature argues that social, ethnic, and cultural distance shape natives' attitudes towards migrants from certain origin countries (O'Rourke and Sinnott, 2006). In the labor context, authors report discrimination in hiring decisions based on immigrants' geographic origin (e.g. Pager, 2007, Bertrand and Duflo, 2017, Neumark, 2018, Lancee, 2021). Our study is thus consistent with earlier literature.

Our results point to the role of linguistic proximity for screening candidates with a tertiary degree. Curricula in a foreign language appear to be more informative if the communication between hosting and sending economies is eased by linguistic proximity. Our results do not reflect immigrants' ability to speak the hosting economy language: highly skilled migrants typically speak one of the main global languages, often the specific language of the destination country. Instead, our findings focus on the cost to employers of inspecting skill certifications, imperative in the case of many occupations. We show that the cost of such inquiry is statistically significant and economically relevant, as proxied by the estimates of linguistic proximity on the migrant wage gap. Our estimates adjust for sending-country fixed effects, hosting-economy fixed effects, and a number of additional confounding factors.

Our study is not without caveats, of course. Foremost, *migrant selectivity* could be relevant on several levels: self-selection into destination countries (Adsera and Pytlikova, 2015), return migration decision (Dustmann and Görlach, 2016), and task specialization (Peri and Sparber, 2009). While our two-step approach delivers estimates of the immigrant wage gap, we are agnostic about the causal interpretation of the gap. The only point that we argue is that methodologically comparable gaps adjusting for individual characteristics are higher for immigrants arriving in linguistically more distant countries *ceteris paribus*. So long as the destination countries' selectivity bias is constant across the sending countries, our estimates remain reliable. If destination country selectivity were pair-wise rather than destination-wise, our estimates could potentially confound selectivity with linguistic proximity. Note that we cannot estimate the model with destination-by-origin fixed effects in the two-step approach, because linguistic proximity is typically time-invariant. However, we are able to adjust the estimates for a number of destination-by-origin characteristics, such as geographical, historical, and economic factors, including a variety of factors suggested in the past to drive migration selectivity patterns (Yao and van Ours, 2015, Adsera, 2015). In addition, our study includes a heterogeneous group of destinations, with both high and middle-income countries. Under the assumption that middle-income destination countries differ in selectivity from high-income ones, our estimates are reasonably comprehensive.

The return migration bias hints that returns to migration are generally overstated due to the inability to observe in the data those individuals who were unsuccessful in the hosting economy (Chiswick, 1978, Constant and Zimmermann, 2011, Dustmann and Goerlach, 2015, Dustmann and Görlach, 2016). Our estimates are provided separately for recent and non-recent immigrants. Our estimates also adjust for years since immigration to a given destination country. Without data on return migration, we cannot refine the estimates further. However, specifications reporting the correlations between linguistic proximity and returns to immigrants' human capital adjust for destination countries' fixed effects. Specifically, as long as the return migration bias is not driven by linguistic proximity, our estimates remain informative. Further research would be needed to investigate the role of linguistic proximity in return migration decisions and thus the bias.

A third type of migrant selectivity is that immigrants self-select into production tasks that tend to be less linguistically intensive than typical natives' tasks (Peri and Sparber, 2009). Such division of labor may stand behind the migrant wage gap per se (D'Amuri and Peri, 2014). However, our estimates adjust for occupation and industry effects. In addition, our two-step approach explores the *dispersion* of returns to human capital across origin and destination countries.

A final caveat relates to measurement issues: linguistic proximity as well as education quality across origin countries are challenging to capture and as such questionable. For linguistic proximity, a variety of robustness checks has been performed with alternative measures and the results are consistent. The common language index by Melitz and Toubal (2014) has more variation than other measures, thus delivering superior statistical quality. For this reason, it is included in this study. Another measure with strong variation used in the recent literature, the country mean TOEFL (Test of English as a Foreign Language) scores by Ku and Zussman (2010), is robust for countries with significant emigration to English-speaking countries, but much less indicative for origin countries with other dominant migrant destinations. For the quality of education, we rely on the most broadly utilized measure delivered by Barro-Lee datasets. The advantage of this data is that it is comprehensive and has a long history (which is relevant for individual-level analysis). However, educational attainment is merely a proxy for the quality of education available in a given country (Hoekstra, 2020). The most promising alternative measure is the *Global Dataset on Education Quality* by The World Bank (see Altinok et al., 2018, Patrinos and Angrist, 2018). This dataset is forthcoming, but is unfortunately not yet available. The data in the World Bank's *Human Capital Index* (see Kraay, 2018) projects the current measures of health and education on future worker productivity. This data correlates well with Barro-Lee data and is relatively new; thus it is not likely to affect stereotypes among employers. Learning-adjusted Years of Schooling (Filmer et al., 2020) suffers from limited data availability among developing countries.

3.6 Conclusion

Immigrant wage gaps are well documented in the literature, but the dispersion of these gaps across origin countries has been less studied. The literature formulates the skill portability hypothesis, which posits that immigrants without sufficient knowledge of the host economy language may be unable to convey their skills. We postulate an alternative mechanism: immigrants with tertiary education typically possess sufficient command of main global languages to convey their skills to potential employers, however, the employers find it more difficult to inspect the credentials behind their university diplomas if those were obtained in a linguistically distant country. This type of mechanism may be less relevant for occupations where certification is internationalized and standardized, but many occupations and fields of education do not have such certification systems.

We put this hypothesis to an empirical test. We exploit individual-level data from nine popular destination countries and document the inequality in returns to education across origin countries. We relate this inequality to linguistic proximity between the origin and destination countries, and we find that greater linguistic distance between origin and destination is associated with a higher average wage gap for skilled immigrants. These results are robust to a range of different specifications. We also find evidence that the role of linguistic proximity is heterogeneous across origin countries: high-skilled immigrants from countries with low average educational attainment experience the largest gap in returns to their tertiary education.

Our findings imply several policy-level recommendations. First, if concerned with equity among tertiary-educated workers, public policy ought to introduce interventions focused on employers. Well-structured information about the educational setting and curricula should be provided, especially in relation to linguistically distant countries. In particular, selective migration policies, which aim to attract individuals with particular sets of skills regardless of their country of origin, ought to be complemented by close communication with employers' organizations and HR departments to facilitate understanding of conditions experienced by the arriving immigrants. Second, while migration networks have been found to be effective among low and middle-skilled workers due to the specificity of hosting labor markets, such networks may deliver particularly high value added for tertiary-educated workers, because they effectively eliminate the linguistic distance friction for the employers. Third, international cooperation with low-income countries may benefit from aid programs fostering the internationalization of academic certificates and diplomas, at the very least providing translations of curricula to the main global languages.

3.7 Appendix B

Table B.1: Summary of language measures used in migration literature

Dataset	Coverage	Data provided	Examples of literature
World Fact Book[a]	267 world entities (mostly countries)	Lists official and spoken languages in a country (and sometimes the percentage of speakers)	[CL] (Mayda, 2010, Ortega and Peri, 2013)
Ethnologue: Languages of the World[b]	7,111 known living languages	Linguistic lineage, i.e., the language family	[CL] for (Karemera et al., 2000, Pedersen et al., 2008); [LP] (Desmet et al., 2009, Belot and Hatton, 2012, Adsera and Pytlikova, 2015)
World Atlas of Languages (WALS)[c]	2,679 languages	Linguistic lineage, i.e., the language family	[LP] (Lohmann, 2011, Isphording and Otten, 2013, Chen, 2013)
Dyen percentage cognate matrix of linguistic distances (Dyen et al., 1992)	Indo-European languages	Language-pairwise linguistic proximity information, based on the vocabulary-wise cognation of 200 words from each language (see Swadesh, 1952)	[LP] (Belot and Ederveen, 2012, Adsera and Pytlikova, 2015)
Levenshtein Linguistic Distance Matrix (Brown et al., 2008, Bakker et al., 2009)	(approx.) 3500 languages	Language-pairwise linguistic proximity information, based on the ASJP	[LP] (Isphording and Otten, 2014b, 2013, Isphording, 2014, Adsera and Pytlikova, 2015)
Linguistic Distance to English (Chiswick and Miller, 1998)	43 languages	Information on linguistic proximity to English (based on the difficulty Americans have in learning these languages)	[LP to English] (Chiswick and Miller, 2001, 2005, Hutchinson, 2005, Isphording and Otten, 2013)
CEPII[d]	195 countries	Country-pairwise language information, including common official, spoken and native language, and two measures of linguistic proximity	[CL] (Grogger and Hanson, 2011, Beine et al., 2011); [LP] (Melitz and Toubal, 2014), *our paper*

Note:
[a] `https://www.cia.gov/library/publications/the-world-factbook/`
[b] `https://www.ethnologue.com/` [c] `https://wals.info/`
[d] `http://www.cepii.fr/CEPII/en/welcome.asp`, see Melitz (2008), Melitz and Toubal (2014)
CL denotes common language. LP denotes language proximity. Isphording and Otten (2013) provides an intuitive illustration of the methodology used by Bakker et al. (2009). The algorithm that calculates the minimal Levenshtein distance is based on pronunciation and vocabulary of 40 (or 100) words (ASJP code) from each language. See also Ginsburgh and Weber (2020) on a short examination of the different measures, main advantages and shortcomings of the methods, and on more economic literature applying linguistic proximity measures.

Table B.2: Individual level datasets used in this study

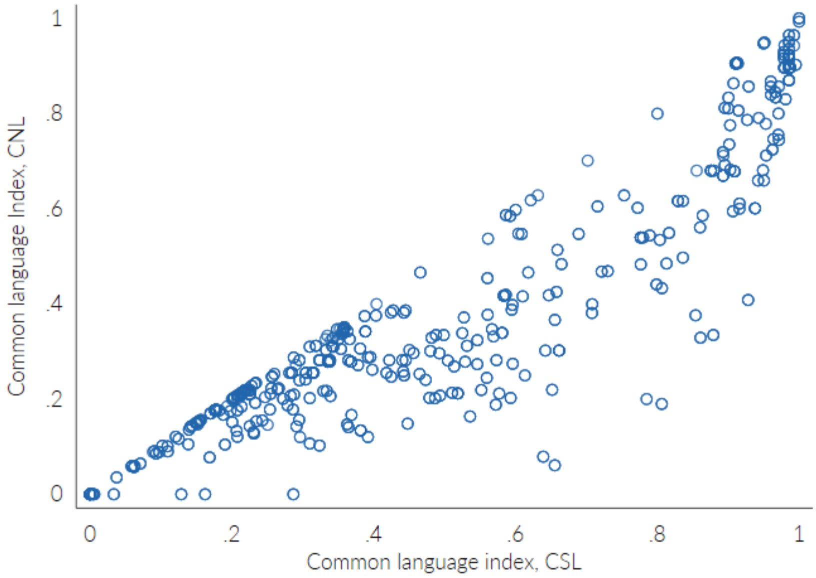
Country	**Dataset**	**Years**
Argentina	Encuesta Permanente de Hogares (*EPH*)	2004-2018
Brazil	IPUMS-published census data	1991, 2000, 2010
Canada	IPUMS-published census data	1981, 1991, 2001, 2011
France	Labor Force Survey (*LFS*)	2003-2012
Germany	Socio-Economic Panel Study (*SOEP*)	1984-2016
Israel	IPUMS-published census data	1983, 1995
Mexico	IPUMS-published census data	1990, 2000, 2010
UK	Labor Force Survey (LFS)	1992-1998, 2000-2007
USA	American Community Survey (*ACS*)	2001-2018

Table B.3: ANOVA decomposition of variance in estimated coefficients

Estimate if $immigrant = 1$ **and**	$HE = 1$		$HE = 0$	
	(a)	(b)	(a)	(b)
Residual variance	29.11	13.33	26.39	8.73
Captured variance	50.16	65.93	34.54	52.20
due to destination country	56%	1%	34%	1%
due to origin country	40%	18%	51%	11%
due to destination-by-origin	-	24%	-	34%
due to migrant sample	1%	0%	4%	2%

Note: specifications denoted by (a) include destination country effects and origin country effects. Specifications denoted by (b) include destination-country effects and origin-country effects as well as destination-by-origin country effects. All specifications include the migrant sample, which has one of the three levels across all specifications: (i) recent migrants; (ii) settled migrants; (iii) joint estimation for all migrants. Recent migrants are defined as residing shorter than 5 years in a destination country. All components shown are highly significant (bootstrapped standard errors available upon request).

Figure B.1: Common language index measures: standardization by CNL and CSL



Note: The figure portrays the correlation between CLI measures, standardized by common native language (CNL), and by common spoken language (CSL). Melitz and Toubal (2014) standardize by common native language (CNL).

Table B.4: Table 3.3 with common language index standardized by common native language

VARIABLES	(1)	(2) (1) + $CLI_{d,o}$	(3) (2) + $\mathbf{Z}_o$	(4) (3) + $\mathbf{Z}_{d,o}$	(5) (4) + $schooling_o$
HE for natives	12.548***	10.269***	9.686***	10.344***	9.269***
	(0.010)	(3.060)	(2.945)	(2.738)	(2.725)
Immigrants	-4.543***	-1.203	-0.597	-1.638	0.878
	(0.026)	(1.997)	(5.329)	(5.043)	(6.302)
HE for immigrants	-3.118***	-1.891	-0.950	-1.365	-1.762
	(0.038)	(2.468)	(3.512)	(3.338)	(3.226)
Linguistic proximity (CLI)					
for immigrants w/o HE		4.475**	3.238	2.728	11.596**
		(2.038)	(2.885)	(2.712)	(5.788)
for immigrants w/ HE		2.308	2.960	2.274	3.598*
		(2.893)	(2.800)	(2.460)	(1.859)
Model specifications					
COD FE	YES	YES	YES	YES	YES
COO FE	YES	YES	YES	YES	YES
Individual $\mathbf{X}$'s	YES	YES	YES	YES	YES
Origin $\mathbf{Z}$'s	NO	NO	YES	YES	YES
Pair $\mathbf{Z}$'s	NO	NO	NO	YES	YES
Clustered S.E.	NO	YES	YES	YES	YES
Observations	41,864,392	42,803,955	42,410,834	42,408,457	42,408,457
R^2	0.235	0.214	0.212	0.213	0.215

Note: Linguistic proximity measured as common language index, following Melitz and Toubal (2014). CLI index standardized by common native language (rather than by common spoken language as in table 3.3). Standard errors clustered at the level of destination-by-origin country pairs in parentheses. $\hat{w}$ is the dependent variable, which is the destination country specific wage percentile for individual i as per equation (3.1). Individual controls (available upon request) include age, age age squared, gender, marital status, no. of children, occupation, industry, and years since immigration. Origin country controls (available upon request) include GDP per capita (PPP adjusted), fertiliy, mortality and population size, merged by the year of arrival at a destination country for each individual i. Country-pair controls (available upon request) include geographical distance between origin and destination countries, as well as contiguity dummy, common colonizer dummy, years at war measure, common religion and common legal system (constant over time). Conventional levels of statistical significance are denoted by asterisk: ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.10$, respectively.

Table B.5: Table 3.4 with common language index standardized by common native language

VARIABLES	(1)	(1a)	(2) p-value< 0.15	(3) w=[t-stat]	(3a)	(4) w=[$migr_{o,d}$]	(4a)
HE	0.123***	0.123***	0.146***	0.223***	0.223***	0.197***	0.197***
	(0.021)	(0.023)	(0.024)	(0.016)	(0.035)	(0.017)	(0.041)
CLI for immigrants							
w/o HE	-0.128***	-0.128**	-0.083	0.004	0.004	0.019	0.019
	(0.058)	(0.066)	(0.065)	(0.081)	(0.104)	(0.095)	(0.215)
w/ HE	0.258***	0.258***	0.243***	0.128***	0.128**	0.164***	0.164**
	(0.045)	(0.050)	(0.050)	(0.041)	(0.073)	(0.038)	(0.098)
Model specifications							
Bootstrapping	NO	YES	NO	NO	YES	NO	YES
DC FE	YES	YES	YES	YES	YES	YES	YES
OC $\mathbf{Z}$'s	YES	YES	YES	YES	YES	YES	YES
Pair $\mathbf{Z}$'s	YES	YES	YES	YES	YES	YES	YES
Clustered S.E.	YES	YES	YES	YES	YES	YES	YES
Observations	1,477	1,477	1,287	1,477	1,477	1,477	1,477
R^2	0.419	0.419	0.457	0.685	0.685	0.549	0.549

Note: Linguistic proximity measured as CLI, following Melitz and Toubal (2014). CLI index standardized by common native language (rather than by common spoken language as in table 3.4). Standard errors clustered at the level of destination country. $\hat{\gamma}_{o,d,t}$ is the dependent variable, which is the destination-by-origin country-specific wage gap for immigrants, with and without tertiary education, relative to natives without tertiary education, as per equation (3.4). Origin country controls (available upon request) include GDP per capita (PPP adjusted), and population size, merged by the year of arrival at a destination country for each individual i. Country-pair controls (available upon request) include the geographical distance between origin and destination countries, as well as contiguity dummy, common colonizer dummy, years at war measure, common religion and common legal system (constant over time). Conventional levels of statistical significance are denoted by asterisk: ***, **, and * denote $p < 0.05$, $p < 0.10$, and $p < 0.15$, respectively. Standard errors in parentheses.

Chapter 4

Confucius Institutes and labor market outcomes of Chinese immigrants in the United States*

Confucius Institutes (CIs), Chinese government-sponsored institutions promoting Chinese language and culture abroad, are known to foster trade, investments, and cultural exchanges with China. Yet scant attention has been given to their labor market impacts. We examine the association between local CI presence and the wages and employment likelihood of Chinese immigrants in the United States. Integrating individual-level American Community Survey (ACS) data with PUMA-level data on CI locations, we find that the local presence of CIs correlates with reduced wages for Chinese immigrants. Furthermore, we observe a U-shaped relationship where each additional local CI decreases wages, albeit at a decreasing rate. An event study reveals that it is not only the actual CI openings being detrimental to the wages of Chinese immigrants, but already the mere announcement of a CI initiates a decline in their wages. Our results are robust across alternative model specifications, but we fail to detect any relationship between CIs and the employment probability of Chinese immigrants.

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4.1 Introduction

Since 2004, China has opened more than 500 Confucius Institutes (CIs) abroad as part of its so-called *soft power strategy* (Hubbert, 2019). More than 110 CIs have been opened in the United States alone. The CIs, most of which offer low-cost language courses and cultural events, are primarily intended to promote international exchange with China and to raise and deepen awareness of the Chinese language and culture (Lien and Oh, 2014, Huang and Lyu, 2019). Some researchers find that opening CIs abroad is associated with trade and foreign direct investment between CI host countries and China (Akhtaruzzaman et al., 2017, Ghosh et al., 2017, Lien et al., 2012, Lien and Co, 2013). Others find that the number of CIs is positively correlated with the number of tourists and foreign students in China (Lien et al., 2014, Lien and Miao, 2018, Huang and Lyu, 2019, Lo et al., 2023). However, little is known about whether the Chinese diaspora living in CI-hosting countries is affected if natives get to learn more about the Chinese language and culture. Recent literature by Keita and Valette (2019) shows that labor market outcomes of immigrants in Germany depend on the attitudes German natives have towards immigrants' origin countries. They direct their finding to a perceived lower trustworthiness natives associate with foreigners from more distant countries, as was found by Cettolin and Suetens (2019).[82] Following these arguments and the intended function of CIs, we may expect that opening CIs in the United States could increase positive attitudes of Americans towards Chinese and decrease potential disadvantages of Chinese immigrants on the United States labor market.

CIs serve to familiarize Americans with China and are intended to reduce any perceived distances (or differences) between the two countries (Lien et al., 2019). Accordingly, CIs could be an effective tool for improving local labor market outcomes for Chinese workers: American employers screening the local labor market may find it easier to recruit Chinese workers and offer them better terms when they perceive to know them better than others. On the other hand, relatively few people (or probably only a specific, interested group of individuals) actually attend language courses and cultural programs at CIs, limiting their potential positive impact. Moreover, the establishment of a new CI triggers local attention in social networks, news, and other media. The coverage thus generated may relate not only to the CI itself, but may also target broader issues related to China and its relations with the United States. The intensification of the China-United States rivalry over the past two decades, combined with debates over CIs affecting academic freedom at host universities, and accusations of CIs potentially even being used for espionage (e.g. Sahlins, 2015), could drive more negative media coverage, triggering new cultural rifts in communities or deepening existing ones. Any CI-induced additional presence of China in local public discourse may therefore intensify (pre-existing) local sentiments against the Chinese-born and potentially worsen their local labor market opportunities.

In order to investigate the impact of CIs on the Chinese diaspora's labor market performance, we use the following research strategy: First, using correlation analysis, we study the average relationship between the local *existence* of CIs and the labor market outcomes of Chinese immigrants living nearby. Second, as a follow-up analysis, we investigate the relationship between the actual *number* of local CIs and our outcome variables. Finally, we set up an event study research design, in which we examine

[82]See also Göran and Hägg (1994) on the role of trust in economic contracts under incomplete information.

whether there tends to be a causal impact of CI establishment on the wage level of Chinese immigrants living nearby and how this effect develops over time.

The empirical analysis reveals a detrimental relationship between the existence of local CIs and Chinese immigrants' hourly wages. Furthermore, we find that the number of local CIs is relevant in this relationship: Our model reveals a U-shaped relationship, where each additional local CI lowers Chinese immigrants' wages but at a decreasing rate. On average, having six open CIs increases the wage gap to the largest extent, and further CI openings then narrow the wage gap. This latter finding is in line with our assumption that the initial attention-triggering effect of establishing CIs exceeds the cultural-distance-reducing effect of language courses and cultural events at local CIs. Our event study findings suggest that establishing new CIs in the United States has a negative effect on Chinese immigrants living nearby. The negative wage effect gradually weakens over the years after an opening.

Our paper contributes to the literature in three ways. First, we document the remarkable wage disadvantages of Chinese immigrants across local labor markets in the United States. Second, we add to the literature on language institutes and their economic impacts. To the best of our knowledge, we are the first to study the correlation between language institutes and labor market outcomes of immigrants from the institute-operating country in the institute-host country. Third, we provide further evidence on the determinants of immigrant integration. Moreover, our results tend to suggest the importance of attitudes the host country population has on immigrants' origin country.

The paper continues as follows: Section 4.2 outlines the CI program and provides an overview of the establishment process of CIs in the United States. Section 4.3 summarizes the literature on the labor market performance of immigrants and the influence of language-learning facilities and outlines our theoretical understanding of how CIs may impact the labor market outcomes of Chinese immigrants. Subsequently, we discuss our data and methodology in section 4.4. Finally, section 4.5 presents our empirical results, followed by the concluding remarks in section 4.6.

4.2 Confucius Institutes in the United States

CIs are non-profit public institutions, which were initiated by Hanban, the *Office of Chinese Language Council International*, administratively part of China's Ministry of Education.[83] Hanban's official website provided an overview on CIs, their intended goals, functioning, financing, and the like.[84] These institutes are typically established at higher education institutions, in particular at universities, which mostly co-finance them together with China's Ministry of Education (Starr, 2009). Their main aim is to provide an institutionalized framework to distribute knowledge on Chinese culture and language worldwide, similar to the missions of German Goethe Institutes, France's Alliance Francaise or the UK's British Council, to name a few. In particular, CIs promote Chinese language and facilitate cultural exchange by offering language courses (mostly free of charge or at cost price) for both university students and the wider public, conducting extracurricular activities for students, providing training for teachers, and hosting pre-

[83] In June 2020, the CI was fully handed over to *Chinese International Education Foundation* CIEF.

[84] Hanban used to publish an annual CIs development report (with information on opening year and cooperating partner of each CI in operation) at: `http://english.hanban.org/` (last accessed: December 21, 2020).

sentations on cultural aspects including business-related topics. CIs also provide advice on establishing and maintaining trade relationships with Chinese companies.

The first CI located in the United States was announced in 2004 and began its operations at the University of Maryland in 2005. A slew of other openings followed throughout the country, resulting in a significant change in the number of United States-based open CIs over the past 20 years.

Table 4.1: Number of open CIs in the United States

Year	2005	2006	2007	2008	2009	2010	2011	2012
Number of CIs	1	10	31	40	56	72	74	86
Year	2013	2014	2015	2016	2017	2018	2019	2020
Number of CIs	99	108	112	114	114	113	91	84

Notes: Own calculation based on Hanban's annual development reports.

Table 4.1 provides the annual count of open CIs in the United States from 2005 to 2020. The table documents a consistent upward trend in the number of open CIs starting from 2005, reaching a peak during the years spanning 2013 to 2018 (with each single year showing roughly 100 or more CIs being open during that time), before decreasing to a level of 91 in 2019 and 84 in 2020 (which is about the same level as in 2012). Although certain places show an exceptionally high number of CIs (New York State had a double-digit number of CIs open since 2013), CIs are rather commonly distributed across the United States: Between 2015 and 2018, at the peak of the annual count of open CIs, there were only three US states (Mississippi, Vermont and Wyoming) not hosting any CIs, i.e. almost every US state hosted at least one open CI, and on average two. As of 2020, with a notable decline in the number of open CIs across the United States since 2018, there were still only seven states that host no single CI (notably, this list includes the state of Illinois, which hosts some of the country's most prestigious universities).[85]

The selection of where CIs in the United States open does not rest solely with Hanban, but also involves local stakeholders in the decision-making process. Typically, local stakeholders voice their interest in having a CI established in their area (arguably to strengthen higher education partnerships), and then collaborate with Hanban to initiate the setup (Lien and Co, 2013). Consequently, these local stakeholders, particularly universities and faculties, hold significant influence in the initial stages of the location selection process. Moreover, many policymakers seek to make Chinese language courses accessible and affordable in their area (Hubbert, 2019), which is also evident by the widespread presence of CIs across the country. However, it is important to note that Hanban emphasizes that CIs target not only the local native population but also the Chinese diaspora. Additionally, there is evidence suggesting a higher likelihood of CIs being established in major cities (Zhou and Luk, 2016, Metzgar and Su, 2017), which aligns with the fact that CIs in the United States are exclusively affiliated with universities. To address these issues in our analysis, we consider the individual's area of residence as a controlling factor, as well as local population density, the local share of Asians and individual-level information on whether a person lives in a urban area or not (see our methodological section 4.4). Maps visualizing the location of open CIs over the years in the United States are available from the appendix, figure C.1.

[85]See also exemplifying maps of existing CIs for various years in figure C.1 in the appendix.

4.3 Literature review

4.3.1 Immigrants' assimilation and racial discrimination

Labor market disadvantages of immigrants are well documented in the literature (early studies include Chiswick, 1978, Borjas, 1985, Chiswick, 1986, Jasso and Rosenzweig, 1988, Borjas, 1987). Most of this literature seeks to attribute these worse labor market prospects to characteristics of the migrating individuals, in particular their limited knowledge of the new labor market, and problems associated with transferring skills from origin to host country. How well immigrants assimilate into the United States labor market, though, has been reported to have changed quite substantially over time (Borjas, 1995, Chiswick and Miller, 2012, Borjas, 2015), and seems to depend on an immigrant's country (and culture) of origin (LaLonde and Topel, 1992, Blau and Kahn, 2007, Borjas and Katz, 2007, Lubotsky, 2007).

However, successful immigrant labor market integration might also depend on the taste of employers, i.e. employers' willingness to hire individuals from certain immigrant groups, and the potential preference for individuals from certain origin countries and/or cultural backgrounds over other applicants. Individual employers' hiring decisions (and contract terms) may thus be influenced by the employer's norms and prejudices, by inherent sociotropic perceptions and by individual taste-based rankings of sub-groups (minorities) of society (see the seminal work by Becker, 1957). This is a different angle compared to most other literature on immigrant labor market outcomes: away from individual characteristics that matter for labor market performance, and towards cultural norms and stereotyping prevalent in the host country society, which matter for the integration of the individual migrant. According to O'Rourke and Sinnott (2006), attitudes towards individuals *of certain migrant backgrounds* depend on the social, ethnic and cultural distance projected on them. Immigrant attributes relevant in this context particularly include language features (Hopkins, 2015, Schmaus, 2020), religion (Adida et al., 2010, Berinsky et al., 2020), and ethnocentric appearances (Kinder and Kam, 2010). Also relevant for the perceived distance of an origin country is the normative context prevalent in society, raised, e.g., by the media coverage of certain countries, religions, etc. (Siebers, 2010, De Coninck, 2020).[86]

As per inter-group contact theory (for instance Amir, 1969, 1976, Stein et al., 2000, Bursztyn et al., 2021), the intensity of interactions between different groups have an effect on the attitudes and behaviors members of these groups display towards each other. Most literature (e.g. Forbes, 1997) finds that the more frequent and cooperative the contact between group members, the more positive attitudes become. The incidence of group contacts between natives and immigrants is not evenly distributed across a country (e.g. Bartel, 1989). Rural areas (without many incoming migrants) tend to oppose migration more (Mayda, 2006, Hopkins, 2010). Typically one finds more favorable attitudes towards immigrants where the local share of migrants is higher (i.e. where the *likelihood of group contact* is higher). Groups of immigrants that make up a higher relative share of the local population are then subject to more favorable attitudes (Allport, 1954, Hewstone and Brown, 1986, Ellison and Powers, 1994, Powers and Ellison, 1995, Hanson et al., 2009, Markaki and Longhi, 2013). In general, attitudes towards migrants

[86] For an extensive literature review on the effect of media on natives' attitudes towards migration and individual migrant groups, see Eberl et al. (2018).

differ both across geographic regions within the host country and across immigrants' countries of origin (Dustmann et al., 2007).

Attitudes matter in economic relationships: For instance, various authors report employers' hiring decisions to be influenced by job applicants' ethnicity (e.g. Bertrand and Duflo, 2017, Lancee, 2021, Thijssen et al., 2021, Veit and Thijsen, 2021).[87] Individuals from more distant countries may be considered less trustworthy for contractual relationships (Cettolin and Suetens, 2019). Indeed, Keita and Valette (2019) show that the labor market performance of immigrants depends on the attitudes natives have towards immigrants' origin countries – the more favorable the attitude towards an origin country, the better the labor market prospects of individuals originating from there.[88] Our study proposes that the opening of CIs may evoke opposition to and bias against Chinese immigrants, potentially affecting their labor market outcomes. This is consistent with the research of Lu and Sheng (2022), which shows that local Covid-19 cases triggered discriminatory attitudes towards Asian minorities in the United States (due to the widespread label of the pandemic under the term "China virus"). We also speculate that like the pandemic, the establishment of CIs could contribute to negative sentiment towards China and its people, triggering (labor market) discrimination.

4.3.2 Influence of language institutes

Language institutes have various intended purposes, but arguably the most important goal is teaching the language offered. Lien (2013) shows that CIs successfully increase the number of individuals learning Chinese in a given host country. Moreover, Lien and Miao (2018) find that the existence of CIs has a positive impact on the number of foreign exchange students studying at their respective Chinese partner universities, which they interpret (partly) as a trust-enhancing effect of CIs on local student communities' attitudes towards Chinese. Lien et al. (2017) examine the channels through which CIs work and suggest that CIs reduce information asymmetries between host countries and China, with effect size increasing with distance in institutional quality. Besides academic success, CIs, therefore, also have substantial economic impacts on host countries and on China itself: Lien et al. (2017) highlight the tourism-accelerating effect that CIs have on China, a finding they share with Ghosh et al. (2017), Lien et al. (2014) and Lo et al. (2023). The latter two also show positive impacts on business travel between CI-hosting countries and China.

There are also other economic impacts of CIs detected in literature: Lien and Co (2013) show that each additional CI set up in a US state increases the export volume of this state towards China by roughly 5 percent. Ghosh et al. (2017) verify that CIs also boost Chinese exports towards CI-hosting countries. Akhtaruzzaman et al. (2017) find that African countries receive larger incoming FDI flows from China in the years subsequent to establishing a CI in their country. Relatedly, Lien and Lo (2017), Lien et al. (2019), Demir and Im (2020), Xu et al. (2020) and Fieles-Ahmad and Huber (2022) find that in general, the existence of cultural institutes, like CIs and German Goethe Institutes, promote both trade and foreign direct investments. Liu et al. (2020) demonstrate that the existence of CIs strengthens economic integration through increased cross-border merger and acquisition actions between Chinese and foreign companies. On the other

[87]See Pager (2007) and Neumark (2018) for excellent overviews on earlier research addressing ethnic discrimination in hiring decisions and other dimensions of labor market outcomes.

[88]Fang et al. (2022) and Thijssen et al. (2021) report similar findings.

hand, however, increased exposure to Chinese imports has been shown to have detrimental effects on local labor markets in the United States, particularly in terms of local unemployment and lower wages (Autor et al., 2013). Moreover, exposure to Chinese imports leads to more negative attitudes of the local population towards minorities while increasing positive feelings towards in-groups (Ferrara, 2023), and reinforces (existing) nationalistic attitudes, as shown by Strain and Veuger (2022) and Steiner and Harms (2023).

Evidence on the effect of language-learning facilities on individual labor market outcomes is rather scarce. Uebelmesser et al. (2022) and Huber and Uebelmesser (2023) find that German Goethe Institutes increase the number of immigrants to Germany originating from the institute-hosting country. They attribute their finding to (assumed) better labor market prospects. Jaschke and Keita (2021) provide evidence that German Goethe Institutes increase immigrants' language fluency at arrival. They further report that there are selection effects – relatively more high-skilled individuals select to migrate to Germany when there is a Goethe Institute existing in a migrant's origin country, and the migrating individuals show more pre-migration labor market experience. To the best of our knowledge, there exist no studies examining the effect of language-learning facilities on the labor market outcome of immigrants from the facilities' origin country. Accordingly, we are the first to study the correlation between language institutes and labor market outcomes of immigrants from the institute-operating country in the institute-host country.

4.3.3 Confucius Institutes and wages of Chinese immigrants

Language-learning facilities, like CIs, may potentially affect labor market prospects via various channels. First, these institutions add to the knowledge of the language they promote in the local population. Natives may then find it easier to communicate with persons from the institutions' home country, increasing understanding *per se*. Dustmann and Fabbri (2003) as well as Schmaus (2020) show that there exists language-based discrimination in labor markets – labor market outcomes increase with language-proficiency of immigrants (see also section 4.3.1). The effect seems to be group-specific, indicating taste discrimination of employers towards certain groups of immigrants. CIs may reduce the extent of ethnic discrimination against Chinese workers due to limiting information asymmetries between employers and Chinese workers (*statistical discrimination theory*, e.g. Phelps (1972), Arrow (1973), Bertrand and Mullainathan (2004), Lancee (2021), Thijssen et al. (2021)). Second, CI language learning facilities are also intended to share knowledge on cultural aspects in the host country's society. Besides lowering information asymmetries, they thereby increase the actual (and perceived) number of contacts between host-country population and Chinese, and might bridge existing cultural differences and misunderstandings (i.e. increase mutual understanding) by facilitating communication between these groups.[89] This may make it easier for employers to engage in economic contracts with individuals – individuals they now perceive to know better or perceive to be able to judge better.[90]

[89]According to *inter-group contact theory*, the number of inter-group contacts typically increases attitudes among groups. See the seminal works by Allport (1954), Amir (1976) and Forbes (1997), and also corresponding empirical evidence by, e.g., Alba et al. (2005) and Hanson et al. (2009).

[90]See also Göran and Hägg (1994) on the influence of *trust* on the willingness to conclude contracts with strangers.

A completely different channel we want to raise here is that CIs may also have effects on the labor market outcomes of Chinese immigrants by triggering *media coverage* – thereby influencing public attitudes towards Chinese workers in the United States indirectly (but no less significantly).[91] Such CI-triggered media coverage may have a positive or negative effect on attitudes towards Chinese, depending on the framing of the information provided.[92] Hu et al. (2022) show that CIs do indeed trigger local media attention and that the news reports not only information on the respective CI, but also regularly covers China and Chinese politics. In general, the public discourse about CIs in the United States is dominated by China's motives behind establishing and funding the institutes, on aspects of Chinese state propaganda, censorship and limited academic freedom, and aggressive cultural interference in local communities (Paradise, 2009, Sahlins, 2015, Zhou and Luk, 2016, Gil, 2017). Hence, according to Wang and Adamson (2015), in the United States, "the institutes are viewed with a considerable degree of ambivalence" (p. 225). Zhou and Luk (2016) report that news coverage on CIs in the United States often involves stories on the 'China threat' and the global competition between both countries. Moreover, they report that CIs are regularly referred to in many media outlets as propaganda tools of the Chinese state, which negatively impacts the local communities that host the CIs.

This is in sharp contrast to the goal of China in setting up CIs. China perceives a more favorable news coverage of China as key to improving its image abroad, with the CI initiative being the main instrument to achieve this (Brazys and Dukalskis, 2019). Liu (2019) highlights the discrepancy between what Hanban expects in terms of media coverage and stories covered due to CI openings and what is actually reported in the media. According to a content analysis by Metzgar and Su (2017), around 40% of CI news coverage in the United States during 2003–2016 reports on new local CI openings, 24% highlights the actual language courses, cultural events, etc. offered by CIs, and 20% deals solely with the above-named controversies associated with CIs. Yang (2020) further acknowledges that the seven most common words in his analysis of media coverage of CI in the United States from 2010–2018 all have a negative connotation.

At least since 2014, when the University of Chicago became the first public institution in the United States to close an existing CI (after about five years of operation),[93] significant public opposition to CIs has become apparent. Since then, more and more universities across the country have decided to close their CIs, while at the same time, CIs have been established in other places. Since 2019 the balance of openings and closures has turned negative, and more CIs close than are established. By the end of 2020 a total of 69 CIs in the United States had ceased to exist (see also section 4.2).

4.4 Data and methodology

4.4.1 Data and variables

We take the information on when and where a CI has been opened (or closed) from Hanban's website. Hanban published an annual list of currently existent (or announced to

[91] See also section 4.3.1.

[92] See for instance the seminal work on the influence of *framing* on actual behavior by Kahnemann and Tversky (1979).

[93] See press release of University of Chicago on this matter: `https://news.uchicago.edu/story/statement-confucius-institute-university-chicago` (last accessed: 01.06.2023).

open) CIs worldwide until 2020. Though every year the current list replaces the previous list, it is possible to collect data on all years relevant to our project via the website *Wayback Machine*.[94] We enhance this information with publicly available data from sources such as university homepages, press releases, newspaper articles, and internet blogs in order to verify announcement and opening data for any CI in the United States.[95] We then identify the exact geographic coordinates of these CIs, which leaves us with a dataset that provides exact information on *when* and *where* CIs in the United States exist or are announced to be opened.

We utilize the IPUMS-published 2005–2019 American Community Survey (ACS) data for individual-level characteristics (see Ruggles et al., 2021). The ACS is a demographics survey program that samples 1% of the United States population annually, continuously collecting data over the sampling year. The dataset provides rich information on our outcome variables, i.e. individual wage levels[96] and the employment status of individuals. Moreover, the ACS data provides us with individual-level covariates such as age, sex, education (in three ISCED categories, i.e. low, medium, highly-educated), marital status, number of children, if an individual is living in a rural or urban area, a person's place of living (PUMA (**P**ublic **U**se **M**icrodata **A**reas) area and state), occupation and industry. This data allows us to run a Mincer-style regression for our analysis, identifying the impact of CIs on prospective individual-level wages. We categorize occupations into five levels (jobs not requiring skills, jobs requiring primary skills, specialists, highly-skilled jobs and managers) and industry into four levels (agriculture, manufacturing, construction and services). We identify Chinese immigrants by country of birth, which also comes with the ACS data. Additionally, we adjust for immigrants' age at arrival (identifying immigrants that entered the United States before turning 18) and recent immigrants (residing in the United States for less than 5 years). While age at immigration is an essential factor for labor market integration of immigrants (e.g. Schaafsma and Sweetman, 2001), identifying recent immigrants enables us to target potential bias from selectivity in return migration (Dustmann and Görlach, 2016).

Our sample consists of employed salaried workers aged 18 to 64 that live in a constant so-called PUMA area. Data-set-provided person weights allow us to obtain nationally representative statistics. We further drop self-employed individuals as well as observations that report negative incomes, where information on the hours worked is missing, or where individuals report having worked strictly 0 (despite being employed) or more than 100 hours per week (similar to e.g. Mishel et al., 2012). Where applicable, we use hourly wages, which we calculate from monthly wage data combined with information on the average hours worked in the past. Our full ACS sample covers all 50 United States states and the federal district of Washington, D.C., between 2005 and 2019. Table 4.2 reports the summary statistics of individual-level variables for our entire sample of 4,376,561 employees. To study the employment likelihood gap of Chinese immigrants, we apply an extended sample of 4,566,732 observations where we include unemployed individuals – the respective summary statistics are available in table C.1 in the appendix.

[94]The website *Wayback Machine* is a non-profit project that saves earlier versions of websites. See `http://web.archive.org/`, last accessed: 01.06.2023).
[95]A full list of data sources used is available from the authors on request.
[96]We use data for consumer price index (CPI) from the World Development Indicators database by the World Bank to adjust wages to real terms. The base year is 2010.

Table 4.2: Individual-level summary statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All obervations				Natives only		Chinese only		t-Test
	mean	sd	min	max	mean	sd	mean	sd	(col. 7 − col. 5)
Chinese immigrant	0.013	0.114	0.000	1.000					
Log hourly wage	2.852	0.755	-7.237	10.604	2.853	0.754	2.822	0.855	-0.030***
Age	39.694	12.834	18.000	64.000	39.671	12.849	41.457	11.502	1.786***
Male	0.503	0.500	0.000	1.000	0.503	0.500	0.488	0.500	-0.015***
Education *(3 ISCED categories)*	2.603	0.520	1.000	3.000	2.606	0.516	2.406	0.747	-0.200***
Married	0.495	0.500	0.000	1.000	0.492	0.500	0.699	0.459	0.207***
# of children	0.739	1.066	0.000	9.000	0.737	1.067	0.864	0.974	0.127***
Urban dummy	0.848	0.359	0.000	1.000	0.846	0.361	0.990	0.101	0.143***
Hours worked	39.556	11.500	1.000	99.000	39.562	11.494	39.082	11.931	-0.480***
# of observations	4,376,561				4,322,611		53,952		

Note: This table shows individual-level descriptive statistics using weighting factors (analytical weighting used to obtain nationally representative statistics). We document respective values for the entire sample in columns (1)–(4), natives in columns (5)–(6), and Chinese immigrants in columns (7)–(8). Column (9) shows the difference in means between columns (5) and (7). The hourly wage is in USD. All values are rounded to the third decimal place.

We merge individual-level observations via the survey year and the region code with macro-level covariates that provide information on the state level and on the regional level (i.e. PUMA regions). PUMAs partition each US state into several smaller regional units that (should) consist of about 100,000–200,000 inhabitants each. The shape and size of these areas thus depend on the *best fit* of the local resident population in a state into these smaller units.[97] Every 10 years, PUMAs' shapes are delineated to accommodate population changes detected in the decennial census.[98] Population changes in one PUMA may thus influence other PUMAs' shapes (but only within the same state). During our sampling period (2005–2019), there were 1930 and 2076 PUMAs before and after the 2010 census delineation (which took effect from 2012 onwards), respectively. Of these, roughly one-third are constant (718 PUMAs), i.e. geographically identical over both periods. As noted above, we only focus on geographically constant PUMA areas. Covariates providing PUMA-level information mostly stem from the National Historic Geographic Information System (NHGIS).[99] While we lack information on where exactly an individual surveyed in the ACS lives, we do know which PUMA region the individual lives in. The PUMA shape files from the United States Census Bureau provide the geographic boundaries of each PUMA, and we can thus determine each PUMA's geographical center. Combined with our dataset on the location of CIs, we calculate the number of CIs within a radius of k km around the midpoint of a PUMA – our main variable of interest. A circle with the mean PUMA area in our sample (roughly 3,000 km^2) would have a radius of slightly more than 30 km. We run our regression models with the following variations of k: We try k = (25 km, 50 km, 100 km), which we assume as reasonable values of the potential influence area of a CI.[100]

Our choice of PUMA-level covariates includes population, population density, income per capita, the share of Asians, the share of the working-age population and the share of low-educated workers. We use population to account for both the higher workforce demand and the higher demand for education institutions in metropolitan centers. Similarly, population density is intended to adjust for the specific labor market characteristics and the likelihood of establishing education centers. The income per capita can capture effects associated with the overall economic performance of a region (including area-specific wage levels). The share of Asians captures the impact of potential Asian diasporas in an area and is a proxy for the likelihood of immigrant labor market integration in a PUMA. The share of the working-age population and the share of low-educated workers are intended to adjust for labor competition factors specific to an area.[101] We report PUMA-level summary statistics in table 4.3.

[97]The mean geographical PUMA size is 3262 km^2, i.e. if PUMAs would have a square shape, their mean edge length would be roughly 57 km. Put differently, the closest PUMA border should typically be about 57 km$/2 = 28.5$ km away from the geographical PUMA center.

[98]For more details on the decennial delineation of PUMAs, see `https://www.census.gov/programs-surveys/geography/guidance/geo-areas/pumas.html` (last accessed: 01.06.2023).

[99]We use the IPUMS-published NHGIS data. See Manson et al. (2013).

[100]Brazys and Dukalskis (2019) use similar values as impacting radius of CIs.

[101]Note the similarity in our choice of covariates to Huang et al. (2020).

Table 4.3: PUMA-level summary statistics

	(1) mean	(2) sd	(3) min	(4) max
# of CI within 25km	0.644	1.499	0	8
# of CI within 50km	1.028	1.832	0	9
# of CI within 100km	1.723	2.362	0	10
Area (km^2)	2,933	10,851	4	239,346
Population (in 1000)	141	28	91	279
Population density (pop./km^2)	2,378	5,340	1	43,467
Income per capita (USD)	30,297	12,309	10,467	132,157
Share of Asians	0.062	0.094	0	0.706
Share of working-age population	0.603	0.088	0.291	0.857
Share of low-educated individuals	0.048	0.038	0.001	0.263
# of PUMAs		718		

Note: This table shows PUMA-level descriptive statistics. All values are rounded to either the nearest integer or to the third decimal place.

At the state level, we use a dummy for party-political orientation, the unemployment rate, the applicable minimum wage level (either state or federal, depending on which is higher), and per-capita state exports to China[102] as covariates. From the National Governors Association and the MIT Election Data Base, we extract data to generate a dummy variable for political orientation that identifies the party of the state governor (Democratic or Republican party).[103] Since the Republican party tends to view foreigners, CIs, and China as potential threats (Gries and Crowson, 2010), this variable is also intended to capture the general attitudes of citizens in this state toward these three issues. We use the unemployment rate and applicable minimum wage level to adjust for both local job opportunities and to account for region-specific wage levels and structures (in particular at the lower end of the wage distribution). These data come from the St. Louis Federal Reserve's FRED database and the United States Department of Labor, respectively. Regions with high export values typically show different labor market patterns than lstates which trade less. Regions with higher exports to China presumably demand a better understanding of China and its language and thus demand more CIs, and also more Chinese-speaking residents. To account for this, we employ per-capita state exports to China sourced from the United States Department of Commerce. State-level summary statistics are reported in table 4.4.

4.4.2 Empirical strategy

In the following analysis, we focus on all individuals living in constant PUMAs. We run three model variations that provide us with information on the nature of the relationship between CIs and labor market outcomes of the Chinese in the United States. We are, first, interested in whether the mere *existence* of CIs nearby has an effect on Chinese immigrants' labor market performance – if there is at least one open CI around, does

[102]We do not use imports because state-level imports data is only available for the period 2008–2019. We tested an exports-only model for this restricted period against a model that includes both exports and imports but did not detect fundamentally different results.

[103]Whenever a governor is neither Democratic nor Republican, the dummy declares the state's political orientation according to last senate seat election.

Table 4.4: State-level summary statistics

	(1) mean	(2) sd	(3) min	(4) max
Unemployment rate	0.058	0.022	0.024	0.137
Applicable minimum wage (USD)	7.428	1.351	5.15	14.00
Exports per capita	315.303	391.677	1.424	2,933.289
# of states		51		

Note: This table shows state-level descriptive statistics. All values are rounded to the third decimal place. Exports are measured in millions of USD.

it matter for Chinese' labor market outcomes? This captures an average effect of CIs on Chinese immigrants over time – no matter how recent or long ago the opening was and without regard to the effect strength of individual CIs. Second, provided we find a sustained CI effect, we are interested in the effect of individual CIs – verifying whether the labor market performance of Chinese immigrants changes with the *number* of CIs available in a PUMA. Finally, using an event study, we try to figure out whether there is a causal impact of opening CIs on Chinese immigrants' labor market characteristics.

Using the entire estimation sample, we first investigate whether the *existence* of open CIs nearby, irrespective of how long they have been open or the total number of open institutes, is correlated with Chinese immigrants' labor market prospects. We estimate the following regression model (4.1) utilizing the full information we have on the existence of *all* open CIs:

$$\begin{aligned} Y_i^{pst} &= \beta_0 + \beta_1 CN_i + \beta_2 CI_{pt}^k + \beta_3 CN_i \cdot CI_{pt}^k \\ &\quad + \boldsymbol{X_i'}\boldsymbol{\gamma_1} + \boldsymbol{Z_{pt}'}\boldsymbol{\gamma_2} + \boldsymbol{Z_{st}'}\boldsymbol{\gamma_3} + \lambda_p + \lambda_{st} + \lambda_t + \varepsilon_i. \end{aligned} \tag{4.1}$$

Y_i^{pst} is the outcome of interest (i.e. the wage or the employment status) of individual i in PUMA p in state s observed in year t. CN_i is a dummy variable identifying Chinese immigrants that takes the value of one if individual i was born in China and zero if i is a native. We employ the term CI_{pt}^k, which is a dummy variable that takes the value one if there is an open CI within k km of PUMA p's midpoint in year t and zero otherwise. We vary k ($k = 25, 50, 100$) in a *robustness check*, with our preferred model specification being $k = 50$ km. $\boldsymbol{X_i}$ is a vector of individual-level social demographic and labor market characteristics. $\boldsymbol{Z_{pt}}$ is a vector of PUMA-level covariates, $\boldsymbol{Z_{st}}$ a vector of state-level covariates. Moreover, we include PUMA fixed effects (λ_p) to adjust for region-specific factors that are time-invariant, plus we adjust for the state (linear) time trend (λ_{st}) that captures state-specific common components affecting individual's wages and/or employment probabilities (e.g. via the overall state structure and changes in a state's legislation, etc.) and we control for overall survey year fixed effects (λ_t). Standard errors are clustered at the PUMA level.

Second, we are interested in whether the *number* of local CIs has an influence on the labor market outcomes of Chinese immigrants. In addition to the level of CI numbers (NCI), we also take its quadratic term into consideration. We expect that the marginal effect of the number of CIs on Chinese immigrants' labor market performance may not be linear but non-linear. There are at least two theoretical foundations for why we expect non-linear effects in the number of local CIs: Foremost, it may be that opposition to the first CI in an area is strong, but people get tired of their resistance with each additional institute set up, meaning a declining effect strength. And further, it may be that the positive effect becomes stronger the more institutes exist, provided that more CIs provide more cultural events and language courses. Accordingly, in order to verify the importance of the number of open CIs, we employ the following empirical model:

$$\begin{aligned} Y_i^{pst} = \alpha_0 + \alpha_1 CN_i + \alpha_2 NCI_{pt}^k + \alpha_3 (NCI_{pt}^k)^2 + \alpha_4 CN_i \cdot NCI_{pt}^k + \alpha_5 CN_i \cdot (NCI_{pt}^k)^2 \\ + \boldsymbol{X_i'} \boldsymbol{\gamma_1} + \boldsymbol{Z_{pt}'} \boldsymbol{\gamma_2} + \boldsymbol{Z_{st}'} \boldsymbol{\gamma_3} + \lambda_p + \lambda_{st} + \lambda_t + \varepsilon_i, \end{aligned} \tag{4.2}$$

where Y_i^{pst} and CN_i have the same interpretation as before. Instead of the *existence* dummies, we use here NCI_{pt}^k for the *number* of CIs that are located within k km radius to the geographical center of PUMA p ($k = 25, 50, 100$) in year t. As in the other shown model specifications, we include covariate vectors $\boldsymbol{X_i}$, $\boldsymbol{Z_{pt}}$, $\boldsymbol{Z_{st}}$, state time trends λ_{st}, and λ_p and λ_t for PUMA and year fixed effects, respectively.

Our third strategy for investigation is an event study design focusing on the *first* CI to be opened in a region. For this analysis, we drop individuals from our sample that live in PUMAs having no CIs around during our entire observation period. Note, however, that many PUMA areas are subject to several nearby CI openings over time. We focus on the very first ('initial') CI opening to address the issue of noise from multiple (effectively overlapping) CI openings. Nonetheless, about 4 percent of PUMA regions are subject to another CI opening within one year of the initial CI opening in the area, and a total of 9 percent report at least one other CI opening within two years. Given this limitation, it is necessary for us to exercise caution when interpreting wage developments subsequent to the initial CI opening.

For our event study, we estimate the following regression model (4.3) using pooled cross-sectional data:

$$\begin{aligned} Y_i^{pst} = \theta_0 + \theta_1 CN_i + \boldsymbol{\theta_2} \boldsymbol{\Gamma_{pt}^j} + \boldsymbol{\theta_3} CN_i \cdot \boldsymbol{\Gamma_{pt}^j} \\ + \boldsymbol{X_i'} \boldsymbol{\gamma_1} + \boldsymbol{Z_{pt}'} \boldsymbol{\gamma_2} + \boldsymbol{Z_{st}'} \boldsymbol{\gamma_3} + \lambda_p + \lambda_{st} + \lambda_t + \varepsilon_i, \end{aligned} \tag{4.3}$$

where Y_i^{pst} is the wage (or alternatively the employment status) of individual i living in PUMA p in state s observed in year t, and CN_i verifies whether a person in our dataset is a Chinese immigrant or not. The variables of interest are event dummies in the vector $\boldsymbol{\Gamma_{pt}^j}$ (see below). $\boldsymbol{X_i}$ is a vector of individual-level social demographic and labor market characteristics, $\boldsymbol{Z_{pt}}$ a vector of PUMA-level covariates, $\boldsymbol{Z_{st}}$ a vector of state-level covariates, and λ_p, λ_{st} and λ_t capture PUMA fixed effects, state time trends and year fixed effects, respectively.

All the elements contained in $\boldsymbol{\Gamma_{pt}^j}$ together capture the timing of the event of interest, i.e. the regional opening of a CI within 50 km of the geographical midpoint of the PUMA area p a person lives in. For each time period (year) t, the dummies identify whether the event takes place in that time period t (i.e. $j = 0$) or how many years the current time period t is before ($j < 0$) or after ($j > 0$) the event. For instance, if the current time period t is two years after the local CI opening (i.e. $j = 2$), then all our event dummies are zero except the dummy Γ_{pt}^2. We design dummies that capture the event timing from $j = -5$ to $j = 5$; values outside this range are included in the respective most extreme dummy categories. We apply dummy Γ_{pt}^{-5} (i.e. five or more years before the opening) as the reference group. As mentioned above, we focus solely on the first CI being opened in a region. Ideally, the labor market performance of Chinese, i.e. the difference in Y_i^{pst} between natives and Chinese, should not vary significantly during the years before the first opening (i.e. when $j < 0$), which would suggest a causal impact of the first opening.

4.5 Results

This section presents and discusses the estimation results. We briefly introduce the relationship between the existence of CIs and the labor market outcomes of Chinese immigrants in section 4.5.1. Afterwards, we focus on the number of CIs in section 4.5.2, provide several robustness checks in section 4.5.3, and discuss the spillover effects as well as the heterogeneous effects in section 4.5.4 and section 4.5.5, respectively. Finally, section 4.5.6 depicts the results of the event study.

4.5.1 Existence of CIs and Chinese immigrants' labor market outcomes

We estimate the regression equation (4.1) and document results in table 4.5 below. We report results for both wage level and employment probability, taking into account CIs within 50 km of the midpoint of the PUMA area in which an individual resides.[104]

Table 4.5: Results – Existence of CIs

VARIABLES	(1) ln(hourly wage)	(2) Employment (0/1)
CN immigrant	-0.149***	0.006***
	(0.023)	(0.002)
CI^{50} existence	-0.003	-0.000
	(0.002)	(0.001)
CN immigrant × CI^{50} existence	-0.119***	0.001
	(0.020)	(0.003)
Model specifications		
Individual-level controls	YES	YES
PUMA-level controls	YES	YES
State-level controls	YES	YES
PUMA FE	YES	YES
Year dummies	YES	YES
State time trend	YES	YES
Observations	4,376,563	4,566,732
R-squared	0.382	0.031

Note: This table shows our main result on the existence of nearby CIs. The dependent variable in column (1) is the logarithm of hourly wage, whereas in column (2) it is individual employment outcome. We only observe natives and Chinese immigrants living in constant PUMAs. Individual-level covariates include age, gender, education, marital status, the number of children, living in an urban area, and the number of hours worked. PUMA-level covariates are PUMA population, population density, income per capita, the share of Asians, the share of the working-age population, and the share of low-educated workers. State-level covariates include party-political orientation dummy, unemployment rate, applicable minimum wage, and the logarithm of exports per capita. Standard errors clustered at the PUMA level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

104 Other specifications where we define vicinity to be 25 km and 100 km can be found in section 4.5.3.

The results in column (1) suggest that being a Chinese immigrant is associated with a roughly 15 percent lower wage than natives living in the same area, which is in line with previous research findings (e.g. Borjas, 1987, Kee, 1995, Adsera and Chiswick, 2007). On top of this effect, if there exists at least one open CI in the vicinity, Chinese immigrants' wages are depressed by another 11.9 percent. Accordingly, our findings suggest that the existence of one or more CIs within 50 km of a Chinese immigrant's residence-PUMA area lowers their wage prospects significantly, even though they already have worse labor market prospects than natives in terms of wages when no CIs are open.

If we turn to the results concerning the employment probability of Chinese immigrants (in column (2)), we find somewhat different results: The probability of being employed is slightly higher for Chinese immigrants than for natives by 0.6 percentage points. This is not surprising because most migrants typically move to other countries citing better job prospects. Migrants' motivation to work is higher than natives', no matter the (entrance) wage. This is also why labor market discrimination is typically found in the wage channel rather than the employment channel (see, e.g. Cain (1986)). We do not find the existence of CIs to be relevant for the employment probability of Chinese immigrants.

4.5.2 Number of CIs and Chinese immigrants' labor market outcomes

Provided the results from section 4.5.1, which show sustained effects of nearby open CIs on local labor market outcomes of Chinese immigrants, we are moreover interested in whether the number of CIs nearby matters in this context. Table 4.6 provides results on the *number of CIs* existing locally for Chinese immigrants' wages and employment outcomes.

Table 4.6: Results – Number of surrounding CIs

VARIABLES	(1) ln(hourly wage)	(2) Employment (0/1)
CN immigrant	-0.149***	0.006***
	(0.024)	(0.002)
# of CI^{50}	-0.003*	0.001**
	(0.002)	(0.001)
(# of $\text{CI}^{50})^2$	0.000	-0.000
	(0.000)	(0.000)
CN immigrant × # of CI^{50}	-0.071***	0.001
	(0.010)	(0.001)
CN immigrant × (# of $\text{CI}^{50})^2$	0.006***	-0.000
	(0.001)	(0.000)
Model specifications		
Individual level controls	YES	YES
PUMA level controls	YES	YES
State level controls	YES	YES
PUMA FE	YES	YES
Year dummies	YES	YES
State time trend	YES	YES
Observations	4,376,563	4,566,732
R-squared	0.382	0.031

Note: This table shows our main result on the number of nearby CIs. The dependent variable in column (1) is the logarithm of hourly wage, whereas in column (2) it is individual employment outcome. We only observe natives and Chinese immigrants living in constant PUMAs. Individual-level, PUMA-level, and state-level covariates are the same as reported in table 4.5. Standard errors clustered at the PUMA level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Column (1) provides results on a non-linear form of our model with the logarithm of the hourly wage as the dependent variable.[105] The model's findings imply that Chinese immigrants living in PUMA areas not in the vicinity of a CI are subject to a 14.9 percent wage penalty, similar to the *CI existence result* from section 4.5.1. Moreover, we detect a non-linear relationship between Chinese immigrants' wage gap and the number of nearby CIs, which is U-shaped. Our results suggest that the wage gap increases with each additional CI, but this impact gets smaller as the number of local CIs increases. On average, having around 6 CIs within 50 km increases the wage gap to the largest extent, and more than 6 CIs narrows the negative impact of CIs again. Column (2) again shows that the difference in the employment likelihood between Chinese immigrants and natives does not relate to the number of nearby open CIs.

4.5.3 Robustness of correlation results

In this section, we provide several robustness checks for the relationship between the number of CIs and the labor market outcomes of Chinese immigrants. First, instead of using 50 km to calculate the number of open CIs established in PUMAs where individuals live, we employ alternative distances. In this way, we can test whether our results are

[105] Rather than using a continuous measure of the number of open CIs within the vicinity of the midpoint of a PUMA, we also tried categorical dummies for the number of CIs nearby. We find similar results. See table C.2.

sensitive to different measures of exposure to CIs. Second, the impact of CIs on Chinese labor market outcomes may be delayed, especially through the channel of changing employers' attitudes towards Chinese workers. Therefore, rather than applying the current number of CIs, we use the values lagged by one year or two years. This strategy could also mitigate a potential reverse causality problem. Third, another possible source of bias may be that we measure a generally favorable or unfavorable attitude towards foreigners over certain years in certain regions (*trend assumption*). This is why we adjust for state time trend (λ_{st}) and state-level political leaning in the baseline model. In the robustness check, we run model variations to find out whether any effects found hold only for Chinese immigrants and not for other migrant groups. Furthermore, we run a robustness check in which we employ the relative wage of Chinese immigrants relative to all other immigrants (rather than all natives).

Varying vicinity-defining radius

As introduced above, in the first robustness check, we vary the vicinity definition for counting the number of open CIs. Table C.3 in the appendix shows results for the non-linear specifications with the number of nearby CIs as was laid out in equation (4.2), but with 25 km, 50 km (baseline) and 100 km radius. It is plausible that the influence of the number of CIs decreases with distance between the PUMA center and a CI. For instance, because the likelihood of receiving information about CIs or the interest of individuals in events organized by CIs decreases with distance.

The alternative model specifications reported in columns (1) and (3) of table C.3 support our baseline results in column (2) of the same table. The coefficient on being a Chinese immigrant is relatively constant over all distances. Moreover, as expected, the magnitude of the impact of the number of nearby CIs declines with the radius k (while the estimates remain statistically significant). The results suggest that our findings are robust to the use of different measures of the number of CIs.

Lagged CI counts

As the second robustness check, we apply the lagged value of the number of CIs with 50 km radius. There are several reasons why this analysis is relevant. First, the impact of CI numbers on Chinese immigrants' labor market outcomes may be delayed. Changes in employers' attitudes towards Chinese workers influenced by the opening of CIs may need some time to evolve. Moreover, any CI that was opened sometime within a certain year is treated as having existed in the whole year, i.e. a CI that was established in November 2011 appears in our data as having been open since the beginning of 2011 – all year round. Furthermore, our micro-level ACS data is a continuous survey that interviews all year round, i.e. individuals may be interviewed before and after the establishment of an institute. Therefore, workers interviewed before an opening of a CI may lead to estimation bias. To gauge the potential importance of these effects, we run two additional non-linear specifications where we lag the CI openings to see whether our results remain robust. The results on a 1-year lagged and a 2-year lagged model variation can be found in column (2) and (3) of table C.4 (in the appendix). We re-document our baseline results in column (1) for easy comparison.

Overall, the model variations show supporting results where the effect size shrinks with number of lags applied. This suggests that the immediate impact of CIs is the largest shortly after an opening, but decreases with time after the event, which is in line with the argument that CIs trigger short-term media attention, which shapes locals' attitudes towards Chinese migrants and reinforces (existing) nationalistic feelings and stances.

Other immigrants as reference group

In the baseline model, we only consider natives and Chinese immigrants in the United States, ignoring other groups of immigrants. There might be concerns that some unobservables could be correlated with the number of CIs at the PUMA level and also associated with immigrants' labor market performance, not necessarily related to Chinese workers. To gauge the potential relevance of this argument, in the third robustness check, we run a variation of the baseline specification, which takes into account a pure migrant sample. The estimation results are depicted in columns (2) – (4) in table C.5 in the appendix, for comparison we report our baseline results in column (1).

We first apply all other immigrant workers who are not Chinese-born as the reference group and compare them with Chinese workers. The estimation results are depicted in column (2) of table C.5. We find that the Chinese earn about 4 percent less than other immigrants in the United States. The number of existing CIs within 50 km has a similar U-shaped effect on Chinese migrants relative to other migrants as to natives presented before – though it is overall smaller in magnitude, which is plausible given the lower absolute difference in wages of Chinese relative to other migrants as when compared to natives. Again, the turning point where the CI-induced wage gap is largest is at roughly 6 CIs within a vicinity of 50 km.

Second, instead of looking at all other immigrants, we now focus on East Asian immigrants, who are culturally closer to Chinese (and may, for many Americans, appear ethnically similar to Chinese), and employ them as the reference group. The estimation results depicted in column (2) of table C.5 suggest that the number of CIs is unrelated to the wage gap between Chinese and other East Asian immigrants in the United States, which gives a hint that East Asian immigrants may also be influenced by the establishment of CIs. Furthermore, this result is supportive of the mechanism of discrimination we hypothesize, according to which CIs negatively affect Chinese-appearing individuals.

Finally, we use European immigrants as the reference group and re-estimate the model. Column (3) of table C.5 shows similar result patterns as the main findings. The U-shaped relationship between the wage gap and the number of nearby CIs is also evident here.

4.5.4 Spillover effects

We found that CIs matter for Chinese immigrants' wage outcomes relative to natives, but also when compared to European immigrants' wages in the United States. We found no negative CI effect relative to other East Asian migrants. We interpret these findings as potential underlying spillover effects: CIs may not only have a detrimental effect on Chinese immigrants' wages but also on other migrants' wages with a somewhat similar ethnocentric appearance or culture as the Chinese – in particular other East Asian individuals. Moreover, if CIs have a negative effect on the attitudes natives project towards certain Asian minority groups in the United States, they may have a (relatively) positive effect on the attitudes that natives have towards other migrant groups. Accordingly, the relative status of Europeans within the universe of immigrant groups in the United

States may have also changed. Europeans now rank relatively higher and are perceived relatively more favorably, while the relative rank of Chinese and other East Asians has declined due to the CI openings.

To investigate whether this argument holds, we apply migrants without Chinese, East Asian migrants without Chinese, and European migrants as the potential treated groups in our model. The reference group still consists of natives. We document the regression results in table C.6 in the appendix; baseline specification results are reported in column (1) for easy comparison. Employing an estimation sample with natives and migrants without Chinese, we find in column (2) a negative impact of the number of CIs on the wage rate of other immigrants. However, the effect size is much smaller than the baseline results. Instead of observing all other groups of immigrants, we now observe East Asian migrants without Chinese and European migrants in columns (3) and (4), respectively. Our estimation results indicate that the number of CIs has a negative effect on East Asian migrants who (may) appear ethnocentrically and culturally similar to Chinese immigrants, and that this effect is also smaller than the one for Chinese immigrants. In contrast, we find European migrants to be even slightly positively affected by the number of CIs, supporting our theoretical foundation that employers have different attitudes towards different migrant groups, and that they rank migrant groups accordingly. Europeans thus seem to move to relatively higher ranks than Chinese and other Asians when CIs open up.

4.5.5 Heterogeneous effects

In the following, we explore the distinct relationships of CIs with Chinese immigrants' labor market outcomes when considering only subgroups of the population. Namely, we investigate the factors of gender and state political orientation.

Gender

First, we examine whether the negative impact of CIs on labor market outcomes differs between male and female Chinese immigrants. In general, gender is important to individual labor market outcomes since there are often gender-specific differences in terms of labor market participation rates, as well as in occupation and industry chosen. In the case of Chinese immigrants, females may also face different challenges in the labor market due to China-specific cultural norms and expectations regarding family structure and women's roles at home and in the labor market. As a result, the labor force participation rate for female Chinese immigrants may be lower than that of male Chinese immigrants, with implications for the outcomes we find (Antecol, 2000, Frank and Hou, 2016). Additionally, employer-based discrimination may be gender-specific, with female Chinese immigrants potentially facing greater barriers or biases in certain occupations or industries and lower ones in others (Constant and Massey, 2005). Although our baseline model takes into account gender, occupation and industry on the individual level, certain occupations or industries that are dominated by either sex may still discriminate against sexes differently, especially for migrants.

Table C.7 in the appendix presents the gender-specific results for the model examining the relevance of the number of nearby CIs. To facilitate comparison, we also report the results from the corresponding baseline model. The estimated coefficients on being Chinese (without having an open CI around) show substantial heterogeneity across sexes:

Male Chinese immigrants face a wage penalty of about 20 percent, while female Chinese immigrants face around 10 percent. Moreover, the number of nearby CIs has a relatively more pronounced effect on male Chinese immigrants. CIs' impact seems to be smaller for female Chinese immigrants, both relative to the male-only sample and to the baseline model. For both sexes, the wage gap peaks when around six CIs are open, consistent with the baseline model.

State political orientation

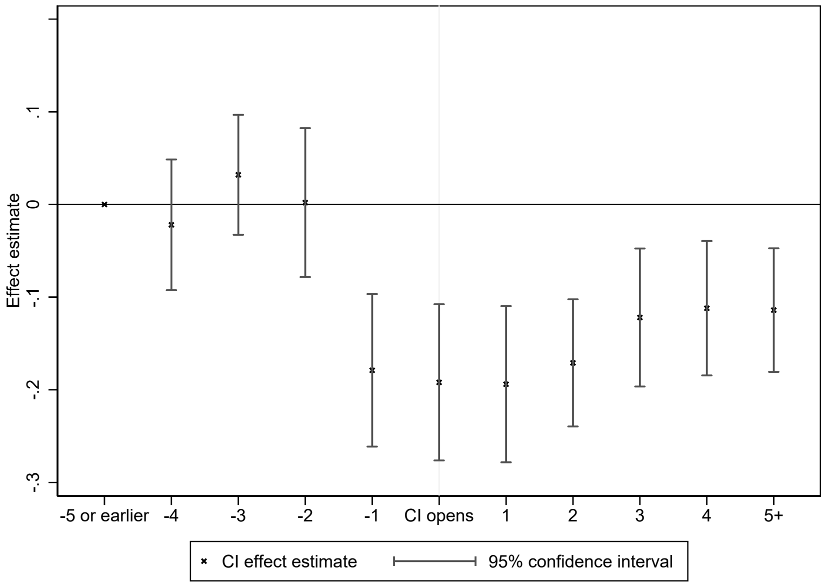
Second, we explore state political orientation as channelling a potentially heterogeneous impact CIs have on the labor market performance of Chinese immigrants across the United States. Specifically, we investigate whether the relationship between CIs and labor market outcomes differs between states that are governed by Democrats versus states that are governed by Republicans. A state's political orientation can have a significant impact on various policies dealing with the integration of immigrants, and it can mirror social dynamics, which may in turn influence labor market outcomes. For instance, US states governed by Democrats may have different approaches to education, immigration, and cultural exchange, which could potentially shape the effects of CIs on the labor market performance of Chinese immigrants differently than states governed by Republicans. Moreover, a state's political orientation may mirror different legacies in state attitudes towards migrants, and thus results may contribute to a more comprehensive understanding of the broader socio-political dynamics that shape the experiences of Chinese immigrants across different states in the United States.

We report our sub-sample results on Democrat-governed and Republican-governed states in table C.8 in the appendix. As evident from the table, the wage gap associated with being a Chinese immigrant (without having a CI nearby) is relatively lower in Democrat-governed states (Chinese immigrants suffer a wage penalty of about 13 percent rather than about 15 percent in the baseline model). When taking into account the impact of CIs, we find an interesting result: The negative relationship between CIs and Chinese immigrants' wages, as seen in the baseline model, is evident only in PUMA areas within Democrat-governed states, with the maximum effect size reached at around six open CIs. In contrast, the number of CIs appears largely unrelated to Chinese immigrants' wages in Republican-governed states (column (3)).

4.5.6 Event study

In the previous analysis, we only focused on the relationship between the existence or the number of CIs and the labor market performance of Chinese immigrants. The opening of a CI, including the location and the timing of the opening, is not purely exogenous. Although we have included several control variables at the individual, PUMA, and state levels to capture potential sources of endogeneity, it is still possible that some unobservables are correlated with the establishment of CIs and the wage gap, which could bias the causal impact. In order to investigate whether opening CIs has a causal effect on Chinese wages, we perform an event study in the following. The sample consists of individuals living in PUMAs where there was at least one open CI during the observation period. We estimate the model illustrated in equation (4.3). We plot the coefficients on the interaction terms ($\boldsymbol{\theta_3}$) in figure 4.1. The regression table with the exact estimation results can be found in table C.9 in the appendix.

Figure 4.1: CI opening – Event study graph



Note: This figure visualizes the main results from the event study regression with hourly wages as the outcome variable, i.e. the coefficients on the interaction term between CN_i and Γ^j_{pt} ("CI effect estimate"). The underlying regression output table for these results is documented in table C.9. The reference period is $j = -5$, i.e. five years (or earlier) before the first CI was opened. We only consider individuals living in a PUMA that is near at least one CI during our sampling period. Individual-level, PUMA-level, and state-level covariates are the same as reported in table 4.5. Standard errors are clustered at the PUMA level.

As evident from figure 4.1, a CI opening has a detrimental effect on the relative wage level of Chinese immigrants living within 50km of a CI, compared to natives. In the year of establishment ("CI opens"), the point estimate shows that Chinese workers suffer from a roughly 19 percent lower relative wage rate compared to the reference period (i.e. five years or earlier before a local CI opens). Furthermore, we find that the effect of opening a CI is persistent over time, although it appears to narrow as time progresses. Note that this is also in line with the result on models with lagged CI opening.

Strikingly, we report a statistically significant and negative effect on the relative wage level of Chinese immigrant workers in the year before a CI opens. We interpret this as an announcement effect, anticipating discrimination against Chinese workers due to the approaching, already announced opening of a CI (which most likely already triggers local media attention). Indeed, for the 50 CIs where we have sufficient information on both the opening and the announcement date, the announcement is on average roughly 9 months before the actual opening. As a subsequent analysis, we shift our focus to the announcement year rather than the opening year as the pivotal event. Due to data limitation, we only have information on the announcement of the aforementioned 50 CIs, subject to a total of 1,015,441 observations. The estimation results are depicted in column (2) of table C.9 and visualized in figure C.2. We find no significant coefficient on the dummy variable one year before the announcement, but we do for the event year and subsequent time periods. Our results tend to suggest that there seems to be a causal impact of the establishment of CIs on the immigrant-native wage gap of Chinese immigrants in the United States.

4.6 Conclusion

There exists an extensive body of literature examining immigrant-native wage gaps worldwide. Still, the drivers of these wage gaps and how they can be overcome is not fully solved. This paper investigates the immigrant-native wage gap by taking a different view than most other literature on the topic: We postulate that it is not only immigrant characteristics that matter in this context. Employers' decisions to hire certain immigrants (and on which terms) are subject to norms and prejudices prevalent in the host country's society. Accordingly, employers rank immigrant groups based on their origin countries and thus favor some migrant groups over others. Countries perceived as more distant (e.g. culturally) rank lower than more proximate countries, and this is reflected in the labor market performance of individuals originating from these countries.

In this paper, we study the influence that host-country language-learning facilities may have on individual labor market outcomes of immigrants. In particular, we investigate the labor market effects of the establishment of CIs on Chinese immigrants living nearby. We propose two theoretical channels through which the opening of CIs may have an effect on Chinese immigrants' labor market prospects: First, CIs may bridge cultural gaps between natives of the host country and Chinese immigrants by offering language and culture courses on contemporary China, thus increasing mutual understanding and making Chinese more trustworthy for labor contracts with American employers (indirectly also increasing their bargaining power). A second theoretical channel points in the exact opposite direction, namely that CIs may trigger attention and news coverage on CIs, but also on topics related to China and the Sino-American relationship. The intensifying China-United States rivalry and debates on CIs affecting academic freedom, coupled with accusations that CIs may be used for espionage and surveillance (among other rather negatively-framed topics), make CIs suspect for many, potentially creating and intensifying sentiments against Chinese immigrants in the nearby communities.

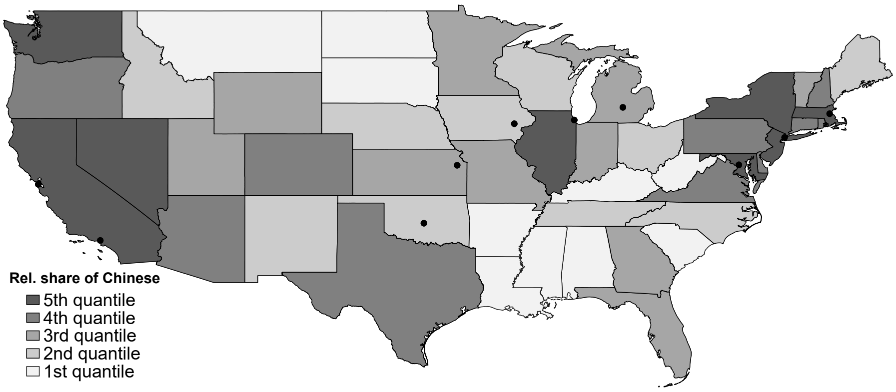
To investigate the relationship between CIs and labor market outcomes of Chinese immigrants, we propose several models that we estimate for the years 2005–2019. In our analysis, we utilize rich individual-level pooled cross-sectional data from the ACS, which we combine with data collected by ourselves on the existence of CIs across the United States. First, for individuals living in PUMAs without CIs we report an average wage gap of Chinese immigrants relative to natives of about 15 percent, a relatively stable finding throughout all our model specifications. Second, our analysis shows that the *existence of one or more CIs* nearby correlates with even lower wages of Chinese immigrants – no matter how recent or long ago the opening was and without regard to the effect strength of single CIs. In a separate model setup, we verify that the *number of nearby CIs* matters, too: The negative effect of CIs on labor market outcomes of Chinese adds up with each additional CI established locally, but the marginal effect per additional CI declines. We find six CIs to increase the wage gap to the largest extent, while every additional CI opened thereafter narrows the gap again. In general, all model specifications appear relatively stable to various robustness checks we run, including varying the distance the CI effect is measured over, lagged model specifications, and varying the reference group. While we find the opening of CIs to be relevant for the wage level of Chinese immigrants, we fail to report such an effect on their employment probability. Finally, an event study reveals that the presence of a local CI depresses Chinese immigrants' wages by 20 percent in the year of its opening. Furthermore, we find the effect of CIs on Chinese workers to last for years. Additionally, we detect a kind of *announcement effect* where the wages of Chinese immigrants already decline one year prior to the actual opening of a CI, coinciding with the public announcement of its establishment.

We also demonstrate that the negative correlation between wages of Chinese immigrants and nearby CIs is not exclusive to them: Other East Asian migrant groups exhibit a similar pattern, although the magnitude of the effect is somewhat weaker. In contrast, European migrants appear to experience a positive impact. Both these findings support our theoretical framework suggesting that employers may rank migrant groups. East Asian migrant groups, on the one hand, face relative disadvantages due to the opening of CIs, while Europeans tend to achieve higher relative wage positions. Investigating effect heterogeneity, we uncover that CI openings have a relatively more pronounced effect for male Chinese immigrants than females. Moreover, the political orientation of the state in which an individual lives plays a significant role. In Republican-governed states, we find no significant discernible effect of CIs on Chinese immigrants, whereas Democrat-governed states demonstrate a substantial and distinctive reduction in wages for Chinese immigrants due to the presence of local CIs.

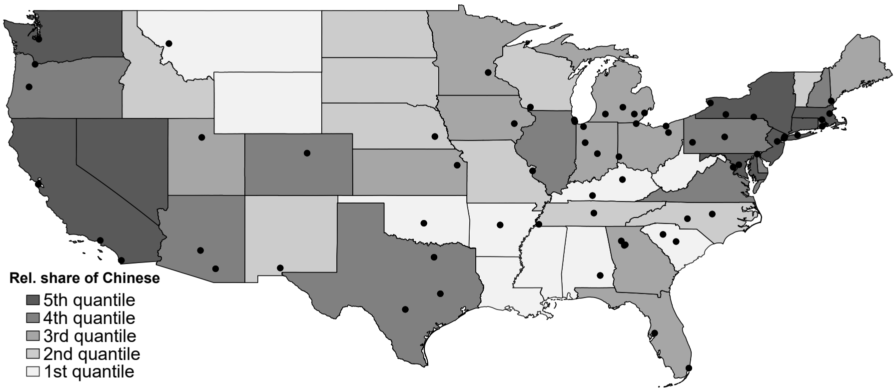
While our paper finds substantial support for the idea that CIs correlate negatively with wages achieved by Chinese immigrants in the United States, we fail to provide final evidence for the actual causal link underlying this finding. One primary challenge in our study is the difficulty in accurately determining the influence of certain CIs on their local environment. CIs exhibit differences in terms of size, budget, teacher quality, student enrollment, potential targetability of the surrounding population, and numerous other (unobservable) factors. Moreover, we are unable to distinguish the proposed positive and negative causal channels in our analysis, given that it is likely that the actual causal effect for our findings is a mixture of both components (and may vary across locations). This also means that we cannot rule out that there are other (local) factors relevant in this context that we may fail to adjust for, which may, in the worst case, drive both wages and the decision regarding where an institute is set up. Future research could directly address this issue. In particular, we think that a text analysis of local news media reports could be a feasible way to identify and understand the impact of CIs. It would also be worthwhile to gain a better understanding of social media in this context, which could act as a multiplier of local populations' attitudes towards Chinese immigrants, and could also play a function in moderating actual local sentiments towards foreigners – raising preexisting negative perceptions of foreign nationals, but perhaps also elevating public awareness of race-motivated discrimination in the labor market and triggering solutions how to overcome such.

4.7 Appendix C

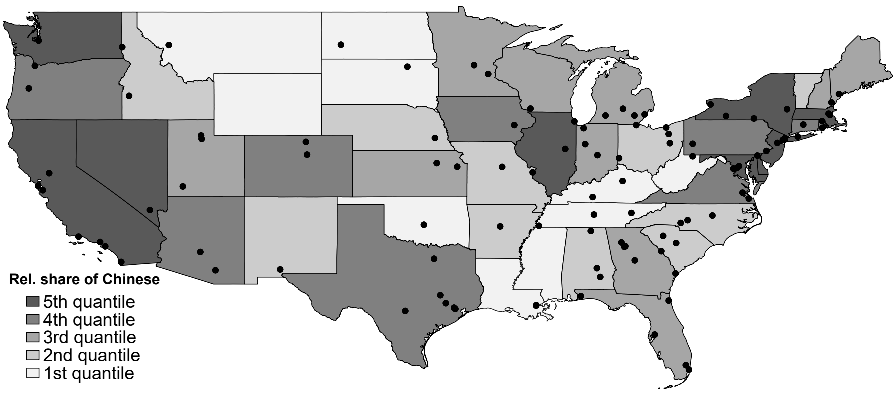
Figure C.1: Existing CIs in the United States



(a) **2006**



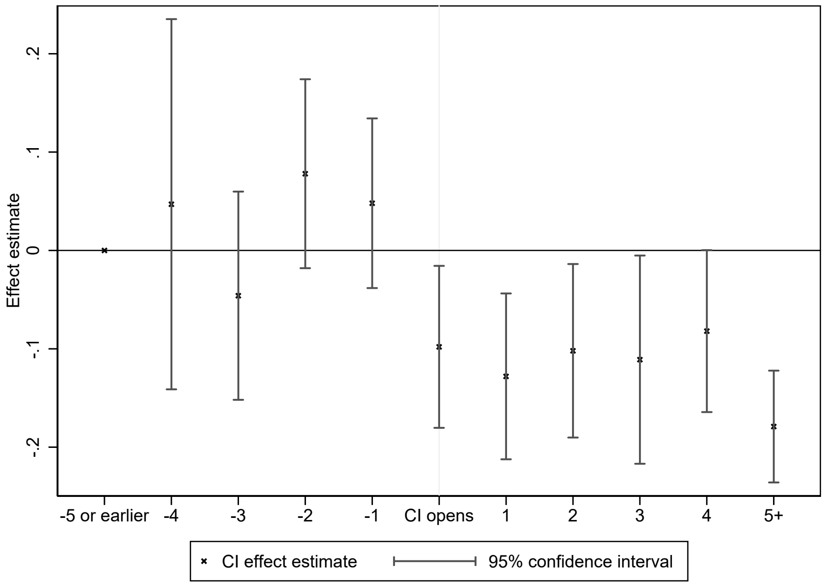
(b) **2010**



(c) **2016**

Source: Own elaboration using IPUMS-published ACS data and data retrieved from `english.hanban.org`. The shading of the states denotes the relative share of Chinese in each state (categorized in quantiles – darkest shade = state with relatively the highest share of Chinese immigrants).

Figure C.2: CI announcement – Event study graph



Note: This figure visualizes the main results from the event study regression on the announcement of a CI to be opened, with hourly wages as the outcome variable, i.e. the coefficients on the interaction term between CN_i and Γ_{pt}^{j} ("CI announcement effect estimate"). The underlying regression output table for these results is documented in column (2) of table C.9. The reference period is $j = -5$, i.e. five years before the CI was announced. We only consider individuals living in a PUMA that is near at least one CI during our sampling period, and where we have sufficient data on the announcement of the CI. Individual-level, PUMA-level, and state-level covariates are the same as reported in table 4.5. Standard errors are clustered at the PUMA level.

Table C.1: Individual-level summary statistics (*employment sample*)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All observations				Natives only		Chinese only		t-Test
	mean	sd	min	max	mean	sd	mean	sd	(col. 7 − col. 5)
Chinese immigrant	0.013	0.113	0.000	1.000					
Employment	0.956	0.206	0.000	1.000	0.955	0.206	0.971	0.168	0.015***
Log hourly wage	2.833	0.769	-7.237	10.604	2.834	0.767	2.814	0.858	-0.019***
Age	39.464	12.891	18.000	64.000	39.438	12.906	41.454	11.548	2.017***
Male	0.505	0.500	0.000	1.000	0.505	0.500	0.489	0.500	-0.016***
Education *(3 ISCED categories)*	2.594	0.524	1.000	3.000	2.597	0.520	2.401	0.749	-0.196***
Married	0.486	0.500	0.000	1.000	0.483	0.500	0.698	0.459	0.215***
# of children	0.731	1.064	0.000	9.000	0.730	1.065	0.862	0.973	0.132***
Urban dummy	0.847	0.360	0.000	1.000	0.845	0.362	0.990	0.101	0.145***
Hours worked	39.340	11.626	1.000	99.000	39.345	11.621	38.960	12.035	-0.385***
# of observations	4,566,732				4,511,173		55,559		

Note: This table shows individual-level descriptive statistics using weighting factors (analytical weighting used to obtain nationally representative statistics) for the employment sample. We document respective values for the entire sample in columns (1)–(4), natives in columns (5)–(6), and Chinese immigrants in columns (7)–(8). Column (9) shows the difference in means between columns (5) and (7). The hourly wage is in USD. All values are rounded to the third decimal place.

Table C.2: Linear models with categorical dummies for the number of CIs nearby

VARIABLES	(1) ln(hourly wage)	(2) Employment (0/1)
CN Immigrant	-0.148***	0.006***
	(0.022)	(0.002)
CN immigrant $\times$ (# of CI^{50} $= 1$)	-0.074***	0.002
	(0.019)	(0.003)
CN immigrant $\times$ (# of CI^{50} $= 2$)	-0.111***	-0.004
	(0.030)	(0.003)
CN immigrant $\times$ (# of CI^{50} $= 3$)	-0.056	0.005
	(0.035)	(0.005)
CN immigrant $\times$ (# of CI^{50} $= 4$)	-0.358***	0.005
	(0.046)	(0.010)
CN immigrant $\times$ (# of CI^{50} $= 5$)	-0.221***	0.009***
	(0.037)	(0.003)
CN immigrant $\times$ (# of CI^{50} $= 6$)	-0.223***	0.001
	(0.035)	(0.003)
CN immigrant $\times$ (# of CI^{50} $= 7$)	-0.165***	0.004
	(0.028)	(0.009)
CN immigrant $\times$ (# of CI^{50} $= 8$)	-0.174***	0.000
	(0.031)	(0.004)
CN immigrant $\times$ (# of CI^{50} $= 9$)	-0.152***	-0.001
	(0.044)	(0.005)
Model specifications		
Individual level controls	YES	YES
PUMA level controls	YES	YES
State level controls	YES	YES
PUMA FE	YES	YES
Year dummies	YES	YES
State time trend	YES	YES
Observations	4,376,563	4,566,732
R-squared	0.382	0.031

Note: The dependent variable in column (1) is the logarithm of hourly wage, whereas in column (2) it is individual employment outcome. We only observe natives and Chinese immigrants living in constant PUMAs. Individual-level, PUMA-level and state-level covariates are the same as reported in table 4.5. Standard errors clustered at the PUMA level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: Robustness check – Varying CIs' influence range

VARIABLES	(1) $k = 25$ km	(2) $k = 50$ km	(3) $k = 100$ km
CN Immigrant	-0.146***	-0.149***	-0.163***
	(0.022)	(0.024)	(0.026)
CN Immigrant × # of CI^k	-0.114***	-0.072***	-0.039***
	(0.011)	(0.010)	(0.008)
CN Immigrant × (# of CI^k)^2	0.012***	0.006***	0.002***
	(0.001)	(0.001)	(0.001)
Model specifications			
Individual level controls	YES	YES	YES
PUMA level controls	YES	YES	YES
State level controls	YES	YES	YES
PUMA FE	YES	YES	YES
Year dummies	YES	YES	YES
State time trend	YES	YES	YES
Observations	4,376,563	4,376,563	4,376,563
R-squared	0.382	0.382	0.382

Note: The dependent variable is the logarithm of hourly wage. Models vary the vicinity for counting the number of open CIs: 25, 50, and 100 km radius. We only observe natives and Chinese immigrants living in constant PUMAs. Individual-level, PUMA-level and state-level covariates are the same as reported in table 4.5. Standard errors clustered at the PUMA level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.4: Robustness check – Lagged local CI counts

VARIABLES	(1) current period	(2) 1-year lag	(3) 2-year lag
CN Immigrant	-0.149***	-0.180***	-0.203***
	(0.024)	(0.026)	(0.028)
CN Immigrant × # of CI^{50}	-0.072***		
	(0.010)		
CN Immigrant × (# of CI^{50})^2	0.006***		
	(0.001)		
CN Immigrant × # of CI^{50} [Lag1]		-0.049***	
		(0.010)	
CN Immigrant × (# of CI^{50} [Lag1])^2		0.004***	
		(0.001)	
CN Immigrant × # of CI^{50} [Lag2]			-0.029***
			(0.011)
CN Immigrant × (# of CI^{50} [Lag2])^2			0.002
			(0.002)
Model specifications			
Individual level controls	YES	YES	YES
PUMA level controls	YES	YES	YES
State level controls	YES	YES	YES
PUMA FE	YES	YES	YES
Year dummies	YES	YES	YES
State time trend	YES	YES	YES
Observations	4,376,563	4,376,563	4,376,563
R-squared	0.382	0.382	0.382

Note: This table shows robustness checks by applying the lagged value of CI numbers. The baseline specification is depicted in column (1). Columns (2) – (3) show results for specifications with the CI count variable lagged by either 1 or 2 years. The dependent variable is the logarithm of hourly wage. We only observe natives and Chinese immigrants living in constant PUMAs. Individual-level, PUMA-level and state-level covariates are the same as reported in table 4.5. Standard errors clustered at the PUMA level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.5: Robustness check – Other immigrants as reference group

VARIABLES	(1) Baseline (*reference: natives*)	(2) migrants w/o Chinese	(3) East Asian migrants w/o Chinese	(4) European migrants
CN immigrant	-0.149***	-0.039***	0.006	-0.121***
	(0.024)	(0.012)	(0.013)	(0.013)
CN immigrant × # of CI^{50}	-0.071***	-0.023***	-0.012	-0.057***
	(0.010)	(0.008)	(0.011)	(0.008)
CN immigrant × (# of $CI^{50})^2$	0.006***	0.002*	0.001	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
Model specifications				
Individual level controls	YES	YES	YES	YES
PUMA level controls	YES	YES	YES	YES
State level controls	YES	YES	YES	YES
PUMA FE	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES
State time trend	YES	YES	YES	YES
Observations	4,376,563	889,532	86,364	127,565
R-squared	0.382	0.373	0.412	0.431

Note: This table shows robustness checks using other immigrant groups as the reference category (*rather than natives*). The baseline specification is depicted in column (1). Column (2) documents results when the reference group is all other immigrants (except Chinese-born). The specification in column (3) uses East Asian migrants without Chinese as the reference group. In column (4) we consider European migrants as the reference group. The dependent variable is the logarithm of hourly wage. Individual-level, PUMA-level and state-level covariates are the same as reported in table 4.5. Standard errors clustered at the PUMA level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.6: Spillover effects

VARIABLES	(1) Baseline (*treatment: Chinese*)	(2) migrants w/o Chinese	(3) East Asian migrants w/o Chinese	(4) European migrants
Immigrant	-0.149***	-0.130***	-0.177***	0.069***
	(0.024)	(0.004)	(0.019)	(0.008)
Immigrant × # of CI^{50}	-0.072***	-0.029***	-0.036***	0.014**
	(0.010)	(0.003)	(0.009)	(0.005)
Immigrant × (# of $\text{CI}^{50})^2$	0.006***	0.003***	0.004***	-0.001
	(0.001)	(0.000)	(0.001)	(0.001)
Model specifications				
Individual level controls	YES	YES	YES	YES
PUMA level controls	YES	YES	YES	YES
State level controls	YES	YES	YES	YES
PUMA FE	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES
State time trend	YES	YES	YES	YES
Observations	4,376,563	5,158,191	4,355,023	4,396,224
R-squared	0.382	0.377	0.382	0.384

Note: This table reports spillover effects of CIs on migrant groups other than Chinese-born. We document the results of the baseline model (all immigrants are Chinese-born) in column (1). Column 2 provides estimation results for a model considering all migrants except for Chinese-born. The specification in column (3) uses East Asian migrants (without Chinese) only. In column (4) we consider exclusively European migrants as immigrants. The dependent variable is the logarithm of hourly wage. Individual-level, PUMA-level and state-level covariates are the same as reported in table 4.5. Standard errors clustered at the PUMA level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.7: Heterogeneous effects – Gender

VARIABLES	(1) Baseline	(2) male only	(3) female only
CN Immigrant	-0.149***	-0.198***	-0.098***
	(0.024)	(0.030)	(0.023)
CN Immigrant $\times$ # of CI^{50}	-0.071***	-0.085***	-0.058***
	(0.010)	(0.012)	(0.011)
CN Immigrant $\times$ (# of CI^{50})^2	0.006***	0.007***	0.005***
	(0.001)	(0.001)	(0.001)
Model specifications			
Individual level controls	YES	YES	YES
PUMA level controls	YES	YES	YES
State level controls	YES	YES	YES
PUMA FE	YES	YES	YES
Year dummies	YES	YES	YES
State time trend	YES	YES	YES
Observations	4,376,563	2,170,895	2,205,668
R-squared	0.382	0.398	0.357

Note: This table shows heterogeneous effects based on gender. We depict the baseline model (including both males and females) in column (1). Columns (2) reports estimation results for a sample of males only. Columns (3) reports the estimation results for a sample of females only. The dependent variable is the logarithm of hourly wage. We only observe natives and Chinese immigrants living in constant PUMAs. Individual-level, PUMA-level and state-level covariates are the same as reported in table 4.5. Standard errors clustered at the PUMA level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.8: Heterogeneous effects – State political orientation

VARIABLES	(1) Baseline	(2) Current governor is Democrat	(3) Current governor is Republican
CN Immigrant	-0.149***	-0.129***	-0.155***
	(0.024)	(0.027)	(0.027)
CN Immigrant $\times$ # of CI^{50}	-0.071***	-0.099***	-0.027
	(0.010)	(0.012)	(0.021)
CN Immigrant $\times$ (# of CI^{50})^2	0.006***	0.009***	0.008***
	(0.001)	(0.001)	(0.003)
Model specifications			
Individual level controls	YES	YES	YES
PUMA level controls	YES	YES	YES
State level controls	YES	YES	YES
PUMA FE	YES	YES	YES
Year dummies	YES	YES	YES
State time trend	YES	YES	YES
Observations	4,376,563	2,381,641	1,994,922
R-squared	0.382	0.384	0.377

Note: This table shows heterogeneous effects based on a state's political orientation. We depict the baseline model (including both Democrats- and Republican governed states) in column (1). Columns (2) reports the estimation results when considering only individuals living in states where the current governor is Democratic (i.e. states where the Democrats won the last state elections). Columns (3) reports the estimation results for individuals living in states where the current governor is Republican. The dependent variable is the logarithm of hourly wage. We only observe natives and Chinese immigrants living in constant PUMAs. Individual-level, PUMA-level and state-level covariates are the same as reported in table 4.5. Standard errors clustered at the PUMA level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.9: Event study estimates

VARIABLES	(1) CI opening	(2) CI Announcement
CN immigrant	-0.144***	-0.135***
	(0.030)	(0.015)
CN immigrant × Γ^{-5} (or earlier) [REF. CAT.]	(*reference*)	(*reference*)
CN immigrant × Γ^{-4}	-0.022	0.047
	(0.036)	(0.096)
CN immigrant × Γ^{-3}	0.032	-0.046
	(0.033)	(0.054)
CN immigrant × Γ^{-2}	0.002	0.078
	(0.041)	(0.049)
CN immigrant × Γ^{-1}	-0.179***	0.048
	(0.042)	(0.044)
CN immigrant × CI event (Γ^{0})	-0.192***	-0.098**
	(0.043)	(0.042)
CN immigrant × Γ^{1}	-0.194***	-0.128***
	(0.043)	(0.043)
CN immigrant × Γ^{2}	-0.171***	-0.102**
	(0.035)	(0.045)
CN immigrant × Γ^{3}	-0.122***	-0.111**
	(0.038)	(0.054)
CN immigrant × Γ^{4}	-0.112***	-0.082**
	(0.037)	(0.042)
CN immigrant × Γ^{5} (or later)	-0.114***	-0.179***
	(0.034)	(0.029)
Model specifications		
Individual level controls	YES	YES
PUMA level controls	YES	YES
State level controls	YES	YES
PUMA FE	YES	YES
Year dummies	YES	YES
State time trend	YES	YES
Observations	2,303,311	1,015,441
R-squared	0.387	0.393

Note: This table shows the estimation results on our event study model specifications, which are also visualized in figures 4.1 (in the main text) and C.2 (in the appendix). Column (1) reports the result for the event study when evaluating CI *openings* as event. The model in column (2) evaluates the *announcement* of CIs as event. The dependent variable is the logarithm of hourly wage. We only observe natives and Chinese immigrants living in constant PUMAs where at least one CI opened during our sampling time. We only consider the first locally established CI, and, in the model reported in column (2) only, where accurate information on the announcement date of the respective CI is available. Individual-level, PUMA-level and state-level covariates are the same as reported in table 4.5. Standard errors clustered at the PUMA level are depicted in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Chapter 5

Automatable jobs and well-being in Germany*

Technological advancements continuously reshape the world of work, altering or entirely replacing human tasks and job roles. Yet, few studies explored whether working in an automatable job impacts the life satisfaction of affected workers. We examine this relationship in Germany, a nation with a manufacturing-intensive economy significantly affected by technological change. Using German Socio-Economic Panel (SOEP) data and Autor and Dorn's (2013) measure of job automatability, we classify the top third of occupations with the highest potential for automation as 'automatable'. Our analysis yields several key findings: First, workers in automatable jobs report lower life satisfaction in Germany. Second, the definition of the reference category is crucial for this outcome. We reveal a non-linear relationship where workers in occupations with a moderate degree of automatability show a positive association with life satisfaction. Accounting for this factor, the link between job automatability and life satisfaction becomes less straightforward. Finally, we find the negative association between working in an automatable job and life satisfaction being especially pronounced among blue-collar and younger workers, a pattern consistent across different reference categories.

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5.1 Introduction

Advancements in automation, information technology, and robotics are fundamentally changing the world of work (e.g., Brynjolfsson and McAfee, 2014, Autor et al., 2022). Technological progress brings about efficiency gains and increased productivity, as well as relief from work-related burdens: machines assist in lifting heavy items, enhancing safety and health; automation of repetitive tasks reduces monotony and physical strain; computerization facilitates remote work and flexible schedules. However, automation may also replace the human factor in many tasks. Consequently, widespread concerns about job displacement accompany workplace automation (Autor, 2015, Blien et al., 2021). Several research suggests that almost any job has the potential for partial automation in the future, and technology could replace up to 47 percent of employment in OECD countries within a decade (Frey and Osborne, 2017, Josten and Lordan, 2020).

Manual and low-skilled workers particularly fear job loss due to workplace automation (e.g., Dekker et al., 2017). Since the fear of job loss is negatively associated with life satisfaction (e.g., De Witte, 1999, Luechinger et al., 2010), workplace automation may similarly lower the overall well-being of workers. Additionally, automation-affected workers may experience reduced wage dynamics in their jobs (Acemoglu and Restrepo, 2020), making them relatively worse off over time. Various studies have linked workplace automation to reduced physical (e.g., Nazareno and Schiff, 2021) and mental health of workers (e.g., Abeliansky et al., 2021). However, automation is also associated with fewer physically intense tasks and fewer workplace-related injuries, ultimately increasing the physical health of workers (Gunadi and Ryu, 2021, Gihleb et al., 2022). Therefore, the net effect of working in an automatable job on life satisfaction remains ambiguous. To the best of our knowledge, only two studies have specifically investigated the association between a job's automatability potential and life satisfaction. Lordan and Stringer (2022) use Autor and Dorn (2013)'s job automation measure and report ambiguous results for their aggregate sample of Australian workers, with the relationship varying across industry, age, sex, and education. Giuntella et al. (2023) demonstrate that workers in Germany exposed to a specific type of automation technology, artificial intelligence (AI), report more concerns about their job security and personal economic situation, ultimately leading to reduced job- and life satisfaction.

In this study, we use data from the German Socio-Economic Panel (SOEP) from 1984 to 2020 to add several new aspects to the existing literature. First, we examine the association between working in an automatable job[106] and life satisfaction among German workers. Germany is the most automation-affected country in Europe (Lordan, 2018); manufacturing and automotive are central sectors of the German economy.

Second, our approach extends beyond a simple utilization of Autor and Dorn (2013)'s job automatability measure to assess its relevance in explaining individual well-being. We aim to explore its explanatory power by testing alternative specifications of the measure, considering that it is rooted in the relative occupational routine task intensity (RTI) of jobs. Higher RTI scores indicate occupations more susceptible to automation by technologies such as computers, robots, and AI. While Autor and Dorn (2013) categorize the top third of occupations by RTI as 'automatable' using a straightforward dummy variable, we refine this categorization to capture a more nuanced understanding of automation's

[106]Throughout the text, we refer to 'automatable jobs' as those occupations with the highest susceptibility to future workplace automation through technological means. Refer to section 5.3.1 for our exact measure.

impact on well-being. Specifically, we maintain the top third as 'most automatable' jobs, introduce the middle third as 'semi-automatable' jobs, and designate the lowest third of occupations by RTI as 'least-automatable' jobs. Surprisingly, our analysis reveals that workers in semi-automatable jobs exhibit significantly higher life satisfaction scores, while those in most-automatable and least-automatable jobs report similar satisfaction levels. This result proves robust across various alternative setups of the measure.

Third, we explore the underlying characteristics behind our baseline finding and the observed reference category effect. We identify a significant and relatively large negative association between working in an automatable job and life satisfaction particularly for blue-collar and younger workers. Interestingly, we find no heterogeneity in these terms when assessing the relationship using the semi-automatable jobs category. Moreover, it is noteworthy that we do not observe heterogeneity due factors like gender, migration background, skill level, or public-sector employment.

The remainder of this paper is structured as follows: Section 5.2 summarizes the literature on well-being in the context of workplace automation. In section 5.3, we discuss our empirical strategy, explain how we measure job automatability and the covariates of interest in our study, and present our data sources. Section 5.4 presents our findings. Finally, the last section offers a summary of the results, a discussion, and reviews avenues for future research.

5.2 Literature review

In this chapter, we explore the literature on the implications of technological change for the labor market, focusing on three main areas. First, we review foundational literature on the influence of workplace automation on the workforce, including methodologies for measuring occupation-level automation risk. Next, we examine research highlighting the significant impact of job insecurity on individual well-being, with workplace automation identified as a major contributor to perceived job insecurity. Finally, we discuss literature that explores the complex relationship between a job's automatability potential, health, and various measures of well-being.[107]

5.2.1 Technological change and its impact on the workforce

Technological change often enhances productivity, yet the prospect of worker displacement by machines has garnered widespread attention (see, for instance, Autor, 2015, Graetz and Michaels, 2018, Acemoglu and Restrepo, 2018, 2020, Blien et al., 2021, Dauth et al., 2021). Early research into the effects of technical innovations suggested that high-skill jobs were typically complemented by technological advancements, while low-skilled jobs were more susceptible to substitution (e.g., Katz and Murphy, 1992, Acemoglu, 2002, Goldin and Katz, 2008), a theoretical foundation known as the skill-biased technological change (SBTC) hypothesis (Johnson, 1997). However, from the mid-2000s onwards, a more nuanced perspective emerged, today summarized as the *routine-biased technological change* (RBTC) hypothesis (a term coined by Goos et al., 2014). It posits that technological change and automation not only affect low-skill jobs

[107]Well-being encompasses diverse dimensions and can thus be assessed through multiple metrics. Researchers commonly interpret well-being as an evaluation of an individual's quality of life or specific life domains, such as one's job or health. The emphasis is often on self-reported (i.e. subjective) life satisfaction as the central, overall indicator of well-being in the social sciences (Ryff and Keyes, 1995).

but also target job roles characterized by a significant proportion of routine cognitive and manual tasks (Autor et al., 2003, Acemoglu and Autor, 2011, Autor and Dorn, 2013). Conversely, certain types of jobs, particularly those involving non-routine cognitive, analytical, and interactive tasks, tend to complement most technological innovations (Spitz-Oener, 2006).

Theoretically, occupations characterized by high RTI can be succinctly delineated using computer code, rendering them amendable to automation via computer software, robots, and similar technologies. Pioneering research by Autor et al. (2003), Autor and Dorn (2013) and Autor (2015) utilized occupational characteristics as outlined in the *Dictionary of Occupational Titles*. This resource provides detailed information on diverse job profiles, enabling the development of a measure that gauges the relative RTI of a job.[108] Testing the RBTC hypothesis with this data, they offered empirical evidence that between 1980 and 2005, occupations with high RTI scores experienced, on average, a significantly greater loss of jobs than occupations with low RTI scores. Moreover, they demonstrated that it was primarily middle-skilled jobs with relatively high RTI scores – offering a compelling explanation for the phenomenon of job polarization in the United States. Their RTI indicator has been applied to analyze labor markets in other countries, including Germany, yielding similar results (see, for instance, Goos et al. (2014) for an analysis of several European countries, Coelli and Borland (2016) for Australia, and Yuhong and Xiahai (2020) for China). For comprehensive literature reviews on measuring workplace automation and identifying occupations at high risk of replacement (or complementation) by technology, refer to Calvino and Virgillito (2018) and Mondolo (2022).[109]

5.2.2 Workplace automation and job insecurity

There exists an extensive body of literature linking unemployment with adverse effects on several indicators of individual well-being.[110] Empirical estimates suggest that unemployed individuals tend to exhibit overall life satisfaction scores of up to 15 percent lower than their employed counterparts (e.g. Di Tella et al., 2001, Helliwell, 2003, Stutzer, 2004). Moreover, it is established that *job insecurity* itself detrimentally affects individual well-being.[111] Job insecurity is associated with lower life satisfaction (Di Tella et al., 2001, Wolfers, 2003, Luechinger et al., 2010, Green, 2011, Blanchflower et al., 2014), reduced job satisfaction (Ashford et al., 1989, Sverke et al., 2002, Clark, 2010, Chadi and Hetschko, 2016), and worse health outcomes, such as mental illnesses (Reichert and Tauchmann, 2017) and physical complaints (Green, 2011, Caroli and Godard, 2016, De Witte et al., 2016).[112] The rationale behind these findings is that job insecurity is a stress factor, mainly determined by reduced work locus of control and increased uncer-

[108] See section 5.3 for a precise description of their RTI measure.

[109] Table D.1 in the appendix provides an overview of several alternative automation measures that have been used in the economics literature.

[110] Seminal work in this area is credited to (Clark and Oswald, 1994). See Frey and Stutzer (2002) and Dolan et al. (2008) for comprehensive literature reviews.

[111] Seminal work by Greenhalgh and Rosenblatt (1984). De Witte et al. (2016) provide an extensive literature overview on this topic.

[112] There is literature addressing potential issues related to reverse causality in this context, including studies by Winkelmann and Winkelmann (1998), Marks and Fleming (1999), and De Witte et al. (2016), which suggest that reverse causality is rather unlikely.

tainty over the future work life, societal status and expected income. These factors lead to increased worrying and also facilitate additional work strain (Cheng and Chan, 2008).

Jobs characterized by a high number of potentially automatable tasks, as indicated by a relatively high RTI score, can contribute to increased job insecurity for individuals. Workers in such jobs may fear being replaced by automated systems or technologies, resulting in concerns about the long-term stability and sustainability of their employment. This can create a sense of insecurity and vulnerability that is outside their own locus of control (Benach et al., 2014, Frey and Osborne, 2017).

However, the impact of job insecurity on well-being varies across individuals, depending on factors such as gender (De Witte, 1999), age (Cheng and Chan, 2008), skill level (Schwabe and Castellacci, 2020) and employability (Green, 2011), as well as an individual's cultural and migration background (Tian et al., 2018) and the type of work contract (Kompier et al., 2009, Chadi and Hetschko, 2016). Occupation and occupational status also play a role; blue-collar workers have been found to suffer substantially more from job insecurity than do white-collar workers (Sverke et al., 2002). Moreover, local economic conditions, which proxy the likelihood of finding alternative employment, can buffer the impact of job insecurity on well-being (Shoss, 2017). Similarly, the relationship varies across countries due to differences in welfare regimes, employment protection legislation, trade union density, and other country-specific labor market features (Benach et al., 2014).

Indeed, Dekker et al. (2017) document heterogeneity in individual fear of automation along these lines. Their study, encompassing 27 European countries, highlights that blue-collar and low-educated workers, unemployed individuals, and female workers exhibit the highest levels of fear regarding workplace automation. Conversely, individuals accustomed to using new technologies at work tend to display lower levels of apprehension toward workplace automation. McClure (2018) reports a similar finding among American workers, emphasizing the heightened anxiety about job insecurity stemming from technological change among marginalized groups. Additionally, Brougham and Haar (2018) demonstrate that increased fear of workplace automation is linked to decreased organizational commitment and career satisfaction, while showing positive correlations with turnover intentions and depression.

5.2.3 Workplace automation and well-being

The impact of workplace automation on well-being spans various dimensions and has recently garnered significant research interest. Aghion et al. (2016) established a theoretical link between technological advancements in the workplace and individual well-being. Their model suggests that while technological disruptions threaten existing jobs, they also create new ones. Consequently, the adoption of new technologies by local firms influences local job turnover, job creation, and job destruction rates, thereby directly and indirectly affecting individuals' life satisfaction within the community. Positive implications for workers' life satisfaction prevail if technological disruptions are mitigated by welfare regime features that sufficiently protect the incomes of potentially affected workers, with Aghion et al. particularly highlighting the moderating function of a generous unemployment benefits system.

Empirical macro-level analyses have investigated the association between certain work-related automation dynamics and local population well-being metrics. For instance, Patel et al. (2018) found for the United States a correlation between county-level job automation risk (assessed by the significance of computerization-affected sectors for the local labor market) and declines in general, physical, and mental health. Similarly, O'Brien et al. (2022) reported that a higher robot penetration in United States commuting zones is associated with higher regional mortality rates, predominantly in mental health-related causes (drugs and suicides, but also cardiovascular diseases). However, Gunadi and Ryu (2021) argued that industrial robotization leads to less physically demanding tasks for the workers concerned. They found that a higher local share of robots per worker is positively related to the general health of low-skilled populations in American cities. Likewise, Gihleb et al. (2022) found a link between the introduction of robots in production processes and local workers' safety and physical health in both the United States and Germany, while also noting a negative association with mental health in the United States (but not in Germany).

At the individual level, numerous studies have particularly examined how the *fear of automation* affects various metrics of worker well-being. According to Schwabe and Castellacci (2020), workers who fear automation (about 40 percent of individuals in their Norwegian sample of industrial workers), particularly those in low-skill roles, report lower job satisfaction. Similarly, Hinks (2021) reports that fear of robots is correlated with lower reported life satisfaction across several countries in Europe, including Germany. The fear of potentially being substituted by automation technology also manifests in sleep disorders among South Korean workers, particularly affecting younger individuals (Baek et al., 2022). Additionally, Blasco et al. (2024) identify a link between French workers' exposure to the (self-assessed) risk of their job being automated and mental health issues. They find that fear of job loss, fear of changing qualification demands, and fear of involuntary internal transfers are indicative drivers for this result.

Other research has specifically investigated the effects of specific technologies on individual worker well-being. Gorny and Woodard (2020) found a negative relationship between occupational computerization risk and job satisfaction across data sets from America, Europe, and Australia. In a study by Nazareno and Schiff (2021), workers in computerization-prone jobs in the United States reported reduced job satisfaction and worse overall health, findings echoed by Liu (2023). However, Nazareno and Schiff also observed that affected individuals experienced lower overall stress levels. Jacobs et al. (2023) reported a similar result on the impact of computerization in a sample of European workers, attributing their findings to occupational tasks associated specifically with creativity, which positively impacted both job security and job satisfaction. Furthermore, Abeliansky et al. (2021) discovered a link between sectoral robot intensities and decreases in worker mental health, particularly pronounced among younger workers, the middle-skilled, and men. In contrast, Liu et al. (2024) reported that robotization in China was positively associated with the physical health of young and less educated workers in their sample, but also negatively associated with the mental health of older and less-educated workers.

Few studies investigate the relationship between workplace technological change and workers' life satisfaction. Giuntella et al. (2023) evaluate the well-being of individuals exposed to AI at their workplace in Germany. They observe heightened concerns about job security and personal economic situations among these individuals, along with reports of lower job and life satisfaction. Notably, they find no evidence of increased mental health issues, such as anxiety or depression symptoms, associated with this exposure. However, Lordan and Stringer (2022), using Autor and Dorn (2013)'s susceptibility to job automation measure, report an ambiguous relationship between working in an automatable job and workers' mental well-being and life satisfaction, based on an Australian sample of workers. They observe modest adverse associations, particularly in manufacturing industries, but find no significant socio-demographic variations that might explain these outcomes.

While existing research has provided valuable insights into the impact of specific technological disruptions and the fear of automation on worker well-being, a notable gap remains in understanding the broader, overall impact of technological change on well-being. Previous studies have primarily focused on specific technologies like computerization or AI, or targeted particular regions with distinct economic structures that may be relatively less affected by employment-threatening automation, such as the service-oriented Australian workforce. These studies may not capture the full spectrum of workplace automation's impact on different sectors and demographic groups. To address this gap, our study leverages the general job automatability measure by Autor and Dorn (2013) within the unique context of Germany, a nation characterized by a significant share of manufacturing jobs and a diverse range of occupations. This approach allows us to investigate not only the overall relationship between working in a potentially automatable job and life satisfaction but may also enhance our understanding of how this relationship varies across different types of jobs, industries, and demographic groups.

Drawing on the foundational literature and the empirical findings reviewed above, we formulate the following hypotheses for our study: First, we hypothesize that working in an automatable job is significantly negatively associated with life satisfaction in Germany. Given the country's industrial composition, we expect that the prevalence of automation-affected workers is more pronounced in Germany than in many other countries, especially compared to the more service-oriented economies evaluated in the literature so far. Thus, individuals identified as working in automatable occupations are likely to be more severely affected by automation in Germany, which facilitates identification. Secondly, following this line of argument further, we also hypothesize that the detrimental association between working in an automatable job and life satisfaction is more pronounced in occupations highly affected by workplace automation, particularly blue-collar jobs. These jobs are more prevalent in Germany's workforce and often involve routine tasks susceptible to automation, thereby exacerbating the overall effect we expect to find for the entire workforce. Finally, we explore the possibility that the relationship between job automatability and life satisfaction may not be linear. We hypothesize that certain levels of automatability might be beneficial, particularly in job profiles where automation does not threaten job security but significantly reduces physical strain or increases efficiency. Such potential non-linear dynamics have not been thoroughly examined in the literature, yet understanding these nuances is crucial for a comprehensive analysis.

In the upcoming empirical analysis, we aim to test these hypotheses, thereby contributing to a deeper understanding of the complex dynamics between job automatability and worker well-being in an industrially diverse economy.

5.3 Data and methodology

In this section, we detail the data sources, variables, and empirical strategy employed in our analysis. We also describe the final estimation sample used to investigate the relationship between job automatability and worker well-being.

5.3.1 Data sources and variables

Individual-level SOEP data

Our study relies on individual-level data collected by the SOEP survey. The SOEP is an annual representative household panel study of the German population conducted since 1984. In the most recent wave of our sample, in 2020, it includes more than 30,000 individuals living in over 15,000 households. The survey is a rich data source providing individual-level data on various measures, including job- and family-related characteristics, political orientation, as well as information on health and well-being. It is widely used to track political and social change in the German population due to its ability to provide information on individual biographies spanning many years. All the covariates used in this study are derived from the SOEP dataset.[113]

We measure our main outcome variable, reported life satisfaction, using individual-level information provided by the SOEP. Each survey year, participants rate their life satisfaction on an 11-point Likert scale ranging from 0 to 10, with higher numbers indicating greater life satisfaction. Following standard practice in the literature, we standardize this measure to have a mean of zero and a standard deviation of one (Dolan et al., 2008). We explore the full potential of the SOEP data by utilizing its panel dimension, which enables us to employ individual-level fixed effects in our estimations. This approach effectively exploits variations at the individual level and addresses unobserved time-invariant individual-level factors, such as personal interpretations of survey questions and scale range (see also section 5.3.2).

Measuring job automatability

Our study draws on the well-established measure of occupational susceptibility to workplace automation developed by Autor and Dorn (2013), which is a pioneering and widely acknowledged approach in the literature based on occupational RTI scores. We prefer this measure over alternative methodologies because it offers a comprehensive assessment of the potential for automation across various technological advancements. Unlike other measures that focus on specific technological aspects such as computerization, robotization, or the influence of AI in the workplace, the measure by Autor and Dorn provides a more inclusive and holistic perspective. It broadly considers technological change, assuming that routine tasks are generally more susceptible to automation, whereas manual and abstract tasks are less likely to be substituted (Spitz-Oener, 2006). Autor and Dorn (2013) define the RTI per occupation as

$$RTI_k = ln(T_k^R) - ln(T_k^M) - ln(T_k^A), \tag{5.1}$$

where T_k^R, T_k^M, and T_k^A represent the absolute levels of routine, manual, and abstract tasks for each occupation k.[114] The RTI index increases as the relative importance of routine tasks within an occupation rises, while it decreases as the number of associated manual and abstract tasks decreases. The use of logarithmic forms ensures a diminishing marginal value of all components. Across occupations, the index ranges from -2.41 to 6.42, with smaller values indicating lower RTI scores.

[113] Additional details about the SOEP survey can be found at `https://www.diw.de/en/soep` (last accessed: 15.05.2024).

[114] This index is based on job task descriptions found in the fourth edition of the US Department of Labor's Dictionary of Occupational Titles (US Department of Labor, 1977).

We utilize the information content of the RTI index by constructing our main independent variable as a binary dummy variable based on the relative RTI index score position of each occupation. Following the approach of Autor and Dorn (2013) and Lordan and Stringer (2022), we define a job as 'automatable' if it falls within the top third of jobs most susceptible to automation in terms of RTI. Furthermore, we adopt another distinctive aspect of Autor and Dorn (2013)'s methodology: its dynamic nature. By identifying the top third of jobs in terms of RTI annually and constructing an employment-weighted automation susceptibility measure, we adjust for the evolving composition of the workforce over time. This dynamic adjustment is crucial for our study as it accommodates shifts in the workforce composition caused by previous technological advancements or other labor market shocks. Unlike static measures, Autor and Dorn (2013)'s dynamic approach ensures that our analysis remains responsive to changes in the job market, making it superior in capturing technology's evolving impact on employment dynamics.[115]

The original data from Autor and Dorn (2013) refer to the United States labor market, coded based on an adapted form of the 1990 Census Bureau's occupational classification scheme (*occ1990*). To adapt this data to the German context, we cross-walk ISCO88 4-digit occupations (available in the SOEP) with the respective census bureau-classified occupations using the crosswalk provided by Humlum and Meyer (2020).[116] For 17 ISCO classifications, multiple occupations exist in the original United States data. We estimated average values for the relative level of routine, manual, and abstract tasks within those occupations to derive meaningful RTI scores. After merging with the SOEP data, we retained 323 4-digit occupations, which signifies a loss of 60 occupations compared to the original SOEP dataset. To ensure data integrity, we dropped observations where RTI information was insufficient. We present summary statistics in section 5.3.3.

5.3.2 Empirical strategy

Main strategy

In our analysis, we focus on working-age individuals for whom we have complete information on all covariates from the SOEP dataset. To utilize the panel dimension of the SOEP and incorporate individual-level fixed effects, we restrict our analysis to individuals present in the SOEP for a minimum of two waves.[117] Our primary objective is to assess whether individual-level variations in Autor and Dorn (2013)'s job automatability measure are associated with individual-level variations in self-reported life satisfaction. To achieve this, we estimate our baseline model, defined in equation 5.2, which serves as the foundation for our analysis:

[115] Incorporating dynamic elements such as employment-weighting is possible in other metrics, such as those focusing specifically on computerization, robotization, or AI. However, this is not necessarily straightforward. The dynamics of technological change vary over time, as does the relative importance of certain technological disruptions. For example, workplace computerization may have been especially prominent during the 2000s, while AI gained widespread importance only relatively recently. This variability becomes even more critical when comparing effects across regions or countries with diverse development levels. The approach of using RTI as proxy of 'automatability' is therefore more adept at capturing technological change in general, and over time (Autor and Dorn, 2013).

[116] The International Standard Classification of Occupations (ISCO) system used in the SOEP dataset is defined by the International Labour Organization (ILO).

[117] Refer to section 5.3.3 for our exact data sample.

$$Y_{i,t} = \beta_0 + \beta_1 A_{i,t} + \mathbf{X}'_{i,t}\beta_2 + \lambda_i + \lambda_t + \varepsilon_{i,t} \tag{5.2}$$

In our regression model, $Y_{i,t}$ represents the reported life satisfaction of individual i surveyed in year t. The variable $A_{i,t}$ is a dummy variable indicating the automatability of individual i's occupation at the 4-digit level in year t. It takes the value of one if the occupation belongs to the top third of employment-weighted occupations with the highest RTI values in that year, and zero otherwise. Additionally, λ_i and λ_t denote person and survey-wave year fixed effects, respectively. The covariate vector $\mathbf{X}'_{i,t}$ encompasses factors potentially associated with both life satisfaction and job automatability. Firstly, it includes individual-level characteristics such as age, age-squared, years of education, marital status, and migration background. By way of example, individuals with higher education levels may gravitate towards positions with lower automation levels but higher skill requirements, potentially influencing their life satisfaction. Secondly, it incorporates job-related covariates such as tenure in years, a squared tenure term, dummies for business size, and a dummy variable indicating employment in the public sector. Furthermore, it incorporates 1-digit occupation and industry dummies, as well as residence state and state-time survey year dummies. This approach helps ensure that we do not capture mere differences across these metrics in our analysis.

Alternative automation measure specification

It is important to acknowledge a potential limitation in the assessment of job automatability as specified by Autor and Dorn (2013). The delineation of the top third of jobs, determined by employment-weighted RTI, represents a somewhat arbitrary threshold for defining which jobs are automatable and which are not. This method may not fully capture the complexity of job automatability. While Autor and Dorn (2013) conducted robustness tests for their chosen threshold and weighting method, they did not extensively explore the significance of the reference group. For example, jobs with the highest levels of RTI might have distinct influences compared to those with the lowest intensity, but not necessarily relative to the jobs in between. Alternatively, the impact of highly routine-task intensive jobs might differ from those in the middle range of the spectrum but not from those at the lower end. In other words, the impact identified by Autor and Dorn through their measure might not solely arise from the contrast between highly routine-task intensive jobs and non-routine intensive jobs. Instead, it could be influenced by the selection and definition of their reference category – specifically, a distinction between highly routine-task intensive jobs and what we classify as *semi* routine-task intensive jobs. This nuance suggests that potential non-linearities and the choice of reference group are important considerations for interpreting any results. Our study aims to explore these aspects further, contributing to a more nuanced understanding of the relationship between job automatability and worker well-being.

We examine this methodological aspect in a different context with a distinct research focus, geographic area, and set of outcomes. Consequently, our findings are by no means comparable to those of Autor and Dorn (2013), but may serve as a reference point for future validations of Autor and Dorn's measure using their original data. In our analysis, we vary the original specification by defining *semi-automatable* jobs as those in the second tertile of the employment-weighted distribution. In this setting, we use the lowest tertile of the RTI occupation spectrum as our reference group, which we denote as *least-automatable* jobs. This approach allows us to maintain the core aspects of Autor and Dorn (2013)'s measure while specifically examining the significance of the reference category.

5.3.3 Estimation sample

We use 35 waves of the SOEP survey, covering the years 1984-2020, capturing the evolving landscape of the German labor market over nearly four decades. We restrict our sample to workers aged 18-65, and exclude self-employed and marginally employed individuals, as well as workers in vocational training. Moreover, certain items on the SOEP questionnaire are occasionally missing, for reasons unknown to us. Consequently, we omit observations with any missing values, resulting in an unbalanced panel sample. Our final estimation comprises 285,346 observations for 50,980 individuals. On average, each individual appears about 5.6 times in our data. Table 5.1 provides summary statistics for our main sample. Around one-third of observations are categorized as employed in automatable jobs according to the Autor and Dorn (2013) concept.[118]

Table 5.1: Sample summary statistics

Main Sample	min	median	mean	max
Automatable	0	0	0.339	1
Semi-automatable	0	0	0.336	1
Least-Automatable	0	0	0.325	1
Life Satisfaction	-4.392	-0.760	0.001	1.661
Female	0	0	0.487	1
# of Children	0	0	0.810	11
Married	0	1	0.646	1
Age	18	41	40.97	65
Years of Education	7	11.50	12.03	18
Tenure	0	6.583	9.945	50.917
Working Hours	0	40	37.48	80
Private Sector	0	1	0.749	1
Observations		285,346		
Individuals		50,980		

Note: Table provides summary statistics for our full data sample. Automation categories as defined in section 5.3.2.

Figure D.3 (in the appendix) offers a visual depiction of the distribution of automatable, semi-automatable, and least-automatable jobs across 1-digit level occupation categories. The figure unveils distinct clusters of occupations predominantly categorized as either automatable, semi-automatable, or least-automatable. Notably, office clerks are predominantly associated with automatable jobs, while professionals exhibit a high concentration of least-automatable positions. Meanwhile, service and craft workers show-

[118] The tertile split is conducted per occupation, consolidating several observations within each occupation. As a result, the actual percentages differ slightly from the expected value of 33 percent.

case a significant share of semi-automatable roles.[119] This stark contrast in automation susceptibility among occupation groups, including within semi- and least-automatable categories, facilitates a nuanced analysis distinguishing between these groups. Importantly, remember our analysis incorporates 1-digit occupation-fixed effects, meaning that we assess the association between automation and well-being *within* these specific occupation groups.

5.4 Results

In the following sections, we unveil the findings of our analysis. Section 5.4.1 initiates the discussion by presenting descriptive evidence regarding the association between working in an automatable job and life satisfaction. Subsequently, section 5.4.2 provides our baseline results and explores the impact of the reference category choice on estimation outcomes. Finally, section 5.4.3 delves into specific outcomes for distinct worker groups.

5.4.1 Descriptive evidence

Table 5.2 presents the differences in means for our main variable, life satisfaction, along with the socioeconomic and job-related covariates used in our study. Panel A contrasts the top third of occupations in the RTI distribution ('automatable jobs') with the lower two-thirds, based on the automation measure originally proposed by Autor and Dorn (2013). Panel B further examines the importance of the reference category by comparing the first ('non-automatable jobs') and second tertile ('semi-automatable jobs'), revealing substantial differences within the lower two tertiles of the RTI distribution.

The table shows that, in Panel A, standardized life satisfaction levels are not systematically different between jobs in the top (i.e., third) RTI tertile and other occupations. However, Panel B reveals a difference in mean life satisfaction between workers in the second and first (lowest) tertiles. These findings suggest that the RTI-life satisfaction relationship may not primarily arise from differences between the highest and lowest tertiles. Instead, the middle tertile seems to play a notable role. Figure D.1 supports this interpretation, showing that life satisfaction tends to be lower in the middle RTI range (the 'semi-automatable' jobs) rather than the lowest range (the 'least-automatable' jobs).

Regarding the other covariates in our analysis, we detect several noteworthy observations. While age differences between the most and least automatable jobs are economically insignificant, workers in the second tertile of the RTI distribution are notably younger than those in the least automatable roles. Additionally, they tend to have lower education levels, shorter job tenure, and are more likely to work in the public sector. These disparities, which may influence both job automatability and life satisfaction, are evident in both the automation measure as proposed by Autor and Dorn (2013) and our alternative specification, reinforcing the rationale for including them as covariates.[120]

[119]When examining individual occupations, noteworthy instances within the automatable jobs category include office clerks (ISCO88 4-digit occupation code number 4190), administrative secretaries (3431), and, broadly, manufacturing laborers. Occupations falling under the semi-automatable category particularly encompass shop, stall, and market salespersons and demonstrators (5220), personal care workers (5132), and nursing associate professionals. Leading the list of least-automatable jobs are teaching professionals (2320), helpers and cleaners (9132), and finance and sales professionals (3419).

[120]We also account for these factors in a heterogeneity analysis (see section 5.4.3).

Table 5.2: Descriptive statistics – Difference in means

Panel A: RTI tertile	1^{st} and 2^{nd}		3^{rd}			
	mean	sd	mean	sd	diff. in means	p-value
RTI	0.03	0.81	3.37	1.29	3.35	<0.01
Life Satisfaction	0.00	0.99	0.00	0.99	0.00	0.47
Female	0.44	0.50	0.58	0.49	0.14	<0.01
# of Children	0.84	1.06	0.75	0.99	-0.09	<0.01
Married	0.65	0.48	0.63	0.48	-0.02	<0.01
Age	40.95	11.45	41.01	11.56	0.05	0.26
Years of Education	12.14	2.75	11.82	2.35	-0.33	<0.01
Tenure	9.77	9.62	10.29	9.83	0.52	<0.01
Working Hours	37.96	12.27	36.54	11.04	-1.41	<0.01
Private Sector	0.73	0.22	0.79	0.20	0.06	<0.01
Observations				285,346		
Individuals				50,980		

Panel B: RTI tertile	1^{st}		2^{nd}			
	mean	sd	mean	sd	diff. in means	p-value
RTI	-0.64	0.56	0.67	0.39	1.31	<0.01
Life Satisfaction	0.03	0.98	-0.03	1.00	-0.06	<0.01
Female	0.42	0.49	0.46	0.50	0.04	<0.01
# of Children	0.85	1.08	0.84	1.05	-0.01	0.05
Married	0.66	0.47	0.65	0.48	-0.01	<0.01
Age	41.34	11.40	40.59	11.49	-0.75	<0.01
Years of Education	12.78	3.10	11.53	2.18	-1.25	<0.01
Tenure	10.07	9.86	9.47	9.38	-0.60	<0.01
Working Hours	38.74	12.97	37.20	11.51	-1.53	<0.01
Private Sector	0.66	0.24	0.79	0.21	0.13	<0.01
Observations				188,744		
Individuals				38,954		

Note: The table shows the weighted means (using the dataset-provided individual-level survey weights) of variables differentiated by the degree of occupational automatability. Panel A differentiates between the first two tertiles and the last tertile, as proposed by the original automation measure by Autor and Dorn (2013). To emphasize potential reference category effects, Panel B differentiates between the first and second tertile of the RTI distribution following the amended measure. The sample covers the years between 1984-2020. All values are rounded to the second decimal place.

5.4.2 Main estimates

We compute the regression model described by equation 5.2 and present the outcomes in the first two columns of table 5.3. In column (1), we provide the estimation results for a model excluding covariates, incorporating only fixed effects. In column (2), we introduce the complete set of control variables. The same procedure applies to columns (3) and (4), where we present results for the amended automation measure.

Turning first to the outcomes using Autor and Dorn (2013)'s original measure, the results displayed in table 5.3 show that working in an automatable job is associated with a significantly lower life satisfaction of about 0.019 standard deviations (evident from

Table 5.3: Workplace automation and life satisfaction

VARIABLES	(1) FE without covariates	(2) FE with covariates	(3) (1) with amended automation measure	(4) (2) with amended automation measure
Automatable	-0.017	-0.019*	-0.003	-0.006
	(0.011)	(0.011)	(0.012)	(0.012)
Semi-automatable			0.024**	0.022**
			(0.011)	(0.011)
Model specifications				
Covariates	NO	YES	NO	YES
Individual-level FE	YES	YES	YES	YES
Occupation (1-digit) FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
State FE	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES
State-time dummies	YES	YES	YES	YES
Observations	285,346	285,346	285,346	285,346
Adjusted R^2	0.465	0.466	0.465	0.466
Within R^2	0.0006	0.003	0.0007	0.003

Note: This table shows our baseline estimates on the relationship between working in an automatable job and life satisfaction. Dependent variable is self-reported life satisfaction. Columns 1 and 2 use the original automation measure by Autor and Dorn (2013), with 'non-automatable' (i.e. 'semi-automatable' and 'least-automatable' categories combined) as the reference category. Columns 3 and 4 use the amended automation measure, with 'least-automatable' as the reference category. The sample covers the years between 1984-2020. All values are rounded to the third decimal place. Standard errors clustered at the individual level are depicted in parentheses: ***: $p<0.01$, **: $p<0.05$, *: $p<0.1$.

column (2), our baseline result). The sign and magnitude of the estimated coefficient are stable when comparing the specifications with and without covariates. However, the coefficient displays significance at the 10 percent level only when accompanied by the complete set of covariates.

Surprisingly, we find a completely different outcome when using the amended measure of job automatability. As evident from columns (3) and (4), the estimated coefficient for working in an automatable job is relatively small and statistically insignificant in this setting. Instead, we find a significantly positive coefficient estimate for working in a semi-automatable job. This estimate demonstrates considerable stability across both models with and without covariates. According to our estimations, working in a semi-automatable job is associated with a 0.022 standard deviation increase in reported life satisfaction. To illustrate the relevance of this result, we compare it to the coefficient estimate of being married. In our sample, being married is associated with a 0.066 standard deviation increase in life satisfaction.[121] Therefore, the estimated magnitude of the coefficient for working in a semi-automatable job is about one-third the magnitude of the coefficient for being married.

Hence, we interpret our finding as highly significant: Occupying a semi-automatable job is linked with a notably higher level of life satisfaction, akin to roughly one-third of the impact observed with marriage. Overlooking the distinct levels of automation

[121] A full table of our estimation results can be found in the appendix, table D.2.

masks a crucial aspect of comprehending the correlation between job automatability and workers' life satisfaction. In our overall sample of German workers, it is not the individuals potentially most impacted by automation who report a disparate level of well-being, but rather those experiencing moderate influence who display a positive deviation in well-being. We propose that these workers adapt to technological change more effectively, enhancing their well-being through gradual adjustment and successful integration of their skills and knowledge with new technologies.

Consequently, our results also have significant implications for firm policies: Automation technology may positively impact life satisfaction, particularly when introduced gradually. Firms should thoughtfully assess both the trajectory and pace of adopting new technology to avoid overburdening their employees. A considered approach to technology implementation can enhance employee happiness, which in turn offers various advantages to the firm (see for instance Oswald et al., 2015). Our findings also align well with psychological literature examining the impact of workplace changes on workers' well-being, emphasizing the importance of adaptability (e.g., Lazarus, 2020).

We conducted additional tests to confirm the robustness of our findings. Firstly, we refined our model specification by categorizing the automatability measure into six groups (sextiles) instead of three (i.e. splitting each category into a further two). The corresponding results can be found in table D.3 in the appendix. From the regression output, we observe positive coefficient estimates only for the semi-automatable sextiles. Notably, the third sextile of the RTI distribution exhibited a statistically significant positive association with life satisfaction, leading us to suspect it as the driving force behind the observed positive association between semi-automatable jobs and life satisfaction. Secondly, we explored the impact of varying the reference category. Instead of using the lowest automation category as the reference, we designated semi-automatable jobs as the reference category. As expected, this analysis (presented in table D.4 in the appendix) reveals that workers in both highly automatable and least-automatable jobs reported significantly lower life satisfaction scores compared to those in the semi-automatable reference category. Finally, we assessed the sensitivity of the specification by adjusting the dynamic components. Instead of annually employment-adjusting the automation measure, as suggested by Autor and Dorn (2013), we maintained this component constant. Specifically, we set the employment shares at the midpoint of our sampling period (i.e., for the year 2000), ensuring that any changes in outcomes are not driven by variations in employment shares but solely by the classification of jobs as automatable. Remarkably, under this specification, we observe only negligible changes in outcomes, reinforcing the robustness of our findings (refer to table D.5 in the appendix).

5.4.3 Worker heterogeneity

The selection of the reference category significantly impacts the estimation results presented so far. Merging the least- and semi-automatable roles into a single reference category obscures critical information and attenuates the actual degree of variability in the data, potentially leading to misinterpretations of findings. Given this, it is imperative to investigate whether specific groups of workers predominantly drive our results. We contend that the nature of work itself plays a pivotal role. Literature suggests that laborers in the manufacturing sector and blue-collar workers are at the forefront of experiencing the impacts of automation (e.g., Acemoglu and Restrepo, 2020). These roles often entail physical and repetitive tasks that are more susceptible to automation (see literature in section 5.2). Conversely, white-collar workers, typically engaged in non-physical and cognitive tasks, may encounter different effects from automation, possibly even benefiting from enhanced efficiency and productivity.[122] Therefore, we hypothesize that blue-collar workers may respond differently to automation compared to their white-collar counterparts.[123]

Another source of worker heterogeneity could stem from their age. We anticipate automation to affect age groups differently: Older workers may particularly appreciate the supportive role of workplace automation, which can alleviate their workload and relieve them of physically demanding or strenuous job roles, potentially enhancing their well-being. Especially in Germany, older workers may also feel relatively more secure about their jobs in an environment of workplace automation, given the country's employment protection legislation, which values both age and tenure and emphasizes social aspects, including age, in dismissal decisions. Conversely, younger workers may perceive workplace automation as more concerning. They have a less robust safety net and face longer-term implications from potential job losses, given their longer working lives ahead. Career trajectories in Germany often follow predictable paths, making job losses particularly daunting for those at the outset of their careers. Additionally, younger workers, with typically healthier and more resilient bodies, may not appreciate the physical relief that comes with workplace automation as much as older workers do. Hence, we anticipate a relatively stronger negative association between working in an automatable job and life satisfaction among the younger employees in our sample, relative to their older counterparts. However, in the case of semi-automatable jobs, these arguments are less likely to apply, given the reduced impact of workplace automation per se. We expect relatively less age heterogeneity here.

We proceed to test both of these avenues in the following analysis, initially exploring the significance of blue-collar jobs, followed by an investigation into the role of age.

Blue-Collar versus White-Collar workers

Table 5.4, columns 3 and 4, present findings from a model specification incorporating interaction terms for individuals categorized as blue-collar workers. For easy comparison, we report the baseline estimates from the previous section in the table's first two columns. Our investigation encompasses both Autor and Dorn (2013)'s conventional measure and the refined version, which incorporates the classification of semi-automatable jobs.

Relative to the baseline results (presented in columns (1) and (2)), significant disparities emerge upon adjusting our estimates to account for workers' blue-collar backgrounds. When considering Autor and Dorn (2013)'s original automation measure (reported in column (3)), we observe that the negative association identified in the baseline specification is primarily driven by blue-collar workers. In this specification, the relationship for workers in automatable jobs (non-blue-collar, i.e. white-collar) becomes statistically insignificant, while the interaction term for being a blue-collar worker shows the expected

[122]We analyze worker heterogeneity here based on blue-collar and white-collar distinctions rather than investigating specific industries, such as manufacturing. This aligns more with recent literature, which emphasizes differentiation based on tasks (such as physical vs. cognitive), not by industries (e.g., Frey and Osborne, 2017, Arntz et al., 2016). Differentiation by industry may aggregate various job roles and thus even mask task-specific relationships.

[123]Note that the presence of blue-collar workers in automatable jobs, and vice versa, is not universal. Well below 30 percent of blue-collar workers are employed in occupations classified as automatable in our sample. For a visual representation, refer to figure D.2 in the appendix.

Table 5.4: Heterogeneity analysis – Blue- versus White-collar workers

VARIABLES	(1) (baseline)	(2) (1) with amended automation measure	(3) (1) with blue-collar interaction	(4) (2) with blue-collar interaction
Automatable	-0.019*	-0.006	0.004	0.018
	(0.011)	(0.013)	(0.014)	(0.016)
Automatable × Blue-Collar			-0.054**	-0.058**
			(0.022)	(0.025)
Semi-automatable		0.022**		0.026*
		(0.011)		(0.014)
Semi-automatable × Blue-Collar				-0.011
				(0.021)
Model specifications				
Covariates	YES	YES	YES	YES
Individual-level FE	YES	YES	YES	YES
Occupation (1-digit) FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
State FE	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES
State-time dummies	YES	YES	YES	YES
Observations	285,346	285,346	285,346	285,346
Adjusted R^2	0.466	0.466	0.466	0.466
Within R^2	0.003	0.003	0.003	0.003

Note: This table shows our baseline estimates on the relationship between working in an automatable job and life satisfaction; in columns (3) and (4) the respective underlying models are additionally interacted with a dummy variable identifying workers in blue-collar occupations. Dependent variable is self-reported life satisfaction. Model specifications are identical to the baseline specification reported in table 5.3 (with the full set of covariates). Columns 1 and 3 use the original automation measure by Autor and Dorn (2013), with 'non-automatable' as reference category. Columns 2 and 4 use the amended automation measure, with 'least-automatable' as reference category. All values are rounded to the third decimal place. Standard errors clustered at the individual level are depicted in parentheses: ***: $p<0.01$, **: $p<0.05$, *: $p<0.1$.

negative sign and statistical significance at the 5 percent level. Additionally, it exhibits roughly twice the magnitude compared to the overall baseline estimate in column (1). Specifically, being a blue-collar worker engaged in an automatable occupation correlates with lower life satisfaction, approximately 0.05 standard deviations lower (this result is evident when adding both estimated coefficients, i.e., $-0.054 + 0.004 \approx -0.05$).

In the model specification using the amended automation measure, we observe an intriguing pattern. The negative deviation of blue-collar workers in automatable job roles persists. However, the overall coefficient on this relationship (i.e., the estimated relationship for the complete set of workers) is non-significant. Automation appears to be associated with lower life satisfaction, but only among blue-collar workers. Additionally, we continue to report a positive deviation of workers in semi-automatable roles relative to the reference category (i.e., workers in least-automatable jobs). Importantly, this association does not significantly differ for blue-collar workers.

We further investigate this relationship by examining all nine levels of 1-digit ISCO occupation groups via interaction terms. However, it is important to note that in all our model specifications presented so far, our estimation strategy adjusts coefficient estimates for these occupation groups anyway. Low underlying observation counts, as witnessed for many occupation groups of particular automation type (see also figure D.3), may therefore have a significant impact on coefficient estimates *by occupation group*, potentially adding noise to the analysis. Consequently, we interpret findings with caution. The results from this analysis are reported in table D.6 in the appendix. Under both Autor and Dorn (2013)'s original automation measure (column 1) and the amended measure (column 2), blue-collar occupations (ISCO codes 6 to 9) show the largest negative interaction term estimates among all the occupation groups for workers in automatable job roles, albeit not always significantly estimated.[124] For white-collar workers, we report ambiguous coefficient estimates with no clear pattern.

Worker age

In the subsequent analysis, we aim to validate our assumptions regarding varying associations by age. For this purpose, we introduce age categories and interact them with the automation measures. Specifically, we define age groups as young (18-27), middle-aged (28-54), and older (55-65) workers, setting young individuals as the reference group in all our estimations. Table 5.5 presents the corresponding estimation results.

Overall, our expectations regarding age are confirmed: When considering (Autor and Dorn, 2013)'s original measure, reported in column (1), we find working in an automatable job is associated with a significantly lower reported life satisfaction value (0.046 standard deviations lower). This baseline finding corresponds to younger workers, the reference category. We find no significant deviation from this result for middle-aged workers. However, for older workers, we observe a significantly positive deviation; the overall relationship between working in an automatable job and life satisfaction appears to be close to zero for old workers (again, this is evident from adding both estimated coefficients, i.e., $-0.046 + 0.054 \approx 0.008$). Taken together, these results suggest that the overall negative relationship we observe in the data is predominantly driven by the group of younger and middle-aged workers, aligning with our expectation that such worker's well-being is particularly affected by the fear of job loss or job insecurity is stronger for those individuals. When examining the model using the amended measure, available from column (2), where semi-automatable jobs are introduced as an additional category, we confirm our baseline results from table 5.3 (working in semi-automatable jobs is positively associated with life satisfaction) and find evidence for our expectations regarding age – that there is no varying relation based on age.

The reference category effect observed in the baseline estimates in table 5.3 persists in the analysis of age differences. However, as evidenced by the blue-collar analysis, aggregating workers into a single sample masks worker heterogeneity and associated outcomes. Therefore, in addition to exploring simple age heterogeneity, we investigate whether the distinction between blue-collar and white-collar jobs is relevant to the associations observed among different age groups.[125] The corresponding results can be found in the appendix, table D.7. The outcomes from this exercise support our overall

[124] However, among those insignificant coefficient estimates, the corresponding p-values are close to the 10 percent level of significance (between 0.11 and 0.14).

[125] Rather than introducing additional interaction terms, we employ a straightforward sub-sample strategy, separately analyzing blue-collar and white-collar workers. This approach is feasible as the division into these categories provides sufficient variation within each subgroup, as demonstrated in the analysis presented in table 5.4.

Table 5.5: Heterogeneity analysis – Worker's age

VARIABLES	(1) (baseline)	(2) (1) with amended automation measure	(3) (1) with age interaction	(4) (2) with age interaction
Automatable	-0.019*	-0.006	-0.046**	-0.023
	(0.011)	(0.013)	(0.022)	(0.026)
Automatable × age 27-55			0.028	0.016
			(0.022)	(0.026)
Automatable × age 56-65			0.054*	0.044
			(0.029)	(0.034)
Semi-automatable		0.022**		0.042*
		(0.011)		(0.024)
Semi-automatable × age 27-55				-0.024
				(0.025)
Semi-automatable × age 56-65				-0.021
				(0.033)
Model specifications				
Covariates	YES	YES	YES	YES
Individual-level FE	YES	YES	YES	YES
Occupation (1-digit) FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
State FE	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES
State-time dummies	YES	YES	YES	YES
Observations	285,346	285,346	285,346	285,346
Adjusted R^2	0.466	0.466	0.466	0.466
Within R^2	0.003	0.003	0.003	0.004

Note: This table shows our baseline estimates on the relationship between working in an automatable job and life satisfaction; in columns (3) and (4) the respective underlying models are additionally interacted with age categories (young, middle-aged, older workers). Dependent variable is self-reported life satisfaction. Model specifications are identical to the baseline specification reported in table 5.3 (with the full set of covariates). Columns 1 and 3 use the original automation measure by Autor and Dorn (2013), with 'non-automatable' as reference category. Columns 2 and 4 use the amended automation measure, with 'least-automatable' as reference category. All values are rounded to the third decimal place. Standard errors clustered at the individual level are depicted in parentheses:
***: p<0.01, **: p<0.05, *: p<0.1.

results from this section thus far: Being a blue-collar worker employed in an automatable job is associated with lower reported life satisfaction, and this relationship is more pronounced among younger workers – a finding consistent across the two measures of automation we apply. Conversely, white-collar workers appear relatively homogeneous, with little variation across age groups.

Additionally, we examined heterogeneity across gender, migration background, skill level, and public-sector employment. However, the corresponding results (detailed in table D.8 in the appendix) revealed no significant differences for any of these factors, regardless of whether considering automatable or semi-automatable jobs. Interestingly, these findings align with earlier research by Lordan and Stringer (2022) for Australia, which similarly detected little variation based on socioeconomic factors.

5.5 Conclusion and discussion

The rapid advancements in automation, information technology, and robotics over the last 40 years have brought about significant transformations in the employment landscape. While these developments promise enhanced efficiency and productivity, they also raise concerns about job displacement and its implications for workers' well-being. Previous research has highlighted the negative effects of workplace automation on various aspects of workers' lives, including (perceived) job security, wage dynamics, and physical and mental health. Concurrently, automation has demonstrated to also have impacts potentially increasing worker well-being, including leading to an enhanced workplace safety, reduced monotonous tasks, and increased opportunities for remote work. Despite the growing significance of this issue, there remains a notable gap in understanding the impact of job automation on overall worker well-being, particularly on life satisfaction. Our study aims to address this gap by building upon previous research, which has yielded mixed results in Australia, and exploring the case of Germany. With Germany's significant reliance on manufacturing and industrial employment—highly susceptible to automation—the nation's workforce is profoundly affected by technological progress.

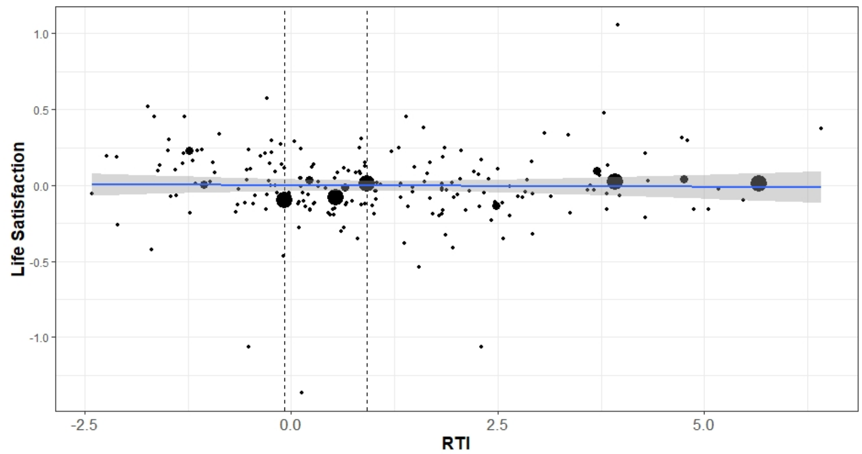
In our study, we examined the relationship between working in occupations at high risk of workplace automation and self-reported individual-level life satisfaction in Germany. By combining German SOEP data with Autor and Dorn (2013)'s well-established job automatability measure and applying individual-fixed effects, we contribute several new insights to the literature. Firstly, we confirmed that working in an automatable job is associated with significantly lower life satisfaction in Germany, equivalent to approximately 0.023 standard deviations. This effect size is roughly one-third of the estimated impact that marriage has on life satisfaction (albeit with an opposite sign), highlighting its substantive magnitude. Secondly, we demonstrated that Autor and Dorn (2013)'s automation measure may lead to misleading interpretations, particularly within our specific context. Its interpretation heavily depends on its application to the data and its implementation design. Autor and Dorn (2013)'s measure identifies the top third of jobs based on RTI as 'most susceptible to automation' and uses a dummy variable to categorize these occupations in the data. By subdividing the original measure's reference category ('non-automatable') into two equally-sized subcategories—'semi-automatable' and 'least-automatable' jobs—we observed a reference category effect in our analysis: While working in semi-automatable jobs showed a significant positive association with life satisfaction, workers in the most automatable jobs in our data did not exhibit a statistically significant difference in life satisfaction compared to those in the reference category, those in least-automatable jobs. This finding suggests that job automation may positively influence overall worker life satisfaction when implemented gradually, i.e. when allowing for continuous adaptation and without immediately threatening job security.

Furthermore, drawing on insights from the literature review, which highlights the nuanced interplay between technological change, perceived job insecurity, and overall well-being across various socioeconomic and job-related dimensions, we investigated heterogeneous outcomes based on type of work and age. Our analysis revealed a notably pronounced negative association between workplace automation and life satisfaction among blue-collar workers, while this association was undetectable among white-collar workers. Interestingly, the observed negative association for blue-collar workers persisted even after accounting for the reference category effect identified with the amended automation measure. Additionally, we found that younger workers exhibited a stronger negative association with job automatability compared to older workers. However, there was little age heterogeneity detected when examining the effects using the proposed amended automation measure. Once again, blue-collar workers emerged as pivotal in the observed relationships.

Our results suggest several important aspects to consider when evaluating the impacts of workplace automation, as well as potential avenues for future research: Firstly, accurately measuring occupational automatability necessitates a careful evaluation of the automation measure and the selection of an appropriate control group. Even minor methodological adjustments can yield different outcomes, underscoring the importance of testing various thresholds and exploring potential distributional irregularities and anomalies, including within the reference group. Secondly, from a firm policy perspective, our findings indicate that companies should adopt a gradual approach to implementing new technologies. Phasing in significant technological changes cautiously may allow the workforce to adapt effectively while avoiding potential woes and fears that might arise when employees are overwhelmed by rapid adaptation demands. This approach could potentially enhance overall worker well-being. Thirdly, our results suggest potential policy implications related to employment protection legislation in Germany. The age of affected individuals may have influenced the associations we observed. Consequently, if improving worker well-being is a priority for the state, policymakers could consider addressing the satisfaction of specific worker groups through targeted legislative measures within employment protection frameworks. Such measures could help alleviate the negative perceptions, concerns, and fears associated with job automation and technological change. And finally, while our heterogeneity analysis aimed to explore the association between working in automatable jobs and life satisfaction across specific occupational groups and various socio-economic dimensions, our findings, consistent with earlier literature, reveal considerable ambiguity across many of the investigated dimensions. This suggests the potential presence of an important yet undiscovered element in the relationship – a missing puzzle piece that warrants further exploration.

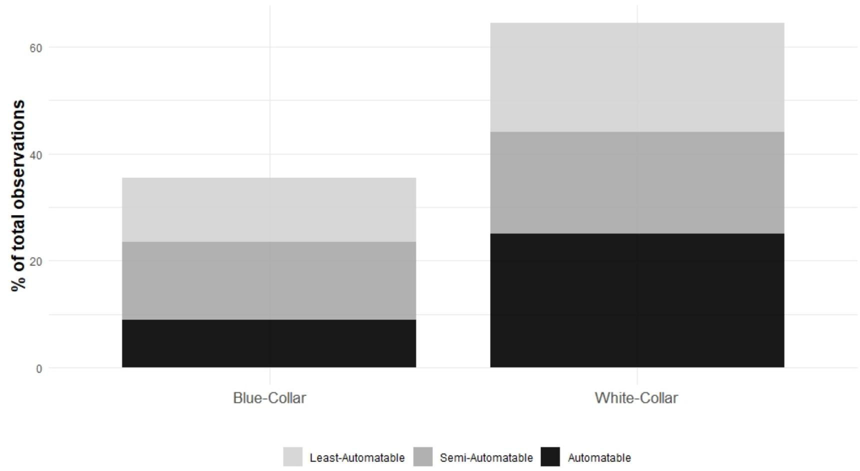
5.6 Appendix D

Figure D.1: Scatterplot – Life satisfaction and RTI scores



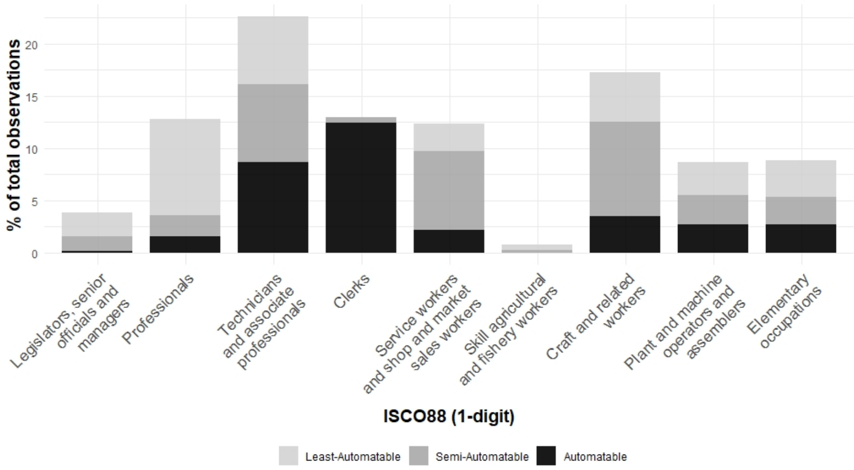
Note: This figure shows the occupational-level combinations of RTI and life satisfaction scores reported in our data. Circle sizes indicate the number of corresponding underlying observations. The horizontal blue line represents a linear fit of the data, with the grey-shaded area denoting the associated 95 percent confidence intervals. The vertical dotted lines represent the RTI thresholds defining the automatability categories used (each category represents about one-third of the total observations; refer to table 5.1): from 'Least-automatable' on the left to 'Automatable' on the right.

Figure D.2: Job automation by worker type



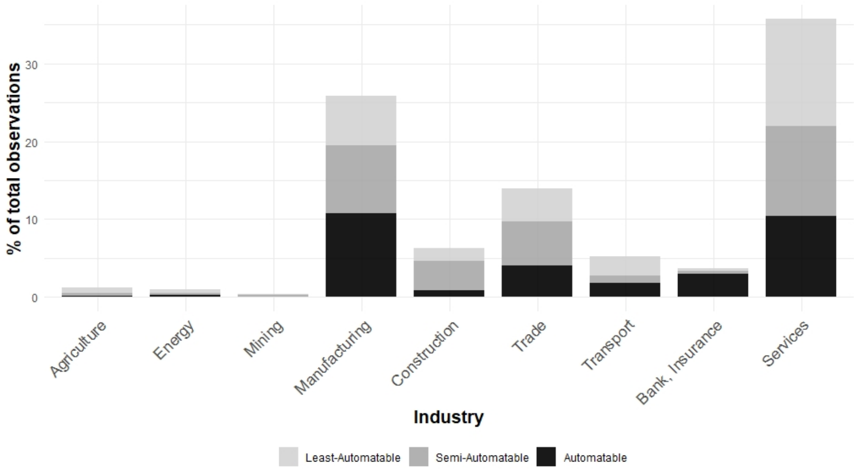
Note: This figure shows the distribution of automation classifications across blue and white-collar occupations. Each category represents about one-third of the total observations; refer to table 5.1: from 'Least-automatable' on the left to 'Automatable' on the right.

Figure D.3: Automation classification by occupation



Note: This figure shows the distribution of automation classifications across different occupations defined on the ISCO88 1-digit level. Each category represents about one-third of the total observations; refer to table 5.1: from 'Least-automatable' on the left to 'Automatable' on the right.

Figure D.4: Automation classification by industry



Note: This figure shows the distribution of automation classifications across industries. Each category represents about one-third of the total observations; refer to table 5.1: from 'Least-automatable' on the left to 'Automatable' on the right.

Table D.1: Automation measures used in economics literature

Measure	**Coverage**	**Data provided**	**Examples of literature**
Occupational Routine Task Intensity (RTI)[a]	323 occupations defined by the 1990 Census Bureau occupational classification scheme (occ1990)	Share of routine tasks among total occupational task content, based on job descriptions in the US Department of Labor's Dictionary of Occupational Titles (DOT)	(Autor et al., 2003, Autor and Dorn, 2013, Goos et al., 2014, Coelli and Borland, 2016, Yuhong and Xiahai, 2020, Josten and Lordan, 2020, Lordan and Stringer, 2022), [*our paper*]
Susceptibility to computerization[b]	702 SOC occupations with O*NET information on task content	Probability of computerization based on occupational task content calculated by an algorithm, originally trained on a subsample of 70 hand-labelled occupations	(Bonin et al., 2015, Arntz et al., 2016, Frey and Osborne, 2017, Patel et al., 2018, Nedelkoska and Quintini, 2018, Gorny and Woodard, 2020, Nazareno and Schiff, 2021, Jacobs et al., 2023, Liu, 2023)
Occupational Computerization Potential[c]	Around 3,900 occupations with accessible vocational information	Share of occupational tasks *currently* substitutable by computers, based on occupation descriptions in expert data base BERUFENET	(Dengler et al., 2014, Dengler and Matthes, 2015, 2018, Dengler et al., 2022)
Sectoral exposure to robots[d]	Manufacturing sector in 40 countries	Number of robots per employee (by area of application, industry [ISIC Rev. 4 classification], type of robot, and others)	(Graetz and Michaels, 2018, Acemoglu and Restrepo, 2020, Schwabe and Castellacci, 2020, Gunadi and Ryu, 2021, O'Brien et al., 2022, Gihleb et al., 2022, Abeliansky et al., 2021, Liu et al., 2024)
Suitability for Machine Learning[e]	964 occupations with a total of 18,156 job tasks in O*NET	Relative occupational share of tasks affected by machine learning technology	(Brynjolfsson et al., 2018)
AI Occupational Impact (AIOI)[f]	702 SOC occupations with O*NET information on task content	Relative relevance of specific AI advancements for skills, abilities, and occupational job roles in the workplace	(Felten et al., 2018, 2020)
AI exposure score[g]	702 SOC occupations with O*NET information on task content	Quantifies the overlap between AI patents text and the text of job descriptions (The metric can be adjusted to include other automation technologies, including software, industrial robots, and similar innovations)	(Webb, 2020, Liu, 2023)
Sectoral exposure to technological change [h]	96 (3-digit) NAICS-level industries	Year-to-year growth in the real stock of intellectual property (in chained prices)	(Makridis and Han, 2021)
Occupational exposure to AI [i]	2-digit ISCO occupations	Self-reported AI exposure	(Fedorets et al., 2022, Giuntella et al., 2023)

Note:
[a] `https://ddorn.net/data.htm`, see Autor and Dorn (2013)
[b] `https://www.onetonline.org/`, see Frey and Osborne (2017)
[c] `https://web.arbeitsagentur.de/berufenet/`, see Dengler and Matthes (2015)
[d] `https://ifr.org/worldrobotics/`
[e] `https://www.onetonline.org/`, see Brynjolfsson et al. (2018)
[f] `https://www.onetonline.org/`, see Felten et al. (2020)
[g] `https://www.onetonline.org/`, see Webb (2020)
[h] `https://www.bea.gov/data`, see Makridis and Han (2021)
[i] `https://paneldata.org/soep-core/`, see Giuntella et al. (2023)
See also Calvino and Virgillito (2018) and Mondolo (2022) on an examination of several measures, main advantages and shortcomings of the methods, and on further economic literature utilizing automation metrics.

Table D.2: Main estimation results – Full output

VARIABLES	(1) (baseline)	(2) (1) with amended automation measure
Automatable	-0.019*	-0.006
	(0.011)	(0.012)
Semi-automatable		0.022**
		(0.011)
married	0.064***	0.064***
	(0.014)	(0.014)
age	-0.271	-0.275
	(788.1)	(787.6)
age squared	0.0002***	0.0002***
	(0.000)	(0.000)
years of education	-0.021***	-0.021***
	(0.007)	(0.007)
log(net earnings)	0.115***	0.114***
	(0.010)	(0.010)
tenure	-0.004**	-0.004**
	(0.002)	(0.002)
tenure squared	0.000	0.000
	(0.000)	(0.000)
Private sector	-0.021*	-0.021*
	(0.013)	(0.013)
Agriculture	-0.009	-0.009
	(0.042)	(0.042)
Energy	0.046	0.046
	(0.046)	(0.046)
Mining	0.127	0.125
	(0.078)	(0.078)
Manufacturing	0.014	0.013
	(0.018)	(0.018)
Construction	0.022	0.020
	(0.024)	(0.024)
Trade	-0.010	-0.010
	(0.020)	(0.020)
Transport	-0.005	-0.004
	(0.027)	(0.027)
Bank,Insurance	0.043	0.042
	(0.034)	(0.034)
Services	0.018	0.018
	(0.016)	(0.016)
Professionals	0.033	0.036
	(0.023)	(0.023)
Technicians and associate professionals	-0.002	-0.004
	(0.020)	(0.020)
Clerks	-0.012	-0.016
	(0.023)	(0.023)
Service workers and shop and market sales workers	-0.041*	-0.050**
	(0.023)	(0.023)
Skilled agricultural and fishery workers	-0.114*	-0.109*
	(0.058)	(0.058)
Craft and related trades workers	-0.033	-0.036
	(0.025)	(0.025)
Plant and machine operators and assemblers	-0.028	-0.031
	(0.027)	(0.027)
Elementary occupations	-0.067***	-0.069***
	(0.025)	(0.025)
company size 20-199	0.014	0.013
	(0.011)	(0.011)
company size 200-1999	0.023*	0.023*
	(0.013)	(0.013)
company size $\geqslant$ 2000	0.032**	0.031**
	(0.013)	(0.013)
Observations	285,346	285,346
Adjusted R^2	0.466	0.466
Within R^2	0.003	0.003

Note: This table shows the full regression outputs of our baseline estimates (including the complete set of covariates) on the relationship between working in an automatable job and life satisfaction. These results are also summarized in table 5.3 in the main text. Column (1) reports outcomes using the original automation measure by Autor and Dorn (2013), with 'non-automatable' as the reference category. Column (2) uses the amended automation measure, with 'least-automatable' as the reference category. All values are rounded to three decimal places (except for the variable 'age-squared', where we report four decimal places for the estimated coefficient). Standard errors clustered at the individual level are depicted in parentheses: ***: p<0.01, **: p<0.05, *: p<0.1.

Table D.3: Alternative automation measure with six levels

VARIABLES	(1)
Job Automation Quantile	
2. Quantile	-0.009
	(0.017)
3. Quantile	0.030*
	(0.016)
4. Quantile	0.022
	(0.016)
5. Quantile	-0.010
	(0.017)
6. Quantile	-0.003
	(0.019)
Model specifications	
Covariates	YES
Individual-level FE	YES
Occupation (1-digit) FE	YES
Industry FE	YES
State FE	YES
Year dummies	YES
State-time dummies	YES
Observations	285,346
Adjusted R^2	0.466
Within R^2	0.003

Note: This table shows regression estimates on the relationship between working in an automatable job and life satisfaction. The dependent variable is self-reported life satisfaction. Relative to the baseline specifications in table 5.3, we categorize workplace automation differently here, dividing it into six quantiles. The first quantile (with the lowest potential for workplace automation) serves as the reference category. All values are rounded to the third decimal place. Standard errors clustered at the individual level are depicted in parentheses: ***: p<0.01, **: p<0.05, *: p<0.1.

Table D.4: Varying the reference category

VARIABLES	(1)	(2)	(3)
Automatable	-0.019*	-0.006	-0.028**
	(0.011)	(0.012)	(0.012)
Semi-automatable	(reference)	0.022**	(reference)
		(0.011)	
Least-Automatable		(reference)	-0.022**
			(0.011)
Model specifications			
Covariates	YES	YES	YES
Individual-level FE	YES	YES	YES
Occupation (1-digit) FE	YES	YES	YES
Industry FE	YES	YES	YES
State FE	YES	YES	YES
Year dummies	YES	YES	YES
State-time dummies	YES	YES	YES
Observations	285,346	285,346	285,346
Adjusted R^2	0.466	0.466	0.466
Within R^2	0.003	0.003	0.003

Note: This table shows regression estimates on the relationship between working in an automatable job and life satisfaction. The dependent variable is self-reported life satisfaction. The specification in column (1) uses both semi-automatable and least-automatable jobs as the reference category. The specification in column (2) uses only least-automatable jobs as the reference category, while the specification in column (3) uses only semi-automatable jobs as the reference category. All values are rounded to the third decimal place. Standard errors clustered at the individual level are depicted in parentheses: ***: $p<0.01$, **: $p<0.05$, *: $p<0.1$.

Table D.5: Year 2000-fixed employment weights

VARIABLES	(1) dynamic weights (baseline)	(2)	(3) employment fixed (year 2000)	(4)
Automatable	-0.019*	-0.006	-0.020*	-0.006
	(0.011)	(0.012)	(0.012)	(0.013)
Semi-Automatable		0.022**		0.028**
		(0.011)		(0.011)
Model specifications				
Covariates	YES	YES	YES	YES
Individual-level FE	YES	YES	YES	YES
Occupation (1-digit) FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
State FE	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES
State-time dummies	YES	YES	YES	YES
Observations	285,346	285,346	283,353	283,353
Adjusted R^2	0.466	0.466	0.466	0.466
Within R^2	0.003	0.003	0.003	0.003

Note: This table shows regression estimates on the relationship between working in an automatable job and life satisfaction. The dependent variable is self-reported life satisfaction. We distinguish between the baseline findings using dynamic employment-weighting (as also reported in table 5.3) in columns (1) and (2), and an alternative specification in which the job-automation categorization is fixed over time (set to the year 2000). Note that the number of observations is lower under the fixed specification, as not all occupations reported in other years are available in 2000. The sample still covers all survey waves from 1984 to 2020. All values are rounded to the third decimal place. Standard errors clustered at the individual level are depicted in parentheses: ***: $p<0.01$, **: $p<0.05$, *: $p<0.1$.

Table D.6: Heterogeneous effects – Occupational differences

VARIABLES	(1) (baseline)	(2) (1) with amended automation measure
Automatable	0.080	0.104
	(0.074)	(0.075)
Automatable × 2: Professionals	-0.111	-0.140*
	(0.081)	(0.083)
Automatable × 3: Technicians and associate professionals	-0.059	-0.071
	(0.075)	(0.077)
Automatable × 4: Clerks	-0.082	0.318**
	(0.087)	(0.144)
Automatable × 5: Service workers and shop and market sales workers	-0.124	-0.135
	(0.082)	(0.085)
Automatable × 6: Skilled agricultural and fishery workers	-0.367	-0.362
	(0.250)	(0.255)
Automatable × 7: Craft and related trades workers	-0.140*	-0.147*
	(0.080)	(0.082)
Automatable × 8: Plant and machine operators and assemblers	-0.127	-0.128
	(0.080)	(0.085)
Automatable × 9: Elementary occupations	-0.120	-0.156*
	(0.079)	(0.081)
Semi-automatable		0.068**
		(0.032)
Semi-automatable × 2: Professionals		-0.081*
		(0.048)
Semi-automatable × 3: Technicians and associate professionals		-0.044
		(0.038)
Semi-automatable × 4: Clerks		0.363***
		(0.134)
Semi-automatable × 5: Service workers and shop and market sales workers		-0.051
		(0.044)
Semi-automatable × 6: Skilled agricultural and fishery workers		0.015
		(0.098)
Semi-automatable × 7: Craft and related trades workers		-0.043
		(0.038)
Semi-automatable × 8: Plant and machine operators and assemblers		-0.035
		(0.051)
Semi-automatable × 9: Elementary occupations		-0.094**
		(0.044)
Observations	285,346	285,346
Adjusted R^2	0.466	0.466
Within R^2	0.003	0.004

Note: This table shows our baseline estimates on the relationship between working in an automatable job and life satisfaction; however, additionally interacted with 1-digit occupation categories. All other model specifications are identical to the baseline specification reported in table 5.3 (including the full set of covariates). The dependent variable is self-reported life satisfaction. Column (1) uses the original automation measure by Autor and Dorn (2013), with 'non-automatable' as reference category. Column (2) uses the amended automation measure, with 'least-automatable' as reference category. All values are rounded to the third decimal place. Standard errors clustered at the individual level are depicted in parentheses: ***: $p<0.01$, **: $p<0.05$, *: $p<0.1$.

Table D.7: Sub-samples: Blue-collar versus White-collar including age interaction

VARIABLES	(1) White-Collar	(2)	(3) Blue-Collar	(4)
Automatable	0.008	0.026	-0.147***	-0.128***
	(0.029)	(0.036)	(0.043)	(0.048)
Automatable × age 27-55	-0.003	-0.008	0.111**	0.101**
	(0.028)	(0.035)	(0.044)	(0.049)
Automatable × age 56-65	0.015	-0.011	0.118**	0.140**
	(0.037)	(0.044)	(0.058)	(0.066)
Semi-automatable		0.037		0.032
		(0.036)		(0.036)
Semi-automatable × age 27-55		-0.011		-0.020
		(0.036)		(0.039)
Semi-automatable × age 56-65		-0.055		0.034
		(0.046)		(0.052)
Model specifications				
Covariates	YES	YES	YES	YES
Individual-level FE	YES	YES	YES	YES
Occupation (1-digit) FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
State FE	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES
State-time dummies	YES	YES	YES	YES
Observations	184,116	184,116	101,230	101,230
Adjusted R^2	0.473	0.473	0.471	0.471
Within R^2	0.003	0.003	0.005	0.005

Note: This table shows our baseline estimates on the relationship between working in an automatable job and life satisfaction, where the respective underlying models are additionally interacted with age categories (young, middle-aged, older workers). Columns (1) and (2) report estimates for the sub-sample of white-collar workers, whereas columns (3) and (4) report estimates for the sub-sample of blue-collar workers only. All other model specifications are identical to the baseline specifications reported in table 5.3 (with the full set of covariates). Columns (1) and (3) use the original automation measure by Autor and Dorn (2013), with 'non-automatable' as reference category. Columns (2) and (4) use the amended automation measure, with 'least-automatable' as reference category. All values are rounded to the third decimal place. Standard errors clustered at the individual level are depicted in parentheses: ***: p<0.01, **: p<0.05, *: p<0.1.

Table D.8: Automatable jobs and well-being – Effect heterogeneity

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	AD's original automation measure				amended automation measure			
Automatable	-0.018	-0.023	-0.004	-0.012	-0.007	-0.046	-0.004	-0.001
	(0.014)	(0.045)	(0.042)	(0.012)	(0.016)	(0.055)	(0.047)	(0.015)
Automatable × female	-0.002				0.002			
	(0.020)				(0.023)			
Automatable × migrant		0.004				0.041		
		(0.046)				(0.056)		
Automatable × public sector			-0.008				-0.0009	
			(0.022)				(0.025)	
Automatable × skilled				-0.030				-0.026
				(0.025)				(0.027)
Semi-automatable					0.018	-0.045	0.0001	0.018
					(0.013)	(0.044)	(0.041)	(0.014)
Semi-automatable × female					0.010			
					(0.021)			
Semi-automatable × migrant						0.069		
						(0.045)		
Semi-automatable × public_sector							0.012	
							(0.022)	
Semi-automatable × skilled								0.010
								(0.021)
Observations	285,346	285,346	285,346	285,346	285,346	285,346	285,346	285,346
Adjusted R^2	0.466	0.466	0.466	0.466	0.466	0.466	0.466	0.466
Within R^2	0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.003

Note: This table shows our baseline estimates on the relationship between working in an automatable job and life satisfaction, interacted with gender, migration background, sector and skill. Model specifications are identical to the baseline specification reported in table 5.3 (with the full set of covariates). Columns (1)-(4) use the original automation measure by Autor and Dorn (2013), with 'non-automatable' as reference category. Columns (5)-(8) use the amended automation measure, with least-automatable as reference category. All values are rounded to the third decimal place. Standard errors clustered at the individual level are depicted in parentheses: ***: $p<0.01$, **: $p<0.05$, *: $p<0.1$.

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