More refugees, more problems? – The effect of refugees on perceived institutional quality

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Abstract

In this dissertation, I investigate whether refugee inflows during the European Migrant Crisis in 2015 led to an erosion of institutions in Germany. Therefore, I exploit the plausibly exogenous allocation of refugees to states to assess natives' perception of institutional quality using a Difference-in-Difference model. My results indicate that exposure to refugees has positive effects on natives' assessment of cultural institutions, and minor negative effects on economic institutions. I find that middle-aged natives with little education have the highest fears of societal collapse. While young and middle-aged people hold higher fears of erosion of the economy, older people rather fear the downfall of culture. However, my overall results suggest that there is neither a large-scale subjective nor objective erosion of institutional quality in response to refugee inflows.

List of Abbreviations

AfD = Alternative für Deutschland

ATE = Average Treatment Effect

DiD = Difference-in-Difference

GDP = Gross Domestic Product

OLS = Ordinary Least Squares Regression

SOEP-CORE = Core Dataset of the German Socio-Economic Panel

TFP = Total Factor Productivity

2SLS = Two-Stage Least Squares Regression

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1 Introduction

The European Migrant Crisis in 2015 has been of the most drastic events of humanitarian mass migration in the 21st century. In response to the civil war in Syria and political instabilities in the Middle East, a record number of over 1.2 million people applied for asylum in European countries (Eurostat, 04.03.2016). The huge influx of people from foreign cultures has reignited the debate about the effects of humanitarian migration on the economic and cultural institutions in the host countries.

Parallel to the European Migrant Crisis, a theoretical account called *the new economic case for immigration restrictions* has brought forward the idea that the economic benefits of migration cannot be captured because immigrants transport low-growth institutions into high-growth countries (Collier, 2013; Borjas, 2014). Hence, the institutions responsible for economic growth in high-income countries erode in response to migration. What remains unanswered is whether these arguments for the new case for economic restrictions are also applicable to refugees. Refugees often have experienced massive trauma (Hollifield, 2002), have a lower human capital than migrants (Brell, Dustmann, and Preston, 2020) and face severe legal barriers (Marbach, Hainmueller, and Hangartner, 2018). This makes them one of the most vulnerable groups in society (Martén, Hainmueller, and Hangartner, 2019). Henceforth, critics assumed that the massive displacement of millions of refugees from the Middle East had especially negative effects on economic and social institutions of the host country (Gensing and Reisin, 2020). Thus, the arguments of the new economic case for immigration restrictions could be especially applicable for Germany after the European Migrant Crisis.

To test this hypothesis, I exploit the plausibly exogenous allocation of refugees to German states to assess natives' perception of institutional quality using a Difference-in-Difference (DiD) model. By comparing natives' perceived institutional quality in states with a low refugee-to-population ratio with states with a high refugee-to-population ratio, I can isolate the causal effect of refugees on certain economic and cultural institutions. I find that refugees have a slightly negative effect on the perceived quality of economic institutions and a positive effect on the perceived quality of cultural institutions. My results suggest that middle-aged natives with a low level of education express the highest concerns of institutional erosion. While young and middle-aged people hold higher fears of erosion of the economy, older people rather fear the downfall of culture. The overall effect of refugees on perceived institutional quality is however relatively small. Hence, I find no evidence for a large-scale institutional erosion in response to refugee inflows.

I start by reviewing the relevant literature on the topic in Chapter 2. I then elaborate on my identification strategy, describe the datasets that I am using and explain my model (Chapter 3). In Chapter 4, I present and discuss my results. Last, I conduct several robustness checks in Chapter 5 before I explain the limitations of my analysis and conclude.

2 Literature Review

In recent years, there has been a great number of insightful works on the economic effects of immigrants on the host country, for example on wages (Ottaviano and Peri, 2012), fiscal balances (Rowthorn, 2008; Nowrasteh, 2015), trade (Parsons and Vézina, 2018), or jobs (Peri and Sparber, 2009; Peri, 2012). Especially the back-and-forth debates around the effect of immigration on wages have shown that there is no clear consensus on the economic effects of immigration (Card, 1990; Borjas, 2017, Clemens and Hunt, 2019).

The literature concerned with the effects of immigration on cultural institutions on the other hand is divided into two categories. One branch of the literature looks at how immigration causes changes in culture that affect economic performance in the long term. The other branch of literature is concerned with the effect of migration on specific cultural institutions, regardless of their relevance to economic performance such as language and personality profiles (Nowrasteh and Powell, 2020, p. 159). While there is a conceptual difference between these branches of research, they often blend and overlap, partly because scholars conceptualize culture differently (See: Chapter 2.3). Measuring the effect of cultural changes on long-term economic growth is however crucial in determining the overall economic benefits of immigration. The question is whether immigration changes the nature of the host countries institutions. Inclusive cultural institutions, such as public safety, property rights, or trust among citizens in general, enhance economic performance in the long term (Acemoglu, Johnson, and Robinson, 2001). According to the Fraser Institute, classic examples of inclusive, high-growth institutions are a small government, a solid legal system with secure property rights, sound money, freedom to trade internationally, and a low number of regulations (Gwartney et al., 2020).

To assess the economic impact of immigrants, an examination of the effects on both economic, and cultural institutions that indirectly affect economic growth, is necessary. To measure how immigration changes the economic institutional arrangements of a country, scholars look at what institutional arrangements facilitate productivity. The assumption behind most of the studies is that immigrants and refugees transmit the institutions of their home county to their host country to a certain degree (Nowrasteh and Powell, 2020, p. 168). Given that the majority of migrant flows occur from poor countries in the South with poor economic institutions to countries in the North with high-growth economic institutions, scholars have expressed their worry that high-growth institutions in western countries erode in response to migration (Collier, 2013, p. 34).

First, I explain the differences and overlaps between refugees and migrants. Second, I review the ortho-

dox theoretical arguments of how immigration enhances productivity and fosters inclusive cultural and economic institutions in the host country. Third, I elaborate on the arguments of the new economic case of immigration restrictions by specifically looking at Borjas (2014) and Clemens and Pritchett (2019) model to assess institutional erosion in response to migration. Last, I develop my research question and explain how it ties into the existing literature.

2.1 Refugees and Migrants – Differences and Overlaps

There are several differences between refugees and migrants that impact the effect on the host country. According to the international and legally binding 1951 Geneva Refugee Convention, a refugee is "someone who is unable or unwilling to return to their country of origin owing to a well-founded fear of being persecuted for reasons of race, religion, nationality, membership of a particular social group, or political opinion." (United Nations High Commissioner for Refugees, 2019, p. 3). The migration decision of refugees is, therefore, *involuntarily* and not motivated by economic reasons. Before being granted the status of a refugee, people must first formally apply for asylum. As long as their application is pending, asylum seekers are granted a temporary subsidiary protection status. A majority of European countries have agreed on a non-binding time limit of 6 months to process the initial asylum application (ECRE, 2016). However, due to the work overload of bureaucrats and the legal possibility to appeal against the decision, the average duration of asylum applications is expected to be longer. The asylum applicants often arrive at the host country with severely less language skills, human capital, legal opportunities to work, and face uncertainty regarding the outcome of their asylum application. In 2020 only 41% of the asylum applicants were granted either refugee status, subsidiary protection or humanitarian status (Eurostat, 2021). With an overall number of around 2.6 million refugees in EU member states, the refugee to population share in the EU is only around 0.6% (United Nations High Commissioner for Refugees, 2019). With 1.1 million refugees, Germany has a slightly higher refugee-to-population ratio of 1.4% (United Nations High Commissioner for Refugees, 2019, p. 3).

The legal differences between migrants and refugees also have implications for their economic integration. As I highlight in Chapter 3.3, refugees face severe limitations in their rights to change their place of residence, their work permit, and the ability to gain citizenship. These legal obstacles combined with exogenous placements of refugees often lead to suboptimal outcomes in the labour market.

However, despite the differences between refugees and migrants in the legal sphere, the long- to mid-term economic impact of refugees can be modelled quite similar to the economic impact of migrants. Salikutluk, Giesecke, and Kroh (2016) use German refugee data from 1990 to 2010 to argue that the economic effects of large refugee inflows can be depicted as a lagged function of low-skilled migration

inflows. In their paper, they employ SOEP-Data (See: Chapter 3.1.1) to find that refugees on average enter the labour market later and work in jobs below their formal qualification (Salikutluk, Giesecke, and Kroh, 2016, p. 751). Since migration is often costly and burdensome, only people with either a high enough skillset or financial background decide to migrate. The absence of such a self-selection process among refugees leads to the situation that also many people without employment in their home country flee to the receiving countries (Salikutluk, Giesecke, and Kroh, 2016, p. 752). Even after the legal restrictions to labour market access end, refugees are still 10% underrepresented in the labour force even 10 years after their flight. The slow integration into the labour market is the main impediment of refugee integration (Aksoy, Poutvaara, and Schikora, 2020).

Using longitudinal data from 1985 until 2015, d'Albis, Boubtane, and Coulibaly (2018) find that higher numbers of asylum seekers in western countries Granger-cause a higher GDP per capita in the long run. Comparing the findings of d'Albis, Boubtane, and Coulibaly (2018) and Salikutluk, Giesecke, and Kroh (2016) one could infer that integration of refugees into the labour market is particularly slow in Germany compared to other Western European countries.

Employing the tools of causal inference avoids the endogeneity of self-selection among migrants, which is one of the key obstacles in migration research (Felbermayr, Grossmann, and Koehler, 2015). It is well documented that immigrants prefer places with better institutional quality (Cebula and Clark, 2011). In the literature, there are several examples where exogenous refugee allocation is used as an instrumental variable for predicting the impact of migration. Exogenous settlement mechanisms for refugees function as a proxy to estimate the effects of immigrants in the absence of self-selection (Steingress, 2018; Parsons and Vézina, 2018).

The estimations on the number of refugees who will go back to their home country once the cause for their flight was eliminated vary significantly. A recent survey among 1600 Syrian refugees in Germany, France, the Netherlands, and Sweden yields that around 66% of them do not intend to go back to Syria until the situation eases (TheDayAfter, 2021).

With regards to natives' perception of refugees and migrants, there are only minor differences. A 2019 representative survey among Germans found that a majority of natives welcome immigrants but at the same time see challenges in their integration. Around 63% think that migration will alleviate the lack of skilled labour and around 50% think that immigrants will bring cultural enrichment (Faus and Storks, 2019, pp. 8-12) The same study finds that 70% of respondents support the government in their decision to take more refugees. Moreover, around 80% agree that rejected asylum seekers should be allowed to stay if they are well integrated and have a job. Other studies with different methodologies show a more negative to neutral attitude of natives' towards refugees and migrants (Kober and Köseman, 2019, p. 11). But overall, the differences between studies are greater than the differences

between the attitudes towards refugees and migrants.

Despite the legal differences of refugees and migrants, I will treat the refugee inflows during the European Migrant Crisis as a lagged inflow of lower-skilled immigrants (Salikutluk, Giesecke, and Kroh, 2016). This allows me to test whether the arguments of the *new economic case for immigration restrictions* apply to refugees.

2.2 The Effect of Migration on Economic Institutions

From an economic perspective, there are two main channels through which immigration can lead to productivity gains and therefore foster inclusive economic institutions.

First, immigration can be seen as an international trade of human labour between countries making use of comparative advantage. If Mexican workers have a comparative advantage in landscaping, it makes sense to immigrate to a country in which landscaping is a much-desired service. The allocation of highly specialized workers to countries where their service is needed makes use of the comparative advantage of workers and therefore leads to productivity gains (Nowrasteh and Powell, 2020, p. 44)

Second, immigration leads to economic gains because workers migrate from countries with a lower total factor productivity (TFP) to countries with a higher total factor productivity. These differences in total factor productivity are not necessarily the result of a "knowledge gap" but are largely driven by better institutional arrangements in highly productive countries. A recent study finds that the factor by which real wages increase for immigrants coming to the to the United States is between 16.4 (Yemen) and 1.7 (Morocco) when controlling for a variety of confounders (Clemens, Montenegro, and Pritchett, 2019, p. 201). Therefore, tremendous economic gains can be realized just because people migrate from low productivity countries to high productivity countries.

The overall economic impact of lifting immigration restrictions for migrants has been researched intensively in the last years. In a famous meta-study, Clemens (2011) combined estimates from various studies with different research designs to estimate how lifting international barriers would enhance global GDP (Figure B1). A variety of studies show that lifting all barriers to labour mobility and enabling open borders could lead to at least a doubling of global GDP. These alleged massive positive economic effects of migration lead to Clemens' conclusion that free migration is "a trillion-dollar bill on the sidewalk" (Clemens, 2011, p. 83).

Moreover, a majority of leading economists have expressed a rather positive view of migration in the recent past. An overwhelming amount of 70% of Europe's most prolific economists (weighted by confidence) agreed or strongly agreed with the statement that people who migrated to Europe between 2015 and 2018 are likely to contribute more in taxes than they receive in benefits and public services

(Figure B2).

While there is a rather broad consensus that high-skilled immigration can have a positive impact on the overall economic situation, the effects on certain economic aspects are disputed. Contrary to the findings of Alesina, Baqir, and Easterly (1999) and Alesina, Glaeser, and Sacerdote (2001) that ethnic division undermines support for the welfare state, Brady and Finnigan (2014) empirically demonstrated that immigration does not per sé undermine support for the welfare state. The debate around the effects of immigration on wages has been going back and forth between scholars since Card (1990) published the first paper to examine the effects of the Mariel boatlift (Borjas, 2017; Clemens and Hunt, 2019; Ottaviano and Peri, 2012). With regards to wages, studies by Borjas (2015, p. 120) and Ottaviano and Peri (2012) reach different conclusions. While both find positive effects of immigration on the well educated, Borjas (2015, p. 120) finds a -1.7 % decrease in the wages of native-born American high school dropouts, while Ottaviano and Peri (2012, p. 152) find a modest 1.1 % increase in wages. The different results mostly stem from different methods of matching skill levels. Hence, the effects of immigration on the job market also depend on whether the skillset of immigrants and natives are either complementary or supplementary (Borjas, 1995, p. 6). While the debate is nowhere near settled, there is a consensus that the effect on wages seems to be positive for non-high school dropouts. Concerning the fiscal impact of immigration, Nowrasteh (2015) and Rowthorn (2008) find that most studies assess it as "relatively small and rarely more than 1% of GDP" (Nowrasteh and Powell, 2020, p. 28).

The main reason that the debates around the effect of immigrants on the host countries' economy keep on flaring up is that new econometric techniques, such as skill-based matching of population groups or causal inference techniques, are employed. Especially the emergence of the skill-cell literature, in which wage and employment effects are researched by matching the skill levels of natives and immigrants, allows a more nuanced interpretation of the winners and losers of immigration. Using this technique, Chassamboulli and Palivos (2014) and finds that job opportunities for natives do not decrease in response to immigration. This is because immigration is not a one-way supply shock to the labour market, but is accompanied by a labour demand shock as well.

While some effects on certain economic institutions are still disputed, the overall economic effect seems to be positive. As a result, "the basic economic case for free trade in labour is even stronger than the classic case for free trade in goods" (Nowrasteh and Powell, 2020, p. 29).

2.3 The Effect of Migration on Cultural Institutions

The effect of immigration on culture is a more contested topic in the literature. Much of the controversy stems from 'culture' being a slippery topic in political economy that is hard to define[1]. Douglas North conceptualizes culture as an "adaptive process that accumulates partial solutions to frequently encountered problems in the past" (North, 2005, p. 36). North emphasizes that culture is not a fixed set of values and beliefs, but an ongoing process that both depends on exogenous changes of the environment and endogenous changes in the beliefs of the members of society directed at providing common solutions for problems. Joel Mokyr expands on this when he defines culture as "a set of beliefs, values and preferences, capable of affecting behavior, that are socially (not genetically) transmitted an that are shared by some subset of society." (Mokyr, 2017, p. 8). Both definitions conceptualize culture as a subject of change that influences the behaviour of a group. Since I am researching the new *economic* case for migration restrictions, I will only focus on how immigration influences culture in a way that changes economic performance[2]. Since many of the cultural practices do not directly affect economic outcomes, much of the literature on the effects of culture on economic growth has primarily focused on "[general] trust as the most important cultural characteristic that can explain differences in economic development" (Nowrasteh and Powell, 2020, p. 161). There are several microeconomic models that establish a theoretical corridor on how higher trust levels enhance economic growth: either through social capital sparking more investment in productive ideas that have economy-wide effects (Akçomak and ter Weel, 2009), through less investment in protective purposes (Bjørnskov, 2017) or just through lower transaction costs that increase productivity (Nowrasteh and Forrester, 2020).

The literature on the effects of immigration on culture is again based on the idea that immigrants transmit certain cultural norms, mostly generalized trust levels, to their host country (Nowrasteh and Powell, 2020, p. 168). To isolate the effect of culture on economic outcomes, scholars often compare individuals with different cultural backgrounds living in the same institutional environment with each other. The transmission of cultural norms thereby often takes place between different generations and differs significantly across countries. Algan and Cahuc (2010) use data from descendants of US immigrants to demonstrate that their ancestor's country of origin is a strong predictor for their inherited trust levels. By comparing the inherited trust levels of immigrants in 1935 with trust levels of their descendants in the 2000s, they show that immigrants to the US adapt rather slow to domestic trust levels. The longitudinal data structure allows them to "identify the sizeable causal impact of inherited trust on worldwide growth" (Algan and Cahuc, 2010, p. 2060). Hence, one causal theoretical

[1] Robert Solow once ranted that most attempts to explain different economic trajectories through culture "end up in a blaze of amateur sociology" (quoted in Krugman, 1991, p. 93, n.3).

[2] A more comprehensive discussion on the effect of immigration on cultural institutions that only marginally affect economic performance can be found in Nowrasteh and Powell (2020, pp. 97–185).

channel through which immigration impacts cultural institutions relevant to economic growth is that immigrants transmit low trust levels from poor countries to rich countries and only adapt very slowly to higher trust levels.

Nowrasteh and Forrester (2020, p. 2) argue that there are severe flaws in the trust-growth literature, mainly due to poor methods and bad data. Furthermore, theoretical predictions of the trust-growth literature based on cross-country studies only rarely align with the predictions from micro-level experiments in trust games (Nowrasteh and Forrester, 2020, p. 6).

Another theoretical channel through which immigration affects trust levels is through increased ethnic and racial diversity. A recent meta-study covering 87 studies has found a statistically significant negative partial correlation coefficient of -0.0283 for diversity and social trust across all studies (Dinesen, Schaeffer, and Sønderskov, 2019). However, "the effects are small and confined mostly to racial differences between blacks and whites, not ethnic differences caused by migration" (Nowrasteh and Powell, 2020, p. 172, See also: Alesina and Ferrara, 2005). Other studies such as Alesina, Harnoss, and Rapoport (2016) even find positive effects of birthplace diversity on economic growth.

With regards to the effect of immigration on culture, there is no definite conclusion. The generalized trust literature appears to be lacking robustness, and the ethnic segregation literature yields conflicting results.

2.4 The New Economic Case for Migration Restrictions

In recent years, doubts about the feasibility of easily reaping the economic benefits of free migration have been raised. Critiques have insinuated that it is unlikely that "billions of immigrants can move to the industrialized economies without importing the 'bad' institutions that led to poor economic conditions in the source country in the first place." (Borjas, 2014, p. 169). Henceforth, the erosion of institutions of highly productive countries could be an unwanted by-product of immigration which can exceed the overall economic benefits. This argument has been called in the literature *the new economic case for migration restrictions* (Clemens and Pritchett, 2019).

One of the first proponents of the "new economic case for migration restrictions" was Paul Collier in his 2013 book "Exodus". Collier criticizes that "Migrants are essentially escaping from countries with dysfunctional social models [...] The cultures – or norms and narratives – of poor societies, along with their institutions and organizations, stand suspected of being the primary cause of their poverty." (Collier, 2013, p. 34). The fundamental assumption of this theory is that migrants take the culture of their home country with them, which results in clashes with the host countries' culture because both cultures are somewhat incompatible (Collier, 2013, p. 68). The general assumption that norms are transmissible across cultures through migrants has been widely empirically proven (Alesina, Giuliano,

and Nunn, 2013; Fisman and Miguel, 2007).

The assimilation of the norms and culture from migrants of low TFP countries does affect the productivity of both migrants and locals. The erosion of inclusive institutions in high TFP countries therefore affects both immigrants and natives alike. However, Collier does neither elaborate on the determinants of such an institutional erosion nor does he supplement his theory with any empirical assessment. The economist George Borjas extends Colliers's theory and supplements it with an empirical model to exactly assess the impact of immigration on US institutions. Borjas (2015, p. 968) estimates the following model that schematically tries to capture the benefits of immigration:

$$\alpha_N^* = (1 - \lambda)\alpha_N + \lambda\alpha_S \tag{1}$$

The dependent variable α_N^* refers to overall economic performance. The parameter λ denotes the erosion of the host countries institutions as a response to immigration. If λ takes the value 1, then immigrants from the South[3] import all institutions and norms that caused the poor economic performance which leads to the economic collapse of the North. If λ takes the value 0, then immigrants from the South do not import any of the bad institutions. Assuming that immigration is costless and immigration restrictions are revoked, Borjas assumes that people will move to the North until the wages in North and South are equal at the optimal migration rate M^*. Running a few simulations, Borjas finds that "if λ were equal to 0.5, the net gain [of free immigration] falls from $ 40 trillion to $ 8.8 trillion. [...] if λ were equal to 0.75, the net gains become negative [...]". (Borjas, 2015, p. 969). If one includes the costs of moving for the immigrants, then the world global production and the US GDP would diminish already at a λ of 0.5 (Nowrasteh and Powell, 2020, p. 35).

Although Borjas does not try to empirically estimate λ, he concludes that "the gains from unrestricted immigration depend largely on how the infrastructure in the receiving economies adjusts to the influx of perhaps billions of persons. Although we have no idea about how this adjustment will pan out, there will be an adjustment." (Borjas, 2015, p. 970). Thus, in Borjas' model there exists an efficient level of migration restriction at the optimal migration rate M^*. In fact, countries deliberately not picking up the "trillion dollar bills on the sidewalk" and "ignoring the advice of the social engineers" (Borjas, 2015, p. 972) just showcases their revealed preferences: they know that unrestricted migration will lead to economic losses and henceforth keep their migration restrictions. It remains important to say that Borjas refrains from making any claims about the ethical dimension of such immigration restrictions; his argument is solely based on economic reasoning.

[3]Borjas speaks of "The global South" and "The global North" for poorer and richer countries respectively. To present his argument accurately, I shall use his terminology.

2.5 Clemens and Pritchett's Epidemiological Model

Following the work of George Borjas, Clemens and Pritchett (2019) construct an epidemiological model. They cast doubts about the chain of causation that underlies Borjas' model of efficient migration restrictions: "That [Borjas'] chain runs roughly from immigration barriers to 'homogeneity' to 'trust' to 'market supporting institutions' and thus to higher productivity." (Clemens and Pritchett, 2019, p. 155). This chain of causation has faced conflicting empirical evidence on many links, such as from Alesina, Harnoss, and Rapoport (2016), who find that a greater diversity of country-of-birth on a country level is correlated with a better economic performance. Lewis and Peri (2015) moreover find that sub-national regions with a greater number of immigrants experience higher income growth.

Clemens and Pritchett employ a general equilibrium model that assumes a "dynamic optimal migration rate that maximizes world economic product during the process of globally equalizing the marginal product of labour (=wages)" (Clemens and Pritchett, 2019, p. 155). Contrary to Borjas' static optimal migration rate M^*, Clemens and Pritchett estimate a dynamic migration rate that changes based on the parameters in the model. Moreover, instead of estimating one parameter λ to assess institutional erosion in the North, their model is based on three parameters (Clemens and Pritchett, 2019, pp. 158–159).

- **transmission**: understood as the degree to which origin-country total factor productivity is embodied in migrants
- **assimilation**: understood as the degree to which migrants' productivity determinants become like natives' over time in the host country
- **congestion**: understood as the degree to which transmission and assimilation change at higher migrant stocks.

Clemens and Pritchett estimate the transmission parameter by looking at the difference in wages between Americans and immigrants and assume that 100% of the initial difference in wages is explained through differences in the TFP between the home country of the immigrant and the USA. Then, they empirically estimate the transmission rate by calculating the TFP gap for various countries. Their results vary from 0.497 for Bangladesh to 0.157 in Guyana (Clemens and Pritchett, 2019, p. 157). This means that certain cultures transmit more of their culture into the US than others.

The assimilation parameter is estimated by using cross-sectional data of migrant earnings as a fraction of earnings of comparable natives. Using the initial difference in earnings as a starting point, an annual individual assimilation rate α is estimated. Using immigrants from Ghana as an example with an initial earnings difference of 0.296 and an annual assimilation rate of 0.107, it takes an average immigrant from Ghana around 15 years to reach the mean income level of US natives. (Figure B3).

Last, the congestion parameter is estimated using census-defined microdata from 2008-2012. The congestion parameter is most effectively estimated in smaller regional areas, such as cities and districts. In their findings, the slope of the regression is positive from 0% to 30% immigrants (Figure B4). They also find that a congestion rate of 0.5 represents "a conservatively high upper bound on the magnitude of any true congestion effects" (Clemens and Pritchett, 2019, p. 25). This means as long as the fraction of foreign-born people from the overall population is below 50%, there is hardly any measurable congestion effect. As an example, in New York only 40% of the residents are foreign-born (MOIA, 2018).

Using these estimations for the three parameters, Clemens and Pritchett find that the new economic case for migration restrictions is overstated. Instead, "the economic gains from additional immigration outweigh any deterioration in destination country productivity caused by immigrants" (Nowrasteh and Powell, 2020, p. 64), even under very conservative assumptions. Henceforth, the new economic case for migration restrictions brought forward by Collier (2013) or Borjas (2015) is overstating the negative effects of institutional erosion in response to migration while underestimating the positive effects of it.

2.6 Research Question

With my dissertation, I complement the existing research by looking at natives' *subjective* perception of institutional quality using survey data. Subjective attitudes and assessments can change in response to refugee and migrant inflows and diverge from objectives measures. Individuals for example could perceive migrants to be a threat to their personal economic situating despite no objective parameters that back their assessment. If they, however, stick to their assessment, then they are likely to save more and consume less, which eventually has macroeconomic implications that affect the overall impact of immigration in the long term. Modern macroeconomic models rely on this endogenous preference formation of agents, which could lead to a gap between the short-term effect of migration on culture and economic institutions, and a long-term effect that comes about due to endogenous preference adaption.

Henceforth, in measuring the changes in natives' assessment of institutional quality in response to refugees, my research sheds light on the institutional erosion hypothesis of Borjas (2015) and therefore contributes to the literature on the new economic case of migration restrictions. Moreover, I test whether the arguments of the new case of immigration restrictions are applicable to refugees. Since the allocation mechanism of refugees in Germany is exogenously based on GDP per capita and population, it could be seen and a simplified resemblance of how immigrants would self-select in the absence of exogenous allocation.

First, I investigate the effect of refugees on natives' assessment of the erosion of economic institutions.

Therefore, I use the questions "How concerned are you about the overall economic development?", "How concerned are you about your personal economic development?" and "How concerned are you with your job security?". The individual assessment of the overall and their personal economic development is crucial for an individual's decisions about spending and investing. If individuals estimate a lower growth rate in response to higher numbers of immigrants and refugees, they will invest less and save more. If saving rates become too high, then growth rates could diminish as too little private capital is flowing into investments. Based on these considerations, I can formulate the following hypotheses to be tested in my analysis:

H0: Natives do not change their subjective assessment of the quality of economic institutions in response to higher numbers of refugees.
H1: Natives change their subjective assessment of the quality of economic institutions in response to higher numbers of refugees.

Second, I investigate whether natives change their assessment of cultural institutions. Therefore, I utilize the questions "How concerned are you about the overall development of crime?", "How concerned are you about the development of hostility to foreigners?" and "How concerned are you with immigrants coming to Germany?". These questions tackle the assessment of institutions such as public safety and general openness to foreigners. I do acknowledge that there might not be a clear cut between economic institutions and cultural institutions that affect economic growth. Natives' concerns about immigration for example are not only motivated by cultural resentments but also by economic reasons. Nonetheless, I do think that a conceptual separation between concerns about economic and cultural institutions makes sense to see whether some institutions are more affected than others by increasing numbers of immigrants. Thus, I can derive another pair of hypotheses:

H2: Natives do not change their subjective assessment of the quality of cultural institutions in response to higher numbers of refugees.
H3: Natives change their subjective assessment of the quality of cultural institutions in response to higher numbers of refugees.

Last, I combine both cultural and economic institutions into an index to investigate whether there is an overall impact on institutions. With this index I am trying to measure how natives perceive the deterioration of institutions, as described by Borjas's parameter λ: more concerns about crimes rates, for example, indicate that natives anticipate an erosion of the institution "public security", more

concerns about the about immigrants threaten the institution of openness towards foreigners. Henceforth, I can investigate whether the new economic case for migration restrictions is true by empirically estimating how natives asses the parameter of institutional erosion λ.

3 Method and Approach

3.1 Data Sources

3.1.1 SOEP-Core-Survey

I obtain my main information about the demographic and socio-economic characteristics of the native population in Germany from the German Socio-Economic Panel (SOEP) Core Survey. The SOEP Survey is a representative annual panel survey of private households in Germany (Goebel, 2019). Starting as a single study back in 1984, the SOEP now covers a wide range of areas and can be combined with a variety of sub-datasets to investigate different topics. The latest survey of 2019 contains information from around 15.000 individuals. The unique and interesting feature of the SOEP dataset is that it questions the same representative households and the individuals that are currently part of the household every year since 1984. This longitudinal data structure allows me to investigate how individuals change their subjective evaluations as a response to refugee inflows over time.

To keep the dataset representative and balanced, 'refresher samples' and special samples were added to the original sample. The overall sample is then subject to demographic inflows. Individuals can only drop out of the active sample through either death, moving abroad or attrition (Figure B6). Therefore, it is assured that the SOEP-Core dataset remains representative of the German population over the years.

As described in Chapter 2.6, I use six different questions to assess the subjective institutional erosion. The question I am concerned with is "How concerned are you about the following issues?". The respondents then rate their degree of concern on the following issues on a scale from 1-3 with 1 being "very concerned", 2 being "somewhat concerned" and 3 being "not concerned at all":

- The economy in general
- Their own economic situation
- Crime in Germany
- Immigration to Germany
- Hostility towards foreigners or minorities in Germany
- (If they are employed) Their job security

All of these questions are part of the survey in every year and scored on the same scale, which is why I do not need to standardize them. Combining the individual answers to the different questions, I then construct an overall index of perceived institutional erosion. The answers of the respondents are displayed in the summary statistics in Table 1. If one separates the answers between natives living in states with a low refugee-to-population ratio and people living in states with a high refugee-to-population ratio (See: Chapter 3.4), one can already observe quite large differences. Since the sample is representative of the German population and the sample size is quite large, this allows me to make plausible estimations of the effect of refugees on institutions.

Table 1: Summary Statistics

	Control			Treatment		
	Count	Mean	Standard Deviation	Count	Mean	Standard Deviation
Crime	32707	1.720763	.6932582	136762	1.807439	.7052814
Hostility Foreigners	36266	1.954227	.7145456	153289	1.955326	.7151798
Immigrants	32668	1.897484	.7540804	136589	1.993631	.7605933
Job	21201	2.424508	.6861647	100059	2.531706	.6532642
Overall Econ	36380	2.041946	.6326378	153479	2.074466	.6325829
Personal Econ	40064	2.095946	.7021062	170554	2.192772	.7054279
Overall Index	40142	2.010619	.4726553	170905	2.085014	.4721601
N	40142			170905		

Since the dataset contains information of the socio-economic situation of representative German households as well as some indicators of their political beliefs, the SOEP-Survey is well suited for my identification strategy. It provides me with the information necessary to track changes in the socio-economic status as well as political beliefs of the native population. To restrict my sample to the native population, I drop all observations of people living in Germany for a period shorter than 5 years. Moreover, I only investigate the response of adults and therefore do not include the supplementary questionnaires on teenagers and kids. Henceforth, the number of questioned individuals per year varies from 24.344 in 2015 to 29.756 in 2017. When I pool the data, I have 211.843 individual observations in my dataset.

3.1.2 State-level Controls

A major concern for my identification strategy is the existence of time-varying but state-fixed effects that are both directly correlated with the perceived socio-economic status of the individuals and

indirectly correlated with the number of refugees. Therefore, it is important "to control for changes in the underlying distribution of characteristics between control and treatment cities [units] over time" (Lozano and Steinberger, 2010, p. 12). Hence, I control for state-level effects that vary across states and time such as unemployment rate, population density, previous migrant stock, GDP per capita, and demographic trends in my analysis. I obtain all of my data from the ESS NUTS 1 control dataset (European Social Survey ERIC, 2019).

3.1.3 Time-level Controls

Controlling for state-level political institutions in my model would only decrease variation between states and therefore is not helpful. But it could also be the case that natives' subjective assessment of institutional quality is dependent on the national political climate. For example, the national political culture is likely to shape the attitudes of individuals in every state. Therefore, it makes sense to employ year-fixed effects on a national level to account for changes that affect all individuals in the year of the interview the same way. Hence, I employ the national level of satisfaction with the work of Angela Merkel, which is monthly monitored through representative survey data from Forschungsgruppe Forschungsgruppe Wahlen (2021) as a proxy for national shocks to the political culture that affect all states equally.

3.2 The European Migrant Crisis as a Natural Experiment

During the European Migrant Crisis in 2015, around 450.000 people applied for asylum in Germany (Figure B5). The majority of refugees coming to Germany were from Syria, Afghanistan, and Iraq. The stark increase in asylum applications in Germany started in 2015 and reached its peak in 2016 with about 710.000 asylum applications. Hence, the massive humanitarian migration of mostly people from the Middle East to Germany can be seen as a natural experiment, which has been used widely in causal inference studies (Dustmann, Vasiljeva, and Piil Damm, 2019; Tolsma, Laméris, and Savelkoul, 2021). Hence, the European Migrant Crisis is a suitable natural experiment for my analysis.

3.3 Identification Strategy

In my analysis, I rely on an exogenous mechanism of refugee allocation to estimate the average treatment effect (ATE) of refugees on institutions. In every natural experiment in the social sciences, there are three core assumptions for the ATE to be identified. First, every subject must be randomly assigned with a known probability to either the treatment or the control group (Random Assignment Assumption). Second, the non-interference assumption must hold. This means that any spillover

effects must be ruled out, meaning that the potential outcome of a subject $_i$ is unaffected by the treatment status of another individual $_j$. Third, the exclusion restriction must hold. That means that the potential outcomes for $Y_i(0)$ and $Y_i(1)$ only respond to the treatment d_i and are not a function of any confounding variable z. In other words, the treatment D_i must be statistically independent or exogenous of potential outcomes and background attributes (z):

$$Y_i(0), Y_i(1), z \perp\!\!\!\perp D_i \tag{2}$$

The ATE is only correctly identified if all of these three assumptions hold simultaneously. In my dissertation, I employ the identification strategy of Aksoy, Poutvaara, and Schikora (2020) and use Germany's "exogenous placement of refugees upon arrival across counties [and states] and the fact that they cannot freely choose their place of residence for a period of at least three years" (p. 1) as exogenous treatment mechanism. To assure that my identification strategy holds, three assumptions have to be met.

- Exogenous Allocation
- Mobility Restrictions
- Mandatory Participation

3.3.1 Exogenous Allocation

The exogenous allocation of refugees in Germany consists of a two-step process. First, the initial allocation of refugees to different states is determined by the EASY-Quota System and the Königssteiner Key. The Königssteiner Key is an annually calculated quote to allocate refugees across states based to $\frac{1}{3}$ on the state's population and to $\frac{2}{3}$ on the state's tax revenue. The goal of the Königssteiner Key is to equally spread the social and economic burden and responsibilities across the different states in Germany. Then the states use different mechanisms to allocate refugees across counties. Nine out of 16 states use the relative population share as main determinant to allocate refugees across counties (Geis and Orth, 2016), other states rely on population density or "fixed-population based quotas assigned by decree" (Aksoy, Poutvaara, and Schikora, 2020, p. 5). However, due to the data limitations of the SOEP-Core dataset, I can only conduct my analysis on a state (NUTS 1) level. Nonetheless, this allows me to exploit plausibly exogenous allocation of refugees across states to estimate the causal effect of the refugee-to-population ratio on natives' perceived institutional quality.

To empirically assess the validity of the exogenous allocation assumption, I run several regressions to

assess what determines the number of assigned refugees in a state for each year from 2010 to 2018. As shown in Table A5, the amount of assigned refugees is highly correlated with the population size and the GDP per capita at state level in every year. Other factors such as unemployment rate, population density, and demographic trends are likely significant due to a high initial correlation with GDP per capita and the population size. Since the number of refugees assigned through the Königssteiner Key is highly dependent on the population and the GDP per capita of a certain state, the allocation of refugees to states is truly exogenous. Therefore, this part of my identification strategy holds.

3.3.2 Mobility Restrictions

Furthermore, refugees are not only exogenously allocated across states through the Königssteiner Key but are also severely limited in their mobility. The Integration Act of 2016 obliges refugees to stay in their initial place of residence for at least three years after their arrival[4]. Although refugees can apply for a change of residence during the first three years at their initial residence, a change has only been permitted in very seldom circumstances (Aksoy, Poutvaara, and Schikora, 2020, p. 6). The mobility restrictions circumvent the reverse causality that migrants prefer places with better overall institutional quality (Cebula and Clark, 2011), higher labour market demand (Card, 1990), and similar pre-existing ethnic enclaves (Edin, Fredriksson, and Åslund, 2003). Using individual characteristics of refugees such as education level, level of German, and gender from the IAB-BAMF-SOEP survey of refugees in Germany, Aksoy, Poutvaara, and Schikora (2020, p. 6) find that there is no endogenous self-selection of migrants on local characteristics due to the mobility restrictions.

Even though the mobility of refugees is legally limited, there are bilateral collaborations between some states that recently allowed refugees to change their place of residency. Up to this day, Bremen and Lower Saxony, and Berlin and Brandenburg have an agreement that allows refugees to choose their residency between both states. However, these cooperations have only been in place since 2017. The bilateral collaborations between states do not impose any significant challenge for my identification strategy. As Aksoy, Poutvaara, and Schikora (2020, p. 8) have demonstrated, only 8% of refugees in Germany moved outside of their initially assigned state of residence. Thus, the mobility of migrants is severely limited.

Furthermore, access to labour markets has been facilitated in recent years. After abolishing the nine months employment ban for new refugees in 2014, refugees are now allowed to enter the German labour market three months after submitting their asylum request (Aksoy, Poutvaara, and Schikora, 2020, p. 5). The same is true for access to education. For children, the compulsory schooling system in

[4]For more information in this see §12a AufenthG.

Germany requires them to attend school after three to six months. Adult refugees' right to education on the other hand is guaranteed to be the same as that of any other foreigner in the same situation.

3.3.3 Mandatory Participation

Moreover, the identification strategy relies on mandatory participation of both states and refugees in the allocation program. In other words, the federal state enforces full compliance of states and refugees and therefore minimizes the chance of endogenous self-selection of refugees in Germany. Therefore, my identification strategy assumes that the allocation of refugees across states is truly random and exogenous. Thus, the identification strategy is suitable to estimate the causal impact of refugees on natives' perceived institutional quality.

3.4 The Model

To estimate the causal effect of refugee inflows on natives' perceived erosion of cultural and economic institutions, I exploit the plausibly exogenous allocation of refugees to different regions in Germany using a DiD-model. Therefore, I divide the 16 NUTS 1 regions in Germany into states that were assigned a high number of asylum applicants in 2015 relative to their population (Treated) and those that were assigned a low amount of asylum applicants in 2015 relative to their population (Control). The treatment and control group are separated at the median level of the refugee per population ratio at around 0.1%. Hence, both my treatment and my control group are strongly balanced with 8 subjects in each group. Since the allocation mechanism to assign refugees is also partly based on the population size of states in Germany, I do not use the absolute number of refugees but the refugee-to-population ratio. I choose 2015 and not 2014 as my decisive year because the refugee-to-population ratio in the states in 2015 is determined by the Königssteiner Key in 2014. The idea is that states with an already high assigned refugee-to-population ratio in 2015 will have a significantly higher refugee-to-population ratio in the future due to the massive refugee inflows during the European Migrant Crisis. The refugee-to-population ratio varies from 0.009% in Thuringia up to 0.02% in Bremen. The mean refugee-to-population is 0.01%. Table 2 shows what states belong into each group.

The differences between the refugee-to-population ratio are relatively small across states. Interestingly, all city-states like Bremen, Hamburg, or Berlin and small states like Saarland and Mecklenburg-Vorpommern are in the treatment group. This division leads to an interesting discussion of the effects of population density and the rural-urban divide (See Chapter 4.1.1).

Table 2: Division of the Treatment and the Control Group

Control		Treatment	
State	Ref-to-pop	State	Ref-to-pop
Thuringia	.0095305	Bremen	.0220354
Brandenburg	.0097483	Hamburg	.0212192
Saxony	.0101732	Berlin	.0202732
Bavaria	.0104514	Saarland	.0182046
Rhineland-Palatinate	.0104647	Mecklenburg-Vorpommern	.0149424
Baden-Württemberg	.0110772	Northrhine-Westfalia	.0143116
Saxony-Anhalt	.0112031	Lower Saxony	.0130418
Schleswig-Holstein	.012399	Hessen	.0128407

The differences in the natives' socio-economic characteristics are then estimated by using a DiD-model that compares individuals between treated NUTS 1 regions and control NUTS 1 regions in the SOEP sample. Since there is a clear treatment timing for all subjects in my study, my DiD-model is a simple 2x2 canonical DiD-model with a clear pre- and post-period and a clear treatment and control group assignment. My model takes the standard form of a difference-in-difference model (Goodman-Bacon, 2021):

$$y_{it} = \gamma + \gamma_i TREAT_i + \gamma_t POST_t + \beta^{2x2} TREAT_i * POST_t + u_{it} \tag{3}$$

The unobserved heterogeneity between the treatment and the control group is then 'differenced out' (Card and Krueger, 1994, p. 773). Henceforth, the estimated ATE isolates a causal effect. To estimate the ATE, I employ the following model:

$$\begin{aligned} SOEPScore_{i,c,t} =& \alpha + \beta_1 * TimeDummy_{2015} * Treated_{c,2015} + \beta_2 * \delta_{i,c,t} + \beta_3 NUTS1_{c,t} \\ &+ \beta_4 Migrant_stock_{c,t} + \beta_5 RefugeeInflows_{c,t} \\ &+ \beta_6 Migrant_stock_{c,t} * RefugeeInflows_{c,t} + \epsilon_{i,c,t} \end{aligned} \tag{4}$$

The $SOEPScore_{i,c,t}$ refers to the perceived quality of institutions of individual $_i$ in state $_c$ at time $_t$. The perceived quality of institutions is measured through a broad spectrum of questions outlined in Chapter 3.1.1. In the results, I report both the outcome of every single question as well as the effect of the comprehensive index.

$TimeDummy * Treated_{c,2015}$ is a binary variable that consists of an interaction of two other binary variables. The Time Dummy is a dummy that indicates the time of the treatment and takes the value

0 before 2015 and the value 1 for and after 2015. The Treated Dummy is a dummy variable that takes the value 0 for the control group and the value 1 for the treatment group. Hence, the interaction term only becomes 1 for states in the treatment group after 2015.

$\delta_{i,c,t}$ refers to a vector of time-variant control variables for the individual $_i$ in state $_c$ at the time $_t$. The vector includes an Age Dummy, a dummy variable for health status, a dummy variable for relationship status, and a dummy variable for employment status. One could argue that variables such as religion and education could be time-variant as well because people can catch up on educational qualifications or change their religion. However, my dataset suggests that both variables tend to be rather sticky and are therefore treated as individual-fixed effects and are only included in equation 5.

$NUTS1_{c,t}$ refers to a vector of state-level effects that vary both across time and state. The vector includes the state-level unemployment rate, the population density, the GDP per capita, and the demographic trends.

$Migrant_stock_{c,t} * RefugeeInflow_{c,t}$ is an interaction term between the previous migrants stock and the new incoming refugees. The reason for including this interaction term is straightforward: maybe the effect of more refugees in a specific state is especially pronounced if the state already has a significantly high stock of migrants (Nowrasteh and Powell, 2020, p. 79). An already high stock of migrants in a specific state could be an indicator that the regional institutions are conducive to foreigners. The interaction term, therefore, serves as a proxy to assess the congestion parameter as highlighted in Chapter 2.5: if a certain state already has a high migrants stock, then an additional number of refugees could cause a congestion effect and slow down the assimilation rate of foreigners (See: Clemens, Montenegro, and Pritchett, 2019, p. 159). Hence, the interaction term provides an adequate assessment of the relation between new incoming refugees and the already existing migrant stock. To circumvent an omitted variable bias, I include both variables in the interaction term also as single variables in the full model.

$\epsilon_{i,c,t}$ describes the error term in my model. For the estimation to be unbiased, the "idiosyncratic error term $\epsilon_{i,c,t}$ must also be uncorrelated with each explanatory variable across all time periods." (Wooldridge, 2016, p. 463).

With regards to the individual control variables, it is important to separate between individual-fixed effects and time-variant individual effects. Individual control variables like sex and immigration background are for example individual-fixed effects. Since individual-fixed effects in a panel data settings lead to omission due to collinearity, I cannot include them in the panel data regression. The same is true for time-fixed effects that only vary across time periods but affect every observational unit in the same way. Omitting these variables in a panel data setting does not lead to an omitted variable bias because it is a feature of panel data that unit-specific unobserved heterogeneity is 'differenced out'.

The only relevant issue is that the unobserved heterogeneity must be uncorrelated with the observed covariates (Mundlak, 1978). Any arbitrary correlation between the error term ϵ and the covariates does not lead to a serial correlation bias and hence does not violate the strict exogeneity assumption. It can be assumed that variables like sex or immigration are uncorrelated with the other covariates in my analysis. To assure that my model is without extreme multicollinearity, it is useful to show the correlations between all covariates in my model (Table A6). Looking at the initial correlation between all covariates in my regression, the highest correlation exists between previous migrant rates and the GDP per capita (0.8595). All other pairwise correlations are low, which is why I can exclude inflated p-values due to multicollinearity.

Other individual effects such as health status, age, or employment status do change regularly over time and hence are important to control for. To nonetheless explore the effect of individual-fixed effects and time-fixed effects, I pool the waves and treat them as repeated cross-sectional data to investigate the effect of individual-fixed effects. The model looks as follows:

$$\begin{aligned} SOEP_{Score} =& \alpha + \beta_1 * TimeDummy_{2015} * Treated_{c,2015} + \beta_2 * \delta_{i,c,t} + \beta_3 * \zeta_{i,c,t} + \beta_4 NUTS1_{c,t} \\ &+ \beta_5 * TFE_t + \beta_6 Migrant_stock_{c,t} + \beta_7 RefugeeInflows_{c,t} \\ &+ \beta_8 Migrant_stock_{c,t} * RefugeeInflows_{c,t} + \epsilon_{i,c,t} \end{aligned} \tag{5}$$

The variable in $\zeta_{i,c,t}$ refers to a vector of time-invariant control variables for the individual $_i$ in state $_c$ at the time $_t$. The vector includes and a religion dummy, an educational dummy, a dummy for having an immigration background, and a sex dummy.

The variable TFE_t stands for the time-fixed effect in year $_t$. In only include the nation-wide satisfaction with Angela Merkel as a time-fixed effect to control for the national political climate.

3.5 Parallel Trends

One of the most critical assumptions about DiD-Models is the assumption of parallel trends (Cunningham, 2021, p. 478). To estimate the ATE in a DiD-model, an estimation of the unobserved counterfactual $E[Y_k^0|POST]$ is made on the assumption that both the control and the treatment group follow parallel trends. If the assumption of parallel trends in a DiD-Model is violated, the unobserved counterfactual is wrong and therefore the ATE is biased. To investigate whether this assumption hold, scholars rely mostly on comparing "placebo pre-treatment leads of the DD-coefficient" (Cunningham, 2021, p. 478). However, the proof that the treatment and the control group followed parallel trends in the pre-treatment period is no sufficient justification for the assumption that parallel trends will continue in the future (Kahn-Lang and Lang, 2020). Testing the parallel trend assumption

in DiD-Models can be quite challenging. Scholars have developed different methods to test the parallel trend assumption under a set of restrictions using fixed-length confidence intervals (Armstrong and Kolesár, 2018) or sensitivity analysis (Rambachan and J. Roth, 2020). Rambachan and J. Roth (2020) argue that the main limitation of the pre-trend analysis is that the counterfactual is essentially constructed based on the research design chosen. Only selecting on trends that pass the pre-testing can create a bias in the average treatment effect. That is why they introduce testing the sensitivity of DiD results to pre-trends. However, in my simple case of a canonical DiD with simultaneous treatment timing, I instead resort to a visual approach of checking the parallel trends assumption. The large data availability allows me to study the trends of both the treatment and the control group up to 3 periods before the treatment timing. Due to the word limit, I will only illustrate and test the pre-trend assumptions for the significant model specifications. The graphical analysis of the parallel trend assumption is displayed in the appendix for concern about crime (Figure B8), the personal economic situation (Figure B9) and immigrants (Figure B10). On the left side of the chart, the observed means of the population are shown. The vertical line shows the year before the treatment occurred. The plot on the right is based on the idea to use treatment leads of the DiD-estimator in the pre-treatment period. The graph is constructed using an augmented DiD-model that estimates the difference between the slopes of the treatment and the control group ζ in the pre-and post-treatment period. Using the standard interaction term between a time dummy (d_t) and a treatment dummy (ω), the estimation looks as follows:

$$\gamma_{c,t,i} = DiD_{i,s,t} + \omega_i d_{t=0}\zeta_1 + \omega_i d_{t=1}, \zeta_2 + \epsilon c, t, i \tag{6}$$

Then, the model centers the continuous-time variable around its minimal value, which "provides a common reference point at the first observed time point such that deviations from parallelism are easily detectable" (STATA Manuals, 2021). Then, a Wald-Test is conducted on ζ against 0 to investigate whether the linear trends are parallel prior to the treatment (STATA Manuals, 2021).

By looking at the graphical diagnostics of the pre-treatment parallel plots, it seems that the parallel trends assumptions holds in all significant model specifications. The Wald-Test confirms that the parallel trend assumption holds for crime (P-Value: 0.2482), immigration (P-Value: 0.1547), and for the personal economic situation (P-Value: 0.6443). Henceforth, we can assume that both states with a high and with a low refugee-to-population ratio in 2015 followed a similar trend before the European migrants crisis in 2015.

Normally, scholars also employ a Granger-causality-like test to investigate whether there are any anticipation effects in response to the treatment. Since I am utilizing an unforeseeable natural experiment, there is very little possibility that people could have foreseen the treatment and adapted their behavior

and expectations long before the treatment timing.

3.6 Standard Errors

Bertrand, Duflo, and Mullainathan (2004) find that conventional standard errors in DiD-Estimations are often severely understated due to serially correlated outcomes. In a panel data setting with repeated observations, the serial correlation of errors within units decreases over time. Thus, clustering on a state-time level is likely to produce underestimated standard errors because, for example, the GDP state X in 2010 is likely to be correlated with the GDP of state X in 2011. To address the issue of serially correlated units within a panel data setting, Bertrand, Duflo, and Mullainathan (2004) advise to cluster or bootstrap standard errors.

Another issue with the standard errors in Difference-in-Difference estimations is that they can lead to a downward bias and an over-rejection of standard asymmetric tests once the numbers of clusters are low (Cameron, Gelbach, and Miller, 2008). While there is no exact threshold of a low number of clusters, Cameron, Gelbach, and Miller (2008, p. 414) consider an interval from 5 to 30 clusters to be a low number. There are several ways to adjust the standard errors in that case. Bell and McCaffrey (2002) use the method of bias reduced linearization that adjusts the linear degrees of freedom to calculate a bias-corrected standard error using a low number of clusters. Imbens and Kolesar (Imbens and Kolesar, 2012) even argue that the Bell-McCaffrey method of degrees-of-freedom adjustment is more robust even in cases with high numbers of clusters. Donald and Lang (2007) propose a similar method that relies on an aggregation method to adjust standard errors.

In my analysis, I employ the suggested method of Bertrand, Duflo, and Mullainathan (2004) and cluster the orthodox standard errors on the state level. However, I include both Bell and McCaffrey (2002) and Donald and Lang (2007) method of adjusting standard errors as robustness checks in Chapter 5.4.

4 Results

4.1 Average Treatment Effects

Before I run my full model described in equation 4, I first calculate the ATE without using any control variables in my model. The results are reported in Table 3. Then, I show the average treatment effect using the full model from equation 4 in Table 4. A graphical depiction of all average treatment effects is shown in Figure B7. A graphical analysis of the trends for all significant models can be found in the appendix for crime (Figure B8) for the personal economic situation (Figure B9) and for immigrants

(Figure B10).

Table 3: ATE without any Control Variables

VARIABLES	(1) Crime	(2) Job	(3) Personal Econ	(4) Overall Econ	(5) Hostility	(6) Immigrants	(7) Index
ATE	0.0421***	-0.00270	-0.0147	0.00492	0.00345	0.0645***	0.0128
	(0.00967)	(0.0151)	(0.0128)	(0.00809)	(0.0133)	(0.0133)	(0.00736)
Observations	169,469	121,260	210,618	189,859	189,555	169,257	211,047

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In the restricted model, the ATE is significant only for concerns about crime and immigrants. Both times, the ATE is positive, meaning that a higher number of refugees causes fewer concerns about crimes rates and immigrants among natives.

If one includes control variables, both coefficients become slightly smaller but remain significant. Additionally, the coefficient for concerns about the personal economic situation now turns out to be significant as well. Since STATA uses listwise deletion in regression when missing values occur, the number of observations is smaller in the full model than in the restricted model. However, the number of observations in the full sample is still around 20.000.

Table 4: ATE in the Full Model from Equation 4

VARIABLES	(1) Crime	(2) Job	(3) Personal Econ	(4) Overall Econ	(5) Hostility	(6) Immigrants	(7) Index
ATE	0.0296***	-0.0146	-0.0199**	0.00133	-0.00322	0.0412***	0.00477
	(0.00908)	(0.0123)	(0.00800)	(0.00803)	(0.0109)	(0.0121)	(0.00572)
Observations	166,669	118,141	190,681	185,695	171,105	166,463	191,011

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

4.1.1 The Perceived Erosion of Economic Institutions

First, the ATE for concern about job security is -0.0146. That means that natives living in a state with a higher number of refugees per population are on average 0.015 (0.5%) points more concerned about their job when controlling for state-, time- and individual-fixed-effects. This effect is, however, not significant. There are fewer observations in this regression compared to the other ones, because people without a full-time job, apprentices, and interns are excluded from the question. The coefficient for concerns about the overall economic situation is also not significant. However, there exists a significant

effect of refugee allocation on the perceived personal economic situation of natives. Natives in states with a higher number of refugees per population are on average 0.02 points (0.6%) more concerned about their personal economic situation. The effect is, however, relatively small. There are several possible explanations for the higher concern about their personal economic situation among natives in states with higher numbers of refugees.

First, the negative coefficient for concerns about the personal economic situation speaks against the findings of Steinmayr (2016, p. 24) and Czymara and Schmidt-Catran (2016). Using electoral data from Austria, Steinmayr (2016) finds that the presence of refugees in the neighbourhood decreases the likelihood to vote for a far-right party in Austria. He hypothesizes that the positive attitudes of natives towards refugees could in fact be a short-term outcome of mere exposure to refugees that reduces initial reservations. However, he adds, "In the medium- and long-run other aspects such as fear of increased competition for scarce resources may then lead to a tougher anti-immigration stance of the population" (Steinmayr, 2016, p. 24). Henceforth, natives regard refugees as competitors in the labour market only in the mid to long term. Czymara and Schmidt-Catran (2016) use individual survey data from Germany to find that "individually perceived labour market competition seems to play only a minor role in natives' acceptance of migrants" (p. 193).

My results, however, show that natives could perceive refugees as competitors in the fight for scarce resources also in the short run. This is especially interesting because refugees are initially prevented from entering the labour market and could only become potential competitors in the job market after their three-month employment ban ends. Therefore, the negative coefficient for concerns about job security could mean that people adapt their expectations and assume that a fair share of refugees will not return to their home country and eventually surge into the labour market. This explanation could be seen as a "Lucas-critique" of immigration economics: Agents adapt their behaviour and attitudes in the short term because they anticipate the effects of migrants in the long term (Lucas, 1976).

Second, it could be the case that natives assess the skillset of refugees to be similar to their own. The short-term effects of immigration on labour market outcomes mostly depend on the elasticity of production factors such as labour and capital. If the skillset of migrants is a substitute to native workers, then, ceteris paribus, competition for jobs will increase and wages for natives will fall. As Borjas (1995) formulated it: "[...] the immigration surplus is small when labor and capital are easily substitutable" (p. 8). Given that refugees are often less specialized than migrants (See: Salikutluk, Giesecke, and Kroh, 2016), this argument would apply mostly to less-specialized workers and low-educated workers. However, if immigrants' skillset is seen as supplementary to natives, meaning that the domestic labour demand curve is relatively inelastic to immigrants, then wages can increase both for immigrants and natives (Borjas, 1995, p. 6). Assuming that natives regard refugees as potential competitors in the

labour market in the future, their subjective valuation of the skillset of refugees becomes crucial.

Third, natives regard immigrants not only as competitors in the job market but also in the welfare system (Alesina, Glaeser, and Sacerdote, 2001). Alesina, Baqir, and Easterly (1999) find that "a one standard deviation increase in the ethical fragmentation is associated with a fifth of a standard deviation decrease in the share of welfare spending" (p. 1263). Hence, natives could fear that refugees "exploit the welfare state", leaving them with less for themselves.

However, it is unlikely that natives base their subjective assessments of institutional quality only on economic studies. Instead, the perhaps simplest yet most powerful explanation is that populist political parties gained huge momentum and converted it into anti-immigration rhetoric that mobilized and convinced a large number of people. The "capture" of the refugee crisis by populists was the purveyor for an "European Trust Crisis" (Algan, Guriev, et al., 2017) which planted the seeds for the rise of populism in Europe. Germany was no exception to this development with the right-wing populist AfD being the strongest opposition party after the 2017 general election. The fearmongering of populists covered both the cultural and the economic sphere. The debate around the effects of refugees on the welfare state was especially controversial. Bartholomae, Woon-Nam, and Rafih (2020) find that "the occurrence of welfare-chauvinistic effects [...] led to the success of the AfD in the 2017 election" (p. 1). The exaggerated fear of unprecedented negative welfare effects of refugees could have triggered a more negative assessment of natives' personal economic situation. The term 'economic refugee' was often employed to delegitimize legitimate reasons for flight and preach the myth of an 'immigration into the welfare state' (Giuletti, 2014). Hence, there are sufficiently reliable indications that populist parties used the European refugee crisis to stir of economic fears of the native population.

Overall, the hypothesis $H0$, that natives do not change their subjective assessment of the quality of economic institutions, can be rejected. Since, ceteris paribus, natives worry more about their personal economic situation in states with more refugees compared to states with fewer refugees, natives do change their assessment of economic institutional quality in response to refugee inflows. That being said, the effect is relatively small. Natives do not perceive refugees as a fundamental threat to economic stability.

4.1.2 The Perceived Erosion of Cultural Institutions

If one looks at the effect of refugees on cultural institutions, two model specifications are significant in my regressions.

First, the coefficient for concern about more immigrants coming to Germany is highly significant: natives living in a state with a higher refugee-to-population ratio are on average 0.0412 (1,3%) points

less concerned about more immigrants coming to Germany.

These results are in line with the empirical findings that refugee allocation can have negative effects on the electoral turnouts of anti-immigration parties (Steinmayr, 2016). In the case of Germany, Aksoy, Poutvaara, and Schikora (2020, p. 16) find that there is a high initial correlation of 0.57 between voting shares for the AfD and resentments against migrants. Given this high initial correlation, voting for a right-wing party can function as a meaningful proxy for anti-immigration beliefs. Contrary, Dustmann, Vasiljeva, and Piil Damm (2019) find that a larger local concentration of refugees leads to an increase in vote shares for right-wing parties with anti-immigration agendas using Danish electoral data. Dinas et al. (2019), Bratti et al. (2020) and Hangartner et al. (2019) find similar results for Italy and Greece. However, Dustmann, Vasiljeva, and Piil Damm (2019) also report that in urban municipalities "refugee allocation causes – if anything – a decrease in the vote share for anti-immigration parties." (p. 2035). They explain their result by computing an index of residential segregation for refugees and non-western immigrants and find that the involuntary exposure to refugees is about 10% higher in rural municipalities compared to urban municipalities (Dustmann, Vasiljeva, and Piil Damm, 2019, p. 2064). There seems to be a difference between rural and urban areas that could also influence my findings. In my treatment group, there are three densely populated city-states (Hamburg, Berlin, and Bremen) and the geographically and population-wise relatively small Saarland. This could mean that the low concern with more immigrants could also be driven by a rural versus urban divide within states. In my analysis I tried to control for this factor by using the population density of a state as a control variable.

The second significant variable in my regression is concern about crime rates. In states with a higher refugee-to-population ratio, natives are on average 0.03 points (1%) less concerned about crime rates. This results is interesting in two ways.

First, right-wing parties often connect higher number of refugees with rising numbers of crime (Hermanni and Neumann, 2019). If their hypothesis would be true, one would expect a higher subjective fear of crime rates in states with more refugees. Since the opposite is true, this speaks for the hypothesis that natives 'update their priors' in response to refugee exposure. Hence, part of the antidote against the "European Trust Crisis" created by populists could be to simply expose natives to the perceived threat. When natives realize that previous migrants have not caused a large scale surge in crime rates, they are less hostile to more migration.

The second interesting aspect of this finding is that a previous study on the effects of refugees on German natives has found that "the individual level of respondent's fear of crime represents a crucial moderator of the perception of refugees as threatening" (Hermanni and Neumann, 2019, p. 349). Hermanni and Neumann (2019) use representative survey data from Dresden, a city with high initial

vote shares for the AfD, to investigate what factors determine the perception of refugees. The role of natives' fear of crime as a crucial moderator highlights that natives perceive public safety as a crucial cultural institution. The positive effect of refugees on perceived crime rates could indicate that exposure to refugees significantly lowered the overall fear of refugees. Once refugees are not regarded as a threat to public security anymore, their assessment becomes more rational. These findings are supported by Schaub, Gereke, and Baldassarri (2021), who find that local exposure to refugees "pulled both right- and left-wing leaning individuals more towards the center" (p. 686). Less concern with crime rates does, however, not mean that refugees reduce crime rates, but rather that irrational fears are mitigated through an exposure effect. Overall, my results and previous studies such as Hermanni and Neumann (2019) and Schaub, Gereke, and Baldassarri (2021) show that exposure to refugees leads to a convergence to more central positions, lowering previously exaggerated fears on natives' through updating prior beliefs.

Overall, natives have fewer fears of cultural erosion in states with a higher refugee-to-population ratio. Exposure to refugees even reduces natives' fear of immigrants and higher crime rates. Therefore, I can refute $H2$ and accept $H3$.

4.1.3 The Full Model

After the interpretation of the average treatment effect, the rest of the model also holds some interesting insights as well. First, the compiled index of concern with institutional erosion is not significant. The regression table including every NUTS 1 control variable is reported in the appendix (Table A7). The state unemployment rate is negatively correlated with the perceived personal economic situation, the perceived overall situation, and concern about more hostility towards foreigners. As expected, the GDP per capita is also negatively correlated with concern about crime rates and concerns about job security. Interestingly, neither the previous migrant stock nor the interaction between the previous migrant stock and new refugee inflows is significant. That means that I do not find evidence of a "congestion" effect: Previous migrant stocks do not influence how natives perceive future refugees. In this regard, my results diverge from Nowrasteh and Powell (2020, p. 81) who finds evidence that previous migrant stocks affect the outcomes of contemporary refugees.

In a 2018 paper, Franz, Fratzscher, and Kritikos (2018) find that differences in local unemployment rates, education levels, and shares of refugees and foreigners only explain little variation in the vote share of the AfD in the 2017 general election. Instead, they report that an ageing populating and negative demographic trends are the strongest predictors for voting turnouts for the AfD. However, in my analysis the population density is only significant in one model specification while demographic

trends are significant in 3 model specifications. The effect of population density is very minor; For example, an increase of 100 people per square kilometer in a state would only lead to a 0.6% increase in concerns about crime rates. The same minor effect can be observed with regards to demographic trends; A net increase of 1000 people in the crude rate of national change of population would increase the concern about jobs only about 0,06%. Hence, contrary to Franz, Fratzscher, and Kritikos (2018), population density and demographic trends only play a minor role in assessing perceived institutional quality.

4.1.4 Comparing Subjective and Objective Institutional Erosion

My analysis has demonstrated that the overall impact of immigration on the perceived personal economic situation is slightly negative. On the other hand, exposure to immigrants reduces natives' fears of crime and immigrants. To assess whether this change of subjective institutional quality is rational, one must compare the subjective erosion of institutions against objective measures. The subjective increase in concern about crime rates could be a result of fearmongering of right-wing parties. Frauke Petry, at this time chairwomen of the AfD, lamented that the situation will escalate the more "uneducated and often aggressive young male refugees will come and commit crimes", which could lead to "civil war like circumstances" (quoted in: Gensing and Reisin, 2020).

Looking at crime rates, there has not been a significant overall spike since the European migrants crisis and the long-term amount of criminal offences is declining since 2016 (Figure B11). However, there are some caveats when interpreting these findings. The number of committed and attempted homicides by refugees has risen from 233 in 2015 up to 447 in 2017 (Gensing and Reisin, 2020). While this number has gone down to 357 in 2019, refugees are still severely overrepresented in homicide statistics compared to natives (Gensing and Reisin, 2020). The picture for other severe criminal offences such as mayhem and sexual offences looks similar. This development could at least indicate that something like Borjas' institutional erosion is slowly starting to take place. However, the overwhelming majority of refugees that came during the migrant crisis were young men between 18 and 25 that are statistically already overrepresented in crime statistics. Moreover, refugees are often severely traumatized and face limited opportunities to work and uncertainty regarding the outcome of their asylum application. In the wake of the overrepresentation of refugees in severe criminal offences, there might be a justification that people fear an erosion of public safety in the short term, despite an overall decrease of overall criminal offences. However, the research from other mass migration instances in the past suggests that cultural transmission is taking place rather slowly (Algan and Cahuc, 2010). If these findings are applicable to Germany's situation, then it can be expected that crime rates of refugees will continue to fall in the upcoming years.

Moreover, people in states with a higher refugee-to-population ratio have expressed less concern about immigrants coming to the country. I assume that an exposure effect causes the positive assessment, just as Steinmayr (2016, p. 24) hypothesizes. On the other hand, they are slightly more concerned about their personal economic situation. The literature on the effects of immigration on wages has been discussed in length in Chapter 2. As I emphasized, many of the previous studies on the impact of refugees on wages are hard to compare due to the legal obstacles of entering the labour market in Germany (See: Fallah, Krafft, and Wahba, 2019) or due to the long time period of often more than 10 years they observe (d'Albis, Boubtane, and Coulibaly, 2018). While the adjusted median wage has been slightly growing in Germany since 2015 (OECD, 2021), it is unclear how much of the trend is explained through immigration. Hence, a detailed assessment of the influence of refugees on wages in Germany after the European Migrant Crisis remains to be done. However, there is evidence that refugees in Germany after the European Migrant Crisis have not displaced native workers in the short run but are themselves struggling to find employment (Gehrsitz and Ungerer, 2018). Fasani, Tommaso, and Minale (2020) find that employment bans for refugees at the initial place of residence reduces the probability of employment by around 15% in subsequent years. Moreover, assuming that there is an effect of refugees on low-wage workers, it does seem to be very time-persistent. A recent study of European countries from 1995 to 2017 finds a negligible effect of refugees on the growth of low-wage work in continental Europe that vanishes after 5 years (Andersson, Eriksson, and Scocco, 2019).

In conclusion, neither the economic situation nor developments in crime rates suggest that an objective large scale institutional erosion is taking place. No reviewed study here finds evidence for a substantial economic erosion. With regards to crime rates, it is true that some problems emerged in response to refugee inflows. However, it is hardly justifiable to speak of "civil war like circumstances in Germany" (Gensing and Reisin, 2020) and the downfall of public security, considering that crime rates are continuously falling since 2016.

4.2 Individual Dynamics

To research how individual characteristics influence the perceived erosion of certain institutions in response to migration, I must treat the data as repeated cross-sectional data. The feature of within-fixed effects or difference-in-difference estimations using a panel data structure is that time-invariant unobservable characteristics, such as gender or immigration status, are already controlled for. Henceforth, trying to control for time-invariant observable characteristics in a panel data setting leads to an omission due to collinearity. Hence, I must treat the data as repeated cross-sectional data to analyse individual effects. The results of the cross-sectional OLS are reported in Table A8. Due to the word limitation of this essay, I cannot elaborate on the size of all individual characteristics in all differ-

ent model specifications. Hence, I will only explain certain effects that I regard as especially interesting.

4.2.1 Health

First, I investigate the effect of an individual's health status on concern about various institutions. The partial marginal effects are plotted in Figure B12. Individuals with a better self-reported health status also are up to 0.3 points (10%) less concerned about institutional erosion overall. The relationship between self-reported health status and individual perceived institutional erosion is negative and gradual. Several studies establish a link between peoples' economic outcomes and their health status (Cole and Neumayer, 2006, Remes et al., 2020. Thus, it appears to be evident that individuals with a worse self-reported health status are more worried about, for example, their job security or their personal economic situation. Moreover, a worse health status could induce a more general pessimistic worldview which favors cultural pessimism as well.

4.2.2 Age

Second, the predictive margins of age groups show an approximate parabola (Figure B13). People under 25 and over 65 report low levels of perceived institutional erosion, while the middle-aged group from 35-55 reports the highest levels of institutional erosion. Interestingly, there is a divide among age groups in their assessment of cultural versus economic institutions. Looking at economic institutions, younger people express 0.2-0.3 points (6%-10%) more concern about institutional erosion (Figure B15). This could mean that the economic wellbeing of young people is closely tied to the overall economic development. During recessions, young and inexperienced labour market participants are often particularly hard hit (Aassve, Cottini, and Vitali, 2013). Contrary, retired seniors are often not immediately affected by negative developments in the labour market. This dependency could, at least partly, explain the serenity of older people in their assessment of economic institutions. Similarly, a significant part of people under 25 are possibly still enrolled in higher education or employed as apprentices. Hence, they are less likely to view refugees as immediate competitors in the labour market.

Looking at the assessment of cultural institutions on the other hand indicates that older people are on average about 0.1 points (3 %) more worried about migrants' effect on culture (Figure B16). The more negative assessment of the cultural impact of migrants by older natives could be driven by a more general attitude of *cultural pessimism*. Franz, Fratzscher, and Kritikos (2018) find similar results when they report that an ageing population is one of the strongest predictors for positive electoral

outcomes for the AfD.

4.2.3 Education

The educational levels of the respondents also contain interesting information on how education shapes perceived institutional erosion. The marginal effect of education on perceived institutional quality is plotted in Figure B17. One interesting finding is that people with a lower level of education have more concerns about their job security, their personal economic situation, and about the overall economic situation while controlling for relevant confounders. In the overall index, natives with a high school diploma (A-levels) are on average 0.2 (6%) points less concerned about institutional erosion than native drop-outs. This could have several reasons. In his critique of Card (1990) paper on the effects of the Mariel Boatlift people on wages, Borjas (2017) matches the skill level of immigrants to natives and finds that supplementary immigration disproportionately affects the wages of low-skilled workers. Whilst these results are highly contested (Clemens and Hunt, 2019), other papers have found similar results for lower educated people and high school dropouts (Dustmann, Frattini, and Preston, 2012). Moreover, the standard labor market competition model asserts that natives' will be most opposed to immigrants who match their skill level due to concerns about labour market competition (Haaland and C. Roth, 2020). Henceforth, it can be assumed that both the skill level of refugees and the skill level of natives are one of the key determinants of natives' attitudes towards refugees (Hainmueller and Hiscox, 2010). Thus, the fears of lower educated people in states with more refugees could be rational. The interaction effect between education and age shows that fears of institutional erosion are most prevalent among middle-aged, low-educated natives (Figure B14). People with higher education (A-levels and technical college certificates) fear institutional erosion less than lower educated throughout their life. One exception is that below the age of 25, the differences in education levels have little effect on perceived institutional erosion.

4.2.4 Gender

If one compares gender dynamics in the SOEP-CORE dataset, it becomes evident that women on average express more concerns than men about institutional erosion (Figure B18). However, the effect is neither significant nor very large; females only worry on average 0.06 (2%) points more than males about institutional erosion.

Overall, I find convincing evidence that the respondents' characteristics influence their assessment of institutional erosion. Especially the differences between age groups and education seem to have a great impact on the perceived institutional quality. Hence, my findings diverge from Hermanni and Neumann

(2019), who find that respondents' age, gender, education, and income status does not significantly influence their perception of refugees.

5 Robustness Checks

5.1 Endogenous Treatment

The Königssteiner Key is based to $\frac{2}{3}$ on tax revenue generated by the state and to $\frac{1}{3}$ on the GDP per capita of that state. d'Albis, Boubtane, and Coulibaly (2018) have demonstrated that there exists a Granger-causality between a higher number of refugees and GDP per capita growth in Western Europe in the long run. However, due to obstacles in labour market participation and uncertainty over their asylum application, refugees can be a macroeconomic burden in the short term (d'Albis, Boubtane, and Coulibaly, 2018, p. 3) and have little to no impact on short-term GDP growth. Thus, the assignment of the treatment cannot only be written as a function of allocation of refugees according to an exogenous mechanism but also as a function of GDP per capita and previously assigned number of refugees. In this case, the estimated ATE is inconsistent because the treatment assignment is endogenous. This would imply that the covariance between the variable of interest and the error term is not 0:

$$SOEP_{c,t,i} = \alpha + \beta_1 Time * Treated + \beta_2 Controls + \epsilon_{c,t,i} \tag{7}$$

with

$$COV(Treated, \epsilon_{c,t,i}) \neq 0 \tag{8}$$

Hence, the random assignment assumption would be violated and the ATE would be biased. To address the potential issue of an endogenous treatment, I perform a Two-Stage Least-Squares (2SLS) estimation as a robustness check. Therefore, I first regress the endogenous explanatory variable (Treated, which is based on the refugee-to-population ratio) on the instrument and all other exogenous variables.

$$Ref - to - pop_{c,t} = \alpha + \pi_1 GDP_{c,t} + \pi_2 Controls_{c,t,i} + \epsilon_{c,t,i} \tag{9}$$

Since now GDP should be uncorrelated with the error term $\epsilon_{c,t,i}$, I can estimate the unbiased coefficient of $\hat{\pi_1}$ and $\hat{\pi_2}$

$$Ref - \hat{to} - pop_{c,t} = \hat{\alpha} + \hat{\pi_1} GDP_{c,t} + \hat{\pi_2} Controls_{c,t,i} + \epsilon_{c,t,i} \tag{10}$$

Using the new coefficient for refugee-to-population ratio, I then can estimate the true values of the parameters $\hat{\beta}_1$ and $\hat{\beta}_2$:

$$SOEP_{c,t,i} = \hat{\alpha} + \hat{\beta}_1 Ref - \hat{to} - pop_{c,t} + \hat{\beta}_2 Controls_{c,t,i} + \epsilon_c, \hat{t}, i \tag{11}$$

In the same way, I also instrument population. The results of the 2SLS regression can be found in Table A9. A graphical analysis of the differences in ATE's between the normal model and the endogenous treatment model is shown in (Figure B19). I find that using a 2SLS before a difference-in-difference regression has no major effect on the size of the coefficients. However, the larger standard errors in the 2SLS regression cause some coefficients to be insignificant now. For example, the concern about crime is now only significant on a 10% level, while concern about immigration is still significant on the 5% level. In general, the standard errors in the endogenous treatment model are higher. But since the magnitude of the coefficients is not affected, and only the coefficient for the personal economic situation turned insignificant, I can assert that my results are robust to an endogenous treatment model specification.

5.2 A Multinominal Ordered Probit Model

Since the dependent variables in my models depicted in equation 5 are scaled on a 1 to 3 scale with 1 being "very concerned", 2 being "somewhat concerned" and 3 being "not concerned at all", one can also estimate them using a multinominal ordered probit model. In econometric practices, multinominal probit models are used to estimate ordinal outcomes. To avoid the pitfalls of using a linear probability model for estimating a binary outcome (See: Wooldridge, 2016 p. 249), a transformed function G that strictly takes values only between 0 and 1 is used to model the binary outcome (Wooldridge, 2016, p. 560)

$$P(y = 1|x) = G(\beta_0 + \beta_1 x_1 + ... + \beta_k x_k) = G(\beta_0 + x\beta) \tag{12}$$

With

$$0 < G(z) < 1 \tag{13}$$

The results of the ordered probit regression are reported in Table A10. A graphical comparison of the effect sizes in the normal model and the effects sizes in the ordered probit are shown in figure B20. The size of the coefficients is again only slightly affected by the different model specifications. However, there are again slight changes in the standard errors. In the ordered probit model, only the value for concerns about immigration is significant on the 10% level. In general, the standard errors

in the ordered probit model are larger than in the ordered probit model. Nonetheless, my results is robust to a multinominal ordered probit model specification.

5.3 Alternative Lags

Using leads and lags in panel data settings can be useful to adjust for either seasonal or long-term trends. When estimating the local economic conditions, studies regularly employ different leads and lags. Aksoy, Poutvaara, and Schikora (2020) and Åslund and Rooth (2007) propose a 2-year lag for unemployment as a control variable for measuring local economic conditions. Lagging GDP per capita could also provide interesting findings. If, for example, only the lagged GDP per capita values would be significant, we could assume that a state-level specific steady state of economic development has been reached (Bassanini and Scarpetta, 2003, p. 22). This would for example contradict an endogenous growth model and speak against the findings of d'Albis, Boubtane, and Coulibaly (2018), that refugees Granger-cause a higher GDP per capita.

In my original model, I have included no lags due to potential endogeneity in the treatment timing. A low GDP per capita in 2013 or 2014 could determine the treatment or control status of a state in 2015 when the European Migrant Crisis occurred. Nonetheless, I still examine whether my results are robust when I include different lag specifications of relevant control variables. Therefore, I analyse whether alternative lag specifications have an impact on the size and significance of the three coefficients that were significant in my standard model (Concern about crime, immigrants, and the personal economic situation). I then run 2 regressions per coefficient where I first lag every economic control variable by the factor 1 and then by the factor 2. As an example, the model "Crime Lag 1" runs the following equation:

$$\begin{aligned} SOEPScore_{i,c,t} =& \alpha + \beta_1 * TimeDummy_{2015} * Treated_{c,2015} + \beta_2 * \delta_{i,c,t} + \beta_3 NUTS1_{c,t-1} \\ &+ \beta_4 Migrant_stock_{c,t-1} + \beta_5 RefugeeInflows_{c,t-1} \\ &+ \beta_6 Migrant_stock_{c,t} * RefugeeInflows_{c,t-1} + \epsilon_{i,c,t} \end{aligned} \tag{14}$$

Where NUTS 1 refers to the state-level fixed effects on NUTS 1 level all lagged by the factor 1. The equation is then repeated for each dependent variable and each lag specification. The results of the regressions are displayed in Table A11. The visual representation of the ATE is displayed in Figure B21. As one can see in the regression table, the size of the coefficients is again not affected by the lag specifications. However, the lag specifications cause slight changes in the p-values of the coefficients. The ATE for concern about crime and immigration are robust to different model specifications using

alternative lags. The ATE for concern about the personal economic situation is, however, not robust to lagging control variables. This indicates that natives assess their personal economic situation dynamically.

5.4 Different Standard Errors

To investigate, whether the low amount of clusters in my analysis leads to a downward bias in my standard errors, I employ both Bell and McCaffrey (2002) and Donald and Lang (2007) methods of adjusting standard errors as robustness checks (Table A12 and Table A13). My robustness checks yield that using Bell and McCaffrey's standard errors does not significantly affect my results. However, none of my results are significant if I employ Donald and Lang's standard errors.

6 Limitations

One of the most severe limitations of my approach is the high variance within states. Most of the studies researching the effect of refugees on natives used data on either NUTS 2 or NUTS 3 level to study the effect of immigration on the host country population (Dustmann, Vasiljeva, and Piil Damm, 2019; Steinmayr, 2016; Katz, B., Noring, L., & Garrelts, N., 2016). Including more local control variables enables a more fine-grained analysis that better exploits the variation between entities. For example, it is likely that the urban versus rural divide plays an important part in natives' perception of institutional quality, as the results from Franz, Fratzscher, and Kritikos (2018) and Schaub, Gereke, and Baldassarri (2021) suggest. However, due to data protection laws, an analysis on either NUTS 2 or NUTS 3 level is only possible at the DIW Center in Berlin, which had no capacity during the COVID-19 pandemic. This makes a more local analysis in my case difficult.

Another possible limitation is that the differences in the refugee-to-population ratios are very minor across states. The absolute number of refugees assigned to states is relatively low compared to the number of state inhabitants, which leads to a very small range of refugee-to-population ratios (0.009% in Thuringia to 0.02% in Bremen). Henceforth, the cut-off point at the mean can be perceived as arbitrary, if one considers that the difference between Schleswig-Holstein's (Control) and Hesse's (Treatment) refugee-to-population ratio is less than 0.001%. However, since the assignment of refugees is not completely random, but based on the GDP per capita of a state and its population, the exogenous allocation of small, relative numbers of refugees can serve as a proxy of how immigrants

would self-select in the absence of an exogenous allocation mechanism. Hence, the small absolute number and the small relative differences in the refugee-to-population ratio across states can nonetheless convey important information on the effects of refugees and migrants on natives' perceived institutional quality.

Moreover, it could be the case that the lack of some individual control variables could cause an omitted variable bias. For example, due to data protection laws, I do not have access to data on natives' exact income. Since income is likely to be correlated with perceived institutional quality, leaving out income effects could lead to an omitted variable bias. In this case, the ATE's in my model would not be consistent. I do, however, take care of this problem by controlling for variables that are at least indirectly strongly correlated with income, such as employment status and education.

Last, the individual characteristics of refugees and migrants are crucial in determining the perceived institutional quality of natives'. Using survey data from 4000 adults in Germany, Lergetporer, Piopiunik, and Simon (2021) demonstrate that informing natives about the education level of refugees "increases labor market competition concerns, decreases fiscal burdens concerns and positively affects general attitudes toward refugees" (p. 1). These insights make it harder to aggregate the effect of refugees with heterogeneous individual profiles on natives' on a state level. The lacking data on refugee characteristics also makes it impossible to account for agglomeration effects among refugees from the same ethnic group. A recent study by Stips and Kis-Katos (2020) using German refugee data shows how ethnic groups among refugees function as informal networks that can grant formal labour market access. Hence, the number of refugees from the same region can be an important determinant of the assimilation rate of refugees that affects the degree of institutional erosion. In future studies, this data limitation could be overcome by using the confidential IAB-SOEP-BAMF survey of refugees (DIW, 2021) instead of aggregated data from the Federal Office of Statistics.

7 Conclusion

In this dissertation, the effect of refugees on natives' perception of institutional quality in Germany was investigated. In exploiting the exogenous allocation of refugees across states using a Difference-in-Difference model, I found that the overall effect of refugees on natives' perception of institutional quality is relatively small. The effect of refugee inflows on natives' assessment of their own personal economic situation is slightly negative. Contrary, I found robust evidence that exposure to refugees lowers natives' fear of immigrants and rising crime rates. By looking at respondents' individual characteristics, I found that middle-aged low-educated people have the highest overall concern about institutional erosion. Moreover, my findings suggest that elderly people worry more about cultural erosion while

younger people are more concerned about the erosion of economic institutions. Both effects are very minor.

My findings speak against the hypothesis that the European Migrant Crisis caused a large-scale institutional erosion in Germany. Since the inflow of large numbers of often traumatized refugees had little effect on institutions, it is unlikely that migrant waves will have a significantly different effect. Therefore, my results are in line with Nowrasteh and Powell (2020, p. 283) assessment that the new economic case for migrations restrictions overstates the institutional erosion in response to migration, at least in the short term. There might be detrimental effects on economic growth in the medium to long term, which remain to be explored. The new economic case for migration restrictions does not provide substantial reasons to cap migration limits. Whether any moral or sociological argument provides sufficient justifications for migration controls is for others to judge.

There are no direct policy implications that should be drawn from my research. However, my findings support the hypothesis that the fearmongering of populists led to an initial fear of institutional erosion of natives due to refugee inflows. Therefore it remains important that democratic parties wage the war against populist anti-refugee rhetoric. Convincing natives that refugee inflows do not cause the downfall of economic and cultural institutions would be an important step towards a more rational debate on migration policy.

8 References

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A Appendix Tables

Table A5: Determinants of Refugee Allocation

VARIABLES	(1) 2011	(2) 2012	(3) 2013	(4) 2014	(5) 2015	(6) 2016	(7) 2017	(8) 2018
population	0.00472***	0.00471***	0.00550***	0.00768***	0.0105***	0.0190***	0.0187***	0.0194***
	(3.59e-05)	(3.91e-05)	(2.98e-05)	(3.32e-05)	(2.12e-05)	(3.71e-05)	(4.29e-05)	(5.53e-05)
gdp_per_cap	1.108e+06***	1.231e+06***	1.021e+06***	1.344e+06***	803,726***	2.513e+06***	2.198e+06***	2.411e+06***
	(26,458)	(27,378)	(19,881)	(28,095)	(14,899)	(33,173)	(37,606)	(44,459)
unemployment_rate	1,509***	1,678***	921.1***	2,084***	2,978***	11,929***	11,392***	13,795***
	(52.84)	(46.90)	(47.66)	(70.94)	(84.29)	(280.5)	(356.1)	(413.9)
demographic_trends	-1.260***	-1.537***	-1.424***	-2.086***	-1.730***	-3.883***	-3.938***	-4.032***
	(0.0118)	(0.0121)	(0.00834)	(0.0121)	(0.00797)	(0.0197)	(0.0207)	(0.0226)
population_density	5.621***	7.055***	8.052***	10.11***	12.02***	9.818***	13.28***	11.97***
	(0.119)	(0.0926)	(0.0937)	(0.125)	(0.136)	(0.315)	(0.358)	(0.349)
age	-13.08***	-8.031**	-14.97***	-5.397	-13.21***	-44.26***	-63.90***	-73.20***
	(3.690)	(3.503)	(2.957)	(3.528)	(2.734)	(6.386)	(6.921)	(7.401)
Constant	-56,203***	-63,640***	-52,714***	-78,341***	-61,915***	-181,380***	-167,537***	-185,413***
	(990.4)	(1,001)	(746.3)	(1,113)	(785.9)	(2,136)	(2,415)	(2,754)
Observations	18,719	18,356	21,517	20,941	24,344	26,284	29,756	27,723
R-squared	0.959	0.970	0.979	0.981	0.991	0.982	0.978	0.980

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A6: Correlation Matrix of Covariates

	Sex	Education	Immigrant	Health	Children	Age	Employment	Unempl.	Pop.-Density	GDP	Demographics	Migrants
Sex	1.0000											
Education	0.0041	1.0000										
Immigrant	-0.0187	-0.0079	1.0000									
Health	0.0628	-0.2078	-0.1555	1.0000								
Children	0.1428	-0.0885	0.0056	0.1224	1.0000							
Age	0.0225	-0.1799	-0.2900	0.2870	0.3751	1.0000						
Employment	0.2169	-0.1859	0.0620	0.1686	-0.0099	0.1695	1.0000					
Unempl.	0.0137	0.0224	-0.1910	0.0620	0.0280	0.1239	0.0186	1.0000				
Pop.-Density	-0.0025	0.0803	0.0315	0.0050	-0.0375	-0.0080	0.0245	0.3874	1.0000			
GDP	-0.0096	0.0427	0.2005	-0.0630	-0.0493	-0.1124	-0.0107	-0.6594	0.2076	1.0000		
Demographics	-0.0068	-0.0029	0.0113	-0.0155	-0.0048	-0.0317	0.0054	-0.1097	0.2161	0.2298	1.0000	
Migrant	-0.0135	0.0705	0.2372	-0.0552	-0.0653	-0.1369	0.0020	-0.4888	0.4157	0.8595	0.1115	1.0000

Table A7: Full Model

VARIABLES	(1) Crime	(2) Job	(3) Personal Econ	(4) Overall Econ	(5) Hostility	(6) Immigrants	(7) Index
Unemployment	0.00972	-0.0131*	-0.0202***	-0.0125**	-0.0188**	0.00511	-0.00804
	(0.00690)	(0.00615)	(0.00553)	(0.00526)	(0.00876)	(0.0113)	(0.00560)
Demographic Trends	-8.34e-07	-1.27e-06**	-1.57e-06	2.16e-07	-2.30e-06***	1.44e-07	-1.61e-06**
	(7.77e-07)	(4.92e-07)	(9.35e-07)	(7.37e-07)	(7.06e-07)	(1.23e-06)	(5.94e-07)
GDP per capita	-12.27**	-14.51*	0.964	8.248	0.979	-12.99	-3.205
	(5.356)	(7.466)	(3.933)	(4.799)	(11.85)	(8.442)	(4.160)
Population Density	0.000192***	6.67e-05	-5.81e-06	-4.76e-05	-5.35e-05	0.000145	4.42e-05
	(6.45e-05)	(6.31e-05)	(4.77e-05)	(5.32e-05)	(9.66e-05)	(0.000102)	(6.05e-05)
Migrant Stock	-0.0100	-0.0122	-0.00206	0.0106	0.0184	-0.00171	-8.54e-05
	(0.0118)	(0.0141)	(0.0106)	(0.0105)	(0.0180)	(0.0143)	(0.0108)
Refugee Inflows	-3.04e-07	-1.44e-07	3.95e-07*	-1.03e-07	9.77e-09	-2.99e-07	-2.84e-08
	(3.27e-07)	(5.05e-07)	(2.05e-07)	(3.09e-07)	(2.75e-07)	(3.70e-07)	(1.37e-07)
Refugees * Migrants	1.28e-08	-1.88e-08	1.07e-08	3.22e-09	3.29e-08	8.65e-09	
	(3.28e-08)	(1.19e-08)	(2.07e-08)	(1.97e-08)	(2.28e-08)	(9.20e-09)	
Constant	2.315***	3.097***	2.282***	1.735***	2.120***	2.505***	2.283***
	(0.192)	(0.218)	(0.155)	(0.185)	(0.379)	(0.248)	(0.153)
Observations	166,669	118,141	190,681	185,695	171,105	166,466	191,011

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A8: Individual Effects from the Full Model from Equation 5

VARIABLES	(1) Crime	(2) Job	(3) Personal Econ	(4) Overall Econ	(5) Hostility	(6) Immigrants	(7) Index
Age 25-35	0.000531	-0.0193	-0.102***	-0.0461	-0.0201	-0.0490***	-0.0692***
	(0.0264)	(0.0347)	(0.0268)	(0.0367)	(0.0268)	(0.0157)	(0.0135)
Age 35-45	-0.00959	0.00526	-0.0580	-0.0874***	-0.0646**	0.00981	-0.0827***
	(0.0350)	(0.0299)	(0.0338)	(0.0289)	(0.0279)	(0.0152)	(0.0206)
Age 45-55	-0.0295	0.0217	0.0192	-0.0797**	-0.0929***	-0.0279	-0.0843***
	(0.0307)	(0.0356)	(0.0272)	(0.0315)	(0.0250)	(0.0168)	(0.0168)
Age 55-65	-0.0901***	0.173***	0.189***	-0.0894**	-0.121***	-0.0141	-0.0512***
	(0.0276)	(0.0300)	(0.0274)	(0.0382)	(0.0264)	(0.0178)	(0.0125)
Age > 65	-0.146***	0.424***	0.525***	-0.0237	-0.0347	0.0329	0.0328**
	(0.0322)	(0.0223)	(0.0179)	(0.0346)	(0.0280)	(0.0241)	(0.0138)
Female	-0.0989***	0.0278*	-0.00647	-0.0687***	-0.125***	-0.0279**	-0.0553***
	(0.00836)	(0.0148)	(0.00777)	(0.00615)	(0.0100)	(0.0107)	(0.00450)
Immigrant	-0.0433***	-0.102***	-0.122***	-0.0450***	0.0698***	0.0160	-0.00146
	(0.0119)	(0.0201)	(0.0121)	(0.0139)	(0.0136)	(0.0252)	(0.00814)
Relationship	-0.0480***	0.0397***	0.0719***	-0.0244***	-0.0407***	-0.0385**	-0.00768
	(0.00864)	(0.00775)	(0.0111)	(0.00679)	(0.0117)	(0.0151)	(0.00553)
Health: Good	-0.0887***	-0.0987***	-0.146***	-0.0699***	-0.0513**	-0.0992***	-0.108***
	(0.0104)	(0.0154)	(0.0170)	(0.0120)	(0.0192)	(0.0237)	(0.0104)
Health: Ok	-0.159***	-0.199***	-0.293***	-0.147***	-0.0767***	-0.166***	-0.187***
	(0.0134)	(0.0258)	(0.0160)	(0.0129)	(0.0192)	(0.0156)	(0.00911)
Health: Bad	-0.185***	-0.307***	-0.418***	-0.176***	-0.0809***	-0.210***	-0.239***
	(0.0130)	(0.0245)	(0.0152)	(0.0149)	(0.0194)	(0.0218)	(0.00867)
Health: Very Dad	-0.232***	-0.409***	-0.575***	-0.200***	-0.0475*	-0.267***	-0.289***
	(0.0196)	(0.0497)	(0.0317)	(0.0343)	(0.0240)	(0.0336)	(0.0143)
Children	0.0121	-0.00610	-0.0404***	-0.00513	0.0171	0.00633	-0.000454
	(0.0117)	(0.0151)	(0.0126)	(0.0112)	(0.0164)	(0.0195)	(0.00872)
Part-Time	0.0116	0.0353*	-0.0875***	0.0189	-0.0135	0.000531	-0.00728
	(0.0155)	(0.0167)	(0.0113)	(0.0137)	(0.00888)	(0.0159)	(0.00796)
Unemployed	-0.00540	0.0211	-0.134***	-0.00135	0.0129	-0.00270	-0.103***
	(0.0164)	(0.0385)	(0.0127)	(0.00947)	(0.00981)	(0.0141)	(0.00742)
Secondary school	0.0942***	0.0969***	0.0929***	0.0237*	-0.0114	0.0865***	0.0588***
	(0.0198)	(0.0114)	(0.0184)	(0.0112)	(0.0137)	(0.0293)	(0.0144)
Advanced high school	0.246***	0.173***	0.193***	0.0790***	-0.00786	0.307***	0.157***
	(0.0429)	(0.0203)	(0.0172)	(0.0247)	(0.0222)	(0.0485)	(0.0188)
A-Levels	0.392***	0.227***	0.262***	0.113***	-0.0361*	0.452***	0.223***
	(0.0308)	(0.0160)	(0.0109)	(0.0189)	(0.0197)	(0.0322)	(0.0149)
Religious	-0.0205	0.0142	0.00848	-0.0248**	0.000905	0.0260	-0.00279
	(0.0211)	(0.0189)	(0.0162)	(0.0102)	(0.0129)	(0.0235)	(0.0150)
Constant	2.228***	3.139***	2.191***	2.298***	1.741***	2.390***	2.579***
	(0.134)	(0.156)	(0.136)	(0.124)	(0.175)	(0.157)	(0.0811)
Observations	24,232	14,625	26,300	24,222	26,242	24,210	26,357
Adjusted R-squared	0.105	0.074	0.146	0.037	0.079	0.089	0.116

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A9: ATE using an Endogenous Treatment Model, where the Treatment Status is determined by GDP per capita and Population

VARIABLES	(1) Crime	(2) Job	(3) Personal Econ	(4) Overall Econ	(5) Hostility Foreigners	(6) Immigrants	(7) Index
ATE	0.0464*	-0.00489	-0.0211	-0.0157	0.0222	0.0761**	0.0184
	(0.0265)	(0.0190)	(0.0168)	(0.0216)	(0.0294)	(0.0369)	(0.0213)
Observations	166,669	118,141	190,681	185,695	171,105	166,463	191,011

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A10: ATE using an Ordered Probit Model

VARIABLES	(1) Crime	(2) Job	(3) Personal Econ	(4) Overall Econ	(5) Hostility Foreigners	(6) Immigrants	(7) Immigrants
ATE	0.0529	-0.00777	-0.0396	-0.0468	0.0157	0.102*	-0.0422
	(0.0449)	(0.0386)	(0.0291)	(0.0469)	(0.0569)	(0.0616)	(0.0456)
Observations	166,875	118,316	190,859	185,876	171,312	166,664	25,523

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A11: ATE using Different Lag Specifications

VARIABLES	(1) Crime Lag 1	(2) Crime Lag 2	(3) Immigration Lag 1	(4) Immigration Lag 2	(5) Personal Econ Lag 1	(6) Personal Econ Lag 2
unemployment_rate_lag1	0.000251		0.00173		0.000483	
	(0.00145)		(0.00146)		(0.00106)	
demographic_trends_lag1	-2.04e-08		1.57e-07		2.45e-07*	
	(1.11e-07)		(2.11e-07)		(1.27e-07)	
gdp_per_cap_lag1	0.373		0.286		-0.159	
	(0.327)		(0.376)		(0.283)	
population_density_lag1	-2.68e-07		-1.83e-06		-1.69e-06	
	(2.01e-06)		(2.62e-06)		(1.95e-06)	
migrant_rate_lag1	0.00635		0.0103		-0.0242	
	(0.0106)		(0.0105)		(0.0143)	
refugees_lag1	-4.66e-08		4.43e-09		3.57e-08	
	(2.75e-08)		(4.82e-08)		(4.61e-08)	
c.migrant_rate_lag1#c.refugees_lag1	4.45e-09**		2.76e-09		-2.19e-09	
	(2.07e-09)		(3.56e-09)		(3.14e-09)	
unemployment_rate_lag2		-0.00121		0.00279		-0.00126
		(0.00129)		(0.00167)		(0.00176)
demographic_trends_lag2		4.86e-08		2.53e-07		1.73e-07
		(1.94e-07)		(3.57e-07)		(1.72e-07)
gdp_per_cap_lag2		-0.774		0.767**		-0.591
		(0.531)		(0.300)		(0.616)
population_density_lag2		6.38e-06**		-6.20e-07		1.50e-06
		(2.96e-06)		(3.08e-06)		(3.04e-06)
migrant_rate_lag2		0.000938		-0.000644		0.00113*
		(0.000998)		(0.00102)		(0.000541)
refugees_lag2		4.63e-08		1.44e-07		1.50e-07
		(1.47e-07)		(1.89e-07)		(9.92e-08)
c.migrant_rate_lag2#c.refugees_lag2		-2.61e-09		-8.57e-09		-1.05e-08
		(1.06e-08)		(1.29e-08)		(6.94e-09)
ATE	0.0381***	0.0406***	0.0623***	0.0652***	-0.00630	-0.0118
	(0.00902)	(0.00957)	(0.0136)	(0.0134)	(0.00890)	(0.0115)
Constant	1.917***	2.002***	2.053***	2.113***	2.413***	2.239***
	(0.0808)	(0.0343)	(0.0847)	(0.0230)	(0.109)	(0.0382)
Observations	166,873	166,874	166,662	166,663	190,857	190,858

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A12: ATE using Bell and McCaffrey's Method of Standard Error Calculation

VARIABLES	(1) Crime	(2) Job	(3) Personal Econ	(4) Overall Econ	(5) Hostility	(6) Immigrants	(7) Index
ATE	0.0296**	-0.0146	-0.0199*	0.00133	-0.00322	0.0412***	0.00477
	(0.0107)	(0.0168)	(0.00951)	(0.00997)	(0.0133)	(0.0138)	(0.00681)
Observations	166,669	118,141	190,681	185,695	171,105	166,463	191,011

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A13: ATE using Donald and Lang's Method of Standard Error Calculation

VARIABLES	(1) Crime	(2) Job	(3) Personal Econ	(4) Overall Econ	(5) Hostility	(6) Immigrants	(7) Index
ATE	0.0398	0.0620	0.00749	0.0646	0.0504	0.0793	0.0581**
	(0.0476)	(0.0528)	(0.0371)	(0.0444)	(0.0514)	(0.0555)	(0.0268)

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

B Appendix Figures

Figure B1: Efficiency Gain from the Elimination of International Barriers

Table 1

Efficiency Gain from Elimination of International Barriers

(percent of world GDP)

All policy barriers to merchandise trade	
1.8	Goldin, Knudsen, and van der Mensbrugghe (1993)
4.1	Dessus, Fukasaku, and Safadi (1999)[a]
0.9	Anderson, Francois, Hertel, Hoekman, and Martin (2000)
1.2	World Bank (2001)
2.8	World Bank (2001)[a]
0.7	Anderson and Martin (2005)
0.3	Hertel and Keeney (2006, table 2.9)
All barriers to capital flows	
1.7	Gourinchas and Jeanne (2006)[b]
0.1	Caselli and Feyrer (2007)
All barriers to labor mobility	
147.3	Hamilton and Whalley (1984, table 4, row 2)[c]
96.5	Moses and Letnes (2004, table 5, row 4)[c]
67	Iregui (2005, table 10.3)[c,d]
122	Klein and Ventura (2007, table 3)[e]

[a] These studies assume a positive effect of trade on productivity; the other trade studies assume no effect.

[b] Change in consumption rather than GDP.

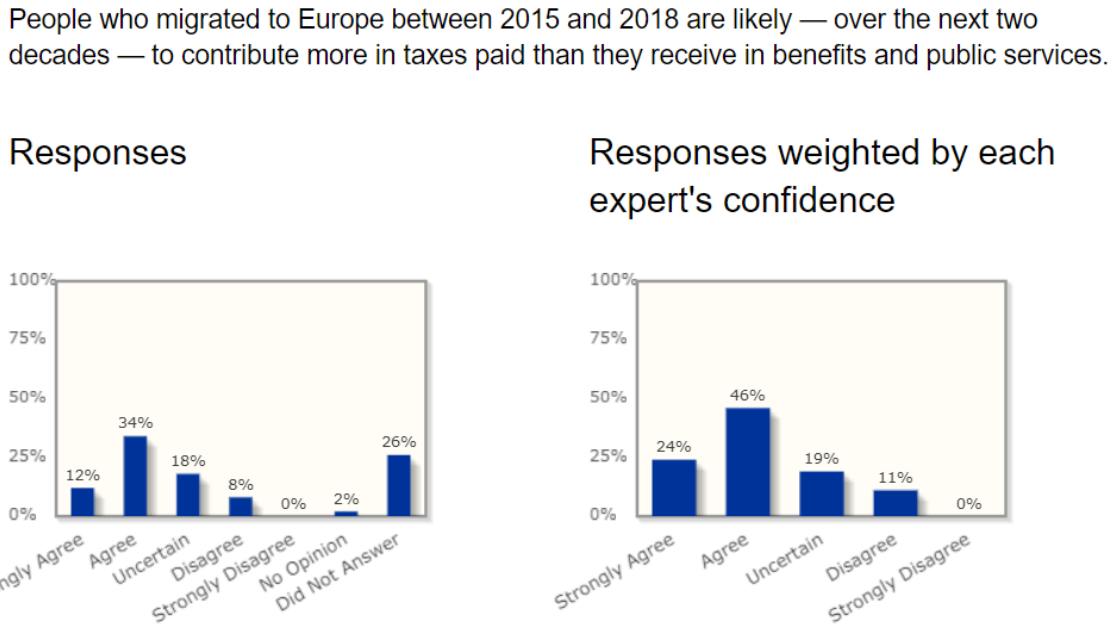
[c] Assumes two factors of production, immobile capital, and no differences in total factor productivity. Estimates from Hamilton and Whalley and from Moses and Letnes cited here assume no differences in inherent productivity of migrants and nonmigrants. Some much smaller estimates in Moses and Letnes assume that poor-country emigrants at the destination are 1/5 as productive as nonmigrants at the destination, which (as the authors note in their footnote 12) is certainly extremely conservative.

[d] Computable general equilibrium (CGE) model.

[e] Assumes three factors of production and international differences in total factor productivity in a dynamic growth model.

Source: Clemens, 2011, p. 85

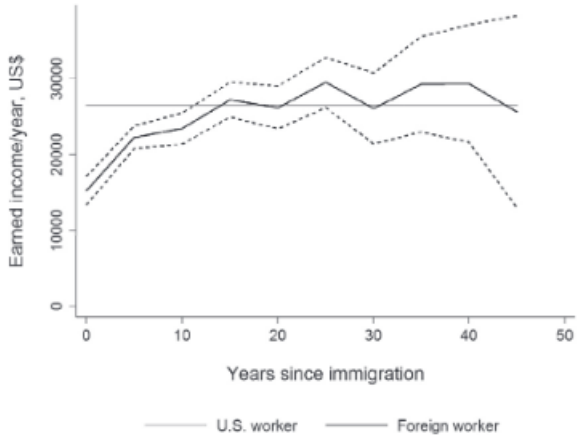
Figure B2: IGM Expert Panel



Answers of leading European economists to question of the effects of refugees on public finances. Source: IGM Expert Pa

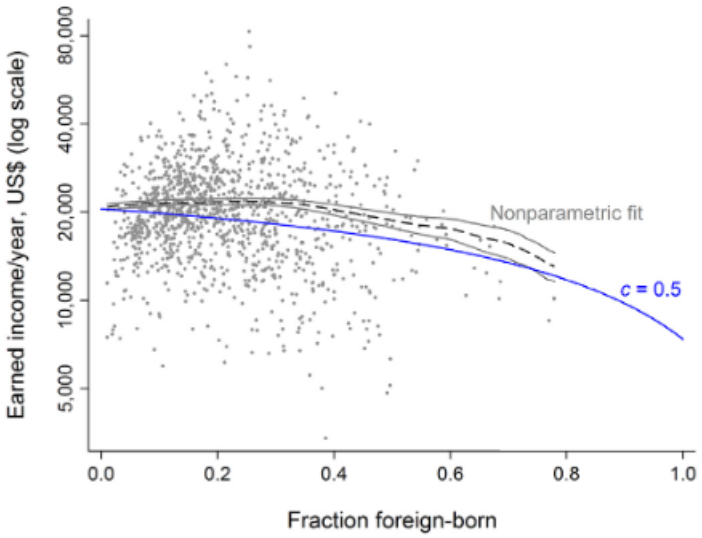
Figure B3: Assimilation of Wages of Ghanaian Migrants in the US after their Migration

(a) Ghana



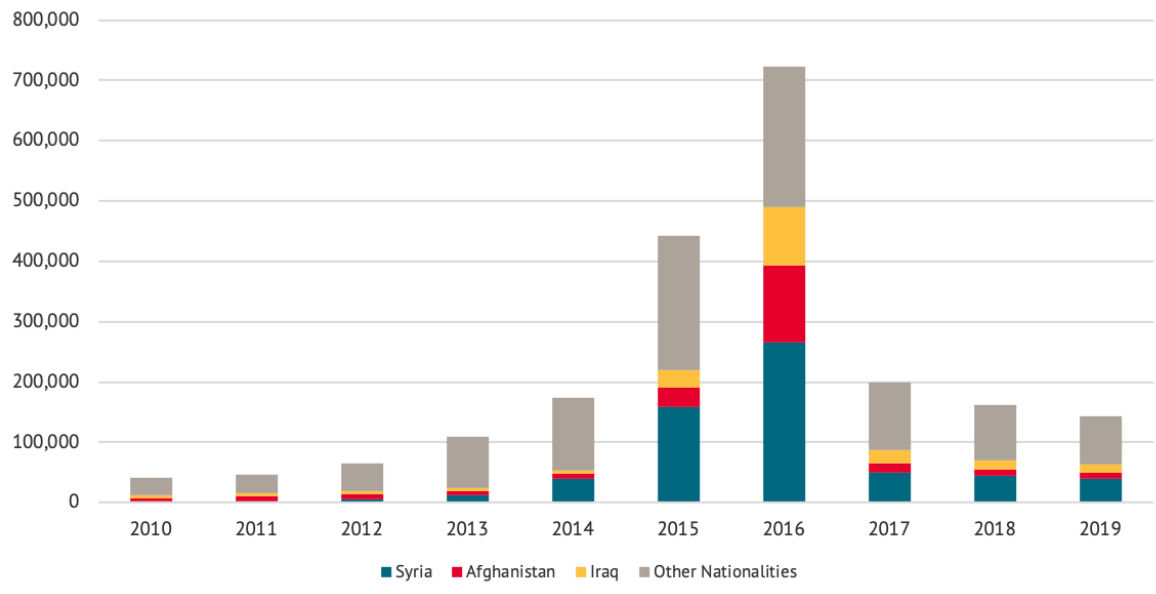
Source: Clemens and Pritchett, 2019, p. 160

Figure B4: Congestion Effect of Migrants in US Regions with non-parametric fit of 0.5 as Upper Bound



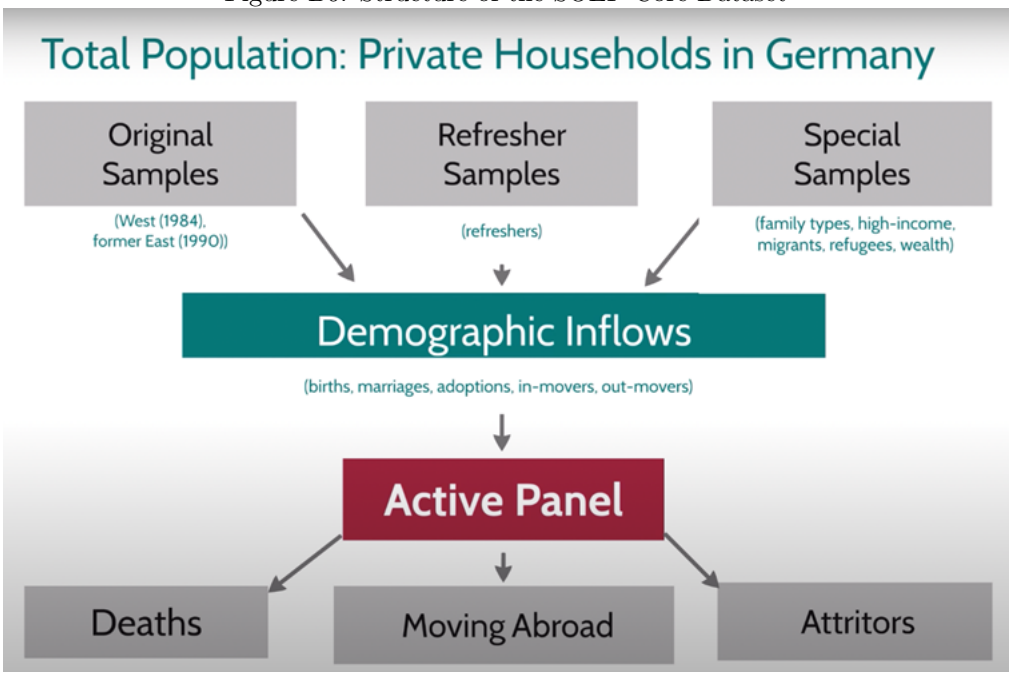
Source: Clemens and Pritchett, 2019, p. 161

Figure B5: First-time Asylum Applications in Germany, 2010-2019



Source: Keita, S. & Dempster, H., 2020

Figure B6: Structure of the SOEP-Core Dataset



Source: SOEP, 2019

Figure B7: All ATE's from the Full Model from Equation 5

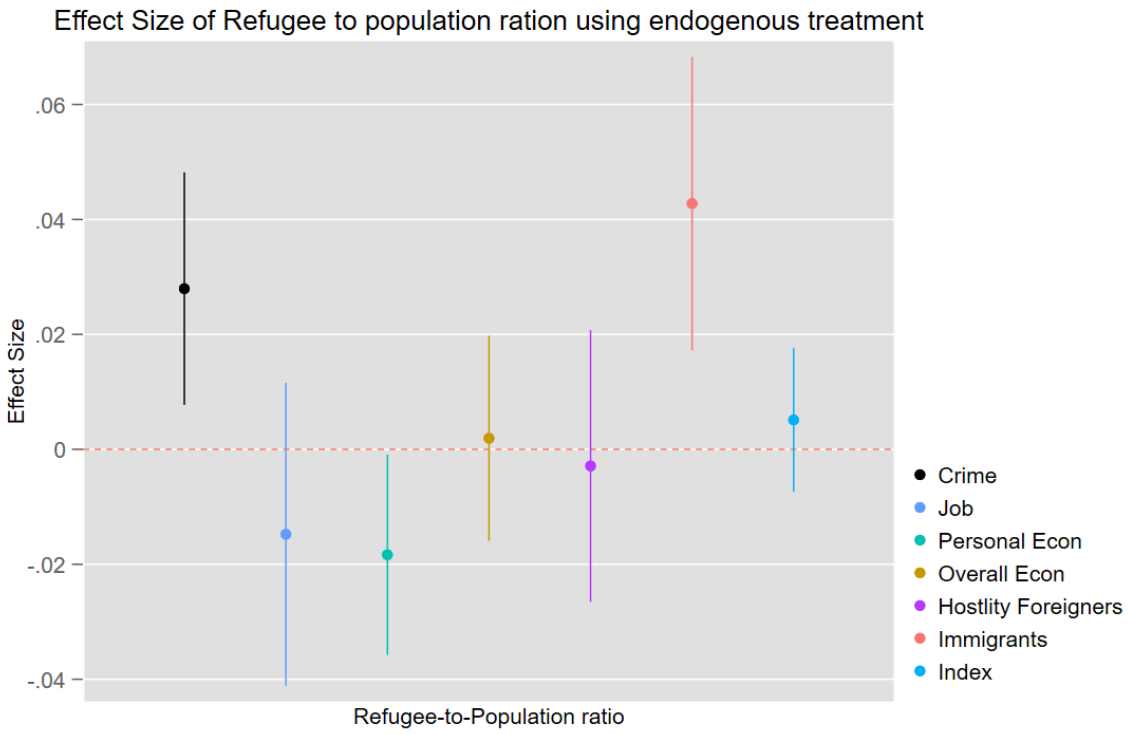


Figure B8: Parallel Trends for Concern about Crime

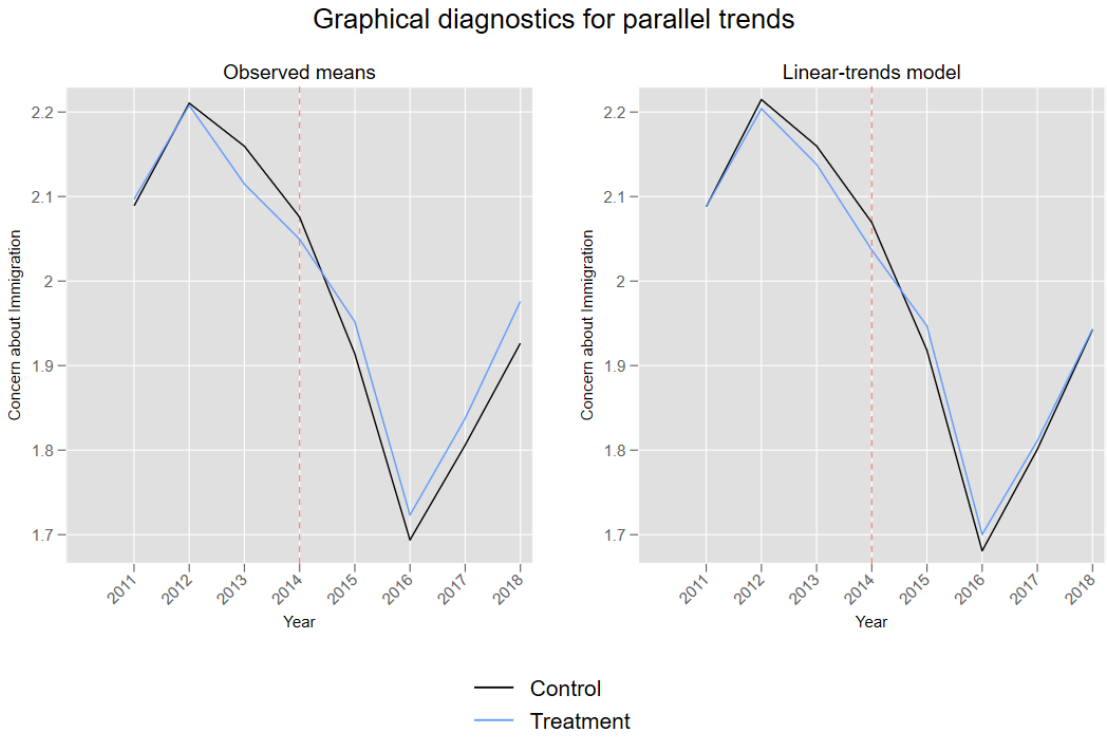


Figure B9: Parallel Trends for Concern about Personal Economic Situation

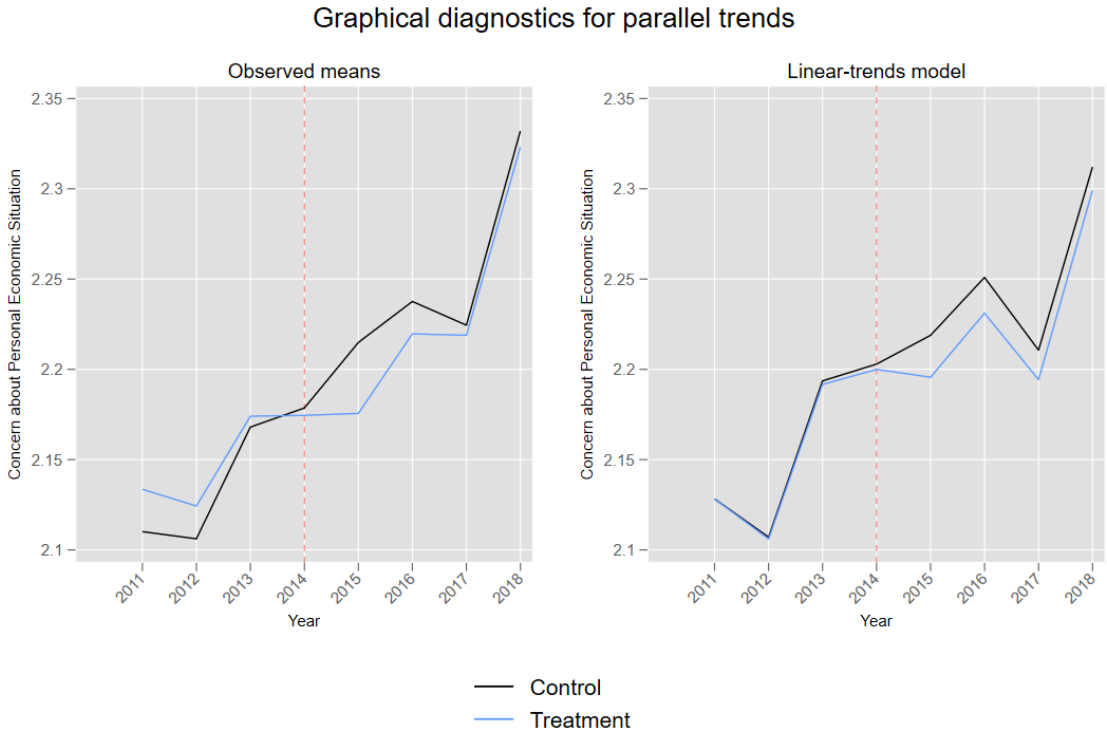


Figure B10: Parallel Trends for Concern about Immigrants

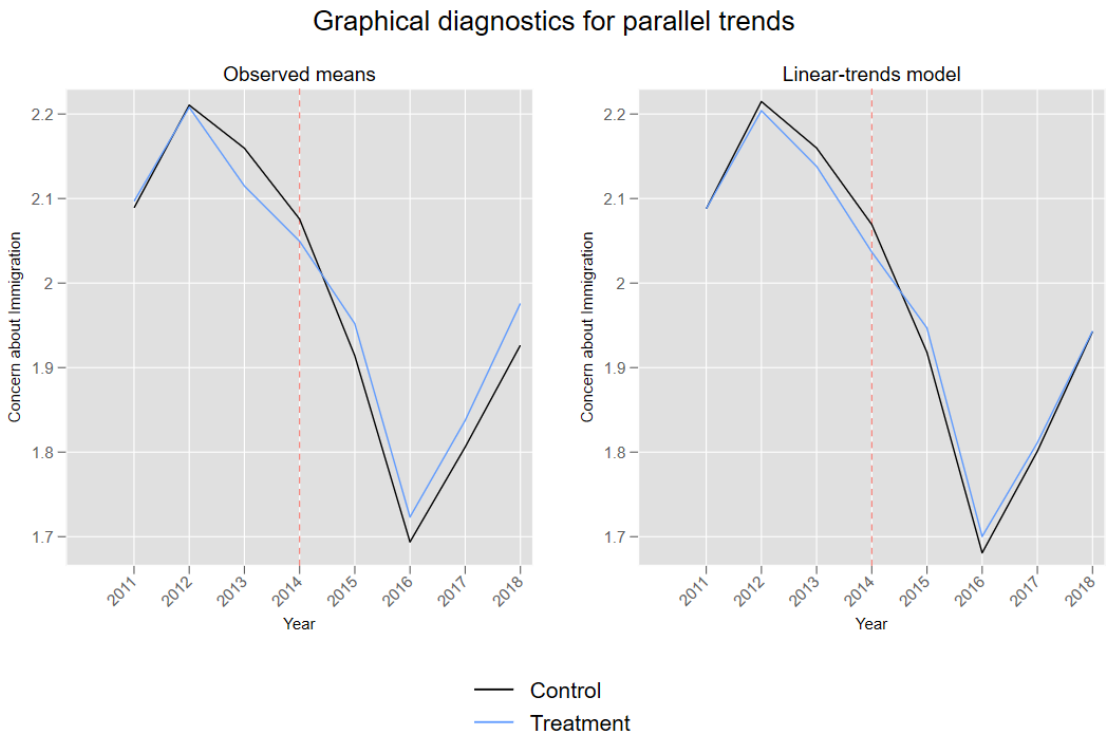
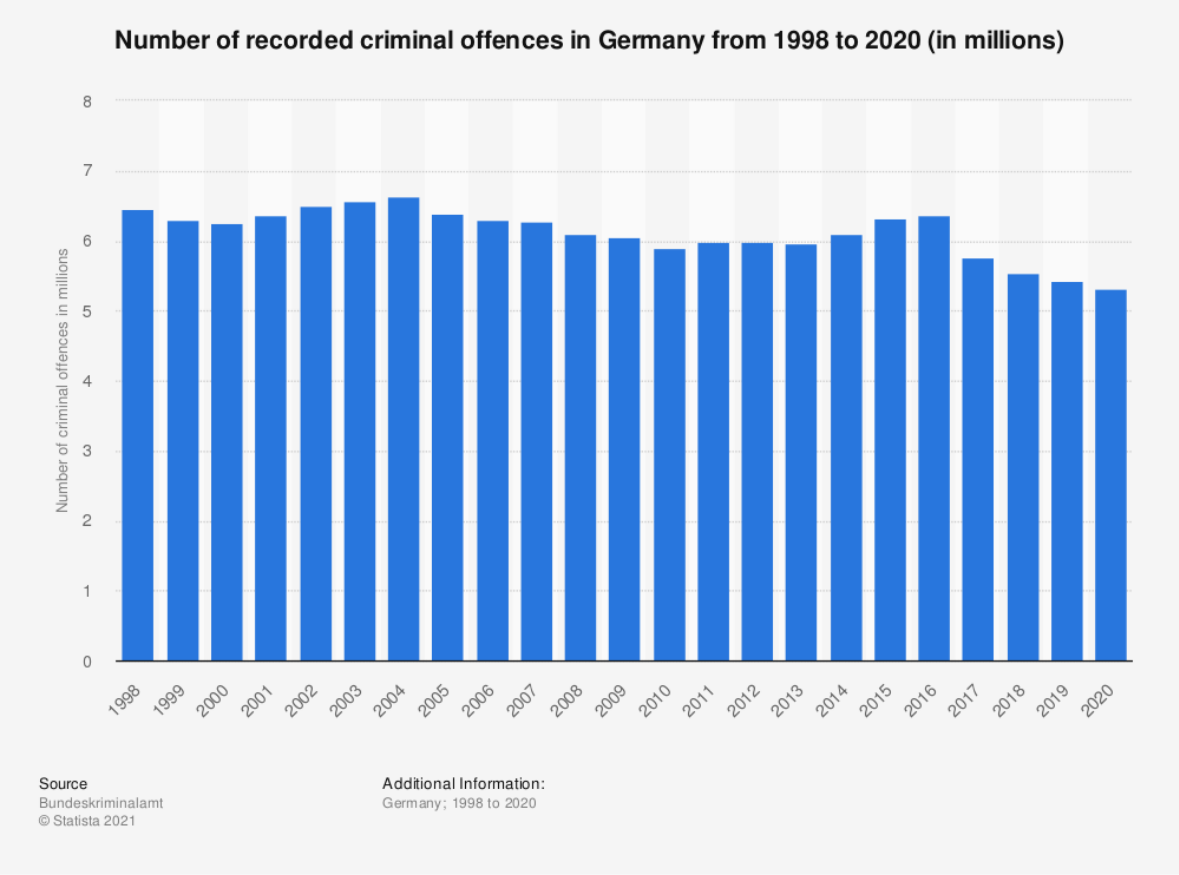


Figure B11: Number of recorded Criminal Offences in Germany from 1998 to 2020



Source: Bundeskriminalamt, 2021.

Figure B12: Effect of Health on Perceived Institutional Erosion (overall Index)

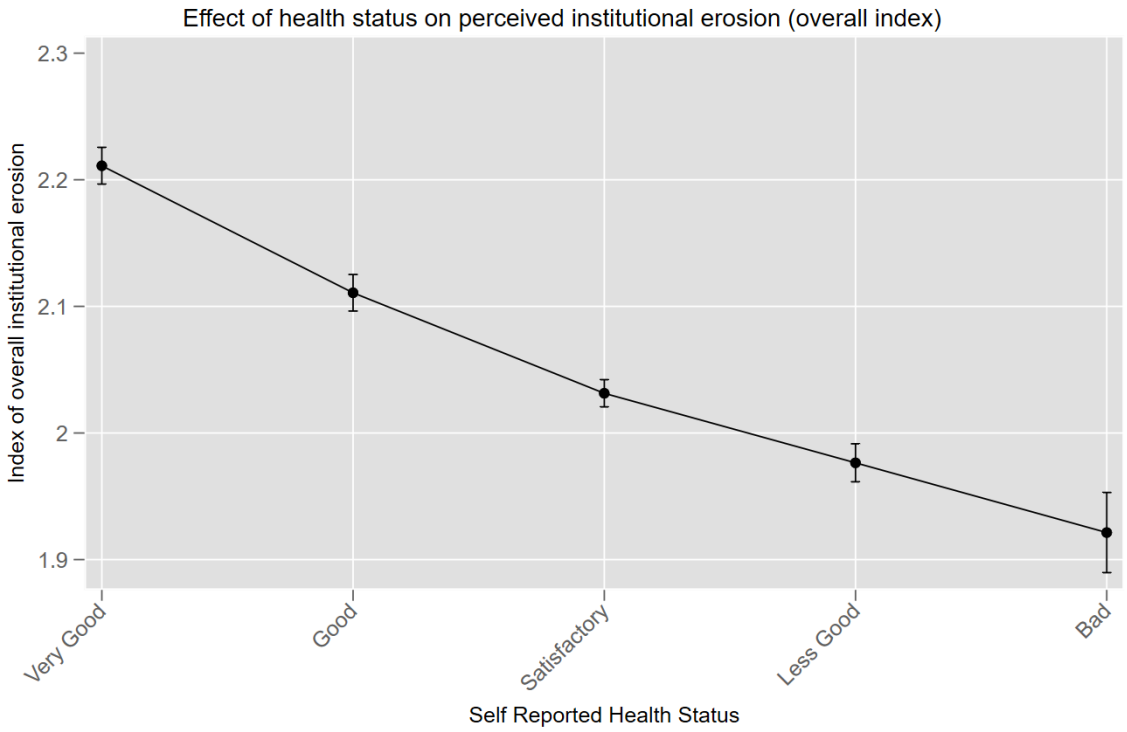


Figure B13: Effect of Age on Perceived Institutional Erosion (overall Index)

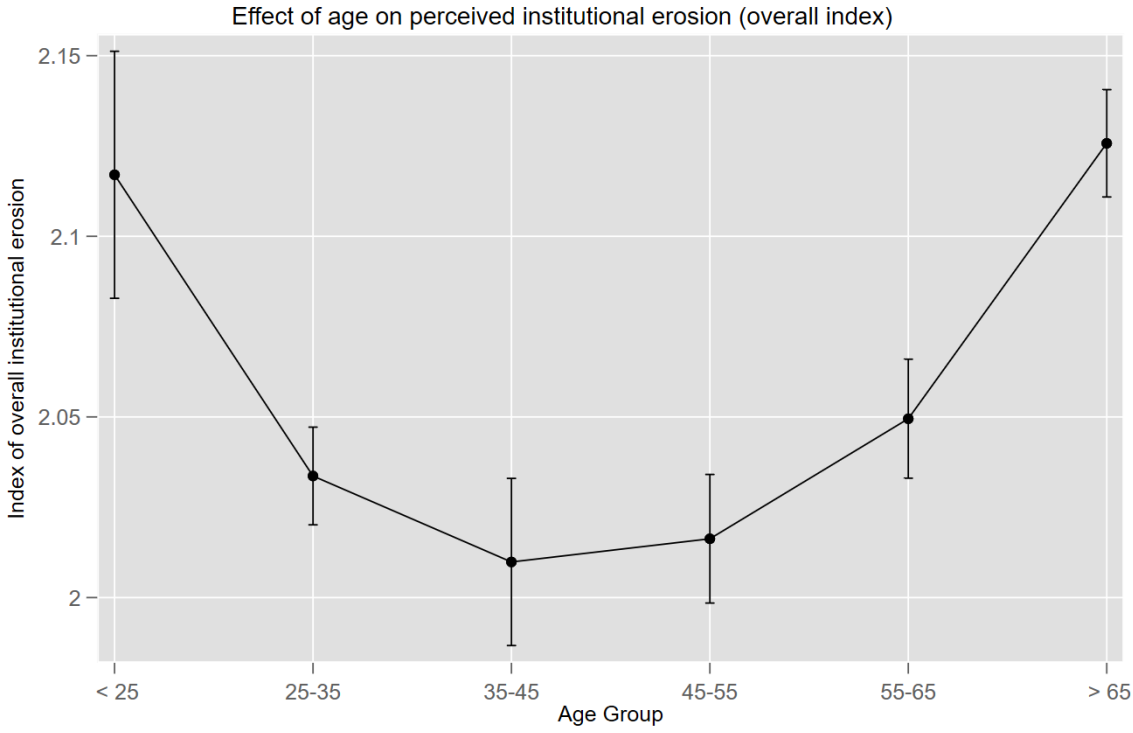


Figure B14: Interaction Effect of Age and Education on Perceived Institutional Erosion

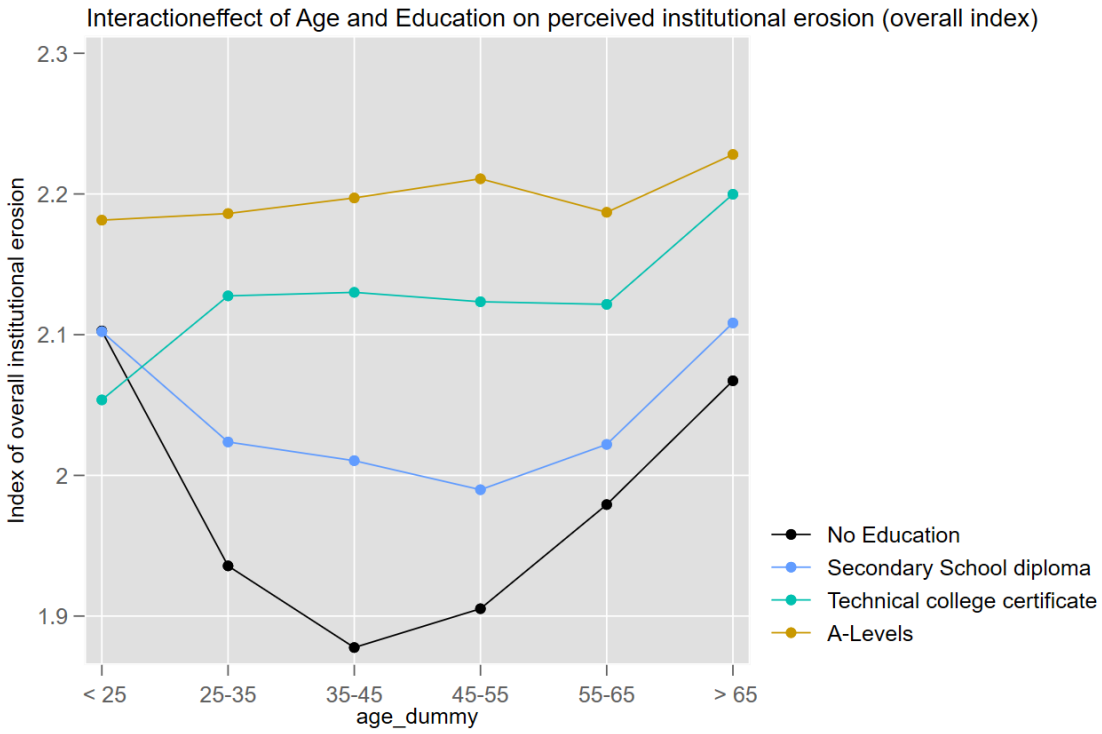


Figure B15: Effect of Age on Perceived Economic Institutional Erosion

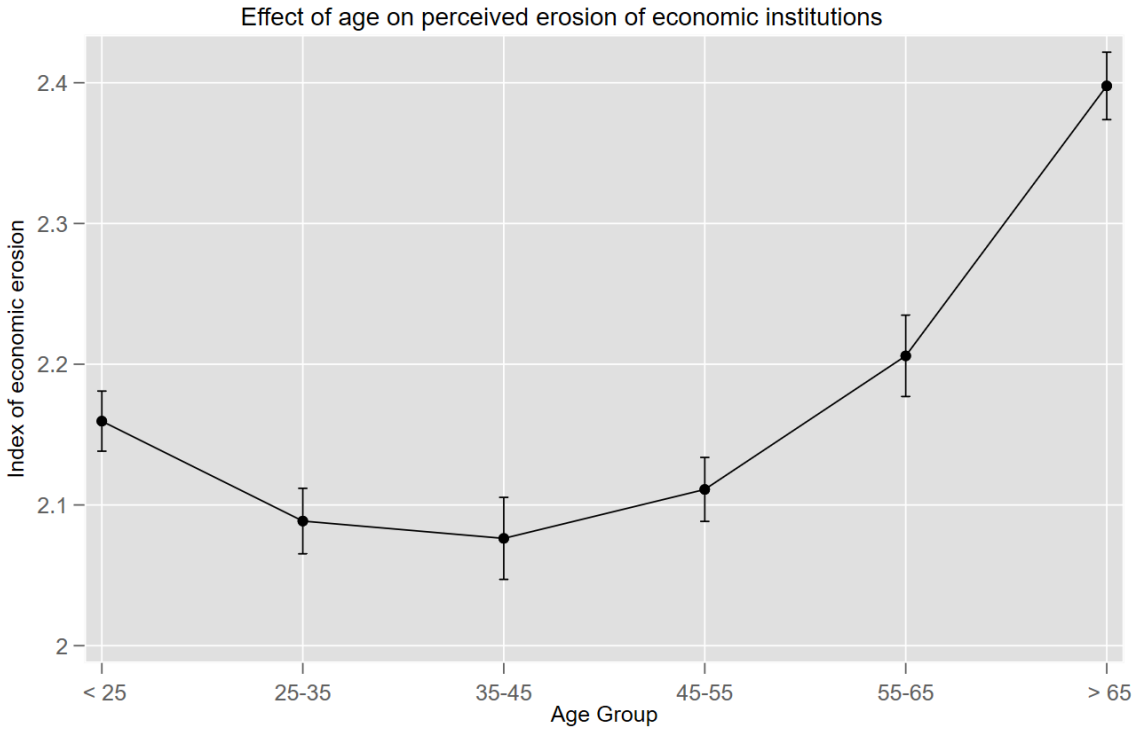


Figure B16: Effect of Age on Perceived Cultural Institutional Erosion

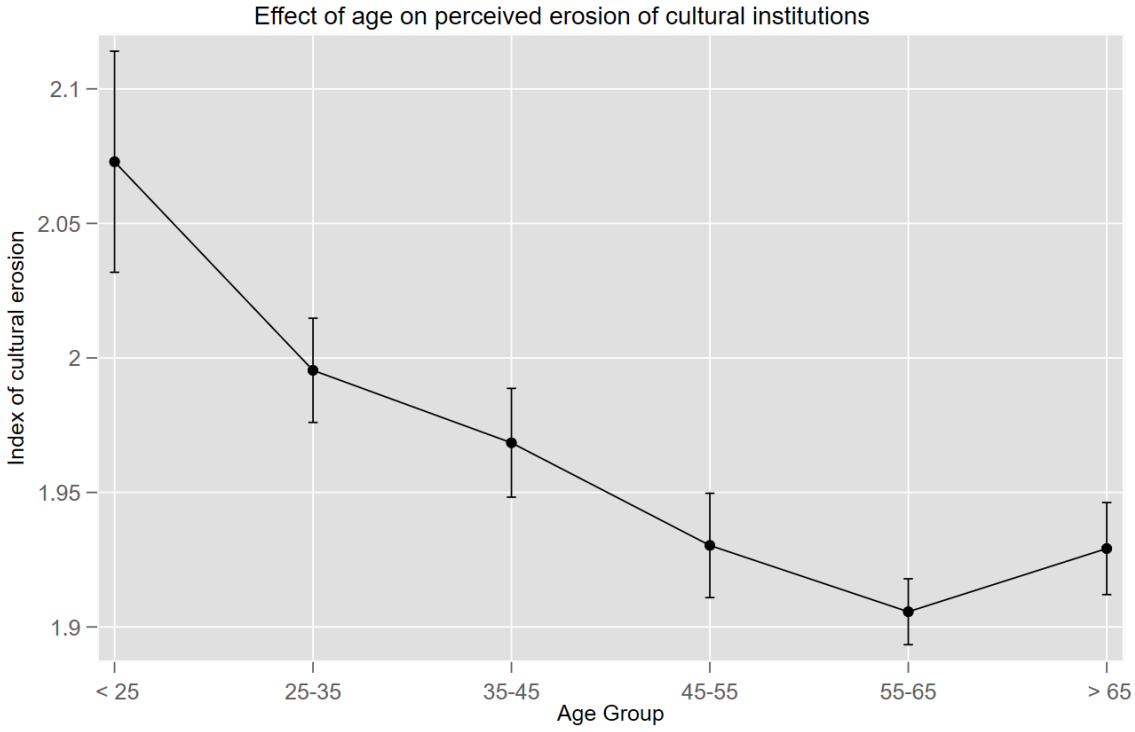


Figure B17: Effect of Education on Perceived Institutional Erosion (overall Index)

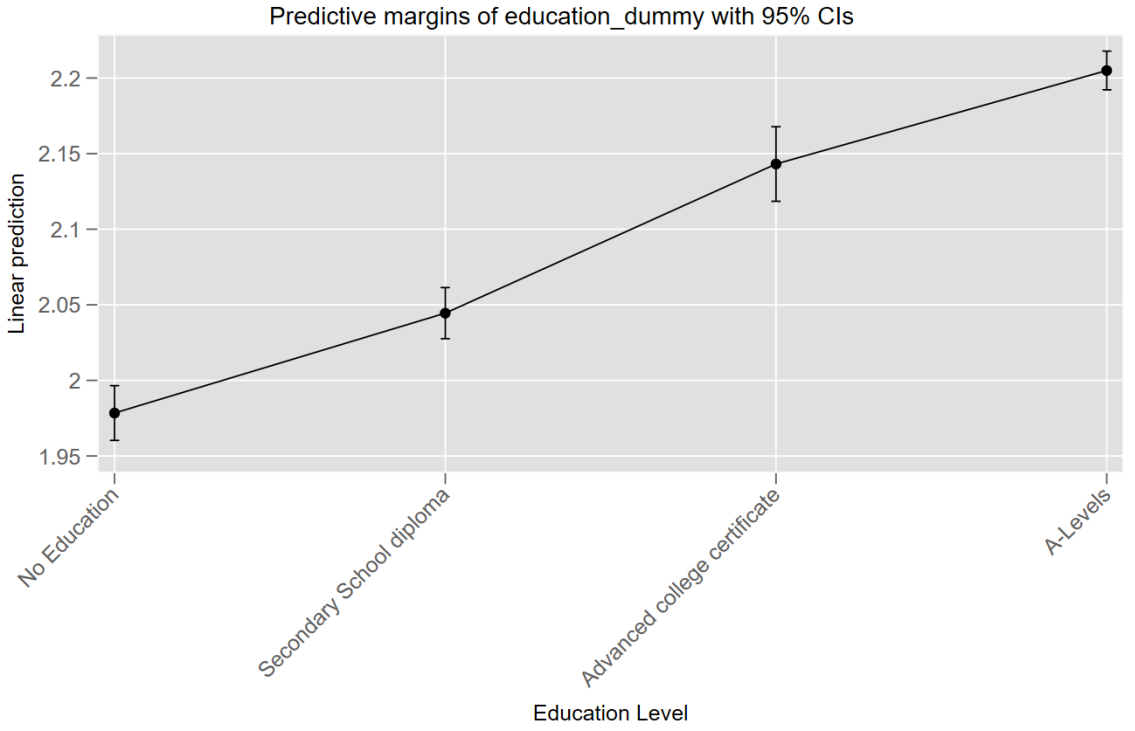


Figure B18: Effect of Sex on Perceived Institutional Erosion (overall Index)

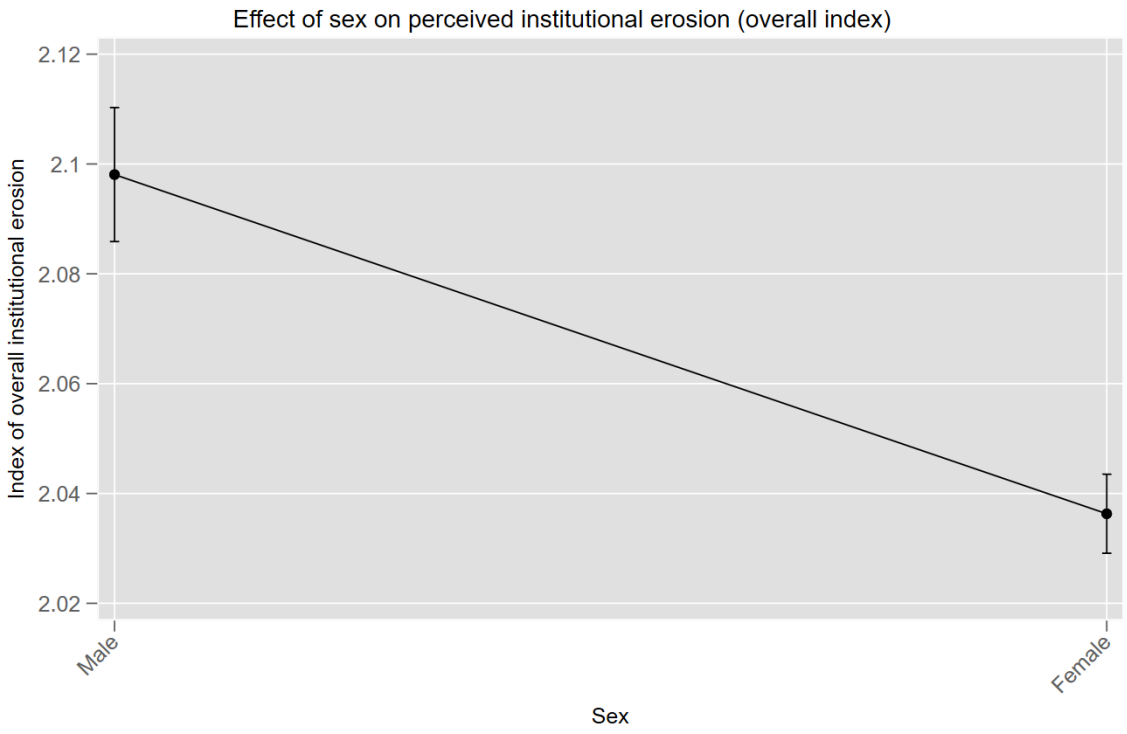


Figure B19: ATE in the Normal Model versus ATE in the Endogenous Treatment Model

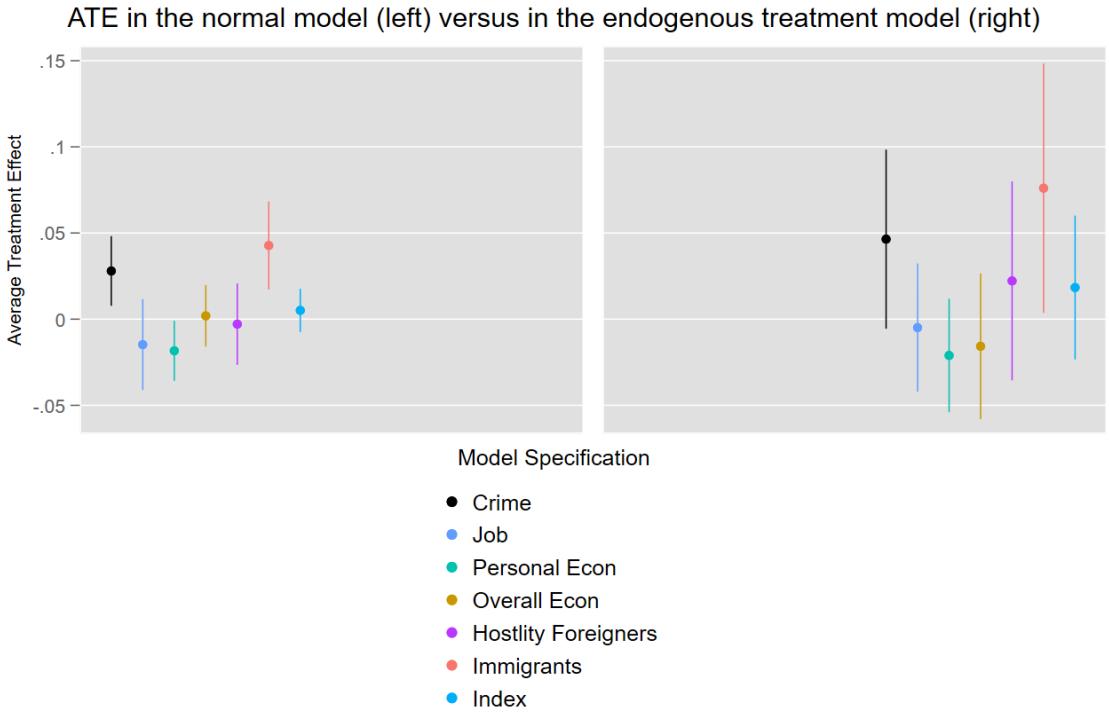


Figure B20: ATE in the Normal Model versus ATE in the Ordered Probit Model

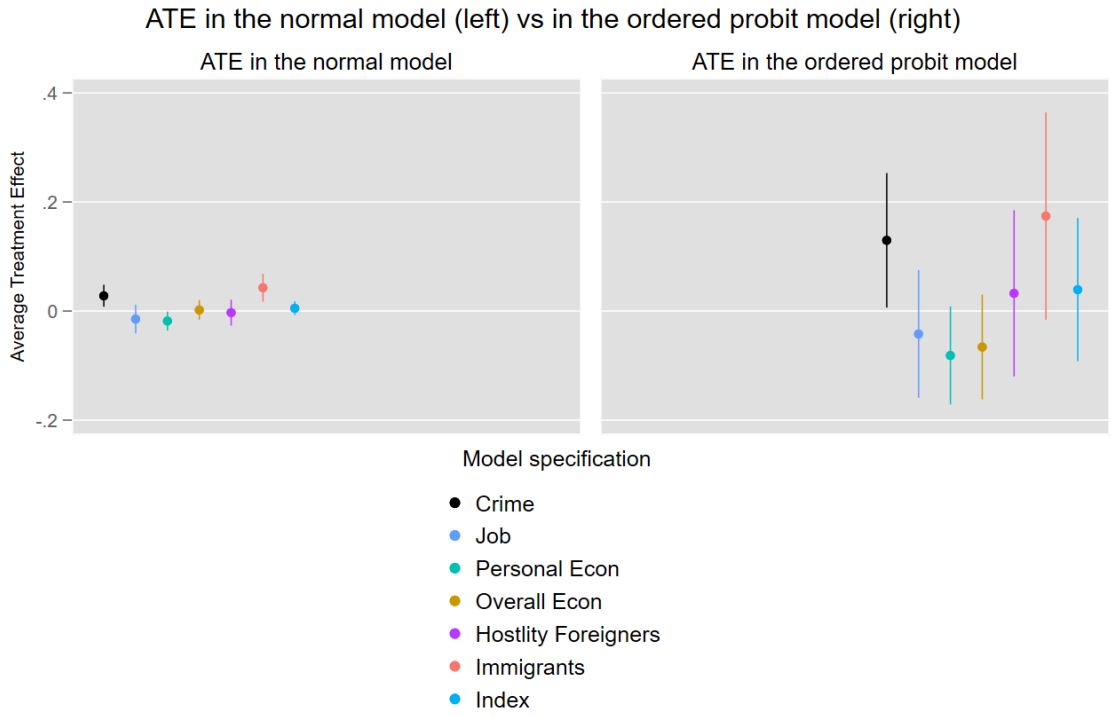


Figure B21: ATE using Different Lags of Control Variables