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The Fiscal and Labour Market Impacts of Return Migration: Evidence from Germany

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Abstract - Throughout the 1990s, Germany offered temporary protection to hundreds of thousands of refugees fleeing the violent Yugoslav wars. Subject to distinctive *Duldung* ("toleration") statuses that were only extended while the wars persisted, these migrants returned from Germany in large numbers from 1995 onward. This paper examines how these return migration flows affected public finance and labour market outcomes at the German county level. Constructing a panel of 400 counties over 8 years (1995-2002), and instrumenting non-German emigration intensity with a measure of geographical distance to the former Yugoslavia, this study finds, overall, limited evidence for significant impacts of refugee emigration on aggregate fiscal and labour market outcomes. However, some results indicate that income taxes per capita and average wages in manufacturing and mining sectors were positively affected. The paper adds to a relatively scarce literature on the fiscal and labour market effects of emigration in high-income countries, and elucidates short-term economic impacts of return migration, while adding perspectives on contemporary debates on refugee returns in Germany.

Keywords: Return Migration, Public Finance, The Labour Market

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AI Disclosure Statement

In the process of writing this thesis, I have utilized some artificial intelligence (AI) tools. More precisely, I have used ChatGPT and Grammarly. The former has assisted with R coding (refining code blocks and flagging errors), German-English translations and Latex formatting, and the latter has been a proofreading tool, correcting grammar and punctuation. All AI outputs have been cautiously examined by me to make sure they are correct, fit their purposes and do not undermine the originality of my own work.

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1 Introduction

Alongside a surge in the number of refugees and forcibly displaced people worldwide, reaching record levels in 2025 (United Nations High Commissioner for Refugees [UNHCR], n.d.), the salience of migration in public opinion has increased in the European Union (Fiorio et al., 2024). Within this domain, return migration has become one of the most topical discussions, particularly post-December 2024 in Germany, following the Assad regime’s fall in Syria (Hammer et al., 2025). At the center of these political debates are often the perceived fiscal costs of hosting refugees (Hennessey & Hagen-Zanker, 2020; Ruist, 2020), and whether to promote return migration or not. In the interest of offering insights into this debate, this study examines both the fiscal and labour market impacts of large-scale return flows on German counties between 1995 and 2002.¹

In the 1990s, Germany provided protection to over 700,000 refugees² from the former Yugoslavia³. These migrants were faced with a distinct and somewhat unorthodox *Duldung* ("toleration") legal status, which meant a life in uncertainty (Dimova, 2006) with de facto restrictions on labour market access (Bosswick, 2000; Liedtke, 2002). Crucially, the statuses were also, in principle, deportation-postponement devices that were only renewed every few months as long as the Yugoslav wars went on (Bahar et al., 2024; Dimova, 2006). So, for instance, when the Dayton Peace Accord was signed and ratified in 1995, signaling peace in Bosnia and Herzegovina, large-scale returns became a possibility for Germany. They capitalized, causing significant emigration flows seemingly not induced by economic policies or conditions. By the turn of the century, most of the Yugoslavian refugees had been repatriated (Bahar et al., 2024).

Due to endogeneity concerns⁴ and data limitations in the migration variable, an instrumental variable strategy is implemented. By instrumenting non-German emigration intensity in German counties with geographical distance to the former Yugoslavia, these concerns are arguably, to a sufficient extent, alleviated. Intuitively, it might seem unconventional to apply such an instrument, but there is a precedent in the migration economics literature for instrumenting migration flows with a measure of distance (Akgündüz et al., 2018; Peri, 2009), and the instrument leveraged in this study is found to yield a strong first-stage correlation with the endogenous emigration variable and to sufficiently withstand the challenges posed to it concerning the exclusion restriction.

This paper contributes to the literature in numerous ways. Firstly, it focuses on the impacts of return migration on fiscal and labour market conditions in a high-income host country, which is fairly unprecedented. Those papers that have previously addressed return migration have either focused on the refugees’ country of origin (Bahar et al., 2024) or inferred impacts on the host countries based on previous literature on immigration (see, for example, Dadush (2018)). Deviating from previous scholarship, this paper offers evidence-based insights that can help in the assessment of the short-term economic impacts of return migration and shed light on more contemporaneous debates.

¹The inclusion of the latter strand of outcomes allows for a discussion on the interconnectedness between the labour market and public finances. And to some extent, complement the somewhat limited availability of suitable dependent variables related to public finance.

²Refugees in this context are those who were forcibly displaced from the former Yugoslavia, and either received an approved asylum status, or a *Duldung* ("toleration") status. Those in the process of seeking asylum are not encompassed by this term.

³The country’s name was officially The Socialist Federal Republic of Yugoslavia (Weller, 1992), but for simplicity, it is referred to as just Yugoslavia. Whenever there is a reference to the "former Yugoslavia", it means the collection of countries/territories (in the form they existed in the 1990s/2000s) that previously constituted the socialist Republic. Additionally, Yugoslav and Yugoslavian will be used interchangeably as the demonym.

⁴There is a persistent concern that the return migration flows and decisions were not entirely unrelated to economic conditions, despite them seemingly being induced by the ending of the Yugoslav wars and the withdrawal of their protection statuses. More specifically, although there were critiques of Germany’s repatriation program and the push/force nature of it (Bosswick, 2000; Dimova, 2006), some form of agency remained with the migrants.

Secondly, it applies a top-down (and close-to-static⁵), rather than an accounting, approach in assessing the fiscal impacts of emigration, which allows for ripple effects linked to consumption and investment to be captured in the estimates. There are papers accounting for such effects and highlighting their importance (Clemens, 2022; Clemens et al., 2018), but none have, to the best of my knowledge, allowed for such effects to be captured in a study of return migration. Thirdly, it makes a contribution by using a large sample of sub-national territorial entities. With some exceptions (see St. Clair (2024)), most papers assess impacts on the national level in studies of migration and public finance.

Lastly, by including outcome variables concerning both public finance and labour markets in the same setting, this study allows for a more evidence-based debate on their interconnectedness. Previous scholarship has on many occasions discussed such interlinkages, but rarely provided results on them in the same study. Through these holistic outlooks, this paper also allows for the elucidation of potential (a)symmetries between immigration and emigration of refugees, by contrasting its findings with previous research.

Analyzing the impacts of emigration intensity in one year on the fiscal and labour market in the next, this study finds, like many public finance studies on *immigration*, insignificant and/or negligible impacts on aggregate public finance outcomes such as fiscal capacity and operating budget expenditures, but an increase in income tax per capita. The latter result is possibly driven by compositional changes in the population: those who leave are assumed to be relatively less endowed with income. The labour market results give a similar picture, as emigration intensity is not found to influence unemployment rates.⁶ However, there is a significant increase in the average wages in the manufacturing and mining (MM) sectors, conceivably explained by a negative labour supply shock. Overall, however, not many of the coefficients are statistically significant, and there are generally large uncertainties on both the holistic and granular impacts of return migration on the German county level.

These findings are generally found not to be driven by mere spatial trends and economic gradients across space or by certain states that had stricter migration policies and distinguished economic conditions.⁷ Followingly, these results are, at least directionally, consistent and can to some extent indicate short-term compositional changes in the population and the labour force (with respect to education and income levels, employability, and participation) that can influence more granular fiscal and labour market outcomes.

They are also consistent with what one would expect based on previous research and the context at hand. Although the results have limited value for the debate on furthering integration efforts versus returns in themselves (as this analysis is not a counterfactual one), they generally show few significant alleviations on the public coffers when return migrations are occurring. Additionally, they can, assuming some legitimacy to the youth unemployment rate estimate (p-value just over 0.05) and lower income level of refugees, highlight the short-term negative consequences of having employment bans and restrictions

⁵It is not fully static (concerned with one fiscal year) as the focus is not on one fiscal year throughout the whole study, but close-to-static in the sense that it analyzes the next-year effects of migration. The implications of this modelling choice are elaborated on in the discussion section (Section 8).

⁶Although the youth unemployment rate has a p-value right above 0.05.

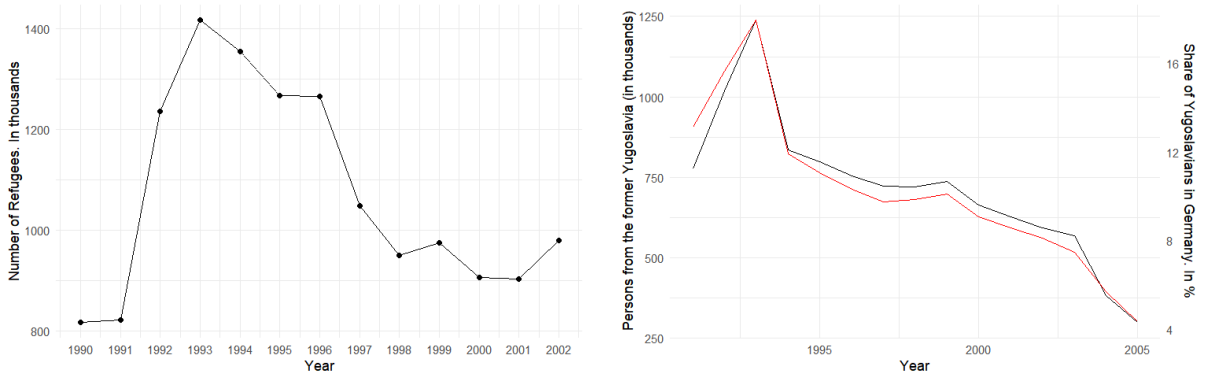
⁷The states in question are Bavaria and Baden-Württemberg, as they had harsher attitudes and policies towards Yugoslav immigrants (Bossick, 2000), and are economically distinguishable from other states in Germany. Excluding them from the sample makes previously statistically significant results insignificant, but the estimates are in the same direction and sometimes larger in magnitude. Given the large drop in sample size and statistical power in such an exclusionary analysis, these estimates are built upon less valuable variation and have large uncertainties. Additionally, removing these states decreases the sample size impactfully, and weakens the first-stage correlation, reinforcing the argument to interpret these robustness check results with caution.

for the migrants, and complement strands of the literature that argue that there are more substantial benefits for host countries to reap with better labour market integration policies than to employ costly deportation options.

The remainder of the paper is structured as follows: Section 2 provides more details on the historical background for the study. Section 3 encompasses the literature review and some brief theoretical considerations. Sections 4 and 5 cover data and the empirical strategy, respectively. The main results are presented in Section 6, followed by a check of their robustness in Section 7. Section 8 discusses the drivers of the results, their implications, and the validity of the study. Lastly, Section 9 contains concluding remarks.

2 Historical background

In the 1990s, the violent and deadly Yugoslav wars unfolded and caused massive international and internal displacement. In the former Yugoslavia, almost 16% of the population of 23,528,230 was displaced, with Bosnia and Herzegovina and Kosovo seeing half of their respective populations move for war-related reasons (Radović, 2005). Germany accommodated a substantial amount of these internationally displaced refugees, hosting about 700,000 Yugoslav refugees (Bahar et al., 2024), of which over 300,000 came from Bosnia and Herzegovina (Dimova, 2006).



(a) Number of refugees (under UNHCR's mandate) and other persons of concern in Germany over time. In thousands. Source: Author's rendering of UNHCR (2025a) data.

(b) Black line (primary axis): Yugoslavians in Germany (in tsd). Red line (secondary axis): Yugoslavian share of the total population. Source: Author's rendering of Rahlf et al. (2015) data.

Figure 1: Refugee and Yugoslavian population developments in Germany

In line with tougher immigration policies in general, exemplified by further restrictions on asylum seekers in the 1980s and 90s (Bosswick, 2000), these refugees were not provided with refugee status under the terms of the 1951 Geneva Convention (Dimova, 2006). On the contrary, they were offered a special *Duldung* ("toleration") status (Bahar et al., 2024; Bosswick, 2000; Dimova, 2006).

This status signified *temporary* protection, and had existed in Germany even before 1990 (Dimova, 2006). The decision to apply this unconventional legal status to most⁸ of the Yugoslav refugees was not

⁸Most of the literature on the area deals with Bosnians in Germany, of whom about 80 percent had this status (Bosswick, 2000). Naturally, there were Yugoslavians fleeing the war who attempted the route through the asylum system in Germany.

merely made by Germany, but by the European Union⁹, with the justification for the decision being that the Geneva Convention was not tailored to a large influx of refugees (Dimova, 2006). For the purposes of this paper, the Duldung status matters, mainly, for two reasons: (1) restricted labour market integration and (2) its deportation-postponement trait that enabled large-scale returns.

Firstly, those who arrived before 15 May 1997 were entitled to apply for work permits for specific jobs, but only if there was no German citizen available for it (Liedtke, 2002). Persons immigrating into Germany after this date, and who were subject to the Duldung status, saw a general employment ban of at least one year, similar to the one that applied to asylum seekers (Bosswick, 2000). Despite these restrictions, they had, unlike asylum seekers, the right to move and settle anywhere within Germany during their time in the country (Liedtke, 2002).

The second, and perhaps most important, point for this study is the way the Duldung status allowed for large-scale returns from Germany, for example, following the Dayton Peace Accord signed in December 1995.¹⁰ Despite the refugee-related formulations of this accord and the emphasis put on voluntariness, the Duldung status, which was only renewed every few months as long as the war in Bosnia continued, allowed Germany to develop an immediate repatriation plan that received criticism for being forceful (Bosswick, 2000; Dimova, 2006). Such returns, arguably forced or at least highly encouraged and pushed onto Bosnian refugees, became viable as the “tolerated” status was not a legal residence status, but rather a deportation-postponement status (Bahar et al., 2024; Dimova, 2006).

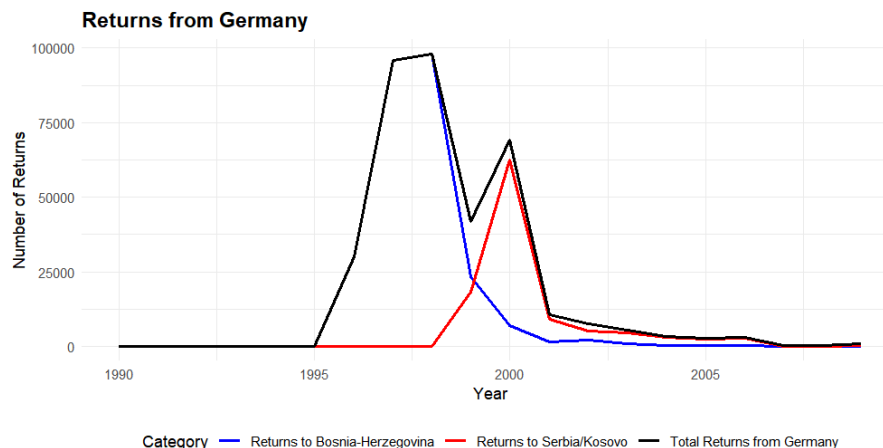


Figure 2: Returns from Germany. In absolute numbers. Source: Author’s rendering of UNHCR (2025b) data.

Resultingly, a large share of the Bosnians and other Yugoslavians residing in Germany in 1995 for humanitarian purposes¹¹ were returned (see Figure 2) in the years that followed. Germany signed, for

Yet, given the tougher asylum policies and the alteration of Article 16 in the base law (Bosswick, 2000), this was often not a viable option for protection. Particularly, if you could not prove that you, as an individual, were under political persecution to be encompassed by Article 16a of the German constitution (Dimova, 2006).

⁹The EU interior ministers introduced a new “temporary” category for the pending inflow of refugees from Yugoslavia, during a meeting in Maastricht in 1991 (Dimova, 2006).

¹⁰The accord was a general framework agreement for peace in Bosnia and Herzegovina between the Republic of Bosnia and Herzegovina, the Republic of Croatia, and the Federal Republic of Yugoslavia (effectively, the country of Serbia and Montenegro (Calhoun, 2000)), containing an annex devoted specifically to refugees and displaced persons (Organization for Security and Co-operation in Europe [OSCE], 1995). In said annex, Article 1 states that all refugees and displaced persons have the right to return, and that the early return is an important objective, calling upon states that have accepted refugees to promote this. The article also specifies that such returns should be voluntary and in line with international law, emphasizing the possibility of using the repatriation assistance frameworks and programs offered by the UNHCR.

¹¹According to Dimova (2006), there were 662,700 labour migrants from former Yugoslavia in Germany in 1990. Many

instance, a readmission agreement with Bosnia in November 1996, which was followed by an intensive repatriation programme launched in 1997 (Bosswick, 2000). According to Bahar et al. (2024), “many” of the 700,000 Yugoslavian refugees in Germany had been repatriated by the year 2000. For Bosnians, the number of returns was about 250,000 until autumn 1998 (Bosswick, 2000). Around the turn of the century, following the Kosovo war in 1998/99, the return flow numbers increased again (red line in Figure 2). These flows were characteristically similar to those of Bosnians, because they were also induced by 180,000 Kosovo-Albanian “toleration”-refugees that resided in Germany in mid-1999 (Bosswick, 2000).

3 Previous research and theoretical considerations

Scrutinizing the literature on migration economics reveals that most studies have examined the labour market effects of immigration, often focusing on voluntary migration. Yet, there is a growing emphasis on the impacts of international forced displacement, both in the public finance and labour market strands of the literature. This paper builds on the research done in this growing field, but also addresses some voids.

Below follows an overview of the key strands in the related literature, where key findings, methods, and potential theoretical considerations are covered. In each, there is also a discussion of how this study contributes to that particular strand.

3.1 Return migration

As the main type of migration examined in this study is return migration of refugees, it is useful to provide some insights into this relevant but scarce literature. A natural starting point is with the study conducted by Bahar et al. (2024), as this study aims to exploit the same return/emigrational “shock” that these authors use. In their analysis, leveraging confidential German administrative data from the SSC system, they perform an industry-level analysis on knowledge diffusion¹² and how the outflows affected the export performance of Bosnia and Herzegovina. Applying a difference-in-differences model, they find that industries in Bosnia with a higher return intensity see 1-1.6% larger exports. They do not, however, utilize the same variation in migration flows to estimate economic or fiscal impacts on Germany, nor its counties.

There has, on the other hand, been work done on the economic and fiscal implications of returning refugees from the host country’s perspective. In a policy research paper, Dadush (2018) asks whether host countries have an economic self-interest in encouraging the return of refugees. To a large extent, he finds that the answer in economically advanced countries today is no, as the economic and fiscal effects of the various types¹³ of immigration seem to be beneficial over the long run, along with the high costs of deportation. In line with papers that deal with immigration of refugees (e.g., Bach et al. (2017), Holler and Schuster (2020), and Ruist (2020)), he argues that the costs of hosting asylum seekers and refugees are front-loaded, while the benefits are back-loaded (i.e., take years to materialize). Such trajectory dynamics have implications for interpretations of research findings, as one might overstate the “benefits” of refugee returns in static analyses (more on this further down) if the refugees have only been in the

of these were not subject to the Duldung status, and did not see the repatriation pressure being applied to them following the Dayton agreement.

¹²Return migration is also often linked to circular migration and knowledge remittances and diffusion (see, for instance, Di Iasio and Miguelez (2022)). However, these migration flows are often voluntary in nature.

¹³Encompassing migrants with various legal statuses. i.e., economic/labour migrants, refugees, (un)successful asylum seekers and irregular migrants.

host country for a short period of time.¹⁴

Overall, this strand of literature has not seen many studies making use of a migration shock, and those that exist often pertain to development indicators in the country of origin (i.e., the destination of the return migration) and changes in their industrial composition, rather than fiscal or labour market outcomes in the host country. This study can address this void, and also provide more explicit empirical evidence on the linkages between public finance and return migration rather than relying on theorizations and conjectures based on the immigration literature, as was the method of Dadush (2018).

3.2 Public finance

As migration has become more politicized and controversial (Kaczmarczyk, 2015; Vargas-Silva, 2015), there has been a growing focus on the impact of migration on public finances. While one might intuitively expect significant fiscal impacts of migration, the research shows that this is rather ambiguous and often context-dependent, with often small and negligible effects (Dadush, 2018; Hennessey & Hagen-Zanker, 2020; Kaczmarczyk, 2015).¹⁵

Many authors aim to say something about the net fiscal contributions of various migrant groups (Hennessey & Hagen-Zanker, 2020; Vargas-Silva, 2015). But whether the studies find that migrants add more to the public coffers than they draw from them, or vice versa, is dependent on the host country and migrant characteristics, and the modelling choice made in the research designs (Clemens, 2022). A theoretical framework that aims to capture the first two factors is shown in Figure 3.

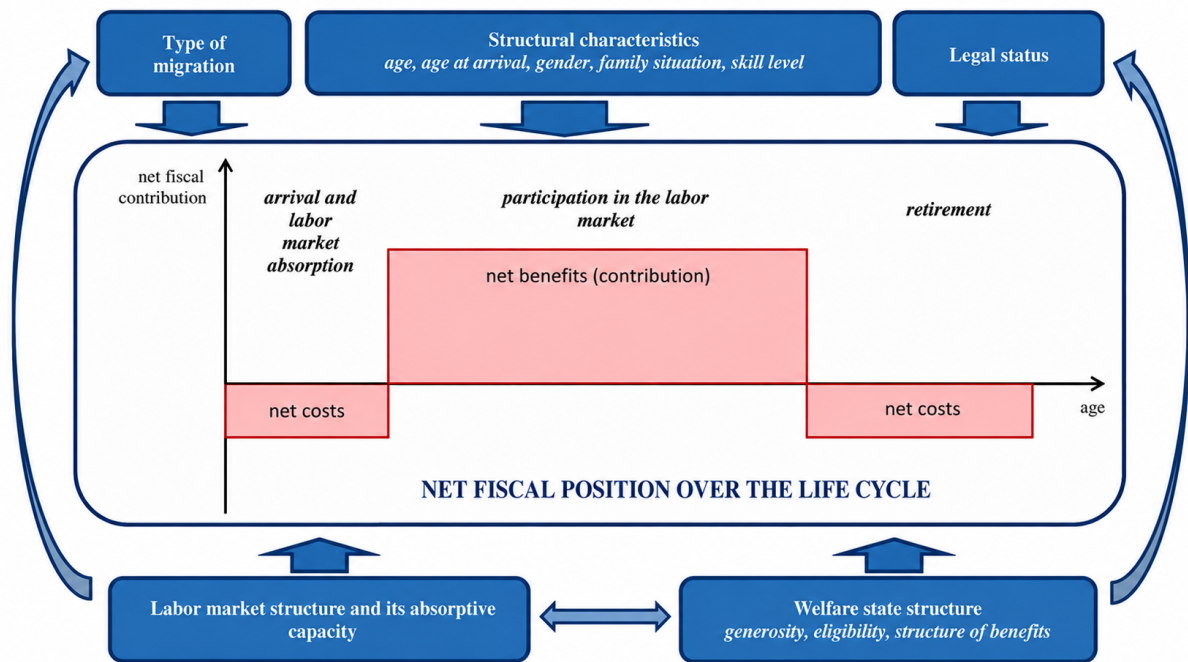


Figure 3: Net fiscal position influencing factors across the life cycle. Source: Kaczmarczyk (2015)

The figure shows how the net fiscal contributions of immigrants can evolve over the life course, and which factors are typically important determinants. Those factors that are found to matter the most are legal

¹⁴As they can still be in the stage of the life-cycle (see Kaczmarczyk (2015) figure) where they are net fiscal beneficiaries, and “removing” them from the population will positively affect the fiscal indicators in focus.

¹⁵Particularly when it is not decomposed based on legal status, demographics, etc.

status (Kaczmarczyk, 2015), labour market access (Clemens et al., 2018; Marbach et al., 2018), and the structure of the welfare state (Dadush, 2018; Kaczmarczyk, 2015). Refugees can often see restrictions (due to their legal status) on their labour market access, or at least their possibilities to find appropriate, well-paid employment opportunities (Dimova, 2006), potentially causing them to end up as net beneficiaries in generous welfare systems.¹⁶ That is particularly evident if the amount of time spent in the host country is low, as per Figure 3. Other migrant groups often have a higher propensity to see positive net fiscal contribution results in analyses conducted on them (see, for example, Fiorio et al. (2024)).

This timing aspect is essential in a number of ways, but becomes particularly evident in modelling and research design questions. The two main approaches are static and dynamic. While the first often seeks to analyze the fiscal impacts of migrants in a single fiscal year, the latter aims to incorporate an intertemporal (and sometimes intergenerational) feature into the analyses through net present value calculations (Hennessey & Hagen-Zanker, 2020; Vargas-Silva, 2015). Consequently, if you apply a static approach, one risks having results overly dependent on which stage the examined migrant group is at in the Kaczmarczyk life-cycle model in the analysis time-frame, exemplified by the large public finance deficit of 40 billion SEK Ruist (2020) finds for refugees in the 2014-/2015-waves into Sweden.

Nonetheless, doing dynamic analyses on refugees (Bach et al., 2017; Holler & Schuster, 2020; Ruist, 2020) and non-decomposed migrant groups (Chojnicki & Ragot, 2016; Storesletten, 2003) requires many assumptions on demographics, policies, education levels, time-consistency of labour market structures, which accounts (revenues and expenditures) to include and through which channels migrants affect the economy. Resultingly, the researchers have many degrees of freedom, and findings can vary substantively for this reason.

By applying a top-down approach that does not require many assumptions on modelling or which accounts to include in the analysis, which would be required in an accounting model, the degrees of freedom are restricted, and there are no limitations to the channels through which the migrants can affect public finance outcomes. Therefore, there is a possibility to capture potential ripple effects, i.e., not account-specific investments and consumption in various markets, that can stimulate the economy (Alix-Garcia et al., 2018) and influence fiscality (Clemens, 2022; Clemens et al., 2018; Dadush, 2018; Labanca, 2020). Although at the expense of information on how these effects play out.

This study can contribute to this strand of literature by delving into emigration rather than immigration, and, for example, shed light on whether the theoretical considerations offered in the literature also apply here, presumably with a flipped sign, as the direction of migration is switched. Those studies that have dealt with emigration and public finance are often related to remittances (Campos-Vazquez & Sobarzo, 2012) and/or outward labour migration (Desai et al., 2009; Ivandić, 2023). Thus, this study can help fill the gap in the linkages between public finance and emigration that is not economically induced.

Furthermore, this paper offers insights into fiscal impacts at a sub-national level. Some scholarship has assessed the heterogeneous impacts across various levels of administration/government, like, for instance, St. Clair (2024), when he revisits the Mariel boatlift¹⁷, but most research has focused on national effects. While this study cannot assess such heterogeneity, knowing that only the county level is assessed, it can help develop an understanding of the dynamics at more granular levels of administration/government.

¹⁶There are often more positive fiscal impacts of refugee immigration in a country like the U.S. (see Clemens (2022)) compared to, for instance, Sweden (Ruist, 2020), Germany (Bach et al., 2017) or Austria (Holler & Schuster, 2020).

¹⁷Known case study in the migration economics literature, covering the migration shock when many Cuban (and Haitian) migrants set foot in Miami in 1980.

Lastly, as this study also includes labour market outcomes, it can purposefully discuss the interconnectedness between public finance and the labour market using the same “migration shock” context and model. Papers on migration and public finance often discuss these interlinkages (see, for example, Clemens et al. (2018), Clemens (2022), Ruist (2020), and Dadush (2018)). Still, they rarely include joint results and findings in these.

3.3 Forced migration and the labour market

The final main strand of literature inspected is the one on the labour market effects of forced migration. While the field of migration economics, historically, has often treated migrants as one homogenous group, there is now increased dedication to asylum seekers and refugees in academia. Accounting for such heterogeneity between migrant groups is important for policymaking and external validity of migration analyses (Becker & Ferrara, 2019; Ruiz & Vargas-Silva, 2013).

Forced migration studies occur quite often, as using large episodes such as wars, civil conflicts, and disasters, offer exogenous variation in migration flows that can be exploited for causality (Becker & Ferrara, 2019). However, as for public finance outcomes, the effects seem to be mixed, context-dependent (ibid.) and, on average, small (Clemens et al., 2018). For host communities, the winners of a large influx of forced migrants are often those who can exploit the surge in labour supply the best, as well as producers who supply markets with essential goods (Ruiz & Vargas-Silva, 2013). On the contrary, losers are often found to be those people in the labour force who have similar levels of education and/or skill set as the new workers. As such, results rely heavily on the substitutability of workers (Becker & Ferrara, 2019).

Through the formalization of a theoretical model surrounding local adjustments to labour supply shocks driven by *immigration*, Monras (2021) assumes that in the short run, the whole labour supply shock is absorbed through prices (wages and rental prices) rather than through employment channels. In the longer run, these prices are assumed to move back to a pre-shock level. In this framework, while not formalized explicitly, substitutability plays a key role (reinforcing the notions of other scholarship) in the sense that those affected the most in terms of seeing wage and employment changes are those with the same education levels (often referred to as “skill sets” in the literature) as the immigrant group causing the shock (Monras, 2021). Using these theoretical considerations, one can conjecture symmetric results for emigration and a negative labour supply shock, with the only difference being the sign change.

Determinants of other labour market effects, such as (un)employment rates, include policy factors. Formal labour market access (LMA) is not something that refugees and other forced migrants¹⁸ can take for granted and is found to be very influential for labour market absorption and participation in the short- and long-run (Clemens et al., 2018; Marbach et al., 2018). Examining employment bans on asylum seekers from the former Republic of Yugoslavia (mostly from Kosovo) in Germany at the turn of the century, Marbach et al. (2018) find that reducing the length of an employment ban can yield substantial benefits for both Germany and the migrants themselves. These benefits are shaped through increased economic integration, growth in the employment rate, the realization of potential tax revenues, and the foregoing of certain welfare expenditures.

Another Germany-specific study, conducted at the county level, reveals that the surge in Syrian and

¹⁸Especially not asylum seekers in Germany (see Marbach et al. (2018)).

Iraqi migration inflows actually caused a slight decrease in total unemployment, both for low- and high-immigration counties (Gehrsitz & Ungerer, 2022). However, other non-German citizens saw a rise in their unemployment rate, perhaps again indicating the importance of changes in the labour force’s complementarity.

Resembling the public finance literature summary and the contributions of this study relative to the strand, there is a scarcity of explicit analyses that deal with emigration from high-income countries and their labour market impacts. Most studies that look at the linkages between emigration and the labour market have a “brain drain”¹⁹ perspective, or look at effects on sending populations that are mostly located in low-income countries.

Also here, the interconnectedness between the fiscal and labour market impacts comes into play. Like the fiscal strand, few (if any) papers include results on public finance outcomes in their labour market studies, despite their discussions of the implications of their results for fiscality. More broadly, as a consequence of this, there is a possibility of providing a more holistic and evidence-based discussion of these interdependencies in a study like this one.

4 Data

To obtain data on migration, fiscal and labour market variables, I mainly employ two German databases²⁰: *Regionaldatenbank Deutschland* (Statistische Ämter des Bundes und der Länder, 2026) and *INKAR – Indikatoren und Karten zur Raum- und Stadtentwicklung* (Bundesinstitut für Bau-, Stadt- und Raumforschung, 2026). The key variables used in the analysis are summarized in Table 1.²¹

4.1 The independent variable and instrument

Importantly, the data on migration does not contain the nationality, nor the destination for the migratory movement. Subsequently, one cannot with full certainty say that the emigration of non-Germans abroad, which is the main migration variable applied in this study, is linked with the return migration of the former Yugoslavians. Firstly, one cannot show that they are from the former Yugoslavia. Secondly, the emigrants can go to other places than back to the former Yugoslavia, for instance, through resettlement in a third country. Jointly, this can exacerbate endogeneity issues in an OLS identification strategy, as the main independent variable contains emigration that is not linked to the war endings in the former Yugoslavia, but rather due to economic conditions. Such issues are very common in studies of migration economics (Becker & Ferrara, 2019) and are partly due to the field’s historical lack of solid and granular data points, especially on return migration (Dadush, 2018). The IV approach undertaken attempts to extract the “exogenous” variation of this outward migrant flow from German counties, but more on this can be located in the empirical strategy section (Section 5).

¹⁹Brain drain refers to the departure of skilled and talented people for economic reasons (Becker & Ferrara, 2019).

²⁰Unlike Bahar et al. (2024), who have access to confidential German administrative data, I cannot place Yugoslavian refugees in various establishments in their respective regions. Hence, I need to make use of more aggregate migration variables that suffer from a lack of granularity.

²¹The control variable descriptions should be, to a large extent, satisfactory in the table. Hence, they are not offered a subsection like the others. There is, however, one thing to note on the urban area dummy. This dummy is coded using the administrative definition of urbanity. This does not mean that other counties, i.e., those that are not encapsulated by the pool of counties with a 1-encoding, cannot have municipalities or smaller cities/villages within them with urban area dynamics.

Table 1: Variable descriptions and sources

Variable	Description	Source
<i>Independent variable and instrument</i>		
Emigration intensity (EIA)	Emigration of non-Germans per 1000 inhabitants. Here, emigration is international, not intranational, meaning that those encompassed by these statistics are leaving Germany as a whole, not just their respective counties.	Regionaldatenbank Deutschland: Table 12711-03-02-4-B (Emigration) and Table 12411-03-02-4-B (Population data).
Geographical distance	Distance, in km, from the former Yugoslavia. Calculated using centroid coordinate data from shapefiles.	German counties: Bundesamt für Kartographie und Geodäsie (2025). Former Yugoslavia: Sevdari and Marmullaku (2023).
<i>Dependent variables</i>		
Fiscal capacity per capita	A measurement of tax-raising potential, under standardized conditions (constructed using uniform tax rates across municipalities/counties).	The INKAR database.
Income tax per capita	Average tax revenue received from the residents' incomes.	The INKAR database.
Operating budget expenditures	All non-investment/capital spending in the municipalities' ^a budgets. Includes social transfers and services.	Regionaldatenbank Deutschland. Table 71517-02-03-4.
Average MM wages	Gross wages in manufacturing and mining sectors (in tsd Euros) divided by the number of employees in these sectors.	Regionaldatenbank Deutschland: Tables 42111-05-01-4-B (wages) and 42231-01-04-4-B (employment)
Unemployment rate (1998-)	Share of unemployed persons in the labour force. In percent.	The INKAR database.
Youth unemployment rate	The share of unemployed individuals aged 15-24 relative to the age group's population. In percent.	The INKAR database.
<i>Control variables</i>		
GDP	Gross domestic product, measuring the total value added by economic activities in the county.	The INKAR database.
Manufacturing and mining (MM) employment share	Number of employees in the MM sectors divided by the total number of employees in the county.	Regionaldatenbank Deutschland 42231-01-04-4-B for MM employment. The INKAR database for total employment data.
Population	Number of inhabitants in a county.	Regionaldatenbank Deutschland. Table 12411-03-02-4-B.
Non-German share of the population	Number of non-Germans (determined by country of origin) inhabitants divided by the total population.	Regionaldatenbank Deutschland. Table 12411-03-02-4-B.
Long-term unemployment share	The share of long-term (> 1 year) unemployed persons among all unemployed individuals. In percent.	The INKAR database.
Urban area	A dummy variable, where it takes the value 1 if the county is "urban" ^b and 0 otherwise.	The INKAR database, the Regionaldatenbank Deutschland, and Table 11111-0002 from the Statistisches Bundesamt (2026) GENESIS-Online database.
Rate of natural increase (RNI)	Difference between births and deaths per 1000 inhabitants.	The INKAR database.
Young-to-old ratio (working-age population)	Ratio of young (15-19 y.o) to older (60-64 y.o) working-age individuals. In percent.	The INKAR database.

Notes: Wherever Regionaldatenbank Deutschland is mentioned, see Statistische Ämter des Bundes und der Länder (2026) in the bibliography. Similarly, for data coming from INKAR, see Bundesinstitut für Bau-, Stadt- und Raumforschung (2026).

^aAlready scaled up to the county level in the database.

^bFollowing the administrative definition of an urbanized area (i.e., being a "kreisfreie Stadt").

Furthermore, the instrument is based on geographical coordinates retrieved from shapefiles on German counties (Bundesamt für Kartographie und Geodäsie, 2025) and European countries (Sevdari & Marmullaku, 2023). With the centroid coordinates of each county and the coordinates of the geographical midpoint in the former Yugoslavia (located close to Sarajevo), the geographical kilometer (linear) distance between them is calculated.

4.2 Main dependent variables

Even though all the dependent variables are briefly summarized in Table 1, some of them could use some further elaboration to preserve clarity and transparency. One of them is the fiscal capacity measure. Focusing on the revenue side of the public finances, this variable aims to capture the potential for tax revenues, irrespective of tax policy. In the calculations, to ensure cross-county comparability, all municipalities and counties have the same tax rates for various sources of revenues (VAT, income tax, business tax, property tax, etc.). Also, intergovernmental transfers are excluded. Effectively, this is not only a measure of potential fiscal strength, but it will also capture variation in economic strength, economic activity, and business environment (through its impact on firm profits).²²

While the income tax per capita is closely tied to actual local income levels, it should be noted that this statistical measure is somewhat based on statutory sharing formulas, distributing to the municipalities (and hence counties when aggregated) their respective shares of national income tax revenues. Like the fiscal capacity outcome, this variable pertains to the revenue side and will potentially capture some variation and changes in economic conditions, such as general income levels, industrialization, and levels of business burgeoning. In contrast, the operating budget expenditure variable can conceivably be less dependent on such fundamental economic conditions (or at least changes in them from one year to the next), while it also covers the other side of public budgets. The downside of having this as an outcome is the level of aggregation. More granular analyses could have been performed if data on specific social expenditures and transfers (such as unemployment benefits) had been available at the county level, but unfortunately, such data were not obtainable.

Switching focus to the dependent variables that are more explicitly linked to the labour market, it is worth mentioning that the average wage variable is constructed using two different tables from the Regionaldatenbank Deutschland that phrase the economic sectors slightly differently. When scrutinizing the definitions, however, they mostly seem to cover the same sectors. Besides, when reviewing this variable in the sample, the mean is 30 thousand euros with a standard deviation of less than 6 thousand euros, which does not seem implausible in that time period. It is also constructed the same for all counties, so even if there is a mismatch between the numerator and denominator, it could still function as a comparison tool of industrial wages between counties.

The respective unemployment rates are fairly straightforward, as they follow general and consensus-based definitions in the field. Yet, due to a lack of data on the more granular levels, figures on the size of the labour force (for the regular unemployment rate) are partly estimated, with a risk of having measurement errors.

²²As can be seen in the empirical strategy section, one aims to control for such baseline conditions by including fixed effects and appropriate controls to isolate the *changes* in emigration intensity that can be determining of this variable.

4.3 The final panel

The final panel dataset consists of 400 counties and 8 years (1995-2002). For some data tables, there were many more than 400 counties listed, due to the pre-territorial reform number of entities that existed in Germany during the time of analysis. Because the Regionaldatenbank offers data points for the 5-digit county ID codes in existence at the time of the recording, and the INKAR database reports for those that were in effect as of 31 December 2023, a choice to harmonize the counties and only keep 400 was made. This harmonization process, in addition to other data management operations, is comprehensively summarized in Appendix A.1.

The time period was set because no data before the year 1995 was available, and the emigrational waves lasted (particularly for Kosovars) until the first years of the new millennium. The “shock” was more intense in the close aftermath of the Dayton peace agreement and post-Kosovo war (see figures 1 and 2), but as a way to increase statistical power and sample size, the first years of the millennium are included.²³

Table 2: Descriptive Statistics

	Mean	SD	Median	Min	Max	N
Emigration intensity (EIA)	5.88	4.23	4.80	0.06	50.10	3200
Geographical distance from the former Yugoslavia	6.89	0.18	6.90	6.39	7.22	3200
<i>Main dependent variables</i>						
Fiscal capacity per capita	6.03	0.40	6.11	4.81	7.20	3192
Income tax per capita	5.41	0.51	5.57	4.02	6.28	3200
Operating budget expenditures	19.06	0.73	18.97	17.60	21.95	3153
Youth unemployment rate	4.93	1.87	4.65	0.97	10.81	3200
Unemployment rate (1998-)	9.77	4.71	8.30	2.63	24.93	2394
Average MM wages	3.40	0.19	3.41	2.85	4.03	2355
<i>Main controls</i>						
GDP ₁₉₉₅	14.96	0.75	14.87	13.46	18.24	3200
Manufacturing and mining (MM) employment share ₁₉₉₅	0.18	0.09	0.17	0.02	0.50	2744
Population ₁₉₉₅	11.98	0.63	11.92	10.49	15.06	3200
Non-German share of the population ₁₉₉₅	7.89	4.85	7.18	0.12	26.33	2928
Long term unemployment share ₁₉₉₅	28.61	6.68	28.73	10.88	48.59	3200
Urban area	0.24	0.43	0.00	0.00	1.00	3200
Rate of natural increase (RNI)	-1.14	2.56	-1.01	-9.57	7.11	3200
Ratio of young to old working-age population	90.41	15.57	89.14	54.77	164.10	3200

Notes: Summary statistics for key variables used in the main analyses. These statistics are only from the main sample years, i.e., 1995-2002. Geographical distance, GDP, employment in mining and industry, fiscal capacity, income tax, budget expenditures, MM wages, and population are all log-transformed. Shares, rates, and ratios are in percent (i.e., on a scale of 0-100 and not 0-1). The 1995-specific baseline control variables have, unlike the others, only cross-county variation (within 1995).

5 Empirical strategy

This study aims to assess and evaluate the fiscal and labour market impacts of outflows of non-Germans from German counties during a time period where the country saw a profound surge in these migratory movements. Using natural experiments, like this, is not uncommon in studies in the field of migration economics (Bahar et al., 2024; Gehrsitz & Ungerer, 2022; St. Clair, 2024). The reason is that the independent variable of migration often is endogenous, and choosing events with significant shocks is a way

²³Also, inspecting the F-statistic plot (Figure 5) below shows that the instrument turns weak quickly after the year 2002. The same pattern is shown if you do this plot for single years (without year FEs, though) instead of time windows.

to allow for the exogenous part of the variation to become more prevalent and extractable. The ending of the Yugoslav wars, combined with the unorthodox “toleration” status of the Yugoslav refugees in Germany, which allowed for strong return/deportation pushes, created emigration flows that are suitable for research purposes.

Rather than using an approach that requires a substantial amount of strong assumptions on how migrants affect the labour market and the fiscal budgets, like accounting and dynamic approaches do (Hennessey & Hagen-Zanker, 2020; Vargas-Silva, 2015), a more static and top-down approach is applied. As such, the chosen econometric model can capture ripple effects in the economy that impact budgets and economic outcomes through key markets (St. Clair, 2024) and through demand and investment channels (Clemens, 2022).

On the downside, however, the model cannot capture the intertemporal aspect of migration effects, and they are sensitive to which migrant and host characteristics (Hennessey & Hagen-Zanker, 2020; Vargas-Silva, 2015) were at play in this particular case of former Yugoslavians in Germany. Likewise, too few breakdowns of the variables can mask potential heterogeneity in effects (Becker & Ferrara, 2019).

5.1 The Ordinary Least Squares (OLS) model

A top-down approach using this natural experiment setting can be condensed into the following structural equation:

$$Y_{c,t} = \beta EIA_{c,t-1} + \gamma \mathbf{Z}_{c,t-1} + \theta \mathbf{C}_c + \alpha_s + \lambda_t + \epsilon_{c,t} \quad (1)$$

where Y is the fiscal/labour market outcome for county c at time t , and emigration intensity (measured as emigrants per 1000 inhabitants) for non-Germans going abroad ($EIA_{c,t-1}$) is the main explanatory variable. Due to the nature of the geographical distance instrument, which is time-invariant, there cannot be county fixed effects included in this specification, as all the distance variation would be absorbed in the first-stage estimations. Resultingly, identification relies more heavily on other controls.²⁴

Vector $\mathbf{Z}$ contains structurally slow time-variant controls²⁵, such as the percent ratio of young (15-19) to older (60-64) working-age population and the rate of natural increase (birth rate minus death rate), to condition β on general demographic development. Vector $\mathbf{C}$ contains controls aimed at compensating for the lack of county fixed effects by including baseline characteristics of each county in 1995 in the regressions. These include the population, GDP, the share of employment in manufacturing and mining sectors, the non-German share of the population, an urban area (kreisfreie Stadt) dummy, and the long-term share of unemployment. The first three are to control for economic size and level of industrialization, and the fourth is to control for initial (pre-shock) migrant levels. The fifth and sixth are to control for differences between urban and rural areas and baseline labour market conditions/strength. To account for disparities in (unobservable) characteristics between regions in Germany that are not captured by the controls, state fixed effects (α_s) are included.

Likewise, there are year fixed effects (λ_t) incorporated into the model. This is to control for common (national) macroeconomic shocks that might occur in a given year. Standard errors are clustered at

²⁴A sensitivity analysis on the inclusion of controls can be located in Appendix A.3.

²⁵Structurally slow means that the variables do not respond quickly or with great magnitude to shocks but adjust over time. Although it is possible to argue that the migration shocks in the magnitude order seen in this study can affect the demographic composition in the working-age population in counties, especially if Yugoslavians were mostly young migrants, there is still legitimacy for the claim that demographic patterns in each county are more deeply established and sticky.

the county level.²⁶ The time subscripts, t and $t-1$, indicate that the coefficient of interest, β , measures how one additional non-German emigrant going out of Germany (per 1,000 inhabitants) in one year affects the fiscal, budgetary, and economic conditions of the county in the next. The reason for this specification is that the effects of emigration are likely not to materialize instantaneously, but with a delay.²⁷

There are, despite ample controls and fixed effects, concerns about the exogenous nature of the explanatory variable, and the subsequent bias on the β -coefficient. These concerns stem both from the literature, where migration variables are notoriously endogenous (Becker & Ferrara, 2019; Gehrsitz & Ungerer, 2022; Marbach et al., 2018), and the Yugoslav outflow characteristics at hand. The second point relates to the determinants of the return flows. They are arguably a function of the refugees' agency and the extent to which local governments can influence the speed of the returns. Since not all refugees were expelled immediately, nor at the same time, there were potentially some migrants taking economic and political considerations into account in their return/emigration *decisions*.²⁸

There is a possibility to argue that regressing the outcomes on a lagged regressor might alleviate some of these endogeneity concerns, as it is more challenging to substantiate the argument of reverse causality or simultaneity when you analyze the impact of something in the past on something in the present. Unfortunately, there are still endogeneity concerns persisting, as emigration intensity in one year might be correlated with the economic and fiscal environment in the next. For instance, if people who decide to migrate out of Germany (and hence, also out of their county) for reasons related to their anticipations of what their economic prospects will be in the next year, lags are not enough to deal with the endogeneity in the regressor.

In the context of this analysis, the main biases come from the aforementioned simultaneity and reverse causality concerns, connected to the potential omission of local (i.e., county-level) economic shocks that affect emigration rates and are naturally connected to the economic/fiscal outcome variables (again, reinforced concern as the endogenous regressor is very general and not applied to Yugoslavians specifically). Moreover, it could be a concern if policy decisions (especially in the South of Germany (see Bosswick (2000)) that aim to induce the returns of the former Yugoslavians are impacted by or could alter the environment in which changes in the public budgets and labour markets take place. Introducing an instrument that plausibly exploits the exogenous²⁹ ending of the Yugoslav wars and the distance element to migration decisions/forces can conceivably address these concerns.

5.2 The instrumental variable (IV) approach

In order to overcome the shortcomings of the OLS design, an instrumental variable (IV) approach is undertaken. A sound IV approach, as attempted in this study, can split the variance of the endogenous emigration intensity variable into exogenous and endogenous components, where the exogenous part is used in the estimation (Bound et al., 1995). To achieve this, it does, however, require a strong instrument

²⁶As there are sufficiently many of them, and county errors are likely serially correlated over time.

²⁷There are, however, reservations about the timing of the emigration within a year. It is virtually impossible to determine at what time in the year people emigrated with the data at hand (the frequency of all candidate migration variables is yearly). Consequently, if people left at the end of year t , the effects could first materialize in year $t+2$, assuming this stickiness is rooted in reality.

²⁸There is inherent simultaneity of migration decisions and economic/fiscal/political conditions. This issue is exacerbated when you consider the nature of the migration variable, as it can capture non-Yugoslavians going abroad, who might move for economic reasons.

²⁹The ending of the wars, for example, the signing of the Dayton Peace Accord, cannot convincingly be argued to be dependent on local or regional economic conditions in Germany.

that only determines fiscal and economic outcomes through its impact on emigration intensity. To put it differently, the instrument (z) needs to be relevant ($Cov(z_c, EIA_{c,t}) \neq 0$) and satisfy the exclusion restriction ($Cov(z_c, \epsilon_{c,t}) = 0$, i.e., be exogenous with respect to the error term in the structural equation). If these assumptions hold, the IV estimator can consistently estimate β . The instrument used in this study is (log) geographical distance from the different counties to the geographical midpoint of the former Yugoslavia.

5.2.1 The main IV specification/model

The second stage:

$$Y_{c,t} = \beta E\hat{I}A_{c,t-1} + \gamma \mathbf{Z}_{c,t-1} + \theta \mathbf{C}_c + \alpha_s + \lambda_t + \epsilon_{c,t} \quad (2)$$

The first stage:

$$EIA_{c,t} = \phi \log(\text{geoDistance}_c) + \zeta \mathbf{Z}_{c,t} + \omega \mathbf{C}_c + \sigma_s + \tau_t + u_{c,t} \quad (3)$$

Here, the second stage is essentially the structural equation explained in the OLS design, but where emigration intensity is predicted with log distances, and the first stage is the endogenous regressor on the left-hand side being instrumented by (log) geographical distance from a county c to Yugoslavia. The first stage includes the same controls and fixed effects to help ensure consistency of the estimator. The geographical distance is log-transformed to improve interpretation, reduce the leverage of outliers, and to accommodate the key consideration that migration sensitivity does not diminish linearly with distance.

The underlying idea of this instrument is that counties located closer to the former Yugoslavia saw a higher propensity for emigration in the migration shock period, and that geographical distance from German counties to Yugoslavia is itself uncorrelated with county-specific economic and fiscal outcomes, conditional on controls and aside from its impact on emigration intensity.

One could have argued that the Königsteiner Schlüssel, a spatial allocation mechanism for asylum seekers, would be a more appropriate instrument, given its prevalence in migration economics papers on Germany (Bahar et al., 2024; Gehrsitz & Ungerer, 2022) and its inherent element of randomization. However, given the special *Duldung* status of Yugoslavians in Germany that allows for free intranational movement (unlike for asylum seekers) return migration is likely asymmetric compared to immigration destination and settlement. Hence, the distance instrument has some clear advantages.³⁰

Throughout the 20th century, before the Yugoslav wars, there was extensive labour migration from Yugoslavia to the states of Bavaria, Baden-Württemberg, and North Rhine-Westphalia (Molnar, 2014), creating Yugoslav migrant networks in the Southern/Southwestern parts of Germany (see Appendix A.2 for the persistence of these networks). As migrant networks are key determinants of migration patterns (Nishimura & Czaika, 2024), it can reasonably be conjectured that the right-to-free movement *Duldung*-holders sought out these places, and saw their departures occur from these areas. While this argues in favor of the distance instrument at the expense of asylum seeker allocation mechanisms, due to the asymmetry of initial resettlements versus departure locations, it raises endogeneity concerns. These can stem from omitted variable bias if results are driven by economic conditions that are systematically different in these network-exposed areas versus the rest of the country. However, this is found not to be that worrisome when the control vector is extended to incorporate a control for smooth spatial trends in

³⁰Other than being feasible to retrieve, unlike the spatial allocation key (Schlüssel), which is determined at the state level.

the robustness checks section.

Further endorsement of the instrument can be located in the migration economics literature, through its prevalence (Akgündüz et al., 2018; Del Carpio & Wagner, 2015; Peri, 2009)³¹, and the corresponding argumentation of such instruments' validity (Clemens, 2022). Finally, in the literature on push and pull factors, it is argued that migration, and in particular, return migration Dadush (2018), is more likely to happen with short distances (Nishimura & Czaika, 2024).

5.2.2 The relevance assumption

The first main assumption on relevance can be empirically tested, for example, through estimations of first-stage F-statistics on the excluded instrument. These F-statistics are reported for all specifications in the results section and indicate sufficient strength in the instrument (all are way above 10, which is the Stock-Yogo rule-of-thumb threshold for instrument strength (Stock & Yogo, 2002)). Evident from the first-stage results reported in the next section is also that the coefficient on the instrument is negative and statistically significant as hypothesized, unlike in the 2006-2013 period. This suggests that, between 1995 and 2002, larger distances implied relatively lower emigration intensities.

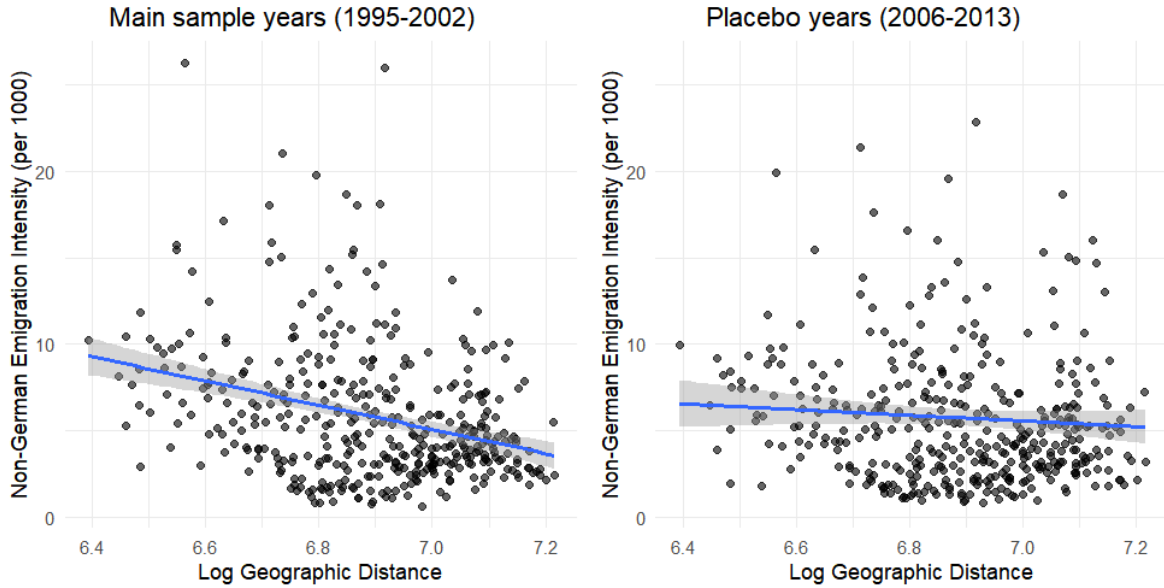


Figure 4: Emigration intensity of non-Germans going abroad versus geographical distance from the former Yugoslavia in two different time periods. One with the hypothesized shock (left panel) and one “placebo” period (right panel). There are 400 data points in each scatter plot, representing each of the counties' average non-German emigration intensity in the respective time periods.

5.2.3 The exclusion restriction

Conversely, when moving on to the exclusion restriction, it cannot be tested in the same way. For the assumption to be violated, the instrument would have to be a determinant of economic and fiscal outcomes, other than through its impact on emigration flows. Such threats do exist and require further

³¹Should be noted that the Del Carpio paper uses geographical distance from the Syria-Türkiye border as a baseline for inflows of Syrians into Türkiye, and that their identification relies on *deviations* from this.

addressing. These can mainly be grouped into time-invariant correlations or period-specific concerns.

i) Category 1: Time-invariant correlations

It is not unrealistic that there are time-invariant conditions at hand that might lead to correlations between the instrument and the outcome variables that do not channel through the emigration intensity variable.

Firstly, there is spatial heterogeneity within Germany with respect to industrial compositions and labour markets (Lotfalipour, 2025), which might be reflected in the outcome variables examined in this paper. If counties closer to the former Yugoslavia differ systematically from those further away with respect to public finances, labour market structures, migrant networks, and industrial composition, the estimates may be confounded if they are not properly handled. State fixed effects can at least partially address this issue. By conditioning on time-invariant state characteristics, cross-state heterogeneity is less concerning. Systematic differences within states are, on the other hand, not handled by these fixed effects. For such differences, there are controls for economic size, urbanization, initial migrant shares of the population, and labour market strength. Additionally, in the section on robustness checks, there are specifications where latitude- and longitude-based spatial controls are included, explicitly furthering evidence that the results are not driven by mere spatial trends or economic gradients across space.

Secondly, there is a potential threat to identification if time-invariant migration flows to or via other countries, such as Türkiye and Austria, are driving the instrument's validity. Persistent migration corridors that penetrate the same spatial regions could violate the exclusion restriction if they make counties closer to the former Yugoslavia be differentially exposed not only to the examined shock, but also to overarching migration dynamics in the region that could, independently, impact economic outcomes in the host areas.

If such (relatively) time-invariant corridors were largely meaningful, they would lead to F-statistics being roughly constant over time. However, by looking at Figure 5 below, where the F-statistics are plotted for different time windows, the migration shock years as described in this study (1995-2002) have a much stronger first-stage relationship than the subsequent years (where the instrument would be too weak). As such, this threat is arguably not something to be concerned with in this case.

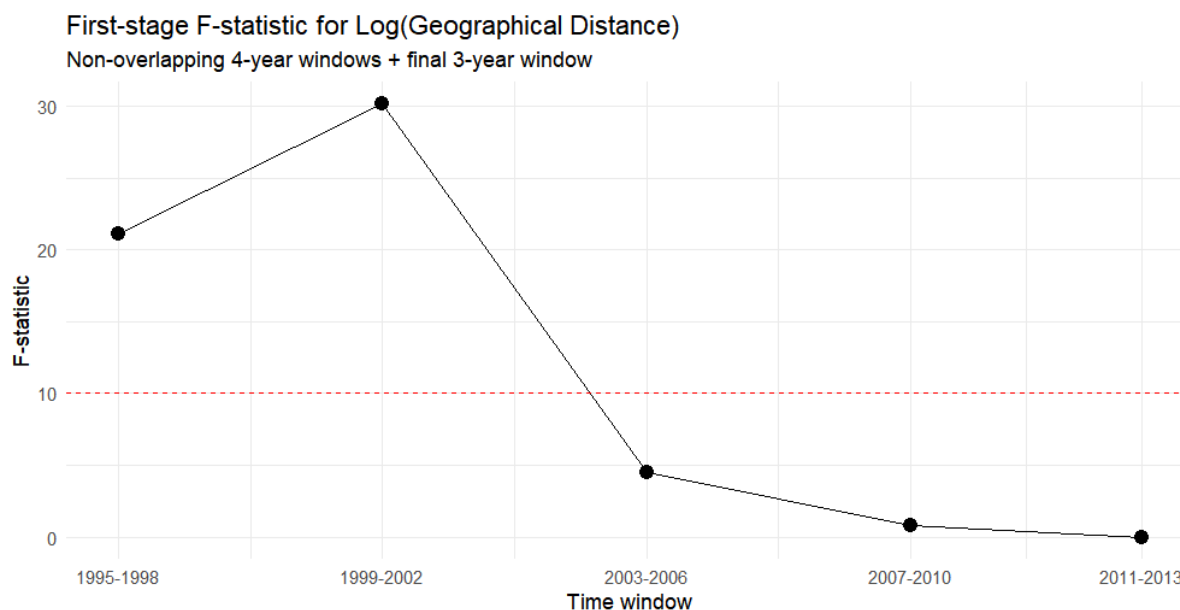


Figure 5: First-stage F-statistics plotted for different time periods. The statistics are based on the main specification including all controls and fixed effects.

ii) Category 2: Period-specific concerns

The exclusion restriction could potentially be violated because of context-specific events and policies that were “only” present in the shock period examined. The main threat in this regard is that the attitudes and policies in states further South and Southeast (primarily Bavaria and Baden-Württemberg) were stricter towards the Yugoslav refugees than in other parts of Germany (Bosswick, 2000). Consequently, there is a risk that counties in these states that have shorter distances to the former Yugoslavia have attitudes and policies that cause a surge in emigration intensity, along with influencing the public finances and labour market conditions of the counties. Put differently, emigration intensity is potentially not the only channel through which geographical distance is associated with the outcomes set out in this analysis.

One measure in place to combat this is the inclusion of state fixed effects, which, in theory, should adjust for common policies, labour market conditions, and budget capacities in these “problem-states”. However, if there is within-state heterogeneity along geographical gradients with respect to political conditions, there is a bigger concern. Luckily, in a robustness check where Bavaria and Baden-Württemberg are excluded, outcome coefficients remain relatively stable.³² Also, if one assumes that most of these attitudes and policies towards these refugees are somewhat rigid over time in the Southern states of Germany towards various migrant groups that also have migration paths along these latitudes ³³, one should again see a stronger instrument in the non-shock years, which one does not (Figure 5).

³²Despite a loss of important variation and sample size, that weakens the first stage.

³³Hence, I relax the period-specific assumption of the issue.

6 Main results

The key results of this paper are summarized in Table 3.³⁴ The IV estimates are mainly the ones used for interpretation, whereas the OLS estimates can function as a baseline correlation to be compared with. For both, all controls and fixed effects are included.

Table 3: Main results. Main sample (1995-2002)

	(1) Fiscal capacity per capita		(2) Income tax per capita		(3) Operating budget expenditures	
	OLS	IV	OLS	IV	OLS	IV
Panel A: Fiscal outcomes						
Emigration intensity (EIA)	0.0029 (0.0017)	0.0059 (0.0189)	0.0017 (0.0019)	0.0372* (0.0184)	0.0062** (0.0022)	-0.0477 (0.0270)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,466	2,466	2,472	2,472	2,441	2,441
R^2	0.92	0.92	0.94	0.94	0.97	0.97
Within R^2	—	0.66	—	0.47	—	0.95
First-stage F-statistic	—	40.84	—	39.07	—	38.79
Panel B: Labour market outcomes						
	(4) Average MM wages		(5) Unemployment rate		(6) Youth unemployment rate	
	OLS	IV	OLS	IV	OLS	IV
Emigration intensity (EIA)	0.0066** (0.0020)	0.0497* (0.0205)	-0.0275 (0.0235)	-0.2934 (0.2323)	-0.0536*** (0.0148)	-0.2730 (0.1389)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,117	2,117	1,848	1,848	2,472	2,472
R^2	0.78	0.78	0.87	0.87	0.75	0.74
Within R^2	—	0.57	—	0.47	—	0.40
First-stage F-statistic	—	29.59	—	34.39	—	39.07

Notes: County-clustered standard errors in parentheses. The dependent variables in specification groups 1, 2, 3, and 4 are log-transformed. The first-stage F-statistics vary across specifications because of different samples (based on the availability of data). For general first-stage results and F-statistics with a consistent sample, see Table 4. Significance codes: *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

6.1 Fiscal outcomes

Panel A shows no clear pattern of impacts of return migration on public finance outcomes. This is to a large extent consistent with the migration literature which often finds small and/or insignificant effects on fiscality. For fiscal capacity per capita, which is a measure of the counties' and their respective municipalities' tax-collection abilities, the IV coefficient suggests that 1 additional (international, not intranational) emigrant per 1000 inhabitants in a county in one period is associated with an approximate 0.59%³⁵ increase in the fiscal capacity per capita in the following period, ceteris paribus. This coefficient is, although, not statistically significant. A potential explanation is that this variable is, albeit conceptually fascinating, quite aggregated and smooth, potentially masking channels and mechanisms through which emigration has a more direct influence.

An IV coefficient that is statistically significant at the 5 percent level, however, is viewed in the IV

³⁴Some additional ones can be found in Appendix A.4.

³⁵Since this is a log-level model, the formula for obtaining the percent interpretation is $(e^\beta - 1) * 100\%$, but since one can get close to the percent interpretation by multiplying the coefficient by 100, and this offers simplicity, this route is chosen.

specification on (2) income tax per capita. According to this coefficient, an additional emigrant going abroad per 1000 inhabitants corresponds to a 3.72% increase in the income tax per capita in the next year. This seems quite large, especially compared to the OLS estimate, which is over 20 times lower, and could be due to several factors explored further in the discussion section.

Lastly, turning to operating budget expenditures, it is noteworthy that the OLS estimate is significant and positive, in contrast with the IV estimate, which is insignificant and negative. Intuitively, it is more logical that the coefficient is negative, as people emigrating, all else equal, should lead to a dampened demand for social services and support, and reduce spending as the population decreases (through a form of scaling effect). Yet, this IV coefficient remains statistically insignificant. It is possible that this, as for the fiscal capacity variable, is due to the aggregation level of the variable and that stronger effects on a more granular level are concealed. Another possibility is that spending is rigid from one year to the next, or simply that there was budgetary substitution from migrant-specific support to something else.

6.2 Labour market outcomes

Inspecting the results for (4) MM (Manufacturing and mining) wages specifications, both the OLS and IV estimates indicate a significant positive effect of the abroad-emigration intensity on average wages in these sectors. The coefficients suggest that average wages increase by 0.66 and 4.97 percent, respectively, for every additional emigrant per 1000 residents. There is plausibility behind the direction and significance of the coefficient, with potential determinants and channels elaborated in a couple of sections (more precisely, Section 8.1.1) below.

Regarding unemployment, both among the whole working-age population and its subsample of those under 25 years old, there appears to be no effect of emigration, as none of the IV results are statistically significant. The youth unemployment rate does, however, have a negative coefficient³⁶ with a p-value of 0.0502, and is only marginally insignificant.

6.3 First-stage-results

Table 4 reports the results when you regress emigration intensity (EIA) on the log-transformed geographical distance from the former Yugoslavia. There are two groups of specifications, one for the main sample years (1995-2002), where one would expect a strong correlation, and one for a placebo sample (2006-2013), where one would not expect the same relationship. Within both samples, the specifications differ in the set of fixed effects and controls included (but across, they are equal).

³⁶Which would be interpreted differently than panel A outcomes (as this is a percentage and not a log-transformed value), if significant, namely a drop of 0.27 percentage points.

Table 4: First-stage results

	Dependent variable: Emigration intensity (EIA)					
	Main Sample (1995-2002)			Placebo Sample (2006-2013)		
	(1)	(2)	(3)	(4)	(5)	(6)
Distance from the former Yugoslavia	-6.971*** (0.9477)	-9.192*** (2.337)	-4.538** (1.457)	-1.640 (1.342)	-3.389 (2.499)	1.570 (2.405)
GDP ₁₉₉₅			2.996*** (0.6736)			1.750 (1.179)
Manufacturing and mining (MM) employment share ₁₉₉₅			-7.748** (2.422)			-1.312 (2.818)
Population ₁₉₉₅			-2.474** (0.8163)			-2.324 (1.242)
Non-German share of the population ₁₉₉₅			0.5561*** (0.0535)			0.4510*** (0.1135)
Long-term unemployment share ₁₉₉₅			-0.0486 (0.0264)			-0.1690** (0.0553)
Urban area			-0.3329 (0.5139)			0.5480 (0.7284)
Rate of natural increase (RNI)			0.1673* (0.0740)			0.7736*** (0.1630)
Young/old ratio (among working-age population)			0.0004 (0.0122)			0.0401 (0.0305)
<i>Fixed Effects</i>						
Year FE	No	Yes	Yes	No	Yes	Yes
State FE	No	Yes	Yes	No	Yes	Yes
Observations	3,200	3,200	2,472	3,200	3,200	2,472
R^2	0.086	0.34	0.68	0.0036	0.19	0.48
Within R^2	—	0.033	0.53	—	0.003	0.38
F-statistic (excl. instrument)	299.992	106.924	39.067	11.386	8.885	2.119
Level at which SEs are clustered	County	County	County	County	County	County

Notes: Standard errors in parentheses. Geographical distance, GDP, employment in manufacturing and mining, and population are log-transformed. Specifications (3) and (6) include a full set of controls, reducing the sample size to 309 counties as some counties lack data for certain controls. Note that these results are general and can potentially deviate from the exact first stages that correspond to the main results above. That is because some counties lack data for certain dependent variables, or that the sample of years is different. The actual first-stage F-statistics are located at the bottom of the "economic results" panels/tables. Significance codes: *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

Across all the main sample specifications, there appears to be a strong, statistically significant negative association between the distance instrument and the endogenous variable. This is just as expected, as the prediction was that the further the distance³⁷ from the Yugoslavian midpoint, the less relative emigration would be seen. One can also note that the 1995 GDP, the non-German share of the population, and the rate of natural increase are positively significant, presumably driven by migrant networks that are located in economically strong areas that see higher birth rates.³⁸ On the other hand, the statistically significant negative coefficients on industrial employment share and population can possibly be explained by ties between strong labour market attachments and lessened emigration propensity, and a denominator effect, respectively.

For the main sample specifications, the F-statistics indicate a strong instrument (well above the rule-of-thumb of 10 for a single included endogenous variable (Stock & Yogo, 2002)), even with the full set

³⁷This distance is log-transformed, as a linear interpretation would make little sense. It is more interesting to look at percentage changes in distance, rather than in kilometer units.

³⁸Difficult to see death rate being the driver, unless health diverges substantively from one county to the next.

of fixed effects and controls. Hence, specification (3), with a high R^2 and useful controls for the main results, is the main one used in Tables 3 and 5.

This is not the case for the placebo sample, where the coefficient changes when you add controls, and all specifications show an insignificant instrument, with corresponding low values for the F-statistics. As such, there appears to be a timing component to the instrument strength, which further endorses its validity, as previously discussed in the empirical strategy section.

7 Robustness checks

Building on the first-stage results table, it is helpful to look at the second-stage results in the placebo sample when you use the placebo-weak distance instrument. These results are encapsulated in Table 5, except for the industry wages DV, for which there were no available data in the placebo time period.

Table 5: Placebo results. Placebo sample (2006-2013)

	(1) Fiscal capacity per capita		(2) Income tax per capita		(3) Operating budget expenditures	
	OLS	IV	OLS	IV	OLS	IV
Panel A: Fiscal outcomes						
Emigration intensity (EIA)	0.0013 (0.0018)	-0.1062 (0.1762)	-0.0043* (0.0017)	-0.1278 (0.1977)	0.0023 (0.0021)	0.1891 (0.7911)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,472	2,472	2,472	2,472	2,013	2,013
R^2	0.82	0.83	0.90	0.90	0.95	0.95
Within R^2	--	0.57	--	0.45	--	0.93
First-stage F-statistic	--	2.12	--	2.12	--	0.22
Panel B: Labour market outcomes						
	(4) Average MM wages		(5) Unemployment rate		(6) Youth unemployment rate	
	OLS	IV	OLS	IV	OLS	IV
Emigration intensity (EIA)	NA (-)	NA (-)	0.0270 (0.0196)	-0.2273 (0.6087)	0.0010 (0.0063)	0.1201 (0.3244)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	NA	NA	2,464	2,464	2,472	2,472
R^2	NA	NA	0.83	0.83	0.82	0.82
Within R^2	--	NA	--	0.53	--	0.36
First-stage F-statistic	--	NA	--	2.19	--	2.12

Notes: County-clustered standard errors in parentheses. The dependent variables in specification groups 1, 2, 3, and 4 are log-transformed. The first-stage F-statistics vary across specifications because of different samples (based on the availability of data). For general first-stage results and F-statistics with a consistent sample, see Table 4. There were no available data in the period for MM wages. Significance codes: *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

Glancing at this table reveals that when a weak instrument is applied, during a time period where there should be minimal connection between the distance to Yugoslavia and emigration rates, there are no significant results at all, often with standard errors larger (in absolute terms) than their respective coefficients. Correspondingly, the IV coefficient directions are chaotic and often in the opposite direction from what one would expect. Like the previous arguments (see Figure 5 and Table 4), this shows that the instrument's strength in the main sample is not driven by time-invariant patterns of migration and

cannot convincingly be used to estimate fiscal and labour market effects in the absence of the Yugoslavian emigration shock.

Furthermore, to see that the IV results presented in Table 3 were due to emigration and distance to the former Yugoslavia, and not just by geography and state-specific factors and policies, two robustness checks are carried out. The first is adding a quadratic polynomial of latitude and longitude³⁹ to the control vector, following the paper by Dell (2010), where you essentially control for smooth spatial variation across Germany's geography, so that economic and fiscal gradients across space are accounted for.⁴⁰ Put differently, this control aims to capture within-state geographic disparities that are potentially correlated with the outcomes (as previous specifications only have state fixed effects).

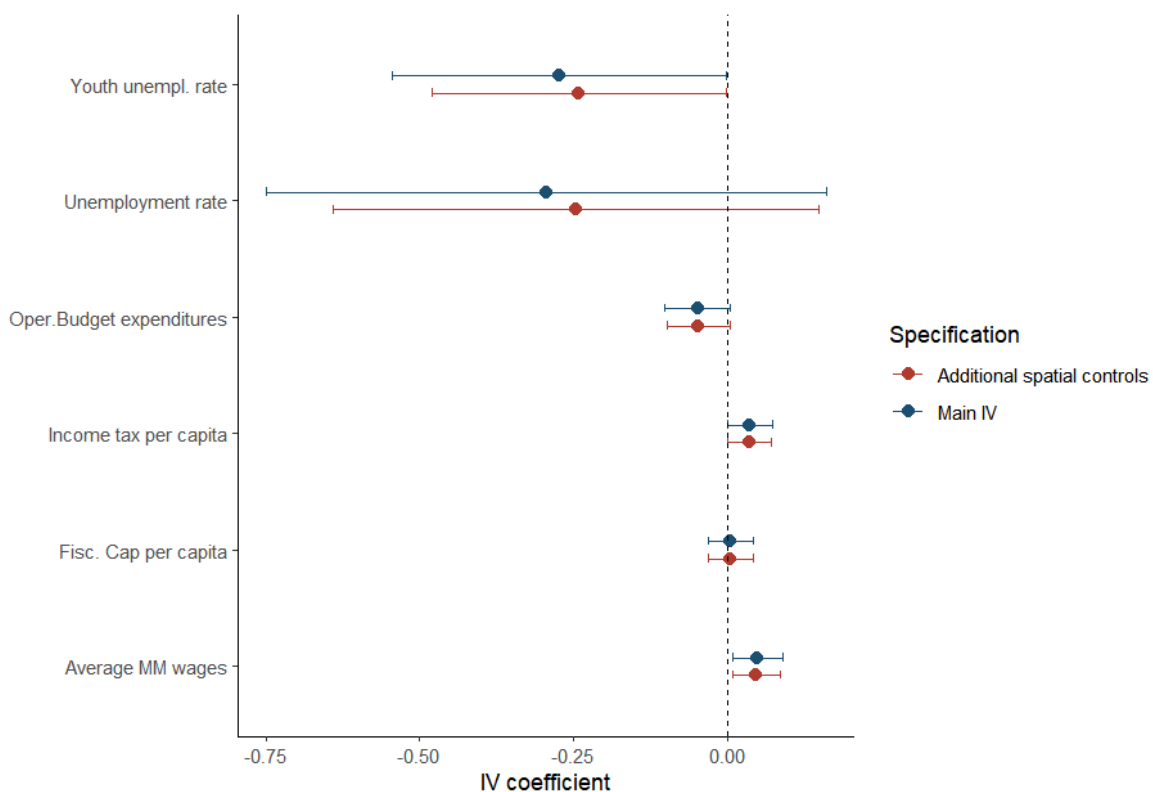


Figure 6: Main IV coefficients (as presented in Table 3) and the IV coefficients when you control for flexible spatial trends. Including 95 percent confidence intervals. The F-statistics for the spatial control specifications range from 30.48 (MM wages) to 41.60 (Fiscal capacity).

In the figure above (Figure 6), one can see a comparison between the coefficients of the main IV specification (including all controls and fixed effects) and the coefficients one gets when adding the Dell-style flexible spatial control. One can see from this plot that the coefficients are generally stable and that the general takeaways with respect to the outcomes remain the same. These results suggest that the original results presented in Table 3 were not driven by within-state geographic and economic gradients.

The second robustness check is to exclude the states of Bavaria and Baden-Württemberg from the

³⁹The polynomial looks like: $lat + lon + lat^2 + lon^2 + lat * lon$.

⁴⁰Just applying a linear control (a straight plane) does not capture effect curvatures across space and assumes that all variation is due to simple north-south and east-west dynamics. Regional clusters (such as in Ruhr) that can deviate from surrounding areas despite being close geographically, would, for example, not be adequately handled.

sample (keeping in mind that this reduces statistical power and harms the instrument's strength, as F-statistics drop from about 40 to around 10) in the event that there is within-state heterogeneity with respect to political attitudes and policies that the state fixed effects cannot capture⁴¹ (see Section 5.2.3, category 2 concerns).

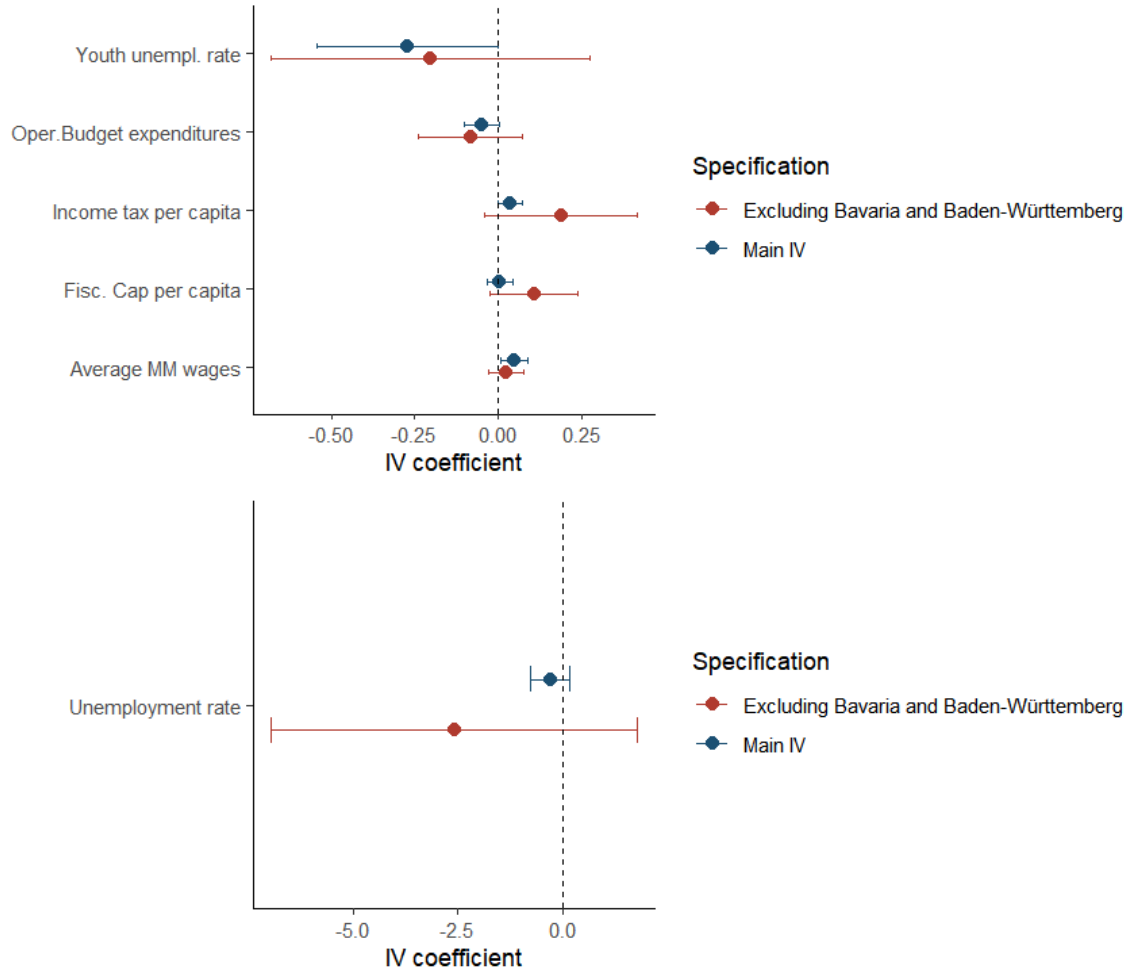


Figure 7: Comparison of the main IV coefficients (as presented in Table 3) and the IV coefficients when you exclude the states of Bavaria and Baden-Württemberg. The figure is split into two, as the coefficient of the unemployment rate is much larger in magnitude than the other dependent variables. This is likely related to the weak first stage for this specification (4.22). The F-statistics for the other state-excluding robustness check specifications range from 11.58 (MM wages) to 16.46 (Fiscal capacity).

If there is a within-state geographic gradient to harshness of policies that state fixed effects in Bavaria and Baden-Württemberg would not account for, Figure 7 is informative about what the results become when you remove them. Giving a slightly different picture than the previous robustness check, this figure shows that removing these two states from the sample can make the results a bit more sensitive. For the most part, coefficients stay in the same direction, and F-statistics are above the Stock-Yogo threshold of 10^{42} , but the precision is exacerbated and the instrument is weaker (F-statistics of around 10). By doing

⁴¹However, it is not unreasonable to assume that the state fixed effects cover most of the ground necessary for this to not to be an issue, as policy environments and attitudes spread from neighbouring counties quickly, causing a convergence in outcomes within a state.

⁴²Except for the specification on the unemployment rate, with the already smaller sample of data having only data from 1998 onwards.

this exercise, one can show that there is some consistency to the estimates, and that there is arguably limited evidence that attitudes and policies in these specific states are confounding the main results. The lack of precision can plausibly be attributed to the loss of observations and important variation.

8 Discussion

8.1 Drivers, mechanisms, and validity

In this part, there are some discussions that are based on insignificant coefficients, although with clear reservations on the magnitude (and keeping in mind the uncertainty), as the results logically follow each other and are often aligned with *a priori* conjectures based on the literature in the field.

8.1.1 Drivers and economic mechanisms

Upon review, the results are generally coherent with the existing scholarship in the literature of forced migration economics. In fact, insignificance and uncertainty are, as mentioned previously, often key features of research on the labour market (Becker & Ferrara, 2019) and the fiscal impacts of migration (Hennessey & Hagen-Zanker, 2020; Kaczmarczyk, 2015). Likewise, for the outcomes that are significant or close to significant, there are plausible explanations for the direction of these estimates when you consider the context in which these estimates are made and the estimates found for the other dependent variables in this paper.

For the most granular public finance outcome, the income tax per capita measure, one can say that the coefficient should be interpreted in light of compositional effects of emigration on the population. As this estimate is significantly positive, it can indicate that those who are captured by the emigration measure were, on average, below the “stayers” in terms of contributing to the income tax base. This result might be driven by an examined emigration group that consists of refugees who have, relative to “stayers”, seen fewer fruitful employment opportunities. Hence, their abilities to contribute to the revenue streams of their respective counties’ finances might also have been weaker. They also had, presumably, a weak incentive to do so, as many were expecting to leave Germany when the wars ended (Dimova, 2006).

An explanation for why the fiscal capacity estimate is insignificant and lower than the one on income tax is essentially two-fold. Firstly, the migrants can impact the tax base through other means, for example, with their consumption, rent payments, investments, and cheap labour supply (which can increase firm profits through lower marginal costs), also referred to as ripple effects (Clemens, 2022; Dadush, 2018; St. Clair, 2024). Secondly, and likely the bigger determinant, there is a high level of aggregation in the variable. Even if there is compositional adjustment in the population, fiscal capacity, as it comprises all tax revenue sources, will be sticky and not subject to profound changes from one year to another.

Similar aggregation limitations apply to the operating budget expenditures measurement, as such expenditures are also likely to be rigid from one year to the next. Yet, its IV estimate can contain useful information as it is directionally consistent across specifications and controls (as per robustness checks and Appendix A.3) and fairly close to significant (p-value of 0.078). The direction of the IV coefficient is negative, which is cogent given the context. As these operating expenditures contain social services and transfers such as unemployment benefits, the (somewhat dubious) negative coefficient can potentially be driven by a drop in the number of welfare-dependent people.⁴³

⁴³This would occur naturally as a pure scaling effect as well, irrespective of the characteristics of the emigrating group. However, if the examined refugees were overrepresented in unemployment statistics, to which there is no clear evidence,

Holistically, the fiscal effects are statistically indeterminable and/or non-detectable at an aggregate level, but they can, particularly if one reads into the direction of the operating expenditures estimate, underscore the hypothesis that these emigrants found themselves in the first stage of the life cycle in the Kaczmarczyk model, having a net negative fiscal position. However, given their time in the country and their issues of getting fruitful employment opportunities in a deportation-expecting environment⁴⁴, this can seem to be a result of the policy environment.

Turning to the drivers of the labour market outcomes (panel B in Table 3), and viewing them together, it is noteworthy that the predictions of Monras (2021) seem to be fairly accurate. In the short run, Monras' theoretical model argued that the labour supply shock is absorbed by the prices. Assuming robustness to the p-values and viewing the average wages in manufacturing and mining as a proxy for the average wages across refugee-concentrated sectors⁴⁵, it can appear that such an absorption mechanism did take place in these years.

It might seem odd that there were seemingly no significant changes in the unemployment rates, especially when you consider the employment bans and labour market conditions the refugees in Germany faced at the time (Bosswick, 2000; Liedtke, 2002). In contrast, there are some mechanisms that could explain the insignificant results on unemployment rates. Firstly, unemployment rates are persistent over time and have other crucial determinants, such as the business and investment climate. Secondly, if there is rigor to the ripple effects channel, emigration could lead to less consumption and investment (i.e., less stimulus in the economy) that could somewhat offset the outflows of people who are presumably over-represented in unemployment statistics. Lastly, there could be an issue of noise and lack of statistical power (particularly in the case of the "normal" unemployment rate, for which data points first appear in 1998).⁴⁶

Say, for the sake of the argument, that the sole capturers of the labour supply shock are the prices and marginal costs; this can occur in a multifaceted way. For one, a negative shock to the local labour supply increases the upward pressure on wages, as labour becomes relatively more scarce. For another, assuming representability of the MM wages across sectors, there could be compositional effects related to the education level in the population. When there are large outflows of people, presumptively younger and less educated than the native population, the remaining workforce will, on average, be more highly educated and older and, thus, have higher wages. Note, though, that if wages are rigid (sticky over time), the effect can be somewhat "mechanical" in the sense that the denominator (number of people in the labour force segment(s)) is lowered with emigration, whereas the wages remain unaltered, inducing a positive shift in the variable.

When both fiscal and labour market outcomes are viewed together, there is validity to the claim that there is interconnectedness between the labour market and fiscal impacts. With the aforementioned conditions for employment for the emigrating group, partly reflected in the unemployment rate results (relying more heavily on the youth unemployment rate outcome), having a high income is difficult, causing the income tax per capita to rise upon departure. It also makes sense that when average wages

the effect would be larger in magnitude.

⁴⁴Both Bosnian and Kosovar Albanian refugees were entitled to seek out employment, but since their residence permits expired (and were renewed) every three months, several states refused or were reluctant to issue supplementary work permits (Liedtke, 2002). As such, they were often, in practice, prevented from having attractive jobs.

⁴⁵Due to labour market segmentation and concentration of refugees from the former Yugoslavia in certain sectors, such as construction and cleaning (Dimova, 2006), the wages could work as a proxy here.

⁴⁶Or simply, that wages absorb much of the effects.

increase, even just in some sectors (for which there is evidence), income tax per capita follows. Moreover, if the refugees were overrepresented in reliance on social transfers and services, which would be plausible given their de facto restrictions on having an independent income, one would expect a downward push on operating expenditures.⁴⁷

8.1.2 Validity and empirical limitations

Stepping back and viewing fiscal and labour market outcomes together from an econometric perspective, it can appear that the IV approach often amplifies the OLS effect, but at the expense of precision. This could stem from the method’s advantage in identifying a stronger and more "exogenous" component of the emigration, but at the expense of having less utilizable variation across counties. Additionally, there are concerns with the structure of some dependent variables. The aggregated ones, along with those constructed for the purposes of this paper, could encompass aggregation errors or noise that overshadows the impacts of emigration. Such noise inflates residual variance and lowers the precision of the estimates.

Those estimates that are found to be significant should be interpreted as local average treatment effects (LATE). This means that they are average treatment effects for the subset of counties that saw their treatment statuses or treatment intensities (i.e., emigration intensities) changing because of a regressor (geographical distance from the former Yugoslavia) satisfying an exclusion restriction (Angrist & Imbens, 1995). Put differently, the estimated effects apply to the complying counties (i.e., those whose emigration intensities were induced by their geographical proximity to the former Yugoslavia) and not to all counties in Germany. As such, these estimates may generalize well to comparable migration shocks for territorial entities similar to the compliers in this study, but less so, perhaps, to other migration episodes or systematically different areas.

For standard concerns surrounding IV, the exclusion restriction seems to be more of a concern than the relevance assumption and finite sample bias. Finite sample bias seems not to be that much of an issue, as such biases are more concerning when the instrument is weak, the sample is small, and the R^2 is low (Bound et al., 1995; Murray, 2006), which is not the case. The relevance assumption is tested for in the first stage, and holds for all specifications. With the exclusion restriction, the "evidence" is less clear-cut, but the potential threats are found not to be of major concern.

The lack of county fixed effects, for instance, could have allowed for time-invariant patterns of migration linked with both geographical distance to the former Yugoslavia and the economic performance of counties to prevail, which could have confounded the validity of the instrument. However, as per the discussion around Figure 5, there appeared to be no such emigrational correlation across time, suggesting that the instrument is only “Yugoslav wars”-specific, speaking in favor of its validity.

The test to determine whether geographical gradients themselves were predictors of outcomes, using quadratic spatial polynomial control (see Section 7), finds reassuring stability in coefficients with consistent variance and strong first-stage relationships. Consequently, it appears that the apparent effects were due to emigration intensity rather than just broad geographic and spatial trends. The exercise on the removal of Bavaria and Baden-Württemberg yields similar conclusions, namely that coefficients remain stable (although with weaker first stages and more uncertainty as important variation is lost). This concurs with the hypothesis that, in the event of within-state heterogeneity in the harshness of

⁴⁷Unless the savings were replaced by other expenditures.

attitudes and policies (as state fixed effects cannot capture them) in Southern German states⁴⁸, there is validity to the instrument and robustness to the results.⁴⁹

8.2 Implications

Given the identification limitations and the static-like research design, there is a risk that the results can be somewhat misleading on the overall impact of hosting refugees and the implications of their returns (Dadush, 2018). This analysis, for one, does not include a counterfactual scenario where the refugees stay in Germany and integrate further, so a potential short-term wage increase, reduction in public expenditure and unemployment rates (if these coefficient directions are to be trusted), can, for instance, be at the expense of extensive benefits in the longer run. The undertaken approach in this study is not apt for such a comparison.

Given the scarce literature on large-scale non-economic-driven emigration in a high-income country context, the results in this paper can cast light on the symmetries between the immigration and emigration effects in a European context. Scrutinizing the main results and keeping in mind the potential drivers discussed above, it appears to be symmetric consistency in the effects of in- and outflows. Without speaking to the magnitudes and exact point estimates, it appears that emigration of refugees has similar impacts as immigration of refugees (as per the literature). Both in the sense that, overall, the impacts are often found to be insignificant and filled with uncertainty, and that the effects found are in line with the theorizations on *immigration* (for instance, the Kaczmarczyk life-cycle model and Monras' framework for immigration and the labour market), just with switched signs.

Like most of the literature in migration economics, these results are probably contextually dependent. The Duldung status, lying at the foundation of these analyses, is a rather peculiar and distinctive feature of this context that makes these results, and the implications of them, less valid externally, all else equal. Still, in the German context, this contested legal status is still in play, and was, for instance, applied to approximately 120,000 people at the end of 2023 (Bundesamt für Migration und Flüchtlinge, 2024). As such, the results of these analyses are potentially more valid in newer contexts within Germany's borders and likely less so in other countries that do not offer such statuses.

One of the main motivations to address this research subject was the controversy and topicality of return migration. Particularly in Germany, which immediately halted Syrians' asylum processes following the fall of the Assad regime in December 2024 (Hammer et al., 2025), this topic is on the agenda. Whether the results on former Yugoslavians in the 1990s and early 2000s are comparable to the hypothesized effects of a large-scale return flow of Syrians remains unclear, but there are both arguments in favor and against.

For the Yugoslavian results to apply in the Syrian case, there would need to be similarities in the net fiscal positions and labour market integration between the two groups. One potential determinant of labour market participation and employment gaps is perceived cultural distance in the population (Lundborg, 2013; Quantrill et al., 2026). Someone who is perceived to be further away culturally could see further impediments, although not necessarily formal, to their labour market integration. Subsequently, this could also impede their abilities to avoid welfare payouts, earn an income, and strengthen the public

⁴⁸There is arguably spatial correlation along the geographical gradients within a state with respect to these attitudes and political environments, as it is likely that there are spillovers between counties. As such, the heterogeneity might be limited, making this concern overstated.

⁴⁹Also note that if there is no such within-state heterogeneity in Bavaria and Baden-Württemberg, then the first line of defense, i.e., the state FEs, will capture any confounding variation.

finances. Lundborg (2013) emphasizes religion as a key trait of migrant groups and their labour market integration prospects, saying that migrants from Muslim countries are more often perceived as more culturally distant (in the case of Sweden), and are often more subject to labour market discrimination. Since most of the refugees who arrived from the former Yugoslavia came from Bosnia and Herzegovina and Kosovo, two territories/countries with large Muslim populations (Radović, 2005), this religious dimension appears similar in the two cases. Pulling in the other direction, perceived cultural distance can also be linked with geography, making Syrian migrants (from the Middle East) more distinguishable from Yugoslavian migrants (from Europe) in terms of cultural proximity to Germans.

Next to cultural distance, there is also a time dimension. Syrians arriving in Germany around 2015 have been in the country for about 10 years, which is a few more years, presumably, on average, than the Yugoslav refugees in Germany in the 1990s/2000s.⁵⁰ So if there was a relative advantage of being Yugoslavian in Germany, compared to a Syrian, for cultural and language reasons⁵¹, the time in the country, as proven to be a key factor in labour market integration (Lundborg, 2013), can offset some of these initial differences.⁵²

Naturally, whether the results apply here as well, also relies on the structural characteristics of Germany since the 2015-wave of immigrants. One key thing here is whether Germany allows holders of a toleration status and asylum seekers to work today. While Duldung status holders do not have a formal restriction on their employment prospects now either, they still face a comprehensive administration process (Land Hessen, n.d.). For asylum seekers, Germany has opened up labour market access after 3 months and stepped up integration efforts (Organisation for Economic Co-operation and Development [OECD], 2017). These changes mean, *ceteris paribus*, that migrants today should have better prospects for employment and positive net fiscal positions.

Overall, the generalizability discussion consists of many variables and unknowns. Some of the results from this paper suggest that there are some potential “positive” labour market effects and net fiscal impacts of returning forced migrants (especially if you count on the close-to-significant results to hold), but these are functions of the short time spent in the country, along with policies that actively prevented the migrants from participating fully in the labour market. Nonetheless, the overall takeaways from these results are that aggregate impacts of returning refugees have a limited and/or negligible impact on public finance and the labour market. Thus, if you add the seemingly clear evidence that easing labour market access restrictions and integrating refugees and asylum seekers into the economy can yield vast benefits (Clemens, 2022; Marbach et al., 2018), alongside the high costs of deportation (Dadush, 2018), it seems unlikely that pushing for returns, whether that is for Syrians or any other migrant group, is more beneficial than aiming for a better and speedier integration process.

⁵⁰As Bosnians mostly repatriated in 1995-1998, and Kosovar refugees arriving later due to the Kosovo war in 1998/99 left in the early 2000s.

⁵¹The Latin alphabet is widespread in Bosnia and Herzegovina and Kosovo (except for Republika Srpska in Bosnia, where they often use the Cyrillic alphabet). As such, it could be easier for them to learn German compared to Arabic-speaking people from Syria.

⁵²Note, however, that there were Yugoslavian migrant networks in Germany before the 1990s (Dimova, 2006), potentially making “real integration time” less for subsequent migrant flows, even if they are refugees and not labour migrants.

9 Conclusion

The Duldung toleration status of Yugoslav refugees in Germany was an impactful one. For a large number of refugees who arrived in Germany in the 1990s, who were put in a pre-deportation mindset and environment in Germany, it meant exacerbated economic prospects (Bosswick, 2000; Marbach et al., 2018) and a psychological strain (Dimova, 2006). For Germany, it opened up the possibility of returning them on a large scale once the Yugoslav wars ended.

Employing an instrumental variable strategy, this paper has analyzed the fiscal and labour market impacts of this large-scale return-driven emigration wave. Using geographical distance from the former Yugoslavia as an instrument for non-German emigration intensity at the German county level, some key findings emerge. Firstly, there appear to be insignificant, non-detectable, and/or negligible effects of return migration on both the aggregate fiscal and labour market outcomes. This can be partly due to data and empirical strategy limitations, but such results are not unexpected given the inherent ambiguity in the economic effects of migration in general.

Secondly, one can argue for a tendency in the results that the emigrating group of refugees was relatively more welfare/state-dependent, as they had less rewarding employment opportunities and a lower average income. Thirdly, the results seem to be explainable given the mechanisms investigated in the literature. Specifically, the timing aspect comes into play as the low amount of time predicts a net negative fiscal position (Hennessey & Hagen-Zanker, 2020; Kaczmarczyk, 2015) driven by lack of employment and welfare-dependence. Also, the theorizations of Monras (2021), predicting a stronger effect on wages than (un)employment rates in the short run, seem to hold given the significance level of 5 percent.

Lastly, even though it is not a finding per se, this study can illustrate key considerations to keep in mind when discussing the implications of results in a migration study. As this is a top-down, close-to-static approach that is sensitive to the context at hand, the results only say so much about the total overall impact of returning refugees in a high-income country context. It can yield some insights into the fiscal and labour market effects in the short run, and connect that to evidence found on short-run impacts of immigration found in the literature, but it cannot offer much insight into the intertemporal trade-off between integration and returns. Notwithstanding this limitation, the paper has offered a discussion on this, utilizing pre-existing scholarship, and argued that integration policies that extend formal labour market access to all legal statuses and assure a quicker labour market absorption rate would yield more benefits for the host country than employing a costly deportation alternative.

Even though this paper can offer insights into the short-term fiscal and labour market impacts and their interconnectedness, there is plenty of room for further study. It would be interesting to do a similar analysis in a more contemporary context and/or with more granular data to allow for analysis on sector-specific heterogeneity of wages, or more migrant-related fiscal outcomes on the expenditure side. Additionally, further research could aim to estimate the fiscal and economic benefits/costs of return migration over time by applying a counterfactual model, and contrast that with estimates on deportation costs. Finally, the economic literature on migration, and especially the one on forced migration and returns, could benefit from a more extensive debate on the ethical and psychological implications of policy alternatives. Not only to build awareness surrounding the treatment of migrants, but also to build an economically relevant understanding of the environment in which policies take form, and herein, a further comprehension of the behavior of economic agents.

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A Appendix

A.1 Territorial reforms and data cleaning

The administrative-territorial reforms that occurred in Germany from 1995 to this day have been substantial, particularly in the states of Saxony, Saxony-Anhalt, and Mecklenburg-Western Pomerania. This has implications for which units of counties the various sources of data report for. Hence, this needs to be dealt with in order to create a fully balanced panel.

To help identify which counties are no longer in existence, there is a table (11111-0002) from the Statistisches Bundesamt (2026) GENESIS-Online database that contains this information. Additionally, it states the timing of the cessation of existence, helping with the identification of reforms in which the mergers/reorganizations took place.

A.1.1 Territorial reforms, mergers and data cleaning

The larger territorial reforms are summarized in Table 6. Here, one can see which counties are in the panel currently and which old ones make up this new territorial entity. The weights are population-based (based on municipalities within the counties) at the time of the respective reforms, and are used to compute the appropriate variable values for the post-merged entity (see section on “Method of aggregation for the large reforms”).

Table 6: Territorial reforms in German states (1995-present)

New county as it appears in the balanced panel	Old counties no longer in the panel	Weight
Mecklenburg-Western Pomerania’s territorial reform (2011)		
13003 Rostock, kreisfreie Stadt	13003 Rostock, kreisfreie Stadt	100
13004 Schwerin, kreisfreie Stadt	13004 Schwerin, kreisfreie Stadt	100
13071 Mecklenburgische Seenplatte, Landkreis	13052 Demmin, Landkreis	82.4
	13055 Mecklenburg-Strelitz, Landkreis	100
	13056 Müritzt, Landkreis	100
	13002 Neubrandenburg, kreisfreie Stadt	100
13072 Rostock, Landkreis	13051 Bad Doberan, Landkreis	100
	13053 Güstrow, Landkreis	100
13073 Vorpommern-Rügen, Landkreis	13057 Nordvorpommern, Landkreis	100
	13061 Rügen, Landkreis	100
	13005 Stralsund, kreisfreie Stadt	100
13074 Nordwestmecklenburg, Landkreis	13058 Nordwestmecklenburg, Landkreis	100

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New county as it appears in the balanced panel	Old counties no longer in the panel	Weight
	13006 Wismar, kreisfreie Stadt	100
13075 Vorpommern-Greifswald, Landkreis	13052 Demmin, Landkreis	17.6
	13059 Ostvorpommern, Landkreis	100
	13062 Uecker-Randow, Landkreis	100
	13001 Greifswald, kreisfreie Stadt	100
13076 Ludwigslust-Parchim, Landkreis	13054 Ludwigslust, Landkreis	100
	13060 Parchim, Landkreis	100
Saxony's territorial reform (2008)		
14625 Bautzen	14272 Bautzen, Landkreis	100
	14292 Kamenz, Landkreis	100
	14264 Hoyerswerda, kreisfreie Stadt	100
14521 Erzgebirgskreis	14171 Annaberg, Landkreis	100
	14191 Aue-Schwarzenberg, Landkreis	100
	14181 Mittlerer Erzgebirgskreis	100
	14188 Stollberg, Landkreis	100
14626 Görlitz, Landkreis	14286 Löbau-Zittau, Landkreis	100
	14284 Niederschlesischer Oberlausitzkreis	100
	14263 Görlitz, kreisfreie Stadt	100
14729 Leipzig, Landkreis	14379 Leipziger Land, Landkreis	100
	14383 Muldentalkreis	100
14627 Meißen, Landkreis	14280 Meißen, Landkreis	100
	14285 Riesa-Großenhain, Landkreis	100
14522 Mittelsachsen, Landkreis	14375 Döbeln, Landkreis	100
	14177 Freiberg, Landkreis	100
	14182 Mittweida, Landkreis	100
14730 Nordsachsen, Landkreis	14374 Delitzsch, Landkreis	100
	14389 Torgau-Oschatz, Landkreis	100
14628 Sächsische Schweiz-Osterzgebirge, Landkreis	14287 Sächsische Schweiz, Landkreis	100
	14290 Weißeritzkreis	100
14523 Vogtlandkreis	14178 Vogtlandkreis	100

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New county as it appears in the balanced panel	Old counties no longer in the panel	Weight
	14166 Plauen, kreisfreie Stadt	100
14524 Zwickau, Landkreis	14173 Chemnitzer Land, Landkreis	100
	14193 Zwickauer Land, Landkreis	100
	14167 Zwickau, kreisfreie Stadt	100
14511 Chemnitz, kreisfreie Stadt	14161 Chemnitz, kreisfreie Stadt	100
14612 Dresden, kreisfreie Stadt	14262 Dresden, kreisfreie Stadt	100
14713 Leipzig, kreisfreie Stadt	14365 Leipzig, kreisfreie Stadt	100
Saxony-Anhalt's territorial reform (2007)		
15001 Dessau-Roßlau, kreisfreie Stadt	15101 Dessau, kreisfreie Stadt	100
	15151 Anhalt-Zerbst, Landkreis	20.2
15002 Halle (Saale), kreisfreie Stadt	15202 Halle (Saale), kreisfreie Stadt	100
15003 Magdeburg, kreisfreie Stadt	15303 Magdeburg, kreisfreie Stadt	100
15081 Altmarkkreis Salzwedel	15370 Altmarkkreis Salzwedel	100
15082 Anhalt-Bitterfeld, Landkreis	15154 Bitterfeld, Landkreis	100
	15159 Köthen, Landkreis	100
	15151 Anhalt-Zerbst, Landkreis	36.7
15083 Börde, Landkreis	15355 Bördekreis	100
	15362 Ohrekreis	100
15084 Burgenlandkreis	15256 Burgenlandkreis	100
	15268 Weißenfels, Landkreis	100
15085 Harz, Landkreis	15357 Halberstadt, Landkreis	100
	15364 Quedlinburg, Landkreis	100
	15369 Wernigerode, Landkreis	100
	15352 Aschersleben-Staßfurt, Landkreis	6.7
15086 Jerichower Land, Landkreis	15358 Jerichower Land, Landkreis	100
	15151 Anhalt-Zerbst, Landkreis	7.6
15087 Mansfeld-Südharz, Landkreis	15260 Mansfelder Land, Landkreis	100
	15266 Sangerhausen, Landkreis	100
15088 Saalekreis	15261 Merseburg-Querfurt, Landkreis	100

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New county as it appears in the balanced panel	Old counties no longer in the panel	Weight
	15265 Saalkreis	100
15089 Salzlandkreis	15352 Aschersleben-Staßfurt, Landkreis	93.3
	15153 Bernburg, Landkreis	100
	15367 Schönebeck, Landkreis	100
15090 Stendal, Landkreis	15363 Stendal, Landkreis	100
15091 Wittenberg, Landkreis	15171 Wittenberg, Landkreis	100
	15151 Anhalt-Zerbst	35.5

Sources: Regierungsportal M-V (2011), REVOSax (2013), Statistisches Landesamt Sachsen-Anhalt (2007).

To complete the data management, some other counties required attention as they did not have values or saw smaller mergers themselves:

Göttingen, Landkreis

In the case of Göttingen Landkreis, the code changed in 2016 from 03152 to 03159, but both were present in the pre-cleaned dataset. Both codes had values for various variables, but never at the same time. Therefore, everything was moved over to 03152, and 03159 was eradicated from the panel.

Hannover

In 2001, Hannover Landkreis (03253) and Hannover kreisfreie Stadt (03201) merged into 03241 Region Hannover Landkreis. However, only 03241 appeared in the original pre-cleaned data.

Aachen

In the pre-cleaned data, both 05313 Aachen kreisfreie Stadt, 05354 Aachen Landkreis, and 05334 Stadteregion Aachen appeared. The former two merged into the latter in 2009. As 05313 did not have any values for any relevant variables, and 05353 and 05334 never had overlapping values, everything was moved over to 05334 while removing the other two codes.

Hanau

Hanau (06415) did not have any values across the board, so it was removed completely from the dataset.

Osterode am Harz and Eisenach

Both Osterode am Harz (03156) and Eisenach (16056) were merged into other already-existing counties in 2016 and 2021, respectively. They had values for many of the relevant variables, but not all. Additionally, they did not exist in the shapefiles used to compute distances from Yugoslavia. Resultingly, these two were also removed from the dataset to arrive at a balanced panel comprised of the 400 counties as

they appear in Germany today.

A.1.2 Method of aggregation for the large reforms

The choice of aggregating up to the counties as they exist today was made with respect to simplicity and robustness. For any variable, it is fairly straightforward to sum up or average out (with weights) the values of the old counties and assign this sum/average to what would later become the new administrative and territorial entity. If one were to go in the opposite direction, one would risk portioning out an aggregated value on false terms. Even if this were to be done with population weights, the values assigned to the older/less aggregated counties could be misleading about what the actual value was, as it is not always the case that population reflects the true size of the value of variables. Aggregating, rather than disaggregating, particularly in the case of count/amount variables and mergers of complete (100 % of) counties, avoids this problem.

As the INKAR variables are already reported for the county IDs as they appear today, there was a need to transform the variables from the Regionaldatenbank by summing or averaging them up to the new merged county level. The way in which this was done can be described by using the example of 15089 Salzlandkreis (Statistisches Landesamt Sachsen-Anhalt, 2007):

When Saxony-Anhalt underwent the administrative-territorial reform of 2007, it was decided that 15153 Bernburg (Landkreis), 15367 Schönebeck (Landkreis), and 15352 Aschersleben-Staßfurt (Landkreis), excluding the city of Falkenstein/Harz, would be merged to form 15089 Salzlandkreis. The city of Falkenstein/Harz constituted 6.7 percent of the population of Aschersleben-Staßfurt, meaning that only 93.3 percent of that old county would be moved into Salzlandkreis.

Since there are two main forms in which the variables from the Regionaldatenbank come, namely as shares/percentages or counts/amounts, these cases are handled slightly differently. Say you have two variables: R_1 is an amount variable (for instance, the total number of employed persons) and R_2 is a share variable (e.g., unemployment rate). Before aggregation, the newly merged entity, Salzlandkreis, has no values for these variables. To obtain its value for R_1 , the values for the three old counties are simply summed up using their weights, meaning that one takes 100 percent of the value from 15153 and 15367, but only 93.3 percent of the value from 15352.⁵³ For R_2 , the calculation is a bit different due to the nature of the variable. The new value for the merged entity is an average of the three merging counties, weighted by their population in 2005 (as this is available data from Statistisches Landesamt Sachsen-Anhalt (2007)). However, 15352 Aschersleben-Staßfurt uses only 93.3 percent of its population in this computation.

For share variable R_2 , where B=Bernburg, S=Schönebeck and A=Aschersleben-Staßfurt:

$$R_2^{Salzlandkreis} = \frac{R_2B * PopB + R_2S * PopS + R_2A * (0.933 * PopA)}{PopB + PopS + 0.933 * PopA}$$

This method is not flawless, however, as it assumes that the municipalities making up the old counties (in this case, Falkenstein/Harz and the rest of Aschersleben-Staßfurt) have proportionally the same share of each variable. In many cases, this assumption will not hold, as more urban areas will potentially see

⁵³Whenever the old counties' values are integers, the resulting sum is rounded to the nearest whole number.

disproportionate concentrations of business and general economic activity. Yet, this method is effective when one merges whole and complete counties, as previously argued. It is also simple to implement.

A.2 Bosnian diaspora in Germany



Figure 8: State distribution of the BiH diaspora in Germany in 2016. Source: Halilovich et al. (2018)

A.3 Sensitivity in controls: LOO and LTO analyses

As there is no possibility to include county fixed effects in the IV estimations, due to the time-invariant nature of the instrument, there is an increased reliance on the other controls. Included in the main IV specification, there is an ample selection of controls. The analyses set out here aim to say whether the estimates are sensitive to their inclusions in the control vector(s). The results of the leave-one-out (LOO) analysis can be seen in Figure 9, while the leave-two-out (LTO) analysis results are split into fiscal outcomes (Figure 10) and labour market outcomes (Figure 11).

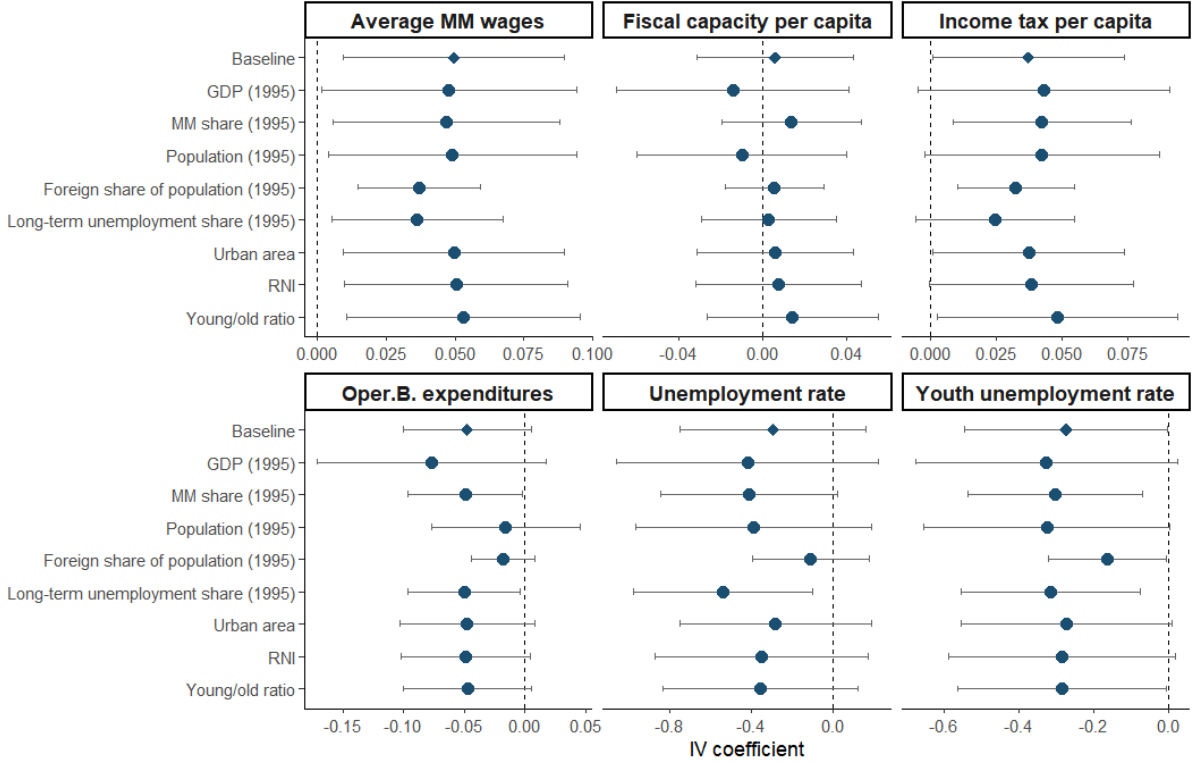


Figure 9: Leave-one-out analyses. Coefficients and confidence intervals when you leave out one control from the control vector(s). The control left out is indicated on the vertical axis. Baseline estimate at the top.

The LOO analysis shows that the coefficients remain steady around the baseline, and in the same direction (except for fiscal capacity). For outcomes that were originally on the borderline of being significant, the exclusion of certain controls can push their p-values below 0.05. However, as these outcomes are somewhat discussed as significant, to highlight some interconnectedness between the labour market and fiscal, they do not change the implications of the results too much.

The same picture is evident in the LTO analyses below. Those outcomes that were significant, or close to significant, have robustness and steadiness in the coefficients when you leave out pairs of controls. Fiscal capacity per capita, which was not close to being significant, generally has more arbitrary coefficients.

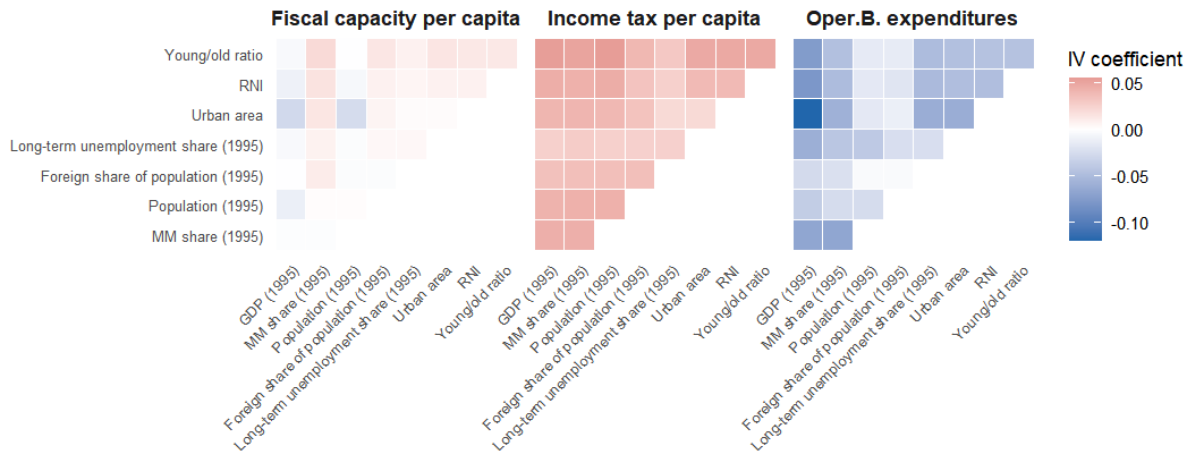


Figure 10: Leave-out-two analyses for the fiscal outcomes. Controls are pairwise dropped, and the coefficients are presented in a heatmap.

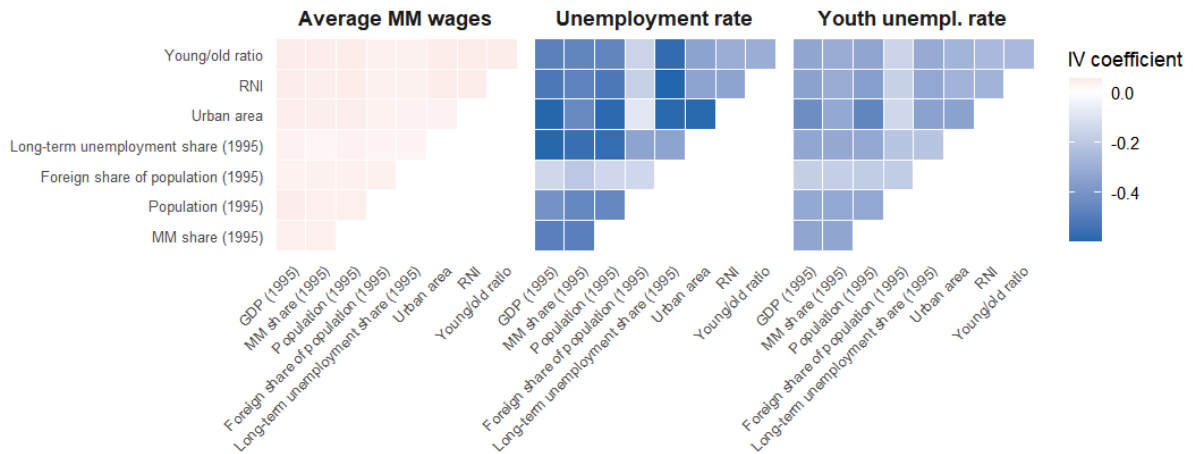


Figure 11: Leave-out-two analyses for the labour market outcomes. Controls are pairwise dropped, and the coefficients are presented in a heatmap.

A.4 Additional results

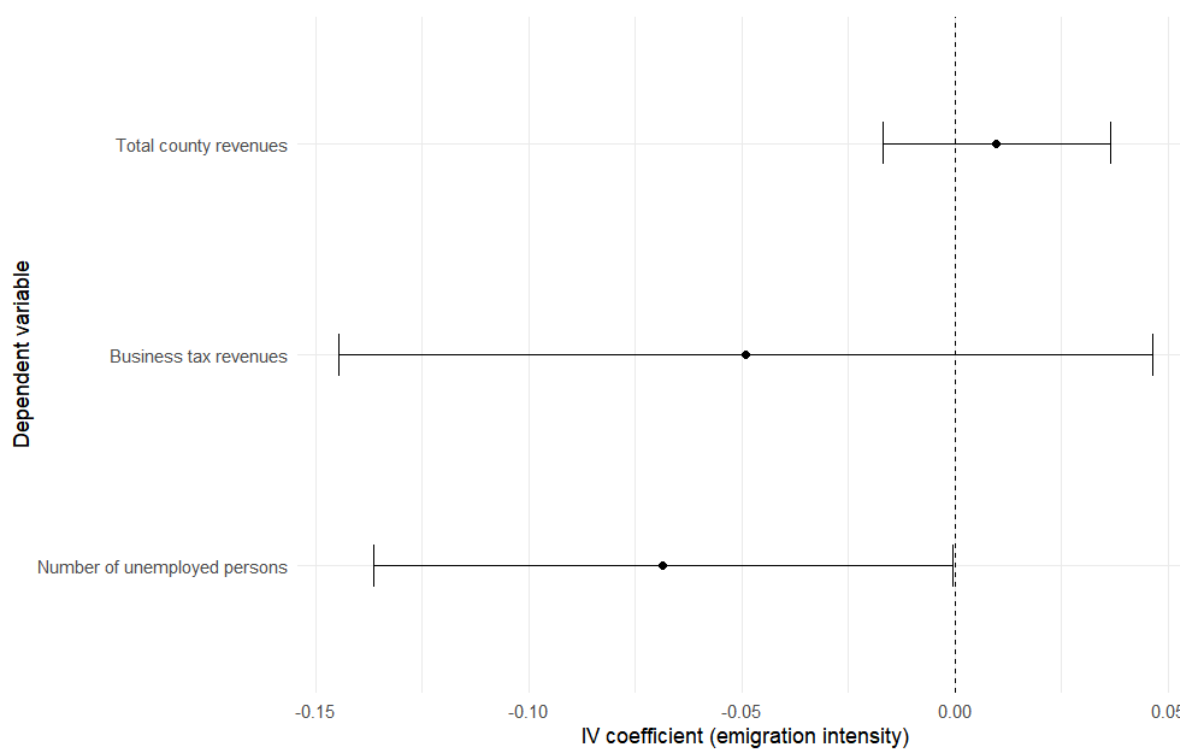


Figure 12: Additional results using the main IV specification (including all controls and fixed effects. Total county revenues cover all revenue sources for public budgets in the county. Business tax revenues are mainly taxes on firm profits. The last DV is the total number of unemployed persons. The two former variables are from Regionaldatenbank Deutschland (Statistische Ämter des Bundes und der Länder, 2026), while the latter is from the INKAR database (Bundesinstitut für Bau-, Stadt- und Raumforschung, 2026).