Four Essays in Applied Microeconomics - Empirical Analyses of Public Policies & Voting Behavior

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von:	Melinda Janina Fremerey, M.Sc. geboren am 27.09.1991 in Hachenburg
Erstgutachter:	Prof. Dr. Justus Haucap
Zweitgutachter:	Jun-Prof. Dr. Andreas Lichter
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Co-authors and Chapters

Part I: Analyses of Public Policies

Chapter 1: The Effects of Private Damage Claims on Cartel Activity: Experimental Evidence

- Co-authors:
  - Olivia Bodnar (DICE, Heinrich-Heine University Düsseldorf)
  - Hans-Theo Normann (DICE, Heinrich-Heine University Düsseldorf)
  - Jannika Schad (University of Siegen)
- Published in "The Journal of Law, Economics, and Organization", 2021.

Chapter 2: Fiscal and Economic Effects of Local Austerity

- Co-authors:
  - Andreas Lichter (DICE, Heinrich-Heine University Düsseldorf)
  - Max Löffler (Maastricht University)

Part II: Analyses of Voting Behavior

Chapter 3: The Role of Globalization Concerns and the European Unemployment Crisis for the Rise of Anti-Establishment Parties

- Single-author

Chapter 4: Becoming Neighbors with Refugees and Voting for the Far-Right? The Impact of Refugee Inflow at the Small-Scale Level

- Co-authors:
  - Lukas Hörnig (Leibniz Institute for Economic Research, Essen)
  - Sandra Schaffner (Leibniz Institute for Economic Research, Essen)

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Introduction

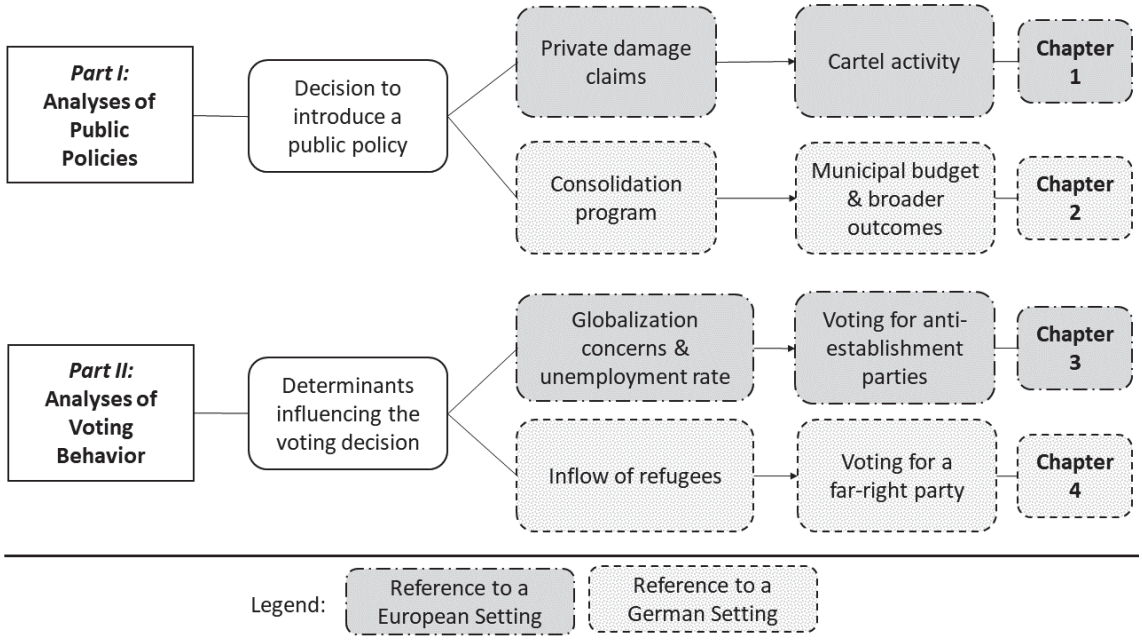
"Man hat behauptet, (...) die Welt werde durch Zahlen regiert, (...) das aber weiß ich, dass die Zahlen uns belehren, ob sie gut oder schlecht regiert werde." [It has been said that figures rule the world but I am sure that figures show us whether it is being ruled well or badly.]

– Johann Wolfgang von Goethe, 1830

Introduction

No economic policy and only few economic institutions are sufficient to remain unchanged forever (European Commission, 2012). Therefore, governments introduce and adjust public policies, inter alia, to shape and improve economic and social conditions. Political decision-makers are not only constantly facing policy decisions but they are also judged by their electorates according to the results they achieve (European Union, 2017). Citizens in democratic systems formally have to or are allowed to decide who should represent them in the political process. This decision lawfully taken at the election is irrevocable and can only be changed in the next election (Wojtasik et al., 2013). Since the decisions of those two actors—governments and voters—often have far-reaching, and sometimes adverse economic, social, and/or political consequences (Bénassy-Quéré et al., 2010; Norris and Inglehart, 2019; Funke et al., 2020), it is of utmost importance to analyze and evaluate the outcomes and drivers of those decisions. As the quote from Johann Wolfgang von Goethe at the beginning of this introduction underlines, it is often assumed that figures and statistics rule the world. However, figures should rather serve as basis to evaluate whether the world is well or badly governed. This dissertation sheds light on what figures and data reveal about specific public policies and voting behavior and their implications.

Figure 1: Overview of the two parts and the respective chapters of this dissertation



Notes: Own illustration

As Figure 1 illustrates, the dissertation is divided into two distinct parts. *Part I* of this thesis empirically examines economic consequences of the introduction of specific public policies by examining the effect of private damage claims on cartel activity, and the impact of a fiscal consolidation pogram on municipal budget and broader economic outcomes, in Chapter 1 and 2, respectively. *Part II* of this dissertation empirically studies different determinants which influence the voting decision. The impact of globalization concerns and the unemployment rate on election outcomes of anti-establishment parties which include far-left, far-right, populist, and Euroskeptic parties is analyzed in Chapter 3, while Chapter 4 examines the effect of the inflow of refugees on voting for a far-right party. In each part of this dissertation, the first chapter refers to a European perspective and the second chapter deals with a German setting. Hence, the first and third chapter of this dissertation analyze the consequences of the introduction of a directive at the European level and the voting behavior in national parliamentary elections in Europe, respectively. The fiscal consolidation program analyzed in Chapter 2 was implemented in the German federal state North Rhine-Westphalia, while Chapter 4 refers to a German setting by analyzing the German federal state election in 2017. The following subsections

provide detailed information on the two parts and the respective chapters of the dissertation.

Part I – Analyses of Public Policies

One of the central tasks of economics is the evaluation of the impact of public policies (Heckman and Vytlacil, 2005). Governments heavily rely on policy evaluations. Almost three-quarters of OECD countries have a legal framework for policy evaluations; with some countries (including Germany) anchoring policy evaluation as a constitutional duty (OECD, 2020). The evaluation of public policies is important because of three main reasons. First, since politics can be understood as the problem-solving actions of state authorities (Rürup et al., 2005), it is of great importance to judge the policy not by its intention but by its results (Friedman, 1976). Policy interventions can exert beneficial but also adverse effects on other dimensions (Bénassy-Quéré et al., 2010). Therefore, policy measures should be consistently and critically analyzed, not only in terms of whether the aim has been achieved, but also with respect to broader and potentially adverse economic consequences. Second, the evaluation of the effectiveness of policy measures is crucial due to limited scope for policy interventions and public budgets (Schmidt, 2007). Third, the evaluation of policies enables economists and policy makers to draw conclusions for future or improved policies (Schmidt, 2007). Economic advices given to policy makers and the assessment of the effect of different possible interventions highly rely on analyses of the impact of already implemented public policies (Bénassy-Quéré et al., 2010). The first part of this dissertation addresses these aspects by evaluating two substantial policy interventions in the fields of competition economics and public economics. The following paragraphs describe and motivate the respective analyses of the public policies in more detail.

Chapter 1 The first policy that the dissertation evaluates is the introduction of private damage claims on the level of the European Union. In 2014, the European Union introduced a directive which significantly strengthened private enforcement (European Commission, 2014). The member states had to implement the instrument of private damage claims based on the European directive into national law which was completed by 2018. Since 1996, European anti-trust legislation was mainly

based on public enforcement and therefore, on the successful tool of leniency programs to uncover cartels (European Commission, 1996). Private enforcement is controversially discussed in the theoretical literature (e.g., Canenbley and Steinvorth, 2011; Knight and de Weert, 2015; Wils, 2009), as private damage claims against cartels may exert negative effects on leniency applications and may lead to a trade-off between public and private enforcement. On the one hand, private damage claims in terms of higher fines should increase deterrence. On the other hand, leniency applicants receive full immunity from public cartel fines but they only have no or restricted protection against private third-party damage claims. This may reduce incentives to apply for leniency and stabilize cartels. To investigate this potential issue, Olivia Bodnar, Hans-Theo Normann, Jannika Schad, and I conduct a laboratory experiment. The subjects in our experiment choose whether to join a cartel and, if they decide in favor of a collusive agreement they may apply for leniency. We contribute to the literature and the discussion on private enforcement by analyzing the possible enforcement of private damage claims when a cartel was detected by the anti-trust authority or revealed by a leniency applicant. Our results suggest that the implementation of private damage claims reduces cartel formation. However, private damage claims make cartels, that form nonetheless, more stable. To investigate a potential policy improvement, we test a novel setting where the first leniency applicant is also protected from private damage claims. In this setting, we find that the negative effect of private damage claims on leniency applications is mitigated.

Chapter 2 The second chapter of this dissertation deals with the evaluation of a large-scale fiscal consolidation program for German municipalities. In general, the debt to GDP ratio in advanced economies (excluding the US) increased by about 60 percentage points from 1960 to 2010 (Yared, 2019). While short-term debt may be an appropriate measure to respond to an economic shock or to smooth expenditures over time, high levels of long-term debt can exert negative effects on economic activity, for instance, by crowding out private capital investments (e.g., see Gamber and Seliski, 2019). Although, the number of governments which implement policies to limit debt is increasing (Lledó et al., 2017), there is an ongoing debate, inter alia, about the effectiveness and the economic and political consequences of those policies (e.g., see Wyplosz, 2013; Braun and Tommasi, 2004). Chapter 2, a joint work with Andreas Lichter and Max Löffler, studies the fiscal and broader economic

consequences of a consolidation policy which targeted heavily financially distressed municipalities in the most populous German federal state, North Rhine-Westphalia, in 2011. In the framework of this policy, municipalities received financial support and had to consolidate their budgets net of transfers within one decade. We use the political discretion in designing the participation criteria of the consolidation program to apply a dynamic difference-in-differences design. We compare municipalities subject to the consolidation program to neighboring municipalities which were equally financially distressed but were not part of the fiscal consolidation program. Our results show that the consolidation program was effective, i.e., municipalities in the program substantially improved their fiscal balance not only based on the transfer payments but also based on own consolidation efforts. While all municipalities participating in the consolidation program were obliged to achieve a balanced budget, there was no regulation for the consolidation strategy. We find that small municipalities subject to the consolidation program increased taxes on property and cut expenditures for local public services. In contrast, large municipalities subject to the treatment have not cut spending on local amenities for residents and consolidated their budget largely by shifting the burden onto firms by increasing their business tax rates and reducing the expenses for business subsidies. While the consolidation effort of large treated municipalities does not considerably affect local economic outcomes in terms of the number of firms or the business tax base, we find decreasing population statistics and house prices in smaller treated municipalities.

Conclusion on *Part I* Both chapters in *Part I* of the dissertation present important policy evaluations and give crucial policy recommendations. The first chapter provides empirical evidence for a possible improvement of the current legislation regarding private enforcement in the European Union, i.e., a better protection of the first leniency applicant from private damage claims in order to mitigate the negative effect of private damage claims on leniency applications. The results of Chapter 2 emphasize the need for analyzing heterogenous responses to a policy and broader economic consequences of a policy. In general, the main objective of policy evaluations is to disentangle the impact of the policy intervention from all other possible coinciding aspects and developments (Schmidt, 2007). For a reliable evaluation of a policy measure and a sound prediction of future developments, establishing causality is the key aspect (Schmidt, 2007). Besides drawing causal inference from randomized

controlled experiments, economists largely rely on observational data and identify causal effects by using a wide variety of strategies (Athey and Imbens, 2017). Chapter 1 and 2 present two different strategies for analyzing causal impacts of public policies. In Chapter 1, causal evidence is based on a laboratory experiment. Due to the drawback of observational data on cartels, i.e., the real number of cartels and changes in cartel formation are unobservable, an empirical assessment of the evaluation of anti-trust policies is very difficult (Bigoni et al., 2012). By conducting a laboratory experiment, we are able to study the introduction of private enforcement in a setting without the sample-selection bias which occurs when considering data from the field. The analysis in the second chapter with respect to the consequences of the consolidation program presents a causal evaluation based on a difference-in-differences approach. Thereby, this dissertation covers two highly relevant economic policy fields and uses different well-established methods to provide causal evidence on the impact of these specific public policies.

Part II – Analyses of Voting Behavior

In the last decades, anti-establishment parties which include far-left, far-right, populist, and Euroskeptic parties, experienced increasing electoral success in particular in European countries. For instance, populist parties increased their vote shares by about 20 percentage points from 1992 to 2019 on average in European countries (Rooduijn et al., 2019). Since populist authoritarian parties pose a threat to the liberal democracy (Norris and Inglehart, 2019), and populist leadership can have significant economic costs (Funke et al., 2020), understanding socio-economic causes for the rise of anti-establishment parties is crucial to prevent the democratic system and the economy from being damaged by the increasing electoral success and influence of anti-establishment parties. So far, political and economic studies largely find negative attitudes towards immigration (see e.g., Dustmann et al., 2019; Halla et al., 2017; Lubbers and Scheepers, 2000), decreased trust in political institutions (see e.g., Algan et al., 2017; Dotti Sani and Magistro, 2016; Dustmann et al., 2017) and/or labor market effects (see e.g., Algan et al., 2017; Dippel et al., 2016) as main causes for the rise of anti-establishment and in particular of populist and far-right parties. However, it is important to test and understand possible mechanisms behind these effects on the election of anti-establishment parties in order to curb

their rise. Therefore, Chapter 3 evaluates a new mechanism behind the effect of the unemployment rate on the success of different anti-establishment parties, that is, the attitude towards globalization. The analysis of the voting behavior on a highly granular level in Chapter 4 explores the effect of natives' direct exposure to refugees on anti-immigrant sentiments measured by the support for a far-right party. The following paragraphs provide detailed information on the respective chapters in the second part of this thesis.

Chapter 3 The last decades are not only characterized by an increase in anti-establishment parties' vote shares in European countries but also by anti-globalization tendencies and the European unemployment crisis. Anti-globalization tendencies are exemplified by protests against free trade agreements, the British referendum to leave the European Union or protectionist trade policies (Meunier and Czesana, 2019; Dür et al., 2020). The European unemployment crisis is characterized by a large increase in the European unemployment rate, which peaked in 2013 with an increase of six percentage points above the European average in 2008 (Eurostat, 2021). This single-authored study investigates the relationship of those three developments. I examine whether the unemployment rate, in addition to its influence on the voting behavior, also affects globalization concerns and whether attitudes towards globalization, in turn, influence the vote shares for anti-establishment parties in national parliamentary elections in Europe. To investigate the role of globalization concerns as a potential mechanism behind the electoral consequence of the European unemployment crisis, I apply two instrumental variable designs and a mediation analysis. As the performance of the construction sector is a reliable indicator to capture the impact of the European economic recession (Thakurta, 1970; Algan et al., 2017), I estimate the effects of the unemployment rate on anti-establishment parties' election outcomes and on globalization concerns by using the instrument of the share of value added of the construction sector. Further, I study the impact of globalization concerns on the voting behavior in favor of anti-establishment parties. Based on the assumption that the expansion of broadband internet promotes specific media consumption that leads to a radicalization of people's ideas and attitudes (Hitt and Tambe, 2007; Brey et al., 2019), the regional shares of households having broadband access serves as an instrument for anti-globalization attitudes. The results show that the unemployment rate has a significant positive impact on the election of far-left and populist parties, and also on globalization concerns. More negative

attitudes towards globalization lead to higher vote shares for populist and far-left parties, while vote shares for the far-right decrease. The result of the mediation analysis suggests that the channel of globalization concerns seems to explain about eight percent of the effect of the unemployment rate on voting for far-left parties.

Chapter 4 The (West) German parliamentary history following the Second World War was marked until recently by an extraordinary feature compared to other European national parliaments. This feature was the absence of a far-right party in the national parliament. However, in the German federal election in 2017, a far-right party entered the national parliament as the third biggest parliamentary group. This party, the Alternative for Germany (AfD), rapidly gained support after its foundation in 2013 and became openly xenophobic, especially after the increased refugee inflow into Germany in 2015 (Schmitt-Beck, 2017; Heckmann, 2016). Lukas Hörnig, Sandra Schaffner, and I investigate the effect of this refugee inflow between 2014 and 2017 on the election outcome of the AfD in the national parliamentary election in 2017 in Germany. We build and exploit a unique set of German small-scale data which allows us to measure the direct exposure of natives to refugees in a 1km x 1km grid cell. Thereby, we consistently analyze the validity of the contact theory (Allport et al., 1954)—i.e., whether the contact between natives and refugees reduces stereotypes and thus, anti-immigration sentiments—in a German setting. Furthermore, we are able to distinguish between the effect of refugee inflows into the neighborhood (1km x 1km) and on the county (NUTS 3) level. In the first place, the allocation of refugees was exogenous and refugees had certain moving restrictions during their asylum process. Based on the assumption that refugees settle into regions where people with the same migration background live, we alleviate remaining concerns regarding endogeneity by a shift-share instrument. We instrument the refugee inflows between 2014 and 2017 by the past settlement of refugees in 2005. We find that a higher refugee inflow into West German urban neighborhoods leads to lower shares of far-right votes, while the refugee inflow into the county positively affects AfD support. In contrast, in East German very urban regions, the inflow at the county level reduces far-right vote shares. In West and East German rural areas, the refugee inflow is not statistically significantly related to far-right voting. The findings suggest that the contact theory is valid for West German urban areas. Other mechanisms on the more aggregated county level seem to drive the positive

effect in West German urban areas and the negative effect in East German very urban regions. Factors like an increased media coverage of the refugee inflow, the (absence of) feared losses, the contact in areas besides the own neighborhood or local integration policies could constitute those mechanisms.

Conclusion on *Part II* The analyses in *Part II* differ in terms of the types and numbers of parties, and the level of data analyzed. Chapter 3 provides a broader picture of the voting behavior by analyzing and distinguishing between the election outcome of different types of anti-establishment parties (far-right, far-left, populist and Euroskeptic parties). Further, the analysis is based on data at the aggregated European regional (NUTS 1) level. In contrast, the analysis in Chapter 4 focuses on the election outcome of a German far-right party and examines the election results at a highly granular level. The findings in Chapter 4 show that studies relying on aggregated data might overlook considerable variation. To address this issue, Chapter 3 provides an additional analysis of political preferences based on data at the individual level which confirms the main results. Hence, the second part of the dissertation does not only study diverse mechanisms and factors which affect the vote shares of specific anti-establishment parties but it also empirically investigates different regions and spatial scales. Both chapters contribute to the political and academic debate on what causes and what does not drive different anti-establishment parties' success.

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Part I: Analyses of Public Policies

1

The Effects of Private Damage Claims on Cartel Activity: Experimental Evidence

Joint work with Olivia Bodnar, Hans-Theo Normann, Jannika Schad

1.1 Introduction

In the airline-cargo cartel case, Lufthansa was the whistleblower and received full immunity from fines, but they were soon after sued privately by Deutsche Bahn for damages amounting to 1.76 billion euros.[1] Would Lufthansa have blown the whistle had they anticipated these damage claims? Do such private damages not provide a strong disincentive to report cartels and apply for leniency? In this paper, we try to answer these questions with evidence from laboratory experiments.

Largely driven by the introduction of leniency programs, cartel authorities can look back at successful years of public cartel enforcement.[2] Leniency policy offers companies involved in a cartel who self-report either total immunity from fines or a reduction in the fines which the authorities would have otherwise imposed on them (European Commission, 2006). As theoretical, empirical, and experimental work shows, leniency policy has a deterrent effect on cartel formation and, as it yields distrust among cartel members, it destabilizes the operations of existing cartels (see, for example, Bigoni et al., 2012; Brenner, 2009; Harrington and Chang, 2009; Miller, 2009; Motta and Polo, 2003; Spagnolo, 2003). For a survey of the research on leniency programs, see Spagnolo (2008).

Damage claims — customers of a cartel may sue convicted wrongdoers for the losses they suffered in civil lawsuits — add an element of private enforcement to anti-cartel policy. Private damage claims have only recently gained attention in Europe. The European Commission started to consider private enforcement with its 2005 Green Paper (European Commission, 2005). It was signed into law in November 2014. In 2018 the last member states implemented the directive on antitrust damages actions into national law (European Commission, 2014, 2018). In the US, private damage claims have existed since the early 20$^{\text{th}}$ century. Here, private enforcement is viewed as an important and long-standing antitrust policy tool since public enforcement is restricted to litigation in order to impose fines on cartel members (Canenbley and Steinvorth, 2011).[3] Despite these differences in the duration of

[1] See Kiani-Kress and Schlesiger (2014) and Michaels (2014). At least initially, private damages far exceeded the total fines of all cartel members which, eventually, summed up to 776 million euros (see European Commission (2017a)).

[2] For example, MAN revealed the EU-wide truck cartel (1997–2011) and received full immunity from the European Commission (EC). Further examples are the vitamins cartel (around 1985–1999) and the air cargo cartel (1999–2006), in which the EC and the US Department of Justice granted full immunity to Rhône-Poulenc, respectively Lufthansa, for revealing the cartel (Department of Justice, 2007, European Commission, 2001, par. (124), 2016, par. (31), 2017b, par. (28)).

[3] Private damage claims account for 90 to 95% of US cartel cases (Knight and Ste. Claire

application, private damages now constitute an important dimension of cartel policy in both the EU and the US.

At first sight, it seems that private damage claims nicely complement public enforcement. They raise the expected penalty for forming a cartel and therefore add to the deterrent effect of the fines imposed by antitrust authorities. Becker (1968) argues that increased sanctions decrease criminal activity.[4, 5] Private damage suits constitute an additional sanction and should accordingly reduce the criminal activity of explicit collusion.

There are, however, growing concerns about the negative effects of private enforcement. As the Lufthansa example shows, the detrimental impact that compensation payments for damaged parties have on the attractiveness of leniency programs are evident. Whereas penalties are waived or reduced for cooperating leniency applicants, the European Damages directive gives only limited protection against third-party damage claims (European Commission, 2014).[6] The effect is aggravated by the fact that cartel members are jointly liable for the entire damage caused by the cartel, and compensation payments are not capped, in contrast to fines which may not exceed 10% of annual turnover (European Commission, 2011). With respect to private damage claims, the European legislation restricts the liability of leniency applicants to the harm caused to their own direct and indirect purchasers. In any event, applicants remain fully liable when non-applicants are not able to entirely compensate the injured parties (European Commission, 2014, par. (38)). In comparison, the US antitrust law limits the liability of leniency applicants to single, instead of treble, damage compensation payments (Antitrust Criminal Penalty Enhancement and Reform Act of 2004, Sec. 213).

(2019)). US law incentivizes private lawsuits, for example, by making the infringer liable for treble damages and by admitting class action suits (Clayton Act, section 4, 15 U.S.C. § 15; Jones, 2016).

[4] More recently, Bigoni et al. (2015) and Chowdhury and Wandschneider (2018) provide experimental evidence of the deterrent effect of penalties on cartels. See also below.

[5] An additional point in favor of private damages, raised by Knight and Ste. Claire (2019), is that private damages can reduce the profitability of sustained collusion. Cartels are no longer monitored by time- and money-constrained competition authorities only, but also by possible private plaintiffs. A higher detection probability reduces the profitability of a cartel, accordingly. This argument is also supported in the work by Lande and Davis (2011).

[6] We will henceforth take a European perspective of this issue in that an existing leniency program was possibly weakened by the introduction of private damages. In the US, private damages predate leniency programs and so the existing anti-cartel policy was strengthened by the introduction of leniency. Nevertheless, the trade-off due to private damages also applies to US antitrust policy. This trade-off, however, might be weakened due to the US antitrust law's limitation of the leniency applicant's liability to single, instead of treble, damage compensation payments.

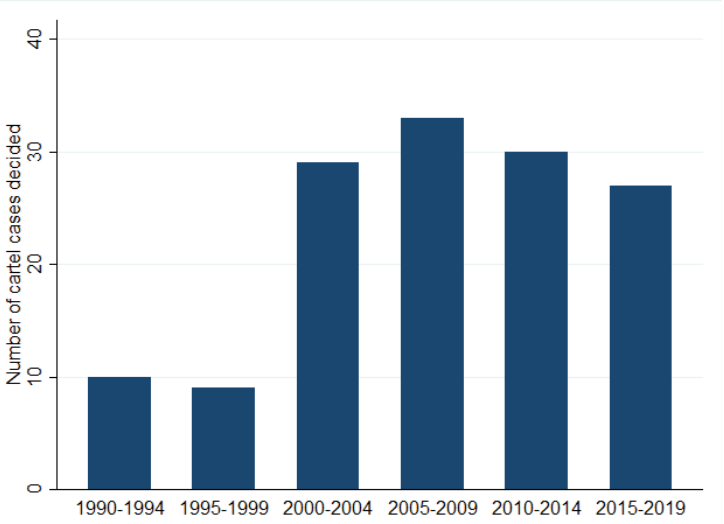
The literature appears to largely acknowledge this artificially created trade-off between private damage claims and public leniency programs. Canenbley and Steinvorth (2011), Cauffman and Philipsen (2014), Hüschelrath and Weigand (2010), Knight and de Weert (2015), Migani (2014), Wils (2003), Wils (2009) argue informally, and Kirst and van den Bergh (2016) formally, that it is less desirable for firms to apply for leniency when they are liable for private damage claims. The higher the expected third-party claims, the lower the incentives to apply for leniency. As this is also anticipated by other cartel members, it could have a stabilizing effect on cartels. This raises the question of whether applying for leniency remains attractive after the introduction of private damage claims. In the thus far most elaborate theoretical treatment of private damages in cartel cases, Buccirossi et al. (2020) argue that the conflict between private damage claims and public leniency programs is only apparent, and that limiting the cartel victims' rights to claim their losses is not necessary. They demonstrate that damage actions will even improve the effectiveness of leniency programs provided the civil liability of the whistleblower is minimized. We return to this important point below.

In the end, whether private damage claims strengthen or weaken the deterrence effects of public enforcement is an empirical question. On the one hand, higher fines should increase deterrence. On the other hand, they may render leniency ineffective. Somewhat surprisingly, we have not been able to find any sound empirical assessment of the effects of private enforcement. Figure 1.1 shows the number of EU cartel cases since 1990. Cartel cases rose sharply in 2000–2004 with the introduction of leniency programs but they are now in decline. This recent drop in cartel cases coincides with the EU's introduction of private damage claims in 2014. Could this decline have been triggered by private damages? The descriptive numbers in Figure 1.1 cannot identify a causal effect of private damages as many factors are uncontrolled for; foremost, because there are no undetected cartels in the sample.

We propose an experimental approach to study the effects of private damages empirically. Laboratory experiments present a readily available testbed which is unaffected by the sample-selection problems, which may bias field-data studies. Bigoni et al. (2012) mention that it is difficult to evaluate the deterrent or stabilizing effects of antitrust policies compared to other law enforcements because the number of cartels and changes in cartel formation is unobservable.[7] Experiments can be a

[7] See Miller (2009) and Harrington and Chang (2009) for empirical identifications of policy effects on the number of detected cartels or cartel duration.

Figure 1.1: Cartel cases decided by the European Commission 1990–2019.



Source: European Commission (2020, section 1.9).

useful instrument for the evaluation of new policy tools and for analyzing the effects of cartel stability *ceteris paribus*.

We build on – and extend – an established experimental literature on the effects of leniency programs. Apesteguia et al. (2007) examine the effect of leniency programs in one-shot Bertrand games. They find that the implementation of the leniency rule tends to increase self-reporting and decrease cartel formation, and leads to significantly lower market prices. Bigoni et al. (2012) and Hinloopen and Soetevent (2008) analyze the repeated Bertrand game. The main result of this literature is that the introduction of leniency leads to a reduction in cartel formation.[8] The effect of private damage claims on leniency programs has not yet been studied.

Besides showing that leniency improves cartel policy effectively, the experimental literature has made further advances. While Bigoni et al. (2012) and Hinloopen and Soetevent (2008) differ in various elements of the experimental design (number

[8] Hinloopen and Onderstal (2014) study the effects of leniency on bidding rings in auctions. Bid-rigging is also analyzed in Luz and Spagnolo (2017) with a novel focus on the effect of corrupt officials involved in the cartelization. Feltovich and Hamaguchi (2018) find that leniency also has a pro-collusive effect due to the lower cost of forming a cartel. This effect is, however, offset by firms' reporting, so the overall effect on collusion is negligible. Clemens and Rau (2018) investigate leniency policies that discriminate against ringleaders and find that this, paradoxically, stabilizes collusion. Andres et al. (2021) add an innovative element to the experimental leniency literature by having participants play the role of the cartel authority. In a cartel experiment without leniency, Gillet et al. (2011) investigate how the managerial decision-making process affects cartel formation and pricing.

of firms, product differentiation, and number of supergames), the most important improvement in the experimental design is that Bigoni et al. (2012) allow deviators to report the cartel even before all prices are announced (and not only after). In contrast to Hinloopen and Soetevent (2008), a "deviate and report" strategy before other cartel firms observe the deviation becomes feasible. This is also the case in the field, and it has the advantage that the deviating firm becomes the first leniency applicant and thus receives full immunity. This strengthens the incentives to deviate. Bigoni et al. (2012) show in their data that this is empirically the case. In their leniency treatment, deviations are usually combined with a secret report, and reporting rates are much higher. (See also section 1.2 below.)

Beyond introducing private damages, we extend the literature by comparing structured and free chat-like communication between participants. Some experiments analyze structured communication in the form of price announcements among players where subjects have boilerplate messages available (Bigoni et al., 2012; Hinloopen and Soetevent, 2008). In the context of cartels, both structured communication and chat seems plausible. Cheap talk is recognized as an important tool for the coordination of cooperative outcomes in experiments (Blume and Ortmann, 2007; Camera et al., 2011; Cooper et al., 1992). In the field of antitrust, experiments identify this kind of chat as a powerful device for fostering collusion (Kruse and Schenk, 2000; Cooper and Kühn, 2014; Fonseca and Normann, 2012; Waichman et al., 2014). While the comparison of chat to structured price announcements has been made for collusion experiments without leniency (recently, Harrington et al. (2016)), it seems promising to conduct this comparison with the inclusion of leniency. Likewise, Apesteguia et al. (2007) and Dijkstra et al. (2020) conduct leniency experiments with chat communication but do not compare to non-chat forms of communication.[9]

Our experiment is designed to analyze the effects of private damage claims on leniency applications, cartel formation, and cartel stability. We have the following main research questions. First, do we observe fewer cartels being established following the introduction of private damage claims? Second, is there a decreasing rate of leniency applications due to private damages? Third, what is the overall balance in terms of cartel prevalence?

The experimental design is largely based on Apesteguia et al. (2007), Bigoni et al.

[9] Landeo and Spier (2009) demonstrate anticompetitive effects of chat-like communication in the context of exclusive dealing.

(2012) and Hinloopen and Soetevent (2008). Subjects play a repeated homogeneous-goods Bertrand triopoly game. They decide whether they want to engage in collusive behavior by communicating about prices, and we vary the communication format available to subjects. We investigate settings with and without private damage claims.

Our first set of results – that are based on a comparison of exisiting private damages to a benchmark in which damage claims are not present at all – is as follows: We show that cartel formation at the individual and the group level is significantly lower with private damage claims. When private damage claims apply, leniency applications decrease notably (although not significantly) and, therefore, cartels seem to be more stable. Overall, the balance is positive as there is an altogether significantly lower level of cartel prevalence. The effect on consumer welfare depends on the form of communication. Private enforcement significantly decreases average prices and therefore increases consumer surplus when communication is structured. Intriguingly, we find the contrary welfare effect in a treatment with chat communication, that is, prices tend to increase, although not always statistically significantly so.

Can the situation be improved, or are the detrimental effects of private damages unavoidable? Buccirossi et al. (2020) show in a theory paper that improved legislation can help, such that damage actions will improve the efficacy of leniency programs.[10] It is not necessary to limit the cartel victims' rights to claim their losses. Buccirossi et al. (2020) point out that the Hungarian Competition Act (in the 2009 version, predating the EU 2014 directive) ensured that the immunity recipient was liable for compensating the cartel's victims only if the other cartel firms were unable to do so. In theory, this destabilizes collusion (increases the minimum discount factor required for collusion). That is, private damages (if treated in this manner) strengthen (rather than weaken) public enforcement.[11]

In an important extension of our experiment, we show that this hypothesis turns out to be empirically sound. We run an additional treatment in which the first leniency applicant is fully protected from damage claims; the other cartel members

[10] Already Spagnolo (2003) argues that leniency programs can be optimal only if they protect a reporting firm from being sued for damages.

[11] Another important topic in this regard is the access to leniency statements and documents. The EU Directive states that national courts cannot impose disclosure of leniency statements and settlements (art. 6[6]). In addition, member states need to ensure that evidence of both categories is either deemed inadmissible in cartel damage suites or is protected according to national rules (art. 7(1)).

are liable. Our data confirm that leniency and damages can be complementary tools that reinforce cartel deterrence and maintain leniency incentives under this assumption. This confirms the theory of Buccirossi et al. (2020) and gives a first hint that the conflict based on the current EU legislation between leniency and damages can be removed by a change in the design of this legislation.

The article is organized as follows: The subsequent section describes the experimental design and explains the treatments in detail. Section 1.3 presents our hypotheses which are the basis for our further analyses in section 1.4. Section 1.5 provides insights of an additional treatment that protects the leniency applicant from damage suits. We conclude in section 1.6.

1.2 The experiment

We choose to study a design that is close to the one incorporated by the EU Directive (European Commission, 2014). This is only one of numerous designs that could be studied. Variations could concern disclosure rules or liability (see Buccirossi et al., 2020). Chapter 1.5 presents one possible variation in the liability for damage claims.

1.2.1 General setup

The market model underlying the experiment is a symmetric three-firm homogeneous-goods Bertrand oligopoly.[12] Demand is inelastic and {101, ..., 110} is the choice set of prices. Firms have constant marginal costs of $c = 100$. There is repeated interaction: the three players are grouped together in one market for the entire duration of the experiment (at least 20 periods).

In our experiment, firms can form cartels, report any existing cartel to a fictitious cartel authority in order to get immunity due to leniency, and may face penalties and private damage claims. Our treatments vary with the implementation of private damage claims and the form of communication. The sequence of events in our experiment is as follows:

1. Decision whether to form a cartel; if all firms agree, communication is enabled and (non-binding) agreements on prices are possible,

[12] Dufwenberg and Gneezy (2000) show that the Bertrand solution is viable for randomly rematched markets with three and four firms but not for two. Huck et al. (2004) find that repeated Cournot markets with four or five firms do not behave collusively. See also Roux and Thöni (2015) for a more recent study.

2. Price decision,

3. Decision whether to report a cartel; unreported cartels may be detected by the cartel authority; in either case a fine is imposed,

4. Private damage claims.

As mentioned in the introduction, Bigoni et al. (2012) allow for both "secret reports" (right after the price decisions) and "public reports" (after feedback on prices). The crucial difference is that, at the stage of the "public" report, subjects know the price chosen by competitors. A deviator would hence report at the "secret" stage in order to become the first leniency applicant. This strengthens the incentives to deviate and report, and makes the leniency scheme more effective. For simplicity, our design nevertheless follows Hinloopen and Soetevent (2008) who implement "public reports" only. Though Bigoni et al. (2012) show that having "secret reports" matters a lot empirically in general, there are reasons to believe that the drawback does not matter much in the present study. For our design and numerical parameters, only the deviating firm has an incentive to report (in fact, has a dominant strategy) whereas the cheated-upon firms should not report. In theory, the deviator is the first (and only) leniency applicant (see the Appendix). Hence, not allowing for "secret reports" should theoretically not reduce deviating incentives too much in our case compared to a variant where they are possible. In the experiment, revenge reports by cheated-upon firms are still possible whereas, in Bigoni et al. (2012), deviators can prevent these by reporting at the first (the "secret") reporting stage. So the drawback might still matter. We add, though, that even if our design reduces the number of leniency applications for this reason, this would be the case for all our treatments. If so, we would still be in a position to observe treatment differences regarding reporting *certeris paribus*.

1.2.2 Detailed account of the stages of the experiment

We now explain the stages in turn.

Stage 1. The three firms simultaneously and independently decide whether they want to establish a cartel. They press either the *discuss price* or the *do not discuss price* button on the computer screen. Only if all three firms decide to participate in price discussions a cartel is established, and a communication window opens.

Depending on the treatment, firms have access to either structured or free chat communication (see section 1.2.3).

Stage 2. Firms simultaneously and independently choose an integer price from the set {101, ..., 110}. The lowest price among the three ask prices p_i with $i \in \{1, 2, 3\}$ is the market price, denoted by $\underline{p}$. Only firms that bid $\underline{p}$ are able to sell their product (Bertrand competition). The inelastic demand is normalized to one, so firm i's profit is:

$$\pi_i = \begin{cases} \frac{p_i - c}{n} & \text{if } p_i = \underline{p} \\ 0 & \text{if } p_i > \underline{p} \end{cases}$$

where c denotes the marginal cost of production of 100 and $n \in \{1, 2, 3\}$ is the number of firms charging $\underline{p}$. Firms learn $\underline{p}$ and their own profit as feedback afterwards. Profit is the gain resulting from the market interaction, which may subsequently be reduced by penalties and private damage claims.

Stage 3. Firms decide whether to report any existing cartel to the authority and thereby apply for leniency. Reporting costs $r = 1$ point (the experimental currency unit) that represent legal fees for filing a leniency application. There is a "race to report": the first leniency applicant gets a 100% fine reduction and the second applicant gets 50%; the third applicant does not receive a reduction. If no participant reports the cartel, it may still be detected by the authority, namely with a probability of $\rho = 0.15$ in each period. If a cartel is detected (either through a whistleblower or the random draw of the authority), each cartel member has to pay a fine, F, equal to 10% of the current period revenue.[13], [14]

Stage 4. Private damage claims may occur after a cartel is detected. Since we do not include cartel customers in our experiment, this stage is not a decision. Rather, the damage claims are simply enforced with a probability of $\sigma = 0.95$.[15] If the private

[13] The revenue is defined as the quotient of the market price and the number of firms that sell at market price, see 1.7.1

[14] These fines are consistent with European policy, including the "race to report" (European Commission, 2002, par. (23)b). Leniency applicants are immune or eligible to reductions of fines levied on infringers by the Commission (European Commission, 2006). Those who are first to report are fully relieved from cartel fines; "subsequent companies can receive reductions of up to 50% on the fine that would otherwise be imposed (European Commission, 2011)." In line with European competition law, fines shall not exceed a maximum of 10% of a firm's overall annual turnover when the respective firm is not eligible to reductions of fines (European Commission, 2011). These parameters are also used in Bigoni et al. (2012) and Hinloopen and Soetevent (2008).

[15] If damage claims are brought to court, the probability that a case is won is presumably relatively high because one goal of the Directive on antitrust damages actions (European Commission,

enforcement case is won in favor of the injured party, the cartel has to compensate 60% of the total damage.[16] The damage inflicted is the difference between the cartel price and the competitive (Nash equilibrium) price, 101 (European Commission, 2014, par. (39)), summed over the number of periods, T, where the cartel formally exists. A cartel is established once all firms in one group decide to communicate by clicking the *discuss price* button. A cartel formally exists as long as it is not reported by a cartel member nor detected by the cartel authority in stage 3. In consequence, the cartel continues to exist even if one or more cartel members deviate from the price agreed upon during the communication phase and do not report. Similarly, a cartel continues to exist even if cartel members communicate only once at the very beginning of the cartel or stop communicating for any number of periods in-between. For each period in which a cartel formally exists, the cartel price is defined as the market price in the given period.

According to the European Commission (2014, par. (37)) cartel members are jointly liable for the total damage, and therefore, each cartel member has to pay one third of the damage compensation. The per-firm per-period damage reads $D_i = \frac{1}{3}(p - 101) \cdot 0.6$ where p is the price the cartel charges in some period and 101 is the counterfactual (Nash) price. For example, fixing the cartel price at 110 (the maximum possible price), the compensation each cartel member has to pay for each period of the cartel's duration is $\frac{1}{3} \cdot (110 - 101) \cdot 0.6 = 1.8$. Table 1.1 summarizes the calculation for the damages and draws a comparison to fines.[17]

2014) is to make it easier for injured parties to get evidence (European Commission, 2015). A large share of private damage claims are also settled out of court (Bourjade et al., 2009).

[16] For two reasons it is reasonable to assume that the total damage is not compensated. First, not all buyers will claim damages, for example, because the buyer structure is fragmented or because it is costly to open a case. Second, it could be the case that part of the damage is passed on in the value chain. The passing-on argument can serve as a strategy of defense of the cartel members against a claim for damages (European Commission, 2014, par. (39)).

[17] Modeling fines which cumulate over the periods that the cartel exists instead of a linear fine would notably reduce the difference between fines and damages. While in our setting non-deviators do not have an incentive to report the cartel, in a setting with cumulated fines also non-deviators could have an incentive to report the cartel as they would suffer from fines of former period's revenues. Hence, with cumulated fines collusion could become less attractive and stable. Further research could analyze whether cumulated fines have effects similar to private damage claims in PDC+ (see section 1.5).

Table 1.1: Comparison of fines and private damage claims.

	Fine	Private damage claims
Probability of imposition (if caught)	100%	95%
Basis	Current period firm revenue	Cumulated damage
Magnitude	10% each firm	60% jointly
Possibility to reduce	Yes	No

1.2.3 Treatments

Our main treatment variable is the presence of private damage claims in stage 4. In the treatment labeled NOPDC, they are absent (there is no stage 4). In treatment PDC, they are potentially imposed. We conduct these two treatments *within subjects*: participants first play NOPDC and then PDC.[18]

Table 1.2: Within-subjects variation of private damages.

Periods	1 ... 9	10	11 ... end
Treatment	NOPDC	NOPDC, introduce PDC after stage 2	PDC
Stages	1	1	1
	2	2	2
	3	3	3
		4	4

Participants first play nine periods of NOPDC (stages 1–3). In period 10, the new PDC rule (stage 4) is announced after stage 2. Then, subjects play PDC (stages 1–4) for the remainder of the experiment.

Each experimental session consists of at least 20 rounds. From period 20 onwards, the session ends with 20% probability. Such a random termination rule is suitable for avoiding end-game effects (Normann and Wallace, 2012). As Table 1.2 shows, subjects play nine periods of NOPDC. In period 10, the rules of the game change as

[18] This within-subjects design allows us to observe cartels that were set up before the introduction of the PDC rule, such that the introduction of private damage claims comes unexpectedly for existing cartels. Empirically, it turns out there are only few such cases, so we refrain from exploiting this advantage of the experimental design.

we introduce private damage claims, after stage 2 (see Table 1.2). From period 11 on, they play PDC for the rest of the experiment. The instructions (see Bodnar et al., 2021) mention that the rules might change during the course of the experiment, but they did not indicate when the change would occur nor what it would entail.[19]

In the field, private damage claims were introduced after and in addition to existing public enforcement, justifying the sequence NOPDC-PDC on which we focus in our experiment. For the sake of completeness, the reverse order PDC-NOPDC may seem warranted. We accordingly conduct sessions with the reverse order of treatments. Thereby, we can control for possible order effects by comparing the first 10 periods of each treatment sequence, for example, the first 10 periods of NOPDC–PDC with the first 10 periods of PDC–NOPDC. In the variant form of reverse order, stage 4 is removed (rather than added) in period 10.

As mentioned, we also modify the communication format in two treatments. This treatment variable is analyzed *between subjects*, that is, the treatment of different communication designs is done in separate experimental sessions. Potential carry-over effects (hysteresis) of the different communication formats make a within-subjects design unappealing in this case.

The communication formats are labeled CHAT and STRUC. (The procedure of structured communication (STRUC) closely follows Hinloopen and Soetevent (2008). It resembles experiments where subjects may announce prices non-bindingly but cannot communicate otherwise (Harrington et al., 2016; Holt and Davis, 1990)). Hence, in sessions with STRUC, participants are only able to suggest a price range for which the good could be sold. Specifically, subjects can enter a minimum and a maximum price (within the range of {101, ..., 110}) in the communication window. In subsequent rounds of price discussions (in the same period), subjects can choose prices from the intersection of all three suggested price ranges from the preceding discussion. If no intersection exists, subjects can choose a price from the complete price range. This iterative process lasts until either the subjects (non-bindingly) agree on a common price or after 60 seconds have passed (which, according to Hinloopen and Soetevent (2008), is sufficiently long enough.) After the communication phase has ended, subjects get feedback on the agreed upon price or the price interval.

In sessions with CHAT, subjects can freely communicate in a chat window. We

[19] An alternative setup would have been to repeat the supergames in order to facilitate learning. This, however, would have precluded the within-subjects "before and after" evaluation of private damages which we considered essential for external validity.

allow for open communication, letting subjects exchange any information they want (except for offensive messages, or messages identifying participants). After 60 seconds, the chat window closes and subjects enter stage 2. Among others, Cooper and Kühn (2014), Fonseca and Normann (2012) and Harrington et al. (2016) have used similar chat devices in oligopoly experiments. Brosig et al. (2003) investigate the issue of the communication format on cooperation in general.

Table 1.3 summarizes our treatments. It also indicates the number of groups and participants for each treatment. In chapter 1.5, we introduce and analyze an additional treatment, which is labeled PDC+ and also involves 48 subjects.

Table 1.3: Overview of treatments.

Sequence	Communication	Number of indep. groups	Number of participants
NOPDC - PDC	STRUC	16	48
NOPDC - PDC	CHAT	16	48
PDC - NOPDC	STRUC	16	48
$\sum$		48	144

1.2.4 Procedures

The experimental sessions were conducted in the summer and fall of 2018 at the DICE-Lab of Duesseldorf University. We had a total of 192 participants. Subjects were students from all over campus. They had previously indicated their general willingness to participate in lab experiments by registering for our database and were then recruited for this experiment using ORSEE (Greiner, 2015).

Upon arrival at the DICE-Lab, subjects were welcomed and allocated to isolated computer cubicles. We used a randomization device to assign the cubicles. After all participants were seated, they were given written instructions. Subjects were given ample time to read the instructions and they had the opportunity to ask the experimenter questions (in private). Then, the actual experiment began.

During period 10, the experiment was interrupted and a second set of written instructions (which explained the change regarding private damages) was distributed. The change of rules was also announced on the computer screen and comprehension was checked with control questions.

The experiment was programmed using z-Tree software (Fischbacher, 2007). Sessions lasted about one hour on average. Payments were as follows. Participants received an initial capital of 5 euros. Cumulated payoffs were added to or subtracted from the initial capital. The exchange rate was one point equal to 0.3 euros. The average payment was 13.08 euros.

1.3 Hypotheses

In this section, we will use the following notation (for a comprehensive overview of all variables and their numerical realizations in the experiment, see Appendix 1.7.1). The collusive profit per firm is denoted π_i^c. In the static Nash equilibrium, each firm earns π_i^n. The profit of a defecting firm is denoted π_i^d. Reporting costs are r. Unless reported, a cartel is detected by the authority with a probability ρ and, if so, the authority imposes a fine F_i^j per firm i and outcome $j \in \{c, d, n\}$, with c for collusion, d for deviation and n for Nash. A busted cartel faces damage claims with probability σ. The per-firm per-period damage is denoted by D_i^j. Damages are cumulated over the time of cartel duration. Fines and damages depend on the price and thus differ in periods of collusion and defection.

We assume that the market game is repeated infinitely many times and that firms discount future profits with a discount factor δ.[20] A further assumption is that firms collude on the maximum price (110) and use a simple Nash trigger to support collusion, such that the static Nash profit, π^n, is the punishment profit after a deviation.[21] For simplicity and following Bigoni et al. (2015), we assume that firms communicate once to establish successful collusion and collude tacitly after a detection by the authority. That is, firms risk being fined only once.[22] Formal proofs of the statements in this section can also be found in Appendix 1.7.1.

Our first hypothesis is about cartel formation, that is, the number of newly

[20] In the experiment, a stopping probability (which corresponds to discounting) is effective only after 20 periods. One can show (proof available upon request) that this changes incentives only qualitatively and to a minor extent.

[21] Colluding on the maximum price seems plausible as this maximizes joint profits. It is possible, however, to lower the threshold discount factor by choosing a lower collusive price. Since this effect is of minor magnitude and similar in all treatments (and hence does not affect our hypotheses), we refrain from exploring this issue in detail. We further note that punishments more severe than Nash are not feasible here because the Nash price is also the lowest price firms may charge.

[22] Alternatively, we could assume that busted cartels never resume the collusion but play Nash after a detection. However, with our experimental parameters the "return to Nash" assumption reduces profits too strongly, such that collusion is not rational for some treatments.

formed cartels. The economic theory of crime predicts that criminal activity decreases in the expected costs of the activity (Becker, 1968). We derive this formally (see Appendix 1.7.1 for details) from the cartel's *participation constraint* which must necessarily be met, see also Bigoni et al. (2015) or Chowdhury and Wandschneider (2018). The expected discounted profit from colluding minus the expected fine (left-hand side of the equation) must be at least as high as the expected discounted profit from competing à la Nash (right-hand side of the equation). For the NOPDC case, we have

$$\frac{\pi_i^c}{1-\delta} - E(F_i^c) \quad \geq \quad \frac{\pi_i^n}{1-\delta}$$

where $E(F_i^c)$ is the expected discounted fine. Private damage claims increase the expected costs of cartel formation because firms now need to cover the expected damages in addition to the fines. For PDC, the cartel participation constraint reads

$$\frac{\pi_i^c}{1-\delta} - E(F_i^c) - E(D_i^c) \quad \geq \quad \frac{\pi_i^n}{1-\delta}$$

where $E(D_i^c)$ is the expected, discounted, and cumulated, per-firm damage payment resulting from successful collusion. The total damage (D^c) is equally split between all three cartel members such that $E(D_i^c) = E(D^c/3)$. For our experimental parameters, both participation constraints are met, but, with private damages, the cartel participation constraint is more severe. We thus maintain:

Hypothesis 1. ***(Cartel formation)*** *Private damage claims reduce the number of cartels.*

The next hypothesis concerns the reporting behavior of firms: In which treatment – PDC or NOPDC – will firms apply for leniency more often? Firms only have an incentive to report a cartel when they deviate from the cartel price (reporting and not deviating is not beneficial because the cartel will cease to exist after the report anyhow). Deviations, in turn, can occur with unexpected trembles in the discount factor (Buccirossi et al., 2015).[23] Given a firm deviates, it is rational for this firm to report in all treatments (this keeps treatments comparable), but a comparison of

[23] Other motives for deviations may occur when firms trade off the risk of a collusive equilibrium against a less risky defect strategy (Buccirossi et al., 2020; Green et al., 2015). Similarly, US Horizontal Merger Guidelines acknowledge the role of disruptive maverick firms (Darai et al., 2019; Kovacic et al., 2007), and such mavericks may have an incentive to deviate, for example, due to a merger motive.

the costs of reporting across treatments shows the following insights. In treatment NOPDC, reporting only involves r, the immediate cost of reporting. In treatment PDC, firms also incur r but they additionally need to pay damages σD_i^d. For the experimental parameters, it turns out that reporting costs for the deviator are more than 2.5 times higher under PDC than under NOPDC (Appendix 1.7.1). Thus, the costs of reporting increase and the incentive to apply for leniency strongly decreases with private damages. Assuming that firms occasionally make mistakes but make costly mistakes less often than cheaper ones, we hypothesize:

Hypothesis 2. ***(Leniency)*** *Private damage claims reduce the frequency of leniency applications.*

We now analyze the dynamic incentives to collude. As mentioned, firms attempt to maximize joint profits with a trigger strategy involving Nash reversion. Cartel firms remain liable for the agreement in future periods, until detected or reported. The incentive constraints required for collusion to be a subgame perfect Nash equilibrium read as follows. Without private damages (NOPDC), sticking to the collusive agreement is (weakly) better than defecting if

$$\frac{\pi_i^c}{1-\delta} - E(F_i^c) \quad \geq \quad \pi_i^d - r + \frac{\delta \pi_i^n}{1-\delta}.$$

With private damages (PDC), colluding is better than defecting if

$$\frac{\pi_i^c}{1-\delta} - E(F_i^c) - E(D_i^c) \quad \geq \quad \pi_i^d - r - \sigma D_i^d + \frac{\delta \pi_i^n}{1-\delta}$$

where we note that damages have to be paid in either case, but they differ in magnitude (see Appendix 1.7.1 for details). Again, the total damage (D^j) is equally shared among all cartel members, which implies $E(D_i^j) = E(D^j/3)$. Let the minimum δ that solves the NOPDC and PDC incentive constraints be δ_{min}^{NOPDC} and δ_{min}^{PDC}, respectively. We find that

$$\delta_{min}^{PDC} < \delta_{min}^{NOPDC}.$$

For the parameters in the experiment, we obtain $\delta_{min}^{NOPDC} = 0.664$ and $\delta_{min}^{PDC} = 0.655$. With a continuation probability of 0.8, both incentive constraints are met in the experiment and so collusion is an SGPNE in either case. We follow the frequently adopted interpretation that a lower minimum discount factor suggests that collusion

is more stable, provided a cartel is actually set up. Hence, we state:

Hypothesis 3. ***(Cartel stability)*** *Existing cartels are more stable when private damage claims are possible.*

An interesting observation is that reporting costs and the incentive constraint under private damages become more severe over time because damages are cumulated. Deviations become more and more costly in later periods. Private damages accordingly have a self-enforcing effect on collusion. In theory, this effect is immaterial, though. All that matters is whether the incentive constraint is met in period zero when the incentive to deviate is at its maximum. The fact that the bill for reporting gets higher and higher could be important, though. For example, unanticipated shocks to collusion may be absorbed only with the high exit cost that the cumulated damages imply.

Our hypotheses suggest an overall ambiguous effect of private damage claims. On the one hand, there should be fewer cartels. On the other hand, cartels should be more stable and there may be less reporting in PDC. The overall balance in terms of cartel prevalence is ex ante not clear and we do not maintain a directed hypothesis here.

Statement 4 (Cartel prevalence) *The overall effect of private damage claims on cartel prevalence is ambiguous.*

As with cartel prevalence, we do not maintain a directed hypothesis about market prices (the measure for consumer welfare). Market prices (the lowest of the three ask prices) are affected by (at least) two channels. First, market prices may decrease because, according to hypothesis 1, fewer cartels are formed with private enforcement, leading to more competitive prices. Second, any existing cartels would suffer less from leniency (hypothesis 2) and may be more stable (hypothesis 3) and should therefore have higher market prices, on average. The overall effect is ambiguous. Of course, we can look at the effect of PDC for cartelized markets only. But, even here, the effect is ex-ante ambiguous. On the one hand, cartels under PDC may collude more successfully due to a selection effect (only rather collusive-minded firms form a cartel despite the more severe constraints). On the other hand, cartel members could fear damage claims and therefore lower the prices.

Statement 5 (Market prices) *The overall effect of private damage claims on market prices is ambiguous.*

Our final hypothesis is about the impact of the different forms of communication. Existing experimental evidence (Cooper and Kühn, 2014; Fonseca and Normann, 2012) suggests cartels are more stable when subjects can communicate. It appears that open communication fosters trust between players (Brosig et al., 2003). Also, subjects can communicate entire strategies rather than just price targets. Furthermore, chat communication can enhance the understanding of the mutual benefits of collusion in their group. Kruse and Schenk (2000) observe that only one group member has to understand the profit-maximizing strategy and can use the chat to convince its group members to comply.

Hypothesis 6. ***(Impact of communication)*** *Compared to structured communication, unrestricted communication increases cartel formation and stability.*

1.4 Results

To analyze the impact of private damage claims, we foremost analyze the data within subjects. That is, we compare the first 10 periods (NOPDC) to the subsequent 10 periods (PDC). We restrict the analysis to observations from periods 1 to 20 in order to exclude potential end-game effects. With the help of the reverse-order control treatment, we then compare the data between subjects to exclude possible order effects (both PDC and NOPDC data from periods 1 to 10). We use non-parametric tests like the Wilcoxon matched-pairs test (WMP) for the within-subjects analysis and the Mann-Whitney U test (MWU) for the between-subjects analysis. With the WMP-Test, we match the NOPDC with the PDC observations of each group. For all analyses, we first take the average per group as one observation and aggregate across groups afterward. In total, we have 16+16 observations. When we analyze the share of firms that report a cartel, we generally have fewer observations because the analysis is conditional on having a cartel in the first place, which is not the case for all groups.

We complement the non-parametric tests with linear regression models (ordinary least squares) with and without time fixed effects. We run the estimations separately for each communication treatment. Due to the fixed group structure, we cluster standard errors at the group level. We bootstrap the standard errors with 1,000 replications. Statistical significance levels are indicated by an asterisk, where $^{+}$ ($p < 0.15$), * ($p < 0.1$), ** ($p < 0.05$), *** ($p < 0.01$). We report two-sided p-values throughout.

An overview of the summary statistics of our main results is displayed in Table 1.4. The precise definition of each variable can be found in Table A2 in Appendix 1.7.2. The exact values underlying Figures 1.2 to 1.9 can be obtained from Table 1.4.[24]

Table 1.4: Summary statistics of the results in the treatments NOPDC–PDC (STRUC and CHAT).

	STRUC		CHAT	
	NOPDC	PDC	NOPDC	PDC
Propensity to collude	0.619 (0.142)	0.394 (0.192)	0.578 (0.288)	0.225 (0.289)
Share cartel	0.207 (0.153)	0.019 (0.054)	0.271 (0.373)	0.063 (0.250)
Share report	0.462 (0.230)	0.296 (0.339)	0.103 (0.214)	0.000 (0.000)
Cartel stability	1.000 (0.000)	2.167 (0.866)	6.556 (3.522)	8.000 (1.441)
Cartel prevalence	0.238 (0.178)	0.063 (0.163)	0.325 (0.380)	0.163 (0.359)
Market price	102.706 (2.009)	101.681 (2.095)	105.913 (3.969)	107.038 (4.227)

Average results per treatment (standard deviations in parentheses).

1.4.1 Cartel formation

Hypothesis 1 states that cartel formation decreases when private damage claims are introduced. Consider the individual level first: how often do subjects press the *discuss price* button when they are not already in a cartel? (For this and all other variable definitions, consult Table A2 in the appendix.) Without private damages, the average propensity to collude in STRUC (CHAT) is 61.9% (57.8%), see Figure 1.2 and Table 1.4. With PDC, the average propensity to collude decreases to 39.4% (22.5%), and the reduction is significant (STRUC: WMP, $p\text{-}value = 0.0007$; CHAT: WMP, $p\text{-}value = 0.0015$). For both communication treatments, the individual propensity to form a cartel declines by about 35–22 percentage points when PDC are possible. The estimation results of the linear probability model in Table 1.5 are also consistent with hypothesis 1. We see that the dummy variable PDC is highly significant and economically substantial.

[24] For the exact values of PDC taken from the sequence PDC-NOPDC in Figures 1.4, 1.7, 1.9 see Table A7. Refer to Table 1.4 for the exact values of NOPDC for the sequence NOPDC-PDC.

Figure 1.2: The impact of PDC on the individual propensity to collude in STRUC (left) and CHAT.

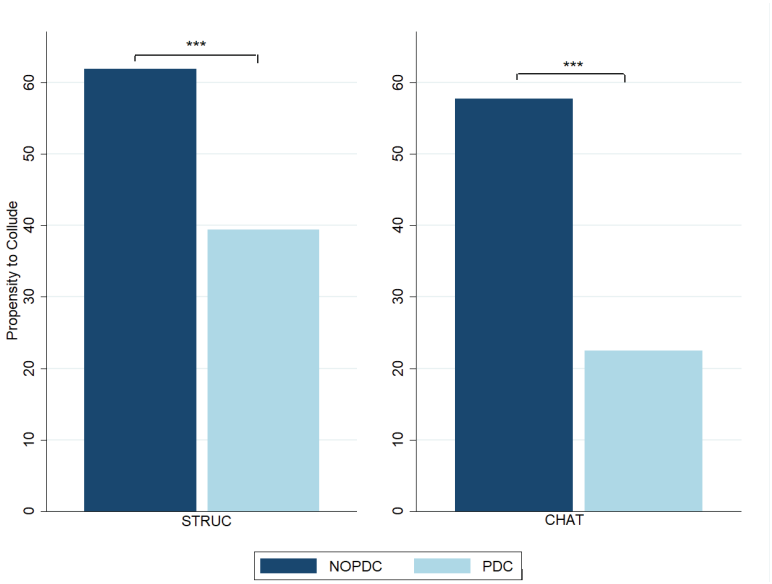


Table 1.5: Individual decisions to communicate – linear regression.

	(1) Collude	(2) Collude	(3) Collude	(4) Collude	(5) Collude
PDC	-0.225***	-0.219***	-0.208***	-0.604***	-0.0925***
	(0.0353)	(0.0482)	(0.0497)	(0.0926)	(0.0317)
constant	0.592***	0.381***	0.583***	0.729***	0.550***
	(0.0350)	(0.0605)	(0.0537)	(0.0648)	(0.0524)
Time FE	No	No	Yes	Yes	Yes
Sample STRUC	Yes	No	Yes	No	Yes
Sample CHAT	No	Yes	No	Yes	No
Sample pooled	No	No	No	No	Yes
N	960	960	960	960	1,860
R^2	0.051	0.060	0.063	0.106	0.033

Standard errors in parentheses.

Sample pooled combines data from NOPDC-PDC and PDC-NOPDC.

$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Next, consider the market (or group) level. Here, we ask the question, how often is a cartel actually established? This is the case when all three group members press the *discuss price* button, given they are not already in a cartel. Figure 1.3 and Table

1.4 show the results. We observe that, with PDC, the share of newly formed cartels is strongly and significantly reduced (STRUC: WMP, $p\text{-}value = 0.0007$; CHAT: WMP, $p\text{-}value = 0.0087$). As above, this holds for both communication treatments, STRUC and CHAT. The regressions in Table 1.6 confirm that the effect is significant.

Figure 1.3: The impact of PDC on the number of cartels in STRUC (left) and CHAT.

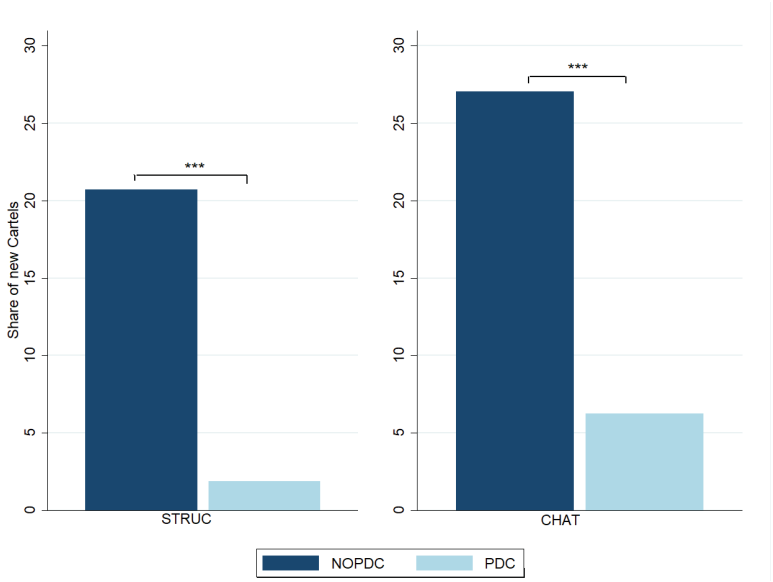


Table 1.6: Group decisions to communicate – linear regression.

	(1) Collusion	(2) Collusion	(3) Collusion	(4) Collusion	(5) Collusion
PDC	-0.181***	-0.0813***	-0.125^{+}	-0.375***	-0.0773***
	(0.0311)	(0.0130)	(0.0817)	(0.116)	(0.0244)
constant	0.194***	0.0875***	0.125^{+}	0.375***	0.166***
	(0.0344)	(0.0172)	(0.0817)	(0.116)	(0.0628)
Time FE	No	No	Yes	Yes	Yes
Sample STRUC	Yes	No	Yes	No	Yes
Sample CHAT	No	Yes	No	Yes	No
Sample pooled	No	No	No	No	Yes
N	320	320	320	320	620
R^2	0.089	0.037	0.119	0.183	0.069

Standard errors in parentheses.

Sample pooled combines data from NOPDC-PDC and PDC-NOPDC.

$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

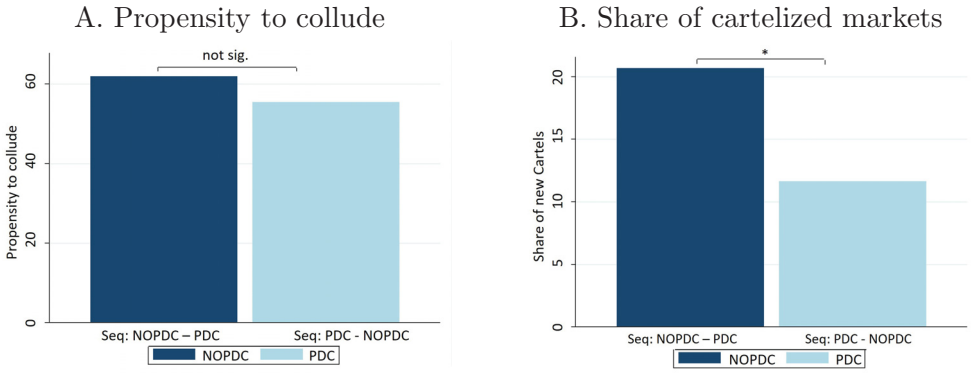
Exploiting the treatment with the reverse sequence PDC-NOPDC with structured communication, we compare the first 10 periods of the NOPDC-PDC sequence with the first 10 periods of PDC-NOPDC sequence. This allows us to additionally conduct the comparison NOPDC and PDC between subjects, thereby excluding order effects.[25] Figure 1.4 shows that the possibility of PDC reduces cartel formation in STRUC both at the individual (A) and at the group (B) level. The reduction is statistically significant at the market level ((A) MWU, $p\text{-}value = 0.153$ (B) MWU, $p\text{-}value = 0.0899$).[26] For the sake of completeness, results of the PDC-NOPDC session analyzed within subjects can be found in Appendix 1.7.7. We also control for possible order effects by analyzing whether our main variables of interest are significantly different under each treatment, when the treatment is run in periods 1–10 rather than 1–20. We do so by conducting MWU-tests (see Appendix 1.7.8), as well as a pooled data analysis from the main structured treatment and the reverse-order treatment (see Table 1.5-1.10 column (5)). These results suggest that there are no order effects. However, the pooled data analysis yields slightly smaller effects. One possible explanation might be that the introduction of private damages would reduce cartelization, but the effect would be amplified by the regime shift and would

[25] Due to bankruptcy we exclude one group in the reverse-order treatment from our analysis.

[26] Linear regressions, available upon request, yield the same result.

become smaller in the long run.

Figure 1.4: Cartel formation in STRUC: between-subjects comparison with PDC data from treatment with reverse order (PDC-NOPDC).



Result 1 (Cartel formation) With PDC, there are significantly fewer attempts to form a cartel (individual level) and significantly fewer successfully formed cartels (group level).

1.4.2 Leniency applications and cartel stability

Hypotheses 2 and 3 are about leniency behavior and cartel stability. For these analyses cartels need to have actually been formed in the first place. We compare the first nine periods NOPDC and period 11 to 19 PDC.[27]

Leniency applications

Hypothesis 2 suggests that there will be fewer leniency applications with PDC. We first analyze the share of individual reporting decisions by each group, that is, we consider the sum of subjects in each group revealing the cartel over all periods that any cartel exists (see Table A2 for the definition of *share report*).

Figure 1.5 and Table 1.4 show that PDC significantly decreases the share of leniency applications in each group in STRUC (STRUC: WMP, *p-value* = 0.101;

[27] For the analyses of leniency applications and cartel stability, we exclude period 10 (for reasons of symmetry also period 20). Subjects decide whether to report a cartel after private damage claims are introduced. Thus, period 10 belongs to neither PDC nor NOPDC. For the analysis of variables other than stability this problem does not exist because decisions about cartel formation or price setting were made before the introduction of private damage claims.

CHAT: WMP, $p\text{-}value = 0.3173$). In the case of STRUC, the effect is also economically substantial.

Figure 1.5: The impact of PDC on the individual reporting decision in STRUC (left) and CHAT.

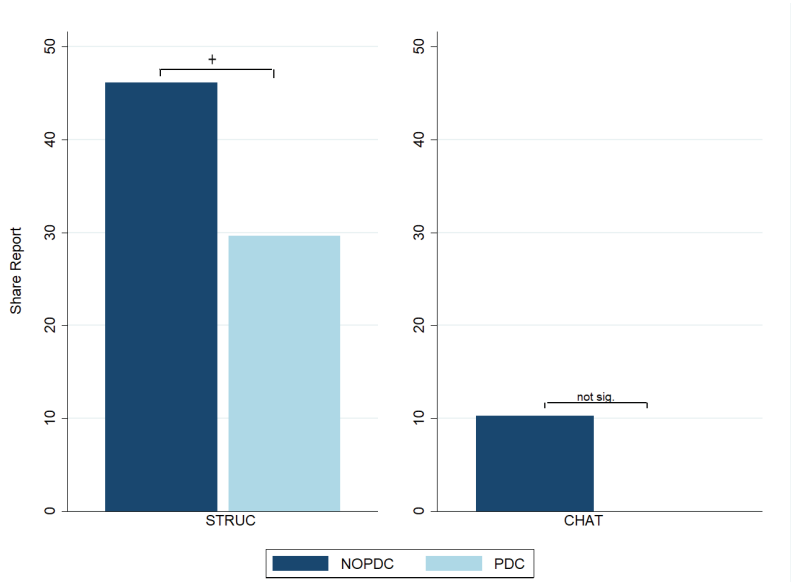


Table 1.7 reports a linear regression of PDC on the individual decision to report a cartel. In STRUC as well as in CHAT the number of cartel members applying for leniency decreases when PDC occur. However, this effect is only significant in the STRUC regressions without time fixed effects. The between-subjects comparison indicates that the share of leniency applications does not differ between NOPDC and PDC. Our interpretation is that subjects may have had too little time – only one repetition of the supergame – to learn the impact of private damages and are thus not more disinclined to report than in NOPDC.

Table 1.7: Individual decision to report a cartel – linear regression.

	(1) Report	(2) Report	(3) Report	(4) Report	(5) Report
PDC	-0.264^{+}	-0.0347	-0.167	-0.0556	-0.0400
	(0.178)	(0.0250)	(0.128)	(0.0494)	(0.0839)
constant	0.412***	0.0347	0.167	0.0556	0.270
	(0.0674)	(0.0250)	(0.128)	(0.0494)	(0.165)
Time FE [Period 1-19, without 10]	No	No	Yes	Yes	Yes
Sample STRUC	Yes	No	Yes	No	Yes
Sample CHAT	No	Yes	No	Yes	No
Sample pooled	No	No	No	No	Yes
N	129	216	129	216	252
R^2	0.050	0.012	0.138	0.077	0.114

Standard errors in parentheses.

Sample pooled combines data from NOPDC-PDC and PDC-NOPDC.

$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Result 2 (Leniency rate) Compared to NOPDC, there are fewer leniency applications with PDC.

Cartel stability

Hypothesis 3 is that cartels become more stable as we introduce private damage claims. In order to analyze cartel stability, we compare the mean number of periods when a cartel was stable,[28] in NOPDC and PDC, conditional on cartel existence. Cartels that are formed and uncovered in the same period count as stable for one period (see also Table A2.) Descriptive results show that the mean of cartel stability roughly doubles in STRUC (in NOPDC 1.0 stable period compared to 2.2 in PDC). In CHAT, stable periods increase from 6.6 in NOPDC to 8.0 in PDC (see Table 1.4). Whereas this result is in line with hypothesis 3, we cannot make any statement about significance because there are too few groups forming a cartel in NOPDC and PDC. For the same reason, we cannot conduct survival estimates.

Result 3 (Cartel stability) With PDC, cartels are more stable.

[28] A cartel is stable until it is reported or detected by the authority. Of course, cartels may continue to set a high price after being reported or detected. For such pricing behavior, they cannot be penalized.

In connection with hypothesis 3, we noted that private damages have an enforcing effect on stability over time because damages cumulate. Cartels should, accordingly, be more strongly discouraged from reporting the longer they exist.

1.4.3 Cartel prevalence

We finally look at cartel prevalence, defined as the percentage of periods where a stable cartel existed. Result 1 on the one hand, and results 2 and 3 on the other, suggest an overall ambiguous effect of PDC on cartel prevalence: fewer cartels are formed but these remaining cartels are more stable. (Due to this ex-ante ambiguity, statement 5 in section 1.3 is not a directed hypothesis about prevalence.) What is the overall balance?

Figure 1.6 and Table 1.4 show the results. For the communication treatment STRUC, we find cartel prevalence present in 23.8% (NOPDC) and 6.3% (PDC) of all groups over all periods. In CHAT, we see 32.5% (NOPDC) and 16.3% (PDC) of periods where a stable cartel existed. That is, there is a strong and significant reduction in cartels due to PDC in both communication treatments (STRUC: *p-value* = 0.0051 and CHAT: WMP, *p-value* = 0.0139). The linear regressions in Table 1.8 confirm this.

Figure 1.6: The impact of PDC on cartel prevalence in STRUC (left) and CHAT.

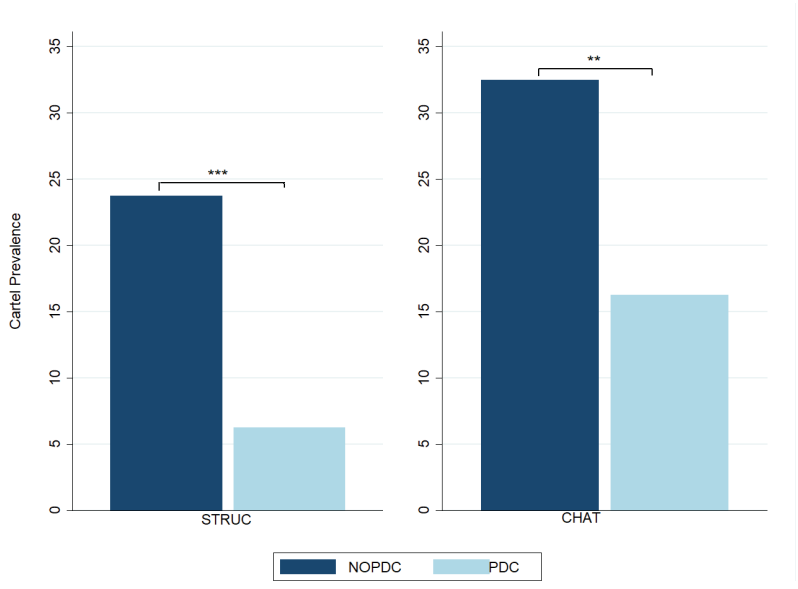


Table 1.8: Cartel prevalence – linear regression.

	(1) Prevalence	(2) Prevalence	(3) Prevalence	(4) Prevalence	(5) Prevalence
PDC	-0.175***	-0.163**	-0.0625	-0.250**	-0.111**
	(0.0484)	(0.0797)	(0.106)	(0.105)	(0.0461)
constant	0.237***	0.325***	0.125^{+}	0.375***	0.183***
	(0.0413)	(0.0915)	(0.0817)	(0.116)	(0.0653)
Time FE	No	No	Yes	Yes	Yes
Sample STRUC	Yes	No	Yes	No	Yes
Sample CHAT	No	Yes	No	Yes	No
Sample pooled	No	No	No	No	Yes
N	320	320	320	320	620
R^2	0.060	0.036	0.096	0.061	0.056

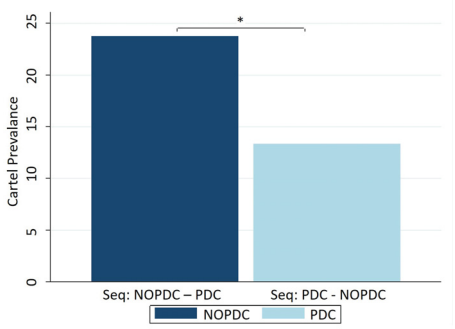
Standard errors in parentheses.

Sample pooled combines data from NOPDC-PDC and PDC-NOPDC.

$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

We again analyze the treatment with the reverse order, PDC-NOPDC and compare the first 10 periods in NOPDC to those in PDC. The results are similar: the between-subjects test is significant (MWU, $p\text{-}value = 0.0842$).

Figure 1.7: Cartel prevalence in STRUC: between-subjects comparison with PDC data from treatment with reverse order (PDC-NOPDC).



Result 4 (Cartel prevalence) There are significantly fewer cartelized periods with PDC.

1.4.4 Prices and Consumer Welfare

To complete the analysis of cartel behavior, we examine the market price. This is the lowest price of the three individually entered prices in stage 2.[29] The market price is the relevant factor for consumer welfare (see statement 5 in section 1.3).

Table 1.9: Market price – averages per treatment.

	STRUC		CHAT	
	NOPDC	PDC	NOPDC	PDC
Market price non-cartels	102.049 (1.897)	101.589 (2.089)	104.566 (3.807)	106.621 (4.373)
Market price cartels	104.654 (2.570)	103.278 (1.669)	109.250 (2.050)	109.967 (0.058)
Market price all markets	102.706 (2.009)	101.681 (2.095)	105.913 (3.969)	107.038 (4.227)

Standard deviations in parenthesis. Seq: NOPDC–PDC.

We compare the average market price with and without private damage claims across the CHAT and STRUC treatments as shown in Table 1.9 and Figure 1.8. We see that PDC reduce prices in STRUC, but CHAT shows the opposite pattern. This concerns the overall average ("all markets") as well as the market prices of cartelized and non-cartelized markets. The differences are statistically significant in STRUC (STRUC: WMP, $p\text{-}value = 0.0034$; CHAT: WMP, $p\text{-}value = 0.2513$). In order to control for possible order effects, we conduct the between-subjects comparison based on PDC data from the treatment with the reversed order PDC-NOPDC. Figure 1.9 verifies the lower overall market prices in PDC with STRUC communication (WMU, $p\text{-}value = 0.0511$).

[29] For an analysis of individual ask prices see Appendix 1.7.5.

Figure 1.8: The impact of PDC on market prices in STRUC and CHAT.

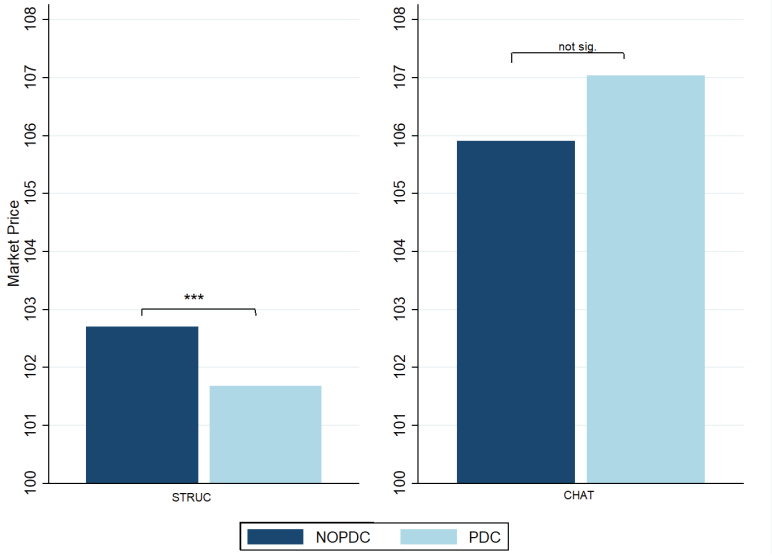


Figure 1.9: Market price in STRUC: between-subjects comparison with PDC data from the treatment with reverse order (PDC-NOPDC).

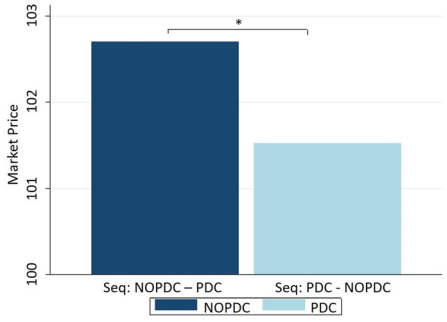


Table 1.10 reports the results from a regression analysis on the dependent variable $MarketPrice$. The results confirm previous observations that market prices significantly decrease in the subsample of STRUC if private damage claims are introduced (Table 1.10, column 1). They significantly increase in CHAT.

Table 1.10: Market price – linear regression.

	(1) MarketPrice	(2) MarketPrice	(3) MarketPrice	(4) MarketPrice	(5) MarketPrice
PDC	-1.025***	1.125*	-1.563***	1.750+	-0.349**
	(0.256)	(0.588)	(0.468)	(1.174)	(0.150)
constant	102.7***	105.9***	102.8***	104.5***	102.5***
	(0.482)	(0.957)	(0.415)	(0.981)	(0.294)
Time FE	No	No	Yes	Yes	Yes
Sample STRUC	Yes	No	Yes	No	Yes
Sample CHAT	No	Yes	No	Yes	No
Sample pooled	No	No	No	No	Yes
N	320	320	320	320	620
R^2	0.044	0.017	0.060	0.031	0.055

Standard errors in parentheses.

Sample pooled combines data from NOPDC-PDC and PDC-NOPDC.

$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Result 5 (Market prices) With STRUC communication, PDC significantly decrease average market prices and therefore increase consumer surplus. With CHAT communication, PDC increase market prices and therefore decrease consumer surplus.

What could be the intuition for the contradicting effects in CHAT and STRUC? Recall that statement 5 in section 1.3 is not a directed hypothesis in the first place. Prices could be lower when private damage claims apply because there are fewer cartels and remaining cartels might be reluctant to set higher prices because of the risk of paying damage claims. This is what might be going on in STRUC. In CHAT, subjects have the chance to coordinate their behavior even beyond a cartel breakdown. We suggest that the price effects in CHAT are triggered by a hysteresis effect, that is, prices that stick to the collusive level even after someone busts the cartel (per definition). For a detailed discussion of the price effects please refer to Appendix 1.7.3 and 1.7.4.

Result 5 raises the question of how much weight should be given to the two opposing results. Given that communication is often open and in person in real-world cartels, it appears that the second part on the effects with CHAT should be given more emphasis, suggesting that damage claims in this form lower consumer

welfare.[30]

1.4.5 Structured vs. chat communication

Our experimental design enables us to analyze not only the effect of private damage claims but also the impact of different types of communication designs on cartel formation and stability. As expected from hypothesis 6, we see quadrupled stability in CHAT compared to STRUC across both treatments, NOPDC and PDC (see Table 1.4). This is also emphasized by the result that infringers apply less often for leniency ($p\text{-}value = 0.0011$) (see Figure 1.5). These results are in line with the literature observing that CHAT communication helps to better coordinate (for example, Fonseca and Normann, 2012; Fonseca et al., 2018), or generally, that communication facilitates collusion (see e.g., Bigoni et al., 2019; Cooper et al., 1992; Cooper and Kühn, 2014; Waichman et al., 2014).

Perhaps surprisingly, the propensity to collude—new attempts to collude at the subject level—is significantly higher in STRUC compared to CHAT ($p\text{-}value = 0.0150$) (see Figure 1.2). There are two explanations for this seemingly counterintuitive result. First, CHAT communication facilitates trust among group members and makes group members stick to the agreements more often and, as seen above, report the cartel less frequently. As a result, subjects in CHAT need to press the *discuss price* button less often. Secondly, the lower fraction of subjects deciding in favor of a new price discussion in CHAT is explained by agreements to stick to the collusive price after cartel breakdown. Subjects in CHAT are able to agree on setting the same price as under collusion after they have been detected and without renewing their price discussion. This is not possible in the STRUC design. This can be seen from the following excerpts of communication (translated from the original German), groups agree to communicate only once:

- `Without in future rounds without [sic] communication then?` (group 5, period 1)
- `Yes but not more communication in the next rounds` (firm 3)

  `Ok, no more communication and 110` (firm 2)

  `Alright. Yes. Always 110, no more communication and no reports.` (firm 1, group 13, period 1).

[30] On hysteresis see also Harrington (2004).

Market prices are higher in CHAT compared to STRUC across all types of markets (p-$value$: 0.0218) (see Table 1.9). As already mentioned, higher prices in CHAT can be explained by an hysteresis effect that keeps prices high even after a cartel breaks down. In line with that, we see much less variation in collusive market prices in CHAT compared to STRUC (p-$value$: 0.0001) (see Table 1.9).

To conclude, CHAT allows subjects to better coordinate their practice compared to STRUC, which leads to an increased stability and hysteresis of cartel prices.

Result 6 (CHAT vs. STRUC) Cartel stability is higher and there are significantly fewer leniency reports in CHAT. The propensity to collude is significantly lower in CHAT.

1.5 Protection from damages for the leniency applicant

Although in an overall assessment of PDC we find a decreasing cartel prevalence in PDC, the results of the preceding section 1.4 also suggest that private damage claims may lower leniency application rates so that cartels are more stable. This negative effect of PDC on leniency and cartel stability suggests a careful reconsideration of the tool of private enforcement.

Better protection of whistleblowers is an obvious option. Kersting (2014) proposes an approach in which the leniency applicant can obtain full compensation for damage payments from its co-infringers. Similarly, Kirst and van den Bergh (2016) suggest a reduction of damage payments of leniency applicants corresponding to the reduction in fines. This should remove the tension between private and public enforcement. As formally demonstrated by Buccirossi et al. (2020), damage claim actions and leniency programs can reinforce each other when the first leniency applicant's liability is minimized (or even eliminated) also with respect to damage claims. This scheme corresponds to the former Hungarian legislation before the implementation of the directive on antitrust damage actions (Buccirossi et al. (2020); European Commission (2014)). In a related piece of experimental evidence, Mechtenberg et al. (2020) analyze whistleblowing in the context of corporate fraud. They find that an increase in reports can be triggered by better whistleblower protection.

In order to test such a potential improvement of the current European legislation, we introduce a new treatment called PDC+. In this new treatment, the first leniency

applicant is fully protected from private damage payments. The remaining two cartel firms jointly pay the damage payment (which remains at 60% of excess Nash industry profit). That is, the remaining cartel members, no matter whether they also reported the cartel, have to pay half of the per-period damage compensation, $D_i^j = \frac{1}{2}(p_i^j - 101) \cdot 0.6 = D^j/2$. By contrast, in our standard PDC treatment, all three cartel members pay one third of the damage ($D^j/3$). Private damage claim actions in PDC+ are enforced with a probability of $\sigma = 0.95$ and they are cumulated over time, as in PDC. If no reporting takes place the cartel authority detects the cartel by probability $\rho = 0.15$, the design follows the PDC treatment as explained in section 1.2.

We study one of several possible designs to analyze the effect of the benefit of the leniency applicant. As in the main treatment, we conduct the experiment also on the basis of a within-subjects comparison. Thus, the treatment order is PDC-PDC+ and we use the STRUC communication treatment. Participants first play nine periods with private damages as above, followed by PDC+ in the remaining periods. Again, the rules of the experiment change in period 10 and PDC+ is introduced after stage 2 (price decision). The extension of the experiment was conducted in the structured communication setting and was programmed using z-Tree software (Fischbacher, 2007). The sessions took place in January and July 2020 and involved 48 participants.

What are our hypotheses for PDC+? First, the participation constraints in PDC+ and PDC are the same because fines and damages for successful collusion do not change compared to PDC (only deviation and reporting change). We thus do not expect an impact on the frequency of cartels. The costs of reporting are much lower in PDC+ in the case of a deviation, as the deviator will report (which costs r) but pays no fine and no damages (because of the damage-leniency of PDC+).[31] Second, the incentive constraint in PDC+ changes compared to PDC because damages have to be paid only in the case of stable collusion. The incentive constraint thus becomes

$$\frac{\pi_i^c}{1-\delta} - E(F_i^c) - E\left(\frac{D^c}{3}\right) \quad \geq \quad \pi_i^d - r + \frac{\delta \pi_i^n}{1-\delta},$$

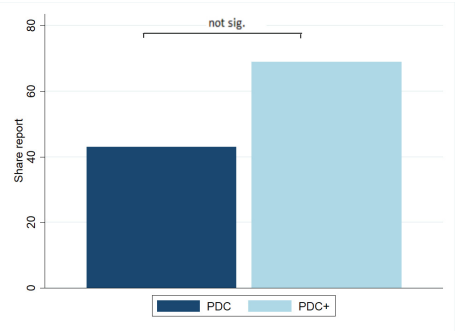
which is more severe than the constraint obtained above for PDC, so $\delta_{min}^{PDC+} > \delta_{min}^{PDC}$.

[31] In the PDC+ treatment, the deviator strategically sets a lower deviation price than in the NOPDC and PDC treatment. The lower price prevents rival firms from reporting (after observing the deviation) and thus maximizes deviation profits. Please refer to Appendix 1.7.1 for detailed explanations.

For the parameters in the experiment, we obtain $\delta_{min}^{PDC+} = 0.682$ whereas $\delta_{min}^{PDC} = 0.655$. That is, PDC+ hinders collusion as intended by the new policy. For all statements, see Appendix 1.7.1 for details.

Hypothesis 7. ***(Protection from damages for the first leniency applicant)*** *More cartels will be reported in PDC+ than in PDC.*

Figure 1.10: Share report in STRUC: within-subjects comparison from the treatment PDC-PDC+.



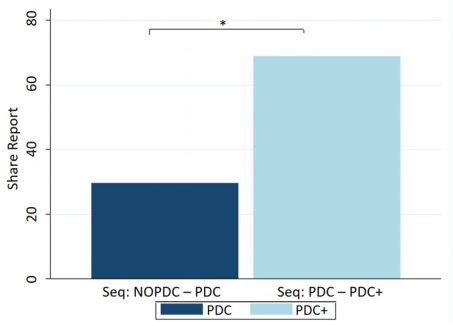
The results support the notion that PDC+ results in lower cartel stability. Cartels break down more often due to a higher share of reports by individuals. The within-subjects design results based on group level can be seen in Figure 1.10. We see a reporting share of 43% in the PDC treatment and a reporting share of 68.9% in the PDC+ treatment, resulting in an increase of 25.9 percentage points. The same holds for the number of stable cartel periods. In the PDC+ treatment, cartels are, on average, 0.33 periods less stable compared to the PDC treatment. Whereas this result is in line with hypothesis 1 for the PDC treatment, we cannot make any statement about significance because there are too few groups forming a cartel in PDC and PDC+.[32]

Results also hold in a between-subjects analysis. In the PDC treatment, we observe a reporting share of 29.6% and 68.9% in the PDC+ treatment, which is significantly higher in PDC+ (see Figure 1.11) ($p\text{-}value = 0.0929$).[33] Linear regressions in Table A5 and Table A6 in the appendix also show that PDC+ significantly increases the share of leniency applications (see 1.7.6 in the appendix).

[32] An overview of the summary statistics of our other variables can be found in section 1.7.9.

[33] In order to exclude any possible 'Corona Pandemic effects', we conduct a robustness check which can be found in the appendix in Table A11.

Figure 1.11: Share report in STRUC: between-subjects comparison with PDC data from NOPDC-PDC and PDC+ data from PDC-PDC+.



Result 7 (Protection from damages for the first leniency applicant) Compared to PDC, there are more leniency applications in PDC+.

1.6 Conclusion

Private damage claims, introduced into European law through Directive 2014/104/EU (European Commission, 2014), are controversially discussed. This is especially the case when it comes to the adverse effects private damages may inflict on the well-established and successful tool of leniency. Leniency applicants' fines are waived or reduced, but their damage claim payments are not reduced, at least not completely, or they are capped only to a certain degree. Private enforcement may therefore decrease incentives to apply for leniency and may result in more stable cartels.

Our work contributes to the literature in two ways. The main goal of our paper is to provide a first quantification of the trade-off between leniency and private damage claims based on the current EU legislation in an experiment. Our design builds on the literature on leniency experiments (Apesteguia et al., 2007; Bigoni et al., 2012; Dijkstra et al., 2020; Hinloopen and Soetevent, 2008). We analyze a repeated cartel game where firms can discuss prices and may later apply for leniency. We extend the literature by allowing for private damages when a cartel is uncovered. Our second contribution is that we vary the form of communication by analyzing structured price announcements vs. unrestricted chat.

The results are as follows. First, we show that the propensity for cartel formation decreases as private enforcement is introduced. Second, when private damage claims exist, subjects tend to apply for leniency less often. Third, the implementation of damage claims has a stabilizing effect on cartels. Fourth, overall there

are fewer stable cartels with private damage claims. Fifth, we find ambiguous results regarding consumer surplus depending on the type of communication. Private enforcement decreases prices in a structured communication treatment yielding a rise in consumer surplus, whereas prices seem to increase when subjects are not restricted in communication, implying a decrease in consumer welfare. Sixth, chat-type communication not only lowers the incentives for leniency applications, it also increases cartel stability. Our take on the new instrument is mixed. Overall, cartel prevalence is lower with private damages, suggesting a beneficial impact. However, private damage claims seem to negatively affect leniency application rates and cartel stability.

The ambiguous results suggest a careful reconsideration of the tool of private enforcement. Buccirossi et al. (2020) suggest that improved protection from damages for whistleblowers may avoid the negative impact private damages have on leniency.[34] In an extension of our main treatments we show that, when leniency applicants are additionally protected from private damages, firms report cartels more often. So our data confirm the proposal. The policy implication of our study is, thus, that cartel law should probably be amended by increasing the protection from damages of the first leniency applicant to facilitate private action on other cartel members. This is likely to improve both cartel destabilization through stronger leniency-induced temptation to deviate and victim compensation.

One disclaimer is that we only analyze one set of parameters for the damages. Different magnitudes and likelihoods of the damages may lead to different results. Further experiments along this line are promising for future research. Another aspect of private enforcement that is not captured in our experimental design is that buyers have higher incentives to uncover cartels themselves when damage claims are possible. This is a likewise interesting question for future research. Moreover, the treatment of cumulated (and/or higher) fines instead of linear fines and cumulated private damage payments could provide a promising starting point for future research.

[34] Buccirossi et al. (2020) further point out the importance of the disclosure of information in the first leniency statement.

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1.7 Appendix

1.7.1 Variables and theoretical model of the experimental setup

To prove the statements in the main text for the experimental parameters and equilibrium realization of the variables, consider the parameters in Table A1. We analyze treatments NOPDC, PDC, and PDC+ in turn.

Table A1: Definition of variables and values realized in the experiment.

Definition	Variable	Numerical realization in experiment
Detection probability	ρ	0.15
Damage liability probability	σ	0.95
Discount factor & continuation probability	δ	0.8
Reporting cost	r	1
Marginal cost of production	c	100
Nash price	p_i^n	101
Collusive price	p_i^c	110
Deviation price in NOPDC & PDC	p_i^d	109
Deviation price in PDC+	p_i^d	108
Nash revenue	R_i^n	101/3
Collusive revenue	R_i^c	110/3
Deviation revenue	R_i^d	deviator: p_i^d, others: 0
Nash equilibrium profit	π_i^n	$(101-100)/3 = 1/3$
Collusive profit	π_i^c	$(110-100)/3 = 10/3$
Deviation profit	π_i^d	deviator: $(p_i^d - 100)$, others: 0
Fine under collusion	F_i^c	$0.1 \cdot R_i^c = 11/3$
Fine under deviation	F_i^d	deviator: $0.1 \cdot R_i^d$, others: 0
Fine under Nash pricing	F_i^n	$0.1 \cdot R_i^n = 10.1/3$
Number of firms that pay damage	N	$\in \{2, 3\}$
Damage payments collusion	D_i^c	$D^c/N = 0.6 \cdot (110-101)/N = 5.4/N$
Damage payments deviation	D_i^d	$D^d/N = 0.6 \cdot (p_i^d - 101)/N$
Damage payments Nash	D_i^n	$D^n/N = 0.6 \cdot (101-101)/N = 0$

NOPDC

Following (Bigoni et al., 2015, Appendix A.1), we assume that firms communicate once to establish successful collusion, but are able to collude tacitly following a detection by the competition authority. This implies that cartel firms risk being fined only once on the collusive path. (See footnote 22 above.)

With a probability of detection of ρ, a firm i has to pay a general fine F_i^j depending on outcome $j \in \{c, d, n\}$, with c for collusion, d for deviation and n for Nash. The factor δ discounts future payoffs. The net present value of the fine is obtained as follows: In each period, the cartel is either detected and has to pay F_i^j (happens with probability ρ), or the cartel is not detected (which happens with probability $1-\rho$) but might have to pay the fine in the next period (and accordingly this potential fine has to be discounted by δ). If the next period is reached, the same contingencies arise again, and so on. The stream of potential fine payments reads:

$$E(F_i^j) = \rho F_i^j + (1-\rho)\rho\delta F_i^j + (1-\rho)^2\rho\delta^2 F_i^j + (1-\rho)^3\rho\delta^3 F_i^j + \dots .$$

Multiplying both sides of the equation with $\delta(1-\rho)$, we have

$$\delta(1-\rho)E(F_i^j) = (1-\rho)\rho\delta F_i^j + (1-\rho)^2\rho\delta^2 F_i^j + (1-\rho)^3\rho\delta^3 F_i^j + \dots$$

and therefore we obtain

$$E(F_i^j) = \frac{\rho F_i^j}{1-\delta\,(1-\rho)}$$

as an expression for the discounted expected firm-specific fine $E(F_i^j)$.

The *participation constraint* in NOPDC states that colluding must be more profitable than competing (static Nash equilibrium)

$$\frac{\pi_i^c}{1-\delta} - E(F_i^c) \;\geq\; \frac{\pi_i^n}{1-\delta}.$$

Using the numerical values of the experiment, we find

$$14.948 \;\geq\; 1.667.$$

So the participation constraint is met for our experimental setup.

Before analyzing the incentive constraint, we need to analyze whether or not a deviator will report the cartel to the authorities. The deviator will undercut the

collusive price with a deviation price of $p_i^d = 109$. Reporting incurs a cost of r and no fine because of leniency. Not reporting saves the reporting cost but involves the risk of the cartel being fined due to detection. The authority may detect the cartel during the period of the deviation (resulting in fine F_i^d) or in a later period when firms play the Nash price as a punishment for the deviation (a cartel formally exists until a cartel member reports it or the cartel is uncovered by the cartel authority). Comparing reporting versus not reporting, we get

$$r = 1 \; < \; \rho F_i^d + \delta(1-\rho)E(F_i^n) = 2.708.$$

That is, a deviator will report.

The *incentive constraint* in NOPDC requires that there should be no incentive to deviate from collusion, given such deviation triggers a return to the static Nash equilibrium price. The incentive constraint accordingly reads

$$\frac{\pi_i^c}{1-\delta} - E(F_i^c) \;\; \geq \;\; \pi_i^d - r + \frac{\delta \pi_i^n}{1-\delta}.$$

Using the experimental parameters, we solve for the minimum discount factor required for collusion and obtain

$$\delta_{min}^{NOPDC} \geq 0.664.$$

This implies that colluding at the highest price of 110 is a subgame perfect Nash equilibrium in our setup. Alternatively, we can plug $\delta = 0.8$ into the incentive constraint and obtain

$$14.948 \;\; \geq \;\; 9.333$$

with the same implication.

PDC

In the treatment of PDC, the expected fine (has to be paid at most once) and deviation price ($p_i^d = 109$) remains the same. The expected private damages also have to be paid only once (when the cartel busts), but the analysis differs because damages are cumulated over time. The stream of discounted potential damage

payments is

$$\begin{aligned} E(D_i^j) &= \rho\sigma D_i^j + (1-\rho)\delta\rho\sigma 2D_i^j + (1-\rho)^2\delta^2\rho\sigma 3D_i^j + (1-\rho)^3\delta^3\rho\sigma 4D_i^j + ... \\ \delta(1-\rho)E(D_i^j) &= (1-\rho)\delta\rho\sigma D_i^j + (1-\rho)^2\delta^2\rho\sigma 2D_i^j + (1-\rho)^3\delta^3\rho\sigma 3D_i^j + ... \end{aligned}$$

Taking the difference $E(D_i^j) - \delta(1-\rho)E(D_i^j)$ yields

$$(1 - \delta(1-\rho))E(D_i^j) = \rho\sigma D_i^j + (1-\rho)\delta\rho\sigma D_i^j + (1-\rho)^2\delta^2\rho\sigma D_i^j + (1-\rho)^3\delta^3\rho\sigma D_i^j + ...$$

and therefore (proceeding as above with steady fines)

$$E(D_i^j) = \frac{\rho\sigma D_i^j}{(1-\delta(1-\rho))^2}$$

which, for the experimental parameters, becomes $E(D_i^j) = 1.3916 D_i^j$.

The *participation constraint* in PDC reads

$$\begin{aligned} \frac{\pi_i^c}{1-\delta} - E\left(F_i^c\right) - E\left(\frac{D^c}{3}\right) &\geq \frac{\pi_i^n}{1-\delta} \\ 12.443 &\geq 1.667. \end{aligned}$$

This participation constraint is also met for the experimental parameters, but it is more severe than the one above under NOPDC since it has less slack. We conclude that private damages deter more cartels.

We obtain the *incentive constraint* in PDC as follows. First, we have to compare the report vs. not report cases. A deviator who reports has to pay the reporting cost, r, and damages $\sigma D^d/3$ whereas a deviator who does not report faces the fine F_i^d and damages $\sigma D^d/3$, with detection probability ρ as well as the expected Nash fine $E(F_i^n)$. For our experimental parameters, we see that reporting is better than not reporting:

$$r + \sigma\frac{D^d}{3} = 2.52 < \rho F_i^d + \frac{\rho\sigma\frac{D^d}{3}}{(1-\delta(1-\rho))} + \delta(1-\rho)E(F_i^n) = 3.421.$$

The incentive constraint reads

$$\frac{\pi_i^c}{1-\delta} - E(F_i^c) - E\left(\frac{D^c}{3}\right) \geq \pi_i^d - r - \sigma\frac{D^d}{3} + \frac{\delta\pi_i^n}{1-\delta}.$$

Solving for the minimum discount factor required for collusion obtains

$$\delta_{min}^{PDC} \geq 0.655.$$

That is, $\delta_{min}^{NOPDC} > \delta_{min}^{PDC}$. Or, applying $\delta = 0.8$ to the incentive constraint, yields

$$12.443 \geq 7.813.$$

Comparing the minimum disocunt factors (0.664 vs. 0.655), we conclude that PDC makes collusion more stable than NOPDC.

The calculations of the incentives to report are based on the assumption that deviations take place in the first period. For NOPDC, the incentive to report does not change over time as the fine remains unchanged when reporting takes place in later periods. However, in PDC the incentive to report does change. It decreases with the duration of the cartel as damages are cumulated. The highest incentive to deviate is, nevertheless, present in period zero, so the repeated-game incentive constraint above is the one that is relevant when solving the overall game.

PDC+

In the PDC+ case the *participation constraint* remains the same

$$\frac{\pi^c}{1-\delta} - E(F_i^c) - E\left(\frac{D^c}{3}\right) \geq \frac{\pi^n}{1-\delta}$$

because fines and damages for successful collusion do not change compared to PDC (only reporting incentives change).

Again, we first examine the reporting incentives before turning to the incentive constraint. In previous treatments reporting only incurred costs (r) without generating any benefit for non-deviators. Accordingly, only the deviator reports. Instead, in PDC+ non-deviators may benefit from reporting for two reasons. First, by reporting, the non-deviators get the opportunity to eliminate their damage payments. Second, if the non-deviators do not report while the deviator reports, the non-deviators must co-fund the deviator's damage payment ($D^d/3$ increases to $D^d/2$). For our setup, though, it turns out (details available upon request) that deviators will not report, provided the deviator deviates with $p_i^d = 108$. By choosing $p_i^d = 108$ rather than $p_i^d = 109$ the deviator prevents reports by non-deviators and

also strongly maximizes the deviation profit. Thus, as in the other treatments, only the deviator reports in PDC+.

The incentive constraint becomes

$$\frac{\pi^c}{1-\delta} - E(F_i^c) - E\left(\frac{D^c}{3}\right) \quad \geq \quad \pi^d - r + \frac{\delta\pi^n}{1-\delta}.$$

In terms of the minimum discount factor required for collusion with a deviation price of 108, we get

$$\delta_{min}^{PDC+} \quad \geq \quad 0.682.$$

Taking the continuation probability of 0.8 into account yields

$$12.443 \quad \geq \quad 8.333.$$

As expected, PDC+ makes collusion more demanding than PDC and NOPDC. That is, PDC+ hinders collusion as intended by the new policy.

1.7.2 Definitions of variables

Table A2: Definition of the main variables.

Variable	Definition
Propensity to collude	Number of periods in which a subject chooses to enter the communication stage when a cartel does not already exist over the total number of periods in which a cartel does not exist.
Share cartel	Number of periods in which all three subjects of a group choose to enter the communication stage when a cartel does not already exist over the total number of periods in which a cartel does not exist.
Share report	Number of active reports of a cartel (click 'report button') by a group member over all periods that a cartel existed (newly formed cartel or liability from former periods). We exclude periods 10 and 20.
Cartel stability	The number of periods when a cartel was stable divided by the number of cartels of the group. A cartel is stable until it is reported or detected by the authority. We exclude periods 10 and 20.
Cartel prevalence	Number of periods in which a cartel exists (all three subjects of a group choose to enter the communication stage or are liable from former periods) over all periods of a treatment (10 periods).
Ask price non-cartel markets	Average ask price when a cartel does not exist.
Ask price cartel market	Average ask price when a cartel does exist (newly formed cartel or liability from former periods).
Ask price all markets	Average ask price in both non-cartel and cartelized markets.
Market price non-cartel markets	Lowest price of a group when a cartel does not exist.
Market price cartel market	Lowest price of a group when a cartel does exist (newly formed cartel or liability from former periods).
Market price all markets	Lowest price of a group in both non-cartel and cartelized markets.

1.7.3 Welfare effects and group dynamics over time

In section 1.4.4 we note that (competitive, cartelized, and all-market) prices decrease in STRUC and increase in CHAT. What could be the intuition for the contradicting price effects in CHAT and STRUC? Figures A1 and A2 yield evidence on the triggers of the contrary effects in prices. In short, they show that prices in STRUC are likely to be lower when private damage claims apply because there are fewer cartels and remaining cartels charge lower prices. Instead, in CHAT the counter-intuitive result is triggered by a hysteresis effect. In CHAT, subjects have the chance to coordinate their behavior even beyond a cartel breakdown and therefore stick to collusive prices until the game ends.

Figures A1 and A2 provide an overview of each group's cartel dynamics in STRUC and CHAT. The blue line plots the binary group dependent variable *collusion*, which is one for a cartelized and zero for a competitive market. The red line shows the course of the market price. The dots mark the cause of the cartel breakdown: The black dot indicates a breakdown due to reporting by at least one firm, the green dot characterizes a breakdown following a detection by the authority. Thus, the graph indicates a stable cartel when the blue line moves along its upper boundary without interruptions by dots. The dashed vertical line in each group-figure marks the switch from the NOPDC to the PDC treatment.

A comparison of Figures A1 and A2 shows that the contradicting effects has two main reasons:

i. Prices are overall more **stable** in PDC in CHAT (blue line) compared to STRUC and

ii. prices **remain** at a collusive level after cartel breakdown in CHAT (9 out of 11 groups keep prices at 110 after the cartel breakdown; see group 4, 5, 7, 8, 9, 10, 12, 13, 15), while prices are (by far) less stable in STRUC (exception: group 16). We call the price endurance in CHAT hysteresis.

Figure A1 displays the group dynamics in STRUC. From Figure A1, we infer that collusive prices are in most cases below the maximum (that is, 110) and drop even further in the PDC treatment, which leads to a lower mean price for collusive markets. Why does the price for all markets decrease? In combination with fewer cartels in PDC we suggest that cartels are more reluctant to set higher prices in PDC due to the risk of paying damage claims. We conclude that the introduction of

private damage claims has positive welfare effects in STRUC. From Figure A1, we can also learn that the mean market price for non-collusive markets drops in PDC because some groups manage to increase prices above the collusive level without cartelizing in the first few periods of the game (see e.g., groups 2 and 7).

Figure A2 shows clearly how the possibility to coordinate in a chat fosters collusion and price coordination compared to STRUC such that prices remain at the collusive level even after the collusion is busted. The following communication excerpts confirm our conjectures (translated from the original German):

- only communicate 1x. EVERYONE ALWAYS 110 NO reporing, more profit is not possible, otherwise we go back to 101 and no one earns anything (group 3, period 1)
- everyone 110 in every round (group 5, period 1)
- All right. Yes. Always 110, no further communications and no reports. (group 13, period 1)

Groups coordinate to *always* stick to the collusive price of 110 such that the collusive prices survives the cartel breakdown. It might seem counter-intuitive that hysteresis affects prices in all markets. There is an easy explanation for this: Once a cartel is busted (marked by a black or green dot) the market is identified as competitive even though prices stick to the collusive level. This causes the increase in overall prices and prices in competitive markets. As collusive prices are more volatile in NOPDC (group 2, 11), we observe slightly increasing prices in collusive markets. In contrast to STRUC, cartel prices in CHAT do not decrease after the introduction of private damage claims (e.g., because of the fear of higher damage payments following a detection). We may infer that in the CHAT treatment the introduction of private damage claims has a negative effect on welfare.

Figure A1: Collusive activity and market price by group for the treatment in STRUC.

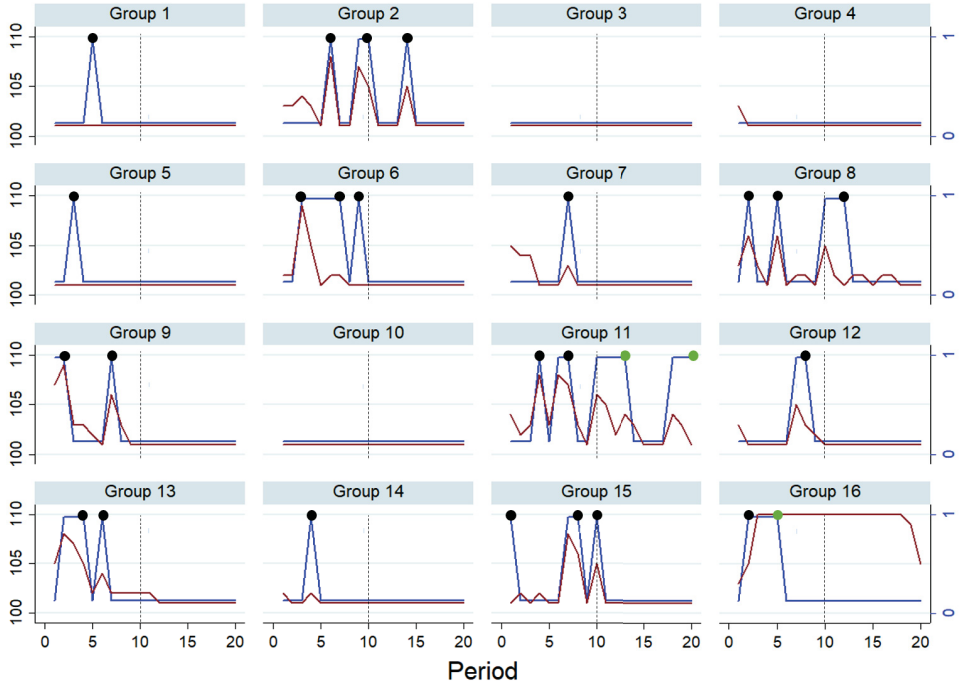
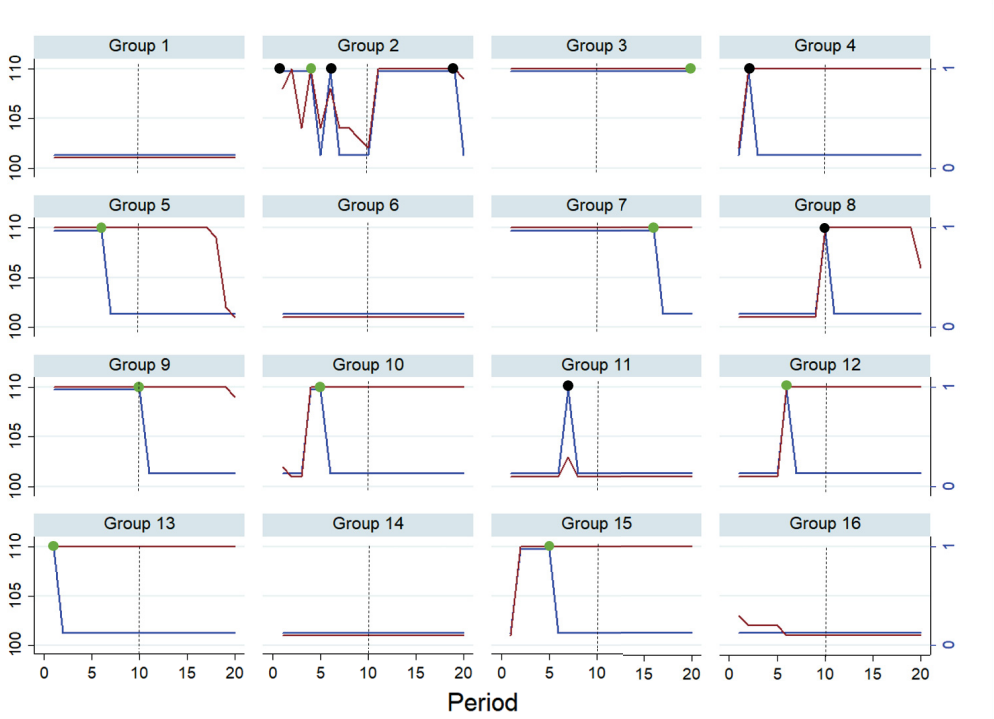


Figure A2: Collusive activity and market price by group in CHAT.

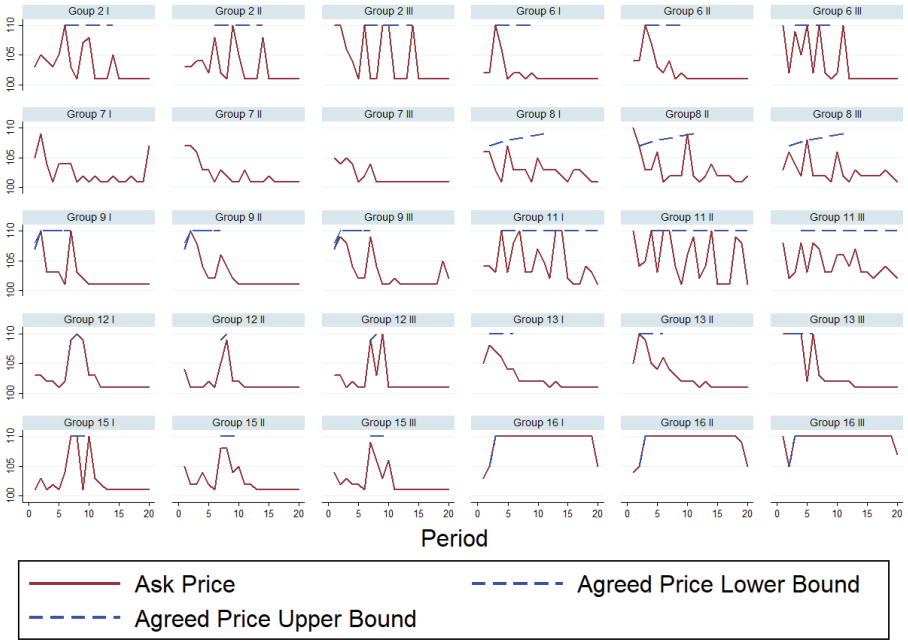


1.7.4 Deviations from agreed price

Figures A3 and A4 give an overview of the agreed-upon price during the communication stage and the (independently set) ask price. If subjects decide to discuss prices and agree on a single price, this is displayed by the blue line. In STRUC, price discussion can result in an interval of agreed prices. Figure A3 indicates this by the upper and lower bound of agreed prices (see e.g., group 9).

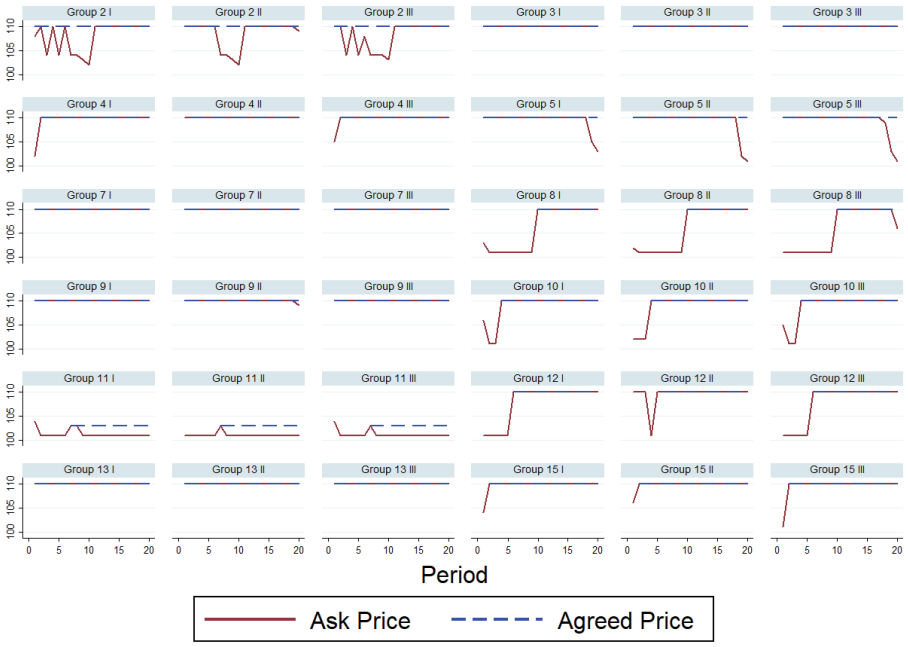
In Figure A4, we can observe a more stable price setting following the agreed price even in periods without a cartelized market in CHAT. Figure A3, which considers STRUC, provides an indication of lack of trust in collusive markets (this does not apply to group 16). For example, although group 2 in STRUC agrees on setting a price of 110, all three subjects never simultaneously set the agreed price as their individual ask price, instead they continuously undercut the agreed price. In contrast to that, in Figure A4 group 7 gives a perfect example of subjects sticking to the agreed price although prices were not discussed in this period. This behavior emphasizes our explanation of hysteresis regarding subjects not communicating but setting high prices.

Figure A3: Agreed price and set price by subject in STRUC.



Note: Groups that do not discuss prices or agreed on an interval of 101 to 110 are excluded.

Figure A4: Agreed price and set price by subject in CHAT.



Note: Groups that do not discuss prices or agreed on an interval of 101 to 110 are excluded.

1.7.5 Ask Prices

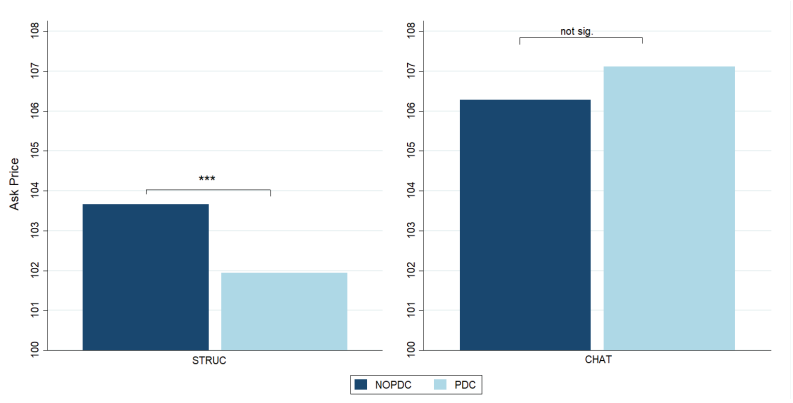
In this section we investigate the ask (or offer) price. The ask price is the price firms individually demand in stage 2. Figure A5 (and the bottom line in Table A3) illustrate the overall change in ask prices. We see the same pattern as in the above analysis of overall market prices. It shows for treatment STRUC an average overall ask price of 103.67 in NOPDC and 101.94 in PDC. This is statistically significantly different (STRUC: WMP, $p\text{-}value = 0.0011$). The difference in ask prices of NOPDC and PDC in CHAT is not statistically significant (CHAT: WMP, $p\text{-}value = 0.6033$).

Table A3: Ask price – averages per treatment.

	STRUC		CHAT	
	NOPDC	PDC	NOPDC	PDC
Ask price non-cartels	102.885 (1.899)	101.835 (2.125)	105.036 (3.727)	106.700 (4.351)
Ask price cartels	106.158 (2.537)	104.852 (2.727)	109.328 (2.016)	109.989 (0.019)
Ask price all markets	103.669 (2.062)	101.938 (2.162)	106.277 (3.803)	107.110 (4.203)

Standard deviations in parenthesis

Figure A5: The impact of PDC on ask prices in STRUC (left) and CHAT.



In Table A4 we estimate an ordinary least squares (OLS) model with the dependent variable *Askprice* (all markets). The results show that PDC have a negative effect on ask prices in the subsample of STRUC (Table A4, column 1), whereas PDC have a positive impact on ask prices in CHAT at a 15% level (Table A4, column 2).

Table A4: Ask price – linear regression.

	(1) Price	(2) Price	(3) Price	(4) Price	(5) Price
PDC	-1.731***	0.833^{+}	-3.542***	0.458	-0.442**
	(0.317)	(0.573)	(0.460)	(1.046)	(0.201)
constant	103.7***	106.3***	105.0***	106.1***	104.8***
	(0.492)	(0.916)	(0.417)	(0.748)	(0.308)
TIME FE	No	No	Yes	Yes	Yes
Sample STRUC	Yes	No	Yes	No	Yes
Sample CHAT	No	Yes	No	Yes	No
Sample pooled	No	No	No	No	Yes
N	960	960	960	960	1,860
R^2	0.084	0.010	0.116	0.014	0.108

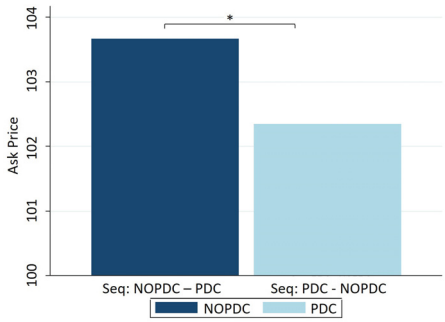
Standard errors in parentheses.

Sample pooled combines data from NOPDC-PDC and PDC-NOPDC.

$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A6 shows the analysis of the sequence of reverse order PDC-NOPDC in STRUC. The robustness check confirms the significantly lower ask prices in PDC (WMU, $p\text{-}value = 0.0785$).

Figure A6: Ask price in STRUC: between-subjects comparison with PDC data from treatment with reverse order (PDC-NOPDC).



1.7.6 Effect of PDC+ on share to report

Table A5 reports a linear regression of PDC+ on the individual decision to report a cartel in a within-subjects comparison. PDC+ has a significant positive effect on the share of cartel members applying for leniency.

Table A5: Share report within subject comparison from the treatment PDC-PDC+ linear regression.

	(1) Report	(2) Report
PDC+	0.222*	0.778***
	(0.126)	(0.141)
constant	0.492***	0.222^{+}
	(0.0756)	(0.141)
Time FE E [Period 1-19, without 10]	No	Yes
Sample STRUC	Yes	Yes
Within Subject	Yes	Yes
N	84	84
R^2	0.037	0.148

Standard errors in parentheses

$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6 shows a linear regression of PDC+ on the share to report a cartel in a between-subjects comparison considering data of PDC from the treatment NOPDC-PDC and data of PDC+ from the treatment PDC-PDC+. Again, the share of applying for leniency significantly increases with PDC+. These specifications only take existing cartels into account for which reason the analyses are based on only a small number of observations.

Table A6: Share report between subject comparison with PDC data from NOPDC-PDC and PDC+ data from PDC-PDC+ linear regression.

	(1) Report	(2) Report
PDC+	0.581***	0.490***
	(0.192)	(0.143)
constant	0.133	0.0588
	(0.168)	(0.0440)
Time FE [Period 11-20]	No	Yes
Sample STURC	Yes	Yes
Between Subject	Yes	Yes
N	51	51
R^2	0.350	0.533

Standard errors in parentheses

$^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1.7.7 Within-subjects results reverse-order treatment (PDC-NOPDC)

For the robustness check of our main analysis we only use the PDC data from the session PDC-NOPDC (see chapter 1.4). This allows us to explore any potential order effects, because we only analyze the first 10 periods for both the NOPDC and PDC treatment. For the sake of completeness, Table A7 shows an overview of the summary statistics of our reverse-order treatment within subjects. There are no statistically significant differences between PDC and NOPDC in the within analysis of the reverse-order treatment.

Table A7: Summary statistics of the results in treatments PDC–NOPDC (STRUC).

	STRUC		Test
	NOPDC	PDC	p-value
Propensity to collude	0.573 (0.193)	0.555 (0.120)	0.7114
Share cartel	0.134 (0.222)	0.117 (0.137)	0.2264
Share report	0.383 (0.267)	0.451 (0.263)	0.5176
Cartel stability	1.611 (0.656)	1.333 (0.476)	–
Cartel prevalence	0.180 (0.283)	0.133 (0.150)	0.4956
Market price	101.2 (0.314)	101.527 (0.680)	0.0364

Average results per treatment. Standard deviations in parentheses.

1.7.8 Controlling for order effects – Between-subjects results across treatments

For the robustness check of our main analysis we only use the PDC data from the session PDC-NOPDC (see chapter 1.4). This allows us to explore any potential order effects, because we only analyze the first 10 periods for both the NOPDC and PDC treatment. For the sake of completeness, we also control for order effects within the same treatment under different time regimes. We analyze whether our main variables of interest are significantly different under each treatment, when this treatment is run in periods 1-10 rather than 11-20. We test this by conducting a Mann-Whitney U test (one per treatment) across the two samples. Tables A8 and A9 show an overview of the summary statistics of our between-subjects comparison across treatments. Lots of our main variables show no differences. This suggests that order effects are no major concern. Additionally, we formally control for possible order effects by using pooled data for our regressions in section 1.4 Tables 1.5 to 1.10. These results provide further evidence that there are no order effects.

Table A8: Summary statistics of the results across different time regimes NOPDC (STRUC).

	NOPDC		Test
	NOPDC -1st half	NOPDC 2nd half	p-value
Propensity to collude	0.619 (0.142)	0.573 (0.193)	0.4733
Share cartel	0.207 (0.153)	0.134 (0.222)	0.0847
Share report	0.462 (0.230)	0.383 (0.267)	0.5454
Cartel prevalence	0.238 (0.178)	0.180 (0.283)	0.1272
Market price	102.706 (2.009)	101.2 (0.314)	0.0059

Average results per treatment. Standard deviations in parentheses.

Table A9: Summary statistics of the results across different time regimes PDC (STRUC); PDC -1st half & PDC 2nd half .

	PDC		Test
	PDC -1st half	PDC 2nd half	p-value
Propensity to collude	0.555 (0.120)	0.394 (0.192)	0.017
Share cartel	0.117 (0.137)	0.019 (0.054)	0.0124
Share report	0.451 (0.263)	0.296 (0.339)	0.4525
Cartel prevalence	0.133 (0.150)	0.063 (0.163)	0.0671
Market price	101.527 (0.680)	101.681 (2.095)	0.1533

Average results per treatment. Standard deviations in parentheses.

1.7.9 Within-subjects results for the main variables (PDC-PDC+)

For the sake of completeness, Table A10 shows an overview of the summary statistics of our main variables from our PDC-PDC+ treatment with an MWU within-subjects test. The share of cartelized markets, cartel prevalence as well as the market price are significantly smaller under the PDC+ regime.

Table A10: Summary statistics of the results across different time regimes PDC (STRUC); PDC & PDC+.

	STRUC		Test
	PDC	PDC+	p-value
Propensity to collude	0.501 (0.243)	0.467 (0.243)	0.4688
Share cartel	0.132 (0.130)	0.044 (0.081)	0.0261
Share report	0.430 (0.224)	0.6889 (0.241)	0.1655
Cartel prevalence	0.138 (0.131)	0.044 (0.081)	0.0239
Market price	103.456 (3.122)	102.313 (2.826)	0.0041

Average results per treatment. Standard deviations in parentheses.

In order to control for any possible order effects we use the PDC data from the session NOPDC-PDC and the PDC+ data from the session PDC-PDC+ (see chapter 1.5 Figure 1.11). In this approach, we only analyze the last 10 periods for both the PDC and PDC+ treatment. Due to special circumstances arising from the 'Corona pandemic' we control whether the sessions before and during the 'Corona pandemic' are significantly different from each other. We test this by conducting a Mann-Whitney U test between the PDC treatment from the session PDC-NOPDC and the PDC treatment from the session PDC-PDC+. Table A11 shows an overview of the summary statistics of our between-subjects comparison across treatments. For all our main variables of interest there is no statistically significant difference leading to a conclusion of no 'Corona pandemic effects'. This result supports our between-subjects comparison in Figure 1.11.

Table A11: Summary statistics of the results across different time regimes PDC (STRUC); PDC 1st half (PDC-NOPDC) & PDC 1st half (PDC-PDC+).

	STRUC		Test
	PDC 1st half (PDC-NOPDC)	PDC 1st half (PDC-PDC+)	p-value
Propensity to collude	0.555 (0.120)	0.501 (0.243)	0.8896
Share cartel	0.117 (0.137)	0.132 (0.130)	0.7271
Share report	0.451 (0.263)	0.430 (0.224)	0.8327
Cartel prevalence	0.133 (0.150)	0.138 (0.131)	0.9016
Market price	101.527 (0.680)	103.456 (3.122)	0.0400

Average results per treatment. Standard deviations in parentheses.

2

Fiscal and Economic Effects of Local Austerity

Joint work with Andreas Lichter, Max Löffler

2.1 Introduction

Over the past centuries, many advanced countries have seen rising debt to GDP ratios at different levels of government, particularly due to a secular rise in spending outpacing tax revenue collection (Yared, 2019). While the temporary take-up of debt may act as a suitable fiscal stimulus to counteract short-run economic shocks and smooth public expenditures over time, most economists acknowledge that excessively high debt levels exhibit sizable negative effects on economic growth in the long run (see, e.g., Reinhart et al., 2012; Hébert and Schreger, 2017; or Romer and Romer, 2018). Countries across the world have increasingly implemented policy measures to curtail or reduce debt at different levels of government. For example, the prevalence of fiscal rules that set limits to government spending in order to curb debt accumulation at either the federal, regional, or local level has increased by an order of magnitude over past decades (Lledó et al., 2017). Alesina et al. (2019a) further identify around 200 austerity policies in 16 selected OECD countries from 1978 to 2014 that aim at the (drastic) reduction of governmental budget deficits by means of spending cuts and/or tax increases. However, to date, the costs and benefits of such fiscal consolidation policies remain heavily discussed among politicians and economists alike, comprising debates about the policies' overall economic impact, their political consequences, as well as their optimal design (see, e.g., Blyth, 2013; Wyplosz, 2013; and Alesina et al., 2019b).

In this paper, we analyze the fiscal and economic consequences of a large-scale fiscal consolidation program in North Rhine-Westphalia, Germany's most populous state. In 2011, the state government enacted the so-called *Stärkungspakt Stadtfinanzen*. The policy imposed substantial austerity measures on targeted municipalities: local budgets had to be consolidated six (ten) years after the program's imposition (net of transfers). In turn, targeted cities received sizable financial support via additional intergovernmental grants. Consolidation plans were tightly monitored and had to be approved by fiscal oversight committees. However, treated municipalities had large discretion about their preferred way of how to balance local budgets beyond the earmarked intergovernmental grants and mandatory expenditures: through local spending cuts, tax increases, or a combination of the two.

We use this set-up to analyze three different questions: Did the local austerity measure meet its ultimate goal, i.e., did municipalities consolidate their budget? If yes, how did they achieve consolidation beyond and above the additional grants provided? And, last, did spending cuts or tax increases induce general equilibrium responses among local residents or firms? To provide causal evidence on these questions, we exploit political discretion in the composition of the group of municipalities treated by the policy. While all major parties and advisors from various disciplines agreed on the urgent need for fiscal consolidation in financially distressed municipalities, substantial disagreement prevailed about the specific set of municipalities to target. Ultimately, the acting government coalition decided to link program participation to municipalities' level of equity. However, and in contrast to the case of businesses, the level of equity lacks "any predictive power for [German] municipalities' true financial scope of action" (Mühlenkamp and Magin, 2010). Government advisors and opposition parties indeed advocated alternative criteria determining treatment; criteria that would have led to a different and/or broader group of municipalities subject to the policy (see, e.g., Junkernheinrich et al., 2011; Landtag Nordrhein-Westfalen, 2011a). We exploit this political discretion in the actual composition of the treatment group for identification and follow the empirical mindset of Greenstone et al. (2010), Busso et al. (2013), and Kline and Moretti (2014) by comparing treated municipalities to municipalities that were equally financially stricken but ultimately not subject to the policy. Common pre-trends in observable characteristics between treatment and control group support the validity of the dynamic difference-in-differences design.

Based on this set-up, the study proceeds in three steps. First, we show that the program helped treated municipalities to consolidate their budgets. Five to seven years after treatment, municipalities improved their fiscal balance by around 230 EUR per capita on average—and by around 170 EUR net of the policy's earmarked transfers. Moreover, the policy increased treated municipalities' chance of reporting an annual surplus by roughly 40 percentage points. The estimated effects on fiscal consolidation are robust to various specifications of the difference-in-differences design and remarkably homogeneous across the distribution of treated municipalities. Second, and in contrast to the homogeneous effects on fiscal budgets, we find substantial heterogeneity in municipalities' implemented strategies of fiscal consolidation. Smaller municipalities with less than 20,000

inhabitants primarily consolidated their budgets by setting higher local taxes on property and cutting spending on local public services. In contrast, larger municipalities (with 20,000 inhabitants or more) implemented no spending cuts on residents' local amenities but shifted part of the burden of consolidation on firms through higher local business tax rates and lower business subsidies. Third, we show that the consolidation-induced financial burden on firms had no negative effects for the local economy. Both the municipalities' business tax base as well as the number of firms remained unchanged in response to treated municipalities' fiscal consolidation efforts. However, we find economically sizable and statistically significant negative effects of fiscal consolidation on population levels and house prices for smaller municipalities. These results provide strong evidence of general equilibrium and capitalization effects in the spirit of Rosen (1979); Roback (1982) models: if local amenities decrease or taxes borne by residents increase, population levels are expected to decline, and so are equilibrium house prices.

Overall, the results of this paper add to two distinct strands of the literature. First, we provide new insights about the consequences of fiscal consolidation programs. Until today, most evidence on the financial, economic, and political effects of fiscal rules and austerity policies is based on cross-country comparisons, which gives rise to various endogeneity concerns (see, e.g., Perotti, 2013; Heinemann et al., 2018; or Arias and Stasavage, 2019; for overviews).[1] Notable exceptions are studies by Grembi et al. (2016), Christofzik and Kessing (2018), and Carreri and Martinez (2021), who exploit within-country variation in municipalities' exposure to fiscal rules in Italy, Germany, and Colombia, respectively. The three studies provide consistent evidence that fiscal rules and oversight matter. If municipalities' scope for financial actions is eased, they generate larger deficits either because of lower local taxes (Grembi et al., 2016) or higher spending (Christofzik and Kessing, 2018). In turn, tighter fiscal rules decrease local governments' deficits; notably, without inducing meaningful reductions in local public good provision in the case of Colombia (Carreri and Martinez, 2021). Our paper speaks to these papers by providing within-country evidence on the effects of a large austerity policy that

[1] To lessen concerns of endogeneity, recent cross-country studies by Romer and Romer (2010), Guajardo et al. (2014), Jordà and Taylor (2016) and Alesina et al. (2019a) limit their focus to policies of fiscal consolidation that are detached from business cycle considerations. While this "narrative approach" limits concerns of endogenous policy implementation, threats of unobserved confounders at the national level as a source of bias remain.

imposed strict consolidation requirements but allowed municipalities to choose their preferred strategy for reaching this goal. This enables us to study municipalities' consolidation plan choices and their fiscal and broader economic consequences in a joint framework. Among others, this allows us generating insights about the consequences of spending- versus tax-based fiscal consolidation policies. Alesina et al. (2019a) argue that spending-based austerity polices are less harmful than tax-based policies, reducing GDP to a lesser and shorter extent. On the other hand, Fetzer (2019) and Galofré-Vilà et al. (2021) provide within-country evidence that drastic cuts in government spending can have stark political consequences, with locally-varying impacts of government-induced austerity explaining varying vote shares for the UK Independence Party (UKIP) between 2010–2015 and the National Socialist German Workers' Party (NSDAP) in Germany in the early 1930s, respectively. We add to this evidence by investigating the economic consequences of tax- versus spending-based consolidation. Our results indicate that the economic consequences of such reforms depend on municipalities' strategies of consolidation effort. We find little evidence that the consolidation-induced financial burden on firms has negative economic consequences for larger municipalities. In contrast, we show reduced local spending and a generally higher burden of consolidation on local residents causes population levels and house prices to fall.

These latter results are line with the predictions of standard spatial equilibrium models in the spirit of Rosen-Roback (Rosen, 1979; Roback, 1982). A decline in local amenities and/or an increase in taxes borne by local residents is expected to reduce local population levels and decrease house prices because these areas become less attractive places to live (see, e.g., Moretti, 2011; Albouy, 2016; Agrawal et al., 2022). In theory, the same mechanism should also hold true for firms: facing higher local taxes on profits and fewer subsidies in response to municipalities' fiscal consolidation, we may expect firms in larger municipalities to shrink or even move away. Recent studies by Serrato and Zidar (2016) and Giroud and Rauh (2019) for the U.S. indeed demonstrate that higher state-level taxes reduce employment and induce firm mobility. We hypothesize that our small and insignificant effects may be reconciled with the fact that the policy-induced burden on firms is too low to offset the benefits from location-specific production amenities that exist in larger municipalities, e.g., due to sector-specific concentration, agglomeration effects, or the supply of high-skilled labor (Diamond, 2016; Serrato and Zidar, 2016).

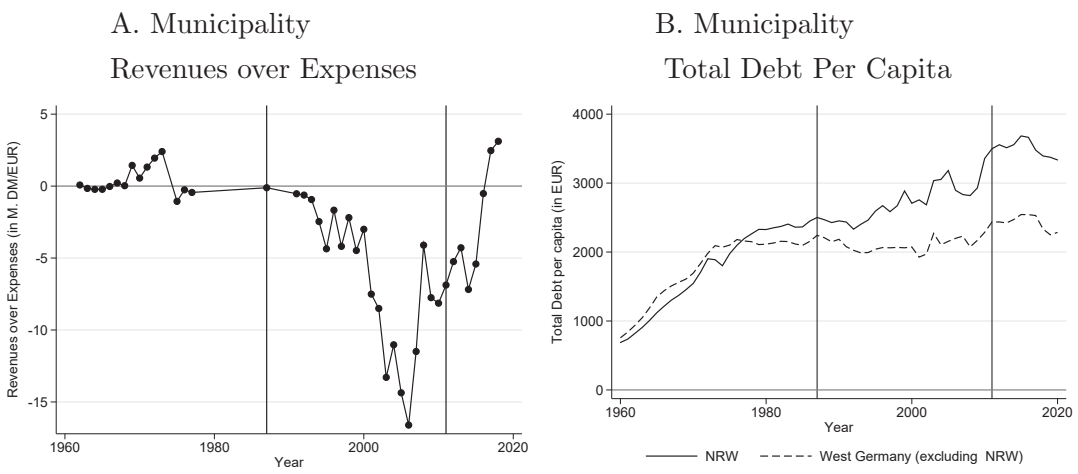
The remainder of the paper is structured as follows. In Section 2.2, we discuss the underlying institutional setting and the reform of interest, as well as describe the data assembled. In Section 2.3, we discuss our proposed difference-in-differences design, the strategy's underlying assumptions as well as potential threats to identification. In Section 2.4, we present our results. We first investigate the overall effect of the policy on fiscal consolidation, before analyzing municipalities' strategies of balancing budgets. In a third step, we analyze how different consolidation strategies may have caused broader (and unintended) economic consequences. Section 2.5 concludes.

2.2 Institutional Background and Data

2.2.1 Municipal Finances in North Rhine-Westphalia

We study a fiscal policy reform in Germany's most populous state, North Rhine-Westphalia (NRW), which is home to approximately 18 million inhabitants. NRW accounts for about one fifth of Germany's total population and GDP, respectively, and has long served as the economic powerhouse of the country. After World War II, the resident coal and steel industry was a key driver of Germany's economic miracle during the 1950s and 60s and transformed the state into one of the most important economic areas in Europe (Hassink, 1993; Goch, 2002). However, from the late 1960s onward, the rise of international competitors and lacking product demand caused many steel and coal regions to experience substantial unemployment and structural change (Hospers, 2004). Industrial transformation was of quite different success across NRW. Whereas some regions, such as the Rhineland in the south and Ostwestfalen-Lippe in the north-east of the state, today host a diverse set of industries, see little unemployment and rising population figures, others face continuing reductions in local employment and population—causing NRW to perform worse than many other West German states in terms of unemployment or GDP per capita (see Appendix Figure B1).

Figure 2.1: Municipalities' Fiscal Strain over Time



Notes: Figure A plots the evolution of municipalities' difference between revenues and expenditures in NRW over the period from 1962–2020. Figure B shows the evolution of municipalities' total debt per capita for NRW and West Germany (excluding NRW) from 1960–2020. All variables are adjusted for inflation. The vertical lines indicate the years of the introduction of the *HSK* in 1987 and of the *Stärkungspakt* in 2011, respectively. Sources: Federal Statistical Office and Statistical Office of NRW.

These adverse economic developments have long put considerable fiscal strain on all different levels of government in NRW; and in particular on municipal councils, Germany's lowest administrative unit of government. German municipalities have long enjoyed substantial rights of self-governance, both in terms of revenue collection and spending. Among others, municipalities levy taxes on local firm profits (*Gewerbesteuer*) and property (*Grundsteuer*), while fulfilling obligatory duties—such as the provision of child care or waste disposal—and providing non-obligatory amenities to the local population, e.g., the operation and preservation of theaters or sport facilities. Hereby, municipalities' actions are directed by the postulate of budget balance: in every year, revenues need to offset expenses. Municipalities may only take up debt in order to finance investments and to secure liquidity in the short run. However, with local spending (duties) outpacing revenues (see Panel A of Figure 2.1), municipalities' debt has been steadily risen since the late 1960s (see Panel B of the respective figure); in particular due to surging levels of short-term loans (see Appendix Figure B2).[2]

[2] The notable increase in local spending can be linked to the transfer of tasks from higher levels of government. Examples include municipalities' duty to cover the costs of accommodation for welfare recipients (imposed in 2005), and obligation to provide daycare for children under the age of three (imposed by the national government in 2008); duties that came without appropriate

Fiscal Oversight Policies. To ease municipalities' rising fiscal strain and attain financially-sustainable local budgets, a system of fiscal oversight was first implemented in NRW in 1987. The *Haushaltssicherungskonzept* (henceforth: *HSK*), which is still in place today, comes into effect once annual expenses surpass revenues and reserves need to be depleted in a given magnitude in order to balance budgets.[3] Municipalities subject to the *HSK* are required to align their budget with higher levels of government and have to propose strategies to achieve a balanced budget in future years. In case of non-compliance or the presentation of non-sufficient consolidation plans, the respective subordinate authorities are allowed to block a municipality's budget for non-obligatory expenditures. The number of municipalities subject to the *HSK* increased quickly after its introduction, from eight out of 396 municipalities in NRW in 1987 to 135 in 1997, peaked in 2005, and remained rather stable thereafter at a level of around 100–130 covered municipalities per year (Junkernheinrich et al., 2011).

However, the implemented measure of fiscal oversight had no lasting effects on municipalities' financial strain (Geißler, 2009). As shown by Figure 2.1, the gap between municipalities' overall expenses and revenues, and hence the increase in municipal debt, became particularly sizable between the mid-1990s and late-2000s and led to persistent calls for additional measures targeting municipalities' tight fiscal situation (see, e.g., Articus, 2010; Junkernheinrich, 2010; Rosenfeld, 2010). These calls were echoed by all major parties in NRW. However, proposed solutions first centered around strategies that aimed at the rise and stabilization of municipalities' tax revenues (see, e.g., the coalition agreement of the 2005–2010 state government).

The Stärkungspakt as an Explicit Austerity Program. In December 2011, the succeeding coalition government eventually implemented an explicit consolidation policy to tackle municipalities' growing budget deficits: the *Stärkungspakt Stadtfinanzen*. The policy provided direct financial means for the group of targeted municipalities over a ten year period (2011–2020) but also imposed strict consolidation requirements: municipalities had to reach budget balance six (ten)

additional funds from higher levels of government (see, e.g., Holler et al., 2017).

[3] In detail, municipalities have to provide a consolidation plan according to the *HSK* to higher levels of government if the budget (plan) foresees a reduction of the general reserve by more than 25% (5%) within one financial year (in two consecutive years), or if the municipality depletes the general reserve within the medium-term budget planning period (§ 76 GO NRW).

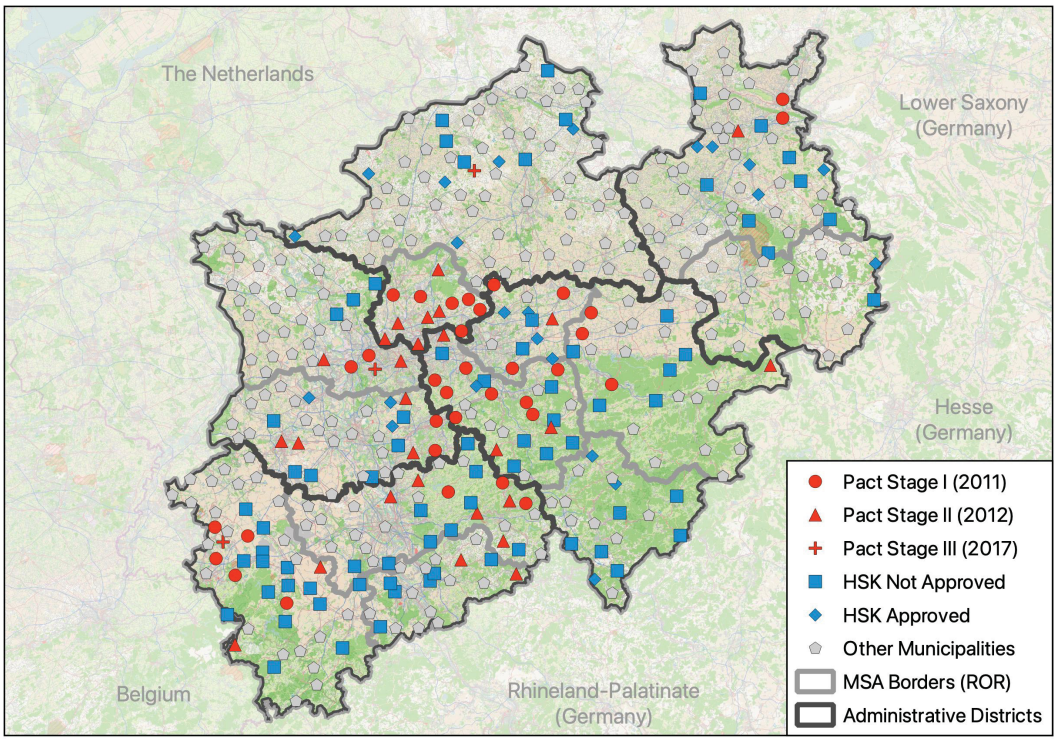
years after the start of the program (net of the intergovernmental grants provided). Compliance with the stipulated consolidation efforts was strictly enforced. Municipalities that did not comply were deprived of their self-government rights, and decisions about local spending and revenues were transferred to higher levels of government. However, while the expected scope and timeline of consolidation was tightly prescribed, treated municipalities had large discretion about their preferred way of balancing budgets beyond and above the earmarked transfers: through spending cuts, tax increases, or a combination of the two.

Municipalities entered the consolidation scheme in two phases. First, in 2011, those municipalities that reported negative equity in 2010 or—based on their 2010 budget plan—were predicted to reach negative equity by 2013 were assigned to the austerity program. Participation was obligatory and non-deferrable. Second, those municipalities that were foreseen to report negative equity between 2014–2016 were allowed to join the consolidations scheme in 2012. Despite its voluntary nature, all eligible municipalities entered the scheme.[4]

Figure 2.2 plots the spatial distribution of municipalities across NRW that were targeted by the policy. In total, 61 municipalities entered the scheme in the two respective phases (34 in stage I, 27 in stage II). The map illustrates that participating municipalities are spread out across the state's five administrative districts (*Regierungsbezirke*, equivalent to NUTS-II regions). Yet, there is also some amount of spatial clustering. The metropolitan statistical area "Emscher-Lippe", the northern part of the Ruhr area, comprises twelve of the 61 treated municipalities, the metropolitan area "Bochum/Hagen" eleven. However, there are treated municipalities in nine of the state's 13 metropolitan statistical areas (henceforth: MSAs, *Raumordnungsregionen*). In our baseline specification of the difference-in-differences design we limit identification to within-MSA comparisons (see Section 4.3 below).

[4] In 2017, the *Stärkungspakt* was amended and three additional municipalities entered a third phase of the program. We drop these municipalities from our empirical analysis as there is hardly any post-treatment data.

Figure 2.2: Spatial Distribution of Treatment



Notes: This map displays the 396 municipalities in the German state North Rhine-Westphalia. Colored in red are those municipalities that participated in the *Stärkungspakt* at different stages (as indicated by the different symbols). Municipalities colored in blue indicate municipalities that were subject to the *HSK* in 2010, but not subject to treatment. The map further highlights the borders of the five administrative districts and 13 metropolitan statistical areas in NRW.
Maps: © GeoBasis-DE / BKG 2015 and OpenStreetMap contributors.

2.2.2 Data

For the purpose of our analysis, we combine information from various data sources. Foremost, we make use of detailed information on municipalities' annual financial accounts that are harmonized and provided by NRW's statistical office (*IT.NRW*). Among others, the accounts comprise information on municipalities' varying sources of revenues, types of expenditures, their short- and long-run debt, as well as their annual surplus and level of equity. The latter information has been provided since 2009, when German municipalities had to alter their accounting systems from cash-based (*Kameralistik*) to accrual accounting (*Doppik*). With this change in accounting rules, municipalities had to create an opening balance and derive their respective level of equity; an institutional feature we exploit for identification below. However, the change in accounting rules also limits the comparability of

municipalities' revenues and expenses before and after 2009 to some extent. Prior to 2009, municipal revenues comprised the amount of cash that was received in a given year from a given source (*Einnahmen*), whereas they comprise all earned annual revenues since 2009, irrespective of when the respective cash was actually received (*Erträge*). The same methodological difference holds true for municipalities' expenses. To this end, we have to limit parts of our empirical analysis to the period from 2009–2018 due to the lack of consistent data. However, we extend the pre-reform period whenever possible in order to comprehensively test for common trends in outcome variables between treatment and control group.

Focusing on 2010, i.e., the year before policy implementation, Panel A of Appendix Table B1 shows that municipalities' revenues primarily comprise three key pillars: (i) local taxes, (ii) intergovernmental grants, and (iii) fees. With regard to local taxes, constituting around 27.1% of municipalities' total revenues, the local business tax (*Gewerbesteuer*) and the local property tax (*Grundsteuer B*) constitute the most important sources of revenue, accounting for 19.7% and 6.6% of municipalities' total revenues on average. Intergovernmental grants account for around 44.7% of all revenues and primarily stem from higher levels of government & fiscal equalization schemes (17.5%) or the apportionment of revenues from the personal income and value added tax (17.7%); taxes levied at the national level and partially allocated to municipalities according to fixed formulas. Around 11.1% of municipalities' revenues further stem from fees and levies, e.g., from administrative or usage charges. The remaining 17.1% stem from other sources, such as the gains from privatization or profits of municipal companies.

Panel B of Appendix Table B1 in turn summarizes municipalities' spending along different items. It becomes apparent that—despite expenditures on tax remittances and interest payments— municipalities spend most on the provision of public local services (around 21%). Municipal spending on social security as well as local infrastructure (e.g., roads, public transport, or health care facilities) account for approximately 12% and 14%, respectively. Local expenditures on education make up for around 8%, spending on public goods for around 6%, and business subsidies for around 1.5% of municipal expenditures on average. In the empirical analysis, we investigate the presence of policy-induced spending cuts in these categories. Panel C of the respective Table summarizes municipalities' fiscal balance as of 2010.

The average municipality reported higher expenditures than revenues - and only 12% of all municipalities in NRW reported a balanced or positive budget in that year.

To study the broader economic consequences of fiscal consolidation, we further draw upon detailed annual information on municipalities' number of firms, business tax base (as a proxy of local GDP), population, and house prices. Annual information on municipalities' total number of firms come from the Research Institute of the Federal Employment Agency (*IAB*), information on municipalities' business tax base from *IT.NRW*. Annual population figures stem from the latter source, too. Annual average house prices for each municipality base on the *RWI-GEO-RED: Real Estate Data* dataset provided by *FDZ Ruhr*, which contains information on all housing and flat sale advertisements on Germany's largest online real estate portal, *ImmoScout24*. Panel D of Appendix Table B1 provides the corresponding descriptive statistics.

2.3 Empirical Approach

2.3.1 Estimation Model

To derive causal effects, we set up a dynamic difference-in-differences design that compares the pre- to post-reform evolution of fiscal and economic outcomes in municipalities subject to the policy with the respective evolution of outcomes in municipalities unaffected by the austerity program. Econometrically, we regress a given outcome variable y—say a measure of fiscal balance, different sources of revenues, expenditure types, or indicators of regional economic performance—of municipality m in year t on leads and lags of dummy variable indicating program participation:

$$y_{mt} = \sum_{k} \beta_k Pact_m^k + \lambda_m + \rho_{zt} + \theta_{ct} + \varepsilon_{mt}. \tag{2.1}$$

The model further includes three different sets of fixed effects to account for unobserved confounders. First, we include municipality fixed effects (λ_m) that control for unobserved time-invariant confounders at the level of treatment and limit identification to within-unit changes over time to. Second, we absorb time-varying common shocks at the broader regional level by including flexible time trends (ρ_{zt}) for each metropolitan statistical area (MSA). Ultimately, this limits identification to the

comparison of changes in outcomes in treated and untreated municipalities from the same MSA.[5] We show below that the comparison of geographically-close and economically-connected municipalities balances treatment and control group considerably in terms of observable pre-treatment characteristics. Third, we allow for differential trends among municipalities subordinate to a given county (*kreisangehörige Gemeinden*) and municipalities constituting an independent county themselves (*kreisfreie Städte*), denoted by θ_{ct}. Term ϵ_{mt} denotes the error term. We cluster standard errors at the county level in our baseline specification.

Heterogeneous Treatment Effects. Treated municipalities joined the consolidation program at two distinct points in time: either in 2011 or 2012, subject to their (predicted) level of equity (see Section 2.2.1 for details). In our baseline analysis, we ignore these different years of program entry because treatment assignment happened at the same time for both groups. Hence, even if some municipalities received the program's transfers and had to comply with the prescribed consolidation efforts one year later than others, they might have anticipated treatment and, therefore, changed their local policies right after assignment to the scheme. However, we acknowledge that this approach may blur the short-run effect pattern and may mask heterogeneity in treatment responses across the two groups. Therefore, we also (i) estimate treatment effects for both groups separately and (ii) apply estimators that are robust to heterogeneous treatment effects in two-way fixed effects models (see, e.g., Sun and Abraham, 2020; Borusyak et al., 2021).

2.3.2 Identification

To discern reform effects from possible common shocks unrelated to treatment, we compare pre- to post-reform changes in outcomes in our treatment group to the corresponding changes in municipalities not subject to the fiscal consolidation scheme. To make treatment and control group as comparable as possible prior to treatment, our control group comprises those municipalities that were also subject to considerable fiscal strain in 2010—i.e, subject to fiscal oversight and insufficient voluntary consolidation efforts just as the control group—but did not enter the program because their level of equity was predicted to remain positive until at

[5] In robustness checks, we show that our estimates remain qualitatively unchanged when allowing for time-varying local shocks at broader and finer regional levels.

least 2017.[6]

Crucial for identification, the chosen cutoff determining treatment lacked any economic rationale. Neither does the level of municipalities' equity provide "predictive power for municipalities' true financial scope of action" (Mühlenkamp and Magin, 2010), nor does it affect municipalities' financing costs. Every German municipality faces the same default risk as the national government (European Banking Authority, 2021), which is due to the implicit bail-out of municipalities by higher levels of government that leaves all municipalities with the same financing conditions irrespective of their level of debt (Fritze, 2019). Moreover, (reaching) negative equity does not trigger any political or fiscal consequences and affects neither municipalities' scope of decision-making nor incoming or outgoing transfers. Government advisors and opposition parties advocated for alternative measures determining treatment that would have lead to different and/or broader compositions of the group of municipalities subject to fiscal consolidation (see, e.g., Junkernheinrich et al., 2011; Landtag Nordrhein-Westfalen, 2011a). Following the empirical mindset of Greenstone et al. (2010), Busso et al. (2013) and Kline and Moretti (2014), we thus base identification on the quasi-random assignment of treatment among equally financially-distressed municipalities due to political discretion in the actual composition of the treatment group.

To corroborate the plausibility of the proposed empirical design, we systematically compare municipalities in treatment and control group with regard to trends in observable fiscal and broader economic variables before reform implementation. We do this by regressing changes in fiscal measures, revenues and expenses, as well as broader economic outcomes over the period from 2000–2008 on an indicator variable for program participation. This method follows closely the test for covariate smoothness suggested by Lee and Lemieux (2010). Given the number of outcome variables under study, we expect variables to indicate significant differences by random chance. To further test this possibility, we combine the single hypothesis tests into one joint test for significant pre-trend difference between treated and untreated municipalities and test the hypothesis that all pre-trend differences are

[6] Note that all municipalities that entered the *Stärkungspakt* were subject to the *HSK* and had a non-approved consolidation plan in 2010. The only exception is Bönen, whose consolidation plan was approved.

zero.

Table 2.1 shows step-by-step how our proposed identification strategy is able to balance pre-reform trends in treated and untreated municipalities. In column (1), we first compare treated municipalities' evolution of outcomes prior to program implementation to all other municipalities in NRW. We find several economically sizable and statistically significant differences. Among others, treated municipalities saw adverse developments in tax revenues, working-age population levels and local economic performance, e.g. number of plants, relative to all untreated municipalities. In column (2) we control for MSA fixed effects. Differences become less pronounced and lose significance when accounting for differences across MSAs, i.e., when comparing treated and untreated municipalities in geographic proximity. However, the joint F-test of all estimated coefficients being zero is still rejected.

In columns (3) and (4), we therefore limit the control group to those municipalities which were—just like the treatment group—in the *HSK* and had showed insufficient consolidation plans but were not treated because of their (predicted) level of equity. We see that restricting the control group along this dimension helps balancing the sample along pre-reform outcome trends, especially when differences across MSAs are accounted for. In column (4), none of the outcome variables show significant differences in pre-reform trends, and we cannot reject the joint F-test for all pre-trend differences being zero (p-value 0.655). We take these non-diverging pre-trends between treatment and control group as suggestive evidence in support of the identifying assumptions of the difference-in-differences design.

2.4 Empirical Results

We next present the results of our empirical analysis. In Section 2.4.1, we first assess whether and to what extent municipalities subject to the consolidation policy balanced their budgets. We start with a simple graphical illustration of the observed differential changes in treated and untreated municipalities' average fiscal balance over time, before presenting the corresponding regression results from the difference-in-differences (Diff-in-Diff) strategy as laid out in Section 4.3. In light of the observed consolidation effort, we then identify the municipalities' pursued strategies to achieve fiscal balance beyond and above the policy-induced transfer payments (Section 2.4.2). In Section 2.4.3, we eventually test whether consolidation-induced

Table 2.1: Smoothness in Pre-Reform Outcome Trends (2000–2008)

	All Counties		Estimation Sample	
	(1)	(2)	(3)	(4)
Total Debt (P.C.)	-0.080	-0.128	-0.001	-0.001
	(0.061)	(0.094)	(0.009)	(0.010)
Total Loans (P.C.)	-0.030	-0.094	0.022	0.011
	(0.061)	(0.094)	(0.016)	(0.017)
Total Revenues (P.C.)	0.034	-0.055	0.066	-0.038
	(0.132)	(0.148)	(0.165)	(0.186)
Business Tax Revenues (P.C.)	-0.016	0.050	0.078	0.089
	(0.151)	(0.167)	(0.174)	(0.169)
Property Tax Revenues (P.C.)	-0.277**	-0.095	-0.253*	-0.190
	(0.124)	(0.155)	(0.142)	(0.185)
Revenues from Transfers (P.C.)	0.124*	0.105	-0.031	-0.040
	(0.067)	(0.097)	(0.060)	(0.073)
Total Expenses (P.C.)	0.102	0.017	0.064	0.061
	(0.149)	(0.175)	(0.191)	(0.205)
Operating Budget (P.C.)	-0.078	-0.132	-0.281	-0.191
	(0.156)	(0.184)	(0.211)	(0.222)
Working-Age Population	-0.642***	-0.302***	-0.227	-0.020
	(0.109)	(0.107)	(0.144)	(0.129)
Number of Workers	-0.276**	-0.090	-0.059	-0.029
	(0.138)	(0.158)	(0.192)	(0.238)
Number of Plants	-0.403***	-0.233*	-0.127	-0.179
	(0.119)	(0.139)	(0.157)	(0.192)
Average Wages	0.122	0.152	0.052	0.099
	(0.151)	(0.166)	(0.183)	(0.199)
MSA Fixed Effects		YES		YES
Observations	393	393	137	137
Joint F-Test	7.993	2.577	2.057	0.795
p-value	0.000	0.003	0.024	0.655

Notes: This table presents the results of our covariate smoothness test. In column (1), we separately regress each covariate on the *Stärkungspakt* indicator using all municipalities. Specification (2) is based on our estimation sample, restricting the analysis to municipalities that were subject to the *HSK* with a not sufficient consolidation plan in 2010. In column (3), we control for differences across NUTS-II regions. In column (4), we control for MSA fixed effects. All variables show growth rates over the period 2000-2008. Standard errors are two-way clustered at the municipality level. Significance levels are * $p < .1$, ** $p < .05$, *** $p < .01$. The reported F-test statistics and the corresponding p-values test the null hypothesis of all coefficients being jointly equal to zero in a stacked regression (Lee and Lemieux, 2010).

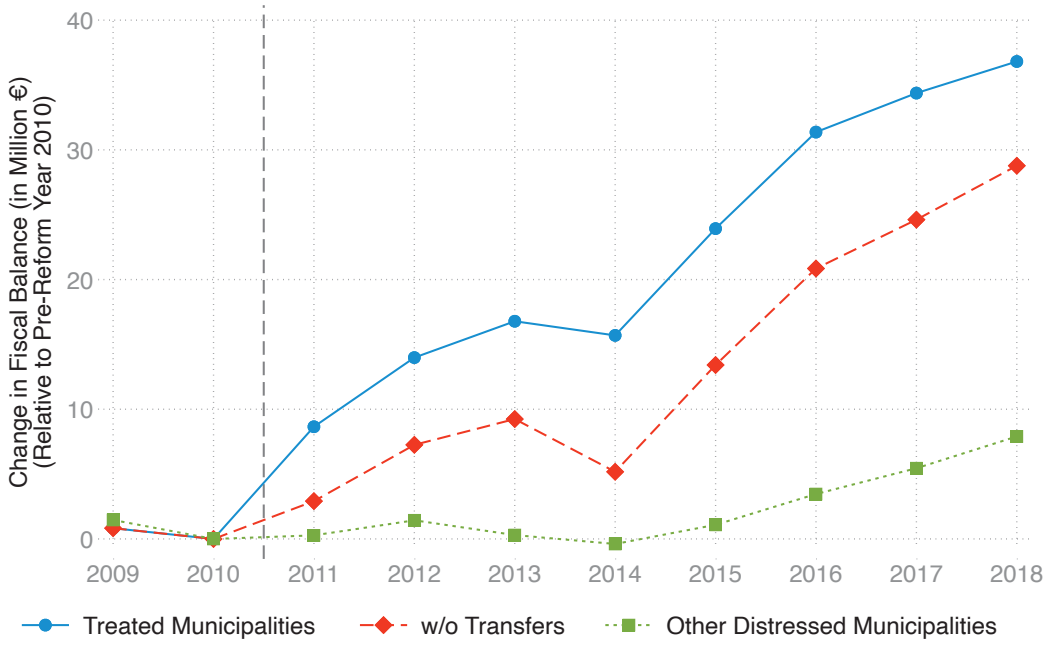
increases in local tax rates and/or cuts in local spending led to unintended (economic) consequences due to behavioral responses of local firms or residents.

2.4.1 Fiscal Effects

Descriptive Evidence. We start investigating the consequences of the consolidation program by visualizing the evolution of municipalities' average annual fiscal

balance—i.e., the difference between municipalities' revenues and expenditures—separately for the treatment and control group. Figure 2.3 shows the corresponding results. For the ease of comparison, we normalize the evolution of each groups' fiscal balance relative to the pre-reform year of 2010. We see that municipalities subject to the fiscal consolidation program consolidated their budgets over the full period of investigation from 2011 to 2018. On average, treated municipalities improved their annual fiscal balance by around 38 million EUR by 2018 compared to 2010. Notably, this improvement of treated municipalities' fiscal budget is almost linear when abstracting from the small dip in 2014.

Figure 2.3: Evolution of Municipalities' Annual Fiscal Balance



Notes: This graph displays the evolution of treated and untreated municipalities' mean fiscal balance relative to 2010, the year before program implementation. The solid line (in blue) plots the outcomes' average overall evolution for the set of treated municipalities, the dashed line (in red) the respective evolution net of those transfers explicitly linked to the consolidation policy. The dotted line (in green) plots the respective evolution of the outcome for the control group.

The corresponding evolution of untreated municipalities' average fiscal balance suggests that the largest part of this development may be indeed linked to the fiscal consolidation program under study. Municipalities in the control group also experience improvements in their average fiscal balance; yet, to a much smaller extent. Average fiscal balance of municipalities in the control group improved by around eight million EUR by 2018 relative to 2010. Thus, average consolidation was four to

five times higher in the treatment than in the control group eight years after policy implementation. The similar evolution of treated and untreated municipalities' average fiscal balance prior to the reform further suggests that this effect is not driven by differential secular trends of municipalities in control and treatment group.

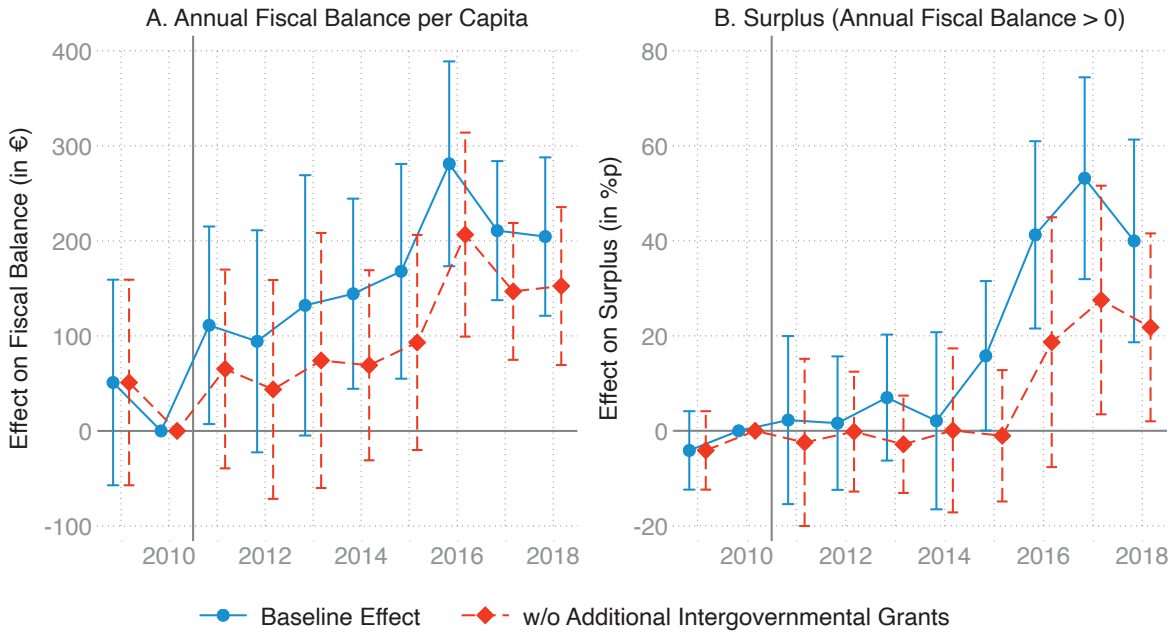
We further infer from Figure 2.3 that treated municipalities consolidated their budgets above and beyond the impact stemming from the policy-induced transfers. Treated municipalities average consolidation decreases by around 10 million EUR in 2018 when excluding the respective earmarked transfers but remains remarkably higher (by around 20 million EUR in 2018) than the average contemporaneous consolidation effort of municipalities in the control group; a difference that may be linked to treated municipalities' own consolidation efforts, e.g., by the introduction of higher local taxes or reduced local spending. We explore this possible impact of the consolidation program in the subsequent Section 2.4.2 below.

Diff-in-Diff Results. We next present the corresponding effects of the policy on municipalities' fiscal balance when using our baseline specification of the Diff-in-Diff strategy as laid out in Section 4.3. The effects, displayed in Panel A of Figure 2.4, corroborate the descriptive evidence of Figure 2.3. Net of municipality fixed effects, MSA $\times$ year and city $\times$ year fixed effects, we see that the policy had no effect on municipalities' fiscal balance prior to reform implementation, but a positive and statistically significant effect thereafter. The estimated treatment effect becomes statistically significant right after the policy intervention and grows over time. In the long run, i.e., five to seven years after reform implementation, the scheme led municipalities to improve their fiscal balance relative to the control group by around 230 EUR per capita per year.[7] Notably, around 30% of this long-run effect can be attributed to those transfers explicitly linked to program participation. Net of transfers, the average long-run consolidation effect amounts to around 170 EUR per capita. Comparing the post-reform effect patterns for the two specifications of the outcome variable (i.e., municipalities' fiscal balance including and excluding the earmarked transfers) further reveals that most of the treatment effect in the first years after policy implementation can be attributed to these earmarked transfers; the cor-

[7] We choose municipalities' fiscal balance per capita as our main specification of the outcome variable to account for outliers that may possibly drive estimated effects when specifying the outcome in levels. Appendix Figure B3 shows that effects remain qualitatively identical when taking the outcome in levels or as a transformed log specification.

responding treatment effect on municipalities' fiscal balance net of transfers remains statistically insignificant and rather small until 2015. In contrast, own consolidation efforts via increased taxes or reduced spending become notable in the longer run (compare the respective point estimates for both specifications of the outcome in 2016–2018). Below, we investigate (the timing of) municipalities' own consolidation strategies in more detail.

Figure 2.4: Effects on Fiscal Consolidation



Notes: This graph plots the point estimates, β_k, and corresponding 95% confidence intervals of the Diff-in-Diff model as laid out in Equation (2.1). The dependent variable refers to municipalities' annual fiscal balance (net of Pact transfers) in Panel A, and municipalities' probability of having an annual surplus (net of Pact transfers) in Panel B. The outcome in Panel A is re-scaled by each municipalities' level of population in 2008. The regressions include municipality, MSA × year, as well as city × year fixed effects. Standard errors are clustered at the county level.

While the preceding results indicate that the policy helped targeted municipalities to improve their financial situation, the ultimate goal of the program was to improve local finances in such a way that municipalities no longer experienced budget deficits. Therefore, upon entry into the scheme, municipalities committed to achieve balanced budgets (or report a fiscal surplus) ten years after program adoption (six years when including the earmarked transfers). Panel B of Figure 2.4 reports the corresponding estimated treatment effects. Again, we find that the policy had no effect on the outcome prior to reform implementation. Moreover, we see that treatment effects

remain close to zero and statistically insignificant in the first years after the policy intervention. In the longer run, however, statistically significant and economically sizable treatment effects emerge. Five to seven years after treatment, municipalities subject to the reform had an approximately 40 percentage points higher chance to report a balanced or positive budget compared to those municipalities in the control group. We thus conclude that municipalities reached the policy's first ultimate goal: balanced budgets six years after treatment. The results further indicate that municipalities are likely to reach budget balance net of the earmarked transfers ten years after treatment, too. Effects on the corresponding variable net of transfers appear with an additional temporal lag and are of smaller size, but statistically significant up until the end of our observation period in 2018.[8]

Sensitivity Checks and Heterogeneity. We conduct several sensitivity checks to assess whether the estimated effects are robust to alternative specification choices. First, we show that effects are very similar when controlling for coarser or finer regional fixed effects at the level of the NUTS-II and local labor market regions, respectively (see Panel A of Appendix Figures B4 and B5). Second, we show that treatment effects are unchanged when explicitly accounting for the fact that municipalities started the consolidation scheme in two distinct phases (either in 2011 or 2012). Recall that in our baseline model, we assign the same treatment year to both groups because notification of program participation occurred at the same time for both groups. Hence, even if some municipalities received the program's transfers and had to comply with the prescribed consolidation efforts one year later than others, they might have anticipated treatment and, therefore, changed their local policies right after assignment to the scheme. However, as this baseline approach may in turn blur the estimated short-run effect pattern and mask effect heterogeneity across the two groups, we allow for heterogeneous treatment effects by the start year in the simple Diff-in-Diff model, as well as apply the estimator of Borusyak et al. (2021) that explicitly account for heterogeneous effects across cohorts in two-way fixed effects models. Results, displayed in Panels B and C of Appendix Figures B4 and B5, show that effects on fiscal consolidation are (i) very similar for both groups and (ii) unaffected by explicit corrections for possible heterogeneous effects across the two treatment groups.

[8] Data for years after 2018 were still unavailable in 2022. This lack of data prevents us from investigating whether the ultimate policy goal was actually reached. However, the estimated effects suggest that this was indeed the case.

Last, we also test for heterogeneous effects across the size distribution of treated municipalities. Upon policy implementation, politicians debated whether the policy's design might differently affect smaller and larger municipalities due to differences in their overall tax capacity (Landtag Nordrhein-Westfalen, 2011b,c). Ideas to customize the consolidation policy accordingly were yet discarded. Results displayed in Panel (D) of Appendix Figures B4 and B5 show that smaller municipalities (with less than 20,000 inhabitants) and larger ones (with 20,000 inhabitants or more) consolidated their budget by almost equal amounts.[9] Five to seven years after policy implementation, small (large) municipalities subject to the fiscal consolidation policy improved their fiscal balance per capita by 240 (229) EUR on average compared to those municipalities in the control group.

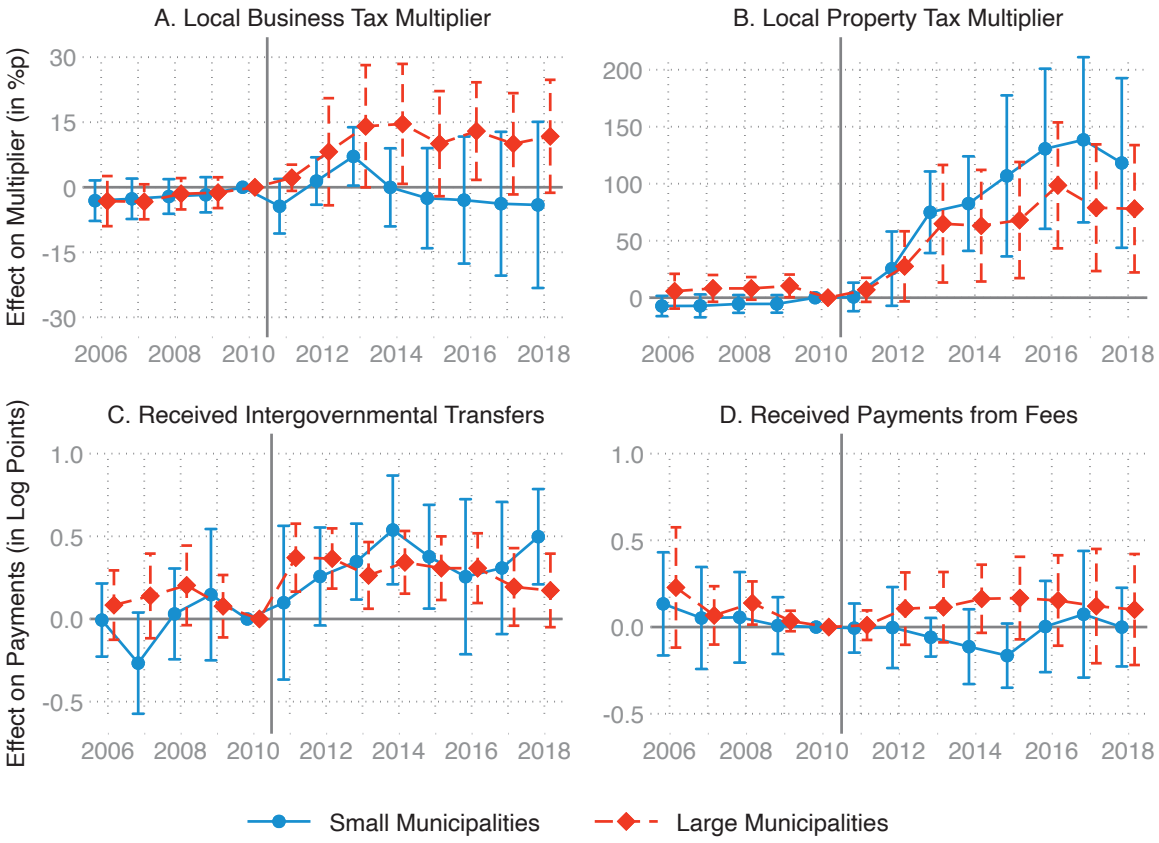
2.4.2 Strategies of Consolidation

In light of the policy's positive impact on municipalities' fiscal budget, we next aim at identifying those margins of adjustments that helped municipalities to consolidate budgets above and beyond the policy-induced transfer payments. Notably, while effects on fiscal budgets were remarkably homogeneous, we uncover substantial differences in municipalities' consolidation strategies by population size, which we will present as follows.

Increased Revenues. We first investigate whether municipalities consolidated budgets by increasing their revenues. Specifically, we look at the four most important sources of municipalities' revenues: (i) the local business tax, (ii) the local property tax, (iii) intergovernmental transfers, as well as (iv) local service fees and levies. Figure 2.5 plots the corresponding treatment effects for smaller and larger municipalities—based on an augmented specification of the baseline Diff-in-Diff design as laid out in Equation (2.1) that allows for heterogeneous effects via an interaction term and includes additional size group $\times$ year fixed effects.

[9] We follow the definition of the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR) and allow for heterogeneous effects among rural municipalities (*Landgemeinden*) and smaller cities (*Kleinstädte*) versus medium-sized and large cities (*Mittelstädte* and *Großstädte*). This split is almost equivalent to a split along municipalities' median pre-reform population size; a split that leads to very similar results (not reported). Note that no municipality changes its classification throughout the period under investigation.

Figure 2.5: Effects on Tax Rates, Transfers, and Revenues from Fees & Levies



Notes: This graph plots the point estimates, β_k, and corresponding 95% confidence intervals of the Diff-in-Diff model as laid out in Equation (2.1), allowing for heterogeneous treatment effects by municipality size via an interaction term. The dependent variable refers to municipalities' annual local business tax multiplier in Panel A, annual local property tax multiplier in Panel B, received intergovernmental transfers in Panel C, and revenues from fees in Panel D. The outcomes in Panel C–D are log-transformed. The regressions include municipality, MSA × year, city × year as well as size group × year fixed effects. Standard errors are clustered at the county level.

Panels A and B of Figure 2.5 highlight the differential effects of treatment on the tax setting behavior of smaller and larger municipalities. From Panel A, displaying the corresponding treatment effects on the local business tax rate, we first infer that pre-trends are small and statistically insignificant for both groups. Hence, treated and untreated municipalities did not alter their local business tax rate in systematically different ways prior to reform implementation. Post treatment, we find no effect for smaller municipalities but a positive and statistically significant one for larger municipalities subject to the fiscal consolidation scheme. This effect builds up over the first three years and remains constant thereafter. On average,

larger municipalities subject to treatment raised their respective local business tax by around 11.5 percentage points (2.5% relative to the size group's mean tax rate).

Smaller municipalities subject to the policy, in turn, increased the local property tax rate—and to a much bigger amount than larger ones (see Panel B). In more detail, we see that estimated pre-trends for this outcome are very close to zero and insignificant for both groups. Post treatment, both groups of municipalities increase the local property tax rate over time, yet to a notably different extent. Five to seven years after treatment, the policy caused smaller municipalities to increase their local property tax multiplier by around 130 percentage points (30% relative to the group's pre-reform mean), whereas larger municipalities increased the corresponding multiplier by around 85 percentage points (19% relative the group's pre-reform average). The graphs thus provide some evidence that smaller and larger municipalities subject to treatment both consolidated their budgets by setting higher local taxes. However, they put the burden of taxation on different shoulders. Smaller municipalities disproportionately increased the property tax; a tax borne by local residents. In contrast, larger municipalities shifted the burden of higher taxation on local residents and businesses alike.

Panel C of Figure 2.5 next reports the corresponding effects on municipalities' annual amount of transfers received. In line with expectations, we find that both smaller and larger municipalities subject to treatment received higher transfer payments after reform implementation than municipalities in the control group. This effect is (at least to some extent) mechanical given that program participation was accompanied by sizable earmarked transfers. However, two features are worth pointing out. First, pre-trends are small and insignificant for both groups of treated municipalities, which lends further credibility to the implemented Diff-in-Diff design. Second, when subtracting the annual (log) amount of *Stärkungspakt* transfers from the annual total amount of transfers received (e.g., due to fiscal equalization schemes and other intergovernmental transfers), we find small and statistically insignificant treatment effects (see Appendix Figure B6 for the corresponding estimates). This implies that municipalities' entry into the consolidation scheme did not systematically affect the receipt of other transfers (from higher levels of government) that may have eased or worsened municipalities' overall financial situation. Last, Panel D plots the corresponding treatment effects on municipalities' (log) revenues that ac-

crue from fees and levies (e.g., due to administrative or usage charges). We detect no sizable or significant treatment effects for this source of revenue.

Reduced Spending. We next investigate whether treated municipalities consolidated their budgets via cuts in local spending, too. Since the change of municipalities' accounting system in 2009 (cf. Section 2.2.1), detailed information on municipalities' spending on different items is given on an annual basis. In the following analysis, we focus on seven aggregate spending categories (that jointly account for municipalities' total expenditures): (i) taxes, levies, or interest payments to remit, as well as spending on (ii) public local services, (iii) social security, (iv) infrastructure, (v) education & science, (vi) public goods (such as theaters, sport facilities, parks), and (vii) business subsidies. Again, we allow for heterogeneous effects for smaller versus larger municipalities by adding an interaction term and including size group $\times$ year fixed effects to the baseline Diff-in-Diff model as specified in Equation (2.1). All outcome variables are specified in logs, i.e. all estimates may be interpreted as semi-elasticities.

Table 2.2 plots the corresponding long-term treatment effects—defined as the average treatment effect five to seven years after the reform. The corresponding effects for all leads and lags of the underlying Diff-in-Diff model are provided in Appendix Figure B7. Again, we detect notably different responses among smaller versus larger municipalities subject to the fiscal consolidation scheme. Smaller municipalities subject to the fiscal consolidation scheme cut spending on local public services—such as the residents' registration office, the traffic office or the building supervisory board—by roughly 18.5% compared to the control group in the long run, whereas larger municipalities did not reduce spending on this item. In contrast, we detect a sizable and significant decline in spending on business subsidies among the larger municipalities subject to treatment. On average, they decreased spending by around 50% relative to the control group over the long run. Smaller municipalities did not cut back spending on this item in turn. For all other spending items, we detect no statistically significant treatment effects. While some of these estimated semi-elasticities are of small magnitude, others are sizable but imprecisely estimated.

Table 2.2: Effects on Local Spending

	Remitted Taxes & Levies (1)	Public Local Services (2)	Social Security (3)	Infra-structure (4)	Education & Science (5)	Public Goods (6)	Business Subsidies (7)
Smaller Municipalities	-0.045	-0.185**	-0.162	0.167	0.073	0.076	-0.086
	(0.047)	(0.082)	(0.124)	(0.171)	(0.206)	(0.146)	(0.256)
Larger Municipalities	0.054	0.010	0.036	0.030	0.106	0.034	-0.508*
	(0.069)	(0.060)	(0.053)	(0.196)	(0.081)	(0.079)	(0.272)
Number of Observations	1,290	1,290	1,290	1,290	1,290	1,290	1,279
Share of Payments (in %)	37.36	20.71	14.41	13.36	7.45	5.41	1.32

Notes: This table shows the estimated long-term effects of fiscal consolidation on municipalities' spending for different items. All outcomes are in logs, such that estimated coefficients can be interpreted as semi-elasticities. Estimates are the simple average of $\widehat{\beta}_{2016}$—$\widehat{\beta}_{2018}$ and stem from an augmented version of the Diff-in-Diff model as laid out in Equation (2.1), allowing for heterogeneous effects by size via an interaction term. All regressions include municipality, MSA × year, city × year as well as size group × year fixed effects. Standard errors, clustered at the county level, are displayed in parentheses. The shares of payments (in %) are the shares of the respective payment items on the total payments in 2016–2018. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The Importance of Each Margin of Consolidation. To provide an overview about the relative importance of each margin of consolidation for the two groups of treated municipalities, we translate the corresponding long-term treatment effects on different sources of revenues and spending items (cf. Figure 2.5 and Table 2.2) into per capita terms and relate them to municipalities' overall fiscal consolidation efforts. Figure 2.6 summarizes the results of this exercise.

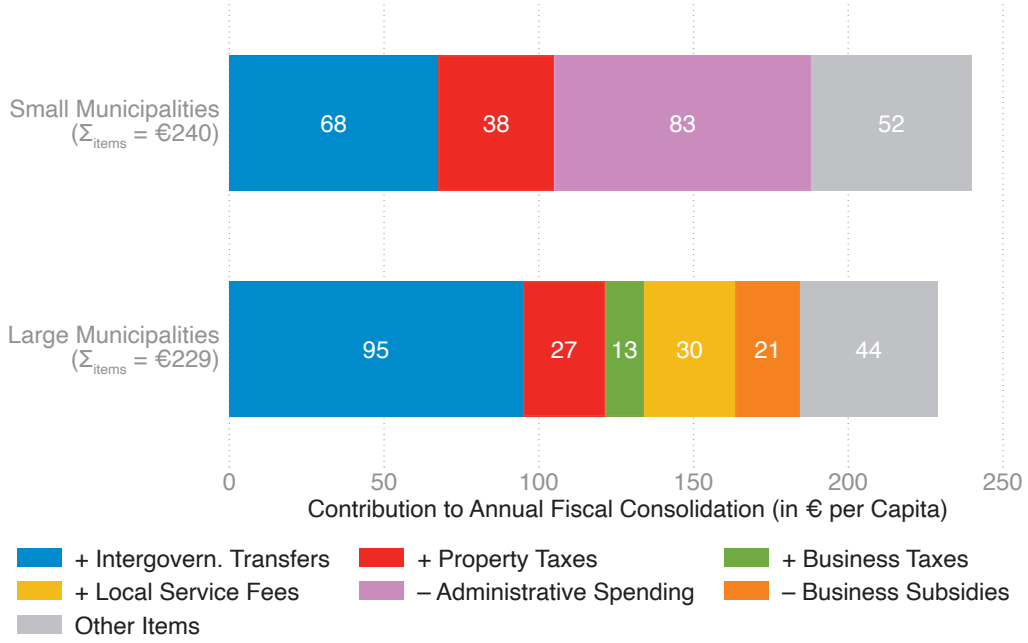
As shown in Section 2.4.1, smaller and larger municipalities subject to treatment display similar improvements in their annual fiscal balance per capita: smaller (larger) municipalities improved their fiscal balance (revenues over expenses) by around 240 EUR (229 EUR) per capita relative to the control group five to seven years after the consolidation program was enacted. However, smaller and larger municipalities differed markedly in their consolidation strategies. Smaller municipalities focused on three key margins: first, and most importantly, smaller municipalities substantially reduced their spending on local public services by around 83 EUR per capita on average—accounting for more than one third of their total consolidation efforts. Second, around 28% of total consolidation can be explained by those earmarked transfers explicitly linked to program participation (68 EUR per capita on average). Third, smaller municipalities also strongly increased their local property tax rate in response to treatment, which in turn led to an increase in annual revenues of around 38 EUR per capita on average.

In contrast, for larger municipalities we observe that around 40% (95 EUR) of their total consolidation effort can be attributed to the policy-induced transfers. In addition, larger municipalities subject to treatment did not cut back on spending for local public services. Rather, they imposed some additional fiscal burden on firms by setting higher business tax rates (13 EUR per capita, amounting to 6% of total consolidation) and reducing financial support for local businesses (21 EUR per capita, 9%). As the set of smaller municipalities subject to treatment, larger municipalities also raised local property tax rates; yet, to a smaller extent than municipalities of lower population size. The average additional revenue due to higher property tax rates amounts to 27 EUR per capita for larger municipalities.

While the differences in consolidation strategies are remarkable, they may well reflect differences in (perceived) tax competition at the local level. Smaller munic-

ipalities are generally more concerned with firm mobility that may in turn cause the erosion of the local business tax base. In general, larger municipalities have less concerns in this direction as they may offer more (productive) amenities to local establishments, which may alleviate relocation responses to local business tax increases.

Figure 2.6: The Importance of Municipalities' Different Consolidation Strategies

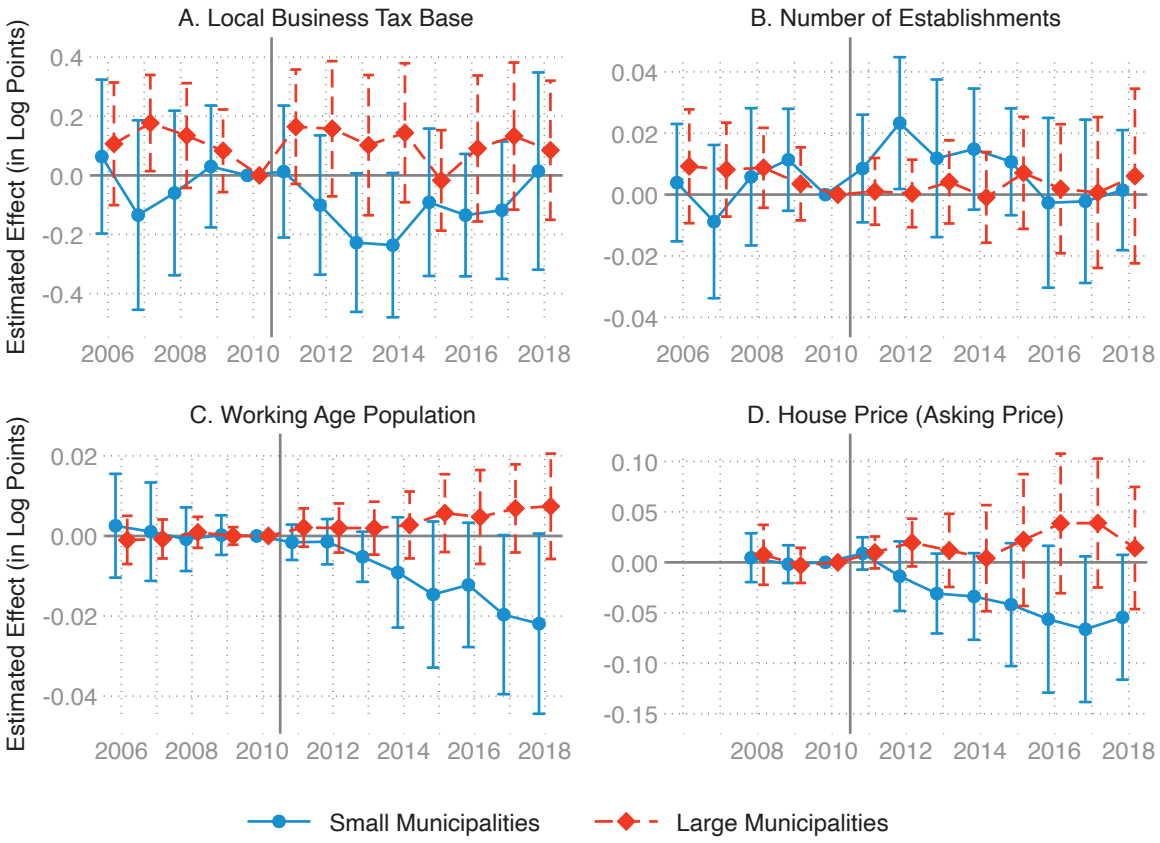


Notes: This graph summarizes the contributions of different sources of revenues and spending items on municipalities' fiscal consolidation for smaller and larger municipalities, respectively. Depicted figures are measured in per capita EUR terms and based on the estimates presented in Figure 2.5 and Table 2.2, averaging over the period from 2016 to 2018.

2.4.3 Broader Consequences of Municipalities' Fiscal Consolidation

Last, we assess whether the policy-induced cuts in local spending and increases in local tax rates caused broader (economic) consequences. Spatial equilibrium models predict that local spending on amenities as well as local taxes are capitalized in local prices (wages, housing) and thereby affect the location choice of firms and households. In light of the different consolidation strategies of smaller and larger municipalities subject to treatment, we thus assess the policy's impact on the spatial equilibrium outcomes below.

Figure 2.7: Effects on Broader Economic Outcomes



Notes: This graph plots the point estimates, β_k, and corresponding 95% confidence intervals of the Diff-in-Diff model as laid out in Equation (2.1), allowing for heterogeneous treatment effects by municipality size via an interaction term. The dependent variable refers to municipalities' annual local business tax base in Panel A, annual local number of establishments in Panel B, working age population in Panel C, and house prices (asking prices) in Panel D. All outcomes are log-transformed. The regressions include municipality, MSA × year, city × year as well as size group × year fixed effects. Standard errors are clustered at the county level.

Panel A of Figure 2.7 shows that large municipalities indeed see hardly any change in local economic activity as approximated by the local business tax base. Even though these municipalities partly consolidate via increased local business tax rates, we do not find any meaningful response in terms of profit shifting or reduced local economic activity. This picture is confirmed in Panel B, showing that the number of local establishments remains unchanged after treatment. For smaller municipalities, both sets of estimates are considerably more volatile and point to some loss in economic activity at the local level (Panel A), whereas the number of establishment seems unaffected (Panel B). The likely explanation is that

multi-establishment firms shift part of their economic activity to other regions, or that part of the operations is shut down altogether. However, estimates are quite imprecisely estimated, which warrants caution when interpreting this result.

Turning to the household side, we detect a clearer effect pattern. Post treatment, a negative effect on the size of the working-age population in smaller municipalities subject to treatment emerges and becomes statistically significant five to seven years after treatment. In the long run, population levels declined by around 1.7% in smaller treated municipalities compared to the control group on average. We reconcile this result with the predictions of standard models of spatial equilibrium (Rosen, 1979; Roback, 1982), where residents respond to increases in local taxes and/or reductions in local amenities by outmigration. In contrast, we detect no corresponding effect for larger municipalities. These differences may be rationalized twofold. First, they may be due to the different consolidation strategies pursued. Larger municipalities did not reduce spending on local public services and put a relative lower burden on local residents. Second, larger cities may offer more and different amenities, which may attenuate residents' migration responses in turn.

As predicted by standard models of spatial equilibrium, we further find that the decline in population levels in smaller municipalities subject to fiscal consolidation is accompanied by a lagged negative response of local house prices. Five to seven years after policy intervention, asking prices for houses and flats in smaller municipalities subject to treatment declined by around 5% compared to the control group.

2.5 Conclusion

In this paper, we study the fiscal and broader economic consequences of a large-scale consolidation program for municipalities in North Rhine-Westphalia, Germany's most populous state. The so-called *Stärkungspakt Stadtfinanzen* was implemented in 2011 and supported financially distressed municipalities through intergovernmental transfer payments but obliged targeted municipalities to consolidate their budget net of transfers within a decade. Political discretion in the criteria governing municipalities' participation allows the implementation of a dynamic difference-in-differences design, comparing the set of treated municipalities to their equally financially-distressed neighbors.

Overall, the results of our analysis show that the local austerity program under study was indeed effective. Treated municipalities consolidated their budget by about 230 EUR (170 EUR) per capita and had a 40 (20) percentage points higher likelihood of a positive annual surplus compared to the control group (excluding the intergovernmental transfers of the consolidation program). Whereas the policy-induced consolidation effects are remarkably homogeneous across the distribution of targeted municipalities, smaller and larger municipalities differed notably in their strategies of consolidation. Smaller municipalities with less than 20,000 inhabitants primarily consolidated their budgets by setting higher local taxes on property and cutting spending on local public services. In contrast, larger municipalities (with 20,000 inhabitants or more) implemented no spending cuts on residents' local amenities but shifted part of the burden of consolidation on firms through higher local business tax rates and lower business subsidies.

In a final step of the analysis, we show that the consolidation-induced financial burden on firms had no negative effects for the local economy. Both the municipalities' business tax base as well as the number of firms remained unchanged in response to treated municipalities' fiscal consolidation efforts. However, we find economically sizable and statistically significant negative effects of fiscal consolidation on population levels and house prices for smaller municipalities. These results provide strong evidence of general equilibrium and capitalization effects in the spirit of Rosen (1979) and Roback (1982) models.

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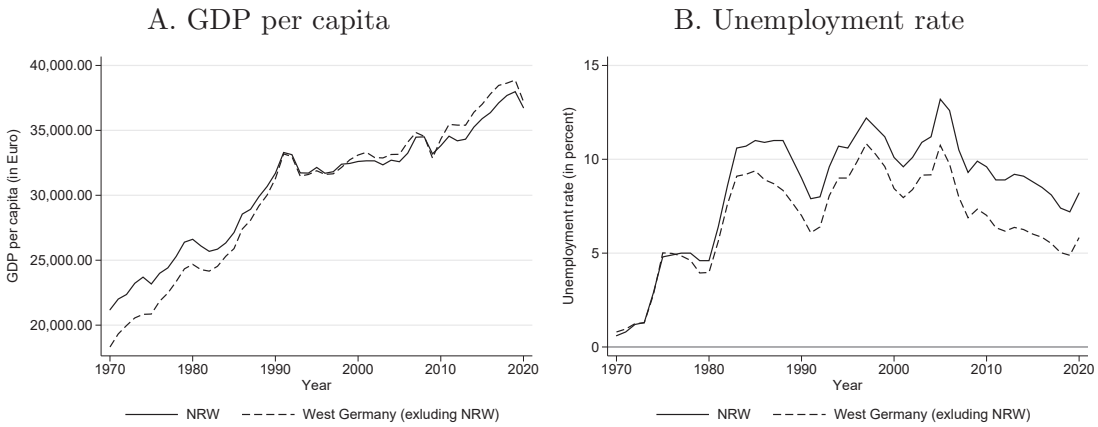
2.6 Appendix

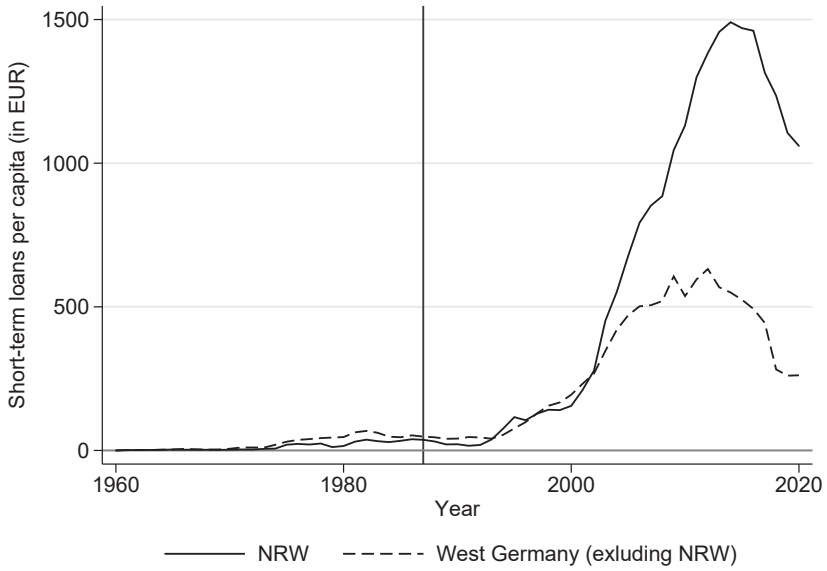
2.6.1 Additional Descriptive Statistics

Figure B1: The Evolution of Per Capita GDP and Unemployment over Time



Notes: Panel A of this figure plots the evolution of the inflation-adjusted GDP per capita over the period from 1970–2020 for NRW and all other West German territorial states. Panel B plots the corresponding evolution of the unemployment rate. *Sources:* Federal Statistical Office and Federal Employment Agency.

Figure B2: The Evolution of Short-Term Loans over Time



Notes: This graph plots the inflation-adjusted evolution of municipalities' total short-term loans per capita in NRW and all other West German territorial states over the period from 1960–2020. The vertical line indicates the year of the introduction of the *HSK* in 1987. Source: Federal Office of Statistics

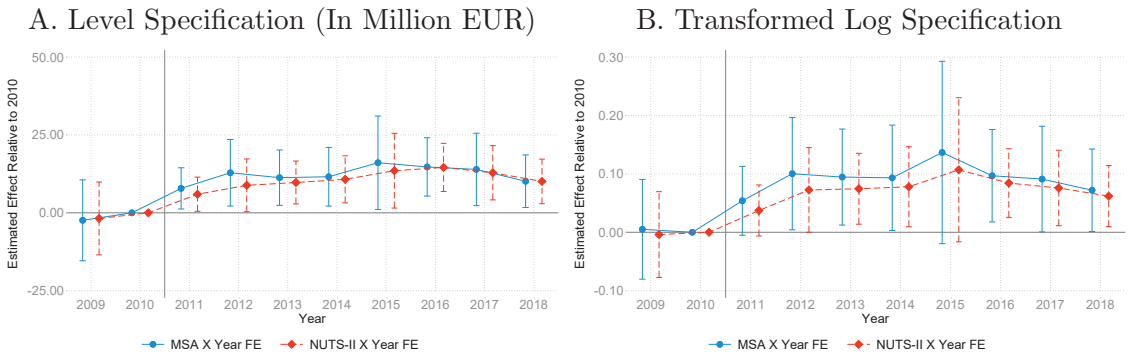
Table B1: Descriptive Statistics

	All Municipalities	Not in Sample	In Sample	Treatment Group	Control Group
	(1)	(2)	(3)	(4)	(5)
A. Sources of Revenue (in %)					
Local Taxes	27.1	28.7	24.1	21.6	26.1
Local Business Tax	19.7	21.1	16.9	14.9	18.4
Local Property Tax	6.6	6.6	6.4	6.0	6.8
Transfers	44.7	45.1	44.1	44.5	43.8
Higher Levels of Government / Equalization Schemes	17.5	17.0	18.4	20.6	16.6
Apportionment of National Taxes	17.7	18.0	17.1	15.5	18.3
Fees	11.1	11.2	10.8	10.9	10.6
Other Sources	17.1	15.0	21.0	23.0	19.4
B. Sources of Expenses (in %)					
Remitted Taxes, Levies and Transfers	38.1	38.4	37.6	35.1	39.5
Public Local Services	20.8	20.7	20.8	21.1	20.5
Social Security	11.7	10.4	14.1	17.3	11.6
Infrastructure	13.8	14.2	13.1	12.8	13.3
Education & Science	8.3	8.7	7.6	6.8	8.2
Public Goods	5.9	6.1	5.5	5.3	5.7
Business Subsidies	1.5	1.5	1.3	1.4	1.2
C. Budget					
Revenues over Expenses (in EUR per capita)	-120.4	-65.1	-224.4	-323.3	-145.1
Share Positive Annual Surplus (in %)	12.1	15.1	6.6	3.3	9.2
Total Loans (EUR per capita)	2,015	1,493	3,001	3,895	2,283
Short-term Loans (EUR per capita)	527	198	1,149	1,779	644
Long-term loans (EUR per capita)	1,487	1,294	1,851	2,116	1,639
D. Broader Outcomes					
Business tax base (in M. EUR)	5.2	5.3	5.0	7.0	3.4
Total Number of Plants (in Thousands)	1.0	1.0	1.2	1.7	0.8
Total Number of Workers (in Thousands)	15.1	13.8	17.7	26.3	10.8
Average Daily Wage	65.4	64.6	67.0	67.1	66.9
Population (in Thousands)	44.3	37.9	56.4	83.4	34.6
Average House Prices Per Sqm	1,487	1,475	1,508	1,558	1,468

Notes: This table displays the means of the respective outcomes in the pre-treatment year 2010. Column (1) provides the mean for all 396 municipalities in NRW. Column (2) refers to 259 municipalities which are not in the sample of analysis, i.e., municipalities that are not part of the HSK or have an approved HSK. Column (3) refers to the 137 municipalities in our sample which cover all municipalities subject to the *Stärkungspakt* and subject to the *HSK* whose consolidation plan was not approved in 2010 but exclude thee municipalities which entered the consolidation program in 2017. The Treatment Group (Column (4)) are 61 municipalities subject to the consolidation program. The Control Group in Column (5) represents 76 municipalities that did not participate in the consolidation program and whose HSK was not approved. Source: See desription of the dataset used in section 2.2.

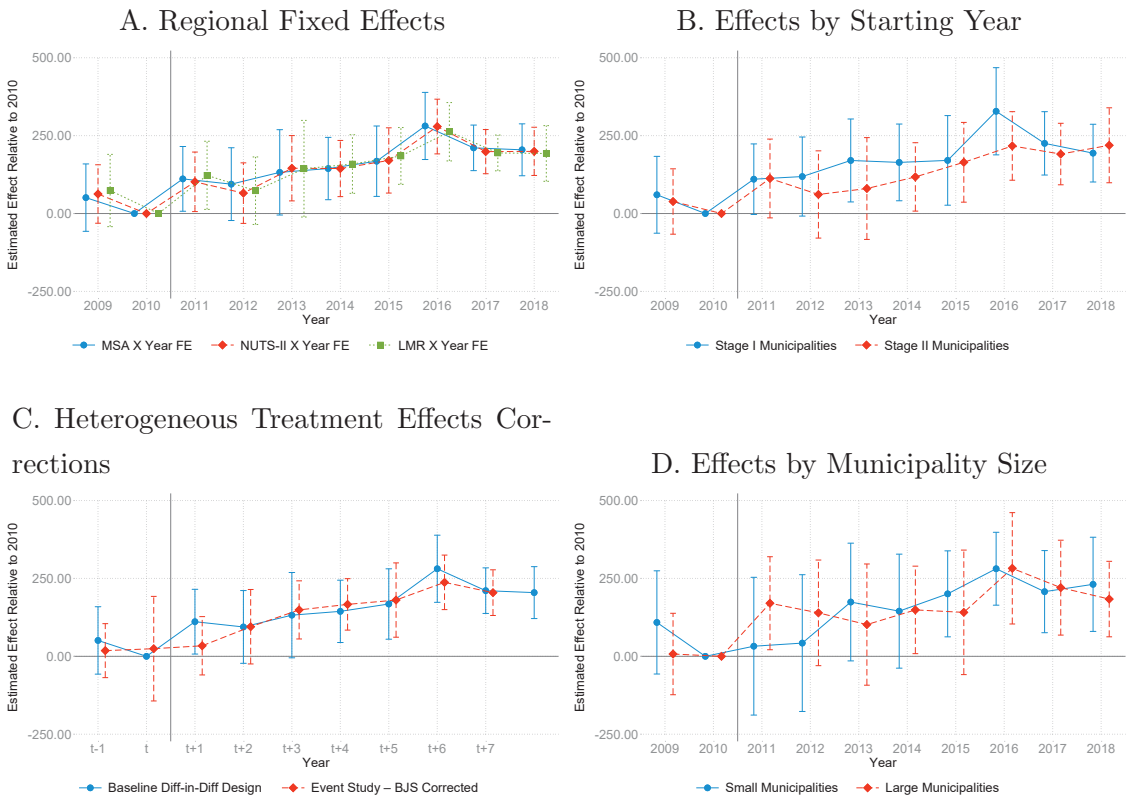
2.6.2 Additional Results

Figure B3: Effects on Fiscal Balance – Alternative Specifications of the Outcome Variable



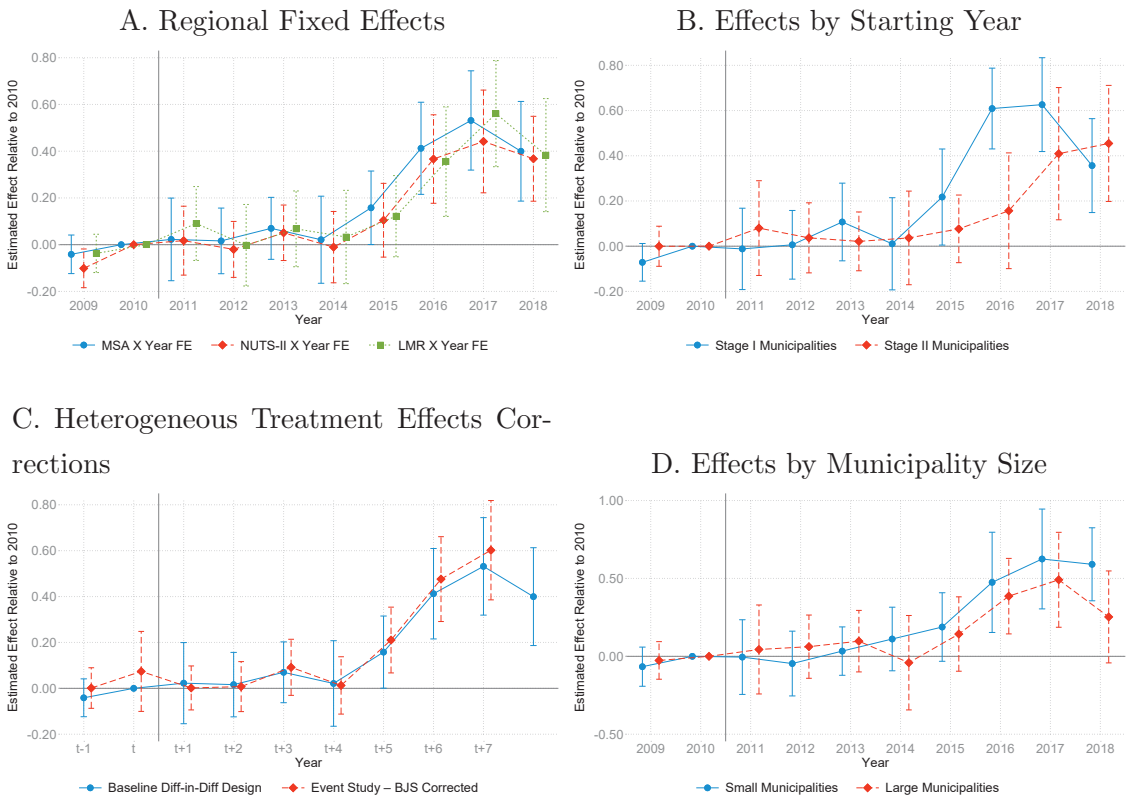
Notes: This graph plots the point estimates, β_k, and corresponding 95% confidence intervals of the Diff-in-Diff model as laid out in Equation (2.1). The dependent variable refers to municipalities' annual fiscal balance in Million EUR in Panel A and municipalities' log-transformed annual fiscal balance in Panel B. The regressions include municipality, MSA × year (in Panel A) or NUTS-II × year (Panel B), respectively, as well as city × year fixed effects. Standard errors are clustered at the county level.

Figure B4: Effects on Fiscal Balance per Capita – Sensitivity Checks & Heterogeneity



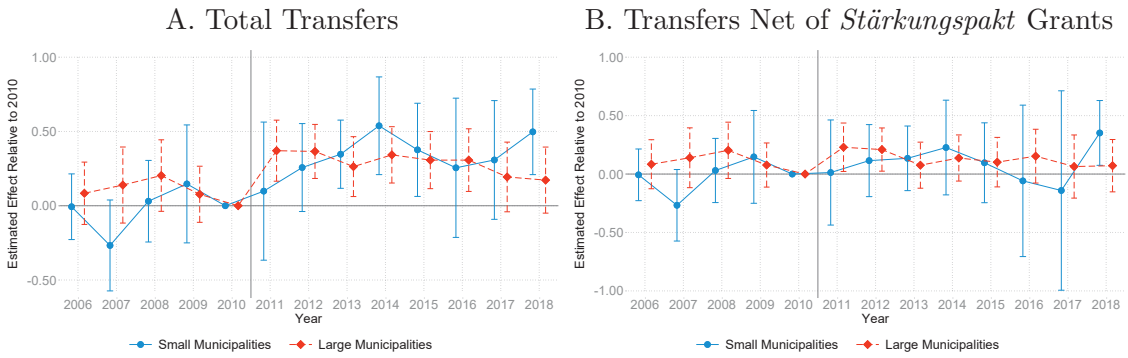
Notes: This graph plots the point estimates, β_k, and corresponding 95% confidence intervals of the Diff-in-Diff model as laid out in Equation (2.1). The dependent variable refers to municipalities' annual fiscal balance re-scaled by each municipalities' level of population in 2008 in all Panels. The regressions in Panel A include municipality, city × year fixed effects and either MSA × year, NUTS-II × year or labor market region (LMR) x year fixed effects. The regressions in Panel B and C include municipality, MSA × year, as well as city × year fixed effects. The regressions in Panel D include municipality, MSA × year, city × year fixed effects as well as size group × year fixed effects. In Panel B, the regressions allow for heterogenous treatment effect of municipalities entering the consolidation program in stage I (2011) and stage II (2012), respectively. Panel C shows the baseline Diff-in-Diff Design based on description in Equation (2.1) and the Event Study correction of two-way fixed effects models based on (Borusyak et al., 2021). In Panel D, the regressions allow for heterogenous treatment effect by municipalitiy size via an interaction term. Standard errors are clustered at the county level.

Figure B5: Effects on Fiscal Surplus (Annual Fiscal Balance > 0) – Sensitivity Checks & Heterogeneity



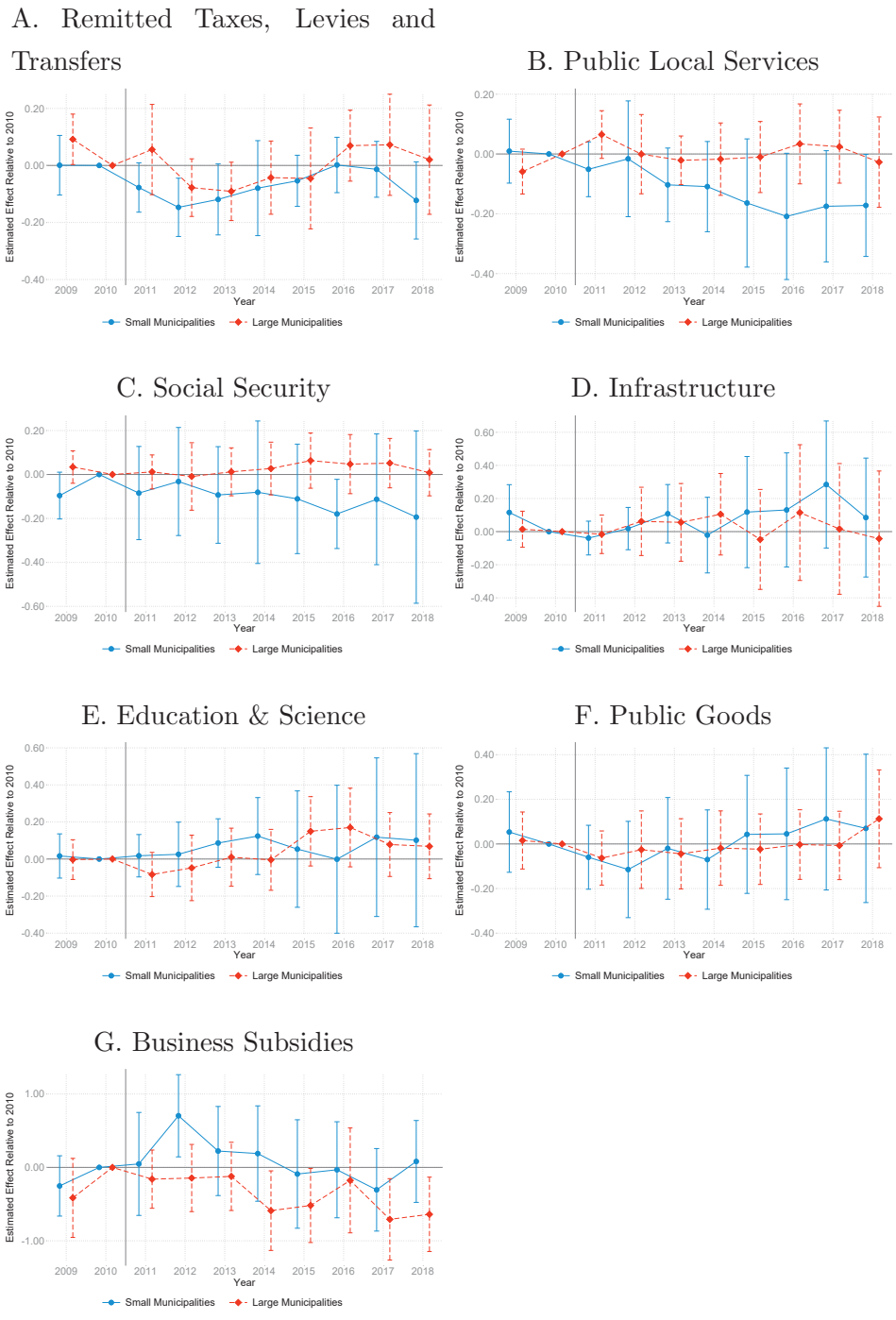
Notes: This graph plots the point estimates, β_k, and corresponding 95% confidence intervals of the Diff-in-Diff model as laid out in Equation (2.1). The dependent variable refers to municipalities' probability of having a positive annual surplus in all Panels. The regressions in Panel A include municipality, city × year fixed effects and either MSA × year, NUTS-II × year or labor market region (LMR) x year fixed effects. The regressions in Panel B and C include municipality, MSA × year, as well as city × year fixed effects. The regressions in Panel D include municipality, MSA × year, city × year fixed effects as well as size group × year fixed effects. In Panel B, the regressions allow for heterogenous treatment effect of municipalities entering the consolidation program in stage I (2011) and stage II (2012), respectively. Panel C shows the baseline Diff-in-Diff Design based on description in Equation (2.1) and the Event Study correction of two-way fixed effects models based on (Borusyak et al., 2021). In Panel D, the regressions allow for heterogenous treatment effecty by municipalitiy size via an interaction term. Standard errors are clustered at the county level.

Figure B6: Effects on Transfers (Net of *Stärkungspakt* Grants)



Notes: This graph plots the point estimates, β_k, and corresponding 95% confidence intervals of the Diff-in-Diff model as laid out in Equation (2.1), allowing for heterogeneous treatment effects by municipality size via an interaction term. The dependent variable refers to total transfers received by the municipalities in Panel A and the transfers received by the municipalities minus the grants from the *Stärkungspakt* in Panel B. The outcomes are log-transformed. The regressions include municipality, MSA × year, city × year as well as size group × year fixed effects. Standard errors are clustered at the county level.

Figure B7: Effects on Local Spending



Notes: This graph plots the point estimates, β_k, and corresponding 95% confidence intervals of the Diff-in-Diff model as laid out in Equation (2.1), allowing for heterogeneous treatment effects by municipality size via an interaction term. The dependent variable refers to municipalities' spending on remitted taxes, levies and transfers in Panel A, public local service in Panel B, social security in Panel C, infrastructure in Panel D, education and science in Panel E, public goods in Panel F and spending on business subsidies in Panel G. The outcomes are log-transformed. The regressions include municipality, MSA × year, city × year as well as size group × year fixed effects. Standard errors are clustered at the county level.

Part II: Analyses of Voting Behavior

3

The Role of Globalization Concerns and the European Unemployment Crisis for the Rise of Anti-Establishment Parties

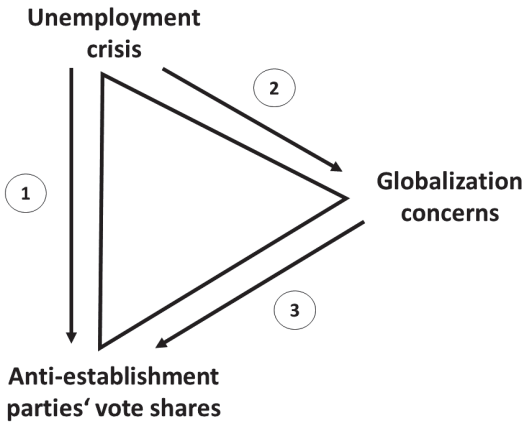
3.1 Introduction

Anti-establishment parties—covering populist, far-left, far-right and Euroskeptic parties—experienced a wave of success in European countries in the last decades (Norris and Inglehart, 2019; Rooduijn et al., 2019). Since the leadership of those parties, and in particular of populist parties can cause medium and long-term economic costs (Funke et al., 2020), it is important to understand possible mechanisms behind the success of anti-establishment parties in order to curb their rise. This paper examines the relationship between the development of increasing support for anti-establishment parties and two other socio-economic phenomena that accompanied the recent rise of anti-establishment parties: first, the European economic crisis and second, an increase in anti-globalization tendencies. The former is, among others, indicated by a large increase in the European average unemployment rate (Guichard and Rusticelli, 2010; Algan et al., 2017). The latter is exemplified by protests against free trade agreements, protectionist trade policies, or by the British referendum in 2016 to leave the European Union (Meunier and Czesana, 2019; Dür et al., 2020).

While the literature largely acknowledges peoples' trust erosion in political institutions (Algan et al., 2017; Dal Bó et al., 2018; Dotti Sani and Magistro, 2016; Dustmann et al., 2017; Guiso et al., 2017) or negative attitudes towards immigration (Dustmann et al., 2019; Halla et al., 2017; Van der Brug et al., 2000; Dülmer and Klein, 2005; Lubbers and Scheepers, 2000), as one of the reasons for the rise of anti-establishment parties, this paper focuses on another fundamental attitude, which is likely to affect the voting behavior—that is the attitude towards globalization. Several studies further indicate that anti-establishment parties benefited from the European economic crisis due to an increased unemployment rate (Algan et al., 2017; Dal Bó et al., 2018; Dustmann et al., 2017; Lindgren and Vernby, 2016). This paper contributes to the understanding of the role of globalization concerns as a potential mechanism behind the electoral consequences of the European unemployment crisis. I analyze the questions whether the unemployment crisis, besides its effect on the voting behavior, also shapes attitudes towards globalization and whether globalization concerns, in turn, affect anti-establishment parties' vote shares. Further, I examine whether anti-globalization attitudes are a mediator for the effect of the unemployment rate on the election of specific anti-establishment parties. Figure 3.1

illustrates the studied relationship of the three key factors.

Figure 3.1: Analyzed relationship



Notes: Own illustration

The empirical analysis relies on a panel dataset of 84 European Nomenclature of Territorial Units for Statistics (NUTS 1) regions covering the period from 2008 to 2017. I exploit the regional average of responses to the Eurobarometer survey on whether globalization is an opportunity for economic growth, and anti-establishment parties' vote shares are based on national parliamentary elections in Europe.

To evaluate the role of globalization concerns for the relationship between the unemployment crisis and the election of anti-establishment parties, I proceed in two steps. In a first step, I separately estimate the effects as presented in Figure 3.1. I examine which type of anti-establishment parties (far-right, far-left, populist or Euroskeptic parties) benefits from a rise in the unemployment rate and what is the effect of the European unemployment crisis on globalization concerns. The paper then evaluates how globalization concerns affect the voting decision in favor of anti-establishment parties. When estimating the effect of economic indicators or of social attitudes on voting behavior endogeneity concerns arise, inter alia, due to omitted variables (such as wage drops, the shadow economy or traditionalism). To address this issue, I apply two instrumental variable (IV) designs. Based on the assumption that the performance of the construction sector is a reliable indicator to capture the impact of a recession of the economy (Thakurta, 1970; Algan et al.,

2017), the unemployment rate is instrumented by the share of gross value added of the construction sector to estimate the effect of the European unemployment crisis on anti-establishment parties' election results and on globalization concerns, respectively. As the expansion of broadband internet promotes specific media consumption which facilitates the radicalization of people's ideas and attitudes (Hitt and Tambe, 2007; Brey et al., 2019), the share of households having broadband access serves as an instrument to estimate the effect of anti-globalization attitudes on the voting behavior. In the second step of the analysis, I turn to a mediation analysis to relate these effects.

The results show that an increase in the unemployment rate does not only have a significant positive effect on the vote shares of far-left and populist parties, but it also increases globalization concerns. More negative attitudes towards globalization, in turn, significantly increase far-left and populist vote shares, while it leads to a drop in far-right votes. On a first glance, this result might be quite surprising since far-right parties are often perceived to have anti-globalization tendencies, for instance, based on nativism or anti-immigration sentiments (Kriesi and Pappas, 2015). The analyzed globalization concerns however, measure the attitude towards the economic effect of globalization. One explanation for this contrasting effect of globalization concerns on far-left and far-right voting could be that the electorate of far-right parties is not denying that globalization is an opportunity for economic growth but other factors resulting from globalization—like robotization, immigration or import competition—might rather increase far-right votes (see, e.g., Anelli et al., 2019; Dustmann et al., 2019; Halla et al., 2017). Another explanation could lie in the different focus of the economic programs of far-left and far-right parties. Far-right parties' economic programs are often blurred or try to draw attention to more non-economic, socio-cultural issues (Mudde, 2007; Rovny, 2013; Elias et al., 2015). Thus, voters of far-right parties do not seem to have the expected anti-globalization attitudes and generally believe that globalization boosts the economy. In contrast, far-left parties clearly emphasize the rejection of the prevailing economic system (Kriesi and Pappas, 2015) which ties well with higher globalization concerns of the electorate of far-left parties. A mediation analysis captures the extent to which the channel of globalization concerns drives the effect of the unemployment rate on far-left vote shares. The result suggests that approximately eight percent of the total effect of the unemployment rate on

voting for far-left parties is explained by anti-globalization attitudes.

According to the three effects illustrated in Figure 3.1, the paper mainly contributes to three strands of related literature. In general, the study by Inglehart and Norris (2016) states two possible explanations for the recent success of populism: first, the cultural backlash against progressive values such as cosmopolitanism and multiculturalism and second, the economic insecurity steaming from globalization and technological progress or the sharp increase in the unemployment rate. While Inglehart and Norris (2016) find more evidence for the cultural backlash hypothesis, this paper supports the view on the economic roots of the rise of anti-establishment parties. By analyzing the electoral consequences of the European economic crisis, this paper adds to the first strand of literature (Algan et al., 2017; Dal Bó et al., 2018; Dustmann et al., 2017; Lindgren and Vernby, 2016). Some studies also find that import competition from China leads to higher votes for extreme parties, especially for populist and far-right parties (Autor et al., 2020; Colantone and Stanig, 2018b; Dippel et al., 2016). Economic factors are also influencial for the outcome of the referendum of Great Britain to leave the EU (Carreras et al., 2019; Colantone and Stanig, 2018a; Fetzer, 2019). The second strand of related literature addresses the link between globalization concerns and an economic crisis. Bell and Blanchflower (2011) find that being unenmployed is associated with a more skeptical opinion of globalization. Lundsgaarde (2018) and Feasel and Muzumder (2012) show descriptive evidence for a decrease in positive attitudes towards globalization in European countries which is assumed to be associated with the economic recession in Europe. The third strand of related literature deals with the analysis of the relationship between the public opinion on globalization and the voting behavior. Mader et al. (2020) show that the attitude towards globalization is a relevant indicator for the voting behavior in Germany. While they find that a more positive attitude towards globalization is associated with a lower likelihood of voting for a German far-left party (Die Linke) and a far-right party (Alternative für Deutschland), Bell and Blanchflower (2011) findings suggest that right-wingers have positive attitudes towards globalization. The results of De Vries and Hoffmann (2016) suggest that those people feeling close to populist parties are mainly motivated by their fear of globalization. As Mader et al. (2020) point out, it is important to analyze how attitudes towards globalization affect the election outcome in other countries. The paper at hand addresses this question

on a European level and extents the literature by consistently dealing with the economic causes and electoral consequences of globalization concerns.

The paper is structured as follows: The subsequent Section 3.2 describes the underlying dataset, while Section 3.3 provides background information. Section 3.4 and Section 3.5 present the identification strategy of instrumental variable designs and their results. Section 3.6 provides the mediation analysis. Section 3.7 discusses the results and Section 3.8 concludes.

3.2 Data

The analyzed dataset consists of three different main sources, each covering one of the analyzed key variable. First, I exploit survey data in order to capture the regional public opinion on globalization. Second, the unemployment rate serves as an indicator for the European economic crisis. Third, data on vote shares for anti-establishment parties in national parliamentary elections is the basis for the analysis of the voting behavior. In the following, each data source is described in detail.

Public opinion on globalization: For the regional attitude towards globalization, I use survey data of the Eurobarometer.[1] The question of interest evaluates the inquiry "Please tell me to what extent you agree or disagree with the following statement: Globalization is an opportunity for economic growth".[2] Respondents could choose from 4 answer categories "totally agree", "tend to agree", "tend to disagree" and "totally disagree". Higher values of the variable on the attitudes towards globalization indicate the disagreement to this statement. The individual cross section data of the Eurobarometer covers more than 200,000 survey responses. I take the average of the individual answer values across years and regions to create a panel dataset. All regions are standardized on the same regional level (NUTS 1).[3] For a better interpretability of the empirical results, I take the logarithm of

[1] Eurobarometer is a survey data collection from the European Commission on cross sectional individual level. It consists of approximately 1,000 face-to-face interviews per country.

[2] Note that no definition of the term globalization was given.

[3] NUTS 1 are one official classification of territorial units of the European Union representing regions with, for instance, 590,667 inhabitants (Luxembourg) and up to 17,890,100 inhabitants (North Rhine-Westphalia) in 2017. The data of the Eurobarometer does not include the regions PT2, PT3 and FI2 which are only small parts or islands of Portugal and Finland. I standardize all data on the same NUTS 2013 classification.

the regional averages of the attitude towards globalization. Control variables from the Eurobarometer include the regional mean of respondents' birth year, gender, marriage status, the share of people who think they live in an urban area and who trust the national government.

Unemployment rate: The source of the economic variables in this study is Eurostat. The main variable of interest is the regional unemployment rate. Additional controls contain information on the regional migration and education level.[4]

European national parliamentary election outcomes: The European Election Database covering national parliamentary elections is the main source for the regional vote shares of each party before 2014. For election outcomes of national parliamentary elections after 2014, I mainly exploit the database by Alvarez-Rivera (2019). In other cases, a country's individual website is considered.[5] For the creation of a balanced panel dataset, the current distribution of parliamentary votes serves as a basis. Thus, the election outcome from one year is kept for the following years until a new election occurs.[6]

Classification of political parties: The final dataset only comprises regional vote shares for far-right, far-left, populist and Euroskeptic parties, respectively. The classification of parties by Rooduijn et al. (2019) categorizes European parties which won at least 2 percent of the votes in at least one national parliamentary election. According to Rooduijn et al. (2019), far-right parties are nativist and authoritarian (definition based on Mudde, 2007). In contrast, far-left parties perceive social and economic inequality as a consequence of the existing political and social structure. They oppose the present socio-economic system of capitalism and promote an alternative economic regime (definition based on March, 2012). In general, the political spectrum of populist parties varies between both poles of the political scale. They all share the idea that the society is split into two antagonistic

[4] The unemployment rate covers people aged between 15 and 74. Migration presents the regional demographic change of the crude net migration rate. Education is measured by the share of people aged between 25 and 64 with tertiary education (levels 5-8).

[5] There are no election results on regional level available for Romania and France. Therefore, the data is based on the overall election outcome on country level.

[6] This also includes election outcomes before 2008. For instance, the vote shares from the Belgian election in 2007 are taken for the years 2008 and 2009. In 2010, a new parliamentary election took place in Belgium, such that the vote distribution of parliamentary election changes in the dataset in 2010.

groups. The first group refers to "the people" while the second group presents "the corrupt elite"(Mudde, 2004). Euroskeptic parties are characterized by the rejection of the process of European integration and encompasses parties which promote to leave or not to join the EU (definition based on Taggart and Szczerbiak, 2004). Again, Euroskeptic parties can include either left or right wing parties. While the far-left blames the EU for being too liberal and pro-market, extreme right parties think of the EU as being too interventionist and pro-welfare state (Guriev, 2018). This and the former explanation of populist parties underline the overlapping classification of the four types of parties.[7]

Some studies analyze the voting behavior by using survey data (e.g., Inglehart and Norris, 2016; Guiso et al., 2017). In contrast, this paper's analysis is based on the actual election outcome. On the one hand, individual voting data might be an advantage because it can be directly linked to other individuals' characteristics. On the other hand, by relying on the survey answers of the respondents, not on the actual election outcome, the effect might be biased since the respondents could misreport which party they voted for because they do not want to reveal their political preferences. This bias can be avoided by analyzing election results.

In total, the final dataset covers 94 national parliamentary elections. The analyzed panel dataset compromises 84 (NUTS 1) regions covering the period from 2008 to 2017.[8] Those regions are located in 27 countries covering the European member states except of Malta and including the United Kingdom.[9]

3.3 Background information

All types of non-mainstream parties have gained vote shares in the last decades on average in European countries (see Figure C2 in the Appendix). These election outcomes in favor of different anti-establishment parties vary a lot across European countries, as Figure 3.2 shows. Far-left parties experience the largest support in

[7] Table C1 in the Appendix shows the correlation of vote shares of the different classified parties.

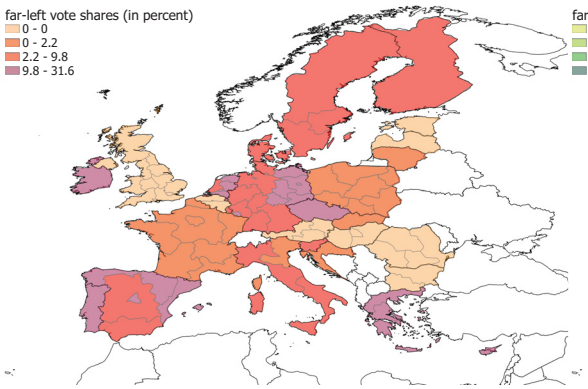
[8] To keep the sample constant across all specifications, the sample size reduces to 767 observations. Table C2 in the Appendix provides the summary statistics of the dataset used.

[9] Countries in the dataset are Austria, Belgium, Bulgaria, Cyprus, Czech Republic, Germany, Denmark, Estonia, Spain, Finland, France, UK, Greece, Croatia, Hungary, Ireland, Italy, Luxembourg, Latvia, Lithuania, Netherlands, Poland, Portugal, Romania, Sweden, Slovakia, Slovenia.

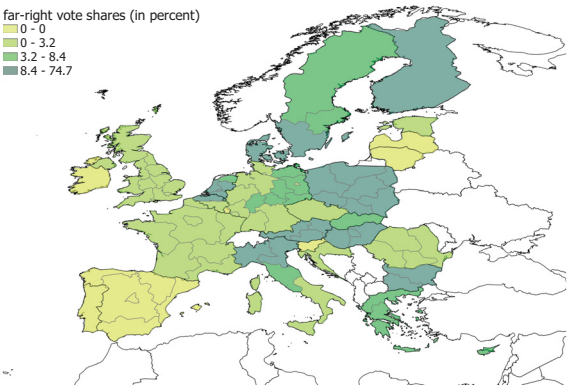
East Germany, Greece, Ireland, Portugal and parts of Spain. Far-right and populist parties are stronger in the East and North of Europe. While there exists no far-right party in Spain or Portugal and no far-left party in UK or Austria, populist and Euroskeptic parties occur in all European countries (expect for Portugal and Latvia, respectively). The regional distribution of vote shares of Euroskeptic parties does not show such a clear pattern but indicates strong Euroskeptic parties in the UK and the Eastern parts of Europe. Anti-establishment parties' vote shares do not only vary a lot across countries but can also be very heterogeneous within countries. The Belgian region Wallonia votes for populist parties on average by only one percent in the analyzed decade, while Flanders exhibit a populist vote share by about 18 percent at the same time on average. In Italy, both Northern regions (Northwest and Northeast) reveal the highest Italian far-right vote shares of twelve and 13 percent on average in the analyzed ten years but the Southern and Island parts of Italy vote for far-right parties by only two and three percent, respectively. The highest average election outcome in Germany for far-left parties refers to 25 percent in the East German region Saxony-Anhalt. In contrast, Bavarian citizens vote for far-left parties only by about five percent on average. Greece exhibits regions with a quite large heterogeneity in Euroskeptic vote shares, i.e., the region Attica (covering among others, Athens) records an election outcome for Euroskeptic parties over the period from 2008 to 2017 which is almost twice as large as the one in the region of Kentriki Ellada (Western, Central Greece). The vote shares of these different anti-establishment parties serve as main outcome variables in the analysis provided in the following Sections.

Figure 3.2: Vote shares for anti-establishment parties in national parliamentary elections 2008-2017

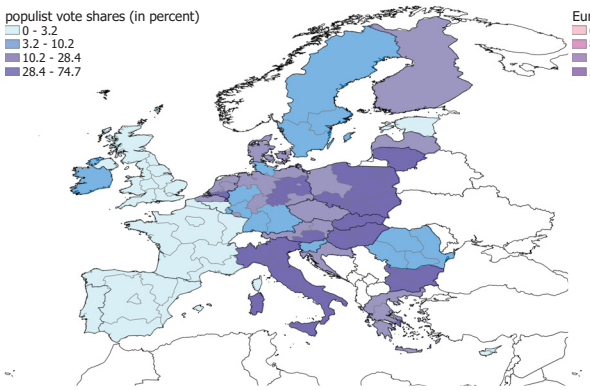
A. Far-left vote shares



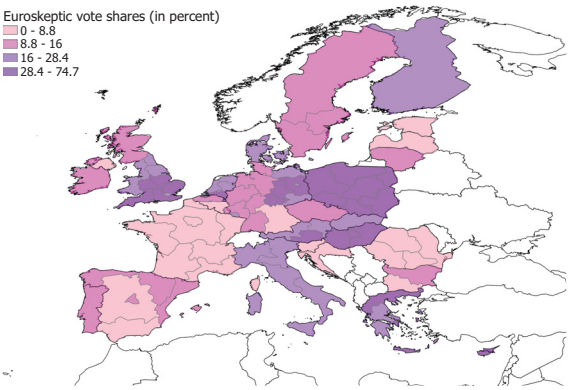
B. Far-right vote shares



C. Populist vote shares



D. Euroskeptic vote shares



Notes: The Figure shows the average vote shares of anti-establishment parties (in percent) between 2008 and 2017 on NUTS 1 level of the members states of the European Union with the exception of Malta, including the United Kingdom. *Source:* Own illustration based on the dataset described in Section 3.2.

The main variables of interest influencing the support of anti-establishment parties are the unemployment rate and globalization concerns. The unemployment rate has risen during the economic crisis on European average with a peak of about eleven percent in 2013. Figure 3.3 (A) maps the regional unemployment rate across Europe in 2013, indicating a division into North-West and South-East Europe. For instance, in 2013, the Spanish unemployment rate was 26 percent, while the German unemployment rate was five percent at that time. The unemployment rate exhibits substantial heterogeneity also within countries and even in countries hit hardest by the economic crisis. In 2013, the unemployment rate was 20 percent

in the South of Italy and only eight percent in the North-East of Italy. In the empirical analysis, I exploit this variation on regional level in order to capture the differences in the hit of the economic crisis across European regions. I expect that the economic crisis, reflected in a higher unemployment rate, leads to more severe anti-globalization attitudes and increases the voting behavior in favor of anti-establishment parties.

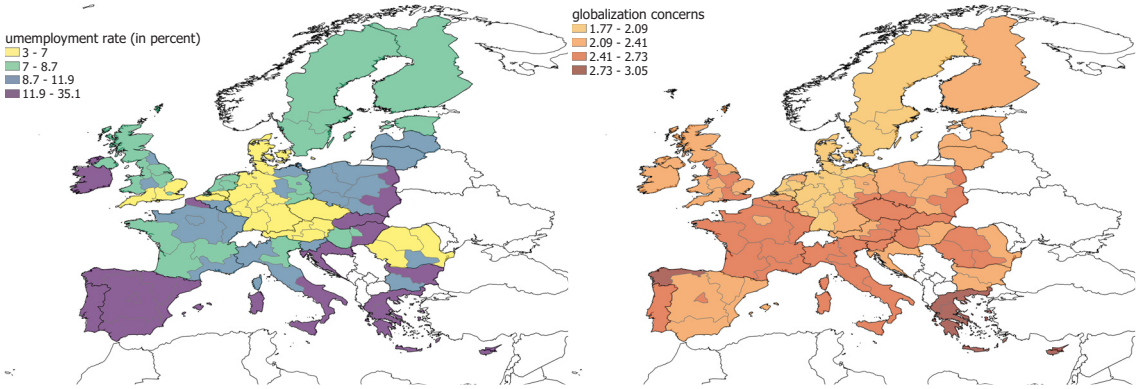
The regional average survey response to the statement "globalization is an opportunity for economic growth" serves as basis for measuring the attitudes towards globalization. Higher values indicate that a larger proportion of residents disagree with this statement. Regions experiencing a larger economic growth show on average a higher acceptance to the statement that globalization fosters economic growth, as Figure C3 in the Appendix indicates. Assuming that economic growth has a positive connotation and beneficial consequences, agreeing to the analyzed statement can be interpreted as a positive attitude towards globalization. Therefore, higher values of the indicator point to larger globalization concerns. The attitude towards globalization became more negative during the period from 2008 to 2017 on average with a peak in anti-globalization attitudes in 2012 and 2014 (Figure 3.3 in the Appendix). Figure 3.3 (B) maps the attitudes towards globalization of European regions in 2013. Overall, Northern European countries are more positive about globalization than other regional country groups.[10] Again, the variation in the public opinion on globalization is quite high across European countries but also within countries. Although on average Germany shows a rather optimistic attitude towards globalization, its regions extremely differ. In 2013, Hamburg has the most positive opinion on globalization in Germany, while Saxony exhibits the most negative German attitude towards globalization which is even above the European average. In the empirical analysis, I use this variation of globalization concern across regions first, as dependent variable and second, to measure its effect on voting for anti-establishment parties.

[10] See also Figure C1 in the Appendix.

Figure 3.3: Unemployment rate and regional attitude towards globalization in 2013

A. Unemployment rate

B. Globalization concerns



Notes: The Figure shows the unemployment rate (in percent) and attitudes towards globalization (indicator varying between 1 and 4) on NUTS 1 level of the members states of the European Union with the exception of Malta, including the United Kingdom. Higher values of the indicator for the regional attitude towards globalization represent higher regional average of not agreeing to statement that globalization is an opportunity for economic growth. *Source:* Own illustration based on the dataset described in Section 3.2.

In order to form expectations for the effect of the peoples' globalization concerns on anti-establishment parties' election outcomes, one need to consider the different parties' position on globalization. Populist parties see globalization as a process that only benefits the elite, which is clearly rejected by populist parties (Andersen et al., 2017). Since populism does not follow a clear political direction and can be associated with the left or right political spectrum, populist parties can promote anti-globalization tendencies with different reasonings depending on their political placement. Far-right could support anti-globalization tendencies due to nativism and anti-immigration sentiments. On the other side of the political scale, far-left parties tend to have anti-globalization tendencies based on their rejection of the underlying socio-economic structure (Kriesi and Pappas, 2015). Since the EU is a supra-national organization representing globalized economic and political progress, Euroskeptic parties can also represent a type of anti-globalization attitude. In the following Sections, I evaluate which type of party benefits from anti-globalization attitudes.

3.4 Empirical identification

As the previous Section indicates, besides the rise of anti-establishment parties, two other phenomena occurred in Europe which are likely to influence the voting behavior. First, the European economic crisis measured by the unemployment rate and second, anti-globalization tendencies. This Section presents the first research step of the paper which examines the link between the three phenomena one by one. Concerns of endogeneity (mainly due to omitted variable bias) arise when estimating the electoral consequences of social attitudes or of economic developments as well as when analyzing the effect of peoples' public opinion on their voting behavior. Therefore, I rely on identification using instrumental variable designs. The identifying assumptions of the applied instruments are presented in the following subsections in more detail.

The effect of the regional unemployment rate on anti-establishment parties' election outcomes and globalization concerns

The effect of the European economic crisis is measured for two outcomes; first, the election of anti-establishment parties and second, regional attitudes towards globalization. Accordingly, the corresponding estimation equation reads:

$$y_{rt} = \beta_0 + \beta_1 unemployment\ rate_{rt} + \beta_2 M'_{rt} + \beta_3 X'_{rt} + T_t + R_r + \epsilon_{rt} \tag{3.1}$$

whereby the dependent variable denoted by y_{rt} either indicates $globalization\ concerns_{rt}$ or $vote\ shares_{rt}$. $globalization\ concerns_{rt}$ measures the logarithm of the regional globalization concerns of NUTS 1 region r in year t. The variable $vote\ shares_{rt}$ represents the national parliamentary vote distribution of specific anti-establishment parties, i.e., far-right, far-left, populist or Euroskeptic parties. The variable of interest $unemployment\ rate_{rt}$ is the regional unemployment rate capturing the hit of the European economic crisis. M'_{rt} includes socio-economic controls. Since migration (inflow and outflow) is expected to be correlated with attitudes towards globalization and vote shares of anti-establishment, especially of far-right parties, I include the rate of net migration as a control in M'_{rt}. Based on the Samuelson-Stolper Theorem (Stolper and Samuelson, 1941), people with low education in more advanced economies, that are rather well endowed with human

capital, are more likely to oppose globalization than high-skilled workers in those countries. Since the education level is assumed to affect not only the attitude towards globalization but it is also expected to have an impact on the regional unemployment rate and election outcomes, it is part of the economic controls. Regional means of respondents' information concerning the gender, marriage, birth year, urban area and trust in the government are added to the regression as survey control variables indicated by X'_{rt}. By including the regional mean of respondents' trust in the government and the regional net migration rate, I control for the two main explanatory factors of the rise of populists on which the existing literature has mainly focused so far. T_t are year time fixed effects of each year from 2008 to 2017. R_r captures region fixed effects on NUTS 1 level. ϵ_{rt} indicates the errors term which captures other factors that affect the voting behavior or attitudes towards globalization which cannot be controlled for. In the main specification, standard errors are clustered at the regional level (84 NUTS 1 regions).

When estimating the effect of the regional unemployment rate on the voting behavior or on attitudes towards globalization different concerns of endogeneity arise. First, the unemployment rate might not pick up the effect of time varying regional factors which capture other aspects of the economic crisis. For instance, in times of an economic recession, wage drops are very common, are associated with a higher unemployment rate and can trigger higher votes for anti-establishment parties. Omitting this variable is likely to lead to an upward bias of Ordinary Least Squares (OLS) estimates. Further, the measurement of the official unemployment rate can be noisy. It might not sufficiently capture the effects concerning activities of the shadow economy. Regions with higher levels of activity in the informal economy are expected to have more critical views on globalization and higher vote shares for anti-establishment parties. Since the relationship between the informal economy and the unemployment rate is controversially discussed and can vary between countries (Mauleón and Sardà, 2017), the direction of the bias caused by the omission of the factor shadow economy is not clear. Second, the issue of sorting might be an issue. People might self-select into regions based on their social and political attitudes. Firms also select into regions with certain economic characteristics like infrastructure or specific human capital which cannot be completely controlled for. This would lead to economically (under)developed regions which have a corresponding unemployment rate that, in turn, can shape

the election outcome or citizen's attitudes. Considering sorting into economically (under)developed regions, OLS would be biased upwards. The issue of sorting however, represents long-term effects, while the paper rather looks at the short-term. A third potential concern arises from reverse causality. One could argue that the rise of anti-establishment parties has triggered the European recession. However, the argument of reverse causality between the unemployment crisis and the rise of anti-establishment parties seems negligible in the short-term. The corresponding timing is different since the main rise of the different anti-establishment parties occurred in the aftermath of the unemployment crisis with its peak in 2013 (see Figure C2 in the Appendix).

To address these endogeneity concerns an instrumental variable approach is considered. The construction sector is a good indicator for the impact of the European unemployment crisis because the construction industry is one of the sectors most affected in times of economic recessions, while it also plays a major role in boosting the economy again through stabilizing employment (Thakurta, 1970). Following Algan et al. (2017), the regional unemployment rate is instrumented by the regional share of gross value added of the construction sector. The respective first stage estimation takes the form:

$$unemployment\ rate_{rt} = \alpha_0 + \alpha_1 construction_{rt} + \alpha_2 M'_{rt} + \alpha_3 X'_{rt} + T_t + R_r + w_{rt} \tag{3.2}$$

The identification strategy relies on three main assumptions. First, the regional share of value added of the construction sector needs to significantly affect the regional unemployment rate. The result in Table C3 in the Appendix shows that the regional share of gross value added of construction has a significant impact on the regional unemployment rate. A decrease in the share of the value added of the construction sector is associated with an increase in the regional unemployment rate which is reflected in a sufficiently large F-statistic. This also holds when controlling for other industry shares of gross value added. Second, the exclusion restriction, i.e., that the construction sector affects the voting behavior and globalization concerns only via the channel of unemployment, needs to hold. The construction sector could possibly affect attitudes and voting behavior also via other mechanisms like human capital, immigration, or corruption. Algan et

al. (2017) examine the potential channel of corruption and find no evidence for a significant correlation between the construction sector and different measurements of corruption. This paper accounts for the other two possible channels when controlling for the determinants of migration and education. Hence, other factors besides unemployment driving the effect of the construction sector on the voting behavior or on attitudes towards globalization cannot be fully ruled out but do not seem to be too plausible since the most realistic channels are ruled out or controlled for. Third, the assumption of independence of the instrument requires that voting for anti-establishment parties or globalization concerns have no impact on the gross value added of the construction sector. A violation of the third assumption does not seem to be a great concern. Attitudes towards globalization are unlikely to have an impact on the gross value added of the construction sector. Also, the success of anti-establishment parties does not seem to have a substantial effect on the regional gross value added of the construction sector because anti-establishment parties are often part of the political opposition and not part of the government.[11] In the case that anti-establishment parties experienced governmental participation this has been rather recently. Hence, even if they would have had an impact on the construction sector this could be assumed as negligible.

The effect of regional globalization concerns on anti-establishment parties' election outcomes

The second factor which is expected to influence the voting behavior in favor of anti-establishment parties are anti-globalization attitudes. To capture the relationship between globalization concerns and the election of anti-establishment parties, the following estimation is conducted:

$$vote\ shares_{rt} = \sigma_0 + \sigma_1 globalization\ concerns_{rt} + \sigma_2 M'_{rt} + \sigma_3 X'_{rt} + T_t + R_r + u_{rt} \tag{3.3}$$

where $vote\ shares_{rt}$ represents different regional anti-establishment parties' vote shares in national parliamentary elections and $globalization\ concerns_{rt}$ is again

[11] In 2017, only eight out of 27 countries exhibit populist parties with governmental participation, namely Austria (FPÖ), Greece (Syriza), Italy (5-Star-Movement), Czech Republic (ANO), Hungary (Fidez), Bulgaria (GERB) and Slovakia (SMER, SNS). Note that only in six of these eight countries, the head of government belongs to the elected populist party (Greece, Poland, Czech Republic, Hungary, Bulgaria, Slovakia).

defined as the logarithm of the regional average of attitudes towards globalization across years. M'_{rt} represents socio-economic control variables, while X'_{rt} indicates survey control variables. T_t and R_r are time and region fixed effects. The error term is denoted by u_{rt}.

In general, concerns of endogeneity arise when estimating the electoral consequences of people's attitudes. First of all, there might exist time-varying regional factors of unobservable (social or political) values which correlate with attitudes towards globalization and which shape the voting behavior. This applies, for instance, to the degree of traditional values of the population. Since people with strong traditional values are expected to be more skeptical about globalization and more likely to support extreme parties, omitting this variable leads to upward biased OLS estimates. Reverse causality between attitudes and anti-establishment parties' success could present a second concern. Social attitudes could be triggered and reinforced due to anti-establishment parties becoming politically stronger, more prominent and salient. Thus, due to the success of non-mainstream parties and the growing prominence of their ideologies, globalization concerns could have become more severe. Third, the issue of sorting might be a potential concern as well. People may move into regions where the other citizens share their attitudes and values. If people sort into regions where their attitude represents the majority this is likely to affect regional election outcomes as well. Again, the issue of sorting indicates rather a long-term effect, while the analysis focuses on the short-term.

I account for these concerns by applying an instrumental variable approach to estimate the effect of regional attitudes towards globalization on election outcomes of anti-establishment parties. Therefore, the regional percentage share of households with broadband[12] access serves as an instrument for globalization concerns. The variable $globalization\ concerns_{rt}$ in equation (3) is substituted by the fitted values of following first stage regression:

[12] In contrast to narrowband connections broadband connections imply fixed broadband connections (DSL, ADSL, VDSL, cable, optical fiber satellite, public Wi-Fi connections) or mobile broadband connections (via mobile phone network, at least 3G, e.g., UMTS, using (SIM) card or USB key, mobile phone or smart phone as modem) (European Commission: https://eur-lex.europa.eu/legal-content/EN/TXT/HTML/?uri=CELEX:32014R1196&from=EN).

$$globalization\ concerns_{rt} = \delta_0 + \delta_1 broadband_{rt} + \delta_2 M'_{rt} + \delta_3 X'_{rt} + T_t + R_r + v_{rt} \tag{3.4}$$

The corresponding empirical identification strategy is based on three main assumptions. First, the share of households in a region having broadband access is expected to affect attitudes towards globalization through the change of the media consumption and the radicalization of internet users. By and large, broadband accounts for fast internet and other technical characteristics of high speed internet. In spite of all its benefits, the internet also reveals harmful effects on knowledge and beliefs that can arise due to false information in so called echo chambers. The latter can radicalize persons by fostering extreme ideas through the repetition in a closed environment that does not allow for alternative opinions (Brey et al., 2019). Liberini et al. (2020) provide evidence that the exposure to social media, especially facebook, reinforces polarization. The contents driving these radicalizations are in particular supported by broadband induced fast internet since broadband access does not only increase the quantity of internet usage but also the usage of specific types of contents, such as portals, entertainment and news (Hitt and Tambe, 2007). Due to this radicalizing effect of the internet based on the broadband expansion, a higher regional share of households having broadband access significantly relates to greater globalization concerns with a sufficiently large F-statistic (see Table C4).[13] Therefore, the instrument fulfills the relevance condition, which is further discussed in Section 3.5. Second, although the exclusion restriction is not testable, it seems plausible that the broadband access shapes peoples' attitudes towards globalization which in turn influence their voting behavior. It seems reasonable to assume that the regional broadband access itself does not have a direct effect on the election outcome but only via the attitudes of the people who vote.[14] It might be possible that

[13] This relationship of broadband access being associated with a more skeptical opinion on globalization is verified when examining regions which are optimistic in terms of GDP-adjusted globalization attitudes in more detail. A higher share of households having broadband access is associated with a lower likelihood of the region being a globalization-optimistic region, see Table C5 and the corresponding explanation in the Appendix.

[14] Besides changing people's attitudes, broadband could also affect the voting behavior by reducing the search costs of finding out where or who to vote for. This argument should translate into increasing voter turnouts. Voter turnouts in European national parliaments however, are declining (Beley, 2019). Therefore, the reduction of information or search costs does not appear to be a plausible channel through which broadband access influences voting behavior.

other attitudes besides globalization concerns drive the effect of broadband access on voting behavior. For instance, as Guriev et al. (2020) point out, mobile broadband expansion increases peoples' awareness of corruption and leads to higher vote shares for anti-establishment parties. The disillusionment regarding a corruptive government is likely to reduce the trust in the government. Since I control for the trust in the government, I can capture (at least part of) this channel. Third, the assumption of the independence of the instrument seems to hold since only few countries recently exhibit governmental participation or leadership of anti-establishment parties. Even if those governments would have affected the share of broadband coverage,[15] this would take some time to show its effects. Therefore, the effect of anti-establishment vote shares on broadband access presents a neglectable concern.

3.5 Results

This Section first provides the results of the effect of the unemployment rate on voting for anti-establishment parties and on globalization concerns. Second, the Section presents the results on the effect of globalization concerns on the electoral performance of anti-establishment parties.

The effect of the regional unemployment rate on anti-establishment parties' election outcomes and globalization concerns

Table 3.1 displays the effect of the regional unemployment rate on the four specific anti-establishment parties' vote shares (column (1) to (4)) and on the regional globalization concerns (column (5)). Panel A in Table 3.1 shows the OLS estimation results indicating that a higher unemployment rate is associated with higher vote shares for far-left, populist and Euroskeptic parties. As expected, an increased unemployment rate also relates to a more negative opinion on globalization, i.e., higher globalization concerns. Panel B in Table 3.1 depicts IV estimates concerning the effect of the instrumented regional unemployment rate on electing anti-establishment parties and on regional attitudes towards globalization. Following Staiger and Stock (1997), I conclude that the employed

[15] Literature shows that anti-establishment, in particular far-right parties have no or only limited impact on policies and only influence established parties in the field of immigration and integration policies (Muis and Immerzeel, 2017). Therefore, it is unlikely that anti-establishment parties affect the broadband expansion.

instrument is not weak since the overall F-Test of the first stage by far exceeds ten (see also Table C3 in the Appendix). IV estimates indicate that the regional unemployment rate significantly affects votes for far-left and populist parties. If the regional unemployment rate raises by one percentage point regional election results for far-left parties and populist parties increase on average by 1.6 and 1.1 percentage points. Considering that the European unemployment rate rose by almost 4 percentage points during the economic crisis (from 2008 to 2013), this increase could have raised far-left or populist votes by about 4.4 and 6.4 percentage points, assuming linearity. Since national electoral thresholds in Europe mainly vary between 3 and 5 percent, the effect of the unemployment crisis on anti-establishment parties could have resulted in those kind of parties possibly crossing the electoral threshold and entering the national parliament. The findings are in line with Kriesi and Pappas (2015) who provide insights on content-related differences between far-left and far-right parties especially in times of economic crisis. Far-right parties stress the cultural diversity of society, while radical left parties emphasize their anti-elitism in economic terms. This framing contributes to the rise of radical left parties in times of economic crisis. Therefore, countries hit hardest by the recession show a stronger rise of radical left parties.

The second effect of the European unemployment crisis concerns the regional public opinion on globalization. As indicated by OLS results, column (5) of Panel B in Table 3.1 shows that an increase in the unemployment rate by one percentage point leads to a rise in negative regional opinion on globalization by 1.4 percent on average. Again, assuming linearity and considering a total increase in the European unemployment rate during the European recession of 4 percentage points, this would lead to an increase in globalization concerns by 5.6 percent.[16]

[16] This would approximately refer to an increase of the European average of attitudes towards globalization indicator in 2013 from 2.35 to almost 2.5 (on a scale from 1 to 4).

Table 3.1: Effect of the unemployment rate on voting for anti-establishment parties and globalization concerns

	Anti-establishment parties' vote shares				
	(1) far-right votes	(2) far-left votes	(3) populist votes	(4) Euroskeptic votes	(5) globalization concerns
Panel A: OLS					
unemployment rate	0.024	0.270**	0.949***	0.885**	0.361**
	(0.173)	(0.131)	(0.302)	(0.354)	(0.150)
Panel B: IV					
unemployment rate	-0.339	1.591***	1.107***	0.644	1.425***
	(0.345)	(0.360)	(0.373)	(0.435)	(0.476)
Observations	767	767	767	767	767

Notes: This Table presents own estimation results based on the database described in Section 3.2. Robust standard errors (clustered at the NUTS 1 region level) are reported in parentheses. All regressions include NUTS 1 region and year fixed effects as well as controls. Controls in Panel A and B contain economic controls (high education, migration), survey controls (share of males, birth year, share of married people, urban region, trust in government). Panel B additionally includes gross value added industry share controls (trade, manufacturing, public, agriculture). Regional unemployment rate is instrumented by share of gross value added of construction sector. Kleibergen–Paap F statistic: 35.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

By and large, OLS underestimates the effects of the unemployment rate on the voting behavior and on globalization concerns. This applies to the case of far-left and populist votes shares and globalization concerns as dependent variables (see column (2), (3) and (5) in Table 3.1). The downward bias could be explained by the fact that the unemployment rate does not sufficiently capture factors such as the shadow economy. Although the relationship between the shadow economy and the unemployment rate is controversially discussed, there exists evidence that the shadow economy negatively affects the unemployment rate (Sahnoun and Abdennadher, 2019). People working in the shadow economy are expected to show higher support for anti-establishment parties and globalization concerns. Therefore, OLS estimates are likely to be biased downwards when estimating the effect of the unemployment rate on voting for anti-establishment parties and globalization concerns without accounting for the impact of the shadow economy. In the case of far-right and Euroskeptic parties' election outcomes as dependent variable, OLS

overestimates the effect of the unemployment crisis (see column (1) and (4) in Table 3.1). The unemployment rate as an indicator of the European economic crisis lacks measuring for example wage drops which are likely to appear in times of crisis with a high unemployment rate. Wage drops are also likely to positively influence peoples' critical opinion on globalization and anti-establishment voting behavior. Therefore, omitting this variable can lead to an upward bias of OLS estimates.

The results presented in this Section underline that the European unemployment crisis does not only have electoral consequences, but it also affects fundamental attitudes like globalization concerns. The following subsection further examines the link between this attitude and voting for anti-establishment parties.

The effect of regional globalization concerns on anti-establishment parties' election outcomes

This subsection presents the electoral consequences due to regional globalization concerns. OLS estimates in Panel A in Table 3.2 provide a first hint that globalization concerns positively relate to the success of populist parties and have an adverse relationship with voting for far-right versus far-left parties. Panel B in Table 3.2 shows the IV estimates. Following Staiger and Stock (1997), the instrument of broadband access is not weak since the corresponding F-Test is larger than ten (see also Table C4 in the Appendix). The regional attitudes towards globalization have a significant effect on all types of anti-establishment vote shares besides Euroskeptic votes. A more negative regional public opinion on globalization leads to higher vote shares of far-left and populist parties, whereas it decreases the election outcome of far-right parties. Consequently, if a region becomes more skeptical about globalization by one percent this gives rise to far-left and populist votes on average by 1.2 to 0.8 percentage points. In contrast, an increase in negative regional views on globalization by one percent causes a drop in far-right votes on average by about 1.2 percentage points.[17] Hence, the presented results hint to the fact that people, who are denying that globalization is boosting economic growth,

[17] Since the previous Section indicates that the regional unemployment rate has a significant effect on both, the vote shares for specific anti-establishment parties and globalization concerns, Table C6 in the Appendix shows an additional specification including the regional unemployment rate as further control variable. The results of the effect of globalization concerns especially with respect to far-left and far-right vote shares hold.

feel closer to far-left than far-right parties.

Table 3.2: Effect of globalization concerns on voting for anti-establishment parties

	Anti-establishment parties' vote shares			
	(1) far-right votes	(2) far-left votes	(3) populist votes	(4) Euroskeptic votes
Panel A: OLS				
globalization concerns	-0.058*	0.070*	0.074*	0.007
	(0.031)	(0.036)	(0.043)	(0.048)
Panel B: IV				
globalization concerns	-1.221***	1.215***	0.846*	0.533
	(0.368)	(0.464)	(0.462)	(0.533)
Observations	767	767	767	767

Notes: This Table presents own estimation results based on the database described in Section 3.2. Robust standard errors (clustered at the NUTS 1 region level) are reported in parentheses. All regressions include NUTS 1 region and year fixed effects as well as controls containing economic controls (high education, migration) and survey controls (share of males, birth year, share of married people, urban region, trust in government). Regional attitude towards globalization is instrumented by share of households having broadband access. Kleibergen–Paap F statistic: 11.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Two lines of argumentation could explain the contradicting effects of globalization concerns on far-left and far-right parties' election outcomes. First, the presented results combined with existing literature suggest that the electorate of far-right parties does not deny globalization's potential positive impact on economic growth but the consequences of globalization might be more essential for electing far-right parties. It seems that it is rather the outcome or the effect of globalization—like robotization, immigration or import competition[18]—which support far-right votes than strictly doubting that globalization is an opportunity for economic growth. Second, the opposing effect of the attitudes towards globalization on far-right and left vote shares could also be attributed to the different parties' areas of focus and their interpretation of issues resulting from globalization. The analyzed survey question captures the attitude towards the *economic* benefit of

[18] See, for instance, Anelli et al. (2019) on robotization, Dustmann et al. (2019) and Halla et al. (2017) on immigration and Dippel et al. (2016) on import competition.

globalization. Although far-right parties are mainly associated with nativism and protectionism, it seems that far-right parties rather emphasize the cultural diversity of the society and often re-frame economic conflicts in cultural terms. The literature finds a blurred economic position of far-right parties. The economic position plays a rather secondary role in the far-right parties' political ideology. Far-right parties seem to align its quite fragmented electorate under non-economic (socio-cultural) issues (Mudde, 2007; Rovny, 2013; Elias et al., 2015). This emphasizes the more prominent role of socio-cultural issues for the electorate of far-right parties than the economic position regarding globalization.[19] Conversely, far-left parties mainly underline their anti-elitism in terms of clearly rejecting the predominating *economic* system (Kriesi and Pappas, 2015). Here, the parties' economic position especially with respect to globalization seems to be essential for the electorate.

A comparison between the OLS and IV estimates reveals that OLS coefficients are smaller than the IV coefficients for all outcomes except for the effect on far-right support (see Table 3.2). The concern of an omitted variable bias with respect to the degree of traditionalism of the population could apply to the far-right election results as dependent variable.[20] However, the differences between OLS and IV estimates could be also driven by the local average treatment effect (LATE) of IV estimates that measures only the effect of the compliers, which are regions with increasing broadband access showing greater globalization concerns.

To conclude, the results presented in this Section suggest a reaction chain of the regional unemployment rate exacerbating mistrust in globalization which, in turn, affects the voting behavior. Since one cannot directly relate these effects with another the following Section provides a mediation analysis.

[19] The relationship of acknowledging globalization is positively affecting economic growth and strong far-right parties is exemplified by Scandinavian countries like Denmark and Finland. OLS regression on a Danish and Finish subsample – available upon request – show a significantly strong negative relationship between attitudes towards globalization and far-right votes (see also Figure 3.3 and Figure 3.2). In terms of economic position, the True Finns party favoring market regulation is regarded as most left-wing party among Scandinavian populist far-right parties. The Danish People's Party is characterized as less strong anti-establishment compared to other Scandinavian populist parties. Both parties, however, clearly emphasize nativist socio-cultural and anti-immigration issues (Nedergaard and Wivel, 2017; Strijker et al., 2015).

[20] People with strong traditional values are expected to be more skeptical about globalization. Based on the explanation of the cultural backlash theory by Inglehart and Norris (2016), supporters of populist far-right parties are characterized, among others, by strong traditionalism. The omission of the factor of traditionalism leads to an upward biased OLS coefficient.

3.6 Mechanism

The presented pattern of effects in the previous Section provide a basis for analyzing the question whether globalization concerns drive the effect of the unemployment rate on the voting decision. This Section introduces a mediation analysis in order to investigate this relationship in more detail.

Empirical strategy: Mediation analysis

To conduct the mediation analysis following separately estimated simple OLS regressions are considered:

$$\begin{aligned} globalization\ concerns_{rt} = \theta_0 + \boldsymbol{\theta_1} unemployment\ rate_{rt} \\ + \theta_2 X'_{rt} + \theta_3 M'_{rt} + T_t + R_r + \tau_{rt} \end{aligned} \tag{3.5}$$

$$\begin{aligned} vote\ shares_{rt} = \gamma_0 + \boldsymbol{\gamma_1} unemployment\ rate_{rt} + \boldsymbol{\gamma_2} globalization\ concerns_{rt} \\ + \gamma_3 X'_{rt} + \gamma_4 M'_{rt} + T_t + R_r + \phi_{rt} \end{aligned} \tag{3.6}$$

In comparison to the previous estimation equations, the variable $globalization\ concerns_{rt}$ occurs as dependent as well as independent variable in equation (5) and (6), respectively. A mediation analysis decomposes the total effect of the unemployment rate on the election outcome into a direct and an indirect effect. To calculate the indirect effect, the impacts of both, the unemployment rate and globalization concerns now need to be jointly estimated on the voting behavior as indicated by equation (6). The coefficients of interest for the mediation analysis are indicated in bold. In terms of the product coefficient method, $\gamma_2 * \theta_1$ present the indirect effect or mediation effect of the unemployment rate on the voting behavior through attitudes towards globalization, while $\gamma_2 * \theta_1 + \gamma_1$ is the respective total effect (see, e.g. Baron and Kenny, 1986; Fairchild and McDaniel, 2017).

Results: Mediation effect

In a first step, a joint significance test evaluates the evidence of mediation. The significance of both, the γ_2 coefficient and the θ_1 coefficient of equation (5)

and (6) provide evidence for mediation (Fairchild and McDaniel, 2017). This indicator of mediation however, only applies for the vote shares of far-left and right parties as dependent variable (see also Table C6 in the Appendix). As already described in Section 3.5, far-right votes are negatively linked to skeptical opinions on globalization (see Table 3.2). According to this, I conduct the mediation analysis only for the case of far-left parties' vote shares as outcome variable.[21]

Table 3.3: Mediation analysis regarding the channel of globalization concerns for the effect of the unemployment rate on voting for far-left parties

	(1) globalization concerns	(2) far-left votes
unemployment rate	0.361**	0.247*
	(0.159)	(0.130)
globalization concerns		0.062*
		(0.036)
Observations	767	767

Notes: This Table presents own estimation results based on the database described in Section 3.2. Robust standard errors (clustered at the NUTS 1 region level) are reported in parentheses. All regressions include NUTS 1 region and year fixed effects. Regressions includes survey controls (share of males, birth year, share of married people, urban region, trust in government) and economic controls (high education, migration).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The results of the mediation analysis show that the effect of the unemployment rate on voting far-left in column (2) in Table 3.3 is not completely mitigated when I include the mediator in the regression estimation. This indicates that no complete mediation but a proportion mediation effect occurs. The mediation analysis suggests that about eight percent of the total effect is mediated.[22] Put

[21] Note that, in general, it is methodologically problematic to run regressions where economic indicators and social attitudes are jointly included as explanatory variables in order to explain voting behavior. This issue is also stated by Colantone and Stanig (2019) (page 145). Equation (6) of the mediation analysis exemplifies this kind of regression. The fact that the joint significant test does not apply to vote shares of populist and Euroskeptic parties as outcome variables should not be interpreted as evidence that economic or social factors do not matter for electing those anti-establishment parties.

[22] The indirect effect of the regional unemployment rate on electing far-left parties is $a =$

differently, eight percent of the effect of the regional unemployment rate on electing far-left parties seem to be explained by doubts that globalization helps economic growth.

It is worth noting that the mediation analysis exhibits some shortcomings. In general, the causality of a mediation analysis is based on the assumption of no endogeneity. However, the estimation of the effect of the unemployment rate and globalization concerns on election outcomes clearly lack exogeneity, as discusses in Section 3.4. A further mediation analysis with unbiased estimations for globalization concerns and the unemployment rate based on instrumental variables is not feasible because the two applied instruments (gross value added of the construction sector and share of households having broadband access) are not independent of each other. This however, would be necessary for a mediation analysis including both instrumental variables (Frölich and Huber, 2017). Also, a causal mediation relying only on a single instrument as proposed by Dippel et al. (2017) is not applicable to this case. Then, the instrument for the treatment (here the share of value added of the construction sector) serves as a single instrument for the treatment and the mediator in estimation equation (5) and (6). The required assumption for this approach—that unobserved confounding factors which affect the mediator and treatment also affect the outcome variable primarily based on their effect on the mediator—seems not to hold. There might exist confounding factors affecting the unemployment rate and globalization concerns which influence the voting behavior without globalization concerns being the primary channel of influence. For instance, factors like the shadow economy could affect the unemployment rate and globalization concerns, as argued above. However, it seems reasonable to assume that it rather influences the voting behavior primarily via other attitudes (for instance, anti-immigration attitudes) than via the attitude towards globalization. Although one should treat the results of the mediation analysis with some caution, the results still suggest that there exists a certain share of the unemployment rate affecting the voting behavior which seems to be explained by the channel of globalization concerns.

$0.361 * 0.062$. The respective total effect is $b = 0.361 * 0.062 + 0.247$. This results in the proportion effect of $\frac{a}{b} = 0.083$.

3.7 Robustness of results

To test the robustness of the results of Section 3.5, I perform several additional estimations. I review the results using a restricted sample without extreme values of the variables of interest, weight regressions by the population, allow for correlated standard errors at the country level, consider country-group-period fixed effects and each election year's outcome instead of the current national parliamentary distribution.

It might be interesting to test whether the results are mainly driven by regions with a very high unemployment rate and great globalization concerns. Table C7 in the Appendix estimates the effect of the unemployment rate on the support for anti-establishment parties and on globalization concerns but restricts the sample to the 90^{th} percentile of the unemployment rate. Results show that when excluding regions with a very high unemployment rate, the effect of the unemployment rate on globalization concerns, far-left, populist and even Euroskeptic vote shares is significantly positive. When considering a subsample which excludes observations with very strong globalization concerns, it seems that in some cases extreme anti-globalization views are likely to drive the main results. As Table C8 in the Appendix indicates, the magnitude of coefficients decreases and the effects of globalization concerns are insignificant in OLS estimations, while the IV estimates for the effect of globalization concerns on far-right and far-left vote shares proves to be robust.

So far, the estimation specification allows errors terms to be correlated on the regional NUTS 1 level. To consider correlated error terms on a higher regional level, estimations in Table C9 and C10 rely on the main specification but use standard errors clustered on the country level. Since the dataset covers 27 countries, the estimations refer to p-values from a wild cluster bootstrap. The main effects presented in Section 3.4 are robust to clusters on country level.

The results presented in Section 3.5 give the same weight to all observations. However, as I analyze election results, it is reasonable to give greater influence to the more populous regions. The estimations in Table C11 and Table C12 in the Appendix are weighted by the number of inhabitants in each region in a given year. The results show coefficients of similar size and confirm the main findings.

Shocks might affect European regions differently. By including country-group-period fixed effects, I account for specific regional developments in Europe. As Table C13 in the Appendix shows, the inclusion of country-group-period effects considerably reduces the magnitude of the coefficients but it reveals robust OLS results (Panel A) and a significant positive effect of the unemployment rate on far-left parties' vote shares and on globalization concerns (Panel B). The OLS results for the relationship between globalization concerns and voting for anti-establishment parties are qualitatively similar but quantitatively smaller and insignificant (Panel A in Table C14 in the Appendix). The negative effect of globalization concerns on vote shares for far-right parties is robust to country-group-period effects in the IV specification (Panel B in Table C14 in the Appendix). However, since the regional share of households with broadband access and its expansion differ between but less within the regions of West, North and South-East Europe, country-group specific period fixed effects decrease the variation such that the instrument for globalization concerns is underpowered.

When, in addition, each election year's outcome instead of the current vote distribution in national parliaments is taken as basis the number of observations obviously drops. The main results of the effect of the unemployment rate on anti-establishment parties' vote shares and on globalization concerns are confirmed (Table C15 in the Appendix). The estimates for the effect of globalization concerns on far-left and populist vote shares are qualitatively similar but smaller in magnitude and insignificant. The impact of globalization concerns on voting for far-right parties is significanlty negative, as in the main results (Table C16 in the Appendix).

In general, one needs to keep in mind that the analysis in the main part relies on aggregated data. Results on individual level in Section 3.9.2 in the Appendix however, support the main take away of the results in Section 3.4. As the individual cross-section analysis in the Appendix reveals, the individual unemployment status and the regional unemployment rate is associated with a lower probability to perceive globalization as a positive driver for economic growth (Table C17 in the Appendix). Unemployed people rather allocate themselves to the left than to the right spectrum of the political scale (Table C18 in the Appendix). The individual attitude towards

globalization is significantly related to the self-placement on the political scale (Table C18 in the Appendix). Right-wingers seem to be more convinced that globalization helps economic growth, while having a positive attitude towards globalization is associated with a lower likelihood of feeling close to the left political spectrum.[23]

3.8 Conclusion

Anti-establishment parties of different political directions gained support in national parliamentary elections in European countries in the last decades. One factor causing the rise of anti-establishment parties is the European unemployment crisis. Also, the cultural backlash, negative attitudes towards immigrants or the trust erosion in political institutions are identified by the literature as major factors driving the success of anti-establishment parties (e.g., Inglehart and Norris, 2016; Dustmann et al., 2019; Algan et al., 2017). This paper identifies another attitude, that is globalization concerns, which facilitates the support for specific anti-establishment parties and is reinforced by the European unemployment crisis. By investigating the role of globalization concerns for the effect of the European unemployment crisis on voting behavior, this paper studies a corresponding mechanism behind the rise of anti-establishment parties.

The paper provides evidence that the European unemployment crisis does not only have a positive impact on the election of far-left and populist parties but it also enhances doubts that globalization is an opportunity for economic growth. Those greater globalization concerns, in turn, lead to higher vote shares for far-left and populist parties, respectively. A mediation analysis casts new light on this reaction chain. The channel of globalization concerns seems to explain about eight percent of the effect of the unemployment rate on voting for far-left parties. A more skeptical opinion on globalization leads to a higher vote share of far-left parties but to fewer votes for far-right parties. One would expect that voters of far-right parties share anti-globalization tendencies, for instance, based on nativism and protectionism. There exist two possible explanations for this contradicting effect. First, it seems that the electorate of far-right parties is per se not doubting that

[23] Note that the definition of right-wing and left-wing parties considered in the cross-section analysis is not necessarily congruent with the term of far-right and left parties based on Rooduijn et al. (2019) analyzed by the panel estimations.

globalization is an opportunity for economic growth but according to the literature, other topics related to globalization—like robotization, immigration or import competition—rather drive far-right votes (e.g., Anelli et al., 2019; Dustmann et al., 2019; Dippel et al., 2016; Dustmann et al., 2019). The second possible explanation relates to the different focus of the economic programs of far-left and far-right parties. The economic programs of far-right parties are often blurred and put more emphasize on non-economic, cultural issues (Mudde, 2007; Rovny, 2013; Elias et al., 2015). Therefore, the electorate of far-right parties could have a different attitude towards the economic benefit of globalization than expected. Far-left parties however, clearly emphasize their anti-elitism by rejecting the predominating economic system. This ties well with the finding that their electorate is denying a positive impact of globalization on economic growth.

A new European recession due to the current pandemic and tensions on the labor market could aggravate negative attitudes towards globalization and affect the voting behavior in favor of anti-establishment parties. The presented results show that one way to reduce the support for anti-establishment parties could be to respond effectively to economic problems during and in the aftermath of economic recessions. Additionally, the results underline the need to come to grips with the issue of anti-globalization tendencies in order to dissipate the appeal of far-left and populist parties in Europe.

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3.9 Appendix

3.9.1 Supplementary Materials and Robustness Checks

Table C1: Correlation between electoral results of different types of anti-establishment parties

Correlation of *far-right* vote shares with vote share for:	
far-left parties	-0.225***
populist parties	0.682***
Euroskeptic parties	0.658***
Correlation of *far-left* vote shares with vote share for:	
populist parties	0.0323
Euroskeptic parties	0.0792*
Correlation of *populist* vote shares with vote share for:	
Euroskeptic parties	0.495***
Observations	767

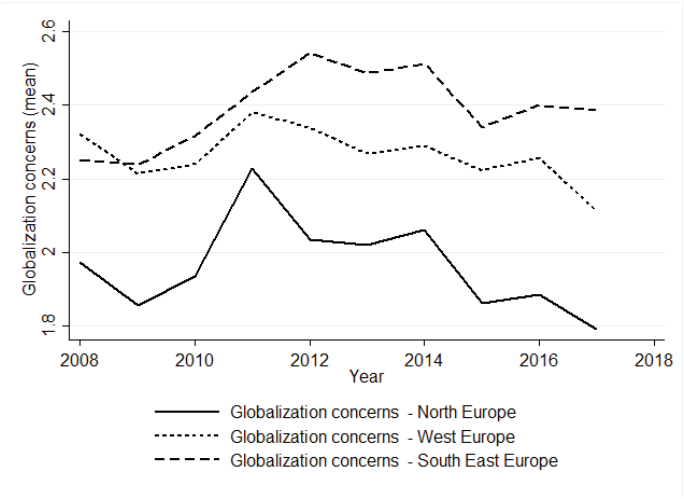
Notes: This Table presents the correlation matrix of electoral results of different types of anti-establishment parties. The classification of parties is based on Rooduijn et al. (2019). The election outcomes are based on the data described in Section 3.2.

Table C2: Summary Statistics

Observations: 767	Mean	SD	Min	Max
	(1)	(2)	(3)	(4)
Main variables:				
far-right vote shares	0.089	0.152	0.000	0.950
far-left vote shares	0.066	0.089	0.000	0.427
populist vote shares	0.187	0.189	0.000	0.950
Euroskeptic vote shares	0.217	0.177	0.000	0.950
unemployment rate	0.095	0.059	0.023	0.351
globalization concerns (log-transformed)	0.816	0.111	0.457	1.154
Instrumental variables:				
share of gross value added of construction sector	0.059	0.019	0.017	0.142
share of households with broadband access	0.705	0.173	0.100	0.970
Economic controls:				
crude net migration rate (per 1,000 inhabitants)	2.543	5.451	-25.2	34.6
share of high educated people	0.289	0.088	0.103	0.571
Survey controls:				
share of males	0.469	0.060	0.261	1.000
average birth year	1963	4.293	1949	1975
share of married	0.525	0.091	0.212	0.875
share of living in urban area	0.290	0.223	0.000	1.000
share of trust in government	0.340	0.167	0.000	0.870
Further controls:				
share of gross value added of trade sector	0.241	0.046	0.128	0.432
share of gross value added of public sector	0.225	0.046	0.120	0.342
share of gross value added of agriculture sector	0.023	0.022	0.000	0.119
share of gross value added of manufacturing sector	0.164	0.070	0.019	0.376

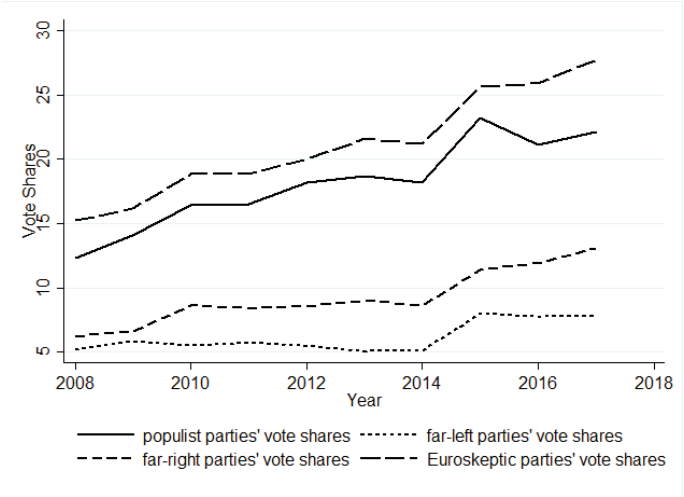
Notes: This Table presents descriptive statistics for all variables based on the data described in Section 3.2.

Figure C1: Attitudes towards globalization across European regions



Notes: This graph shows the attitude towards globalization across European regions based on Eurobarometer survey results 2008-2017. Higher values of globalization attitudes represent more negative attitudes. Country groups are North Europe (Denmark, Finland, Sweden, Ireland) West Europe (Austria, Belgium, Denmark, France, Netherlands, United Kingdom, Luxembourg) South-East Europe (Cyprus, Spain, Greece, Italy, Portugal, Bulgaria, Czech Republic, Eastland, Hungary, Poland, Romania, Slovenia, Slovakia, Croatia, Lithuania, Latvia).

Figure C2: Vote shares of anti-establishment parties in national parliamentary elections



Notes: This graph shows the vote distribution of national parliamentary elections for anti-establishment parties from 2008 to 2017 based on the database described in Section 3.2. The party classification is based on Rooduijn et al. (2019).

Table C3: Effect of the share of regional gross value added of the construction sector on the unemployment rate

	unemployment rate
construction share	-1.018***
	(0.173)
constant	-0.911
	(0.560)
F-test	35
Observations	767

Notes: This Table presents own estimation results based on the database described in Section 3.2. Robust standard errors (clustered at the NUTS 1 region level) are reported in parentheses. The regression includes NUTS 1 region and year fixed effects as well as controls. Controls contain economic controls (high education, migration), survey controls (share of males, birth year, share of married people, urban region, trust in government) and gross value added industry share controls (trade, manufacturing, public, agriculture).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C4: Effect of the regional share of households having broadband access on globalization concerns

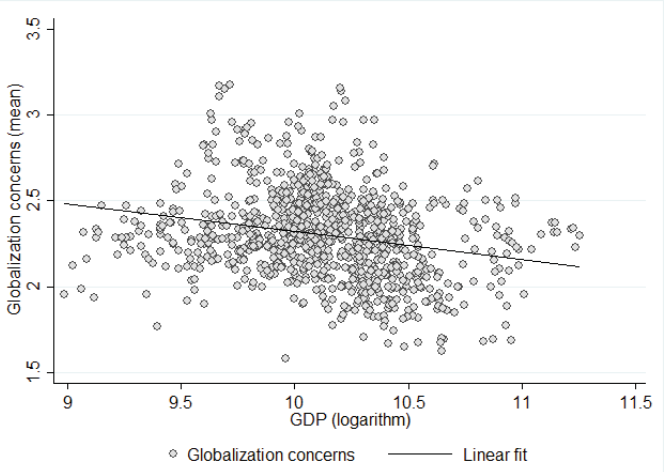
	globalization concerns
broadband share	0.198***
	(0.059)
constant	4.801**
	(1.919)
F-test	11
Observations	767

Notes: This Table presents own estimation results based on the database described in Section 3.2. Robust standard errors (clustered at the NUTS 1 region level) are reported in parentheses. The regression includes NUTS 1 region and year fixed effects as well as controls containing economic controls (high education, migration) and survey controls (share of males, birth year, share of married people, urban region, trust in government).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The relationship of broadband access, GDP and attitudes towards globalization

Figure C3 shows the relationship between the public opinion on globalization and Gross Domestic Product (GDP). Globalization concerns refer to the opinion whether globalization is an opportunity for economic growth. The dependent variable in Table C5 corresponds to regions being more optimistic than the relationship between globalization attitude and GDP would estimate. *optimistic* is a binary variable indicating whether residuals are below fitted values of the regression of globalization attitude on GDP. Optimistic regions are indicated by residuals below fitted values because lower values of globalization indicate more positive attitudes (see Section 3.2 for information on data). Table C5 shows that broadband access is associated with a lower likelihood of being an optimistic region in terms of GDP-adjusted globalization attitude.

Figure C3: Attitudes towards globalization and GDP



Notes: This graph shows the relationship between globalization concerns (measured by the regional average of Eurobarometer survey responses, see Section 3.2) and GDP (based on information provided by Eurostat). Higher values of globalization concerns indicate a more negative regional attitude towards globalization.

Table C5: Effect of the regional share of households having broadband access on an indicator for optimistic regions

	(1) OLS Model optimistic region	(2) Probit Model optimistic region
broadband	-1.427***	-3.678***
	(0.527)	(1.309)
constant	1.224***	1.866***
	(0.279)	(0.693)
Observations	767	430

Notes: This Table presents own estimation results based on the database described in Section 3.2 while data for GDP stems from Eurostat. Robust standard errors (clustered at the NUTS 1 region level) are reported in parentheses. The regression includes NUTS 1 region and year fixed effects. Optimistic is a binary variable indicating whether the residuals are below fitted values of regressing globalization attitude on GDP.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C6: Effect of globalization concerns on voting for anti-establishment parties including the unemployment rate as control

	Anti-establishment parties' vote shares			
	(1) far-right votes	(2) far-left votes	(3) populist votes	(4) Euroskeptic votes
Panel A: OLS				
globalization concerns	-0.059*	0.062*	0.044	-0.022
	(0.032)	(0.036)	(0.041)	(0.043)
unemployment rate	0.046	0.247*	0.933***	0.893**
	(0.175)	(0.130)	(0.302)	(0.354)
Panel B: IV				
globalization concerns	-1.261***	1.229**	0.791*	0.474
	(0.379)	(0.486)	(0.453)	(0.508)
unemployment rate	0.480*	-0.174	0.663*	0.714**
	(0.262)	(0.281)	(0.339)	(0.349)
Observations	767	767	767	767

Notes: This Table presents own estimation results based on the database described in Section 3.2. Robust standard errors (clustered at the NUTS 1 region level) are reported in parentheses. All regressions include NUTS 1 region and year fixed effects as well as controls containing economic controls (education, migration) and survey controls (share of males, birth year, share of married people, urban region, trust in government) and the regional unemployment rate. Regional attitude towards globalization is instrumented by share of households having broadband access. Kleibergen–Paap F statistic: 11.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C7: Effect of the unemployment rate on voting for anti-establishment parties and on globalization concerns, subsample of the 90$^{\text{th}}$ percentile of the unemployment rate

	Anti-establishment parties' vote shares				
	(1) far-right votes	(2) far-left votes	(3) populist votes	(4) Euroskeptic votes	(5) globalization concerns
Panel A: OLS					
unemployment rate	-0.013	0.389**	1.012***	0.977**	0.335**
	(0.223)	(0.163)	(0.330)	(0.399)	(0.161)
Panel B: IV					
unemployment rate	-0.299	1.398***	1.090**	0.949**	1.294***
	(0.382)	(0.379)	(0.438)	(0.461)	(0.431)
Observations	728	728	728	728	728

Notes: This Table presents own estimation results based on the database described in Section 3.2. Robust standard errors (clustered at NUTS 1 level) are reported in parentheses. All regressions include NUTS 1 region and year fixed effects as well as controls. Controls in Panel A and B contain economic controls (high education, migration), survey controls (share of males, birth year, share of married people, urban region, trust in government). Panel C additionally includes gross value added industry share controls (trade, manufacturing, public, agriculture). Regional unemployment rate is instrumented by share of gross value added of construction sector. Kleibergen–Paap F statistic: 35

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C8: Effect of globalization concerns on voting for anti-establishment parties, subsample of the 90th percentile of globalization concerns

	Anti-establishment parties' vote shares			
	far-right votes	far-left votes	populist votes	Euroskeptic votes
Panel A: OLS				
globalization concerns	-0.053	0.031	0.039	0.004
	(0.035)	(0.029)	(0.039)	(0.047)
Panel B: IV				
globalization concerns	-1.188***	0.853**	0.495	0.356
	(0.353)	(0.339)	(0.383)	(0.461)
Observations	728	728	728	728

Notes: This Table presents own estimation results based on the database described in Section 3.2. Robust standard errors (clustered at NUTS 1 level) are reported in parentheses. All regressions include NUTS 1 region and year fixed effects as well as controls containing economic controls (high education, migration) and survey controls (male share, birth year, married share, urban region). Regional attitude towards globalization is instrumented by share of households having broadband access. Kleibergen–Paap F statistic: 12

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C9: Robustness check with standard errors clustered on the country level: Effect of the unemployment rate on voting for anti-establishment parties and on globalization concerns

	Anti-establishment parties' vote shares				
	(1) far-right votes	(2) far-left votes	(3) populist votes	(4) Euroskeptic votes	(5) globalization concerns
Panel A: OLS					
unemployment rate	0.024	0.270	0.949	0.885	0.361
	[0.945]	[0.334]	[0.165]	[0.245]	[0.122]
Panel B: IV					
unemployment rate	-0.339	1.591**	1.107**	0.644	1.425*
	[0.619]	[0.014]	[0.029]	[0.330]	[0.070]
Observations	767	767	767	767	767

Notes: This Table presents own estimation results based on the database described in Section 3.2. P-values [in squared brackets] come from a wild bootstrap with clustering on country level (based on 999 replications). All regressions include NUTS 1 region and year fixed effects as well as controls. Controls in Panel A and B contain economic controls (high education, migration), survey controls (male share, birth year, married share, urban region and trust in government). Panel B additionally includes gross value added industry share controls (trade, manufacturing, public, agriculture). Regional unemployment rate is instrumented by share of gross value added of construction sector. Kleibergen–Paap F statistic: 11

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C10: Robustness check with standard errors clustered on the country level: Effect of globalization concerns on voting for anti-establishment parties

	Anti-establishment parties' vote shares			
	(1) far-right votes	(2) far-left votes votes	(3) populist votes	(4) Euroskeptic votes
Panel A: OLS				
globalization concerns	-0.058**	0.070	0.074	0.007
	[0.008]	[0.123]	[0.181]	[0.940]
Panel B: IV				
globalization concerns	-1.221**	1.215*	0.846	0.533
	[0.008]	[0.054]	[0.188]	[0.621]
Observations	767	767	767	767

Notes: This Table presents own estimation results based on the database described in Section 3.2. P-values [in squared brackets] come from a wild bootstrap with clustering on country level (based on 999 replications). All regressions include NUTS 1 region and year fixed effects as well as controls containing economic controls (high education, migration) and survey controls (male share, birth year, married share, urban region). Regional attitude towards globalization is instrumented by share of households having broadband access. Kleibergen–Paap F statistic: 11

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C11: Robustness check with weighted estimation by population: Effect of the unemployment rate on voting for anti-establishment parties and on globalization concerns

	Anti-establishment parties' vote shares				
	(1) far-right votes	(2) far-left votes	(3) populist votes	(4) Euroskeptic votes	(5) globalization concerns
Panel A: OLS					
unemployment rate	-0.111	0.260*	0.813***	1.064***	0.340*
	(0.150)	(0.131)	(0.244)	(0.386)	(0.172)
Panel B: IV					
unemployment rate	-0.528	1.734***	0.960**	0.619	1.588***
	(0.342)	(0.453)	(0.376)	(0.524)	(0.573)
Observations	767	767	767	767	767

Notes: This Table presents own estimation results based on the database described in Section 3.2. Standard errors (clustered at NUTS 1 level) are reported in parentheses. All regressions are weighted by the population in each region in a given year, include NUTS 1 region and year fixed effects as well as controls. Controls in Panel A and B contain economic controls (high education, migration), survey controls (share of males, birth year, share of married people, urban region, trust in government). Panel B additionally includes gross value added industry share controls (trade, manufacturing, public, agriculture). Regional unemployment rate is instrumented by share of gross value added of construction sector. Kleibergen–Paap F statistic: 28

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C12: Robustness check with weighted estimation by population: Effect of globalization concerns on voting for anti-establishment parties

	Anti-establishment parties' vote shares			
	(1) far-right votes	(2) far-left votes	(3) populist votes	(4) Euroskeptic votes
Panel A: OLS				
globalization concerns	-0.059**	0.081**	0.099**	0.083
	(0.026)	(0.031)	(0.042)	(0.066)
Panel B: IV				
globalization concerns	-1.180***	1.092***	0.918**	1.108*
	(0.337)	(0.328)	(0.377)	(0.632)
Observations	767	767	767	767

Notes: This Table presents own estimation results based on the database described in Section 3.2. Robust standard errors (clustered at NUTS 1 level) are reported in parentheses. All regressions are weighted by the population in each region in a given year, include NUTS 1 region and year fixed effects as well as controls containing economic controls (high education, migration) and survey controls (male share, birth year, married share, urban region). Regional attitude towards globalization is instrumented by share of households having broadband access. Kleibergen–Paap F statistic: 19

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C13: Robustness check with country-group-period fixed effects: Effect of the unemployment rate on voting for anti-establishment parties and on globalization concerns

	Anti-establishment parties' vote shares				
	(1) far-right votes	(2) far-left votes	(3) populist votes	(4) Euroskeptic votes	(5) globalization concerns
Panel A: OLS					
unemployment rate	-0.028	0.289**	0.838***	0.883**	0.263*
	(0.150)	(0.124)	(0.305)	(0.347)	(0.151)
Panel B: IV					
unemployment rate	-0.071	0.974***	0.202	-0.069	0.847*
	(0.397)	(0.337)	(0.503)	(0.583)	(0.481)
Observations	767	767	767	767	767

Notes: This Table presents own estimation results based on the database described in Section 3.2. Robust standard errors (clustered at the NUTS 1 region level) are reported in parentheses. All regressions include NUTS 1 region, year and country-group x period fixed effects as well as controls. Controls in Panel A and B contain economic controls (high education, migration), survey controls (share of males, birth year, share of married people, urban region, trust in government). Panel B additionally includes gross value added industry share controls (trade, manufacturing, public, agriculture). Country groups are North Europe (Denmark, Finland, Sweden, Ireland) West Europe (Austria, Belgium, Denmark, France, Netherlands, United Kingdom, Luxembourg) South-East Europe (Cyprus, Spain, Greece, Italy, Portugal, Bulgaria, Czech Republic, Eastland, Hungary, Poland, Romania, Slovenia, Slovakia, Croatia, Lithuania, Latvia). Periods correspond to 2008-2010, 2011-2013, 2014-2017. Regional unemployment rate is instrumented by share of gross value added of construction sector. Kleibergen–Paap F statistic: 24

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C14: Robustness check with country-group-period fixed effects: Effect of globalization concerns on voting for anti-establishment parties

	Anti-establishment parties' vote shares			
	(1) far-right votes	(2) far-left votes	(3) populist votes	(4) Euroskeptic votes
Panel A: OLS				
globalization concerns	-0.045	0.032	0.039	-0.022
	(0.032)	(0.031)	(0.039)	(0.044)
Panel B: IV				
globalization concerns	-1.743**	0.849	1.079	0.310
	(0.874)	(0.630)	(0.834)	(0.777)
Observations	767	767	767	767

Notes: This Table presents own estimation results based on the database described in Section 3.2. Robust standard errors (clustered at the NUTS 1 region level) are reported in parentheses. All regressions include NUTS 1 region, year and country-group x period fixed effects as well as controls including economic controls (high education, migration) and survey controls (share of males, birth year, share of married people, urban region, trust in government). Country groups are North Europe (Denmark, Finland, Sweden, Ireland) West Europe (Austria, Belgium, Denmark, France, Netherlands, United Kingdom, Luxembourg) South-East Europe (Cyprus, Spain, Greece, Italy, Portugal, Bulgaria, Czech Republic, Eastland, Hungary, Poland, Romania, Slovenia, Slovakia, Croatia, Lithuania, Latvia). Periods correspond to 2008-2010, 2011-2013, 2014-2017. Regional attitude towards globalization is instrumented by share of households having broadband access. Kleibergen–Paap F statistic: 3

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C15: Robustness check with each election year's outcome: Effect of the unemployment rate on voting for anti-establishment parties and on globalization concerns

	Anti-establishment parties' vote shares				
	(1) far-right votes	(2) far-left votes	(3) populist votes	(4) Euroskeptic votes	(5) globalization concerns
Panel A: OLS					
unemployment rat	-0.134	0.717***	1.441***	1.561***	0.485***
	(0.192)	(0.164)	(0.305)	(0.339)	(0.167)
Observations	165	171	183	193	756
Panel B: IV					
unemployment rate	-0.140	0.820**	1.238*	1.869***	1.705***
	(0.308)	(0.407)	(0.665)	(0.583)	(0.470)
Observations	143	149	178	188	756
Kleibergen–Paap F	14	97	16	19	37

Notes: This Table presents own estimation results based on the database described in Section 3.2 but each election year is taken as basis instead of a panel dataset on actual vote distribution in the national parliament. Robust standard errors (clustered at NUTS 1 level) are reported in parentheses. All regressions include country-group and period fixed effects as well as controls. Controls in Panel A and B contain economic controls (high education, migration), survey controls (share of males, birth year, share of married people, urban region, trust in government). Panel B additionally includes gross value added industry share controls (trade, manufacturing, public, agriculture). Country groups are North Europe (Denmark, Finland, Sweden, Ireland) West Europe (Austria, Belgium, Denmark, France, Netherlands, United Kingdom, Luxembourg) South-East Europe (Cyprus, Spain, Greece, Italy, Portugal, Bulgaria, Czech Republic, Eastland, Hungary, Poland, Romania, Slovenia, Slovakia, Croatia, Lithuania, Latvia). Periods correspond to 2008-2013 and 2014-2017. Regional unemployment rate is instrumented by share of gross value added of construction sector. Kleibergen–Paap F is the F statistic of the first stage.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C16: Robustness check with each election year's outcome: Effect of globalization concerns on voting for anti-establishment parties

	Anti-establishment parties' vote shares			
	(1) far-right votes	(2) far-left votes votes	(3) populist votes	(4) Euroskeptic votes
Panel A: OLS				
globalization concerns	-0.131*	0.087	0.009	-0.194
	(0.074)	(0.123)	(0.130)	(0.123)
Observations	165	171	183	193
Panel B: IV				
globalization concerns	-0.437**	0.771	0.240	-0.016
	(0.221)	(0.517)	(0.185)	(0.192)
Observations	116	142	147	157
Kleibergen–Paap F	16	5	17	18

Notes: This Table presents own estimation results based on the database described in Section 3.2 but each election year is taken as basis instead of a panel dataset on actual vote distribution in the national parliament. Robust standard errors (clustered at NUTS 1 level) are reported in parentheses. All regressions include country-group and period fixed effects as well as controls. Controls contain economic controls (high education, migration), survey controls (share of males, birth year, share of married people, urban region, trust in government). Country groups are North Europe (Denmark, Finland, Sweden, Ireland) West Europe (Austria, Belgium, Denmark, France, Netherlands, United Kingdom, Luxembourg) South-East Europe (Cyprus, Spain, Greece, Italy, Portugal, Bulgaria, Czech Republic, Eastland, Hungary, Poland, Romania, Slovenia, Slovakia, Croatia, Lithuania, Latvia). Periods correspond to 2008-2013 and 2014-2017. Regional attitude towards globalization is instrumented by share of households having broadband access. Kleibergen–Paap F is the F statistic of the first stage.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

3.9.2 Individual-level Analysis

This Section analyzes the individual responses based on Eurobarometer's cross section data. In total, I examine more than 220,000 individual responses to Eurobarometer's survey questions. First, the respondents' opinion on whether globalization promotes economic growth is examined. Therefore, the following regression is estimated:

$$globalization_{irt} = \beta_0 + \beta_1 unemployment_{irt} + \beta_2 X'_{irt} + T_t + R_r + p_{irt} \qquad (3.7)$$

where $globalization_{irt}$ is the attitude towards globalization of individual i in region r in year t. This variable is coded binary (positive opinion on globalization). The main variable of interest, $unemployment_{irt}$ is a dummy indicating whether the interviewed person is unemployed or not. X'_{irt} represents further personal characteristics like birth year, gender, marriage, age at the end of education, living in urban area and trust in government corresponding to the control variables applied in the panel analysis.

In Table C17 the dependent variable presents the individual positive opinion on globalization. This means totally agreeing or agreeing to the statement globalization is opportunity for economic growth. Results in Table C17 are based on a probit model with clustered robust standard errors at regional (NUTS 1) level. Interviewed people who are unemployed are significantly less likely to have a positive attitude toward globalization. In column 2 in Table C17, I interchanged the individual unemployment status by the regional unemployment rate. The regional unemployment rate is also negatively associated with positive opinion on globalization. This finding verifies the results of OLS and IV panel estimations presented in the main part of this paper (Section 3.5 and 3.6).

Table C17: Effect of individual factors on positive attitudes toward globalization

	(1)	(2)
	positive globalization attitude	positive globalization attitude
unemployment	-0.137***	
	(0.016)	
unemployment rate		-0.939**
		(0.399)
Observations	224,357	223,394

Notes: This Table presents own estimation results based on the database described in Section 3.9.2. Robust standard errors (clustered at the NUTS 1 region level) are reported in parentheses. The regression includes NUTS 1 region and year fixed. Controls are individual birth year, age of education, gender, marriage status, living in an urban area and trust in government.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Second, the link between the individual unemployment status and the individual political affinity is examined. The variable $rightist$ (or $leftist$ respectively) estimates whether the respondent assigns himself or herself to the right (left) spectrum of the political scale. Thereby, the estimation regression reads:

$$Y_{irt} = \beta_0 + \beta_1 unemployment_{irt} + \beta_2 X'_{irt} + T_t + R_r + s_{irt} \tag{3.8}$$

Interviewed persons answer the question how they would place themselves on a scale between 1 indicating the left and 10 presenting the right end of the political spectrum. Y_{irt} represents either the variable $rightist$ or $leftist$. $rightist$ indicates whether a person answered the question with a number higher or equal to 8. Corresponding to that, $leftist$ implicates whether the person assigns himself/herself to the left end, i.e., values below or equal to 3. Note that since this question allows for a wide range of interpreting the political scale, this indicator is rather poor to estimate the impact on the actual voting behavior of anti-establishment parties. The given answer concerning the political affinity does not need to be congruent to real election choice in parliamentary elections. Hence, the panel estimation use the election outcome on regional level as a more sophisticated measurement of the actual voting behavior.

Results of the probit model concerning the individual political affinity presented in Table C18 confirm the panel estimation results (Section 3.5). People having a positive attitude towards globalization are more likely to have a lower affinity with the left political spectrum and a higher one with the right side of the political scale. Unemployed respondents feel closer to the political left than the political right.

Table C18: Effect of individual factors on feeling close to left or right side of political scale

	(1) rightist	(2) leftist
positive globalization attitude	0.059**	-0.085***
	(0.025)	(0.018)
unemployment	-0.069***	0.068***
	(0.024)	(0.024)
Observations	183,749	164,249

Notes: This Table presents own estimation results based on the database described in Section 3.9.2. Robust standard errors (clustered at the NUTS 1 region level) are reported in parentheses. The regression includes NUTS 1 region and year fixed. Controls are individual birth year, age of education, gender, marriage status, living in an urban area and trust in government.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

4

Becoming Neighbors with Refugees and Voting for the Far-Right? The Impact of Refugee Inflow at the Small-Scale Level

Joint work with Lukas Hörnig and Sandra Schaffner

4.1 Introduction

In many Western European countries, the electoral success of far-right parties has increased in the last decades. Until 2017, Germany was one of the last European countries without a far-right party in the parliament.[1] In the 2017 federal election however, the Alternative for Germany (AfD) became the first far-right party which entered the German federal parliament since the second World War and represented with 12.6 % the third biggest parliamentary group. The rise of authoritarian populist parties, like the AfD, can destabilize liberal democracies (Norris and Inglehart, 2019). Moreover, higher support for far-right parties can also have economic consequences. Because of its low fertility rates, Germany needs immigration in general and especially of high skilled workers. Strong far-right parties, however, put pressure on established parties to implement restrictive immigration and integration policies (see Muis and Immerzeel, 2017, for a literature review). Further, they create a xenophobic atmosphere that makes it less attractive for migrants to immigrate to Germany.

The increased refugee inflows into Europe in 2015 and 2016 were challenging for the societies and raised concerns that inflows increase support for far-right parties, causing an academical and political debate about the overall effect of the large refugee inflow on the voting behavior. The literature finds that negative attitudes towards immigration foster the rise of far-right parties (see, e.g., Van der Brug et al., 2000; Lubbers and Scheepers, 2000; Norris and Inglehart, 2016).[2] These negative attitudes may stem from natives fearing for their economic resources (e.g., jobs, public goods, housing), consistent with the racial threat theory (Campbell, 1965; Quillian, 1995). However, the contact theory by Allport et al. (1954) suggests that natives and immigrants living in close proximity to each other increase the likelihood of encountering each other, thus reducing stereotypes and anti-immigrant attitudes. While the AfD's manifesto is openly xenophobic since the large refugee inflow into Germany in 2015 (Schmitt-Beck, 2017; Heckmann, 2016), its support

[1] Besides in Germany, also in Cyprus, Estonia, Portugal, Spain and in the United Kingdom a far-right party (according to the classification of Rooduijn et al. 2019) entered the national parliament for the first time in the last decades.

[2] Other factors driving the recent rise of far-right parties in European countries are for instance, the effects of import competition from China (Dippel et al., 2015), the European Great Recession and political distrust (Algan et al., 2017), robotization (Anelli et al., 2019) or overall, the cultural backlash (Norris and Inglehart, 2016).

is much stronger in East Germany than in West Germany.[3] Since the proportions of residents with a migration background or refugees are significantly lower in East Germany than in West Germany, this observation points at the contact theory at first glance.[4] To consistently investigate whether contact with refugees fosters greater understanding or economic threats, it is necessary to examine the immediate surrounding in which refugees and natives meet.

In this paper, we investigate the relationship between refugee inflows and far-right votes in East and West Germany separately and on a highly granular level. We collect and exploit data on small-scale results from the federal election in 2017 combined with socio-economic and demographic characteristics on 1km x 1km grid cell level. Using this unique small-scale data allows us to analyze the effect of refugee inflows into the immediate neighborhood on far-right voting. The small-scale analysis is an important feature to test the validity of the contact hypothesis by Allport et al. (1954) as its necessary condition is the immediate interaction between foreigners and natives. Other studies might not be able to capture effects based on the contact theory because the mechanisms are concealed in analyses at aggregated levels. Contact theory requires that natives and refugees meet, but this might not be true for most of a county even with high inflow rates since the inflow could only take place in very few neighborhoods. Hence, studies on higher level overlook important factors like a historically persistent ethnic segregation in Germany (Glitz, 2014). There exists a non-negligible segregation of people from typical refugee countries on county level in Germany wich ranges from 0.08 to 0.35 (with a mean of 0.21). In our neighborhood-level analysis, we exploit the considerable intra-county variation at 1km x 1km cells such that locals are very likely to actually meet refugees living in the same cell. However, we analyze both the relationship of AfD support and inflows into the neighborhood, but also with inflows at the county (NUTS 3) level. By exploiting this aggregated scale, our results are comparable with previous findings of the literature. Further, voters can perceive refugees not only if they move into the same neighborhood, but also when

[3] The number of asylum applications increased by more than 500,000 from 2014 to 2016 (Federal Office for Migration and Refugees, 2016). The AfD gained on average 10.7 % of votes in West Germany and 21.6 % in East Germany in the federal election 2017 (own calculations based on data by the electoral management body (Bundeswahlleiter)).

[4] The share of foreign population in West Germany is 12.9 % and 4.4 % in East Germany in 2017 (German Federal Statistical Office, 2018).

refugees locate in the same county. This is fostered as local media coverage about refugees increases (Steinmayr, 2021) which leads to citizens knowing that refugees live in the same county without directly having contact with them. Therefore, we exploit the variation on county level and analyze the effect of the refugee inflow into a higher aggregate level as well. The relationship at this scale is potentially driven by other mechanisms than the contact at the neighborhood level. At this more aggregated level, it could be the fear of economic loss or changes in local policies.

The institutional setting for the allocation of refugees in Germany eases worries of sorting to some extent. In first place, the allocation of refugees was based on rather exogenous factors like housing capacities. During the official asylum process, refugees have to stay within a predefined area which mainly corresponds to the respective county. Depending on the different federal states' law, refugees who have completed the asylum process, however, were allowed to change residence within the federal state, county, or municipality. We alleviate remaining endogeneity concerns by constructing an instrumental variable approach based on the assumption that refugees settle in regions where other people with a migration background from their country of origin live (see Altonji and Card, 1991; Card, 2001). While there is an extensive literature using past settlements to predict current immigration shocks (e.g., Barone et al., 2016; Halla et al., 2017; Otto and Steinhardt, 2014), in recent years, criticism on this method appeared (Jaeger et al., 2018; Clemens and Hunt, 2019), inter alia, because long-term effects of past immigration might be confounded with the short-term effects of current immigration. We argue that the method is reliable in this context because the share of immigrants from today's typical refugee countries was not large enough to influence the majority society before 2014, while being large enough to constitute ethnic networks – at least in West Germany, so the instrument is not weak. This view is supported by placebo regressions using the 2013 federal election at the municipality level as well as by the large first-stage's F statistic (in West Germany).

Our results show that the inflow of refugees within a neighborhood has a negative effect on the support for the AfD in West Germany. This impact is driven by grid cells in urban counties, while the relationship in very urban and rural counties is insignificant. For all East Germany, we find very large but imprecise effects leading to an insignificant relationship between the inflow of refugees and voting

for the AfD. The insignificance partly changes when investigating heterogenous effects by urbanity: In very urban areas in East Germany, the refugee inflow has a significant negative effect on the AfD support on county level. Again, although large in magnitude, the relationship in rural counties is not significantly different from zero. We show that the effect of the spatial scale of analysis differs within West and also between West and East Germany. While in urban areas in West Germany the grid cell inflow rate is negatively affecting AfD votes, the refugee inflow into the county in West German urban areas increases the AfD support. In contrast, in East German very urban areas, the inflow of refugees in the county decreases far-right votes. These results suggest that in urban West Germany the contact theory seems to be valid, i.e., high inflow rates within the own neighborhood are assumed to facilitate contact of natives with refugees and to foster mutual understanding which, in turn, leads to lower AfD vote shares. However, other mechanisms on the more aggregated county level seem to drive the positive effect of refugee inflow on the AfD election outcome in West German urban areas. This also applies to the negative impact of the county level refugee inflows on far-right election outcomes in very urban East Germany. Those factors, among others, could cover an increased media coverage of refugee allocation in the city, the cities' immigration and integration policies, contact with refugees in other places of the city beside the own neighborhood or the (not materialized) fears of loss.

Additionally, we investigate heterogeneities along other dimensions, i.e., quartiles of the neighborhood unemployment rate and shares of residents aged above 60. Contrary to the racial threat theory, we find a negative relationship of the neighborhood level refugee inflow and AfD support in West Germany also in regions with high unemployment rates. The negative effect of the inflows of refugees at the grid cell level in West Germany and at the county level in East Germany seem to be persistent only in regions with the lowest share of older residents, respectively.

To the best of our knowledge, we are the first to investigate the effects of refugees on far-right voting in Germany on neighborhood level taking heterogeneities between urban and rural areas into account. Hence, we add to the literature on the effects of refugees on the far-right voting behavior along two important dimensions. First, existing studies investigate the impact of refugees on a relatively high level of aggregation. Therefore, they cannot account for direct exposure of natives to

refugees on a small scale. One exception is the paper by Kretschmer and Kruse (2020) analyzing immediate neighborhood effects on attitudes, concluding that the share of refugees in the neighborhood influences German adolescents' attitudes whether immigrants should adopt to the German society. Further, we contribute to studies which demonstrate that the relationship between immigrants or ethnic minorities and far-right voting can vary across spatial scales (Della Posta, 2013; Janssen et al., 2019; Vasilopoulos et al., 2021). Second, we investigate differences between urban and rural areas. Urban-rural gaps with respect to the impact of immigration on far-right parties are well known in the literature. Harteveld et al. (2021) show that immigration is an important factor for far-right support in urban areas, whereas immigration cannot explain the variation in the voting behavior in favor of the far-right across rural areas. By contrast, Barone et al. (2016) and Dustmann et al. (2019) examine heterogeneities with respect to urbanity and find that the positive effect is driven by small municipalities, while the effect is smaller or even reversed in urban areas. Our results support their evidence by underlining that the negative effect at the neighborhood level in West Germany seems to be driven by urban areas and that the negative effect of county level refugee inflows on AfD-vote shares in East German very urban areas vanishes in the analysis of the total sample of East Germany.

While this work analyzes the voting behavior in favor of a far-right party in the aftermath of the large refugee inflow in 2015, several studies investigate the relationship between immigration and far-right voting before the immigration shock in several countries in 2015. Those studies mostly reveal a positive effect which is persistent across different Western European countries, e.g., Otto and Steinhardt (2014) for Germany (Hamburg), Barone et al. (2016) for Italy, Halla et al. (2017) for Austria, Harmon (2017) and Dustmann et al. (2019) for Denmark and Roupakias and Chletsos (2020) for Greece.[5] Evidence for effects of the recent inflow of refugees on far-right voting also points in the direction of a positive relationship in European countries (see Dinas et al., 2019 for Greece, Edo et al., 2019 for France, Campo et al., 2021 for Italy). Kellermann and Winter (2018) and Bredtmann (2020) show positive effects of refugees on far-right voting. Interestingly, the positive effect in

[5] In contrast to the studies identifying a positive effect, Dill (2013) finds evidence for Germany, which rather supports Allport et al. (1954)'s contact theory and indicates a negative relationship between foreigners and far-right voting in the period before the large refugee inflow in 2015.

Bredtmann (2020) is driven by refugees living in centralized accommodations. In contrast, Steinmayr (2021) finds evidence for the contact theory in Austria, i.e., that the interaction between asylum seekers and natives reduces far right votes. Studies that investigate a longer time period including the immigration shock in 2015 – as Tomberg et al. (2021) for Germany between 1998 and 2017 and Edo et al. (2019) for France between 1988 and 2017 – show positive effects of asylum seekers or of immigrants on far-right voting, respectively. In line with our insignificant results in rural areas, other studies also find no effect of refugees on far-right vote shares in different German areas (Schaub et al., 2021 for rural East German municipalities and Gehrsitz and Ungerer, 2017 for Baden Wurttemberg, Rhineland-Palatinate, and Saxony-Anhalt).

The paper is structed as follows: Section 4.2 describes the data set. Section 4.3 explains the estimation strategy. Section 4.4 reports our results, while Section 4.5 shows results for different parties, discusses possible transmission channels, and presents robustness checks of our regressions. Section 4.6 concludes.

4.2 Data and Institutional Setting

The developments of the large refugee inflow into Germany in 2015 and 2016 and the rise of the far-right party AfD in the aftermath were unique in German history since the second World War. This section highlights the institutional settings of refugee allocation within Germany and comments on the success of the AfD. Further, this section provides information on the used dataset. We combine different datasets for our analysis: data on voting behavior, data on refugee inflows and neighborhood characteristics and, finally, indicators for rurality of the different regions. The final cross section data set covers all Germany in the year 2017. In this year, the AfD entered the federal parliament for the first time and became the third biggest parliamentary group.

Refugee Inflow

Europe and especially Germany experienced a large inflow of refugees in 2015 and 2016. Hence, the number of asylum applications in Germany more than tripled in

2016 compared to 2014 (Federal Office for Migration and Refugees, 2017).[6] Due to the large and unexpected influx, the allocation of refugees was initially based on more exogenous factors such as housing capacity. Refugees have been assigned to one of the 16 federal states by predefined quotas (Koenigsteiner Schluessel). Within the federal states they were assigned to reception facilities. Each state has different rules for the distribution between counties. As long as refugees are within the asylum process, they have a domicile requirement which means that they have to stay within a predefined area. After asylum has been granted, the federal states[7] regulate the place of residence within the framework of the integration act[8]. In some states they must reside within the federal state, in some within the county and in some regions within the municipality.[9] In 2017, refugees could still be in the asylum process, but those who had already been granted political asylum were allowed to change their residence and dependent on the federal state's law also their county.[10]

For information on the refugee inflow and neighborhood characteristics we use the RWI-GEO-GRID data[11]. This dataset covers aggregate information for all Germany on the 1km x 1km grid cell level. The definition of grid cells follows the European INSPIRE regulation. For all grid cells we use information on the number of houses and residents. Further, we include the composition of residents by age, gender, and migration background in the analysis. Additionally, for each grid cell, we use information on the unemployment rate, aggregated net income and distribution of credit default risk.

[6] Total Asylum applications in 2016 (almost 750,000) also exceeded the figures in 1992 (430,000), which were the peak in asylum applications to date (Federal Office for Migration and Refugees, 2017).

[7] See https://www.asyl.net/fileadmin/user_upload/beitraege_asylmagazin/Beitraege_AM_2018/AM18-12_themenschwerpunkt_nach_asylverfahren.pdf for details regarding the implementation in the different federal states.

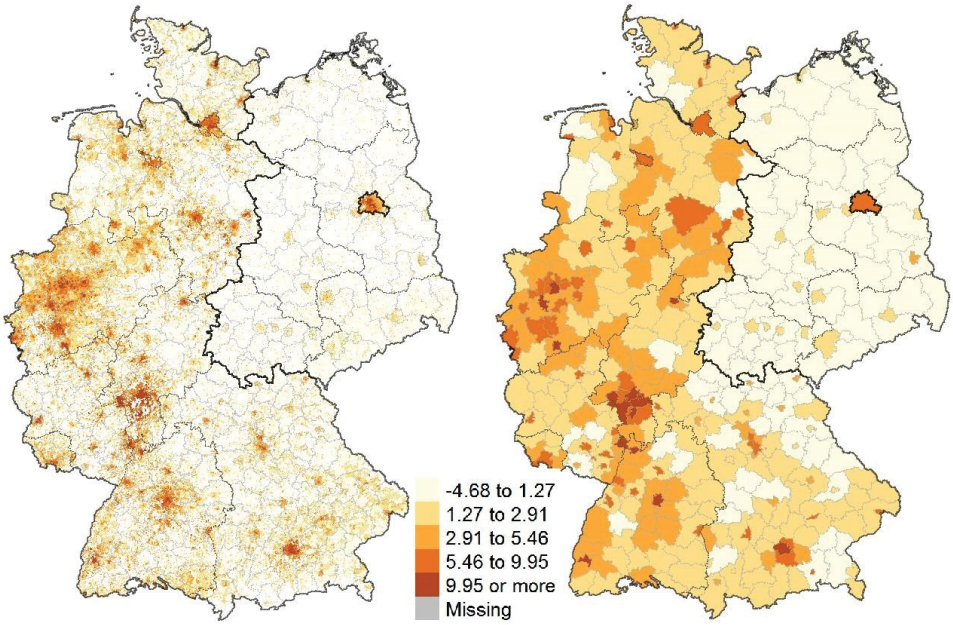
[8] Integrationsgesetz (2016).

[9] Exceptions are possible for taking up a job, education etc.

[10] The mean duration of the asylum process was 5, 7, 11 and 8 months in 2015, 2016, 2017, and 2018, respectively (Federal Government 2017).

[11] RWI and microm (2020)

Figure 4.1: Change of Population from Main Refugee Source Countries at the Grid Cell (Left Map) and County (Right Map) Level (2014-2017)



Notes: Population changes are measured as the share of all residents in 2014 within the 1km x 1km/-county in percent. The map on grid cell level is based on a sample of populated grid cells. Source: Own calculations based on RWI-GEO-GRID (© GeoBasis-DE / BKG 2020)

We construct the (net) inflow of refugees by the number of residents from typical refugee countries (non-European Islamic and African states) in 2017 minus the respective number in 2014 divided by the population in 2014. The information on the migration background in the RWI-GEO-GRID data is originally provided by the microm GmbH.[12] Residents from foreign countries cannot be distinguished by every origin country but by language groups. One group is non-European Islamic states which includes Northern Africa, Middle East, Iran, Pakistan, and Muslims

[12] They calculate the share of residents by analyzing the family names of the cardholders of debit and credit cards. Refugees are very likely to be captured by this process, because since 2016 every legal resident in Germany has the right to open a bank account. This measure was especially directed at asylum seekers and homeless people (Bundesregierung. 2016. https://www.bundesregierung.de/breg-de/aktuelles/jeder-hat-das-recht-auf-ein-konto-321068. 09.09.2021).

of Southeast Asia. This group covers most refugees in the years prior to the election. E.g., in 2015 and 2016 Syria (424,907 asylum seekers), Afghanistan (158,394), Iraq (125,900), and Iran (31,814) were the main origin countries of asylum seekers.[13] Their share is about 64 % of all asylum seekers during 2015 and 2016. As the second biggest origin group of asylum seekers are African countries[14], we define people stemming from the non-European Islamic and African group as refugees. Figure 4.1 shows the refugee inflow rate on the grid cell and the county level. We can observe differences between West Germany and East Germany. While there is a higher inflow rate of refugees in West Germany, especially in urban areas, East Germany generally shows a lower refugee inflow rate. Overall descriptive statistics on refugee inflow can be found in Table 4.1. It becomes obvious that the difference in the mean inflow rate from refugee countries on grid cell level is substantial since it is 2.3 % in West Germany and 0.6 % in East Germany. As Figure 4.1 already indicates, there exists a large variation between East and West Germany, across counties but also within counties on grid cell level. The standard deviation of the county level inflow rate ranges from 0.38 % in East to 2.06 % in West Germany, while the standard deviation of refugee inflows on grid cell level is 0.62 % and 2.33 % in East and West Germany. The grid level variation contains between and within county variation, with most existing studies overlooking the latter. However, within county variation is not negligible. In West Germany, the between county standard deviation is 2.53 % and the within county standard deviation amounts to 1.56 %. The corresponding numbers of East Germany are 0.51 % and 0.53 %. We capture the variation of refugee inflows on both spatial scales, the grid cell and county level, in order to account for potentially different mechanisms how refugee inflows affect the voting behavior.

[13] BAMF. Das Bundesamt in Zahlen 2016. Asyl, Migration und Integration: Nürnberg: Bundesamt für Migration und Flüchtlinge, 2017. https://www.bamf.de/SharedDocs/Anlagen/DE/Statistik/BundesamtinZahlen/bundesamt-in-zahlen-2016.pdf (21.07.2021). & BAMF. Das Bundesamt in Zahlen 2016. Asyl, Migration und Integration: Nürnberg: Bundesamt für Migration und Flüchtlinge, 2016.https://www.bamf.de/SharedDocs/Anlagen/DE/Statistik/BundesamtinZahlen/bundesamt-in-zahlen-2015.pdf (21.07.2021).

[14] African asylum seeker came mainly from Eritrea (29,730) and Nigeria (17,916).

Table 4.1: Descriptive Statistics of the Estimation Sample (observation unit is the grid cell level)

	West (N = 108,279)		**East (N = 28,518)**	
	Mean	St. Dev.	Mean	St. Dev.
AfD vote share (in %) 2017	11.58	4.06	24.75	6.71
Inflow refugees grid cell (in %) 2017-2014	2.32	2.33	0.60	0.62
Inflow refugees county (in %) 2017-2014	2.80	2.06	0.68	0.38
Share males 18-35 (in %) 2014	9.54	1.66	8.57	1.74
Share males 35-60 (in %) 2014	18.90	1.52	20.30	2.37
Share males older 60 (in %) 2014	12.07	2.05	13.56	2.32
Share females 18-35 (in %) 2014	9.43	1.67	8.45	1.75
Share females 35-60 (in %) 2014	18.91	1.53	19.97	2.37
Share females older 60 (in %) 2014	14.38	2.64	16.83	3.26
Children (mean per hh) 2014	0.31	0.10	0.24	0.10
Families (in %) 2014	41.31	27.93	25.40	28.81
Couples (in %) 2014	30.76	22.68	43.25	30.22
Log purchasing power per capita (in %) 2014	9.99	0.14	9.84	0.11
Unemployment rate (in %) 2014	3.96	2.69	8.22	3.65
Population (in thousand) 2014	508.99	1166.70	319.12	788.78
High credit default risk (in % hh) 2014	8.67	16.01	13.39	22.00
Cities (in %)	6.36	24.40	3.56	18.52
Urban counties (in %)	41.83	49.33	6.70	25.01
Rural counties with denser parts (in %)	26.48	44.12	34.99	47.69
Rural counties (in %)	25.33	43.49	54.75	49.77
Immigrants from Turkey (in %) 2014-2011	0.97	1.89	0.09	0.25
Immigration from East EU (in %) 2014-2011	0.86	0.85	0.31	0.50
Immigrants from South EU (in %) 2014-2011	0.95	0.90	0.71	0.68
Immigrants from Asia (in %) 2014-2011	0.03	0.09	0.02	0.24
Other Immigrants (in %) 2014-2011	0.80	0.60	0.90	0.81
Rent index	-0.11	0.20	-0.12	0.17

Notes: Own calculations based on RWI-GEO-GRID and RWI-GEO-vote. The refugee inflow rates are the net inflow rates between 2014 and 2017 in relation to total population in 2014. The immigration from other countries is the inflow rate between 2014 and 2011 in relation to total population in 2011.

German Federal Election in 2017

In the 2017 federal election, the AfD became the first far-right party which entered the German federal parliament since the second World War and represented the

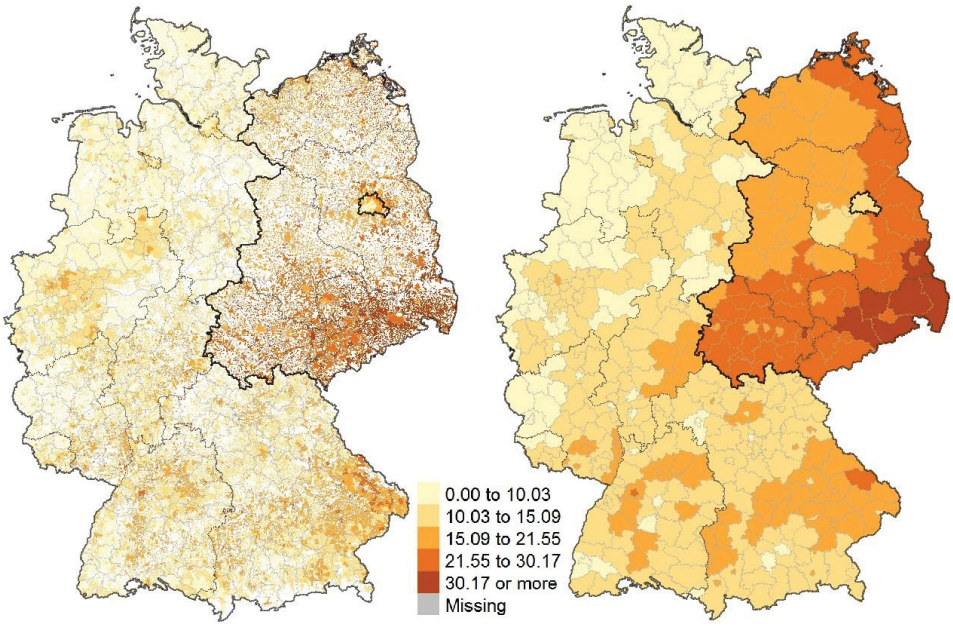
third biggest parliamentary group. While the AfD started as a Euro-critical and economically liberal party, it has become a mainly anti-immigrant party. Especially since 2015, when the inflow of refugees to Germany sharply increased, the AfD opposed the open border policy of the German federal government and demonized the incoming refugees.

To capture this specific voting behavior, we build a data set with small-scale election results for 1km x 1km grid cells for most of Germany for the federal election in 2017. We collect information on election results on electoral district level (Wahlbezirk, smallest regional unit) for all Germany by the head of the electoral management body (Bundeswahlleiter). Unfortunately, the data do neither cover information on the geography of the electoral district nor of the polling station. Municipalities determine the shape of electoral districts along two dimensions. First, electoral districts should not exceed more than 2,500 residents. Second, voters should easily reach the polling station. Based on the assumption that the polling station is the closest to each grid cell within an electoral constituency, we generate a comprehensive small-scale (1km x 1km) dataset of the federal election 2017. Thereby, we combine available information on the shape of all constituencies, geometries of electoral districts if available and the location of the polling station if geometries are not available. The procedure that takes population distribution into account is described in the Appendix 4.7.2. The final data set RWI-GEO-VOTE (Fremerey et al., 2021) is a dataset that will be available through the FDZ Ruhr.

Figure 4.2 illustrates the election results of the AfD on the small-scale level and on the more aggregated county level in the left and right panel, respectively. The clearest pattern, which is observable in both maps, is that AfD voting is higher in East Germany than in West Germany. In addition, the grid cell level map shows substantive variation within counties. Hence, while other studies at higher levels overlook variation within their units of observations, we are able to capture this variation also within counties and additionally look at the immediate neighborhood at grid cell level. The differences in levels of vote shares for the AfD between East and West Germany are displayed in Table 4.1. The mean grid cell vote share for the AfD is about 11.6 % and 24.8 % in West and East Germany, respectively. Our granular data allow us to exploit the within county variation of AfD vote shares which is considerable. The between standard deviation is 3.02 % and the within standard

deviation is 2.5 % in West Germany. The corresponding standard deviations for East Germany are 4.96 % and 3.95 %.

Figure 4.2: Federal Election results at Grid Cell (Left Map) and County (Right Map) Level for the AfD (in %) in 2017



Notes: The map on grid cell level is based on a sample of populated grid cells. Source: Own calculations based on RWI-GEO-Vote; (© GeoBasis-DE / BKG 2020)

Additional characteristics

Table 4.1 also reports the descriptive statistics for additional controls on grid cell level in our regression sample. We distinguish the sample into East and West Germany since the two parts experienced different political systems after the second World War. Although Germany has been reunited for more than 30 years now, both parts of Germany still show differences in terms of the share of urban and rural areas, the unemployment rate, the age decomposition and many other socio-economic factors. As Table 4.1 indicates, the grid cell level unemployment rate in

West Germany is on average more than 4 percentage points lower than in East Germany in 2014. Further, the ageing of the population is more pronounced in the East: The share of the elderly (above 60) is almost 4 percentage points higher in East than in West Germany, while the share of the younger population (18 to 35) is about 2 percentage points lower. We exploit the degree of urbanity of the respective county by applying the classification of counties into four categories by the Federal Institute on Building, Urban Affairs and Spatial Development (BBSR) (siedlungsstrukturelle Kreistypen[15]). This classification divides the German counties into four categories: very urban (cities with at least 100,000 residents that constitute an independent county), urban, rural (rural counties with some denser parts) and very rural counties. Additionally, we compute a rent index on the grid cell level to account for the local housing market. The index is constructed by the mean of residuals on grid cell level of a hedonic price function on log rents per square meter using a rich set of characteristics[16] from the data set RWI-GEO-RED[17]. The data cover the universe of rent ads from the internet platform ImmobilienScout24[18]. In general, the share of immigrants in East Germany is lower than in West Germany. For instance, the East German share of immigrants from Turkey is on average only one tenth of the West German share (see Table 4.1). We control for the share of immigrants which could affect not only the allocation of refugees but also affect the voting behavior. To avoid the problem of bad controls we take lagged values for all controls: stock variables 2014 and change of migrant shares 2011 to 2014.

4.3 Estimation Strategy

The aim of the paper is to analyze the relationship between refugee inflows and voting for the far-right party AfD in Germany (AfD_{ics}). As indicated in the previous section, we expect that the large differences between East and West Germany are also be reflected in a different relationship between refugee inflow rate and AfD vote

[15] https://www.bbsr.bund.de/BBSR/DE/forschung/raumbeobachtung/Raumabgrenzungen/deutschland/kreise/siedlungsstrukturelle-kreistypen/kreistypen.html

[16] Characteristics include the number of rooms, the construction year, an indicator of the level of equipment and urbanity, and dummies whether it is a house (as opposed to apartments) and for first occupancy, as well as respective dummies for the existence of balcony, garden, fitted kitchen, guest toilet, and cellar.

[17] RWI-GEO-RED: RWI Real Estate Data (Scientific Use File)- apartments for rent. DOI: 10.7807/immo:red:wm:suf:v5

[18] ImmobilienScout24 is the largest internet platform for both private and commercial providers of housing ads in Germany (Schaffner, 2020).

shares. Therefore, we run OLS regressions for West and East Germany separately.[19] The equation below shows our regression specification for the election results AfD_{ics} in the direct neighborhood i (1km x 1km grid cell) in county c and federal state s:

$$AfD_{ics} = \alpha + \beta_1 \Delta R_i + \gamma X_i + \theta Z_s + \phi U_c + \epsilon_{ics} \tag{4.1}$$

where the refugee inflow rate in the direct neighborhood in the 1km x 1km grid cell is notated by ΔR_i, the change in the share of residents from refugee countries between 2014 and 2017 relative to the grid cell's population in 2014. We control for neighborhood characteristics X_i, degree of county urbanity U_c and federal state fixed effects Z_s. Since refugee allocation rules determined the refugee inflow mainly at county level, standard errors might be correlated at this level. Therefore, we cluster standard errors at county level.[20]

In a different specification, we replace the inflow rate on grid cell level by the inflow rate on county (NUTS 3) level c, ΔR_c. This specification allows us to investigate if there is some effect of the county level on the spatial voting pattern. There might be spatially varying effects on this level, too. Voters perceive refugees not only if they move into the same neighborhood, but also when refugees locate in the same county or city, as local news start to report about refugees in the county. Also, policies at the county level might differ and could lead to a different relationship than at the grid cell level.[21]

$$AfD_{ics} = \alpha + \beta_1 \Delta R_c + \gamma X_i + \theta Z_s + \phi U_c + \epsilon_{ics} \tag{4.2}$$

In addition to equation (1) and (2), we also include both inflow rates as explana-

[19] Regression results of the joint specification are reported in table D5 in the Appendix.

[20] In total, Germany has 401 counties. Thereof, 325 counties are located in West Germany, while there are 76 counties in East Germany. East Germany has 9 very urban counties, 6 urban counties, 24 rural counties and 37 very rural counties. Therefore, we refer to p-values from a wild cluster bootstrap when investigating the heterogeneity across urbanity in East Germany. West Germany has 58 very urban counties, 125 urban counties, 77 rural counties and 65 very rural counties.

[21] We refrain from analyzing the municipality level because municipalities are not comparable administrative units. There are more than 200 municipalities with a population below 100 in 2017, while on the other extreme urban municipalities often constitute their own county or even federal state like in the case of Hamburg and Berlin.

tory variables. Thereby, we can simultaneously control for both regional scales and infer which scale is decisive in terms of magnitude.

$$AfD_{ics} = \alpha + \beta_1 \Delta R_i + \beta_2 \Delta R_c + \gamma X_i + \theta Z_s + \phi U_c + \epsilon_{ics} \tag{4.3}$$

When estimating the effect of the refugee inflow rates on voting in favor of a far-right party by OLS regressions of equations (1) to (3), concerns of endogeneity arise. First, within the predefined area during a refugee process, refugees could avoid moving into regions which are characterized by strong anti-immigrant sentiments (see Section 4.2). This can lead to a downward bias. Second, a downward bias can also arise due to residents with an anti-immigrant attitude sorting into areas where neighbors share their attitudes and into areas with a low share of foreigners. However, endogenous sorting of natives is unlikely, as Halla et al. (2017) find no evidence that natives move due to the inflow of immigrants. Overall, the German moving rate is quite low and out-migration of xenophobic natives would imply an increase in the German moving rate; however, we observe a decrease.[22] Further, allocation of refugees was rather exogenous in first place (see section 4.2). Some studies leverage the allocation of asylum seekers as a possible source of exogenous variation, see e.g., Gehrsitz and Ungerer (2017) and Dehos (2021). Since refugees were allowed to choose their location within a county after their asylum process was granted, we implement a shift-share instrumental variable approach as a strategy to account for the remaining concerns of endogenous refugees' choice of location.

It is a well-established and long-known fact that immigrants tend to locate in ethnic enclaves in the receiving country to benefit from ethnic networks (Bartel, 1989; Zimmermann, 1996; Bauer and Zimmermann, 1997; Munshi, 2003). This inspired Altonji and Card (1991) and Card (2001) to use past settlement shares of immigrants to instrument for recent immigrant inflows. Afterwards, this methodology has been widely used in the migration literature (see for instance, Dustmann et al., 2005; Saiz, 2007; Gonzalez and Ortega, 2011; Bianchi et al., 2012; Tabellini, 2020). The first stage regression on grid cell level (according (1)) then is

[22] The German moving rate in 2017 was 8.8 % and therefore, the first time below 9 % (techem, 2018).

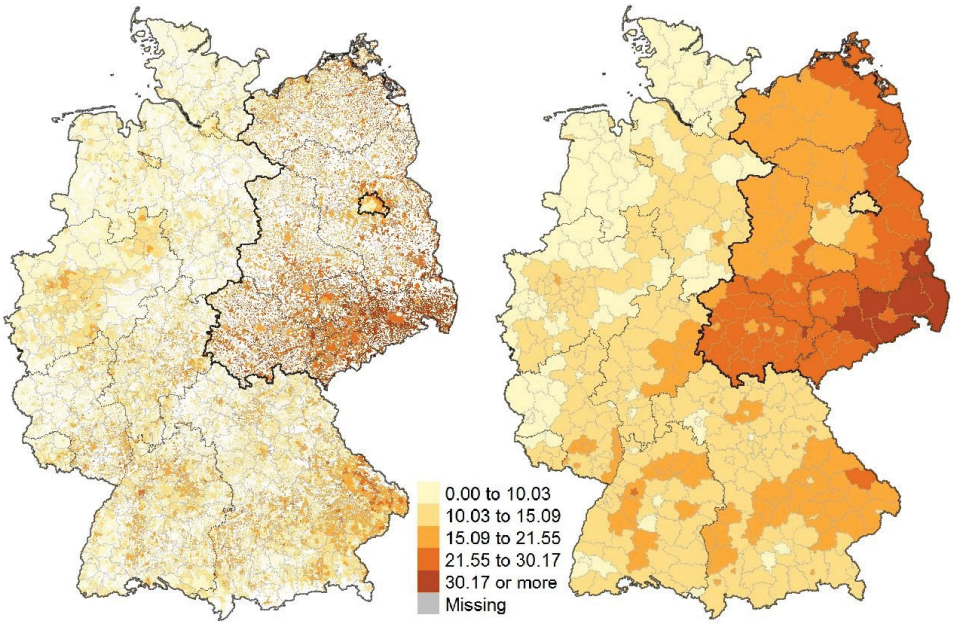
$$\Delta R_j = \sigma_1 R_{j,2005} + \mu_1 X_j + \rho_1 Z_s + u_{js}, \qquad \text{for j=i,c} \qquad (4.4)$$

i.e., the inflow rate of refugees between 2017 and 2014 into the grid cell/county, ΔR_j, is a function of the refugee share in the grid cell/county in 2005, $R_{j,2005}$, a set of covariates and state fixed effects.

The IV strategy mainly depends on three assumptions, namely the relevance, the exclusion restriction, and the independence assumption. The validity of all assumptions is discussed in more depth subsequently. First, the relevance assumption requires that the possibly endogenous variable, the inflow rate, and the instrument, the share of people from African and Islamic countries in 2005, are highly correlated, i.e. σ_1 must be statistically significant different from zero. Derived from the ethnic network literature we expect a positive effect of the past settlement on the current refugee inflow rate.

Figure 4.3 shows that enclaves of African and Islamic minorities already existed in 2005. While this is necessary for the correlation of past settlements and the inflow between 2014 and 2017, the illustrated regional distribution reveals that the share of the minorities in most of East Germany is essentially zero. Also, in West Germany, enclaves strongly concentrate in urban areas. Table D3 gives a more detailed picture of the realizations of the instrument in the total sample and by urbanity. In the very rural counties of East Germany, 85 % of populated grid cells have a past settlement share of zero. This figure is similar for the rural (80 %) and urban area (74 %) in East Germany. Only in East German cities, this share drops to 40 %, suggesting that the instrument can, if any, predict reliable inflow estimates in East Germany only for very urban areas. While rural-urban differences are similar in West Germany, the number of observations with positive shares of minorities is higher in all types of counties, suggesting that, if the expected channel exists, the instrument is more reliable in the West German sample. Therefore, when dividing the sample into different urbanity regions, we apply the IV strategy on grid cell level to the West German subsample only and instrument in the East German subsample only the county inflow rate.

Figure 4.3: Distribution of the instrumental variables: Share of residents from refugee countries in 2005 at the Grid Cell (Left Map) and County (Right Map) Level

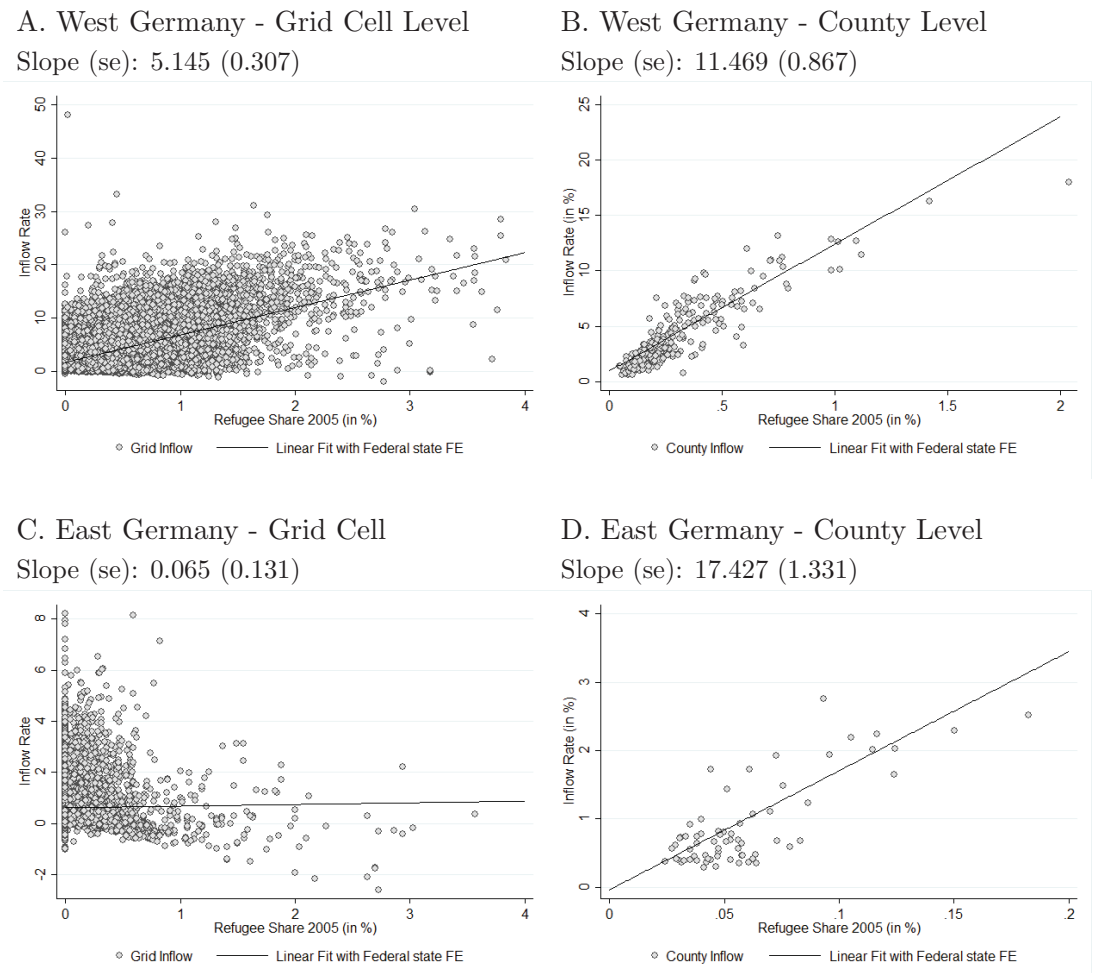


Notes: Residents are measured as the share of all residents within the 1km x 1km/county in percent. The map on grid cell level is based on a sample of populated grid cells. Source: Own calculations based on RWI-GEO-GRID (© GeoBasis-DE / BKG 2020)

To further illustrate the relationship between the past settlement shares and the net inflow rate between 2014 and 2017, Figure 4.4 plots the inflow rate on the grid cell and county level in Panel A and B, respectively, against the respective 2005 refugee share for West and East Germany, separately. For better visibility, we add a linear fit conditional on federal state fixed effects. This reveals a strong and positive correlation in West Germany and a close to zero correlation in East Germany on the grid cell level. On the county level the coefficient more than doubles in West Germany and is even higher in East Germany, indicating that the past settlement patterns predict very strongly the inflow rates at county level. Hence, as mentioned above, we assume the first stage for the grid cell inflow to be only

reliable in West Germany, whereas the county inflow can be instrumented for both regions. Therefore, we instrument the grid cell and the county level inflow rate of refugees in West Germany, while we instrument only the county level inflow rate of refugees in East Germany and include the grid cell level inflow as covariate in the IV regressions.

Figure 4.4: Spatial correlation between refugee share in 2005 and refugee inflow



Notes: Source: Own calculations based on RWI-GEO-GRID

Another condition for the IV strategy is the exclusion restriction, i.e., that the effect of the past settlements of refugees on the AfD's election outcome is only driven by the current refugee inflow rate. Although the usage of past settlement patterns as instrument for current inflow rates is widely used in the literature, criticism on the shift-share instrument appeared in recent years (e.g., Jaeger, Ruist

and Stuhler 2018). One of their main concerns is that long-term effects of past immigration might be confused with the short-term effects of current immigration violating the exclusion assumption. Since this assumption is, in contrast to the relevance condition, not directly testable, our argumentation is twofold.

First, we argue that this criticism does not apply here for two reasons. Typical refugees from the 2014 to 2017 inflow accounted for only a very small share of immigrants in Germany before 2014[23], which is why we do not expect large effects of previous immigration from African and Islamic countries on AfD election outcomes. We strengthen this argument of no long-term effects of the past settlement pattern by running placebo regressions. For this purpose, we look at the effect of the inflow rate between 2014 and 2017 on the AfD result in the federal election of 2013.[24] Election results from the federal election in 2013 are only available at the municipality level, but apart from that we use the same specifications as stated above. Regression results presented in Table D4 indicate no statistically significant effect of the inflow rate on the 2013 AfD election result. This is true for both the grid cell and the county level inflow rate separately and jointly estimated. Further, it holds in the East and West German sample and when estimating the regressions with OLS or IV. These results suggest that we do not capture the effect of past settlement patterns and hence, effects of the inflow rate can be attributed solely to the inflow between 2014 and 2017. Second, we use a rich set of covariates controlling for other possible channels driving the effect of the past settlement of refugees on AfD voting outcome. Since we control for economic conditions (like the unemployment rate, credit default risk, income) and personal characteristics (such as age, gender and family status) we are confident to capture other major channels.

The third assumption that needs to hold for a consistent application of an instrumental variable approach is the independence assumption. It requires that the outcome variable does not influence the instrumental variable. It is very plausible that this condition holds since the voting outcome in 2017 is highly unlikely to

[23] The average share of persons from Islamic and African countries across all grid cells was 0.15 % in West and 0.04 % in East Germany in 2005.

[24] Another channel that could drive potential reverse causation are historical attitudes leading to a lower refugee inflow. However, Cantoni et al. (2019) show that the correlation between historical National Socialist German Workers' Party's (NSDAP) vote shares and refugee shares nowadays is, though negative, quantitatively very small (correlation coefficient, conditional on state fixed effects: -0.09).

influence past settlements of refugees in 2005.

4.4 Results

In this section, we present the main results of our analysis. We regress the share of AfD votes within a 1km x 1km grid cell on the net inflow rate of refugees between 2014 and 2017 within the same grid cell and on the county level. We estimate the regressions for East and West Germany separately, since we expect different relationship based on the observed substantial differences between East and West in both the dependent and independent variables (see Figure 4.1, Figure 4.2 and Table 4.1). However, joint estimation is possible, but is likely to be dominated by the larger West German part.[25] Table 4.2 separately shows our results for West Germany in the upper panel and East Germany in the bottom panel. Columns one to three report OLS estimation results, with the inflow rate on grid cell, county, and both levels jointly. This pattern is repeated in columns four to six with IV regressions, where the inflow rate between 2014 and 2017 is instrumented with the settlement shares of refugee countries in 2005 on the corresponding regional level. All specifications include state fixed effects and a full set of covariates for a year or period before the refugee inflow in 2015 (see Table 4.1). The OLS results in Table 4.2 of Panel A show that there is a negative relationship between the inflow rate on grid cell level on the voting for the AfD in West Germany while there is no relationship between the county level inflow rate and AfD support. The IV regressions confirm the direction of this relationship with the grid cell level inflow and the absence of a statistically significant relation with the county level inflow. The negative effect of the inflow rate on grid cell level is robust against the inclusion of the instrumented county level inflow rate which itself does not have a statistically significant effect on AfD support. A one standard deviation (2.33) higher grid cell inflow rate leads to a reduction in AfD support by 0.48 percentage points when simultaneously controlling for the county level refugee inflow. The IV regressions base on a strong instrument in the first stage, as indicated by the high F-statistics and the statistically significant and positive effects of the past settlement shares on the inflow rate both on grid cell and county level. The estimated first stage coefficient of grid level settlement shares in 2005 for West Germany (column 4 of Table 4.2) of 2.8 suggests that a one standard deviation higher refugee share in 2005 (0.25; see Table D3 for detailed

[25] For regression results of the overall sample see Table D5.

summary statistics of the instrument and the inflow rate) is associated with an about 0.7 percentage point higher inflow rate. The first stage coefficients on county level are even greater in magnitude, while the F-statistic is smaller but remains on a very high level (see columns five and six).

Table 4.2: OLS and IV Regression Results inflow refugees on AfD vote share

	OLS			**IV**		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:						
West						
Inflow refugees in grid cell	-0.127***		-0.149***	-0.145**		-0.207***
	[0.035]		[0.029]	[0.061]		[0.073]
Inflow refugees in county		-0.041	0.063		0.054	0.188
		[0.088]	[0.091]		[0.124]	[0.139]
R2	0.375	0.372	0.375	0.375	0.372	0.374
Observations	108,279	108,279	108,279	108,279	108,279	108,279
				1st Stage Results		
Kleibergen-Paap F-statistic				190.0	138.8	105.9
Refugees in grid 2005				2.827***		2.341***
				[0.205]		[0.165]
Refugee in county 2005					7.742 ***	7.770***
					[0.657]	[0.670]
Panel B:						
East						
Inflow refugees in grid cell	0.009		-0.031	0.285		0.809
	[0.246]		[0.127]	[2.617]		[0.656]
Inflow refugees in county		0.169	0.192		-3.339	-3.861
		[1.041]	[1.042]		[3.039]	[3.398]
R2	0.513	0.513	0.513	0.512	0.488	0.485
Observations	28,518	28,518	28,518	28,518	28,518	28,518
				1st Stage Results		
Kleibergen-Paap F-statistic				1.1	20.6	21.9
Refugees in grid 2005				-0.094		
				[0.089]		
Refugee in county 2005					8.551***	7.576***
					[1.883]	[1.617]

Notes: Robust clustered standard errors on county level are reported in parentheses. Regressions include state fixed effects and full set of covariates.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

While OLS and IV regressions provide qualitatively identical results, quantitatively OLS results are slightly upward biased. This suggests that the sorting is not due to refugees proactively avoiding xenophobic neighborhoods. If refugees sort themselves into less xenophobic neighborhoods, the OLS coefficient would capture this by a stronger negative relationship between inflow rates and AfD support. Rather, a possible explanation could be, that refugees move into shrinking regions with out-migration and low regional amenities. The control variables used, like the rent index, only indirectly or partly capture these factors, while these areas might coincide with precarious social conditions serving as a hotbed for xenophobic sentiments. As OLS estimates are upward biased in the overall sample, we assume OLS coefficients to be an upward bound also for rather rural areas, where the instrument is weak. Further, the local average treatment effect (LATE) of the IV regression measures the effect for those grid cells that react to the instrument: regions with a high share in 2005 receiving a high inflow of refugees. The effects can be driven by some unobserved lower anti-immigration sentiments within these areas. We alleviate these concerns through the placebo regressions of the AfD outcome in the 2013 election (see Table D4, discussion in Section 4.3).

Panel B of Table 4.2 depicts the estimation result for the East German sample. The first stage results in East Germany show that the instrument is strong enough only on the county level. The inflow rate on the county level is both large in magnitude and statistically significant. A one standard deviation higher past settlement share (0.02) is associated with an 0.17 percentage points higher inflow rate on the county level. The coefficient of the past settlement pattern on the grid cell level is essentially zero and the F-statistic is 1.1 (see bottom panel column four).[26] We therefore abstain from further interpreting the IV results for East Germany on grid cell level and do not instrument the grid cell level in the joint specification in column (6) of Table 4.2. Hence, in the joint specification of the inflow rates on grid cell and county level in column (6), only the inflow rate on county level is instrumented and the inflow on grid cell level is added as a covariate. This slightly decreases the first stage coefficient but increases the first stage's F-statistic between columns (5) and (6). OLS and IV regressions both show relatively small

[26] The weak first stage on grid cell level in East Germany could be expected by the descriptive statistics and was already discussed in Section 4.3. There is too little variation of the instrumental variable in East Germany, as most of the grid cells have a refugee share of zero in 2005.

coefficients for the effect at the grid cell level and quite large coefficients for the effect at the county level. However, based on the large standard errors, both effects are imprecisely measured and are therefore statistically insignificant.

As existing literature shows, there can be substantial differences between urban and rural areas regarding voting and attitudes towards immigrants (e.g, Barone et al., 2016; Dustmann et al., 2019). Hence, the insignificant effect in East Germany might hide opposing effects by degree of urbanity. Further, the negative effect in West Germany might be driven by only a subgroup of regions. We further divide our two samples by urbanity. We apply the classification of counties into the four categories described in Section 4.2. Table 4.3 presents the regression results for each of the four groups separately for West Germany. OLS regressions in Panel A of Table 4.3 reveal that the negative correlation between inflow rate at grid cell level and far-right voting in West Germany is driven by all county types except the very rural counties. While in very urban counties and rural counties the coefficient of the grid cell inflow rate is persistently negative but statistically insignificant, it is statistically significant in urban areas. The IV regressions by urbanity suggest that the negative effect on grid cell level of the whole West German sample is driven by the urban areas. The coefficient of the inflow rate on the grid cell level in the joint regression with the county level inflow rate in West German urban counties is similar in magnitude to the overall effects (compare column (6) in Table 4.3 to column (6) in Table 4.2). While the refugee inflow on grid level negatively affects the AfD vote shares, the inflow rate on county level in urban areas increases the support for the AfD in West Germany. For the very rural subgroups, both OLS and IV results suggest no statistically significant relationship between inflow rates on grid cell and county level and AfD support. Since the effect on the grid cell is likely to capture the direct contact between refugees and native, these findings support the contact theory in West German urban areas. However, there exists a countervailing effect of refugee inflow into the county which indicates a positive impact on far-right vote shares.

The results by urbanity for East Germany in Table 4.4 provides some evidence that the insignificant overall effect of refugee inflow on county level (in Table 4.2) might conceal a negative effect in East German very urban counties. A one standard deviation increase in refugee inflow in very urban East German

counties (0.4 standard deviation) leads to a decrease of about 2 percentage points in AfD votes. This IV estimate is quite large in magnitude and robust against the inclusion of the inflow at grid cell level. In the other East German urbanity subgroups, we again observe coefficients with a quite large magnitude on the county level. They are however, again statistically insignificant based on the imprecise estimation indicated by relatively large p-values. This seems to be driven by the weak instrument, as residents from the now typical refugee countries constituted a negligible group before 2014.

Overall, in West Germany, the contact in the direct neighborhood reduces AfD votes shares. This effect seems to be driven by urban counties. In the other West German county groups, we do not observe a significant relationship between refugee inflow rates and AfD support. For the total East German sample, we estimate large negative coefficients with high imprecision, which indicates a statistically insignificant relationship between refugee inflow and AfD support. This average effect conceals a strong negative relationship between inflow of refugees on county level and far-right votes in East German very urban counties. The findings speak in favor of the validity of the contact theory in urban West Germany but not in East German very urban counties, because inflow of refugees in the county does not necessarily imply a direct interaction between natives and refugees. Nevertheless, a city hosting more refugees leads to less support of anti-immigrant sentiments in East German cities, while it leads to an increase in the support of the AfD in West German urban areas. These results replicate findings of Barone et al. (2016), Dustmann et al. (2019) and Harteveld et al. (2021) according to which immigration has heterogenous effects on far-right voting by urbanity. Furthermore, we show like Della Posta (2013) and Janssen et al. (2019) that effects can be different depending on the investigated spatial scale. One explanation for the different decisive spatial scale in East and West Germany could lie in the general differences between the two areas of Germany. As already indicated in Section 4.2, even three decades after the German reunification there still exists a large difference in socio-economic factors between East and West Germany. East German citizens are exposed to a lower share of refugees compared to West Germany not only during the recent refugee inflow but also before 2015. Although the German Democratic Republic (GDR) hosted refugees and migrant workers, state-regulated policies regarding refugees and immigration however, showed more segregative than integrative tendencies

(Bade et al., 2004). All these aspects might foster a totally different experience and therefore, response to the current refugee inflow. Hence, in East German very urban areas, it is not the direct contact in the immediate neighborhood that reduces support for the AfD (as in urban West Germany), but the refugee inflow into the very urban county itself. Therefore, in very urban East German and urban West German areas, other mechanisms besides the contact theory may apply here (as well), for instance, an increase in local media coverage about refugees (Steinmayr, 2021) which leads to citizens knowing that refugees live in the county without having direct contact with them.

Table 4.3: OLS and IV Regression Results inflow refugees on AfD vote share by urbanity for West Germany

West	**Very urban counties**			**Urban counties**			**Rural counties**			**Very rural counties**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: OLS												
Inflow refugees in grid cell	-0.098		-0.149*	-0.134***		-0.158***	-0.043		-0.094*	0.027		-0.066
	[0.080]		[0.088]	[0.040]		[0.030]	[0.063]		[0.050]	[0.152]		[0.067]
Inflow refugees in county		0.109	0.22		-0.018	0.078		0.088	0.157		0.299	0.344
		[0.119]	[0.148]		[0.107]	[0.108]		[0.190]	[0.203]		[0.473]	[0.461]
R2	0.446	0.443	0.451	0.305	0.300	0.306	0.477	0.477	0.478	0.450	0.460	0.460
Observations	8,267	8,267	8,267	48,179	48,179	48,179	27,863	27,863	27,863	23,970	23,983	23,983
Panel B: IV												
Inflow refugees in grid cell	-0.129		-0.143	-0.092		-0.212***	0.138		0.123	0.389		0.632
	[0.162]		[0.205]	[0.064]		[0.063]	[0.269]		[0.270]	[0.385]		[0.399]
Inflow refugees in county		-0.047	0.063		0.377*	0.484**		0.202	0.133		-0.65	-0.999
		[0.146]	[0.248]		[0.217]	[0.220]		[0.418]	[0.403]		[0.675]	[0.716]
R2	0.446	0.440	0.448	0.305	0.279	0.286	0.474	0.477	0.474	0.443	0.433	0.416
Observations	8,267	8,267	8,267	48,179	48,179	48,179	27,863	27,863	27,863	23,970	23,970	23,970
						1st Stage Results						
Kleibergen-Paap F-statistic	101.7	71.9	37.7	83.1	51	58.1	17.4	14.5	9.5	24.1	21.1	10.8
Refugees in grid 2005	3.745***		3.330***	2.626***		2.319***	0.908***		0.794***	0.945***		0.808***
	[0.371]		[0.413]	[0.288]		[0.226]	[0.218]		[0.199]	[0.193]		[0.183]
Refugee in county 2005		6.812***	6.876***		7.219***	7.272***		8.983***	9.020***		9.401***	9.417***
		[0.803]	[0.791]		[1.011]	[1.018]		[2.357]	[2.358]		[2.045]	[2.044]

Notes: Robust clustered standard errors on county level are presented in parentheses. Regressions include state fixed effects and full set of covariates.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4.4: OLS and IV Regression Results inflow refugees on AfD vote share by urbanity for East Germany

East	**Very urban counties**			**Urban counties**			**Rural counties**			**Very rural counties**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: OLS												
Inflow refugees in grid cell	-0.486**		-0.047	-0.274		0.198	0.607		0.217	-0.026		-0.275
	(0.037)		(0.853)	(0.651)		(0.374)	(0.320)		(0.572)	(0.904)		(0.136)
Inflow refugees in county		-4.811	-4.778*		-1.535	-1.67		1.682	1.52		1.987	2.193
		(0.118)	(0.080)		(0.599)	(0.580)		(0.393)	(0.451)		(0.278)	(0.241)
R2	0.655	0.794	0.794	0.292	0.315	0.315	0.553	0.556	0.556	0.378	0.384	0.384
Observations	1349	1349	1349	2121	2121	2121	10867	10867	10867	14181	14181	14181
Panel B: IV												
Inflow refugees in grid cell			-0.028			0.184			1.058			0.583
			(0.900)			(0.245)			(0.808)			(0.682)
Inflow refugees in county		-5.007*	-4.988*		-1.503	-1.619		-1.23	-1.763		-5.25	-5.377
		(0.082)	(0.055)		(0.597)	(0.582)		(0.932)	(0.927)		(0.654)	(0.659)
R2		0.801	0.801		0.322	0.323		0.547	0.547		0.315	0.316
Observations		1349	1349		2121	2121		10891	10891		14206	14206
						1st Stage Results						
Kleibergen-Paap F-statistic		457.6	393.5		75.4	58.3		1.4	1.7		1.4	1.7
Refugee in county 2005		12.831**	12.760**		23.326**	22.208**		5.002	4.373		3.653	3.565
		(0.002)	(0.001)		(0.012)	(0.012)		(0.302)	(0.295)		(0.326)	(0.299)

Notes: P-values (in round brackets) come from a wild cluster bootstrap with clustering on county level (based on 999 replications). Regressions include state fixed effects and full set of covariates.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

4.5 Contextual Discussion of Results

4.5.1 Other parties

The results of the AfD are not independent from the results of all other parties. Therefore, we analyze the relationship between refugee inflow and vote shares also for other parties. We apply the same methods as implemented before but change the outcome variable. We investigate the effect of the refugee inflow rate on all parliamentary groups (i.e. Die Linke – socialists, Grüne – greens, SPD – social-democrats, Union – conservatives, FDP – liberals) and groups of extremist parties (far-left and far-right parties) which are not part of the Bundestag, separately. Panel A and B of Figure D1 present the OLS (left) and IV (right panel) results for the estimated coefficients for the inflow on grid cell and county level jointly for West Germany. The only statistically significant and negative coefficient of inflow rate at the grid cell level on party support in both OLS and IV regressions is found for the AfD. Besides the positive IV effect of the refugee inflow rate at the county level on the AfD votes, positive effects can also be found for the Green party (on the grid cell level in OLS and IV regressions) and for the liberal party FDP (on county level in the IV regression).

Figure D1 also shows the OLS and IV coefficients of the inflow rate on grid cell and county level of the joint specification for the East German sample in Panel C and D. As before, grid cell inflow is not instrumented because of the weak instrument on this geographical level. Hence, the only instrumented variables are the county level inflow rates in the right panel. The refugee inflow on the county level only significantly affects the vote shares for the Union (negatively) and the Green's vote shares (positively). Refugee inflow rates on the grid cell seem rather unrelated to the voting behavior in East Germany. Overall, we do not see many significant (negative) effects of refugee inflow on other parties' election outcome, neither in East nor in West Germany. This underlines our focus on the AfD vote shares as the party of interest when analyzing the impact of the refugee inflow.

4.5.2 Effect Heterogeneities

We observe differences between urban and rural regions in both West and East Germany suggesting a negative relationship between AfD support and inflow rates

at the grid cell and county level, respectively, only in areas with a high degree of urbanity. However, heterogeneities exist not only between urban and rural regions, but potentially also across several other dimensions. We therefore divide the West and East German sample along the quartiles of other important socio-economic characteristics to test for additional heterogeneities. As the relationship between inflow rates and xenophobic voting might differ between economically prosperous and weak regions and depending on the age composition, we chose the unemployment rates and the share of residents aged older than 60 to construct quartiles on the grid cell level. Furthermore, we approximate the openness of a county with the Bohemian index to investigate whether the estimated relationship between far-right voting and refugee inflow is driven by some counties being characterized as tolerant.

We closely follow Florida (2002) to calculate an index for the number of bohemians within each county.[27] This index is particularly large in cities and urban counties but has considerable variation also within county types. In Table D6 in columns two and four, we show the interaction with the Bohemian index and the refugee inflow at the grid cell level. In West Germany, the Bohemian index is negatively associated with AfD support. This suggests that the index captures an open atmosphere within counties which is expected to reduce xenophobic voting behavior. The effect of the grid cell level inflow rate becomes insignificant once the interaction term between Bohemian index and grid cell inflow rate is included which is negative and statistically significant (see column two and four of Table D6). This result in West Germany indicates that the contact theory is more relevant in counties with more bohemians. For East Germany all coefficients remain statistically insignificant. This again emphasizes our finding that even in counties which are characterized

[27] For this index we derive the sum of employees in bohemian occupations which are defined as occupations in art related fields. This includes artisan craftwork and fine arts (KldB 2010 = 933), artisans designing ceramics and glassware (934), artisans working with metal (935), occupations in musical instrument making (936), musicians, singers, and conductors (941), actors, dancers, athletes and related occupations (942), presenters and entertainers (943), occupations in theatre, film and television productions (944), occupations in photography (2332) and occupations in fashion design (2821). We conducted a special statistical request to the federal employment agency and got the number of workers in bohemian occupations at June, 30th of 2017 on county level. One part of the finally used data set stems from the Federal Employment Agency. Since many of these workers are not employed and thus not obliged to be member of the federal social security system, we additionally use member data of the artist social security fund that is a special social insurance for artists and publicists. We got information on all members on the zip code level that we aggregate on county level. We combine the two datasets and calculate the share of workers in bohemian jobs on the population in the county.

by a high share of open-minded residents the AfD election outcome is not statistically significantly related to the refugee inflow at the grid cell level in East Germany.

Table D7 and Table D8 in the Appendix show the same specifications as in the main part but for quartile groups of the unemployment rate at the grid cell level for West and East Germany, respectively. In West Germany, the negative correlation on grid cell level persists in OLS estimations for all quartiles of unemployment rate, while the county level effect seems to be insignificantly related to AfD support (see Table D7). The magnitude of the correlation in areas with very high unemployment rate is comparable to the magnitude of the correlation in areas with lower unemployment rate. Except for the joint specification in the second quartile and the grid cell level inflow rate in the fourth quartile, the estimated coefficients are insignificant in the IV regressions but still of similar magnitude. This indicates that the contact theory is valid to all regions with different unemployment rates. The negative effects of the grid cell inflow rate on AfD support suggest that voters in economically weak neighborhoods do not perceive refugees as a threat to their own economic situation. Although some studies find that immigrants increase far-right voting and that this effect is stronger in areas of high unemployment rate (for instance, see Halla et al., 2017), a meta-study provides mixed results (Sipma and Lubbers, 2020). In East Germany, no significant differences by unemployment rate can be observed (see Table D8).

Table D9 and Table D10 repeat this exercise but group the West and East German sample according to the quartiles of the grid cell level shares of the population aged above 60. The OLS results show that in West Germany, the negative correlation on grid cell level can be observed in all types of regions. The coefficient of the grid cell level inflow is only significant in the joint regression and smaller in magnitude in regions with a higher share of elderly (third and fourth quartile) (see Table D9). The IV estimations confirm the negative effect of the refugee inflow rate on grid cell level only for those regions with the lowest share of older residents. We do not find any significant IV effect of the refugee inflow on the county level on AfD support. These findings, which suggest that the contact theory applies less to the elderly, are consistent with the results of younger generations having more socially liberal views on cultural topics than older generations, while the older generation's values are systematically over-represented in conventional elections due to their higher voter

turnout (Norris and Inglehart, 2019). In East Germany, we find some evidence of a negative effect of the refugee inflow on county level only for the first quartile with a lower share of elderly (see Table D10). Hence, the negative effect of the other mechanisms (i.e. media coverage, the counties' refugee integration policy and others) might rather apply to younger generations than older ones.

4.5.3 Robustness checks

We execute several robustness checks to show that our results are not driven by our sample or regression specifications. We replicate our results using weighted regressions, wider neighborhood definitions, excluding the grid cell from the respective county level, splitting Berlin into its former Federal Republic of Germany (FRG) and German Democratic Republic (GDR) parts, and inserting commuting area and county instead of state fixed effects. Further, we investigate nonlinearity in the relationship between refugee inflow and AfD support. Our findings are robust against all these checks, and we do not identify an inflow threshold above which the negative effect of the inflow rate is offset.

So far, each grid observation entered the regressions with the same weight without differentiating by its population size. In our main specification, we concentrate on unweighted regression estimation because we want to investigate the average relationship across regions and do not focus on the impact on the actual election outcome on the national level. Alternatively, it is possible to weight each observation by the number of residents within the respective grid cell. Table D11 in the Appendix presents the results for the weighted regressions. The densely populated grid cells that are mainly within (very) urban regions are more influential on the results in the weighted regressions. The results in Panel A in Table D11 for West Germany are quite similar to those presented above, although the IV estimate of the grid cell level inflow rate in the joint estimation specification is not significant anymore presumably due to the large standard errors, while the magnitude of the coefficients is quite similar. For East Germany the estimated OLS coefficients are insignificant as it is in the unweighted regressions. The only difference is that in the IV regressions, the instrumented inflow at county level has a statistically significant negative effect. This is likely to be driven at least partly by very urban East German counties for which we observe negative effects on the

county level. Again, the grid cell level inflow rate is not instrumented in the joint estimation speficiation as also in the weighted regression the refugee inflow in the grid cell shows no sufficient first stage result.

The refugee inflow into the county in our main estimations corresponds to the weighted average of the grid cell level inflow within the same county. Therefore, concerns may arise about possible multicollinearity in the joint estimation of the inflow rate at the county level and the inflow rate at the grid cell level. In the joint estimation regression in column three and six in Table D12, we replace the county level inflow rate with a leave-one-out version of the county inflow rate which corresponds to the county level inflow rate without the inflow into the respective grid cell. The results are very similar to those presented in the main result section.

In our main analysis in Section 4.4, we distinguish between the refugee inflow rate within the same 1km x 1km grid cell and the inflow rate within the county. To test for the size of the neighborhood, we investigate the effect of a larger neighborhood (3km x 3km). The estimated OLS coefficients for the larger neighborhood (see Table D13) are similar to those for the grid cells in West Germany. The estimated IV coefficients are somewhat smaller in absolute size and therefore, insignificant. The differences for East Germany are negligible since all estimated coefficients are still insignificant.

One of our main assumptions is that voting and refugee inflow but also their relationship differ between East and West Germany. East and West Germany are defined by the former borders of the GDR and FRG without Berlin which we fully account to West Germany although it is located in the East. To account for possible effects driven by the assignment of Berlin, we further apply a different classification by dividing Berlin into its former GDR and FRG part. The results in Table D14 indicate that the results for West Germany as well as East Germany are stable. Since Berlin is by far the biggest city in Germany its assignment to West and East Germany can significantly influence the heterogenous results by urbanity. As the new categorization of East and West Germany only changes the sample's composition of very urban counties, we only provide the analysis of this group. The results in Table D15 show that when removing the Eastern part of Berlin from the West German very urban sample the instrumented inflow rate at grid cell level and

at the county level stays insignificant, while the former becomes positive. Columns four to six show the corresponding regressions for East German cities, now including East Berlin. The magnitude of the county level effect remains relatively high.

All regressions shown so far capture federal state fixed effects. These fixed effects account for differences at the federal state level. Especially, voting behavior and federal governments significantly differ between federal states. Alternatively, it is possible to account for local commuting area fixed effects since these regions are characterized by similar labor market conditions. However, the German commuting areas are defined on county level and some of them consist of only one county. To avoid multicollinearity problems, we decided to apply federal state fixed effects in the main specification. The problem of multicollinearity is also the reason why we only estimate the regressions for the whole sample and do not split the sample by urbanity when accounting for commuting area fixed effects. We apply the commuting area definition by Breidenbach et al. (2018). The results presented in Table D16 are robust to the inclusion of commuting area fixed effects. We further test the effect of the refugee inflow rate on grid cell level within counties. Therefore, we apply county fixed effects as Table D17 indicates. The inflow of refugees into a grid cell is negatively related to AfD support (except for IV results in East Germany) but is only statistically significant in OLS regressions in both West and East Germany. Taking heterogenous effects across urbanity into account, the results for West German urban areas are robust to county fixed effects (see Table D18).

We further test for some non-linearities in the effect by taking the inflow rate squared as additional regressor in the regression. The results in Table D19 for West Germany suggest that there is no threshold above which the negative effect of the grid cell inflow rate is offset. However, since the county inflow positively and the squared county level negatively affects the AfD election outcome, there exists a concave function of the relationship between the county level inflow rate and voting for AfD. For East Germany the findings in Table D20 do not differ between the linear and quadratic specification, since we do not find any statistically significant relationships in the East German sample.

4.6 Conclusion

This paper analyzes the effect of the inflow of refugees in the direct neighborhood on the voting for the German far-right party AfD. The AfD has increased its vote share from 4.7 % in 2013 to 12.6 % in 2017 which is the third biggest share of all parties in the federal elections in 2017. Between 2013 and 2017 the AfD had become openly xenophobic and the inflow of migrants, especially that of refugees, sharply increased during the same period. On the one hand, the refugee inflow was accompanied by many volunteers who helped regarding, food, clothes, and German language courses for refugees. On the other hand, demonstrations against the inflow of refugees occurred. Although the inflow of refugees has had already decreased in 2017, it was still part of the AfD election campaign.

Support for the AfD is strongest in East Germany where the inflow rate of refugees and generally the share of residents with a migrant background is relatively low. Also, within East and West Germany, refugees are unequally distributed between neighborhoods. There is a public and scientific discussion on the influence of living in close proximity to refugees (and/or any non-natives) on attitudes towards immigration. Sometimes it is argued that natives only need to get to know refugees to reduce stereotypes and anti-immigration sentiments. While this is a simplified version of the contact theory, the racial threat theory states that it is possible that fears and anti-immigration sentiments are more pronounced in neighborhoods with a high share of refugees.

We apply a unique dataset of self-collected small-scale election data and socio-economic and demographic information on a very small-scale neighborhood level (1km x 1km). We exploit this data to empirically examine the relationship between refugee inflows and far-right voting (for the party AfD) in the German federal elections of 2017. Our results reveal important heterogeneities. There are substantial differences between West and East Germany but also between urban and rural areas within West and East Germany. We show that refugee inflows into the direct neighborhood leads to reduced far-right voting in West Germany. This effect is especially driven by urban counties. However, the refugee inflow into the whole county leads to higher right-wing voting in the same West German counties, whereas no relationship between the refugee inflow and AfD support is found in

rural areas. This suggests that the contact theory is valid in urban West Germany. In East Germany, there is no statistically significant relationship for the overall sample. This, however, hides a large negative association between the refugee inflow on the county level and AfD support in very urban East Germany. In contrast to West German urban areas (where the inflow into the neighborhood reduces AfD vote shares) in East German very urban counties, the inflow into the whole county matters for a negative impact on AfD support. In rural areas in East Germany, the effect vanishes. Statistically significant effects on the county level can reflect increased media reports about refugees or the (absence of) feared losses regarding communal finances, increased crime rates etc.

Differences between urban and rural regions can be driven by the sorting of different people into these regions. We interact the inflow rate at grid cell level with the Bohemian index (which indicates a tolerant society) and investigate heterogeneities along economic and demographic dimensions. Our findings indicate that there are indeed heterogeneities: The analysis incorporating the Bohemian index suggests that the negative effect of the grid cell inflow rate is driven by counties with more Bohemians in West Germany. In contrast to the racial threat theory, we find that the negative relationship between refugee inflow and AfD support persists even in regions which face a high unemployment rate. Furthermore, the division of the sample by the share of elderly show that the contact theory in West Germany is valid rather in neighborhoods with low shares of elderly.

Germany has become an immigration country within the last years. Further, current developments as in Afghanistan can again increase the inflow of refugees. Our results indicate that if they are applicable to future behavior a further inflow does not significantly increase AfD voting in the receiving neighborhoods. Rather, in urban regions additional inflows can even reduce the support of the far-right.

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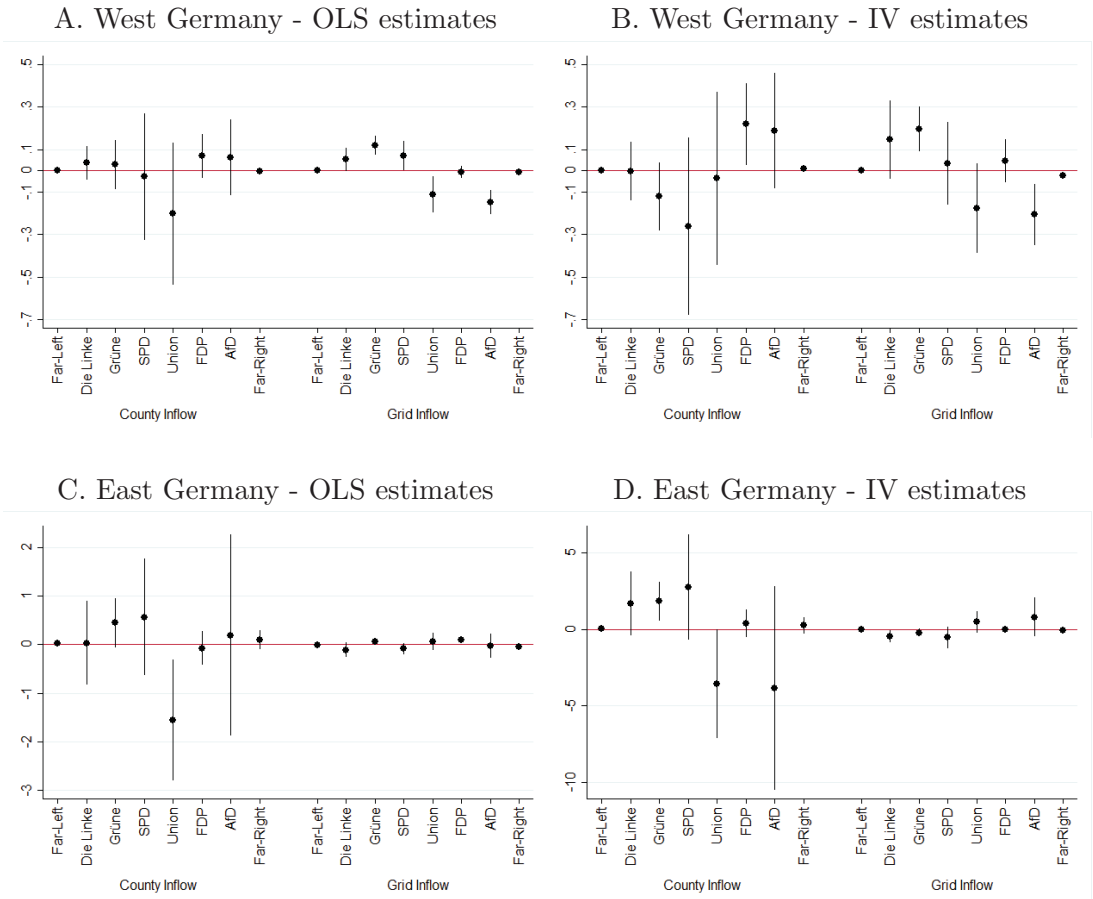
4.7 Appendix

4.7.1 Supplementary Materials and Robustness Checks

Figure D1: OLS and IV Regression Results inflow refugees on other parties' vote shares in West and East Germany



Notes: Far-left parties include the vote shares for MLPD, SGP, B and DKP, while far-right parties compromise the vote shares for NPD and Die Rechte. For graph (c) and (d): The y axes have different scales because both point estimates and standard errors increase strongly in magnitude when running the IV regressions. Coefficients of the Grid Cell Inflow in the IV specification are not instrumented.

Table D1: Federal Election Results 2017: official and data collection

Party	Official	Geo-Vote	Geo-Vote incl postal votes
Union	33	31.56	32.97
SPD	20.5	20.96	20.51
AfD	12.6	13.81	12.58
FDP	10.7	10.29	10.78
Die Linke	9.2	9.67	9.2
Grüne	8.9	8.76	8.97
NPD	0.4	0.43	0.37

Notes: Own calculations based on RWI-GEO-vote and official figures of the Bundeswahlleiter as descriped in section 4.2. Geo-Vote data are weighted by residential population above 18 years. The postal voting votes cannot be distributed below the municipality level.

Table D2: AfD vote share by Urbanity – grid cell level and county level

		AfD vote share Total		**Very uban counties**		**Urban counties**		**Rural counties**		**Very Rural counties**	
		Grid cell	County	Grid cell	County	Grid cell	County	Grid cell	County	Grid cell	County
West Germany	Obs	113,626	113,626	8,276	8,276	48,974	48,974	29,290	29,290	27,086	27,086
	Mean	11.68	11.03	11.59	10.07	11.46	10.66	12.13	11.64	11.62	11.33
	Std. Dev.	3.98	2.89	4.01	2.65	3.32	2.2	4.24	3.02	4.69	3.64
	Min	0	4.94	0	4.94	0	6.14	0	6.99	0	5.25
	Max	36.82	20.68	36.82	19.31	35.9	16.51	33.94	18.3	33.33	20.68
East Germany	Obs	33,249	33,249	1,357	1,357	2,390	2,390	11,928	11,928	17,574	17,574
	Mean	24.92	23.19	20.8	18.57	27.91	26.55	26.95	25.16	23.46	21.75
	Std. Dev.	6.71	5.13	4.29	3.64	3.76	2.44	7.78	6.23	5.76	3.71
	Min	0	12.76	8.09	12.76	11.9	16.06	6.59	15.37	0	17.68
	Max	58.33	35.46	32.9	24.31	47.37	28.9	58.33	35.46	47.54	32.89

Notes: Own calculations based on RWI-GEO-vote and RWI-GEO-GRID.

Table D3: Refugee Inflow and Total Share in 2005 by Urbanity

			Total		**Very urban counties**		**Urban counties**		**Rural counties**		**Very rural counties**	
			Inflow	**Share 2005**	**Inflow**	**Share 2005**	**Inflow**	**Share 2005**	**Inflow**	**Share 2005**	**Inflow**	**Share 2005**
West Germany	Grid cell level	Obs	113,626	113,626	8,276	8,276	48,974	48,974	29,290	29,290	27,086	27,086
		#Zeros	104	51,528	0	665	3	17,002	19	16,359	82	17,502
		Mean	2.42	0.15	7.06	0.45	2.71	0.17	1.6	0.1	1.38	0.07
		Std. Dev.	2.4	0.25	3.89	0.47	2.08	0.25	1.34	0.18	1.14	0.15
		Min	-4.47	0	-0.05	0	-4.47	0	-2.35	0	-1.12	0
		Max	48.1	6.78	48.1	6.78	31.09	6.14	26.12	5.35	15.76	2.12
	County level	Obs	113,626	113,626	8,276	8,276	48,974	48,974	29,290	29,290	27,086	27,086
		#Zeros	0	0	0	0	0	0	0	0	0	0
		Mean	2.91	0.23	8.31	0.6	3.32	0.27	1.88	0.15	1.61	0.12
		Std. Dev.	2.15	0.17	2.39	0.26	1.5	0.14	0.91	0.06	0.74	0.05
		Min	0.58	0.04	4.18	0.24	0.74	0.09	0.58	0.06	0.66	0.04
		Max	18.01	2.04	18.01	2.04	11.44	1.12	7.52	0.42	7.58	0.4
East Germany	Grid cell level	Obs	33,249	33,249	1,357	1,357	2,390	2,390	11,928	11,928	17,574	17,574
		#Zeros	674	26,778	0	552	14	1,761	162	9,522	498	14,943
		Mean	0.61	0.04	1.62	0.09	0.81	0.04	0.55	0.04	0.55	0.03
		Std. Dev.	0.63	0.16	1.07	0.17	0.67	0.12	0.55	0.19	0.54	0.14
		Min	-3.27	0	-1.73	0	-0.88	0	-3.27	0	-2.17	0
		Max	26.26	9.52	26.26	2.7	7.21	1.88	8.2	9.52	8.13	4.67
	County level	Obs	33,249	33,249	1,357	1,357	2,390	2,390	11,928	11,928	17,574	17,574
		#Zeros	0	0	0	0	0	0	0	0	0	0
		Mean	0.69	0.05	1.93	0.12	0.88	0.06	0.63	0.05	0.61	0.04
		Std. Dev.	0.4	0.02	0.4	0.04	0.48	0.02	0.34	0.01	0.21	0.01
		Min	0.29	0.02	1.23	0.06	0.41	0.03	0.29	0.02	0.29	0.03
		Max	2.91	0.18	2.52	0.18	2.24	0.12	2.91	0.09	1.44	0.08

Notes: Own calculations based on RWI-GEO-GRID.

Table D4: Placebo Regression Results for 2013 Elections

	OLS			**IV**		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:						
West						
Inflow refugees in grid cell	-0.016		-0.012*	-0.012		-0.018
	[0.010]		[0.007]	[0.015]		[0.016]
Inflow refugees in county		-0.019	-0.011		0.007	0.018
		[0.026]	[0.027]		[0.041]	[0.045]
R2	0.369	0.369	0.369	0.369	0.369	0.369
Observations	107,953	107,953	107,953	107,953	107,953	107,953
				1st Stage Results		
Kleibergen-Paap F-statistic				190.3	139.2	106.0
Refugees in grid 2005				2.831***		2.343***
				[0.205]		[0.165]
Refugee in county 2005					7.747***	7.775***
					[0.657]	[0.669]
Panel B:						
East						
Inflow refugees in grid cell	0.027		0.045	0.374		0.108
	[0.062]		[0.031]	[0.695]		[0.142]
Inflow refugees in county		-0.054	-0.087		-0.324	-0.395
		[0.273]	[0.274]		[0.628]	[0.708]
R2	0.375	0.375	0.375	0.361	0.373	0.373
Observations	28,299	28,299	28,299	28,299	28,299	28,299
				1st Stage Results		
Kleibergen-Paap F-statistic				1.2	21.0	22.2
Refugees in grid 2005				-0.098		
				[0.089]		
Refugee in county 2005					8.617***	7.629***
					[1.880]	[1.619]

Notes: Robust clustered standard errors on county are reported in parentheses. Regressions include state fixed effects and full set of covariates.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D5: OLS and IV Regression Results for the inflow of refugees on AfD vote share for all Germany

	OLS			**IV**		
	(1)	(2)	(3)	(4)	(5)	(6)
Total Sample						
Inflow refugees in grid cell	-0.019		-0.104***	0.025		-0.083
	[0.043]		[0.030]	[0.072]		[0.080]
Inflow refugees in county		0.147	0.223**		0.226*	0.284**
		[0.093]	[0.095]		[0.122]	[0.139]
R2	0.749	0.750	0.750	0.749	0.750	0.750
Observations	136,797	136,797	136,797	136,797	136,797	136,797
				1^{st} Stage Results		
Kleibergen-Paap F-statistic				174.3	176.2	89.0
Refugees in grid 2005				2.764***		2.190***
				[0.209]		[0.163]
Refugee in county 2005					8.326***	8.320***
					[0.627]	[0.638]

Notes: Robust clustered standard errors on county are reported in parentheses. Regressions include state fixed effects and full set of covariates.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D6: OLS and IV Regression Results inflow refugees and bohemian index on AfD vote share

	OLS		**IV**	
	(1)	(2)	(3)	(4)
Panel A:				
West				
Inflow refugees in grid cell	-0.097***	0.003	-0.100	0.068
	[0.036]	[0.052]	[0.063]	[0.079]
Bohemian index	-3.855***	-2.529**	-3.851***	-2.173*
	[0.844]	[1.114]	[0.854]	[1.313]
Inflow refugees in grid cell x		-0.260**		-0.340*
Bohemian		[0.109]		[0.175]
Observations	108,279	108,279	108,279	108,279
R2	0.39	0.39	0.10	0.11
			1st Stage Results	
Kleibergen-Paap F			179.16	65.36
Refugees in grid 2005			2.78***	1.88***
			[0.207]	[0.309]
Refugees in grid 2005				6.85***
x Bohemian				[0.559]
Panel B:				
East				
Inflow refugees in grid cell	0.096	0.125	0.518	0.507
	[0.263]	[0.403]	[2.623]	[1.966]
Bohemian index	-5.399	-5.290	-5.603	-5.555
	[4.165]	[5.282]	[4.587]	[4.274]
Inflow refugees in grid cell x		-0.095		-0.033
Bohemian		[1.187]		[1.974]
Observations	28,518	28,518	28,518	28,518
R2	0.52	0.52	0.14	0.14
			1st Stage Results	
Kleibergen-Paap F			1.08	3.78
Refugees in grid 2005			-0.09	-0.72*
			[0.088]	[0.420]
Refugees in grid 2005				3.09
x Bohemian				[2.09]

Notes: Robust clustered standard errors on county are reported in parentheses. Regressions include state fixed effects and full set of covariates.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D7: OLS and IV Regression Results inflow refugees on AfD vote share by quartiles of the unemployment rate for West Germany

West	**1. Quartile unemployment**			**2. Quartile unemployment**			**3. Quartile unemployment**			**4. Quartile unemployment**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: OLS												
Inflow refugees in grid cell	-0.168***		-0.160***	-0.087		-0.094**	-0.134***		-0.144***	-0.130***		-0.168***
	[0.056]		[0.043]	[0.056]		[0.042]	[0.051]		[0.038]	[0.038]		[0.044]
Inflow refugees in county		-0.13	-0.022		-0.041	0.02		-0.063	0.027		-0.007	0.12
		[0.127]	[0.131]		[0.122]	[0.123]		[0.101]	[0.099]		[0.080]	[0.091]
R2	0.333	0.331	0.333	0.418	0.418	0.418	0.421	0.419	0.421	0.358	0.352	0.360
Observations	27,247	27,247	27,247	26,970	26,970	26,970	27,082	27,082	27,082	26,980	26,980	26,980
Panel B: IV												
Inflow refugees in grid cell	-0.031		-0.055	-0.179		-0.365**	-0.124		-0.185	-0.139*		-0.168*
	[0.149]		[0.186]	[0.129]		[0.178]	[0.096]		[0.117]	[0.076]		[0.095]
Inflow refugees in county		0.021	0.054		0.186	0.400*		0.059	0.157		-0.021	0.102
		[0.175]	[0.217]		[0.178]	[0.229]		[0.127]	[0.146]		[0.121]	[0.149]
R2	0.331	0.330	0.331	0.417	0.415	0.410	0.421	0.417	0.420	0.358	0.352	0.360
Observations	27,247	27,247	27,247	26,970	26,970	26,970	27,082	27,082	27,082	26,980	26,980	26,980
						1st Stage Results						
Kleibergen-Paap F-statistic	95.2	105.8	38.8	88.2	113.5	38.9	87.6	116.1	59.6	154.5	95.1	67.3
Refugees in grid 2005	1.888***		1.481***	1.963***		1.514***	2.023***		1.620***	3.271***		2.751***
	[0.193]		[0.168]	[0.209]		[0.179]	[0.216]		[0.157]	[0.263]		[0.251]
Refugee in county 2005		7.113***	7.089***		7.652***	7.639***		7.946***	7.979***		7.843***	7.963***
		[0.692]	[0.693]		[0.718]	[0.725]		[0.738]	[0.750]		[0.804]	[0.834]

Notes: Robust clustered standard errors on county are reported in parentheses. Regressions include state fixed effects and full set of covariates.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D8: OLS and IV Regression Results inflow refugees on AfD vote share by quartiles of the unemployment rate for East Germany

East	**1. Quartile unemployment**			**2. Quartile unemployment**			**3. Quartile unemployment**			**4. Quartile unemployment**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: OLS												
Inflow refugees in grid cell	-0.341		-0.122	0.367		0.160	-0.057		-0.045	0.145		-0.011
	(0.412)		(0.622)	(0.371)		(0.590)	(0.815)		(0.795)	(0.630)		(0.941)
Inflow refugees in county		-1.415	-1.323		1.363	1.256		-0.103	-0.069		0.59	0.599
		(0.417)	(0.442)		(0.407)	(0.447)		(0.943)	(0.961)		(0.614)	(0.626)
R2	0.577	0.578	0.578	0.580	0.582	0.582	0.517	0.517	0.517	0.333	0.334	0.333
Observations	7,136	7,136	7,136	7,134	7,134	7,134	7,123	7,123	7,123	7,125	7,125	7,125
Panel B: IV												
Inflow refugees in grid cell			-0.025			1.305			0.924			0.618
			(0.977)			(0.149)			(0.102)			(0.473)
Inflow refugees in county		-1.920	-1.905		-4.944	-5.683		-5.056	-5.710		-1.407	-1.813
		(0.734)	(0.760)		(0.348)	(0.315)		(0.115)	(0.106)		(0.626)	(0.591)
R2		0.578	0.578		0.530	0.526		0.460	0.452		0.319	0.317
Observations		7,136	7,136		7,134	7,134		7,123	7,123		7,125	7,125
						1st Stage Results						
Kleibergen-Paap F-statistic		10.108	10.471		9.203	9.219		19.526	19.345		13.594	16.909
Refugee in county 2005		7.872**	7.211**		6.352**	5.821**		8.032**	7.228**		10.528**	8.998**
		(0.017)	(0.015)		(0.022)	(0.021)		(0.002)	(0.001)		(0.003)	(0.002)

Notes: P-values (in round brackets) come from a wild cluster bootstrap (based on 999 replications). Regressions include state fixed effects and full set of covariates.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D9: OLS and IV Regression Results inflow refugees on AfD vote share by quartiles of the population aged above 60 for West Germany

West	**1. Quartile aged60+**			**2. Quartile aged60+**			**3. Quartile aged60+**			**4. Quartile aged60+**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: OLS												
Inflow refugees in grid cell	-0.203***		-0.188***	-0.106**		-0.126***	-0.063		-0.086**	-0.063		-0.097***
	[0.042]		[0.045]	[0.045]		[0.034]	[0.041]		[0.034]	[0.039]		[0.033]
Inflow refugees in county		-0.204**	-0.049		-0.025	0.06		0.009	0.062		0.034	0.091
		[0.103]	[0.114]		[0.104]	[0.105]		[0.090]	[0.092]		[0.085]	[0.087]
R2	0.406	0.400	0.406	0.421	0.419	0.421	0.375	0.375	0.375	0.342	0.341	0.342
Observations	27,144	27,144	27,144	27,063	27,063	27,063	27,006	27,006	27,006	27,066	27,066	27,066
Panel B: IV												
Inflow refugees in grid cell	-0.212***		-0.224**	-0.009		-0.068	0.060		0.014	-0.052		-0.121
	[0.081]		[0.106]	[0.071]		[0.089]	[0.090]		[0.096]	[0.100]		[0.118]
Inflow refugees in county		-0.135	0.036		0.131	0.171		0.150	0.142		0.166	0.227
		[0.118]	[0.156]		[0.156]	[0.180]		[0.131]	[0.140]		[0.131]	[0.153]
R2	0.406	0.400	0.405	0.420	0.418	0.419	0.373	0.373	0.373	0.342	0.339	0.341
Observations	27,144	27,144	27,144	27,063	27,063	27,063	27,006	27,006	27,006	27,066	27,066	27,066
						1st Stage Results						
Kleibergen-Paap F-statistic	187.8	178.7	57.3	97.6	104.6	49.0	113.1	94.4	69.2	96.4	101.3	57.6
Refugees in grid 2005	3.122***		2.620***	2.326***		1.958***	2.454***		2.014***	2.134***		1.770***
	[0.228]		[0.249]	[0.235]		[0.205]	[0.231]		[0.178]	[0.217]		[0.174]
Refugee in county 2005		7.405***	7.394***		7.349***	7.342***		7.740***	7.809***		8.257***	8.302***
		[0.554]	[0.568]		[0.718]	[0.727]		[0.797]	[0.799]		[0.820]	[0.823]

Notes: Robust clustered standard errors on county level are reported in parentheses. All regressions include state fixed effects and full set of covariates.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D10: OLS and IV Regression Results inflow refugees on AfD vote share by quartiles of the population aged above 60 for East Germany

East	**1. Quartile aged60+**			**2. Quartile aged60+**			**3. Quartile aged60+**			**4. Quartile aged60+**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: OLS												
Inflow refugees in grid cell	-0.164		-0.105	0.185		0.029	-0.175		-0.141	0.424		0.186
	(0.514)		(0.512)	(0.549)		(0.890)	(0.674)		(0.670)	(0.411)		(0.568)
Inflow refugees in county		-0.516	-0.434		0.72	0.699		-0.223	-0.124		1.114	0.977
		(0.757)	(0.805)		(0.506)	(0.522)		(0.857)	(0.930)		(0.457)	(0.486)
R2	0.577	0.578	0.578	0.580	0.582	0.582	0.517	0.517	0.517	0.333	0.334	0.333
Observations	7,136	7,136	7,136	7,134	7,134	7,134	7,123	7,123	7,123	7,125	7,125	7,125
Panel B: IV												
Inflow refugees in grid cell			0.784			1.147			0.975			0.212
			(0.109)			(0.198)			(0.461)			(0.839)
Inflow refugees in county		-6.311	-6.929*		-3.604	-4.302		-3.729	-4.18		0.981	0.872
		(0.117)	(0.099)		(0.367)	(0.320)		(0.427)	(0.419)		(0.826)	(0.871)
R2		0.405	0.397		0.504	0.500		0.474	0.472		0.495	0.495
Observations		7,137	7,137		7,142	7,142		7,116	7,116		7,123	7,123
						1st Stage Results						
Kleibergen-Paap F-statistic		24.587	24.043		17.126	19.770		14.038	16.784		10.636	11.478
Refugee in county 2005		8.409**	7.687**		8.654**	7.628***		8.443***	7.474***		8.434**	7.482**
		(0.002)	(0.004)		(0.001)	(0.000)		(0.000)	(0.000)		(0.007)	(0.004)

Notes: P-values (in round brackets) come from a wild cluster bootstrap with clustering on county level (based on 999 replications). Regressions include state fixed effects and full set of covariates.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D11: OLS and IV Regression Results inflow refugees on AfD vote share (weighted by population)

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:						
West						
Inflow refugees in grid cell	-0.189**		-0.228**	-0.21*		-0.242
	[0.075]		[0.103]	[0.119]		[0.158]
Inflow refugees in county		-0.066	0.113		-0.072	0.108
		[0.066]	[0.121]		[0.090]	[0.175]
R2	0.405	0.393	0.406	0.405	0.393	0.406
Observations	108,279	108,279	108,279	108,279	108,279	108,279
				1st Stage Results		
Kleibergen-Paap F-statistic				79.7	213.0	23.4
Refugees in grid 2005				3.896***		3.321***
				[0.436]		[0.563]
Refugee in county 2005					7.537***	7.779***
					[0.516]	[0.513]
Panel B:						
East						
Inflow refugees in grid cell	-0.132		0.115	-1.327*		1.272**
	[0.302]		[0.135]	[0.788]		[0.559]
Inflow refugees in county		-0.646	-0.748		-3.209*	-4.255**
		[0.994]	[1.028]		[1.688]	[1.982]
R2	0.556	0.558	0.558	0.540	0.531	0.522
Observations	28,518	28,518	28,518	28,518	28,518	28,518
				1st Stage Results		
Kleibergen-Paap F-statistic				9.5	40.7	45.8
Refugees in grid 2005				0.980**		
				[0.318]		
Refugee in county 2005					11.055***	8.812***
					[1.733]	[1.301]

Notes: Robust clustered standard errors on county level are reported in parentheses. Regressions include state fixed effects and full set of covariates.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D12: OLS and IV Regression Results inflow refugees on AfD vote share: refugee inflow at the county level excluding the respective grid cell level

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:						
West						
Inflow refugees in grid cell	-0.127***		-0.148***	-0.145**		-0.206***
	[0.035]		[0.028]	[0.061]		[0.073]
Inflow refugees in county		-0.041	0.063		0.054	0.186
		[0.088]	[0.090]		[0.124]	[0.138]
Observations	108,279	108,279	108,279	108,279	108,279	108,279
R2	0.38	0.37	0.38	0.38	0.37	0.37
				1st Stage Results		
Kleibergen-Paap F				189.98	138.77	106.38
Refugees in grid 2005				2.82***		2.35***
				[0.205]		[0.165]
Refugee in county 2005					7.74***	7.76***
					[0.657]	[0.668]
Panel B:						
East						
Inflow refugees in grid cell	0.009		-0.030	0.285		0.797
	[0.246]		[0.127]	[2.617]		[0.646]
Inflow refugees in county		0.169	0.190		-3.339	-3.840
		[1.041]	[1.039]		[3.039]	[3.378]
Observations	28,518	28,518	28,518	28,518	28,518	28,518
R2	0.51	0.51	0.51	0.51	0.49	0.49
				1st Stage Results		
Kleibergen-Paap F				1.12	20.62	22.05
Refugees in grid 2005				-0.09		
				[0.089]		
Refugee in county 2005					8.55***	7.60***
					[1.88]	[1.61]

Notes: Robust clustered standard errors on county level are reported in parentheses. Regressions include state fixed effects and full set of covariates.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D13: OLS and IV Regression Results inflow refugees on AfD vote share: refugee inflow rate on a larger neighborhood level (3km x 3km)

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:						
West						
Inflow refugees in larger neighborhood	-0.128***		-0.161***	-0.051		-0.096
	[0.046]		[0.040]	[0.065]		[0.076]
Inflow refugees in county		-0.041	0.074		0.054	0.118
		[0.088]	[0.093]		[0.124]	[0.142]
R2	0.375	0.372	0.375	0.374	0.372	0.374
Observations	108,279	108,279	108,279	108,279	108,279	108,279
				1st Stage Results		
Kleibergen-Paap F-statistic				200.1	138.8	115.3
Refugees in larger neighborhood 2005				4.107***		3.442***
				[0.290]		[0.229]
Refugee in county 2005					7.742***	7.788***
					[0.657]	[0.679]
Panel B:						
East						
Inflow refugees in larger neighborhood	-0.01		-0.078	-0.649		1.2
	[0.341]		[0.184]	[4.410]		[1.008]
Inflow refugees in county		0.169	0.229		-3.339	-4.129
		[1.041]	[1.049]		[3.039]	[3.608]
R2	0.513	0.513	0.513	0.511	0.488	0.484
Observations	28,518	28,518	28,518	28,518	28,518	28,518
				1st Stage Results		
Kleibergen-Paap F-statistic				0.9	20.6	23.2
Refugees in larger neighborhood 2005				0.124		
				[0.133]		
Refugee in county 2005					8.551***	7.125***
					[1.883]	[1.480]

Notes: Robust clustered standard errors on county level are reported in parentheses. Regressions include state fixed effects and full set of covariates.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D14: OLS and IV Regression Results inflow refugees on AfD vote share with FRG and GDR region

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Former FRG						
Inflow refugees in grid cell	-0.117***		-0.133***	-0.116**		-0.170***
	[0.034]		[0.023]	[0.052]		[0.059]
Inflow refugees in county		-0.046	0.047		0.047	0.156
		[0.088]	[0.089]		[0.124]	[0.135]
R2	0.373	0.371	0.373	0.373	0.370	0.372
Observations	107,900	107,900	107,900	107,900	107,900	107,900
				1st Stage Results		
Kleibergen-Paap F-statistic				204.1	138.4	134.8
Refugees in grid 2005				2.768***		2.274***
				[0.194]		[0.141]
Refugee in county 2005					7.738***	7.767***
					[0.658]	[0.671]
Panel B: Former GDR						
Inflow refugees in grid cell	-0.076		-0.120	2.929		0.499
	[0.199]		[0.114]	[9.743]		[0.537]
Inflow refugees in county		0.187	0.276		-3.288	-3.606
		[1.035]	[1.038]		[3.024]	[3.296]
R2	0.517	0.517	0.517	0.435	0.493	0.491
Observations	28,897	28,897	28,897	28,897	28,897	28,897
				1st Stage Results		
Kleibergen-Paap F-statistic				0.2	20.7	21.5
Refugees in grid 2005				-0.046		
				[0.102]		
Refugee in county 2005					8.586***	7.845***
					[1.887]	[1.691]

Notes: Robust clustered standard errors on county level are reported in parentheses. Regressions include state fixed effects and full set of covariates.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D15: OLS and IV Regression Results inflow refugees on AfD vote share with FRG and GDR region for very urban counties

	Very Urban West			Very Urban East		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OLS						
Inflow refugees in grid cell	-0.026		-0.064*	-0.384*		-0.253*
	[0.042]		[0.038]	(0.062)		(0.077)
Inflow refugees in county		0.100	0.147		-4.505	-4.332
		[0.117]	[0.123]		(0.778)	(0.779)
R2	0.402	0.403	0.405	0.585	0.669	0.676
Observations	7,923	7,923	7,923	1,693	1,693	1,693
Panel B: IV						
Inflow refugees in grid cell	0.021		0.052			-0.250
	[0.081]		[0.096]			(0.412)
Inflow refugees in county		-0.078	-0.118		-4.581	-4.423
		[0.137]	[0.164]		(0.226)	(0.238)
R2	0.401	0.398	0.394		0.669	0.676
Observations	7,923	7,923	7,923		1,693	1,693
			1st Stage Results			
Kleibergen-Paap F-statistic	195.2	71.5	130.2		429.1	416.5
Refugees in grid 2005	3.438***		2.957***			
	[0.246]		[0.212]			
Refugee in county 2005		6.798***	6.881***		12.836**	12.807**
		[0.804]	[0.789]		(0.002)	(0.002)

Notes: Robust clustered standard errors on county level are presented in squared brackets. The p-values (in round brackets) come from a wild cluster bootstrap with clustering on county level (based on 999 replications). Regressions include state fixed effects and full set of covariates.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D16: OLS and IV Regression Results inflow refugees on AfD vote share with commuting area fixed effects

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:						
West						
Inflow refugees in grid cell	-0.136***		-0.132***	-0.139**		-0.135*
	[0.025]		[0.027]	[0.060]		[0.073]
Inflow refugees in county		-0.106*	-0.015		-0.106	-0.014
		[0.054]	[0.058]		[0.068]	[0.089]
R2	0.584	0.582	0.584	0.584	0.582	0.584
Observations	108,279	108,279	108,279	108,279	108,279	108,279
				1st Stage Results		
Kleibergen-Paap F-statistic				197.8	275.0	101.8
Refugees in grid 2005				2.720***		2.285***
				[0.193]		[0.163]
Refugee in county 2005					9.146***	9.180***
					[0.552]	[0.554]
Panel B:						
East						
Inflow refugees in grid cell	-0.149		-0.152	0.364		-0.001
	[0.117]		[0.092]	[1.787]		[0.199]
Inflow refugees in county		-0.085	0.024		-1.113	-1.112
		[0.697]	[0.708]		[1.295]	[1.401]
R2	0.682	0.682	0.682	0.680	0.680	0.680
Observations	28,518	28,518	28,518	28,518	28,518	28,518
				1st Stage Results		
Kleibergen-Paap F-statistic				1.4	11.0	11.5
Refugees in grid 2005				-0.102		
				[0.088]		
Refugee in county 2005					11.077***	10.317***
					[3.344]	[3.048]

Notes: Robust clustered standard errors on county level are reported in parentheses. Regressions include commuting area fixed effects and full set of covariates.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D17: OLS and IV Regression Results inflow refugees on AfD vote share with county fixed effects

	OLS	**IV**
	(1)	(2)
Panel A:		
West		
Inflow refugees in grid cell	-0.119***	-0.088
	[0.027]	[0.080]
R2	0.637	0.065
Observations	107,368	107,368
		1st Stage Results
Kleibergen-Paap F-statistic		183.3
Refugees in grid 2005		2.209***
		[0.163]
Panel B:		
East		
Inflow refugees in grid cell	-0.196**	0.509
	[0.094]	[1.521]
R2	0.710	0.014
Observations	28,315	28,315
		1st Stage Results
Kleibergen-Paap F-statistic		1.8
Refugees in grid 2005		-0.114
		[0.085]

Notes: Robust clustered standard errors on county level are reported in parentheses. Regressions include county fixed effects and full set of covariates.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D18: OLS and IV Regression Results inflow refugees on AfD vote share by urbanity for West and East Germany with county fixed effects

	Very Urban (1)	**Urban** (2)	**Rural** (3)	**Very Rural** (4)
West				
Panel A: OLS				
Inflow refugees in grid cell	-0.162*	-0.089***	-0.105**	-0.092*
	[0.092]	[0.022]	[0.040]	[0.049]
R2	0.634	0.570	0.644	0.714
Observations	8,248	47,791	27,619	23,710
Panel B: IV				
Inflow refugees in grid cell	-0.173	-0.126**	0.403*	0.660**
	[0.221]	[0.056]	[0.240]	[0.297]
R2	0.634	0.570	0.627	0.690
Observations	8,248	47,791	27,619	23,710
		1^{st} Stage Results		
Kleibergen-Paap F-statistic	62.3	134.8	20.0	21.3
Refugees in grid 2005	3.241***	2.269***	0.860***	0.804***
	[0.411]	[0.195]	[0.192]	[0.174]
East				
Panel C: OLS				
Inflow refugees in grid cell	-0.050	0.154	-0.192	-0.329*
	(0.723)	(0.472)	(0.299)	(0.052)
R2	0.836	0.504	0.779	0.527
Observations	1,349	2,106	10,803	14,057

Notes: For West Germany – Robust clustered standard errors on county level are reported in parentheses. For East Germany – Robust clustered standard errors on county level are presented in squared brackets. The p-values (in round brackets) come from a wild cluster bootstrap with clustering on county level (based on 999 replications). All regressions include county fixed effects and full set of covariates.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D19: OLS Regression Results inflow refugees (squared) on AfD vote share in West Germany

	OLS			**IV**		
West Germany	(1)	(2)	(3)	(4)	(5)	(6)
Inflow refugees in grid cell	-0.054		-0.097***	-0.003		-0.095
	[0.060]		[0.034]	[0.121]		[0.128]
Inflow refugees in grid cell squared	-0.006**		0.003	-0.008*		-0.004
	[0.003]		[0.002]	[0.005]		[0.005]
Inflow refugees in county		0.23	0.261		0.550**	0.565**
		[0.187]	[0.172]		[0.271]	[0.279]
Inflow refugees in county squared		-0.024**	-0.022*		-0.036**	-0.030**
		[0.011]	[0.012]		[0.015]	[0.015]
R2	0.376	0.375	0.319	0.086	0.080	0.085
Observations	108,279	108,279	108,279	108,279	108,279	108,279
				1^{st} Stage Results		
Kleibergen-Paap F-statistic				117.2	46.6	63.6
Refugees in grid 2005				2.344***		1.898***
				[0.217]		[0.167]
Refugees in grid 2005 squared				0.304***		18.091***
				[0.073]		[1.905]
Refugee in county 2005					8.010***	8.039***
					[1.270]	[1.275]
Refugee in county 2005 squared					-0.243	79.126***
					[0.735]	[18.755]

Notes: Robust clustered standard errors on county level are reported in parentheses. All regressions include state fixed effects and full set of covariates.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D20: OLS Regression Results inflow refugees (squared) on AfD vote share in East Germany

	OLS			**IV**		
East Germany	(1)	(2)	(3)	(4)	(5)	(6)
Inflow refugees in grid cell	0.055		-0.31*	0.676		0.835
	[0.321]		[0.177]	[1.981]		[1.177]
Inflow refugees in grid cell squared	-0.01		0.007	0.015		-0.033
	[0.018]		[0.010]	[0.134]		[0.057]
Inflow refugees in county		2.467	0.639		-0.28	-0.634
		[3.120]	[3.619]		[10.800]	[11.249]
Inflow refugees in county squared		-0.978	-1.214		-1.089	-1.123
		[1.113]	[1.479]		[2.995]	[2.967]
R2	0.513	0.515	0.469	0.123	0.102	0.098
Observations	28,518	28,518	28,518	28,518	28,518	28,518
				1^{st} Stage Results		
Kleibergen-Paap F-statistic				1.4	3.9	4.1
Refugees in grid 2005				-0.010		
				[0.138]		
Refugees in grid 2005 squared				-0.027		
				[0.025]		
Refugee in county 2005					5.886	5.321*
					[3.693]	[3.127]
Refugee in county 2005 squared					17.932	145.289***
					[17.037]	[35.620]

Notes: Robust clustered standard errors on county level are reported in parentheses. All regressions include state fixed effects and full set of covariates.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

4.7.2 Description of Voting Dataset

To generate a comprehensive small-scale dataset of the federal election 2017 we proceed as follows. We intersect populated grid cells with a shapefile of constituencies (Wahlkreise). Grid cells, which belong to more than one constituency, were divided into the parts of the grid cell belonging to each constituency. For these grid cells, the adult population was adjusted by the share of the grid cell within one constituency, i.e. if a third of one grid cell is in constituency A and two thirds in constituency B, and nine adults live in the grid cell, then three adults are allocated to grid cell A and six to grid cell B. The implicit assumption is that population is equally distributed within the grid cell. While this assumption is simplifying, it is reasonable because of the small regional scale of the 1km x 1km grid cell. Afterwards each grid cell-constituency combination is assigned to a municipality by its geographic center. Election results are added to the grid cell-constituency combinations in two different ways depending on the type of the original source. If geometries of the electoral districts are available, the grid cell-constituency combinations of the corresponding municipalities are intersected with the electoral district geometries. The votes were divided among the intersection in proportion to the adult population. Finally, the combinations are aggregated on grid cell level. If geometries are not available, the addresses of the polling stations are used. To ensure that no electoral district is allocated to a wrong grid cell, the procedure is repeated for each constituency-municipality combination separately. If addresses of all polling stations within a constituency-municipality combination are known, the polling station is allocated to the grid cell with the minimum distance between the grid cell center and the polling station. By this, the possibility of coarse misspecifications is eliminated. Remaining grid cells cannot be allocated to wards, as the minimum distance to mere addresses of polling station would be prone to errors. Therefore, the grid cells without allocation to a ward, are filled with their population proportionate share of the valid votes at municipality level. Table D1 displays the official German wide election results and our data aggregated. It becomes obvious that the results are quite similar without any systematical shift.

Eidesstattliche Versicherung

Ich, Frau Melinda Janina Fremerey, versichere an Eides statt, dass die vorliegende Dissertation von mir selbstständig und ohne unzulässige fremde Hilfe unter Beachtung der „Grundsätze zur Sicherung guter wissenschaftlicher Praxis an der Heinrich-Heine-Universität Düsseldorf“ erstellt worden ist.

Düsseldorf, der 11.02.2022 — Melinda Fremerey