Determinants and Effects of Fiscal and Industrial Policies

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*It always seems impossible
until it's done.*
(Nelson Mandela)

Admittedly, the fact that these are indeed the final steps of this four-year journey, still has something incredible to me. Without the opportunities, support, and occasional friendly word of encouragement I was offered along the way, this dissertation would not have been possible. I am very grateful.

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Samina Sultan
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Preface

In the wake of the covid-19 pandemic and the ensuing global recession, the role of the state in the economy and its instruments for reform have recently received increased attention. The various fiscal and industrial measures governments around the globe are implementing to tackle the recession have triggered a debate about their appropriateness. For instance, a popular intervention have been reductions to the value-added tax in order to revive consumption. However, it is uncertain how much of the decrease will actually be passed on to consumers and whether the foregone tax revenue could have not been spent more efficiently and effectively (Fuest; 2020). Moreover, there is a controversy about the state taking a stake in companies, such as in the airline or the pharmaceutical industry. Should this, in turn, entail that the government takes an active role in management decisions, and what is the impact on competition?

Despite these current debates and controversies, the role of the state and the influence of its policies have long been an issue in the economic literature. Going back to the Great Depression, John Maynard Keynes' ideas about state intervention became the dominating paradigm at the time (Keynes; 1936). Despite subsequent criticism and refinements, his theory still constitutes some of the principles of modern fiscal and industrial policy and established the notion that the state does matter to the economy. Building on this, the economic literature has found ways to critically evaluate the performance of state interventions. For instance, by estimating who actually carries the economic burden of taxes (Fuest, Peichl and Siegloch; 2018; Fuest, Dolls, Krolage and Neumeier; 2019; Löffler and Siegloch; 2018; Carbonnier; 2007) or the effect of other measures, such as rent controls (Diamond et al.; 2019), it is possible to assess whether policies fulfill their intended goals. Equally important, a strand of literature has focused on the determinants of fiscal and industrial policies, such as political and institutional arrangements (Alesina and Tabellini; 1990; Persson and Svensson; 1989; Roubini and Sachs; 1989).

This dissertation contributes to that research in different dimensions. In Chapters 1 and 2 the institutional setting in Germany is exploited to analyze both the determinants and the effects of fiscal policies. In Chapter 3 the effects of a fiscal policy shift on the European Union are evaluated in a theoretical model. Finally, Chapter 4 focuses on industrial policy

and whether the Chinese government influences the cross-border mergers and acquisition strategies of Chinese companies. This thesis therefore aims for a better understanding of the determinants and effects of fiscal and industrial policies in a number of different contexts. To achieve this goal, I apply a variety of econometric techniques, including an event study design, a differences-in-differences estimation, and an instrumental variables approach. Moreover, I complement the empirical models with a theoretical one in Chapter 3. In the following, the four chapters are summarized.

In *Chapter 1* I evaluate the incidence of the value-added tax. The value-added tax (VAT) is one of the most important tax revenue sources in many countries. However, it is sometimes considered unfair as it ultimately hits consumption, and poorer households spend a greater share of their income on consumption. But this depends on whether, and to what degree, the value-added tax is actually passed on to consumers. Exploiting an exogenous value-added tax reform in Germany, I use an event study and a differences-in-differences approach to investigate the pass-through to consumers for a wide range of commodities. On average, I find a modestly positive but statistically insignificant effect on prices. However, there are differences in tax incidence between commodity groups, ranging from negative price effects to an over-shifting of the tax burden. Simply relying on the average incidence would therefore be misleading. Policy makers should consider this when reforming the VAT rate. Particularly as I show that the consumption of certain commodities seems to differ between income groups in the population, VAT reforms could have unintended distributional effects. Moreover, I observe anticipatory price effects well in advance of the actual implementation of the value-added tax reform. A possible explanation for the low average effect in 2007 could be a simultaneous reduction in social security contributions in Germany. I explore this possibility by comparing the price effects in 2007 with those of a VAT reform in 1998 in Germany, which was not accompanied by a reduction in social security contributions. I indeed find that the magnitude of price effects is noticeably higher in 1998, suggesting that the reduction in social security contributions in 2007 absorbed some effect of the VAT increase. However, for both VAT reforms the price increase is on average statistically insignificant and, therefore, I cannot reject the null hypothesis of no price effect. While one needs to be careful not to interpret this as proof for no price effect, this result at least provides no clear evidence that modest VAT increases are necessarily passed through to consumers and thereby casts doubt on the hypothesis that the VAT is incontrovertibly a regressive tax on consumption. In turn, if the VAT should indeed not be shifted to consumers, it would be carried by producers, ultimately hitting either wages or profits. If the former applies, the VAT would after all cause distortions on the labor-market, which would have important policy implications.

Chapter 2 is based on joint work with Lea Immel and Florian Neumeier. We turn to

the factors that determine fiscal policy. Economic theory predicts that income inequality is such a factor impacting the provision of public services and redistributive policies. But the nature of the relationship between fiscal policy and income inequality is controversial, as there are both hypotheses of a positive and a negative relationship. We therefore exploit the specific institutional setting in Germany, which grants local authorities a high degree of fiscal autonomy, to identify the causal effect of income inequality on local fiscal policy in German city districts. We concentrate on inequality driven by the lower bound of the income distribution, that is, economic deprivation, as this scenario aggravates the importance of redistributive policies. Specifically, we study the effect of three different measures of economic deprivation on fiscal policy, namely the poverty gap, the poverty rate, and the median gap. Fiscal variables include local tax rates as well as spending on local public services. Using a Bartik-style instrumental variables approach, which predicts changes in regional economic deprivation through national income trends, allows us to overcome confounding effects like mobility and spatial segregation. Our results are ambiguous regarding the distributional consequences of economic deprivation. We find that increasing economic deprivation causes local policy makers to increase the local business tax rate, while we do not find significant effects on the local property tax. Given that the local business tax is likely to be perceived as a progressive tax, whereas the perception of the property tax is more ambivalent, this seems like an attempt to make the tax system more redistributive as economic deprivation increases. However, aggregate spending on local public services is negatively affected by economic deprivation. In particular, this effect is driven by a spending cut on welfare, schooling, and sport facilities. As these public services are likely to be of most benefit to lower income groups, our results suggest a negative relationship between economic deprivation and redistributive fiscal policies on the expenditure side. We discuss possible explanations for this ambivalence as well as potential transmission channels.

Chapters 1 and 2 focus on fiscal policy in Germany, whether at the national or at the local level. *Chapter 3*, which is joint work with Clemens Fuest, extends the analytical framework to the European Union. It is motivated by the fact that once the United Kingdom (UK) leaves the European Union (EU) entirely, it is theoretically no longer subject to EU state aid regulations, which prohibit member states from offering preferential tax treatment to certain companies or sectors. Thus, the UK could target its corporate tax policy to specific firms or sectors more aggressively. This may have consequences for corporate tax competition throughout Europe. To investigate the implications of a policy shift towards tax discrimination, we develop a model of tax competition with three countries that initially form a union where countries refrain from using different tax rates in different sectors of the economy. Our analysis of the impact of one country leaving the union leads to three key results. First, we show that the introduction of discriminatory

taxation in one country increases tax policy heterogeneity within the remaining union, regarding tax rates as well as revenue. Second, if the two countries remaining in the union harmonize their tax rates, the introduction of tax discrimination in the third country redistributes tax revenue between the countries remaining in the union. The country with lower taxes before harmonization loses while the high tax country benefits. Third and most importantly, the incentives for tax harmonization among the countries remaining in the union decline as the third country introduces discriminatory taxation. We discuss these results in the context of the debate on the tax policy implications of Brexit. This paper is published in the National Tax Journal (Fuest and Sultan; 2019).

In *Chapter 4* the focus shifts from fiscal to industrial policy. It is joint work with Clemens Fuest, Felix Hugger, and Jing Xing. We analyze whether the cross-border mergers and acquisitions strategies of Chinese companies differ from those of other international investors. The underlying motivation is the growing unease of Chinese investment, particularly in Europe and the US, which roots in the suspicion that the Chinese government takes an active role in shaping these strategies. Critics argue that Chinese investors outbid competitors with help from their government, that the acquisitions lead to undesirable technology transfer, or that they may have negative consequences for the employees of the target firm. This debate, however, is mostly based on speculations and anecdotes. We therefore use a large deal-level dataset on cross-border acquisitions to investigate whether Chinese foreign acquisitions differ from cross-border investment coming from other countries. We find that relative to non-Chinese investors, Chinese acquirers indeed appear to be different in some dimensions. They focus on targets with lower profitability, more assets, higher levels of debt, and more patents. In contrast, we do not find that target countries' institutional qualities, such as political stability and the rule of law, play a different role in determining Chinese cross-border acquisitions than they do for non-Chinese investors. Moreover, Chinese companies do not seem to pay more for targets with given characteristics, questioning the view that they are subsidized to outbid other investors. Policy initiatives like the *Belt and Road Initiative* and *Made in China 2025* influence state-owned but not private Chinese investors, suggesting that geopolitical or technology interests play a role. Finally, a key question is whether Chinese acquisitions have a different impact on the development of target firms, including their employees. In the years after the takeover, target companies acquired by Chinese investors exhibit lower growth in capital productivity but a higher growth of employee compensation. This paper is available as EconPol Working Paper (Fuest, Hugger, Sultan and Xing; 2019).

The four chapters in this thesis are self contained, and each is followed by appendices with additional material. A consolidated bibliography is presented at the end of the thesis.

Chapter 1

Who Carries the Burden of the Value-Added Tax?
Evidence from Germany

1.1 Introduction

The value-added tax is one of the most important tax revenue sources in many countries. For instance, it accounts for roughly 30% of overall tax revenue in Germany. As indirect tax it has the advantage of being hard to avoid, which makes it an efficient tax instrument in terms of revenue collection (Keen and Lockwood; 2010). Another advantage is that, in contrast to the income tax, it supposedly does not distort labor-market decisions. Yet, the value-added tax (VAT) is sometimes considered unfair, as it ultimately hits consumption, and less wealthy households spend a greater share of their income on consumption.[1] Also in that vein, a number of countries have recently announced or implemented cuts to the VAT rate in order to stimulate consumption during the covid-19 pandemic.[2] But all of this hinges on the assumption that the VAT is fully passed through to the consumer. However, this assumption is hardly so straightforward. Depending on, for instance, demand elasticities and industry competition, theory predicts that the burden of the VAT might be shared with the producer (Stern; 1987; Besley; 1989; Weyl and Fabinger; 2013). How the

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[1]Keen (2007) provides as overview of recent criticism and threats to the VAT.

[2]For instance, the German government has implemented a temporary decrease to the standard VAT rate by 3 percentage points and by 2 percentage points to the reduced VAT rate. Moreover, restaurants are temporarily subject to the reduced rate instead of the standard rate. Similarly, the UK has announced a VAT decrease for hospitality, hotel accommodation, and admissions to certain attractions.

tax burden is actually distributed has important policy implications as the progressivity of the tax system is, in part, determined by this question[3] and also its actual impact on the labor-market. This is where this paper aims to make a contribution by exploiting a VAT reform in Germany in 2007. Using detailed price data, I employ an event study and differences-in-differences (DiD) approach to investigate the incidence of the VAT for a wide range of commodity groups. French prices for the same product groups serve as control group. Thereby, I avoid potential general equilibrium effects that could arise with using goods in the same country, not affected by the reform, as control group instead (Benedek et al.; 2019).[4]

I find that on average the 2007 VAT reform in Germany had a modestly positive but statistically insignificant effect on prices. Therefore, I cannot reject the hypothesis that the increase in the VAT has not been passed on to consumers. However, there are differences between individual commodity groups, ranging from negative price effects to an over-shifting of the tax burden. Moreover, I observe anticipatory price effects well in advance of the actual implementation of the reform. Possible explanations for the rather low average effects are discussed. In particular, I focus on a simultaneous reduction in social security contributions in Germany. Employers could have used this reduction in non-wage labor costs to lower prices, particularly in labor-intensive sectors. I explore this possibility by comparing the price effects in 2007 to a VAT reform in 1998, which was not accompanied by a reduction in social security contributions. I indeed find that the magnitude of price effects is noticeably higher in 1998, suggesting that the reduction in social security contributions absorbed some effect of the VAT increase in 2007. However, even in 1998, the price increase is on average statistically insignificant. While this should not be interpreted as proof for no price effect, these results at least provide no clear evidence that modest increase in the VAT are necessarily passed on to consumers. Therefore they question the hypothesis that the VAT is incontrovertibly a regressive tax on consumption. This means that the VAT could also be carried by producers, ultimately hitting either wages or profits. If the former applies, the VAT would after all cause distortions on the labor-market, which would have important policy implications.

I contribute to the literature in several ways. So far a sizeable fraction of the literature on the VAT incidence has concentrated on relatively large reforms that only applied to very particular sectors. For instance, Kosonen (2015) and Benzarti et al. (2020) both study a VAT reform to the Finnish hairdressing sector, where the VAT was decreased from 22% to 8% in 2007 and subsequently increased to its former level again in 2012. Kosonen (2015) studies only the VAT reduction and shows that hairdressers adjusted prices by half

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[3]Saez and Zucman (2019) find that the consumption taxes make the US tax system overall regressive.
[4]Note that there was no change to the VAT rates in France during the considered period.

of what full pass-through would imply. Consequently, hairdressers were able to increase their profits significantly. Benzarti et al. (2020) focus on the asymmetric price reaction to the decrease compared to the increase in the VAT rate. In particular, they find that prices react twice as much to the increase than to the decrease. Similarly, Benzarti and Carloni (2019) exploit a decrease in the VAT on French sit-down restaurants from 19.6% to 5.5% in 2009 to study the impact on workers, firm owners, consumers, and suppliers. They find that firm owners profited the most from the tax cut as there is a large increase in their profits, while consumers benefited least as prices only decreased slightly.[5] Carbonnier (2007) studies two large VAT reforms in France that took place in 1987 for car sales and in 1999 for housing repair services. While taxes are under-shifted on prices in both markets, the degree of under-shifting is significantly lower in the housing repair service market compared to the car market. Carbonnier (2007) argues that this might reflect that market competition is higher in the housing repair service market. Gaarder (2018) exploits a significant drop in the VAT on food in Norway from 24% to 12% to study both incidence and distributional effects of the reform. Using a regression discontinuity model, Gaarder (2018) finds that food prices fully adjust to the 12% decrease in the VAT. Moreover the reform lowers inequality in consumer welfare, partly because the income share that poor households spend on food items is higher and partly due to shifting expenditure patterns after the price changes.[6] However, as these studies focus on comparatively large VAT rate reforms in very particular sectors, their external validity is limited. In contrast, the German reform I study was modest and applied to a wide range of commodities.

Other studies such as Benedek et al. (2019) or the second part of Benzarti et al. (2020) focus on a cross-country comparison by pooling all VAT reforms in the European Union over approximately 20 years. Among other things, Benedek et al. (2019) study the difference in the pass-through depending on whether the VAT reform concerned the standard rate or the reduced rate. They find that prices are only fully adjusted to changes in the standard VAT rate. Benzarti et al. (2020) confirm their previous finding of asymmetric price reactions to VAT decreases compared to increases also for the pooled European dataset, while Benedek et al. (2019) cannot confirm that. Buettner and Madzharova (2017) also conduct a cross-country comparison of 22 European countries but focus on the pre-announcement effect of VAT tax reforms on sales and prices of durable goods. Their results support the assumption that the incidence of the VAT is fully borne by the consumer but also that most of the

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[5]Falkenhall et al. (2018) also exploit a significant drop in the VAT on restaurants and catering services from 25% to 12% in Sweden. Using a synthetic control approach, they show that the restaurant industry performed better after the reform on a number of indicators, such as higher profit margins and employment, but they do not study price effects.

[6]Mariscal and Werner (2018) also look at incidence and welfare effects of the VAT by exploiting two reforms in Mexico, which only applied to certain cities.

price adjustments already take place before the implementation on the reform. Moreover, they find intertemporal shifts in consumption and purchases due to the tax reforms.

This paper also contributes to the literature studying the consequences of the 2007 VAT reform in Germany. Most related, Danninger and Carare (2008) study its effect on core inflation in Germany by comparing VAT and reduced-VAT items. They find that core inflation did not rise much after the implementation of the reform, as there had already been anticipatory effects during 2006. However, the suitability of their comparison group is highly questionable, as the group of reduced-VAT items mostly consists of food. Due to the World food crisis of 2006 to 2008, food prices rose dramatically during that time period. Also, Danninger and Carare (2008) do not provide any evidence that the common trends assumption of their differences-in-differences approach is fulfilled.[7] Buchheim and Link (2017) use the VAT reform to study the effect of new information on expectations. They compare German durable and non-durable goods retailers, arguing that the former had more reliable information about future demand due to the reform. Buchheim and Link (2017) find that durable goods retailers indeed become more forward-looking. Comparing German households during the VAT reform to those in other European countries, D'Acunto et al. (2016) observe an increase in German households' inflation expectations and willingness to buy durable goods. I extend this literature by studying the incidence of the 2007 VAT reform.

More generally this paper is related to the growing literature on tax incidence, such as of the corporate tax (Fuest, Peichl and Siegloch; 2018), the real estate transfer tax (Fuest, Dolls, Krolage and Neumeier; 2019), the property tax (Löffler and Siegloch; 2018), or energy taxes (Stolper; 2017; Fuest, Schober and Woll; 2018). These studies reveal that tax incidence does not follow straightforward assumptions and needs to be validated empirically.

Finally, by also discussing the implications of a simultaneous decrease in non-wage labor costs on commodity prices, this paper contributes to the literature on fiscal devaluations. Fiscal devaluations describe tax reforms that intend to shift the burden of taxation from income to consumption, with the aim of increasing competitiveness. Such reforms were for instance frequently proposed during the Euro crisis to make the Southern European countries more competitive.[8] The literature studying fiscal devaluations has so far mostly concentrated on the effects on trade (see, for instance, Holzner et al. (2018) or Ivens (2018)), while I consider the possible effects on prices.

The remainder of this paper is structured as follows. Section 1.2 describes the insti-

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[7]See Section 1.4 for a more detailed discussion of reduced-VAT items as suitable control group for the 2007 VAT reform in Germany.

[8]Fiscal devaluations were for instance implemented by Spain in 2010 and France in 2014.

tutional setting of the VAT system in Germany and the 2007 reform. In Section 1.3, I introduce and describe the datasets. The empirical model is presented in Section 1.4. The main results on tax incidence are presented and discussed in Section 1.5. A number of robustness tests are conducted in Section 1.6. Section 1.7 concludes.

1.2 The 2007 VAT Reform in Germany

The current value-added tax system in Germany was introduced in 1968.[9] It broadly consists of a standard and a reduced rate.[10] The reduced rate applies to basic needs of everyday life such as most food products, cultural and educational goods and services, and a range of medical products. The standard rate applies to most other commodities. Figure 1.1 shows the development of both rates from 1968 to 2019. The standard VAT rate was set to 10% in January 1968. Over the course of the years, it increased successively by 1 percentage point in July 1968, January 1978, July 1979, July 1983, January 1993, and April 1998 to reach 16%. But the biggest increase took place in January 2007, when it increased by 3 percentage points from 16% to 19%. The reduced VAT rate was initially set to equal half of the standard rate. Thus, in January 1968 it amounted to 5%. Until 1983, it increased together with the standard rate to reach 7%. However, it has not been altered since then.

The 2007 VAT reform was already part of the coalition agreement between the Social Democrats and the Conservatives, which was passed in mid-November 2005.[11] The aim was to raise tax revenue in order to consolidate the budget. Germany needed to cut its debt, as it had violated the 3% deficit-to-GDP rule, part the EU Maastricht Treaty, since 2001. The EU Commission had already opened a deficit procedure. Germany therefore needed to announce credible plans to cut its deficit in order to avoid potential fines. This implies that the 2007 VAT reform in Germany can be considered as independent of future economic conditions and as an exogenous policy shock. In May 2006, the German Federal Parliament (*Bundestag*) agreed on the supplemental budget law, which included the VAT reform. Another important part of the supplemental budget law was a decrease in the unemployment insurance contributions by 2.3 percentage points to 4.2%. While the effect of this on households' disposable income is modest[12] and any effect on prices through the

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[9]The previous system did not include an input tax deduction.

[10]There are certain goods and services that are entirely exempt from the VAT, for example most medical treatments. Moreover, for agricultural businesses there is a special VAT regime with rates of 10.7% and 5.5%.

[11]The coalition agreement also already included the exact increase in the VAT rate by 3 percentage points.

[12]Considering that contributions to the pension and health insurance increased at the same time and also

Figure 1.1: Development of the Standard and Reduced Value-Added Tax Rate in Germany

demand side should therefore be limited, there might be an effect on prices through the supply side, as employers might use this reduction in non-wage labor costs in Germany to lower prices. This might particularly be the case in labor-intensive sectors. I will discuss this in more detail in Section 1.5.3. The VAT reform took its final legislative hurdle when the German Federal Council (*Bundesrat*) approved the respective law in mid-June 2006. There had been some controversy on whether the Federal Council would approve the law. Only a compromise regarding the federal contributions to public transport ensured its timely implementation.[13] Now it was legally certain that the standard VAT rate in Germany would be raised from 16% to 19% on January 1st, 2007. This is why June 2006 is chosen as reference period relative to which price changes are measured in the empirical strategy as will be explained in Section 1.4. The reform was then implemented with the change of the year. It seems to have reached its goal: revenue from the VAT jumped from €111 billion in 2006 to €128 billion in 2007 and has since then steadily increased to €175

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that lower contribution payments increase the income tax base, the decrease in unemployment insurance contributions does not amount to a significant increase in disposable income. Taking the mean income in West Germany and assuming a single household, a back-of-the-envelope calculation results in an increase in the disposable income by €16.1 per month.

[13]See, for instance, Sueddeutsche Zeitung (2006) for media coverage on the decision by the Federal Council in June 2006.

billion in 2018 (Statista; 2019). Moreover, in 2007, Germany was able to comply with the 3% deficit-to-GDP rule, again.

1.3 Data and Descriptive Statistics

Table 1.1: Descriptive Statistics for Price Data

Commodity group	Part A: German Price Index			Part B: French Price Index		
	Mean	Standard dev	N	Mean	Standard dev	N
Alcoholic beverages	102.80	2.74	160	101.93	2.30	48
Audiovisual equipment	91.67	12.07	400	88.26	14.26	144
Clothing	100.71	2.39	640	100.70	1.14	80
Footwear	101.10	2.49	112	100.82	1.10	48
Furniture, carpets, home-textiles	100.74	2.97	432	101.02	2.03	128
Glass- & tableware, household utensils	101.56	2.44	176	102.22	2.32	48
Household appliances	99.54	3.26	272	96.73	3.40	144
Household maintenance	101.83	2.86	224	102.05	2.05	32
Non-alcoholic beverages	106.14	8.94	128	103.31	3.29	48
Personal care	101.41	3.07	448	100.94	2.39	64
Personal items	101.58	2.07	224	101.60	1.88	64
Recreational items	99.27	4.62	368	99.64	4.20	80
Recreational activities	102.95	5.70	80	103.72	2.83	16
Restaurants & hotels	102.88	2.60	320	104.69	4.12	64
Service & repair	103.16	3.56	256	104.89	4.07	144
Stationary	102.10	2.93	144	102.92	2.53	32
Tobacco	106.63	5.17	48	103.35	4.41	48
Tools & equipment	102.19	3.63	240	102.20	3.03	48
Vehicles purchase	103.53	3.66	208	102.45	5.02	112
Total	100.80	5.72	4880	100.23	7.20	1392

Notes: The table shows the mean, the standard deviation, and the number of observations (N) for both the German and the French seasonally adjusted price index by commodity groups.

In order to measure the incidence of the VAT, detailed price data is needed. For Germany, this data is provided by the German Federal Statistical Office. The data is recorded on the most disaggregated 10-digit level based on the *Classification of Individual Consumption According to Purpose* (COICOP). The data is available on a monthly basis from 1991 to 2015. For the given analysis, I restrict the sample to two years before and after the VAT reform, thus, to the years 2005 to 2009.[14] Moreover, I only consider those goods that are subject to the standard VAT rate in the sample period according to the German VAT tax law (*Umsatzsteuergesetz*).[15]

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[14]Benedek et al. (2019) show that noticeable price effects due to a VAT reform take place within a one-year frame before and after the respective reform. Thus, considering a two-year window is a conservative approach. Moreover, considering a longer time window would increase the risk that other events, which also affect prices, take place.

[15]§12 of the German VAT tax law regulates which commodities are subject to what VAT rate. Moreover, the European Commission publishes an annual report on the VAT rate applied to commodities in

This leads to a balanced panel of 305 different commodities, which I categorize into 19 commodity groups. These roughly correspond to the 3-digit level according to the COICOP classification.[16] Note that I deviate from the COICOP classification by grouping all service and repair commodities into a separate category. I also remove fuel and energy commodities, as their prices are not entirely market-driven and even partly state-regulated in Germany.[17]

Figure 1.2: Development of the Price Index for All Commodities
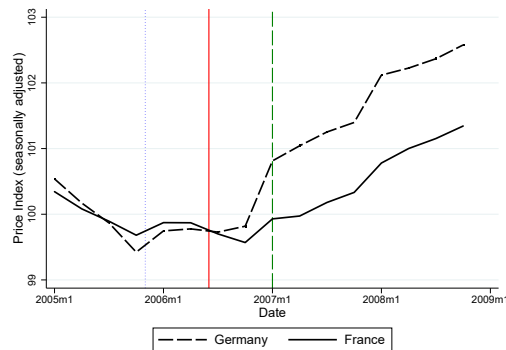
Notes: This figure shows how the seasonally adjusted price index for commodities subject to the standard VAT rate developed in Germany and France from 2005 to 2009 on average. The dotted blue vertical line marks the passing of the coalition agreement in November 2005. The solid red vertical line marks the final legal decision on the reform to the standard VAT rate in June 2006. The dashed green vertical line marks the implementation of the respective VAT reform in January 2007.

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its member states. See *https://circabc.europa.eu/faces/jsp/extension/wai/navigation/container.jsp* for the public documents repository on the VAT rates applied in the EU member states.

[16]Table 1.A1 in the Appendix shows how individual commodities are mapped in the commodity groups according to the COICOP classification. Note that jewelery is removed from the category of personal items, as prices for jewelery increase abnormally in France.

[17]To be precise, for example the electricity price is made up of three components: procurement, sales, margin (22%), network usage fees (24%), duties and taxes (54%). Since 2007 the network usage fees are state-regulated in Germany. The regulation initially led to a decrease of the network usage fees. On the total sum of the first two components and all other duties and taxes, the value added tax is levied. That means it is not simply levied on the wholesale price. Moreover, one should note that the given data contains consumer prices. Hence, it also includes the basic charge for electricity and potential bonuses. Thus, the given data is only to a limited degree suitable to study the VAT pass-through in the electricity sector, as there might be simultaneous developments for example to the network usage fees or the basic charge that also affect the electricity price. Previous studies such as Benedek et al. (2019) have similarly dropped the sector due to the fact that the price is not market-driven. Nevertheless, in unreported regressions, I also look at the VAT incidence for fuel and energy commodities. I find that the point estimates for the price effect are close to zero for both groups and statistically insignificant at the 10%-level. However, given the previous explanation, these results should be interpreted with caution.

I complement the German price data with French data for the same time period.[18] It is retrieved from Eurostat's Harmonized Indices of Consumer Prices (HICP), which is also available on a monthly basis. However, it is on a 5-digit level according to the COICOP classification. Thus, it is less disaggregated than the German data so that I am left with a balanced panel of 87 commodities, which are then also categorized into the 19 commodity groups.[19] To illustrate what the different aggregation levels imply, I will detail it for the clothes sector: the German data contains prices for different kinds of women's clothes, such as dresses or shirts. The French data, meanwhile, only has prices for women's clothes on average, without any further differentiation. The same is true for men's and children's clothing. For both French and German data, all types of clothes are then categorized into the commodity group *Clothing*.

Figure 1.3: Relative Development of the Price Index for All Commodities
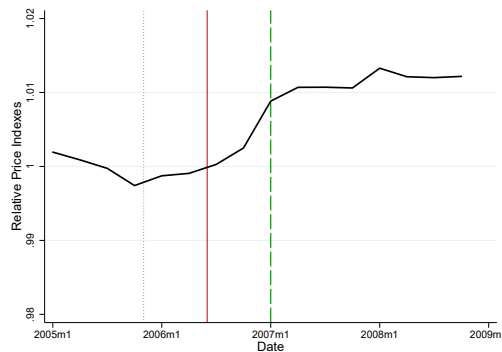
Notes: This figure shows the relative development of the French and the German seasonally adjusted price index for commodities subject to the standard VAT rate from 2005 to 2009 (German price index divided by French price index). The dotted blue vertical line marks the passing of the coalition agreement in November 2005. The solid red vertical line marks the final legal decision on the reform to the standard VAT rate in June 2006. The dashed green vertical line marks the implementation of the respective VAT reform in January 2007.

As the base year for the German price index is 2010 while it is 2005 for the French price data, I furthermore recalculate the base year of the German data to match the French base year.[20] Finally, to remove some of the volatility and seasonality in the data, the price index is first averaged on a quarterly basis for each commodity and, secondly, annually repeating patterns are accounted for. Table 1.1 provides descriptive statistics by commodity group and the overall price index for both Germany and France.

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[18] A detailed discussion of why French prices are chosen as control group follows in Section 1.4.

[19] Note that I code the commodity groups as country specific to account for the fact that the same commodity group in the two countries might be subject to different trends.

[20] Note that the German Federal Statistical Office is currently revising the consumer price index data. The new base year will be 2015 and the data with base year 2010 is no longer available. However, the data I use remains valid, as the current revision does not affect data before 2015.

Figure 1.2 shows how the price index for the selected sample of commodities developed in Germany and France from 2005 to 2009. The dotted blue vertical line marks the passing of the coalition agreement in November 2005, the solid red vertical line marks the final legal decision on the VAT reform in June 2006, and the dashed green vertical line highlights its implementation in January 2007. Prices in both countries first contract and then stagnate. This development is very similar in both countries until the decision on the VAT reform in Germany, which provides first graphical evidence that the French price index is a suitable control group for the German price index, as they follow parallel trends throughout the year and a half before the decision on the reform. Then the trends for the two indices start to differ. While both increase, the increase is steeper for German prices until January 2007, the implementation of the VAT reform. Afterwards, German and French prices follow a similar increasing trend. Figure 1.2 therefore seems to suggest that the effect on prices due to the increase in the VAT took place before the actual implementation of the reform, at the time of its final legal decision. Moreover, the effect seems rather modest. These findings are corroborated by Figure 1.3, which shows the relative development of the German to the French price index. It oscillates around the value 1 until mid-2006 and then jumps to 1.01.

While Figures 1.2 and 1.3 show the average trend for all commodities, the question is whether the development for the individual commodity groups differs. Figure 1.4 therefore shows the development of the German and French price index for selected commodity groups.

Figure 1.4a compares the development of the price indices for the commodity group of alcoholic beverages only. Again, before the decision on the VAT reform in Germany, the two indices follow parallel trends. Then there is a marked and steady increase in both indices but the increase is steeper for German prices. This seems to suggest that prices for alcoholic beverages in Germany did react to the VAT reform, albeit to a modest degree, and that this reaction again took place before the actual implementation of the reform. Figure 1.4b shows that prices for household appliances developed similarly negative in France and Germany until the beginning of 2006. In France, the negative trend continues, while prices stabilize in Germany at around the time of the VAT reform and even increase slightly in 2007. Thus, for the interpretation of the following regression results it is important to note that prices for household appliances in Germany and France do not follow parallel trends from around 2006 onwards and that French prices decrease sharply.[21] In Germany, prices for personal care items, such as hygiene products, increase noticeably around the time the

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[21]Note that the development for French prices is not driven by a one specific commodity within that group. Instead, prices for all commodities in that commodity group decrease sharply.

Figure 1.4: Development of the Price Index for Selected Commodity Groups
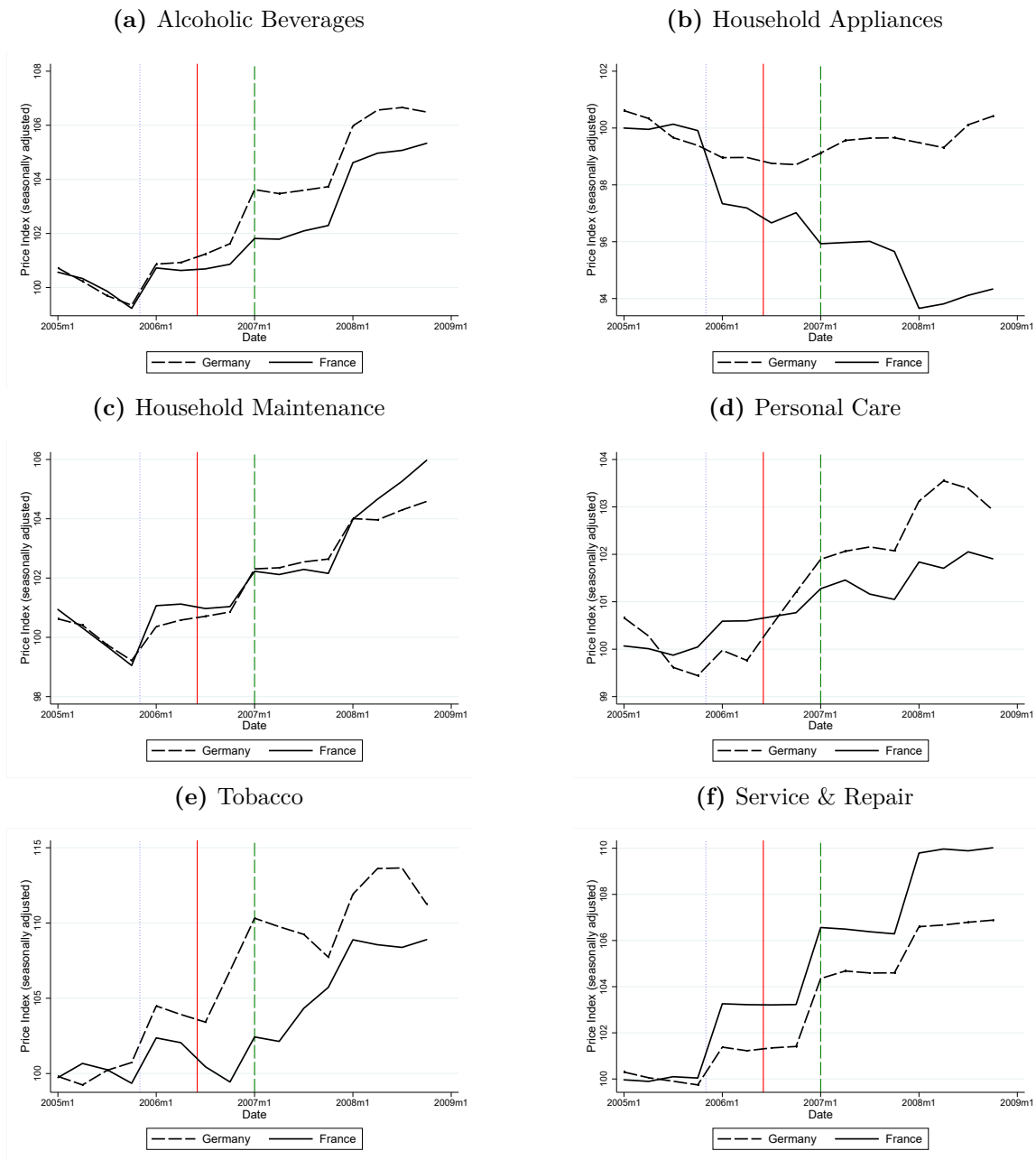
(a) Alcoholic Beverages

(b) Household Appliances

(c) Household Maintenance

(d) Personal Care

(e) Tobacco

(f) Service & Repair

Notes: This figure shows how the seasonally adjusted price index for selected commodity groups developed in Germany and France from 2005 to 2009. The dotted blue vertical line marks the passing of the coalition agreement in November 2005. The solid red vertical line marks the final legal decision on the reform to the standard VAT rate in June 2006. The dashed green vertical line marks the implementation of the respective VAT reform in January 2007.

VAT reform was decided on as Figure 1.4d shows. The same is true for tobacco products.[22]

Thus, also for these commodity groups the VAT increase seems to affect prices before the actual implementation of the reform. However, there are also commodity groups for which this is not true. For instance, Figure 1.4c shows that prices for household maintenance commodities develop very similarly in Germany and France throughout the sample period. The same is true for the commodity groups *Glass-& tableware and household utensils* or *Personal items*.[23]

Finally, Figure 1.4f shows the price development for service and repair commodities. This, for example, includes prices for domestic services by paid staff, repair of household appliances, or hairdressing. In both countries prices increase stepwise with the turn of the year. This suggests that prices for these commodities are sticky, as there might be costs to changing them, for example, printing new signboards in a hair salon. However, the increases are less strong in Germany. This price pattern also holds for the commodity group *Restaurants & hotels*. It is interesting to note that both of these commodity groups are particularly labor-intensive. Thus, the less pronounced price increase in Germany could reflect the decreasing non-wage labor costs due to the decrease in social security contributions, which was another part of the supplemental budget law (see Section 1.2). This will be discussed in more detail in Section 1.5.3. It is important to note, though, that for these two commodity groups, the parallel trends assumption seems to hold less well, as prices in Germany and France already follow different trends before the decision on the VAT reform.

In summary, the average price development in Figure 1.2 only partly provides an accurate depiction of the development for the individual commodity groups.

1.4   Estimation Strategy

I use two different methods to estimate the price effects of the 2007 VAT reform in Germany. First, I implement an event study design to assess the dynamic impact of the VAT reform on the consumer price index. As it provides quarterly estimates of the price effect, the event study design, in contrast to a DiD approach, enables to more precisely locate the timing of any effect. Moreover, the event study design provides an illustration of common pre-trends in commodity prices in Germany and France, which is the key identifying assumption. Second, I estimate the average treatment effect of the VAT reform with a generalized DiD model.

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[22]See Figure 1.4e.

[23]The development for the remaining commodity groups is depicted in Figures 1.B6 and 1.B7 in the Appendix.

1.4.1   Event Study

The following baseline event study model is used to estimate the average quarterly VAT incidence for all commodities:

$$ln(p)_{itc} = \sum_{j=-5}^{10} \beta_j(D^{treat} * I_{t+j}) + \alpha_{ic} + \gamma_t + \epsilon_{itc}. \tag{1.1}$$

The dependent variable is the log of the price index for commodity i in quarter t and country c. D^{treat} is a treatment dummy indicating whether the commodity was affected by the VAT reform or not. I_{t+j} is an indicator for the event window which runs from 5 quarters prior to the decision on the VAT reform to 11 quarters after the decision on the reform.[24] β_j is therefore the coefficient of interest, as it measures the treatment effect, which is the price change for commodities that were exposed to the VAT reform which exceeds the change in the control group in a given quarter.

As reference period relative to which the change in prices is measured, I choose the final legal decision on the VAT reform in June 2006, that is, the second quarter 2006. As explained in Section 1.2, from this point in time there was legal certainty that the reform was going to be implemented in January 2007. Thus, anticipatory price effects seem likely. These would be missed if the reference period is set to the implementation date instead. Figures 1.2 or 1.4a, for example, show that prices did, indeed, already react in June 2006. Moreover, Benedek et al. (2019) find that for increases in the standard VAT rate, anticipation effects are likely. As a robustness test, I alternatively choose November 2005, the date when the coalition agreement was passed, and January 2007, the implementation date of the reform, as base periods. The results can be found in Section 1.6.3.

I include country-specific commodity fixed effects, $\alpha_{i,c}$, to account for time-invariant characteristics by country and commodity that affect the development of the price index. γ_t controls for time fixed effects to capture general quartely price level trends.

1.4.2   Differences-in-Differences

Besides the quarterly event study estimates of the price increase, I estimate the average treatment effect of the VAT reform using the following generalized DiD model:

$$ln(p)_{itc} = \beta_0 + \beta_1 D^{treat} + \beta_2 Post + \beta_3(D^{treat} * Post) + \alpha_{ic} + \gamma_t + \epsilon_{itc}, \tag{1.2}$$

where the dependent variable is again the log of the price index for commodity i in quarter t and country c. D^{treat} is a treatment dummy indicating whether the commodity was affected by the VAT reform or not. *Post* is a binary indicator for the treatment period,

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[24]The event window runs until quarter 10, as the reference month is coded as 0.

which takes on the value 1 for observations after the decision on the VAT reform in June 2006, that is, after the second quarter 2006. β_3 is therefore the coefficient of interest, as it measures the treatment effect. Country-specific commodity fixed effects, α_{ic}, and time fixed effects, γ_t, are again controlled for in all specifications.

1.4.3   Statistical Inference

An important issue in the design of the empirical strategy are assumptions regarding the structure of the error term. The first important assumption regards homoskedasticity. Figure 1.B5 in the Appendix shows that the variance is greater for more extreme values of the dependent variable. Accordingly, using a Breusch-Pagan test, the assumption of homoskedasticity is rejected. I correct for this by using heteroskedasticity-robust standard errors.

The second assumption concerns the correlation of errors within clusters. As shocks might be correlated within a commodity across time, standard ordinary least squares (OLS) would underestimate standard errors (Donald and Lang; 2007; Moulton; 1986). This is corroborated by a test for cross-sectional dependence described in Pesaran (2004) and Pesaran (2015). Thus, clustering standard errors at least at the level of the identifying variation, commodity i, is necessary. To be even more prudent, I follow Angrist and Pischke (2008) in clustering standard errors at a higher level in the baseline specification, namely, the commodity group level. However, this reduces the number of clusters to 38. As few clusters might lead to an underestimation of the correlation, I follow Cameron et al. (2008) and Cameron and Miller (2015) and use the wild cluster bootstrap method to correct for this in my baseline specification.

As robustness tests, I also report the main specification with heteroskedasticity-robust standard errors, clustered standard errors, pairs cluster bootstrapped standard errors, and wild cluster bootstrapped standard errors at the commodity level, instead of the commodity group level (see Table 1.4 in Section 1.6.1). Only when not accounting for intraclass correlation, the null hypothesis of no price effect can be rejected at the 1%-level.

Equations 1.1 and 1.2 are the baseline models to estimate the price effect across all commodities. I also estimate the price effect for specific commodity groups in a slightly adjusted version. Specifically, errors are now wild cluster bootstrapped at the individual commodity level i, rather than at the level of the commodity group.

Finally, Abadie et al. (2017) have recently questioned the common practice to report clustered standard errors. In case of fixed effects regressions they argue that clustering is only necessary when there is heterogeneity in the treatment effect. As this is true here and, moreover, there is also clustering in the assignment because only German commodity prices are treated, clustering standard errors seems necessary even from this perspective.

1.4.4 Identification

The parallel-trends assumption is the necessary condition for causal interpretation in the given setting. It requires that the price indices in Germany and France follow similar trends before and after the VAT reform, if the reform would not have happened. Figures 1.2 and 1.4 provide first graphical evidence that prior to the reform the development in the price indices is very similar, both for all commodities on average and most of the different commodity groups.[25] It therefore seems valid to assume that prices would have behaved similarly in Germany and France absent the policy shock, which makes French prices a suitable control group. Nevertheless, the decision to choose France as control group requires some explanation.

The most obvious control group seem to be commodities subject to the reduced VAT rate in Germany, because that rate did not change in 2007 (see Section 1.2). This group mostly consists of food items, though. That means that their prices were affected by the World food crisis 2006 to 2008.[26] Thus, exactly at the time of the reform to the standard VAT, the commodities subject to the reduced rate were also exposed to a price shock, albeit unrelated to their taxation. Figure 1.B1 in the Appendix shows the development of prices for commodities subject to the reduced VAT rate in Germany. The price shock due to the food crisis is clearly visible. Thus, these items cannot serve as control group to estimate the price effect of the VAT reform. Moreover, Benedek et al. (2019) criticize the use of goods in the same country, not affected by a VAT reform, as control group due to potential general equilibrium effects.

With the disqualification of the reduced VAT items as suitable control group, an alternative are prices in other Euro countries for the same commodities as in Germany but that did not experience a VAT reform during the period 2005 to 2009.[27] A further restriction is the availability of price data at a sufficiently disaggregated level. Although Eurostat makes price indices available for all European countries, the level of detail of the data varies. Thus, the two criteria mean that four countries are left as possible control group: Belgium, France, Lithuania, and Slovenia. Due to the great difference in economic development compared to Germany, the latter two countries do not seem like natural control groups, whereas Belgium and France seem like viable options. As outlined above, French prices do indeed seem to fulfill the parallel-trends assumption. Belgian prices, however, seem to

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[25]The statistical evidence will be provided in Section 1.5.1.

[26]For a number of reasons, global food prices rose substantially during the period 2006 to 2008. For a more complete discussion see, for instance, Headey and Fan (2010).

[27]To avoid complications due to the exchange rate, only countries which use the Euro as currency are considered in the search for potential control groups. This condition precludes, for instance, the United Kingdom.

be more cyclical than German ones, as Figure 1.B2 in the Appendix shows. The price development before the VAT reform is less similar comparing Germany and Belgium than it is comparing Germany and France. Furthermore, the risk of potential price spill-overs is higher for Belgium than for France due to their relative size compared to Germany. Hence, France fulfills the condition for causal interpretation best, which motivates its choice as control group.[28] D'Acunto et al. (2016) likewise conclude that the similarity of pre-shock trends is most pronounced when they only use France as control group. Moreover, Montag et al. (2020) use France as a control group in a differences-in-differences approach to measure the impact of the temporary VAT reduction in Germany in 2020 on fuel prices, arguing that the two countries are very similar in various of dimensions.

Moreover, Figure 1.B3 in the Appendix shows that for a number of macroeconomic indicators, such as inflation, unemployment, or GDP growth, Germany and France show relatively similar trends compared to the other potential control countries. As mentioned above, there was no reform to the VAT rates during the time frame 2005 to 2009 in France. The most recent amendments to the standard VAT rate are a reduction from 20.6% to 19.6% in 2000 and an increase to 20% in 2014. One of the few tax reforms that took place during the sample years in France concerned the taxation of donations and gifts. This was part of a bigger fiscal package by the Fillon administration in 2007, which aimed to liberalize the labor-market, ease the fiscal burden on businesses, and stimulate investment, for instance, by exempting overtime hours from the income and payroll taxes. Another tax reform was a modification to the wealth tax for non-resident French citizens in the summer of 2008. It is unlikely that either of these reforms had a significant impact on commodity prices in France. Besides fiscal reforms, the French administration tried to implement new youth employment laws in 2006 but this legislation had to be scrapped due to ongoing protests. Furthermore, plans to reform the pension benefits system triggered widespread protests in late 2007. Thus, no significant reform was successfully implemented during the sample years in France.

Finally, the German data follows the national definitions of the consumer price index (CPI), whereas the French data follows the definition of the harmonized index of consumer prices (HICP). One might argue that this difference in definition distorts the results. However, as Figure 1.B4 in the Appendix shows, the development of the German price index is very similar whether following the national or the harmonized definition. Thus, any conclusions based on the CPI data should also hold for the HICP data.[29]

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[28]Note that as a robustness test I furthermore control for potential pre-treatment trends in prices. The results remain very similar even with these "detrended" prices. Thus, diverging pre-treatment trends in German and French prices do not seem to drive the results (see Column (1) in Table 1.5 in Section 1.6.2).

[29]I prefer the CPI over the HICP data, as the level of disaggregation is greater for the CPI data.

1.5 Results

1.5.1 Event Study

Figure 1.5: Event Study Estimates for All Commodities
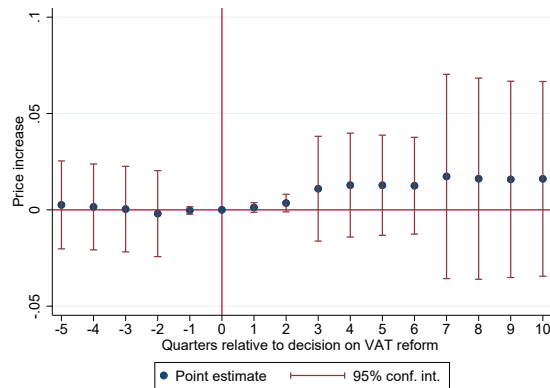
This figure plots quarterly event study estimates and corresponding 95% confidence bands for the baseline event study specification Equation 1.1. The dependent variable is the log of the price index for commodity i in quarter t and country c. Commodity and time fixed effects are included. Standard errors are clustered at the commodity group level. The red vertical line marks the final legal decision on the VAT reform in June 2006. The number of observations can be found in Table 1.2.

In this section, I will first report the results from Equation 1.1. Thus, Figure 1.5 displays quarterly event study estimates of the price increase across all commodities. It corroborates the descriptive findings from Figure 1.2. First, I observe flat pre-trend in prices, which provides statistical evidence that French prices are a suitable control group. Second, the point estimates become modestly positive around three quarter after the decision on the VAT reform. These anticipatory price effects would have been missed if the reference period would have been set back to the implementation of the reform. Therefore, the decision date seems like a suitable choice as reference period.[30] Given that the VAT increases by 3 percentage points from 16% to 19%, the reform would imply an increase in prices by 2.58% if the VAT burden is fully shifted to the consumer.[31] The values of the point estimates in

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[30]The annual pattern in point estimates and confidence intervals is due to sticky prices for commodities in some industries and an increasing variance for observations further away from the reference period. Sticky prices mean that prices for these commodities do not adjust immediately but only at fixed points in time, for example, because it is costly to change prices. For a given time period, the individual prices remain stable but within a commodity group there is still fluctuation. Together with the growing variance for observations further away from the reference period, this leads to the step-like pattern for point estimates and confidence intervals observed in Figure 1.5. Furthermore, it is reinforced by the method used to control for quarterly seasonality.

[31]For the calculation: (119/116 * 100 -100) = 2.58.

Figure 1.5 display a price increase by around 1.5%. This would imply a pass-through of around 60%, which is below the average pass-through of 79% that Benedek et al. (2019) estimate for reforms to the standard VAT in the EU. The lower pass-through could be due to the fact that the VAT reform in Germany affected more than 50% of the consumption basket. This is important because Benedek et al. (2019) also show that for VAT reforms with a broad scope the pass-through to consumers is lower.

However, the effects are not statistically significant at the 5%-level. The relatively broad confidence intervals include under- to over-shifting. Thus, it is difficult to draw any precise conclusions with regards to the incidence. But at least it is valid to conclude that Figure 1.5 lends no straightforward evidence that VAT increases are on average necessarily (fully) passed on to the consumer. In so far, I cannot lend support to the notion that the VAT is incontrovertibly a regressive tax on consumption. The question is now whether this also holds for the individual commodity groups.

Figure 1.6 shows quarterly event study estimates of the price increase for a number of different commodity groups. Figure 1.6a shows that prices for alcoholic beverages in Germany did initially increase statistically significant by around 1.5% after the decision on the VAT reform relative to French prices. The immediate reaction in alcohol prices already before the implementation of the VAT reform is in-line with results by Young and Kwapisz (2001), who find that there is no lag in the response of prices to the excise tax on alcohol in the US.

German prices for household appliances also increase statistically significant by around 2% three quarters after the decision on the VAT reform and continue to increase by as much as 4% in comparison to the base period and relative to French prices as Figure 1.6b shows. This would even imply an over-shifting of the VAT increase to consumers by around 50%. This corroborates results by Buettner and Madzharova (2017), who use a dataset of major domestic appliances. They that the period after the 2007 VAT reform in Germany is characterized by substantially higher prices for these goods. Similarly, D'Acunto et al. (2016) find that the 2007 VAT reform led to an increase in households' willingness to buy durable goods. This could have induced producers to increase prices. Moreover, a further explanation for the price increase might be the fact that most household appliances in Germany are imported and, therefore, could not profit from the lower labor costs due to the decrease in social security contributions, while fully bearing the increased VAT rate. However, it also needs to be stressed that the change is measured relative to prices for household appliances in France, where prices decrease sharply. The large positive point estimates for the price effect for this commodity group therefore are also a reflection of this *relatively* positive development.

A similarly large price increase can be observed for tobacco products for three quarters

Figure 1.6: Event Study Estimates for Selected Commodity Groups
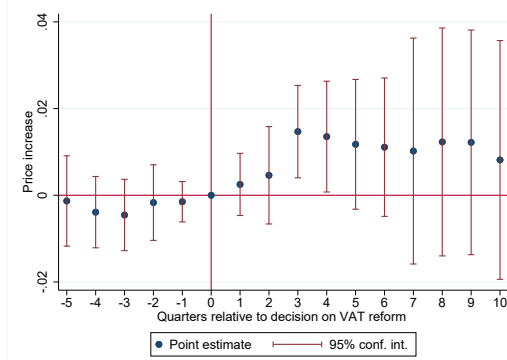
(a) Alcoholic Beverages

(b) Household Appliances

(c) Household Maintenance

(d) Personal Care

(e) Tobacco

(f) Service & Repair

Notes: This figure plots quarterly event study estimates and corresponding 95% confidence bands. The dependent variable is the log of the price index for commodity i in quarter t and country c. Commodity and time fixed effects are included. Standard errors are clustered at the commodity level. The red vertical line marks the final legal decision on the VAT reform in June 2006. The number of observations can be found in Table 1.2.

following the decision on the VAT reform (see Figure 1.6e). As the tobacco industry can be described as monopolistic, the observed over-shifting is possible from a theoretical viewpoint due to imperfect competition (Stern; 1987; Weyl and Fabinger; 2013). However, the effect fades out over time. Meanwhile, the price effect for personal care items remains relatively consistent at around 2% as depicted in Figure 1.6d but it becomes statistically insignificant seven quarters after the reference period. The statistically significant positive price effects for alcohol, personal care items, and tobacco might well be explained by low demand elasticities for these commodity groups. Thus, producers know that they can shift at least part of the tax burden to the consumer, without hurting demand too much.

At the same time, for the commodity group *Household maintenance* no statistically significant price effects can be observed (see Figure 1.6c). This commodity group includes items such as detergents or cleaning agents. As competition might be tight in this sector and costumer loyalty low, producers might be reluctant to pass through any VAT increases in order not to hurt demand.

Finally, Figure 1.6f shows the price effects for service and repair commodities. Following the decision on the VAT reform, the point estimates hover around zero and seven quarters after the reference period even turn negative. An explanation for this unexpected pattern might be the decrease in the social security contributions, which lowers labor costs in Germany. Thus, in the medium-run an indirect effect on commodity prices, especially in labor-intensive sectors, cannot be ruled out. I will discuss this in more detail in Section 1.5.3. However, it also needs to be noted that Figure 1.6f reveals significant pre-trends for this commodity group, suggesting that service and repair prices in Germany and France did not follow parallel trends prior to the VAT reform.[32] Thus, any conclusions based on such a comparison need to be regarded with caution. This similarly applies to the labor-intensive commodity group *Restaurants & hotels*.

Figures 1.B8 and 1.B8 in the Appendix show the event study estimates for the remaining commodity groups. Similar to the average price effect in Figure 1.5, the confidence intervals for many of the individual commodity groups tend to be quite large.

1.5.2   Differences-in-Differences

While the event study estimates enable the detection of any dynamic effects and a visualization of the parallel trends assumption, I estimate the magnitude of the average price effect of the VAT reform using a generalized DiD model. The main regression results from Equation 1.2 are shown in Table 1.2. Column (1) shows the average treatment effect for all commodities and Columns (2) to (7) focus on individual commodity groups.[33] In-line

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[32]This was already visible in Figure 1.4f.

[33]Results for the remaining 13 commodity groups can be found in Tables 1.A2 and 1.A3 in the Appendix.

Table 1.2: DiD Estimates: Main Specification

	(1) All commodities	(2) Alcoholic beverages	(3) Household appliances	(4) Household maintenance	(5) Personal care	(6) Tobacco	(7) Service & repair
Treatment	0.0116 (0.0248)	0.0123 (0.0083)	0.0368** (0.0129)	-0.000583 (0.0078)	0.0111 (0.0128)	0.0392* (0.0152)	-0.0163 (0.0083)
95% CI	[-0.0350, 0.0644]	[-0.0056, 0.0313]	[0.0102, 0.0632]	[-0.0614, 0.0605]	[-0.0183, 0.0481]	[0.0007, 0.0707]	[-0.0331, 0.0006]
Commodity FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	6272	208	416	256	512	96	400
Within R^2	0.0301	0.892	0.271	0.569	0.320	0.834	0.828

Notes: Commodity and quarter fixed effects in all estimations. Wild cluster bootstrapped standard errors are reported (10,000 repetitions) in parentheses.

with the event study estimates, the average treatment effect for all commodities is positive but statistically insignificant. This holds also for the individual commodity groups, except for household appliances (see Column (3)) and tobacco (see Column (6)), for which the positive effects are statistically significant at the 5-% level and 10-% level, respectively. The coefficient implies an average price increase of household appliances by 3.7%, which translates into an average over-shifting of the tax burden by 40% for this commodity group. While the positive sign of the point estimates seems reasonable, the magnitude of the implied effect should be considered with caution due to the fact that the parallel trends assumption for this commodity group does not hold well and that the prices in the control decrease sharply. The effect for tobacco is of similar magnitude and implies an over-shifting by 50%. This is most likely due to the low demand elasticity, which enables producers to shift the tax burden to consumers. A noticeable deviation from the average effect can also be observed for the commodity group *Service & repair* in Column (7). The coefficient is negative but statistically insignificant at the 10%- level. This corroborates the findings in Figure 1.6f.

In summary, whether using an event study or a DiD approach, there is a lack of statistically significant evidence that the burden of the VAT is on average shifted to consumers. It is important, though, to stress that this should not be interpreted as evidence for no price effect. All this means is that the with the given data and approach, I cannot reject the null hypothesis of no price effect. This at least challenges the assumption that the VAT is necessarily carried by consumers. If it is instead carried by firms, it could either hit wages or profits.[34] The former would cast doubt on the hypothesis that the value-added tax causes no distortions on the labor-market and is therefore advantageous in comparison to other taxes, such as the income tax.

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[34]To be more precise, it would affect *pure* profits, that is, profits beyond those provided by the normal return to capital. It would be similar to a cash-flow tax. As investments are fully deductible, they would not be distorted.

Previous literature for VAT *decreases* has found that prices do not fully adjust and that firm owners are able to increase their profits (Kosonen; 2015; Benzarti and Carloni; 2019). For VAT *increases*, this, in turn, would imply that consumers do not necessarily need to carry the entire VAT burden as it is shared with producers, which would be in-line with my results for the 2007 VAT reform in Germany. But this would assume that there is a symmetric reaction to VAT decreases and increases. However, Benzarti et al. (2020) find that this is not the case, as prices seem to react more to increases than to decreases in the VAT rate. It is therefore problematic to base predictions on the effect for producers on the previous literature for VAT decreases. Instead, it would be necessary to extend that research also to VAT increases in order to allow for more precise statements regarding the actual effects on wages and profits and, hence, potentially the labor-market.

Moreover, both the event study and the DiD estimates reveal that the price effect varies between commodity groups. Carbonnier (2007) similarly finds that the consumer share of the VAT burden differs between sectors in France. Simply relying on the average incidence would therefore be misleading and could mask potential distributional consequences. Using representative household data on spending in Germany from the income and consumption survey (*Einkommens- und Verbrauchsstichprobe* EVS) for the year 2003, Figures 1.B10, 1.B11, 1.B12, and 1.B13 in the Appendix show that spending on the 19 previously defined commodity groups does differ between net household income quartiles.[35] For instance, one can observe that the share of spending on tobacco products is almost 5% for the lowest net household income quartile, while it is only 1.3% for the highest income quartile.[36] As I show that there is an over-shifting of the tax burden for tobacco (see Column (6) in Table 1.2), this implies that lower income groups are relatively more affected by this price increase.[37] This demonstrates how differences in tax incidence and consumption patterns can entail distributional consequences to VAT reforms. Policy makers should consider this when reforming the VAT rate.

Beyond that, the event study estimates show that prices already start to react at the

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[35]The EVS is provided by the Statistical Offices of the German States. It is conducted every five years. The year of interest for the analysis is 2003, that is, the closest year available before the VAT reform. The EVS contains detailed information on income, wealth, debt, and consumption expenditures of private households in Germany. It is send out to 60,000 households from all social backgrounds, so that the survey provides a representative sample. However, the participation in the survey is not mandatory.

[36]Spending on the 19 previously defined commodity groups equals 34% of average net household income for the lowest income quartile and 28% for the highest income quartile. For the lowest quartile, quarterly average net household income is €4045.98 and for the highest quartile quarterly average, quarterly average net household income is €18,880.3. In absolute numbers this means that the lowest and the highest net household income quartile spend around €69 on tobacco products in a given quarter.

[37]Note that there is a related literature looking at the regressivity of so-called sin taxes on commodities such as alcohol, tobacco, or sugar (Dubois et al.; 2020; Griffith et al.; 2017).

time of the final legal decision on the reform, six months prior to its actual implementation. This corroborates previous research on anticipatory price effects of VAT reforms.

1.5.3   Comparison to the 1998 Value-Added Tax Reform

Both the event study and the DiD estimates presented above revealed only a modest and statistically insignificant average price effect for the 2007 VAT reform. As explained in Section 1.2, the increase in the VAT from 16% to 19% in 2007 was accompanied by a decrease in the unemployment insurance contributions by 2.3 percentage points. These are part of non-wage labor costs and the reduction therefore could in the medium-run have translated into lower prices, especially in labor-intensive sectors. Thus, the modest price reaction in 2007 might in part be explained by the joint implementation of a VAT increase and a decrease in social security contributions, a so-called *fiscal devaluation*.

Figure 1.7: Development of the Price Index for All Commodities - 1998
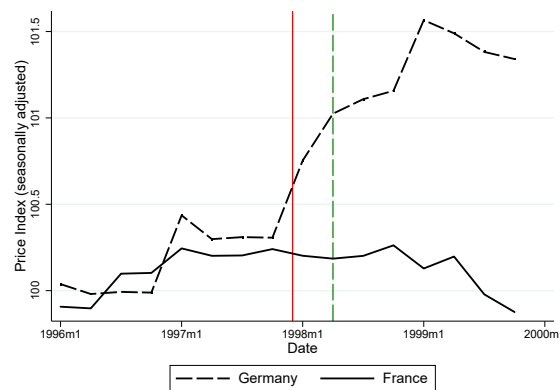
Notes: This figure shows how the seasonally adjusted price index for commodities subject to the standard VAT rate developed in Germany and France from 1996 to 2000 on average. The solid red vertical line marks the final legal decision on the VAT reform in December 1997. The dashed green vertical line marks the implementation of the VAT reform in April 1998.

While it can be argued that particularly any anticipatory price effects are probably due to the VAT reform, because the effect on prices through the labor-market are more likely to show in the medium-run, it is difficult to precisely disentangle the effect of the two reforms. Therefore, I compare the effects of the 2007 VAT reform to those of a VAT reform in Germany which took place in 1998. During that reform the standard VAT rate was increased by 1 percentage point from 15% to 16%, which is a third of the increase in 2007. Importantly, in contrast to the 2007 VAT reform, the 1998 VAT reform was not accompanied by a change in social security contributions and may therefore provide a benchmark of the "pure" VAT price effect. The 1998 VAT reform was decided on by the

German Federal Council in December 1997 and implemented in April 1998. Analogously to the approach described in Section 1.4, I therefore choose December 1997, that is, the last quarter of that year, as reference period relative to which price changes are measured. I construct a sample from 1996 to 2000 again comparing German and French prices.[38]

Figure 1.7 compares the development of German and French prices for the period 1996 to 2000 across all commodities. While the two indices follow very similar trends from 1996 to the end of 1997, verifying that French prices represent a suitable control group also during this time frame, there is a noticeable jump in German prices right at the time the VAT reform was decided on and a further increase after its implementation in April 1998. In comparison to the development from 2005 to 2009 shown in Figure 1.2, the price effect does seem more pronounced in 1998, although the increase in the VAT rate in 1998 is only a third of the increase in 2007. This provides first graphical evidence that the price effects in 2007 might indeed be partly absorbed by the simultaneous reduction in social security contributions.

Figure 1.8: Development of the Price Index for Selected Commodity Groups - 1998
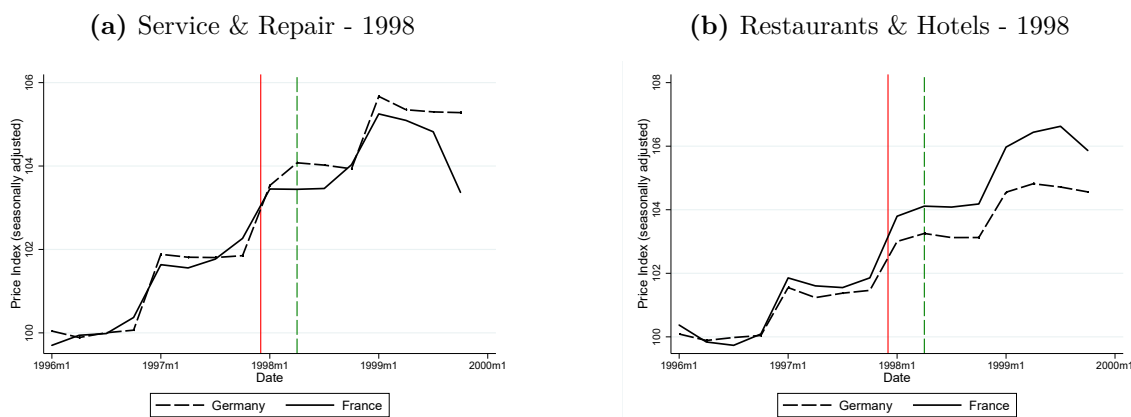
(a) Service & Repair - 1998   **(b)** Restaurants & Hotels - 1998

Notes: This figure shows how the seasonally adjusted price index for commodities subject to the standard VAT rate developed in Germany and France from 1996 to 2000 on average. The solid red vertical line marks the final legal decision on the VAT reform in December 1997. The dashed green vertical line marks the implementation of the VAT reform in April 1998.

As the reduction in social security contributions is most likely to translate into price effects in labor-intensive sectors, Figure 1.8 shows the development of the price index for the two commodity groups *Service & repair* and *Restaurants & hotels*, which are particularly labor-intensive and which had peculiar point estimates for the 2007 VAT reform. In comparison to Figures 1.4f and 1.B7d, it first of all needs to be noted that the assumption of flat pre-trends seems to be fulfilled much better for the 1998 VAT reform for both of these

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[38]Note that there was no VAT reform in France during that time period.

commodity groups. Moreover, for service and repair commodities the reform in 1998 seems to have no price effect, as the development continues to be very similar to that of French prices even after the VAT reform was decided on. Both follow a similarly large stepwise increase over the sample period. In contrast, from 2005 to 2009, French and German prices in that sector seem to follow different trends. Given that this is a labor-intensive sector, this development might have been an implication of a number of reforms which sought to make the German labor-market more competitive, most notably the 2004 Hartz reforms. The reduction in unemployment insurance contributions can also be counted towards these measures.

However, contrary to expectation, the increase in prices for restaurants and hotels in 1998 is less pronounced for German than for French prices. This is likely due to the 1998 World Cup, which took place in France and is found to impact prices for tourist hotels and restaurants in that year (Dauncey and Hare; 2014).

Analogously to the estimation strategies explained in Sections 1.4.1 and 1.4.2, I will now present event study and DiD estimates for the 1998 VAT reform for all commodities and the two commodity groups *Service & repair* and *Restaurants & hotels.*

Figure 1.9: Event Study Estimates for All Commodities - 1998
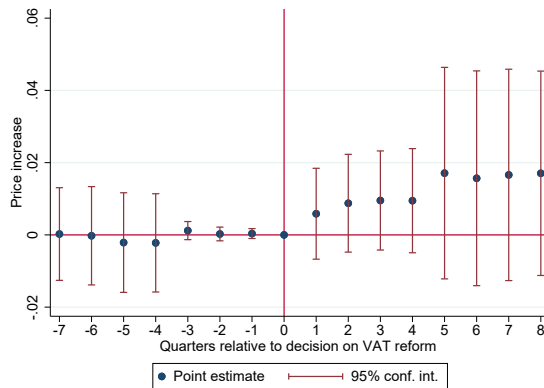
Notes: This figure plots quarterly event study estimates and corresponding 95% confidence bands for the baseline event study specification Equation 1.1. The dependent variable is the log of the price index for commodity i in quarter t and country c. Commodity and time fixed effects are included. Standard errors are clustered at the commodity group level. The red vertical line marks the final legal decision on the VAT reform in December 1997. The number of observations can be found in Table 1.3.

Figure 1.9 again serves to illustrate the parallel trends assumption and to show the dynamic impact of the 1998 VAT reform on prices for all commodities. There is an increase of around 1% to 2% following the decision on the reform. This is surprisingly large given that the VAT rate only increased by 1 percentage point from 15% to 16%, which would imply a price increase by 0.87% if the VAT increase is fully shifted to the consumer. These

findings are corroborated when estimating the average treatment effect. In Column (1) of Table 1.3 DiD estimates for all commodities are presented. The coefficient is positive and would imply an average price increase by 1.28%. Thus, the magnitude of the price effect for the 1998 VAT reform is substantially higher than for the 2007 VAT reform if one considers that the VAT increase in 1998 was only a third of the increase in 2007. This could be an implication of the simultaneous decrease in social security contributions in 2007. However, even in 1998 the price effect remains statistically insignificant in the preferred specification.

Table 1.3: DiD Estimates: Main Specification - 1998

	(1) **All commodities**	(2) **Service & repair**	(3) **Restaurants & hotels**
Treatment	0.0128	0.00485	-0.0105
	(0.0147)	(0.0071)	(0.0060)
95% CI	[-0.0147, 0.0438]	[-0.0095, 0.0190]	[-0.0247, 0.0055]
Commodity FE	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes
N	5456	336	368
Within R^2	0.0343	0.758	0.730

Notes: Commodity and quarter fixed effects in all estimations. Wild cluster bootstrapped standard errors are reported (10,000 repetitions) in parentheses.

Figures 1.10a and 1.10b show quarterly event study estimates for the two labor-intensive commodity groups *Service & repair* and *Restaurants & hotels*. First, it can indeed be confirmed that the parallel trends assumption holds for both groups this time. Moreover, for service and repair commodities the quarterly price effects are positive in 1998. This is also true for the average treatment effect for this commodity group shown in Column (2) of Table 1.3. This is in contrast to the negative effects found in 2007 for this commodity group (see Figure 1.4f). The difference in effects could be an impact of the reduction in social security contributions in 2007, which made labor in Germany cheaper right at the time when the VAT was increased. However, the effect is also statistically insignificant in 1998.

As expected, the price effects for restaurants and hotels are negative, which is likely to be due the 1998 World Cup in France. But they are also statistically insignificant both for the dynamic effects (see Figure 1.10b) and the average treatment effect (see Column (3) of Table 1.3), while they are statistically significant and negative for the 2007 VAT reform.

In conclusion, a comparison between the 2007 and the 1998 VAT reform in Germany does indicate that the effects in 2007 were muted in magnitude by the simultaneous reduction in social security contributions. This would imply that beyond affecting trade, fiscal devaluations also feedback on prices. However, for both reforms the price effects are statistically insignificant. For neither reform there is therefore statistically significant

Figure 1.10: Event Study Estimates for Selected Commodity Groups - 1998
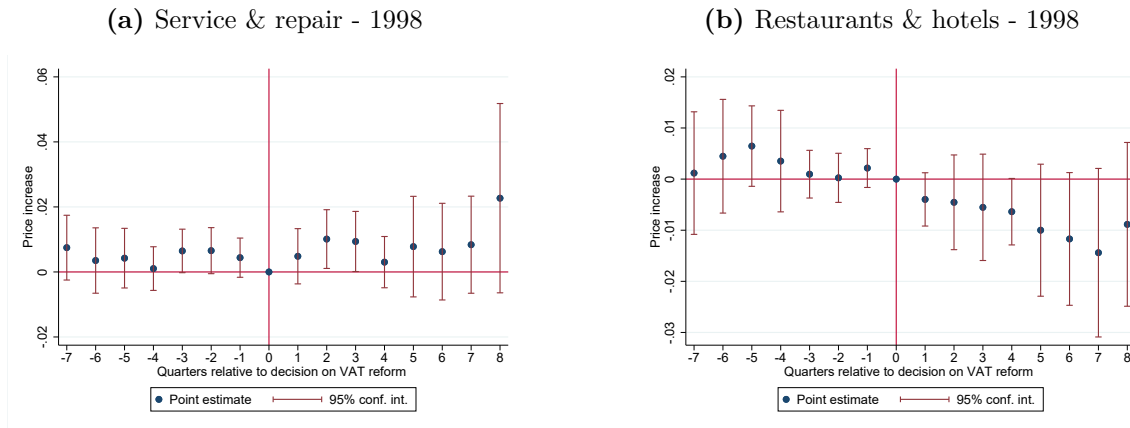
(a) Service & repair - 1998

(b) Restaurants & hotels - 1998

Notes: This figure plots quarterly event study estimates and corresponding 95% confidence bands for the baseline event study specification Equation 1.1. The dependent variable is the log of the price index for commodity i in quarter t and country c. Commodity and time fixed effects are included. Standard errors are clustered at the commodity level. The red vertical line marks the final legal decision on the VAT reform in December 1997. The number of observations can be found in Table 1.3.

evidence that the burden of the VAT increase is shifted to consumers.

1.6   Robustness Tests

I carry out a number of tests in order to verify the robustness of my results. In Section 1.6.1, I change the assumptions regarding the error structure, I control for pre-trends and winsorize the data in Section 1.6.2, and in Section 1.6.3, I change the reference period relative to which price changes are measured in the event study approach.

Table 1.4: DiD Estimates: Robustness - Statistical Inference

	(1)	(2)	(3)	(4)	(5)
	All commodities	**All commodities**	**All commodities**	**All commodities**	**All commodities**
Treatment	0.0116***	0.0116***	0.0116	0.0116	0.0116
	(0.0028)	(0.0033)	(0.0239)	(0.0238)	(0.0102)
95% CI	[0.0061, 0.0170]	[0.0051, 0.0180]	[-0.0369, 0.0600]	[-0.0318, 0.0607]	[-0.0078, 0.0311]
Error term	No controls	Heteroskedasticity robust	Heteroskedasticity robust & cluster	Heteroskedasticity robust & pairs cluster bootstrap	Heteroskedasticity robust & wild cluster bootstrap at commodity level
Commodity FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
N	6272	6272	6272	6272	6272
Within R^2	0.0301	0.6130	0.0301	0.0301	0.0301

Notes: Commodity and quarter fixed effects in all estimations. Wild cluster bootstrapped standard errors are reported (10,000 repetitions) in parentheses in Columns (5) and (6). Within R^2 is reported for Columns (1), (3), (4), (5), and (6). Normal R^2 is reported for Column (2).

1.6.1 Statistical Inference

In Table 1.4, I rerun the baseline DiD equation (see Equation 1.2) with a number of different assumptions regarding the error structure. I continue to control for commodity and time fixed effects in all specifications. In Column (1), I neither control for heteroskedasticity nor correlation of the error terms, while I do control for the former in Column (2). Only for these two specifications the treatment effect is highly statistically significant. This suggests that neglecting the presence of correlation within a commodity group leads to an underestimation of the probability to reject the null hypothesis of no treatment effect. In the remaining specifications intraclass correlation is controlled for but the methods differ. Precisely, in Column (3) errors are clustered at the commodity group level but I do not bootstrap. In Column (4), pairs cluster bootstrapped standard errors at the commodity group are reported. The results are very similar to the main specification in Column (1) of Table 1.2 with wild cluster bootstrapped standard errors, which according to Cameron et al. (2008) and Cameron and Miller (2015) is the preferred method with few clusters. Finally, Column (5) reports wild cluster bootstrapped standard errors at the individual commodity level. This leads to a smaller standard error and narrows the confidence interval in comparison to the main specification. This is likely due to the fact that the number of clusters is now higher. However, it is also the less cautious approach as Angrist and Pischke (2008) recommend to cluster at a higher level, when in doubt.

Table 1.5: DiD Estimates: Robustness - Detrending and Winsorizing Data

	(1) All commodities Detrended	(2) All commodities Winsor 1%	(3) All commodities Winsor 2%
Treatment	0.0119	0.0116	0.00515
	(0.0255)	(0.0248)	(0.0145)
95% CI	[-0.0362, 0.0666]	[-0.0351, 0.0642]	[-0.0253, 0.0360]
Commodity FE	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes
N	6272	6272	6272
Within R^2	0.0239	0.0301	0.148

Notes: Commodity and quarter fixed effects in all estimations. Wild cluster boot-strapped standard errors are reported (10,000 repetitions) in parentheses.

1.6.2 Detrending and Winsorizing Data

The price data is cleaned of a linear pre-treatment trend in Column (1) of Table 1.5. This is done to ensure that results are not biased by different trends in the pre-treatment period for German and French prices. The results remain very similar to the main specification,

suggesting that different pre-treatment trends between German and French prices are not a major issue on average.[39]

Moreover, to verify that the results are not driven by outliers in the data, I report winsorized results at the 1%- and 2%-level in Columns (2) and (3) of Table 1.5, respectively. Again, the results are very robust, especially when winsorizing at the 1%-level.

Figure 1.11: Event Study Estimates for All Commodities - Robustness
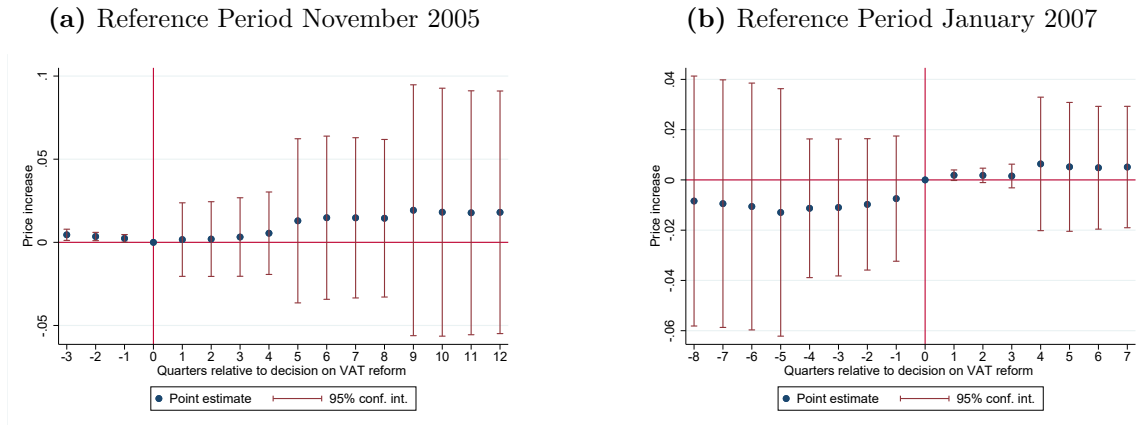
(a) Reference Period November 2005   **(b)** Reference Period January 2007

Notes: This figure plots quarterly event study estimates and corresponding 95% confidence bands for the baseline event study specification Equation 1.1. The dependent variable is the log of the price index for commodity i in quarter t and country c. Commodity and time fixed effects are included. Standard errors are clustered at the commodity group level. In the left panel the red vertical line marks the implementation of the VAT reform in January 2007. In the right panel the red vertical line marks the implementation of the VAT reform in January 2007.

1.6.3   Event Study: Reference Period

As robustness test to the event study approach, I choose two alternative base periods relative to which the price changes are measured. On the one hand, one could argue that there might already be anticipatory price effects when the coalition agreement, which contained the decision on the VAT reform, was passed in November 2005. Figure 1.11a therefore plots the quarterly event study estimates for this case. The point estimates remain close to zero for three quarters following the reference period now. This suggests that there were no anticipatory price effects immediately following the passing of the coalition agreement but that price reactions only set in after another few quarters. Thus, one would miss no relevant anticipatory price effects by setting the reference period back two quarters, which is exactly the time of the final legal decision on the reform. This is the approach

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[39]Note that I specifically control for the linear *pre*-treatment trend only and not the linear trend in the entire sample period, as that would potentially absorb part of the treatment effect. This is particularly problematic in case of dynamic treatment effects (Wolfers; 2006).

followed in the baseline specification (see Figure 1.5).

On the other hand, if one presumes that there are no anticipatory price effects, the reference period should be set to the implementation date of the VAT reform. Therefore, Figure 1.11b alternatively plots the event study estimates for the situation where January 2007, the implementation date, is chosen as reference period. As expected, the treatment effects are now smaller, as one ignores the anticipatory price effects. This suggests that June 2006 is the preferable reference period relative to which price effects are measured. Thus, the baseline specification seems to follow the most appropriate approach.

1.7   Conclusion

The value-added tax is one of the most important tax revenue sources in many countries. It is efficient in terms of revenue collection and is assumed to cause only relatively low distortions on the labor-market. However, it is also often referred to as regressive tax, as it supposedly ultimately hits consumption, and lower income groups spend a greater share of their income on consumption. It is also for this reason that a number of governments have recently implemented VAT cuts in order to revive consumption during the covid-19 pandemic. These attributions and measures all contain inexplicit assumptions about the tax incidence of the VAT, namely that it is necessarily passed on to consumers. However, previous literature has shown that tax incidence often does not follow straightforward assumptions. It is therefore essential to provide empirical evidence of who actually carries the burden of the VAT, also for modest and broad reforms, which have so far received little attention in the literature.

This is where this paper aims to make a contribution by exploiting an exogenous VAT reform in Germany in 2007. Using detailed price data, I implement an event study and DiD approach to investigate the incidence of the VAT for a wide range of commodity groups.

I find that on average the 2007 VAT reform in Germany had a modestly positive but statistically insignificant effect on prices. I can therefore not reject the hypothesis that on average the VAT increase has not been passed through to consumers. However, there are differences between individual commodity groups, ranging from negative price effects to an over-shifting of the tax burden. Simply relying on the average incidence would therefore be misleading. Policy makers should consider this when reforming the VAT rate. Particularly as the consumption of certain commodities seems to differ between income groups in the population, VAT reforms could have unintended distributional effects. Moreover, I observe anticipatory price effects well in advance of the actual implementation of the reform.

A possible explanation for the low average effect in 2007 could be a simultaneous reduction in social security contributions in Germany. Employers could have used this reduction

in non-wage labor costs to lower prices, particularly in labor-intensive sectors. I explore this possibility by comparing the price effects in 2007 with those a VAT reform in 1998 in Germany, which was not accompanied by a reduction in social security contributions. I indeed find that the magnitude of price effects is noticeably higher in 1998, suggesting that the reduction in social security contributions in 2007 absorbed some effect of the VAT increase. This would imply that fiscal devaluations also feedback on prices.

However, for both VAT reforms the price increase is on average statistically insignificant. Therefore, I cannot reject the null hypothesis of no price effect. While one needs to be careful not to interpret this as proof for no price effect, this result at least provides no clear evidence that modest increases are necessarily passed through to consumers and thereby casts doubt on the hypothesis that the VAT is incontrovertibly a regressive tax on consumption. In turn, if the VAT should indeed not be shifted to consumers, it would be carried by producers, ultimately hitting either wages or profits. If the former applies, the VAT would after all cause distortions on the labor-market. This would have important policy implications.

Particularly as the given setting and empirical approach do not allow to draw more precise conclusions on the incidence of the value-added tax for broad and modest reforms, there is scope for future research.

Finally, it is difficult to directly compare the 2007 VAT reform and its effects to the recently implemented VAT reforms in the wake of the covid-19 pandemic for a number of reasons. First, the 2007 VAT reform implied an increase to the standard VAT rate, while the recent reforms are VAT reductions. This is an important difference as Benzarti et al. (2020) show that the price reaction to increases and decreases in the VAT is not symmetric. Second, some of the recent VAT reforms target very particular sectors, for example hospitality and hotel accommodation in the UK, while the 2007 reform in Germany had a broad scope. Benedek et al. (2019) find that the price effects are smaller for broader reforms. Third, the 2007 VAT reform was announced as permanent increase, while most of the recent VAT reforms are announced as temporary. One could argue that sticky prices and menu costs will mean that prices are less likely to adjust to temporary modifications. Moreover, firms face additional bureaucratic costs when the VAT is reformed and the scope to decrease prices in the short-run might therefore be limited.[40] A final important

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[40]At the same time, one could argue that the primary policy aim of temporary consumer tax reductions is an increase in consumer spending, irrespective of the actual price effects. For instance, the UK government introduced a temporary VAT reduction in 2008 as fiscal stimulus during the recession following the financial crisis. Crossley et al. (2014) show that the volume of retail sales did indeed increase as consumers brought forward their purchases. However, firms only initially passed-through the reduction in the VAT by lowering prices. After a few months the price cuts were partly reversed. Thus, the increase in sales was due to intertemporal substitution rather than an income effect. Similarly, Agarwal et al. (2017) show that for

difference between the current situation and the one in 2007 is that we are facing an unprecedented recession in many countries. This means that firms compete more fiercely in many markets, that consumers are likely to be more price sensitive, and that the level of uncertainty is higher.[41]   Altogether this implies that making predictions for the price reactions in 2020 based on those for the 2007 VAT reform in Germany is problematic.

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temporary sales tax holidays in the US, spending on the covered goods increases substantially. For the very short-run, Montag et al. (2020) show that the pass-through due to the temporary VAT reduction in Germany in 2020 on retail fuel prices was fast, substantial, but incomplete, with pass-through rates depending on the competitiveness of the relevant market.

[41]Balleer et al. (2020), for instance, look at how supply and demand forces affect planned price adjustments in the early stage of the covid-19 pandemic in Germany (at the time the temporary VAT reductions were not yet in place). They find that demand deficiencies dominate, which could be due to an expected income risk and higher economic uncertainty. This shows that under the current circumstances there are many factors at play that affect prices.

1.A   Appendix: Additional Tables

Table 1.A1: Definition of Commodity Groups

Commodity group	Commodities
Alcoholic beverages	Brandy, cognac or similar, grain or double grain, liqueur, whiskey, rum, vodka or similar, white wine, red wine or rose wine, sparkling wine, prosecco, champagne, beer, non-alcoholic beer, mixed beer drink, wine spritzer or similar
Audiovisual equipment	MP3 players or the like, loudspeakers or headphones, TV, DVD recorder, DVD player or Blu-ray player, home cinema system, satellite system, digital camera, digital camcorder, zoom lens, binoculars, desktop PC, portable computer, monitor, printer, scanner, joystick, gamepad or similar for PC, organizer or mobile navigation device, calculator or desktop calculator, operating system or other PC application software, unrecorded data carriers, pre-recorded data carriers or music downloads, photo album
Clothing	Fabrics for outerwear, men's suit, men's leather jacket, blazer or jacket for men, men's pants, men's coat, pullover or cardigan for men, sports or work clothing for men, men's shirt, men's T-shirt, men's pajamas, men's underwear, stockings or socks for men, costume, dress or pants suit for women, women's skirt, women's trousers, women's blouse, women's jacket, women's coat, sweater, cardigan or twin set for women, sports and workwear for women, bra, women's shirt, nightgown or pajamas for women, women's underwear, women's tights, children's jacket, children's pants, costume, dress, skirt or pants suit for girls, sportswear for children, shirt or blouse for children, children's shirt, pajamas or nightgown for children, children's underwear, stockings, socks or tights for children, romper suit or two-piece suit for infants, cap or hat, cycling helmet, gloves, tie, scarf or other clothing accessories, knitting wool or other ere haberdashery
Footwear	Classic shoes or casual shoes for men, men's slippers, men's sports shoes, pumps or casual shoes for women, women's slippers, women's sports shoes, children's shoes, toddler shoes, children's slippers, shoelaces or insoles
Furniture, carpets, and home-textiles	Chair or corner bench, cupboard element for fitted kitchen, kitchenette or fitted kitchen, wardrobe, bed, slatted frame or spring frame, mattress, sofa bed, upholstered furniture, living room table or dining table, living room cabinet, desk, computer table or desk chair, bathroom furniture, wardrobe furniture, garden furniture or camping furniture, shelf, wall lamp or ceiling lamp, table lamp or floor lamp, Berber carpet, oriental carpet or the like, carpeting, carpet tile or the like, laminate, finished parquet, linoleum or the like, woolen blanket, duvet, bedspread or the like, duvet cover set or bed sheet, curtain, interior blind or similar, bathroom carpet or bathroom furniture, towel, tablecloth, table runner or similar, garden umbrella
Glass- & tableware and household utensils	Drinking glasses, tableware made of porcelain, baking dish, cutlery, kitchen knife or the like, kitchen scales, mixing spoons, pounders or the like, frying pan, casserole or saucepan, tableware made of metal, plastic or wood, laundry basket or folding box made of plastic, storage container made of plastic, ironing board, baby bottle or the like
Household appliances	Refrigerator, fridge-freezer combination, freezer or freezer, washing machine, dryer, dishwasher, stove, oven or microwave, fireplace, extractor hood, fan or similar, vacuum cleaner, sewing machine, toaster, waffle iron or similar, coffee machine or tea maker, kettle, egg cooker or the like, fully automatic coffee machine, pod machine or the like, electric mixer or blender, iron

Table 1.A1: Definition of Commodity Groups

Commodity group	Commodities
Household maintenance	Heavy duty detergent, mild detergent or special detergent, fabric softener, starch or similar, dishwashing detergent, sanitary cleaner, shoe polish or other shoe care product, metal care product or other care product, all-purpose cleaner or other cleaning agent, aluminum foil, transparent film or the like, filter paper, paper cups or the like, nails, screws or the like, brushes, brooms or other cleaning articles, candles, glue, matches or the like
Non-alcoholic beverages	Cola drink, caffeine-free lemonade, apple juice or similar fruit juice, orange juice or similar fruit juice, multivitamin juice, diet fruit juice, vegetable juice
Personal care	Hair dryer or other hair care device, electric, razor, electric toothbrush, hairbrush, comb or hair clip, bathroom scales, toothbrush, non-electric, wet razor, razor blades or the like, Eau de toilette or perfume, hair shampoo, Hairspray, hair gel or the like, hair color or tint, hand cream, day cream or night cream, children's cream, toothpaste, mouthwash, dental floss or the like, aftershave, shaving cream or the like, lipstick or lip balm, nail polish, make-up, kohl pencil or mascara, fine soap, shower gel, shower bath or bath additive, deodorant spray or deodorant roller, toilet paper, tissue, diapers for babies or toddlers, tampons, facial tissues or other hygiene products
Personal items	Wristwatch or pocket watch, wall clock, alarm clock, stopwatch or the like, battery change for a wristwatch, women's handbag, briefcase, satchel or backpack, suitcase, travel bag or the like, purse, ID bag or the like, disposable lighter, stroller, child car seat, umbrella, sunglasses, weather station
Recreational items	Motorhome, caravan, musical instruments, including accessories, board game, game console, game for game console, electric model train or accessories, construction kit, experiment kit or model kit, tricycle, scooter or other children's sports vehicle, doll, teddy bear or other soft toy, toy car, toy shop or other toy, decorative items for parties, soccer or other sports balls, skis, snowboards or other winter sports items, tennis rackets, table tennis rackets or the like, fitness equipment, inline skates, ice skates or roller skates, football shoes or other special sports shoes, paddling pool, diving goggles or the like, sleeping bag, tent or other camping items, flower pot or planter, flower fertilizer, potting soil, bark mulch, peat or the like
Recreational activities	Visit to an amusement park, services from photo laboratories or similar, ride with cable car or ski lift, fishing permit or similar, fee for gym
Restaurants & hotels	Consumption of meat dishes, consumption of fish dishes, consumption of pasta, pizza, omelets or similar, consumption of soups or stews, consumption of ice cream or other dessert, consumption of other dishes, food for consumption on public transport, consumption of coffee, tea or the like, consumption of fruit juice or vegetable juice, consumption of mineral water, consumption of lemonade or the like, consumption of spirits, consumption of beer, consumption of wine or sparkling wine, drink for consumption on public transport, consumption of food in canteen or cafeteria, overnight stay, overnight stay in youth hostels, rent for holiday apartment or holiday home, campsite fee
Service & repair	Repair of consumer electronics, laying and fixing floor coverings, sanding and sealing parquet flooring, repair on large household appliances, domestic help services, cosmetic repairs and other repairs, hairdresser for men, hairdresser for children, hairdresser for women, fee for tanning salon, cosmetic treatment or the like, painting of a fender, car inspection, car repair, car wash, bicycle repair
Stationary	Calendar, postcard or greeting card, pen, fountain pen or similar, file folder, stamp or other office supplies, envelopes, letter pad or stationery, exercise book, drawing pad, printer paper, pencil, colored pencil, ink box or similar, printer cartridge
Tobacco	Cigarettes, cigars and cigarillos, tobacco

Table 1.A1: Definition of Commodity Groups

Commodity group	Commodities
Tools & equipment	Motor lawn mower, hammer drill, cordless screwdriver or drill, garden tools, hammer, screwdriver or similar, paintbrush or paint roller, locks, keys or fittings, halogen lamp, energy-saving lamp or similar, socket, plug, cable or similar, alarm detector or motion detector, batteries, wallpaper, paints or varnishes, wallpaper paste, thinner or the like, building materials, doors, windows and the like
Vehicles purchase	New cars, vans, used cars, motorcycles, bicycles, car tires, car battery or spark plugs, accessories or spare parts for motor vehicles, car trailers, car wax, paint care products or the like, tires or inner tubes for bicycles, accessories or spare parts for bicycles

Table 1.A2: DiD Estimates for Selected Commodity Groups: Main Specification I

	(1) Audiovisual equipment	(2) Clothing	(3) Footwear	(4) Furniture, carpets & hometextiles	(5) Glass-&tableware, household utensils	(6) Nonalcoholic beverages
Treatment	0.0581	0.00151	0.00256	-0.00149	-0.00701	0.0322
	(0.0058)	(0.0076)	(0.0087)	(0.007)	(0.0246)	(0.0064)
95% CI	[-0.0772, 0.1938]	[-0.0197, 0.0127]	[-0.0203, 0.0227]	[-0.0196, 0.0169]	[-0.0291, 0.0134]	[-0.0479, 0.0996]
Commodity FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
N	544	720	160	560	224	176
Within R^2	0.459	0.274	0.543	0.256	0.647	0.636

Notes: Commodity and quarter fixed effects in all estimations. Wild cluster bootstrapped standard errors are reported (10,000 repetitions) in parentheses.

Table 1.A3: DiD Estimates for Selected Commodity Groups: Main Specification II

	(1) Recreational items	(2) Personal items	(3) Recreational activities	(4) Restaurants & hotels	(5) Stationary	(6) Tools & equipment	(7) Vehicle purchase
Treatment	0.0008	-0.000270	-0.00193	-0.0198*	-0.00527	0.00457	0.0137
	(0.0614)	(0.0237)	(0.0282)	(0.0089)	(0.0078)	(0.0162)	(0.0186)
95% CI	[-0.0191, 0.0135]	[-0.0512, 0.0703]	[-0.3679, 0.4023]	[-0.0383, 0.0015]	[-0.0562, 0.0460]	[-0.0351, 0.0470]	[-0.0319, 0.0537]
Commodity FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	288	448	96	384	176	288	320
Within R^2	0.722	0.0401	0.436	0.891	0.735	0.589	0.624

Notes: Commodity and quarter fixed effects in all estimations. Wild cluster bootstrapped standard errors are reported (10,000 repetitions) in parentheses.

1.B   Appendix: Additional Figures

Figure 1.B1: Development of the Price Index for Commodities Subject to the Reduced VAT
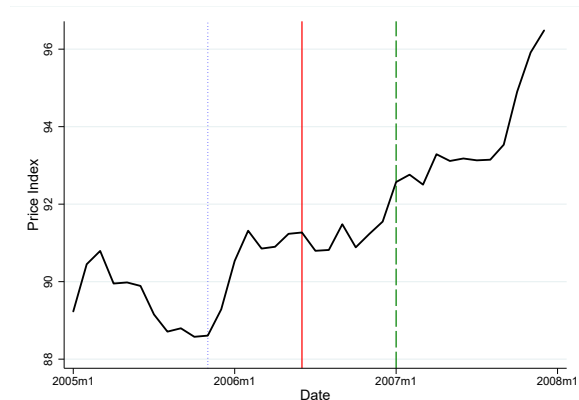
Notes: This figure shows the development of the price index for commodities subject to the reduced VAT in Germany from 2005 to 2009. The dotted blue vertical line marks the passing of the coalition agreement in November 2005. The solid red vertical line marks the final legal decision on the reform to the standard VAT rate in June 2006. The dashed green vertical line marks the implementation of the respective VAT reform in January 2007.

Figure 1.B2: Development of the Price Index for All Commodities: German and Belgian Prices
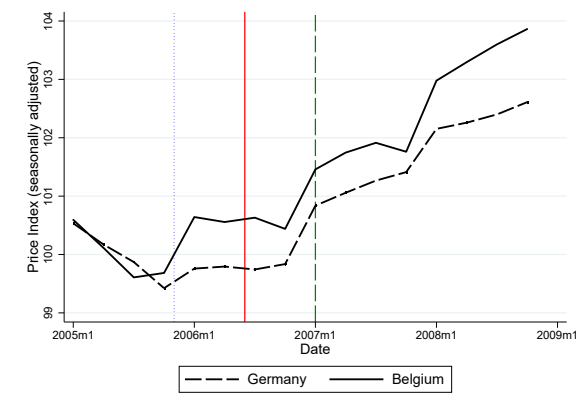
Notes: This figure shows how the seasonally adjusted price index for commodities subject to the standard VAT rate developed in Germany and Belgium from 2005 to 2009 on average. The dotted blue vertical line marks the passing of the coalition agreement in November 2005. The solid red vertical line marks the final legal decision on the reform to the standard VAT rate in June 2006. The dashed green vertical line marks the implementation of the respective VAT reform in January 2007.

Figure 1.B3: Macroeconomic Trends

(a) Inflation

(b) Real GDP Growth

(c) GDP per Capita

(d) Harmonized Unemployment Rate
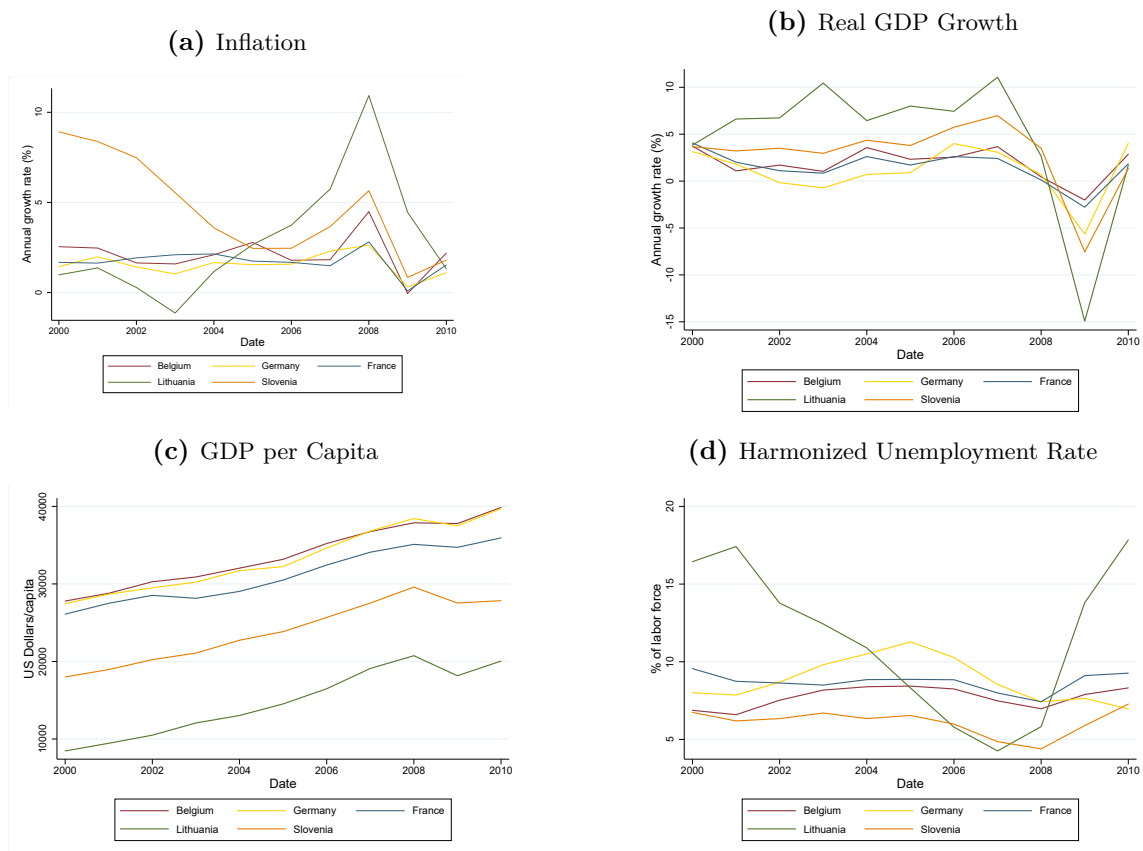
Notes: This figure shows macroeconomic trends for a selection of European countries for 2000 to 2010. Source: OECD.

Figure 1.B4: Development of the German Price Index Following the National (CPI) and the Harmonized (HICP) Definition
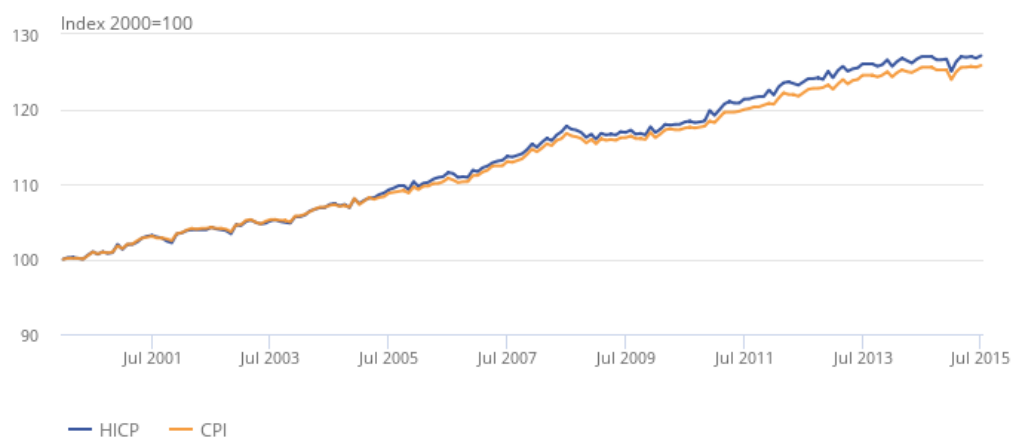
Notes: This figure shows how the price index in Germany develops following the national definition of the consumer price index (CPI) or the harmonized definition (HICP). Source: UK Office for National Statistics (2016).

Figure 1.B5: Residuals
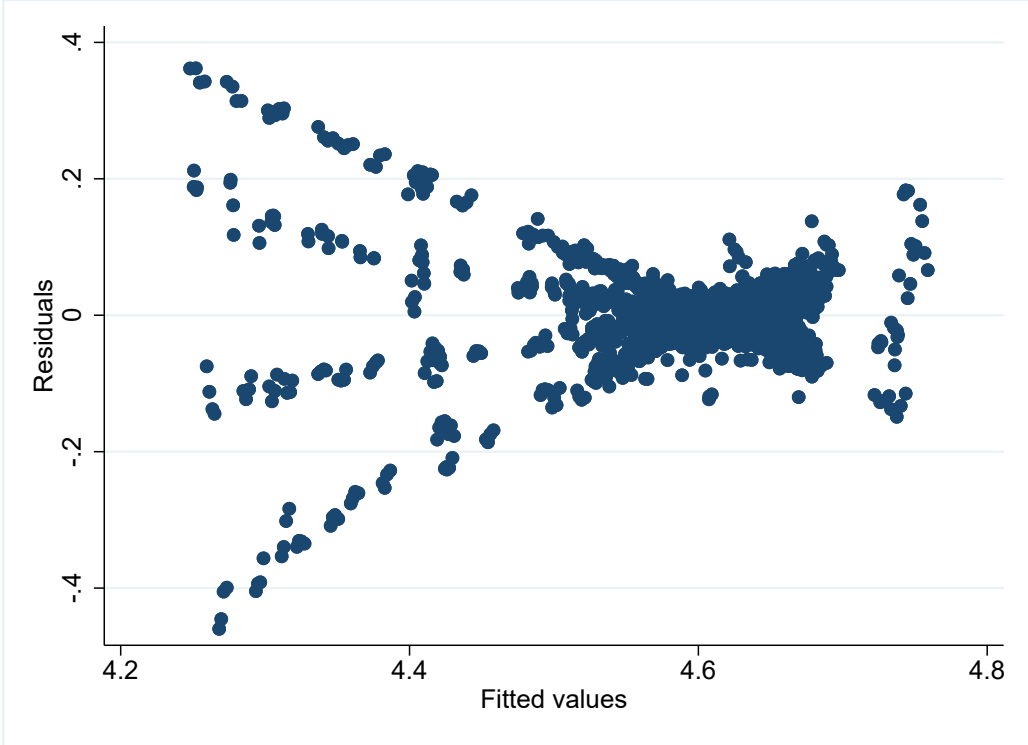
Figure 1.B6: Development of the Price Index for Selected Commodity Groups I
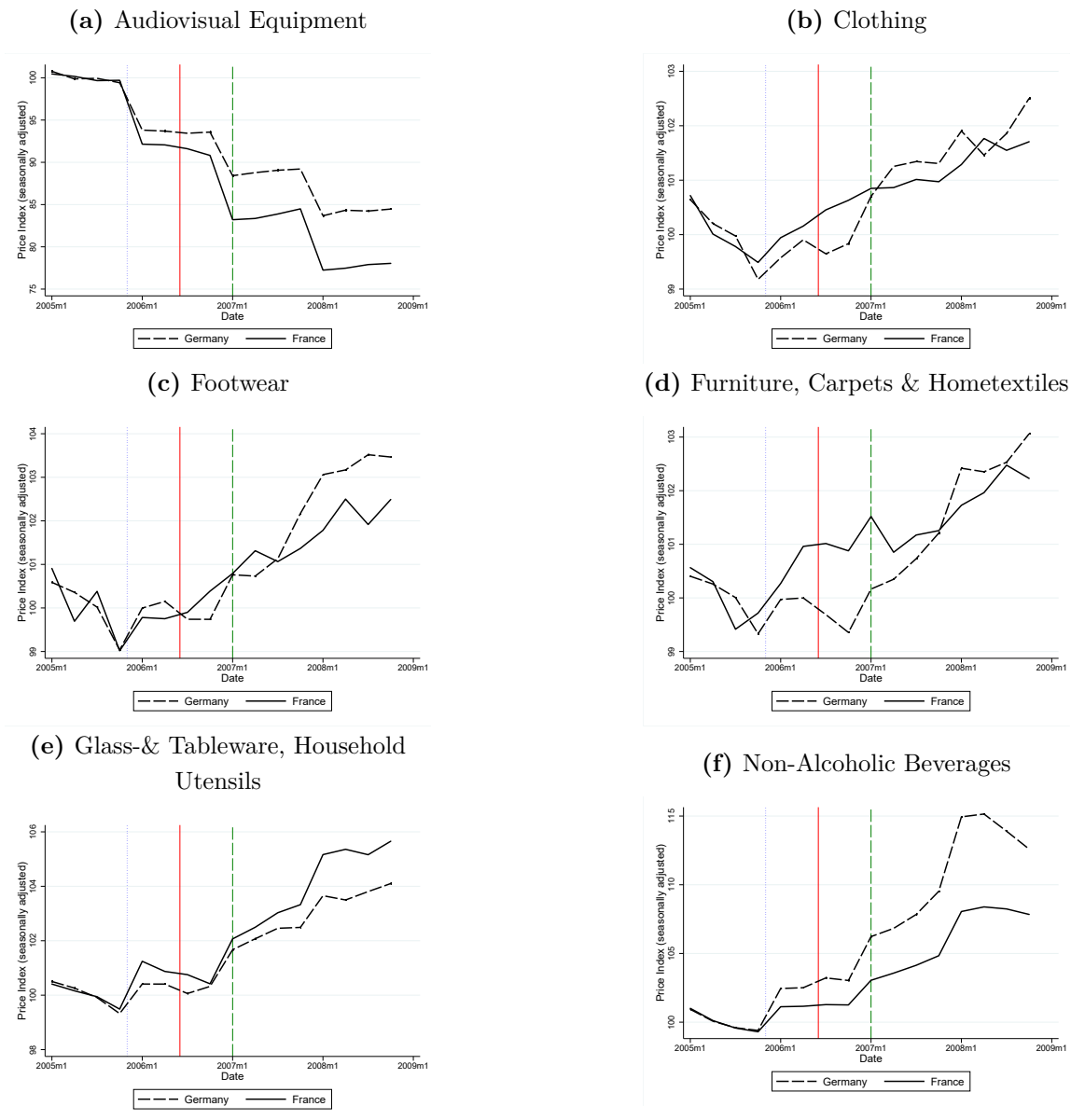
(a) Audiovisual Equipment

(b) Clothing

(c) Footwear

(d) Furniture, Carpets & Hometextiles

(e) Glass-& Tableware, Household Utensils

(f) Non-Alcoholic Beverages

Notes: This figure shows how the seasonally adjusted price index for selected commodity groups developed in Germany and France from 2005 to 2009. The dotted blue vertical line marks the passing of the coalition agreement in November 2005. The solid red vertical line marks the final legal decision on the reform to the standard VAT rate in June 2006. The dashed green vertical line marks the implementation of the respective VAT reform in January 2007.

Figure 1.B7: Development of the Price Index for Selected Commodity Groups II
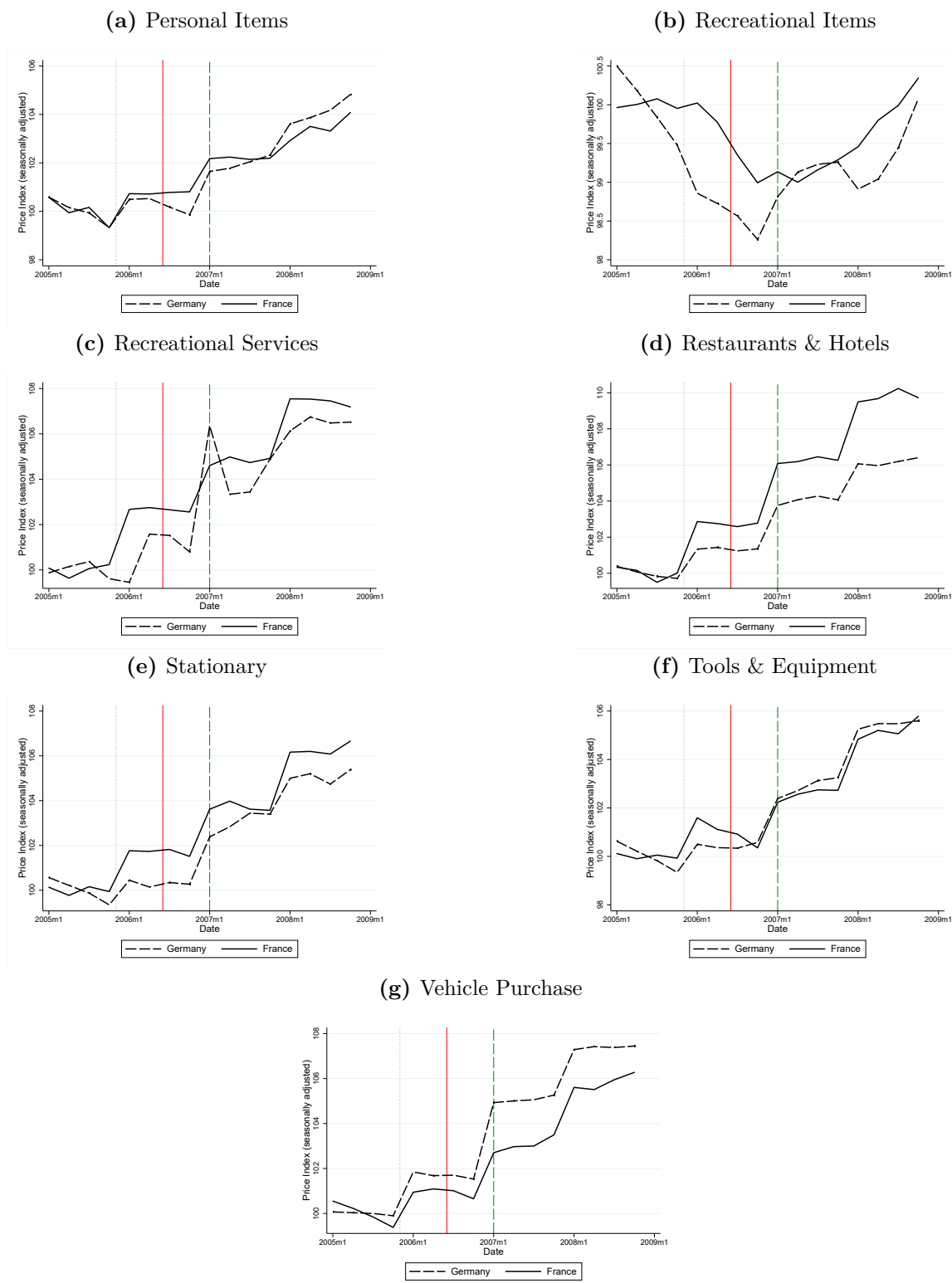
(a) Personal Items

(b) Recreational Items

(c) Recreational Services

(d) Restaurants & Hotels

(e) Stationary

(f) Tools & Equipment

(g) Vehicle Purchase

Notes: This figure shows how the seasonally adjusted price index for selected commodity groups developed in Germany and France from 2005 to 2009. The dotted blue vertical line marks the passing of the coalition agreement in November 2005. The solid red vertical line marks the final legal decision on the reform to the standard VAT rate in June 2006. The dashed green vertical line marks the implementation of the respective VAT reform in January 2007.

Figure 1.B8: Event Study Estimates for Selected Commodity Groups I
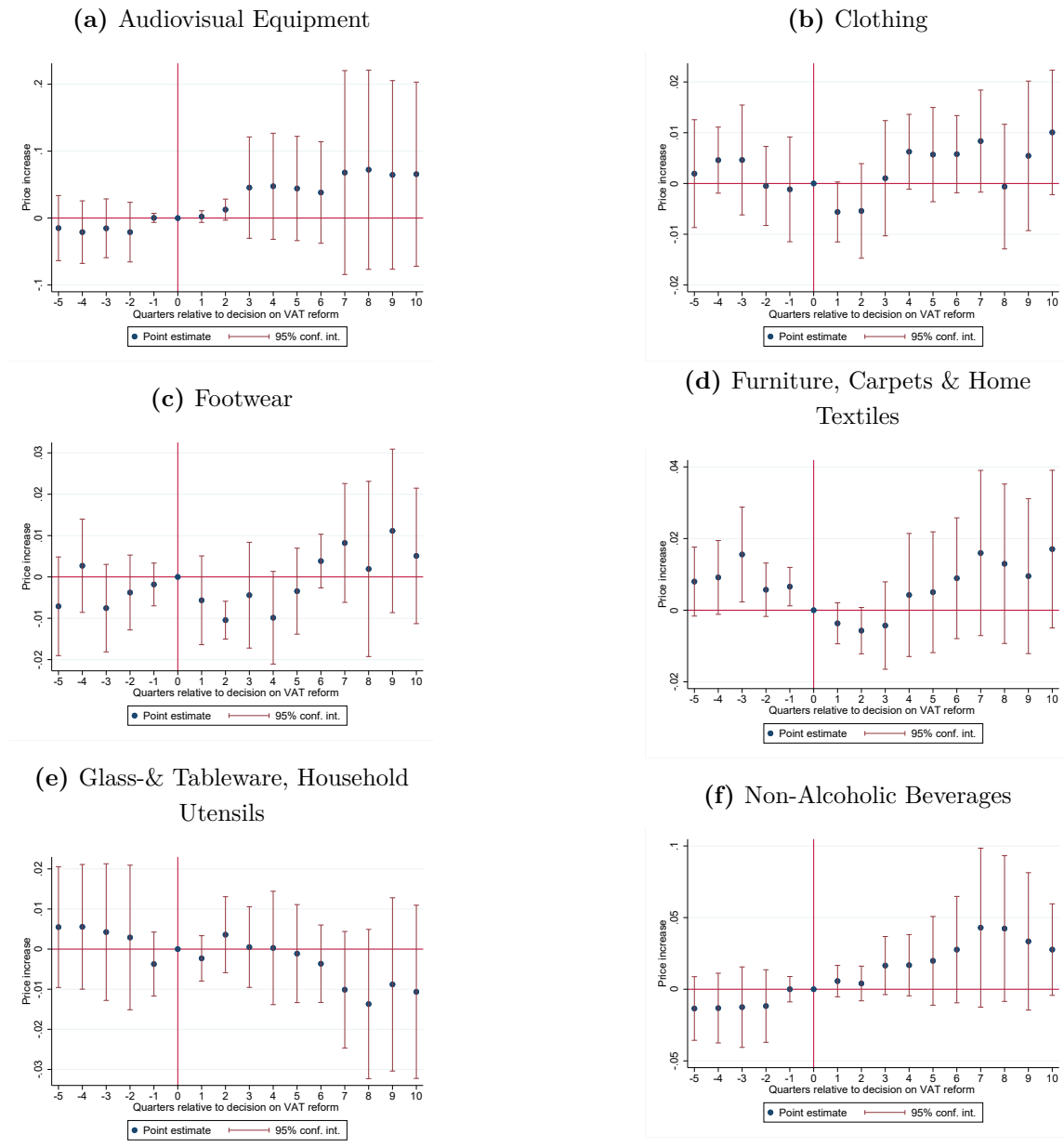
(a) Audiovisual Equipment

(b) Clothing

(c) Footwear

(d) Furniture, Carpets & Home Textiles

(e) Glass-& Tableware, Household Utensils

(f) Non-Alcoholic Beverages

Notes: This figure plots monthly event study estimates and corresponding 95% confidence bands. The dependent variable is the log of the price index for commodity i in month t and country. Commodity, time and country fixed effects are included. Standard errors are clustered at the commodity group level. The red vertical line marks the final legal decision on the VAT reform in June 2006. The number of observations can be found in Table 1.A2.

Figure 1.B9: Event Study Estimates for Selected Commodity Groups II
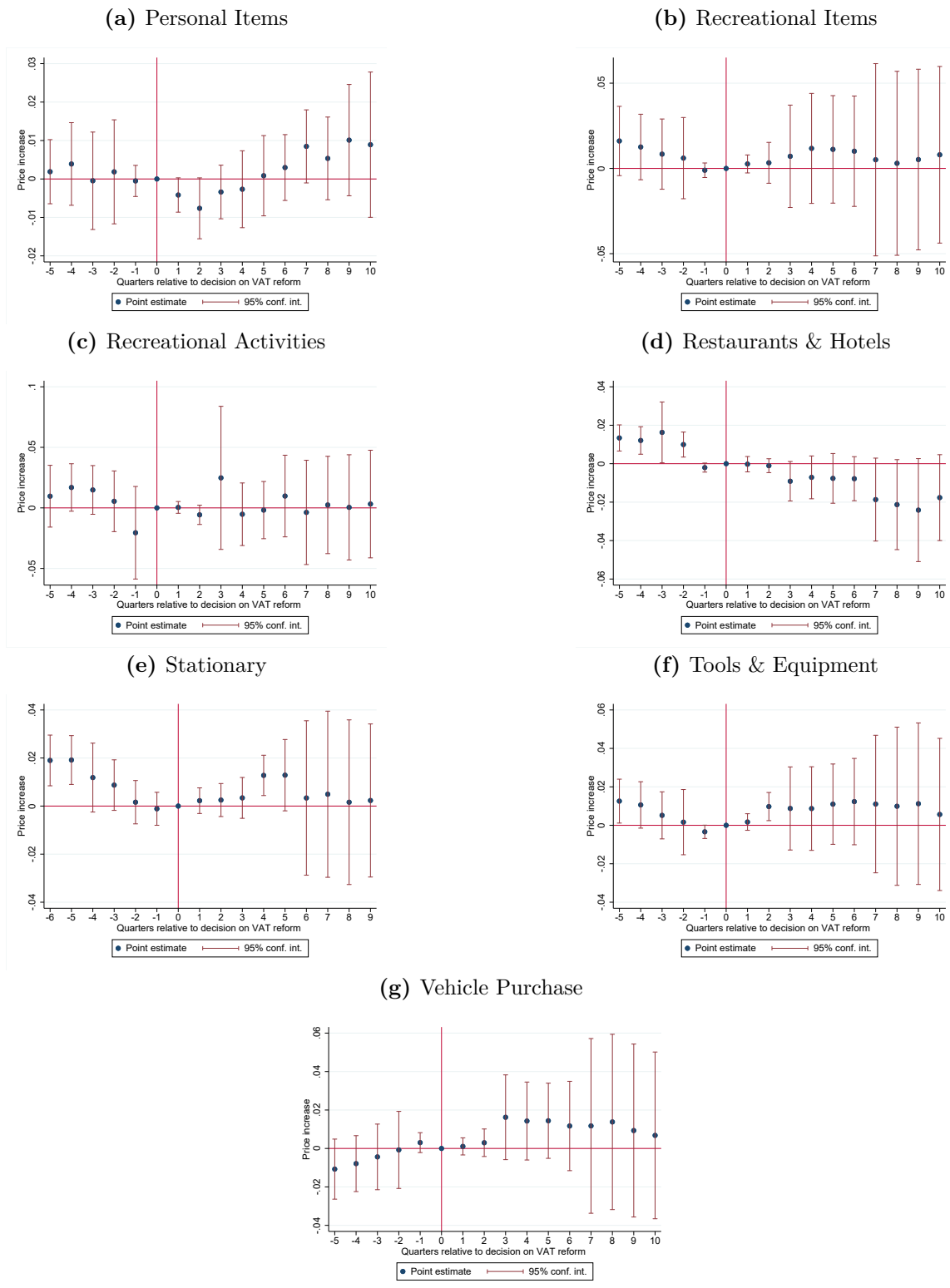
(a) Personal Items

(b) Recreational Items

(c) Recreational Activities

(d) Restaurants & Hotels

(e) Stationary

(f) Tools & Equipment

(g) Vehicle Purchase

Notes: This figure plots monthly event study estimates and corresponding 95% confidence bands. The dependent variable is the log of the price index for commodity i in month t and country. Commodity, time and country fixed effects are included. Standard errors are clustered at the commodity group level. The red vertical line marks the final legal decision on the VAT reform in June 2006. The number of observations can be found in Table 1.A3.

Figure 1.B10: 1st Net Household Income Quartile: Consumption Shares (%)
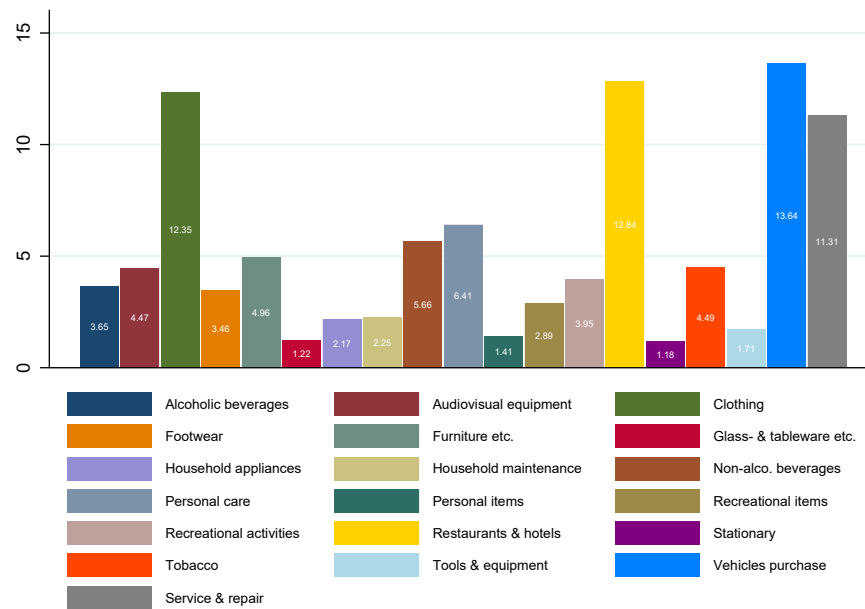
This figure shows consumption shares for 19 previously defined commodity groups for the first net household income quartile in %. This figure is based on data from the income and consumption survey (*Einkommens- und Verbrauchsstichprobe*, EVS) for the year 2003.

Figure 1.B11: 2nd Net Household Income Quartile: Consumption Shares (%)
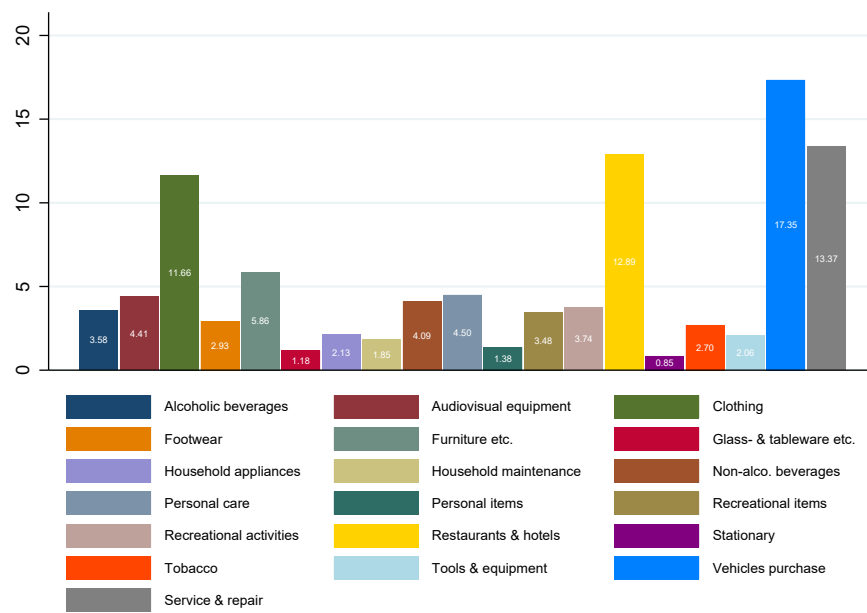
This figure shows consumption shares for the 19 previously defined commodity groups for the second net household income quartile in %. This figure is based on data from the income and consumption survey (*Einkommens- und Verbrauchsstichprobe*, EVS) for the year 2003.

Figure 1.B12: 3rd Net Household Income Quartile: Consumption Shares (%)
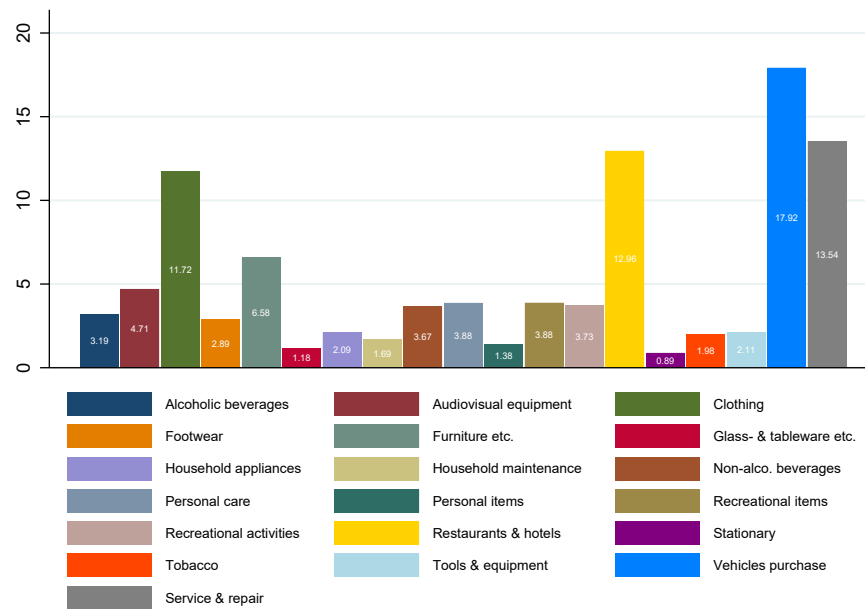
This figure shows consumption shares for the 19 previously defined commodity groups for the third net household income quartile in %. This figure is based on data from the income and consumption survey (*Einkommens- und Verbrauchsstichprobe*, EVS) for the year 2003.

Figure 1.B13: 4th Net Household Income Quartile: Consumption Shares (%)
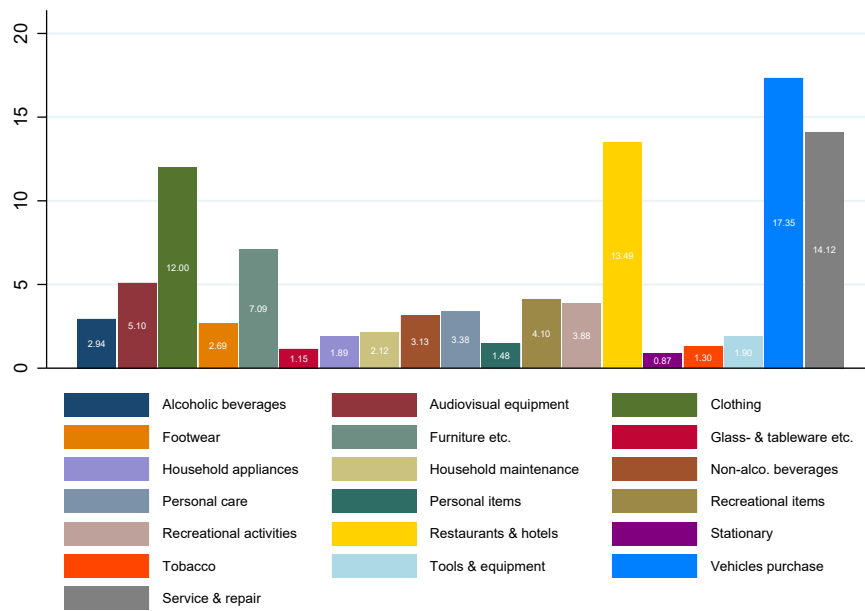
This figure shows consumption shares for the 19 previously defined commodity groups for the fourth net household income quartile in %. This figure is based on data from the income and consumption survey (*Einkommens- und Verbrauchsstichprobe*, EVS) for the year 2003.

Chapter 2

How Does Economic Deprivation Affect Local Fiscal Policy in Germany?

2.1 Introduction

How governments set their tax rates and the quality of the public services they provide is an important determinant of many outcomes. In terms of economic outcomes, for instance, fiscal policy impacts the level of public debt, investment, and even growth (Aghion et al.; 2014; Kneller et al.; 1999). But fiscal policy also influences social outcomes, such as social cohesion or mobility (Schneider; 2010). Moreover, it can also spark political protests, such as the yellow jackets in France who initially protested against an increase in fuel taxes. Thus, it can affect political outcomes as well.

This raises the question of what, in turn, determines fiscal policy. One hypothesis is that as societies become more heterogeneous in terms of income, it also becomes more difficult to agree on the provision of public services and redistributive policies. At the same time, there are also hypotheses indicating a positive association between income dispersion and government size, due a higher demand for redistribution. This is where this paper aims to make a contribution. Our goal is to identify the causal influence of economic inequality on fiscal policy outcomes. While existing contributions concentrate on general measures of income inequality, like the Gini coefficient, we focus on the effect of economic deprivation on fiscal policy. The Gini coefficient is often criticized for being an imperfect measure of income inequality, as very different income distributions can lead to the same realization of the Gini coefficient. For instance, both an increase in income for the rich and a decrease in

income for the poor can lead to a high Gini coefficient. However, it is especially the latter scenario which is of interest here, as increasing economic deprivation, i.e. an increase in inequality driven by the lower bound of the income distribution, aggravates the importance of redistributive fiscal policies. Specifically, we study the effect of three different measures of economic deprivation on fiscal policy, namely the poverty gap, the poverty rate, and the median gap.

We exploit the specific institutional setting in Germany, which grants municipalities and districts a high degree of fiscal autonomy: they can set the rates of a number of different taxes and they can decide on spending for several public services. We use this high level of local variation to identify the effect of economic deprivation on fiscal policy.

For our analysis, we construct a panel data set combining administrative fiscal data for the universe of German city districts with measures for economic deprivation from the German Microcensus. Our sample period covers the years from 1993 to 2016. We use instrumental variables (IV) estimation to draw conclusions about the causal influence of economic deprivation on local fiscal policy in Germany. Following Boustan et al. (2013), we construct instruments for region-specific measures of economic deprivation that are exogenous to asymmetric economic developments, to endogenous political reactions to growing support for radical parties, and to endogenous sorting of individuals into regions.

Our results are ambiguous regarding the distributional consequences of economic deprivation. We find that increasing economic deprivation causes local policy makers to increase the local business tax rate, while we do not find significant effects on the local property tax. Given that the local business tax is likely to be perceived as a progressive tax, whereas the perception of the property tax is more ambivalent, this seems like an attempt to make the tax system more redistributive as economic deprivation increases. However, aggregate spending on local public services is negatively affected by economic deprivation. In particular, this effect is driven by a spending cut on welfare, schooling, and sport facilities. As these public services are likely to mainly benefit lower income groups, our results suggest a negative relationship between economic deprivation and redistributive fiscal policies on the expenditure side. We discuss possible explanations for these ambiguous results as well as potential transmission channels.

The present paper is related to the literature examining the relationship between income dispersion and the size of the state.[1] This relationship has been addressed both theoretically and empirically. The seminal paper by Meltzer and Richard (1981) builds on the median voter theorem. Assuming majority rule, a decrease in the median income relative to the mean income translates into a stronger vote for redistribution. It can therefore

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[1]There are also papers studying other factors that affect local fiscal policy, such as the partisanship of local councils (Wittrock et al.; 2016).

be assumed that increasing economic deprivation makes redistributive fiscal policies more likely.

Epple and Romano (1996) construct a model in which both the public and the private sector provide goods and services. In contrast to the standard assumptions of the median voter theorem, this results in non-single peaked preferences, as the decision of whether to choose a public service or its private alternative depends on the level of the quality of services provided. In this setting, the median voter theorem does not generally characterize the voting equilibrium and the level of public provision is below that preferred by the median income voter as a coalition of poor and rich households favor private over public provision. Thus, an increase in the number of poor and rich leads to a decrease in the public provision of goods and lower taxation.

Economic theory can thus motivate both a positive and a negative relationship between economic deprivation on the one hand and the provision of public services and redistributive policies on the other hand. However, most theoretical models are based on majority rule. Hence, their findings only have limited application in a setting with proportional representation, such as Germany.

The empirical literature on income dispersion and fiscal policy can be broadly divided into two branches: the first group uses cross-country data, the second group focuses on sub-national jurisdictions in countries with a federal system. The first group has the advantage that national governments are typically equipped with more far-reaching fiscal competencies than sub-national governments. The advantage of the latter group is that sub-national jurisdictions are typically more comparable with regard to their institutional and political framework, which mitigates concerns about endogeneity biased estimates. However, to the best of our knowledge, studies focusing on the sub-national level only exist for the US. Moreover, both literature branches typically only rely on the Gini coeffcient as a measure of income inequality.

Milanovic (2000) uses data from the Luxembourg Income Study (LIS) to investigate the relationship between income inequality and redistribution for 24 countries with a fixed effects model. He finds a significant positive relation but no evidence that the median voter theorem can explain the redistribution as the income gain of the middle class proves to be independent from its initial income.[2]

Karabarbounis (2011) uses three different indices of income inequality for 14 OECD countries in the same empirical framework. He argues that not just the median voter, but

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[2]Attempting to test the relationship between redistribution and components of the Mirrless model, Hannu et al. (2018) also employ LIS data for a sample of 14 countries. Using the optimal tax formula to construct redistributive preferences, they find a positive correlation between factor-income inequality and the extent of redistribution, which in turn has a positive effect on governments' propensity for redistribution.

the demands of various income groups of voters determine redistribution. Establishing a positive relationship between the income of the poor and redistribution as well as a negative relationship between income of the middle and upper class and redistribution, he coins the term *one dollar, one vote politico-economic equilibrium:*, an increase in the income of a group of citizens relative to the average income results in redistribution tilting towards the bliss point of that group.

Focusing on the sub-national level, our paper is most closely related to Boustan et al. (2013). Using an IV approach they show that a broadening of the income distribution is associated with an increase in tax collection and expenditures in US municipalities and school districts. They also analyze the impact of a rise in inequality on the composition of local expenditures and find that particularly police, fire protection, and infrastructure receive additional funding. Similarly Corcoran and Evans (2010) find a positive relation between income inequality and educational spending on school district level in the US. Using different inequality measure, Schwabish (2008) shows that both an increase in the upper- as well as the lower-end of the income distribution leads to an increase in social spending on non-health and non-educational goods and services. He uses US state-level data for the time period 1977-2005.

While the above papers focus on the relationship between income inequality and the size of the public sector, other works concentrate on how income inequality affects tax progressivity and tax structure. For the 434 local governments in Norway, Borge and Rattsø (2004) find that a more unequal income distribution causes the tax burden to shift from the poll to the property tax. This points to an increase in redistribution as the property tax in Norway is proportional to income. Looking at various determinants of sub-national tax progressivity in the US, Chernick (2005) finds that greater inequality in pre-tax income distributions is compensated by more progressive tax systems but the effect is relatively small.[3]

As the overview above shows, most existing empirical literature on the sub-national level focuses on the US. Due to the differences in the political system, the possibilities for comparing those results with the German setting are limited. Most importantly, the US uses majority rule, whereas Germany follows the principle of proportional voting, also on the local level. This difference is likely to impact results. Freier and Odendahl (2012), for example, show that majority governments spend more on public services and set higher tax rates, which is in-line with the results for the US presented above. We therefore test whether fiscal policy differs between divided or unified governments in our data.

More generally, our paper is also related to the literature on the effect of diversity

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[3]As measure for the degree of tax progressivity, Chernick (2005) uses the income tax share compared to the sales and excise tax shares in state and local tax systems.

and heterogeneity in society on the size of the welfare state and the willingness to redistribute. Alesina et al. (1999) find that ethnic fragmentation on the local level in the US is associated with a decrease in the share of spending on public goods, such as education or infrastructure. An et al. (2018) connect ethnic fragmentation to income inequality by studying whether the *racial structure* of inequality, i.e. the between-race component of income inequality, affects local provision of public goods in the US. While they do not find consistent evidence that the overall level of income inequality affects local investment in public goods, they find a significant negative relationship between the between-race component of inequality and public goods spending.[4] Chevalier et al. (2018) study the effect of mass migration into West Germany after World War II on local governments' public policy. They document a shift in the tax burden towards farm and business owners and an increase in welfare spending.

The remainder of the paper is structured up as follows. Section 2.2 describes the institutional background of the local government administration in Germany. Section 2.3 presents our data as well as the measures of economic deprivation that we construct. We provide descriptive statistics of our data in Section 2.4. The empirical strategy is explained in Section 2.5. In Section 2.6 we present our results on the relationship between economic deprivation and local fiscal policy. We discuss these results in Section 2.7. Robustness tests are presented in Section 2.8. Section 2.9 concludes.

2.2 Institutional Background of the Local Government Administration in Germany

Due to the federal structure of Germany, power is divided both horizontally as well as vertically between the 16 federal states (*Bundesländer*) and the local authorities, that is, municipalities, districts, and city districts. City districts are large municipalities that constitute their own district, such as Munich or Frankfurt. For reasons of data availability, we restrict our analysis to city districts (see Section 2.3 for details). Municipalities, districts, and city districts constitute the lowest level of the state and administrative structure in Germany. Nevertheless, the German constitution grants them local autonomy within the limits of the law (German Constitution Art. 28). This right of self-governance includes financial and tax sovereignty. Financial sovereignty entitles local authorities to manage their income and expenditure. Tax sovereignty grants them the right to raise taxes as long as this does not violate higher law. The main tax rates that are determined at the local level are two types of property tax rates as well as the local business tax (LBT) rate. To

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[4]Similarly, Lind (2007) and Hero and Levy (2018) find evidence for an inverse relationship of between race inequality and welfare redistribution for the US.

be precise, the local authorities can decide on the *local scaling factor* of the property tax rates and the LBT rate. Particularly the LBT is an important income source for local authorities. In 2016, for example, the LTB contributed 43% to the overall tax revenue of the German local authorities. The two property taxes jointly accounted for 14% of the overall tax revenue (Statistisches Bundesamt; 2017).[5]

The LBT rate is determined by multiplying the respective local scaling factor with the basic rate *(Gewerbesteuermesszahl)*. The basic rate is defined at the federal level and from 1993-2007 was set at 5.0% with a decrease to 3.5% in 2008. In contrast, the local scaling factor for a given year is voted on by the municipal council one year in advance. Thus, changes in the LBT rate are primarily driven by changes in the local scaling factor, which is determined by the local authorities.[6]

There are two types of property tax rates in Germany: property tax rate A, which applies to agricultural areas, and property tax rate B, which applies to residential property. The property tax rates are again determined by multiplying their respective local scaling factor with their respective basic rate (*Grundsteuermesszahl*), which is determined by federal law (Property tax law §14 and §15) and depend both on the value and the type of property. In our main analysis we focus on the property tax B as, together with the LBT, it constitutes the most important local tax instrument in terms of revenue collection. In contrast to countries like Norway where the property tax is proportional to income and thereby constitutes a progressive tax, the incidence of the residential property tax in Germany is not straightforward. It is proportional to the value of the property. However, it is, strictly speaking, not a tax on the ownership of property, which would make it more plausible to classify it as progressive tax as higher income groups in Germany are more likely to own property (Dustmann et al.; 2018). Instead, it is a tax on the "right to reside" as it is part of the apportionable service charges. Thus, landlords can pass it on to their tenants. Indeed, Löffler and Siegloch (2018) show that in the long run both the statutory and the economic incidence of the property tax are borne by the tenant. Hence, it seems more likely that the property tax B is a regressive tax.

On the expenditure side, local self-governance includes only those tasks that fall within the local authorities' own sphere of influence, whereas it does not apply to tasks that the federal government or the respective federal state has transferred to the local authority by law. As we are interested in how local authorities react to changes in local inequality, only tasks that fall under their own sphere of influence are of interest here. In contrast, local authorities have no discretion in spending on tasks that do not belong to their area

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[5]The remaining sources of tax revenue are the local authorities' share of the income tax (37%) and the sales tax (5%) as well as other taxes (2%).

[6]Note that the tax *base* and liability criteria of the LBT are determined at the federal level.

of influence. Instead, they have to spend a specific earmarked amount on these tasks, for which they typically receive grants from the respective federal state. Therefore, we restrict our analysis to those tasks that fall the local authorities' sphere of influence. These tasks differ in their degree of local autonomy. In the case of *voluntary* self-government tasks (*freiwillige Selbstverwaltungsaufgaben*) the local authority is free to choose whether and how to carry out the activity. In contrast, local authorities are obliged to fulfill *mandatory* self-government tasks (*pflichtige Selbstverwaltungsaufgaben*), but are free to choose the manner of provision.

As the definition of the different spheres of influence as well as the different types of self-government tasks is not precisely and consistently specified across the different constitutions and municipal codes of the federal states, we follow the categorization in Postlep (1987). We also cross-check this definition with the constitutions and municipal codes of the federal states where possible and find a great overlap. Accordingly, examples for voluntary self-government tasks are cultural activities such as operating theaters or the construction of sports facilities. Mandatory self-government tasks include school maintenance and waste removal. For the purpose of our analysis, both voluntary and mandatory self-government tasks are of interest as local authorities have at least some degree of autonomy in their provision.[7] To account for the varying definitions of tasks across local authorities, we include regional fixed effects in our regressions (see Section 2.5).

2.3   Data

To analyze the relationship between economic deprivation and local fiscal policies, we construct a unique panel dataset combining local poverty measures with data on local government expenditure and taxation. Our panel covers the years 1991 to 2016 for West Germany and 1998 to 2016 for East Germany. Most importantly, to be able to combine the spending and taxation data on the municipality-level with the poverty measures on the district-level, we restrict our sample to city districts. These are large municipalities that form their own district. Our final sample includes roughly 104 city districts per year[8], of which more than 80% are in West Germany. Our sample of city districts accounts for roughly 30% of the overall German population.

To create our dataset, we mainly rely on three sources. Regional inequality and poverty measures are constructed based on microdata from the German Microcensus (*Mikrozensus*). Data on local taxation is taken from the real property and business tax statistic (*Realsteuervergleich*). Finally, local government expenditure is calculated based on the an-

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[7]See Section 2.3.4 for more details.

[8]The exact number varies 87 in 1993 (West Germany only) and 110 in 2005.

nual account data of municipalities (*Jahresrechnungsstatistik der Gemeinden und Gemeindeverbände*).

2.3.1 The German Microcensus

The Microcensus is a household survey that is carried out annually since 1957 by the statistical offices of the German states (*Statistische Landesämter*) and is administered by the Federal Statistical Office (*Statistisches Bundesamt*). It comprises a representative 1%-sample of the German population, resulting in a sample size of more than 800,000 persons from almost 400,000 households per year. The Microcensus contains information about various demographic characteristics, including the county of residence, employment status, household size, the age of all household members, education levels, and household income, among others. For our analysis, we use the waves from 1991 to 2016. As the Microcensus is not available for the years 1995 and 1996, there is a gap in our sample for these two years. Besides the numerous variables, one major advantage of the Microcensus is its large sample size, which allows us to construct economic indicators at the regional level. Moreover, the Microcensus is administered by a federal agency and there is a legal obligation to answer the questions. Therefore, item-non-response is not an issue. Also, answers must be truthful and complete.

We use information on monthly net household income to construct our measures of economic deprivation. To account for differences in household size, we compute equivalized household incomes using the OECD equivalence scale. In addition, we adjust the income figures for changes in prices using the consumer price index for Germany. Note that the income variable in the Microcensus dataset is interval-censored, i.e., respondents are asked to indicate in which income class they belong to. However, the width of the income classes is rather narrow and the number of income classes is large, varying between 18 and 24, depending on the survey year. In order to obtain continuous household income figures, we apply an imputation approach. That is, we estimate a continuous income figure for each household based on information on a household's income class as well as various socio-demographic characteristics using interval regressions. This imputation technique ensures that the empirical distribution of the continuous income variable matches the shape of the distribution of the income classes. As a result, we obtain a single income figure for each household that is consistent with the observed income limits (see for example Royston (2008)), which we then use to calculate a number of inequality and poverty measures at the district-level.

2.3.2   Indicators of Economic Deprivation

We employ three different indicators of economic deprivation that account for the relative economic well-being of citizens living in a given district compared to the national average. Our first indicator measures the *poverty rate* within a district; that is the share of households within a district living below the national poverty line $z_{pov,t}^{nat}$ in year t. We set the poverty line at 60 percent of the national median income $z_{50,t}^{nat}$, so that $z_{pov,t}^{nat} = 0.6 \times z_{50,t}^{nat}$.

Our second indicator of economic deprivation is constructed in a similar fashion, but it measures the average shortfall from the national poverty line instead of the share of households living below the poverty line. This indicator is widely known as the *poverty gap* and is defined by the following formula:

$$Poverty\ gap_{it} = 100 \frac{1}{n_{it}} \sum_{j=1}^{q} \frac{z_{pov,t}^{nat} - y_{ijt}}{z_{pov,t}^{nat}}, \tag{2.1}$$

where n_{it} is the number of households from city district i at year t that are included in the Microcensus data, and y_{ijt} is the income of household j.

Our final measure of relative economic deprivation is the average shortfall in the incomes of a city district's residents from the national median income. We refer to this measure as the *median gap.* It is constructed as follows:

$$Median\ gap_{it} = 100 \frac{1}{n_{it}} \sum_{j=1}^{q} \frac{z_{50,t}^{nat} - y_{ijt}}{z_{50,t}^{nat}}. \tag{2.2}$$

2.3.3   The Property and Trade Tax Statistic

The property and trade tax statistic collects all information regarding local tax revenues. It is published on a yearly basis by the Federal Statistical Office. Besides recording the local scaling factors of the property taxes and the LBT, it also includes the respective tax revenues as well as information on the municipality's share of the income and sales tax revenue. As the city states, Bremen, Berlin and Hamburg, have a greater degree of autonomy over their budget, we exclude them from our sample for consistency.

2.3.4   Annual Account Data of Municipalities

The municipalities' annual account data for the years 1992-2006 is taken from the statistical offices of the German states. The data is available for all federal states except for the city states Berlin, Bremen and Hamburg. Moreover, for the years 1992-1997 only data for West Germany is available. The data contains information on income and expenditure of German municipalities and specifies to which task the cost or income can be assigned. As

specified in Section 2.2, we focus on voluntary and mandatory self-government tasks for our analysis, as municipalities have at least some degree of autonomy in their provision. We therefore select those expenditure items in the municipalities' account data that fit this definition and create sensible clusters to obtain our final list of expenditure variables.[9] Following this procedure, our final list of expenditure variables includes schooling, welfare, health, sport, culture, public education, waste disposal, fire protection, local police, and roads. These variables only cover costs that fall under the definition of voluntary and mandatory self-government tasks of the municipalities. For schooling expenditure, for instance, this means that the variable includes costs for school maintenance and wages for non-teaching staff but it does not cover wages for teaching staff as these costs incur at the level of the federal states. Moreover, we create an aggregate over all spending on voluntary and mandatory self-government tasks for a given year and municipality, which we label *aggregate spending* in the following.

While the annual account data offers a good approximation of municipalities' income and expenditure, it also needs to be noted that this data does not cover all forms of financing. For instance, so-called *Public private partnerships (PPP)* are not included in the data. Thus, if a school or local road is renovated with the funds of a PPP, this is not included in the annual account data of the municipality. We can therefore only study the effect of economic deprivation on the officially recorded spending on public services.

2.3.5   Control Variables

In our empirical analysis, we include several control variables depicting the demographic, economic, and political situation in a city district. We control for its dependency ratio, population density, the share of foreigners, and mean income. Furthermore, we control for political polarization, which we define as the aggregate vote share for extreme right and extreme left parties at national elections.[10] To account for the fact that city districts also have other income sources besides the revenue generated by local businesses and the property tax, we control for the log of per capita income tax revenue, which the city districts receive from the federal states.[11] Population densities are provided by the Federal Institute for Research on Building, Urban Affairs and Spatial Developments (*Bundesinstitut für Bau-*

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[9]Table 2.B2 in the Appendix shows our selection of expenditure items and how we combined them to create our final expenditure variables.

[10]The definition for extreme right and extreme left parties in Germany follows Dorn et al. (2018). Note that political polarization might be an insufficient control variable as a rise in economic deprivation could result in political polarization. We therefore rerun our main specification without that control variable. The results do not change.

[11]Note that the share of the income tax revenue that city districts receive is fixed, and city districts cannot manipulate it.

, *Stadt-, und Raumforschung, BBSR*). The share of foreigners is taken from the German Regional Database (*Regionaldatenbank Deutschland*) as well as the Statistical Offices of the German states. Federal election outcomes are provided by the federal returning officer (*Bundeswahlleiter*). The remaining control variables are calculated based on individual responses from the German Microcensus.

2.4 Descriptive Statistics

In the following three subsections we provide graphical descriptions of regional economic deprivation, local taxation and spending on public services. Table 2.B1 in the Appendix provides summary statistics for all our dependent and control variables.

Figure 2.1: Measures of Economic Deprivation

(a) Poverty Rate (%)   **(b)** Poverty Gap (%)
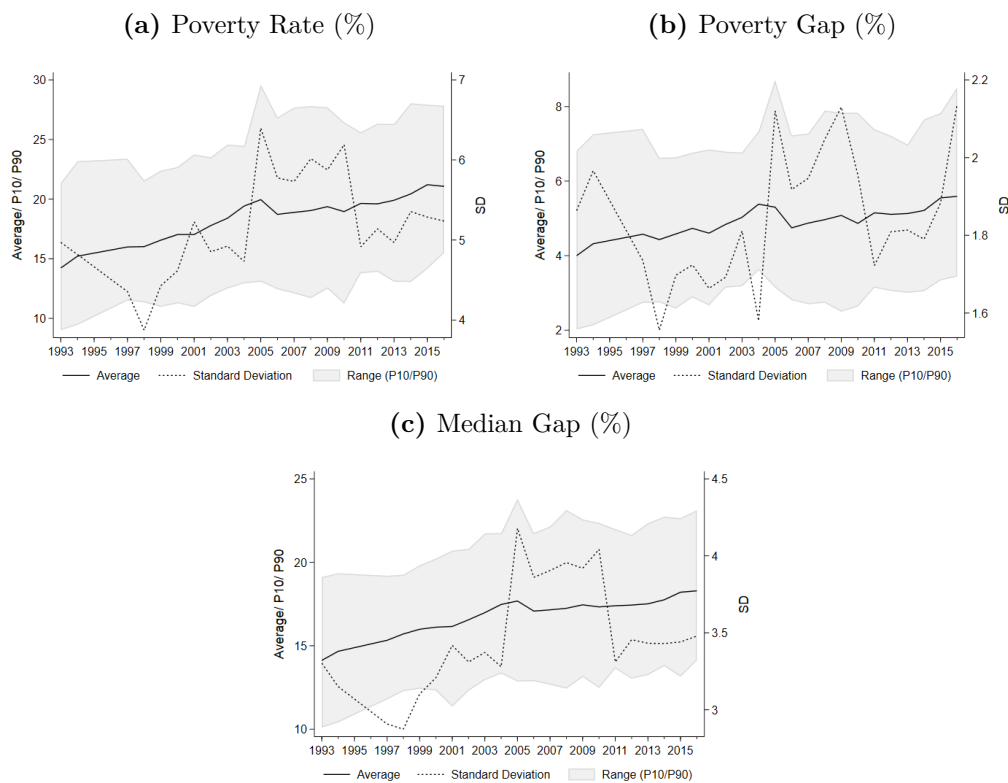
(c) Median Gap (%)

Notes: This figure shows the development of the average poverty rate, the poverty gap, and the median gap of city districts over the sample period from 1993 to 2016. Additionally, the standard deviation and the range (P10/P90) for each measure are displayed.

2.4.1   Regional Economic Deprivation

The average development of the three measures of economic deprivation between 1993 and 2016 for the city districts in Germany is shown in Figure 2.1. All three indicators, the poverty rate, the poverty gap and the median gap, evolve rather similarly. There is a steady increase since 1993 with a peak in 2004/2005. The peak could be due to the Hartz reforms, which were implemented at that time and changed the unemployment and social benefits system in Germany. The largest drop can be observed in the poverty gap, while the median gap displays the smallest decline. From 2006 economic deprivation increases again. For the poverty rate and the poverty gap the increase continues at approximately the same slope as before the peak, while the slope is slightly reduced for the median gap. During the sample period, economic deprivation has therefore become a more urgent problem in German city districts.

2.4.2   Local Taxation

Figure 2.2 illustrates the development of the average local scaling factor of the locally determined tax rates for the city districts from 1993 to 2016. The local scaling factor of the LBT rate starts at a level of just over 400% in 1993 and increases slowly until 1997, when there is a slight decline. Afterwards the local scaling factor of the LBT remains rather stable until 2009. It then increases to reach approximately 430% in 2016 (see Figure 2.2a).

At around 360% the local scaling factor of the property tax B rate in 1993 is lower than that of the LBT rate (see Figure 2.2b). However, the local scaling factor of the property tax B rate increases continuously throughout the sample period, with an increase in the slope in 2009. By 2016 the level of the local scaling factor of the property tax B rate is just under 500%. Finally, the local scaling factor of the property tax A rate increases from just over 250% in 1993 to just over 300% in 2016 (see Figure 2.2c).

Figure 2.2: Local Tax Rates (Local Scaling Factor)

(a) Business Tax Rate (%)   **(b)** Property Tax B Rate (%)

(c) Property Tax A Rate (%)

Notes: This figure shows the development of the average local scaling factor for the business rate rate, the property tax B rate, and the property tax A rate of city districts over the sample period 1993 to 2016 in %. Additionally, the standard deviation and the range (P10/P90) for each measure are displayed.

2.4.3   Local Government Spending

We use the city districts' annual account data to depict how average per capita spending for voluntary and mandatory self-government tasks in German city districts has evolved over the sample period 1993 to 2006. We show the aggregate trend but also break it down into the individual spending categories. The aggregate average per capita spending is fairly stable over time at around €1000 as Figure 2.3 shows.

Figure 2.3: Aggregate Per Capita Spending on Public Services (€)
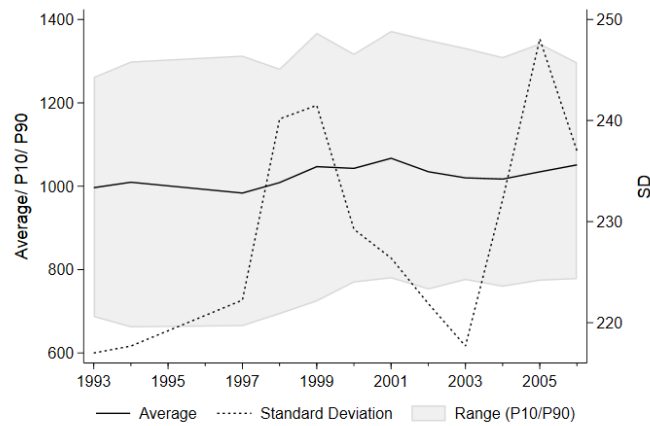
Notes: This figure shows the development of average aggregate per capita spending on public services in the German city districts from 1993 to 2006 in €. Public services include only those defined as voluntary and mandatory self-government tasks. Additionally, the standard deviation and the range (P10/P90) are displayed.

To see whether the composition of spending has changed over time, Figures 2.4a and 2.4b divide aggregate spending for the years 1993 and 2006 into the different categories. The biggest changes can be observed in the categories waste disposal, welfare and schooling. While spending on waste disposal accounts for 27% of aggregate spending in 1993, it drops to 12% in 2006. On the other hand, from 1993 to 2006 the share of spending on schooling and welfare increases by 5% and 10%, respectively. The share of the remaining categories remains fairly stable over the sample period.

Figure 2.4: Composition of Spending on Public Services
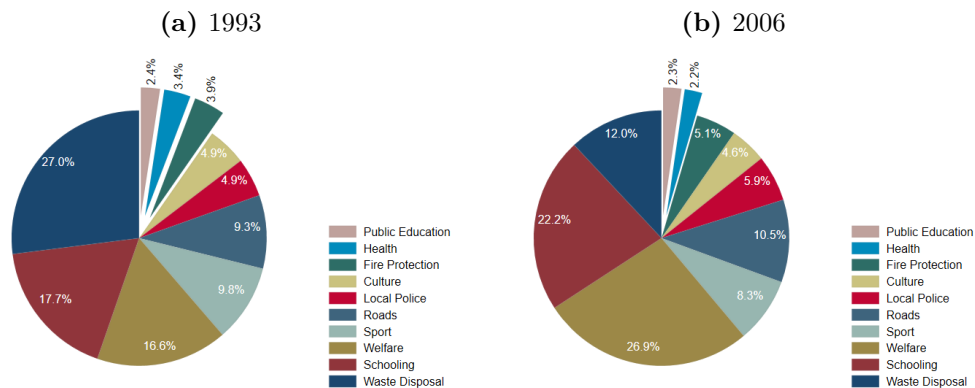
Notes: This figure shows the average share of per capita spending on the different categories of public services in % for the years 1993 and 2006 in the city districts.

2.5   Empirical Approach

To evaluate the influence of economic deprivation on local taxation and public spending we estimate the following empirical panel data model first by ordinary least squares (OLS) and then by an instrumental variables approach:

$$\tau_{it} = \alpha_i + \beta Deprivation_{it} + \gamma' X_{it} + \delta_t + \epsilon_{it} \qquad (2.3)$$

where the dependent variable τ_{it} is either the local scaling factor of the property or business tax in city district i and year t or the city district's expenditures on its mandatory and voluntary self-government tasks.[12] α_i is a city district fixed effect that is included to account for time-invariant regional-specific factors that are related to economic conditions and might affect fiscal policy and δ_t is a year dummy to capture the effect of nation-wide events. We also include several demographic, economic, and political control variables X_{it}. Finally, $Deprivation_{it}$ is a measure of regional economic deprivation. In our empirical analysis, we employ three different measures of economic deprivation: the poverty rate, the poverty gap, and the median gap. Moreover, we consider not only the effect of economic deprivation on the LBT and property tax *rates* but also the respective *revenues* per capita. To account for the skewed distribution of tax revenue, we take logged values.

2.5.1   Endogeneity Concerns and Instrumental Variable Approach

Identifying the causal effect of regional economic deprivation on fiscal policy is challenging since there are several confounding factors that are correlated with both fiscal policy and regional economic conditions, such as preferences of local voters or compensatory transfers from the state or federal government (Boustan et al.; 2013). We try to control for the first by including the vote share of extreme right and left wing parties at the latest federal election in our estimation, and for the latter by including the district's revenue share of the (national) income tax. Nevertheless, we cannot rule out the possibility that there are other relevant variables which we cannot observe and that might distort the results. Besides omitted variable bias (OVB), biased estimates as a result of household sorting, and reverse causality are further concerns. While economic deprivation may induce politicians to adjust local taxes and expenditures, the reverse may also be true. Especially since socio-demographic characteristics as well as preferences for local public good provisions can be decisive for households to settle in a certain region. For example, richer households may prefer to live in regions with higher standards or quality of public services, such as

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[12]To be precise, we use the logged value of per capita spending on mandatory and voluntary self-government tasks.

theaters, operas, schools etc., even though local taxes may be higher. This can lead to a shift in the income distribution.

In order to mitigate concerns regarding biased estimates due to the endogeneity of our covariates, we apply two-stage least squares estimation using instrumental variables for our economic deprivation measures. Following Boustan et al. (2013), we use counterfactual (predicted) economic deprivation measures as our instruments for actual economic deprivation. The instruments are constructed as follows: We first compute the average household income for each percentile of the national income distribution for all survey years (1991-2016). Then, we compute the annual national income growth rate for each percentile. Next, we focus on the initial survey year, determine the income percentile each household belongs to based on the national income distribution, and multiply each household's income with the percentile-specific national income growth rate. In this way, we obtain hypothetical incomes for all subsequent years for each household we observe in the initial survey year.[13] Finally, we use these hypothetical incomes to compute counterfactual regional economic deprivation measures as instruments. These economic deprivation measures indicate how regional economic conditions would have developed in the absence of inward and outward migration and whether each household's income would have changed over time in accordance with the percentile-specific national average. Consequently, our instruments only capture changes in the regional income distribution that are driven by national trends and cannot, by design, be influenced by district-specific trends such as mobility into and out of regions (Boustan et al.; 2013).

The instrumental variables we construct mimic so-called Bartik-style instruments. Goldsmith-Pinkham et al. (2020) discuss the conditions under which these instruments are valid. The authors show that for the exogeneity assumption to hold, differences in initial conditions - here: a city district's income distribution in the base year - must be unrelated to *changes in* (not: levels of) the outcome variable in the following years. To test whether this assumption holds, Goldsmith-Pinkham et al. (2020) propose regressing changes in the outcome variable on the time-invariant variable that indicates the initial conditions interacted with time fixed effects. In principle, this approach resembles an event-study analysis in which the indicator capturing the conditions in the base year is the (continuous) treatment variable. In our setup, this implies regressing changes in the tax variables and public spending on the poverty rate, poverty gap, and median gap in the base year interacted with year dummies. The results are presented in Figure 2.A1 of the Appendix. For the business tax rate and the property tax B rate, the event study coefficients are insignificant at every reasonable level of significance, thus indicating the

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[13]To account for the fact that at especially at the beginning of the 1990s, East and West Germany were still very different, we follow this procedure separately for East and West Germany.

validity of the IV approach. For aggregate spending, we obtain significant event study coefficients in the first three sample years. In later years, however, the coefficients become notably smaller (in absolute terms) and statistically insignificant. We are thus confident that the bias in the IV estimate, when using aggregate spending as the dependent variable, is negligible (if existent at all).

An additional challenge specific to the use of district-level data in Germany is that the number of districts in East Germany has changed considerably after German unification due to various administrative-territorial reforms. For example, from 1990 to 1996, the number of districts in East Germany (excluding East-Berlin) dropped from 215 to 111. For this reason, we are forced to use the income distribution of 1997 to construct our instruments for East German counties. This, however, implies that we cannot use observations on East German districts prior to the federal election held in 1998 when employing an instrumental variable approach.

Table 2.1: Economic Deprivation and LBT - OLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty Rate	0.129			0.003		
	[0.391]			[0.355]		
Poverty Gap		0.049			0.011	
		[0.896]			[0.225]	
Median Gap			0.032			0.003
			[0.892]			[0.647]
Population (Log)	-62.607**	-64.543**	-64.510**	-0.334	-0.331	-0.367
	[0.017]	[0.013]	[0.015]	[0.175]	[0.165]	[0.133]
Share Foreigners	0.109	0.119	0.119	0.009	0.009	0.009
	[0.820]	[0.804]	[0.805]	[0.176]	[0.163]	[0.165]
Dependency Ratio	0.321	0.307	0.306	0.007*	0.007*	0.007
	[0.146]	[0.162]	[0.161]	[0.092]	[0.085]	[0.104]
Mean Income	-0.002	-0.002*	-0.002*	0.000	0.000	0.000
	[0.237]	[0.062]	[0.060]	[0.212]	[0.209]	[0.209]
Radical Right	3.405***	3.400***	3.399***	-0.012	-0.012	-0.013
	[0.007]	[0.008]	[0.007]	[0.307]	[0.327]	[0.302]
Radical Left	-1.008**	-1.025**	-1.026**	0.003	0.003	0.002
	[0.037]	[0.035]	[0.034]	[0.713]	[0.699]	[0.749]
Income Tax Revenue (Log)	0.013	-0.139	-0.121	0.176***	0.173***	0.173***
	[0.997]	[0.972]	[0.976]	[0.000]	[0.000]	[0.000]
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	414.95	414.95	414.95	6.03	6.03	6.03
R^2	0.34	0.34	0.34	0.48	0.48	0.48
N	2214	2214	2214	2222	2222	2222

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in %. Revenue variables are logged per capita.

2.6 Results

2.6.1 Local Taxation

We first address the question how economic deprivation in German city districts affects local taxation. We concentrate on the effect on LBT and the property tax B in our main analysis, as these are the more important tax instruments in terms of revenue collection.[14] Both the effect on the local scaling factor of the respective tax rate and the revenue is reported. We distinguish between two sample periods: a full sample from 1993 to 2016 and a restricted sample from 1993 to 2006. The restricted sample corresponds to the period for which the spending data is also available.

Table 2.2: Economic Deprivation and LBT - 2SLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty Rate	3.734** [0.037]			0.051** [0.023]		
Poverty Gap		5.453** [0.038]			0.115*** [0.005]	
Median Gap			2.936* [0.093]			0.055** [0.040]
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	414.95	414.95	414.95	6.03	6.03	6.03
N	2214	2214	2214	2222	2222	2222
Cragg-Donald	46.08	85.72	73.77	47.40	85.19	72.94
Kleibergen-Paap	13.01	35.68	31.23	13.33	35.20	30.73

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the the 10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in %. Revenue variables are logged per capita.

Full Sample: 1993-2016

For the full sample period from 1993 to 2016, OLS results for the relationship between economic deprivation and the LBT are presented in Table 2.1. For all three measures of economic deprivation, there is a positive effect on both the local scaling factor of the LBT rate (see Columns 1, 2, and 3) and the tax revenue (see Columns 4, 5, and 6).[15] However, this effect is not statistically significant at the 10% level. Moreover, OLS is likely to produce biased estimates (see Section 2.5.1).

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[14]Results for the property tax A are reported in the Appendix.

[15]Note that tax revenue is in log per capita.

Table 2.2 therefore shows 2SLS estimates for the relationship between economic depri-
vation and the LBT. For all three measures of economic deprivation, there is a positive
effect on the local scaling factor of the LBT rate. For instance, a one percentage point
increase in the poverty rate, leads to an increase in the local scaling factor of the LBT
rate of 3.7 percentage points, which translates into an increase of the average local scaling
factor of the LBT rate by approximately 1% (see Column 1). An increase in the poverty
gap has the largest economic effect: a one percentage point increase in the poverty gap
causes the local scaling factor of the LBT rate to increase by 5.4 percentage points (see
Column 2). The median gap has the lowest economic effect. In comparison to the OLS
estimates, the 2SLS coefficients are larger and statistically significant. This shows that
OLS estimates are indeed biased, for example due to an endogeneity problem, sorting, or
reverse causality. In this respect, our results are in-line with Boustan et al. (2013).

Table 2.3: Economic Deprivation and Property Tax B - OLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty Rate	0.287			0.001		
	[0.334]			[0.429]		
Poverty Gap		0.086			0.002	
		[0.891]			[0.248]	
Median Gap			0.351			0.001
			[0.430]			[0.475]
Population (Log)	-5.469	-9.827	-7.156	-0.537***	-0.538***	-0.540***
	[0.922]	[0.861]	[0.899]	[0.000]	[0.000]	[0.000]
Share Foreigners	0.396	0.419	0.415	-0.001	-0.001	-0.001
	[0.699]	[0.685]	[0.687]	[0.521]	[0.535]	[0.533]
Dependency Ratio	0.327	0.294	0.308	-0.001	-0.001	-0.001
	[0.474]	[0.516]	[0.498]	[0.416]	[0.420]	[0.393]
Mean Income	-0.000	-0.002	-0.001	-0.000	-0.000	-0.000
	[0.820]	[0.499]	[0.774]	[0.650]	[0.664]	[0.653]
Radical Right	12.147***	12.136***	12.150***	0.019***	0.020***	0.020***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Radical Left	-1.750**	-1.790**	-1.769**	-0.005***	-0.005***	-0.005***
	[0.020]	[0.017]	[0.018]	[0.001]	[0.001]	[0.001]
Income Tax Revenue (Log)	-26.786***	-27.124***	-26.868***	-0.042***	-0.042***	-0.042***
	[0.007]	[0.006]	[0.007]	[0.009]	[0.008]	[0.008]
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	428.12	428.12	428.12	4.92	4.92	4.92
R^2	0.59	0.59	0.59	0.87	0.87	0.87
N	2215	2215	2215	2224	2224	2224

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the
10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the
effect on tax revenue. The poverty rate, the poverty gap and the median gap are in %. Revenue variables are logged per capita.

We report both the Cragg-Donald and the Kleibergen-Paap F statistics for all three measures of economic deprivation. As both exceed the respective critical values of the weak instrument test proposed by Stock and Yogo (2005),[16] we can reject the weak instrument assumption.

Table 2.4: Economic Deprivation and Property Tax B - 2SLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty Rate	0.889 [0.740]			0.003 [0.637]		
Poverty Gap		1.447 [0.726]			-0.000 [0.980]	
Median Gap			-1.355 [0.638]			-0.003 [0.723]
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	428.12	428.12	428.12	4.92	4.92	4.92
N	2215	2215	2215	2224	2224	2224
Cragg-Donald	47.61	86.45	74.76	47.23	85.12	72.76
Kleibergen-Paap	13.29	36.17	31.71	13.29	35.21	30.67

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in %. Revenue variables are logged per capita.

Economic deprivation not only leads to a higher local scaling factor of the LBT rate but also to a significant increase in the LBT tax revenue. The magnitude and the statistical significance on LBT revenue is similar for the poverty rate and the median gap: a one percentage point increase in either measure leads to a 0.05% increase in LBT revenue per capita. With an increase in revenue by 0.1%, the effect of a one percentage point increase in the poverty gap is even larger and the effect is statistically significant also at the 1% level.

Table 2.3 shows OLS estimates for the local scaling factor of the property tax B rate and revenue. The 2SLS estimates are displayed in Table 2.4. The point estimates suggest a positive relationship between economic deprivation and the property tax B. Again, the magnitude of the OLS results is downward-biased in comparison to the 2SLS estimates. However, even for the 2SLS estimates, we do not find a statistically significant effect of any of our economic deprivation measures on the local scaling factor of the property tax B rate or revenue.[17]

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[16]The critical values for the Stock-Yogo weak ID F-test are 16.38 (10% maximal IV size), 8.96 (15%), 6.66 (15%), and 5.53 (20%)

[17]Results for the local scaling factor of the property tax A rate and revenue are shown in Tables 2.B3

Table 2.5: Restricted Sample: Economic Deprivation and LBT - 2SLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty Rate	7.510**			0.042		
	[0.040]			[0.135]		
Poverty Gap		15.148**			0.164*	
		[0.024]			[0.064]	
Median Gap			7.774***			0.062**
			[0.001]			[0.043]
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	410.05	410.05	410.05	5.86	5.86	5.86
N	1204	1204	1204	1210	1210	1210
Cragg-Donald	15.67	16.74	34.85	15.69	17.22	35.94
Kleibergen-Paap	5.26	5.62	15.76	5.30	5.86	16.60

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10%/5%/1% level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in %. Revenue variables are logged per capita.

Based on the Cragg-Donald and the Kleibergen-Paap F statistics, we can reject the weak instrument assumption for this specification as well.

Restricted Sample: 1993-2006

In addition to the complete sample, we also show results for a restricted sample from 1993 to 2006. As data on spending is only available for this time period, restricting the sample in this way enables us to draw conclusions on how city districts' taxation and spending react to economic deprivation for the same time period.

Table 2.5 shows 2SLS estimates for the LBT.[18] The point estimates for all three measures of economic deprivation are again positive and statistically significant. In comparison to the full sample the magnitude of the point estimates has even increased, suggesting that the economic impact of economic deprivation on the local scaling factor of the LBT rate is even greater during the years 1993 to 2006. In-line with that, the LBT revenue increases, as well. However, this effect is only statistically significant for the poverty gap and the median gap.

The 2SLS results for the property tax B are summarized in Table 2.6.[19] Similarly to the results for the full sample the point estimates for all three measures of economic

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and 2.B4 in the Appendix.

[18] OLS estimates for the restricted sample for the LBT are shown in Table 2.B5 in the Appendix.

[19] OLS estimates for the restricted sample for the property tax B are shown in Table 2.B6 in the Appendix.

Table 2.6: Restricted Sample: Economic Deprivation and Property Tax B - 2SLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty Rate	2.161 [0.439]			0.007 [0.421]		
Poverty Gap		4.522 [0.446]			0.014 [0.441]	
Median Gap			3.362 [0.191]			0.010 [0.195]
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	404.48	404.48	404.48	4.80	4.80	4.80
N	1205	1205	1205	1211	1211	1211
Cragg-Donald	15.96	16.92	35.42	15.86	17.23	35.98
Kleibergen-Paap	5.34	5.70	15.97	5.30	5.86	16.61

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the the 10%/5%/1% level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in %. Revenue variables are logged per capita.

deprivation are positive both for the local scaling factor of the property tax B rate and revenue. However, the effects are not statistically significant at the 10% level.

2.6.2 Spending on Local Public Services

Next we turn to the expenditure side of the local budget and the question how economic deprivation affects how much city districts spend on certain tasks. Our measures for economic deprivation are again the poverty rate, the poverty gap, and the median gap. The time period under consideration is 1993-2006. Table 2.7 shows OLS results for the effect of economic deprivation on *Aggregate Spending* on voluntary and mandatory self-government tasks. For all three measures of economic deprivation, the point estimates are negative. However, the magnitude of the coefficients is low and hence, they are not economically relevant. Moreover, as outlined in Section 2.5.1, OLS estimates are likely to be biased.

Comparing the OLS results with the 2SLS results from Table 2.8, suggests that the OLS estimates are biased towards zero. The 2SLS estimates imply that there is a negative impact of economic deprivation on aggregate spending. For instance, aggregate spending in city districts is reduced by 0.123% when the poverty gap increases by one percentage point. This impact is also statistically significant at the 5% level. Only the negative effect of the poverty rate on aggregate spending is statistically insignificant.

Table 2.7: Economic Deprivation and Aggregate Spending on Public Services - OLS

	(1)	(2)	(3)
Poverty Rate	-0.004**		
	[0.041]		
Poverty Gap		-0.008*	
		[0.067]	
Median Gap			-0.005
			[0.122]
Population (Log)	-0.305	-0.279	-0.267
	[0.321]	[0.363]	[0.390]
Share Foreigners	0.002	0.002	0.002
	[0.620]	[0.656]	[0.676]
Dependency Ratio	-0.005**	-0.005**	-0.005**
	[0.012]	[0.013]	[0.015]
Mean Income	-0.000	0.000	-0.000
	[0.928]	[0.743]	[0.910]
Radical Right	0.001	0.001	0.000
	[0.929]	[0.921]	[0.965]
Radical Left	0.011***	0.012***	0.012***
	[0.008]	[0.006]	[0.005]
Income Tax Revenue (Log)	-0.220**	-0.215**	-0.221**
	[0.040]	[0.042]	[0.038]
Year FE	Yes	Yes	Yes
Mean dep. Variable	6.91	6.91	6.91
R^2	0.10	0.10	0.10
N	1203	1203	1203

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the the 10%/5%/1% level. The poverty rate, the poverty gap and the median gap are in %.

Table 2.8 shows how *aggregate* spending on voluntary and mandatory self-government tasks reacts to economic deprivation. However, it is also of interest how the individual categories are affected. Figure 2.5 therefore plots the 2SLS coefficients for all spending categories by measure of economic deprivation.[20]

Across all three measures of economic deprivation, the pattern for the effect on the categories of spending is fairly similar. For most categories, the effect of economic deprivation is not statistically significant at the 10% level. However, spending on welfare, schooling, and sport is reduced significantly when any of the three measures of economic deprivation increases by one percentage point. In addition, a one percentage point increase in the poverty gap (median gap)leads to a significant reduction in spending on culture (roads).

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[20]Tables 2.B7, 2.B8, and 2.B9 in the Appendix also summarize the 2SLS coefficients for the spending categories by measure of economic deprivation.

Table 2.8: Economic Deprivation and Aggregate Spending on Public Services - 2SLS

	(1)	(2)	(3)
Poverty Rate	-0.023		
	[0.146]		
Poverty Gap		-0.123**	
		[0.046]	
Median Gap			-0.053***
			[0.006]
Demographic Controls	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Mean dep. Variable	6.91	6.91	6.91
N	1202	1202	1202
Cragg-Donald	14.57	17.32	36.77
Kleibergen-Paap	4.84	5.85	17.07

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. The poverty rate, the poverty gap and the median gap are in %.

2.7   Discussion

What are the implications of our results for the effect of economic deprivation on redistribution? It seems that local politicians actually want to make the tax system more redistributive in response to greater economic deprivation: by increasing the respective local scaling factor they raise the LBT rate, a tax on businesses. While Fuest, Peichl and Siegloch (2018) show that the actual incidence of the LBT is shared between corporations and workers, it is unlikely that local politicians consider aspects such as actual tax incidence in their fiscal policy decisions. Instead, it seems more likely that the perception of the LBT as a tax on corporations is what drives policy makers. Thus, raising the LBT rate is likely to be viewed by the general public as a redistributive fiscal policy.

In addition to the positive effect on the LBT, our results reveal that policy makers do not make statistically significant changes to the property tax B rate in reaction to increasing economic deprivation. The incidence as well as the perception of the property tax are less straightforward than in case of the LBT. On the one hand, it is a tax on real estate, and in Germany higher income groups are more likely to own property (Dustmann et al.; 2018). On the other hand, as it is part of the apportionable service charges, landlords can pass it on to their tenants. Indeed, there is empirical evidence that in the long run tenants bear the burden of the property tax (Löffler and Siegloch; 2018). Thus, there is some degree of ambiguity in the perception of the property tax B incidence. This might be an explanation for the fact that policy makers hesitate to adjust the property tax B in reaction to increasing economic deprivation. Thus, our results for the effect of economic

Figure 2.5: Categories of Public Services

(a) Poverty Rate (%)

(b) Poverty Gap (%)

(c) Median Gap (%)
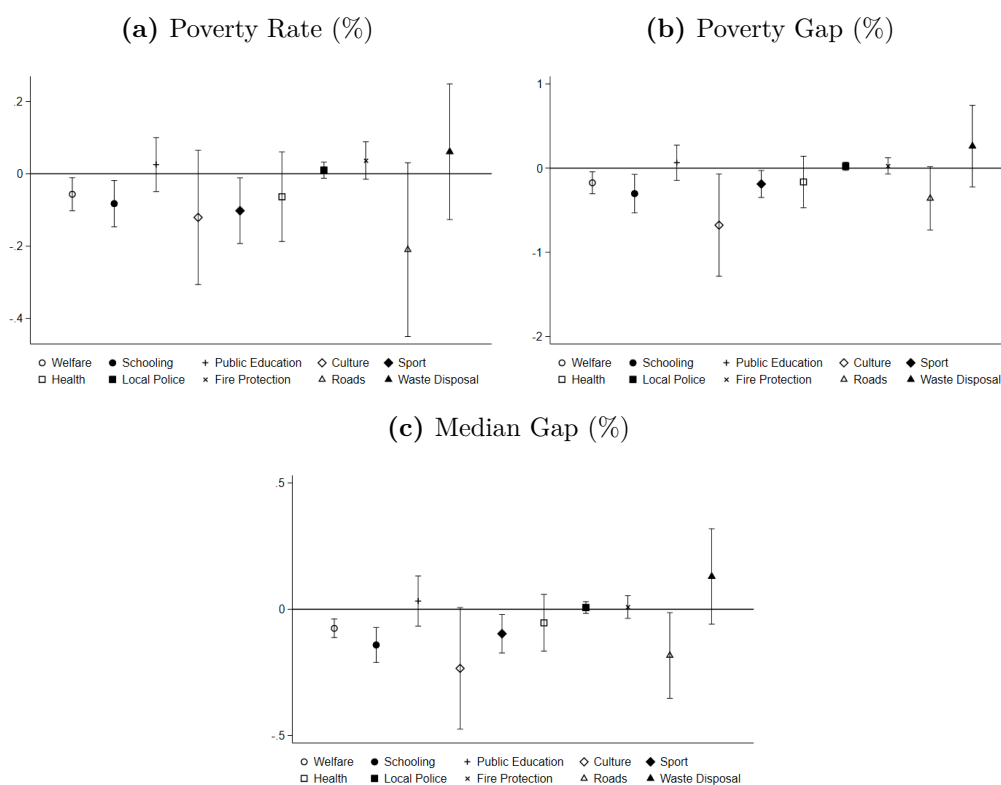
Notes: This figure plots the 2SLS coefficients and the 90% confidence interval for all spending categories by measure of economic deprivation.

deprivation on local taxes rather speak for the intention of local politicians to make the tax system more redistributive. These results are in-line with Borge and Rattsø (2004) and Chevalier et al. (2018).

In contrast, our results for spending on local public services suggest that increasing economic deprivation leads to less redistribution. The provision of most public services should benefit lower income groups most as they cannot afford private alternatives. Thus, a cut in spending on public services is most likely perceived as regressive fiscal policy. However, different categories of public services also target different income groups. For instance, cultural public services, like theater or opera, tend to be perceived as benefiting higher income groups. Other public services, such as welfare, tend to benefit the lower income groups. Moreover, with respect to some public services, such as schooling, it is unclear who benefits most. One the one hand, one could argue that lower income groups benefit most from a high-quality public provision of educational infrastructure. On the other hand, Hayo and Neumeier (2019) show that an investment in human capital is

preferred by those with higher education and income.

As we find that spending on welfare decreases in reaction to economic deprivation, this fiscal measure is more likely to harm lower income groups. The impact of the decrease in spending on schooling and sport facilities is more controversial. But here, too, it is more likely that higher income groups could replace the missing or low-quality public provision with private alternatives. Comparing our findings with the most closely related paper by Boustan et al. (2013), the results seem to contradict one another at first glance. Boustan et al. (2013) find that general expenditure is positively related to inequality. However, this overall effect in their paper is mainly driven by increases in the expenditure on police and fire protection, while the coefficients for welfare or health spending are statistically insignificant. Thus, the overall expenditure effect in Boustan et al. (2013) is driven by increases in spending on public services that are generally not considered redistributive. Similarly, the coefficients for police and fire protection in our data are positive, although statistically insignificant.

Finally, it also needs to be noted that our data on local spending does not include all possible forms of financing. For instance, we do not have data on public private partnerships (PPPs). Hence, we are not able to account for investments in public goods, e.g. the construction of a hospital, that are financed by means of PPPs.

Given any shortcoming in the data, an important difference to the previous literature on the sub-national level is that it focuses primarily on the US, which has a majority rule system. In such a setting the assumptions of the median voter theorem seem applicable. In contrast, Germany follows the principle of proportional voting. Thus, coalition governments are possible and frequent. This could affect our results since Freier and Odendahl (2012) show that in municipalities with a majority government, spending increases. They argue that majority governments can reach agreement more easily, whereas divided governments in a coalition cannot agree on public spending, and may therefore forgo it altogether. As coalition governments are frequent in German city districts, this inability to decide could be an explanation for the decrease in spending that we observe. Meanwhile, the decision on local taxation is more straightforward, as there are only two main tax instruments to decide on. Thus, our ambiguous results regarding the relationship between economic deprivation and redistribution for local taxation and spending could at least partly be due to the particular political system in Germany. We test this possibility by creating two indicator variables, which are proxies for unified and divided governments: *Unified*, which is equal to one if one party obtains more than 50% of the votes in the latest city council election, and *Divided*, which equals one if no party obtained more than 50% of the votes in the latest city council election. We interact both indicator variables with our economic deprivation measures. As the coefficients for both interaction terms

are very similar across the different specifications, we do not find any evidence to support the hypothesis that unified and divided governments make different fiscal policy decisions in reaction to economic deprivation.[21] This also holds for the subcategories of spending, especially for those categories for which we find statistically significant results. Thus, we do not find any evidence to support the hypothesis that divided governments drive our results.

As we find that tax revenue increases while spending goes down, an interesting question is whether policy makers decide to use the excess revenue to reduce debt instead. We therefore plan to extend our analysis to the question of how the level of debt in city districts is affected by economic deprivation. Furthermore, increasing economic deprivation could mean that the earmarked grants which the city districts receive are no longer sufficient, particularly for social welfare. Thus, local policy makers could be forced to contribute a part of their own budget to finance some of these tasks, which, strictly speaking, do not fall under their own sphere of influence. Due to data availability, we currently cannot test this possibility but plan to do so in the future.

Finally, an open question is how economic deprivation actually translates into fiscal policy measures. For this to happen, policy makers and their voters need to be aware of it. One possible mechanism of transmission is increasing segregation as income heterogeneity in a community increases. Previous literature shows that income inequality seems to be accompanied by spatial inequality, as different societal groups in the population are concentrated in different areas (see, for instance, Musterd et al. (2017) or Reardon and Bischoff (2011)). Helbig and Jähnen (2018) study segregation in 74 bigger cities in Germany. This is particularly relevant for our study, as we focus on city districts in Germany, which more or less include the cities they analyze. Indeed, Helbig and Jähnen (2018) find that spatial segregation by social groups has increased in these cities in their sample period from 2002 to 2014 as, for instance, a growing number of receivers of social benefits lives in certain parts of the city. Thus, increasing income inequality and economic deprivation seem to manifest spatially and become apparent in this way. Attentive policy makers could therefore be aware of them and could make corresponding political decisions.

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[21]2SLS results for the LBT, property tax B, and spending can be found in Tables 2.B10, 2.B11, and 2.B12 in the Appendix.

2.8   Robustness Tests

Since it is possible to argue that it takes some time for economic deprivation to translate into actual fiscal policy, we additionally run a lagged specification of Equation 2.3. More precisely, we lag all the economic deprivation measures and the corresponding instruments by one year but not the control variables. 2SLS estimates for the LBT are reported in Table 2.B14 in the Appendix.[22] We consider the full sample from 1993 to 2016. The point estimates for the local scaling factor of the LBT rate and the revenue are positive and statistically significant. Compared to our main specification (see Table 2.2), the effect on the local scaling factor of the LBT rate is even higher in the lagged specification. This could indicate that it does indeed take some time for policy makers to react to economic deprivation.

Table 2.B16 in the Appendix summarizes the lagged 2SLS effects for the property tax B.[23] Similar to our main specification, the effects are not statistically significant.

Finally, Table 2.B18 in the Appendix shows the effect of economic deprivation on aggregate spending for public services for the lagged 2SLS specification.[24] Similar to our main specification in Table 2.8, the point estimates suggest a negative relationship. Moreover, the magnitude and the statistical significance is very similar to those in our main specification. Hence, our results are robust to lagging the economic deprivation measures.

Moreover, to ensure that our results are not driven by outliers in the data, we winsorize our economic deprivation measures at the 1%- and 5%-level. The results for the local scaling factor of the LBT rate and revenue are robust to winsorizing at both levels as Tables 2.B19 and 2.B20 in the Appendix show. The same is true for the local scaling factor of the property tax B rate and revenue (see Tables 2.B21 and 2.B22 in the Appendix). Finally, Tables 2.B23 and 2.B24 in the Appendix show the winsorized results for aggregate spending. Again, both the magnitude of the point estimates and their statistical significance are very similar to our main results. This suggests that our results are not driven by outliers in the data.

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[22]OLS results are shown in Table 2.B13 in the Appendix.
[23]OLS results are shown in Table 2.B15 in the Appendix.
[24]OLS results are shown in Table 2.B17 in the Appendix.

2.9 Conclusion

Fiscal policy determines many important economic, social, and political outcomes. Understanding the factors that affect fiscal policy is therefore highly relevant. Income inequality is found to be such a factor (Meltzer and Richard; 1981). However, it remains controversial whether there is a positive or negative relationship between income inequality and redistributive fiscal policy measures. This paper aims to contribute to the literature by shedding light on this relationship.

First, in contrast to previous literature, we do not use a general measure of income inequality like the Gini coefficient, but focus on inequality driven by the lower bound of the income distribution. Specifically, we study the effect of three different measures of economic deprivation - the poverty gap, the poverty rate, and the median gap - on fiscal policy. This kind of inequality is of particular importance in the given context, as the impact of redistributive measures is even greater when people cannot afford private alternatives due to increasing economic deprivation.

Second, we exploit the specific institutional setting in Germany, which grants municipalities and districts a high degree of fiscal autonomy. They can set a number of different taxes and can decide on spending for several public services. We use this rich level in local variation to identify the effect of economic deprivation on fiscal policy. For our analysis we combine administrative panel data on the universe of German city districts with measures for economic deprivation from the German Microcensus.

Third, using instrumental variables estimation allows us to draw conclusions about the causal influence of economic deprivation on local fiscal policy in Germany. Our instrument predicts changes in regional economic deprivation through national income trends, which allows us to overcome confounding effects like mobility and spatial segregation.

We find that increasing economic deprivation causes local policy makers to increase the local business tax rate, while we do not find significant effects on local property taxes. Given that the local business tax is likely to be perceived as a progressive tax, whereas the perception of the property tax is more ambivalent, this seems to be an attempt to make the tax system more redistributive as economic deprivation increases. In contrast, aggregate spending on local public services is negatively affected by economic deprivation. This effect is driven in particular by cuts in spending on welfare and schooling. As lower income groups probably benefit most from these public services, our results suggest a negative relationship between economic deprivation and redistributive fiscal policies on the expenditure side.

A possible explanation for our ambiguous findings regarding the relationship between economic deprivation and redistribution may lie in the German political system, which makes coalition governments likely. Such divided governments might not be able to agree

on spending on public services, especially as the different parties might serve different voter groups. Decisions on taxation, meanwhile, are more straightforward as there are only two main tax instruments on the local level. We therefore test whether unified or divided city councils make different fiscal policy decision in reaction to increasing economic deprivation. However, our results do not provide any evidence to support this hypothesis.

Further explanations could be that city districts use the excess tax revenue to reduce debt or are forced to invest part of their budget to supplement earmarked grants as economic deprivation drives up social welfare costs. We plan to consider both options in future research.

In summary, our results suggest that economic deprivation is an important factor determining fiscal policy on the local level. However, there remains some ambiguity as to the nature of the effect, which warrants future research.

2.A   Appendix: Additional Figures

Figure 2.A1: Bartik Instrument Test: Poverty Gap

(a) Business Tax Rate   **(b)** Property Tax B Rate

(c) Aggregate Spending
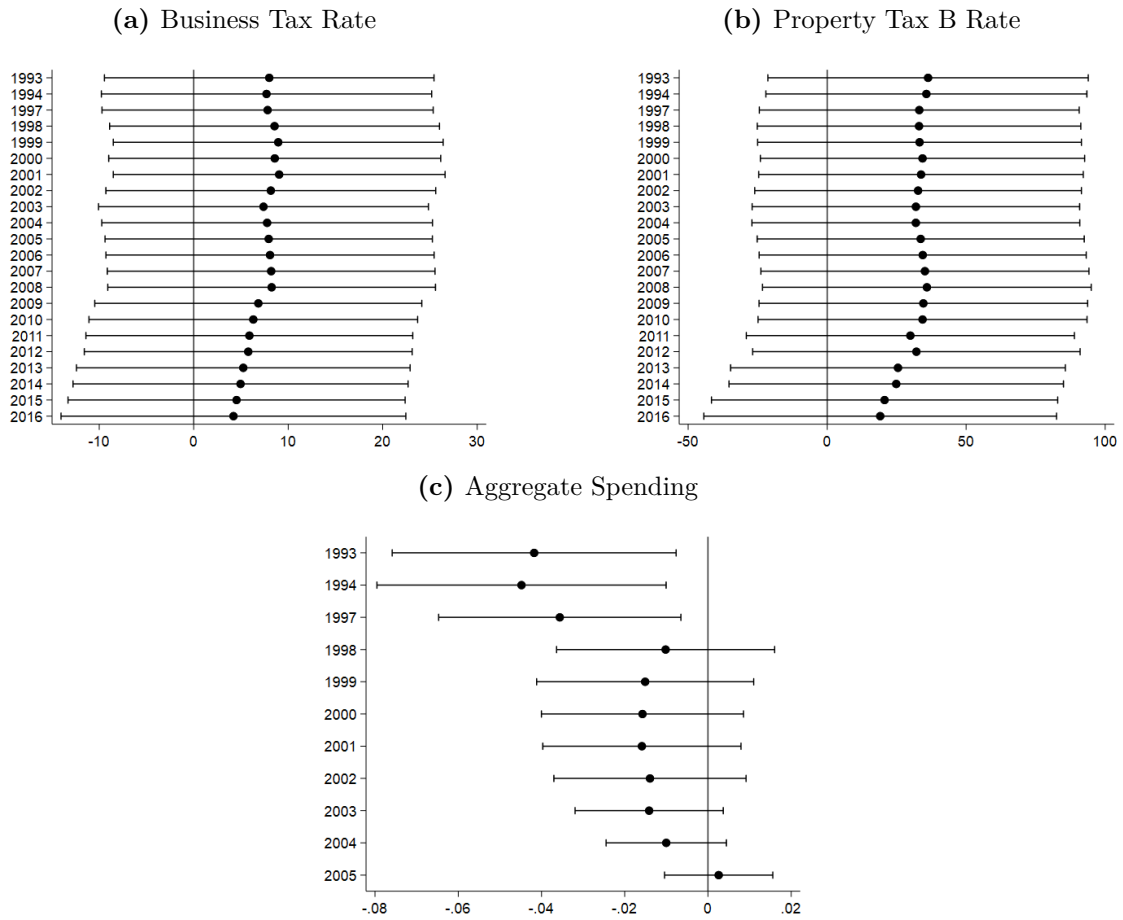
Notes: This figure plots coefficients and the 90% confidence intervals.

2.B   Appendix: Additional Tables

Table 2.B1: Summary Statistics

	1993 - 2016		1993		2006		2016	
	mean	sd	mean	sd	mean	sd	mean	sd
Poverty Rate	18.4	5.5	14.2	5.0	18.7	5.8	21.1	5.2
Poverty Gap	4.9	1.9	4.0	1.9	4.7	1.9	5.6	2.1
Median Gap	16.8	3.6	14.1	3.3	17.1	3.9	18.3	3.5
Business Tax Rate (Local Scaling Factor)	415.1	39.6	403.8	36.6	414.3	37.4	434.1	43.9
Property Tax B Rate (Local Scaling Factor)	428.3	71.5	365.7	44.9	424.8	50.5	501.6	101.7
Property Tax A Rate (Local Scaling Factor)	288.1	68.3	256.6	61.8	285.4	63.8	315.5	72.6
Business Tax Revenues	500.7	343.8	392.7	150.6	595.4	423.2	689.4	408.6
Property Tax B Revenues	142.5	40.5	98.5	20.5	143.0	32.9	181.7	42.9
Property Tax A Revenues	0.9	0.9	0.9	0.9	0.9	0.9	1.0	0.9
Aggr. Spending	1027.6	230.0	996.6	217.0	1051.3	237.0	.	.
Welfare	238.7	75.0	165.9	39.3	283.0	72.6	.	.
Culture	61.4	56.6	49.3	45.9	48.8	43.8	.	.
Schooling	204.8	77.5	176.3	60.6	233.7	86.2	.	.
Public Education	24.8	11.4	24.1	12.4	24.1	11.0	.	.
Sport	91.2	39.3	97.8	33.7	87.0	38.3	.	.
Health	24.7	23.0	33.6	31.2	23.4	19.3	.	.
Fire Protection	48.2	18.3	39.0	15.1	53.7	20.0	.	.
Local Police	56.0	14.3	49.0	12.5	61.7	14.0	.	.
Roads	103.3	53.8	92.6	37.3	110.0	59.4	.	.
Waste Disposal	174.5	151.3	269.0	144.6	125.9	135.6	.	.
Population (Log)	11.8	0.8	11.8	0.9	11.7	0.8	11.8	0.9
Share Foreigners	11.4	5.6	12.4	5.1	11.2	5.5	14.1	5.5
Dependency Ratio	32.8	3.2	31.5	3.0	32.8	3.2	33.3	3.2
Mean Income	1473.6	212.5	1446.2	105.1	1440.4	166.8	1590.5	163.0
Radical Right	3.0	2.1	3.0	1.7	2.2	1.1	6.4	1.5
Radical Left	7.8	8.5	0.4	0.2	9.1	8.1	9.5	7.1
Income Tax Revenue (Log)	5.6	0.4	5.8	0.1	5.5	0.4	5.6	0.7
Observations	2231		86		103		94	

₁Notes: Gini is measured in percent, income percentiles in 100 Euro. Dependency ratio and share of foreigners are measured in percent of the population. Tax revenues and spending categories are in per capita terms.

Table 2.B2: Construction of Expenditure Variables

Classification code in annual account data	Variable
11	Local Police
13	Fire Protection
2	Schooling
32 (321+323), 33 (331+332+333), 34	Culture
350 + 352 + 355	Public Education
451-458 + 460-468 + 47 + 470	Welfare
51 + 54	Health
55 + 56 + 57 + 58 + 59	Sport
63 + 65	Roads
70 + 72	Waste Disposal

Notes: The table shows how we mapped expenditure items that fall under the definition of voluntary and mandatory self-government tasks into our final variables based on their classification code in in the annual account data. In our definition of voluntary and mandatory self-government tasks we follow (Postlep; 1987).

Table 2.B3: Economic Deprivation and Property Tax A - OLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty Rate	-0.010			0.004*		
	[0.973]			[0.081]		
Poverty Gap		0.142			0.012**	
		[0.842]			[0.029]	
Median Gap			-0.151			0.007*
			[0.743]			[0.069]
Population (Log)	71.498*	72.391*	70.319*	-0.217	-0.231	-0.234
	[0.090]	[0.082]	[0.095]	[0.504]	[0.470]	[0.467]
Share Foreigners	-0.214	-0.215	-0.213	-0.009	-0.009	-0.009
	[0.830]	[0.828]	[0.831]	[0.112]	[0.117]	[0.119]
Dependency Ratio	0.337	0.345	0.330	0.002	0.002	0.002
	[0.551]	[0.542]	[0.557]	[0.542]	[0.535]	[0.602]
Mean Income	0.003	0.003	0.002	-0.000	-0.000	0.000
	[0.263]	[0.251]	[0.300]	[0.874]	[0.634]	[0.889]
Radical Right	6.003***	6.012***	5.996***	0.016**	0.017**	0.016**
	[0.000]	[0.000]	[0.000]	[0.046]	[0.041]	[0.047]
Radical Left	-0.268	-0.257	-0.277	-0.004	-0.004	-0.004
	[0.654]	[0.663]	[0.640]	[0.457]	[0.456]	[0.435]
Income Tax Revenue (Log)	12.749**	12.779**	12.646**	0.101***	0.097***	0.100***
	[0.018]	[0.018]	[0.019]	[0.001]	[0.002]	[0.002]
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	287.67	287.67	287.67	-0.43	-0.43	-0.43
R^2	0.41	0.41	0.41	0.06	0.06	0.06
N	2215	2215	2215	2217	2217	2217

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the the 10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in %. Revenue variables are logged per capita.

Table 2.B4: Economic Deprivation and Property Tax A - 2SLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty Rate	2.738			0.045**		
	[0.219]			[0.037]		
Poverty Gap		4.119			0.080*	
		[0.335]			[0.061]	
Median Gap			0.119			0.045
			[0.966]			[0.111]
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	287.67	287.67	287.67	-0.43	-0.43	-0.43
N	2215	2215	2215	2217	2217	2217
Cragg-Donald	47.61	86.45	74.76	48.02	85.00	73.81
Kleibergen-Paap	13.29	36.17	31.71	13.63	35.27	31.25

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in %. Revenue variables are logged per capita.

Table 2.B5: Restricted Sample: Economic Deprivation and LBT - OLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty Rate	0.292**			0.001		
	[0.049]			[0.786]		
Poverty Gap		0.496			0.003	
		[0.120]			[0.758]	
Median Gap			0.269			0.001
			[0.255]			[0.908]
Population (Log)	-79.683**	-83.349**	-84.720**	-0.455	-0.452	-0.477
	[0.032]	[0.025]	[0.027]	[0.368]	[0.375]	[0.345]
Share Foreigners	-0.102	-0.086	-0.080	0.008	0.008	0.008
	[0.815]	[0.846]	[0.857]	[0.275]	[0.275]	[0.276]
Dependency Ratio	-0.599**	-0.607**	-0.614**	0.003	0.003	0.003
	[0.013]	[0.012]	[0.011]	[0.610]	[0.609]	[0.616]
Mean Income	-0.001	-0.001**	-0.001**	0.000	0.000	0.000
	[0.138]	[0.033]	[0.039]	[0.141]	[0.166]	[0.158]
Radical Right	0.104	0.108	0.143	0.002	0.002	0.002
	[0.906]	[0.903]	[0.871]	[0.892]	[0.899]	[0.884]
Radical Left	-0.999**	-1.027**	-1.044**	-0.001	-0.001	-0.001
	[0.027]	[0.023]	[0.022]	[0.877]	[0.880]	[0.856]
Income Tax Revenue (Log)	14.711	14.445	14.809	0.244	0.242	0.244
	[0.201]	[0.209]	[0.198]	[0.149]	[0.152]	[0.148]
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	410.05	410.05	410.05	5.86	5.86	5.86
R^2	0.19	0.19	0.19	0.34	0.34	0.34
N	1204	1204	1204	1210	1210	1210

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in %. Revenue variables are logged per capita.

Table 2.B6: Restricted Sample: Economic Deprivation and Property Tax B - OLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty Rate	0.472			0.002**		
	[0.112]			[0.042]		
Poverty Gap		1.203*			0.006***	
		[0.084]			[0.001]	
Median Gap			0.701			0.003**
			[0.157]			[0.032]
Population (Log)	14.842	13.704	11.177	-0.532***	-0.519***	-0.539***
	[0.769]	[0.787]	[0.824]	[0.000]	[0.000]	[0.000]
Share Foreigners	-0.530	-0.504	-0.487	-0.001	-0.001	-0.001
	[0.681]	[0.694]	[0.704]	[0.760]	[0.784]	[0.804]
Dependency Ratio	-0.314	-0.317	-0.334	-0.003***	-0.003***	-0.003***
	[0.447]	[0.441]	[0.417]	[0.006]	[0.006]	[0.005]
Mean Income	-0.003*	-0.003*	-0.003*	-0.000***	-0.000***	-0.000***
	[0.054]	[0.067]	[0.054]	[0.001]	[0.005]	[0.001]
Radical Right	5.197***	5.163***	5.248***	0.009**	0.009**	0.009**
	[0.000]	[0.001]	[0.000]	[0.022]	[0.027]	[0.021]
Radical Left	-1.174*	-1.181*	-1.216*	-0.006***	-0.006***	-0.006***
	[0.092]	[0.092]	[0.084]	[0.001]	[0.002]	[0.001]
Income Tax Revenue (Log)	43.289***	42.652***	43.567***	0.024	0.021	0.025
	[0.005]	[0.006]	[0.004]	[0.551]	[0.606]	[0.532]
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	404.48	404.48	404.48	4.80	4.80	4.80
R^2	0.46	0.47	0.46	0.82	0.83	0.82
N	1205	1205	1205	1211	1211	1211

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in %. Revenue variables are logged per capita.

Table 2.B7: Poverty Rate and Spending on Categories of Public Services - 2SLS

	(1) Welfare	(2) Schooling	(3) Public Education	(4) Culture	(5) Sport	(6) Health	(7) Local Police	(8) Fire Protection	(9) Roads	(10) Waste Disposal
Poverty Rate	-0.056**	-0.083**	0.026	-0.121	-0.102*	-0.064	0.010	0.037	-0.210	0.061
	[0.042]	[0.034]	[0.572]	[0.286]	[0.065]	[0.399]	[0.452]	[0.242]	[0.151]	[0.593]
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	5.42	5.26	3.11	3.73	4.44	2.75	4.00	3.78	4.52	4.13
N	1202	1202	1178	1194	1202	1165	1202	1202	1201	1169

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. The poverty rate, the poverty gap and the median gap are in %.

Table 2.B8: Poverty Gap and Spending on Categories of Public Services - 2SLS

	(1) Welfare	(2) Schooling	(3) Public Education	(4) Culture	(5) Sport	(6) Health	(7) Local Police	(8) Fire Protection	(9) Roads	(10) Waste Disposal
Poverty Gap	-0.174**	-0.302**	0.064	-0.678*	-0.188*	-0.164	0.021	0.026	-0.360	0.261
	[0.028]	[0.030]	[0.612]	[0.066]	[0.053]	[0.376]	[0.472]	[0.659]	[0.116]	[0.376]
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	5.42	5.26	3.11	3.73	4.44	2.75	4.00	3.78	4.52	4.13
N	1202	1202	1178	1194	1202	1165	1202	1202	1201	1169

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. The poverty rate, the poverty gap and the median gap are in %.

Table 2.B9: Median Gap and Spending on Categories of Public Services - 2SLS

	(1) Welfare	(2) Schooling	(3) Public Education	(4) Culture	(5) Sport	(6) Health	(7) Local Police	(8) Fire Protection	(9) Roads	(10) Waste Disposal
Median Gap	-0.076***	-0.141***	0.032	-0.234	-0.097**	-0.054	0.007	0.009	-0.183*	0.130
	[0.001]	[0.001]	[0.593]	[0.110]	[0.036]	[0.430]	[0.639]	[0.749]	[0.075]	[0.259]
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	5.42	5.26	3.11	3.73	4.44	2.75	4.00	3.78	4.52	4.13
N	1202	1202	1178	1194	1202	1165	1202	1202	1201	1169

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. The poverty rate, the poverty gap and the median gap are in %.

Table 2.B10: Divided Government: Economic Deprivation and LBT - 2SLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty Rate * Unified	3.067**			0.038***		
	[0.023]			[0.008]		
Poverty Rate * Divided	2.373*			0.042***		
	[0.066]			[0.001]		
Poverty Gap * Unified		8.410***			0.092**	
		[0.007]			[0.021]	
Poverty Gap * Divided		5.612**			0.106***	
		[0.043]			[0.002]	
Median Gap * Unified			2.429			0.044*
			[0.247]			[0.052]
Median Gap * Divided			1.890			0.059***
			[0.402]			[0.008]
Controls Interacted	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	415.00	415.00	415.00	6.03	6.03	6.03
N	2217	2217	2217	2225	2225	2225

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in %.

Table 2.B11: Divided Government: Economic Deprivation and Property Tax B - 2SLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty Rate * Unified	2.373			0.012***		
	[0.166]			[0.006]		
Poverty Rate * Divided	1.515			0.010**		
	[0.343]			[0.020]		
Poverty Gap * Unified		5.767			0.027**	
		[0.197]			[0.014]	
Poverty Gap * Divided		1.606			0.016	
		[0.655]			[0.110]	
Median Gap * Unified			2.932			0.016**
			[0.261]			[0.013]
Median Gap * Divided			1.229			0.013**
			[0.642]			[0.043]
Controls Interacted	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	428.14	428.14	428.14	4.92	4.92	4.92
N	2218	2218	2218	2227	2227	2227

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in %.

Table 2.B12: Divided Government: Economic Deprivation and Aggregate Spending on
Public Services - 2SLS

	(1)	(2)	(3)
Poverty Rate * Unified	-0.030		
	[0.121]		
Poverty Rate * Divided	-0.026*		
	[0.065]		
Poverty Gap * Unified		-0.194**	
		[0.016]	
Poverty Gap * Divided		-0.146**	
		[0.025]	
Median Gap * Unified			-0.077***
			[0.004]
Median Gap * Divided			-0.070***
			[0.001]
Controls Interacted	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Mean dep. Variable	6.91	6.91	6.91
N	1202	1202	1202

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the
10 %/5 %/1 % level. The poverty rate, the poverty gap and the median gap are in %.

Table 2.B13: Lagged: Economic Deprivation and LBT - OLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
L.Poverty Rate	0.098			0.004		
	[0.536]			[0.235]		
L.Poverty Gap		0.336			0.009	
		[0.410]			[0.296]	
L.Median Gap			0.111			0.002
			[0.656]			[0.665]
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	415.90	415.90	415.90	6.05	6.05	6.05
R^2	0.34	0.34	0.34	0.47	0.47	0.47
N	2016	2016	2016	2023	2023	2023

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the
10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the
effect on tax revenue. The poverty rate, the poverty gap and the median gap are in %. The economic deprivation measures and the
corresponding instrument are lagged by one year.

Table 2.B14: Lagged: Economic Deprivation and LBT - 2SLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
L.Poverty Rate	3.480**			0.055**		
	[0.020]			[0.025]		
L.Poverty Gap		7.551**			0.139***	
		[0.018]			[0.007]	
L.Median Gap			4.523**			0.075*
			[0.033]			[0.053]
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	415.90	415.90	415.90	6.05	6.05	6.05
N	2015	2015	2015	2022	2022	2022

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in %. The economic deprivation measures and the corresponding instrument are lagged by one year.

Table 2.B15: Lagged: Economic Deprivation and Property Tax B - OLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
L.Poverty Rate	0.310			0.001		
	[0.355]			[0.469]		
L.Poverty Gap		0.069			0.000	
		[0.927]			[0.778]	
L.Median Gap			0.518			0.000
			[0.304]			[0.762]
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	432.32	432.32	432.32	4.95	4.95	4.95
R^2	0.57	0.57	0.57	0.84	0.84	0.84
N	2016	2016	2016	2025	2025	2025

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in %. The economic deprivation measures and the corresponding instrument are lagged by one year.

Table 2.B16: Lagged: Economic Deprivation and Property Tax B - 2SLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
L.Poverty Rate	3.020 [0.226]			0.001 [0.855]		
L.Poverty Gap		5.639 [0.262]			-0.009 [0.468]	
L.Median Gap			1.584 [0.675]			-0.009 [0.353]
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	432.32	432.32	432.32	4.95	4.95	4.95
N	2015	2015	2015	2024	2024	2024

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in %. The economic deprivation measures and the corresponding instrument are lagged by one year.

Table 2.B17: Lagged: Economic Deprivation and Spending on Public Services - OLS

	(1)	(2)	(3)
L.Poverty Rate	-0.001 [0.605]		
L.Poverty Gap		0.000 [1.000]	
L.Median Gap			0.000 [0.947]
Demographic Controls	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Mean dep. Variable	6.92	6.92	6.92
R^2	0.10	0.10	0.10
N	1010	1010	1010

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. The poverty rate, the poverty gap and the median gap are in %. The economic deprivation measures and the corresponding instrument are lagged by one year.

Table 2.B18: Lagged: Economic Deprivation and Aggregate Spending on Public Services - 2SLS

	(1)	(2)	(3)
L.Poverty Rate	-0.018		
	[0.347]		
L.Poverty Gap		-0.108*	
		[0.087]	
L.Median Gap			-0.049*
			[0.052]
Demographic Controls	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Mean dep. Variable	6.92	6.92	6.92
N	1009	1009	1009

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. The poverty rate, the poverty gap and the median gap are in %. The economic deprivation measures and the corresponding instrument are lagged by one year.

Table 2.B19: Winsorized 1%-level: Economic Deprivation and LBT - 2SLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty Rate	3.840**			0.053**		
	[0.037]			[0.025]		
Poverty Gap		5.763**			0.122***	
		[0.040]			[0.006]	
Median Gap			3.151			0.059**
			[0.103]			[0.047]
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	414.95	414.95	414.95	6.03	6.03	6.03
N	2214	2214	2214	2222	2222	2222

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in % and are winsorized at the 1%-level.

Table 2.B20: Winsorized 5%-level: Economic Deprivation and LBT - 2SLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty Rate	4.739**			0.065**		
	[0.042]			[0.032]		
Poverty Gap		6.811**			0.144***	
		[0.043]			[0.009]	
Median Gap			4.083			0.077*
			[0.113]			[0.064]
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	414.95	414.95	414.95	6.03	6.03	6.03
N	2214	2214	2214	2222	2222	2222

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in % and are winsorized at the 5%-level.

Table 2.B21: Winsorized 1%-level: Economic Deprivation and Property Tax B - 2SLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty Rate	0.914			0.003		
	[0.740]			[0.638]		
Poverty Gap		1.529			-0.000	
		[0.726]			[0.980]	
Median Gap			-1.453			-0.003
			[0.637]			[0.722]
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	428.12	428.12	428.12	4.92	4.92	4.92
N	2215	2215	2215	2224	2224	2224

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in % and winsorized at the 1%-level.

Table 2.B22: Winsorized 5%-level: Economic Deprivation and Property Tax B - 2SLS

	Tax Rate (Local Scaling Factor)			Tax Revenue		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty Rate	1.124			0.004		
	[0.740]			[0.638]		
Poverty Gap		1.803			-0.000	
		[0.725]			[0.981]	
Median Gap			-1.876			-0.003
			[0.636]			[0.721]
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. Variable	428.12	428.12	428.12	4.92	4.92	4.92
N	2215	2215	2215	2224	2224	2224

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. Columns 1, 2, and 3 show the effect on the local scaling factor of the tax rate. Columns 4, 5, and 6 show the effect on tax revenue. The poverty rate, the poverty gap and the median gap are in % and winsorized at the 5%-level.

Table 2.B23: Winsorized 1%-level: Economic Deprivation and Aggregate Spending on Public Services - 2SLS

	(1)	(2)	(3)
Poverty Rate	-0.025		
	[0.140]		
Poverty Gap		-0.137**	
		[0.033]	
Median Gap			-0.059***
			[0.006]
Demographic Controls	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Mean dep. Variable	6.91	6.91	6.91
N	1202	1202	1202

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. The poverty rate, the poverty gap and the median gap are in % and winsorized at the 1%-level.

Table 2.B24: Winsorized 5%-level: Economic Deprivation and Aggregate Spending on Public Services - 2SLS

	(1)	(2)	(3)
Poverty Rate	-0.035		
	[0.166]		
Poverty Gap		-0.177**	
		[0.038]	
Median Gap			-0.085**
			[0.015]
Demographic Controls	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes
Political Polarization	Yes	Yes	Yes
Income Tax	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Mean dep. Variable	6.91	6.91	6.91
N	1202	1202	1202

Notes: p-values in parentheses. Standard errors clustered at the city district level. */**/*** indicate significance at the 10 %/5 %/1 % level. The poverty rate, the poverty gap and the median gap are in % and winsorized at the 5%-level.

Chapter 3

How will Brexit affect Tax Competition and Tax Harmonization?
The Role of Discriminatory Taxation

3.1 Introduction

The decision of the United Kingdom (UK) to leave the European Union (EU) has given rise to a lively debate about the implications of this move for corporate tax competition in Europe. In this debate, two predictions are frequently mentioned. The first is that the UK might become a large tax haven on the EU's doorstep, trying to lure investment and jobs away from other countries by offering low tax rates. The second prediction is that, once the UK has left remaining EU member states will finally take steps toward more tax harmonization.

The trouble with the first prediction is that there is currently nothing that stops the UK from cutting taxes to attract investment, certainly not EU membership. There is no minimum corporate tax rate in the EU. In fact, the UK has repeatedly reduced its tax rate in recent years and currently has a much lower rate than comparable countries like Germany, France or Italy (see Figure 3.1).[1]

This chapter is based on joint work with Clemens Fuest (Fuest and Sultan; 2019)

[1]Of course, if Brexit raises trade costs, this may force the UK to cut corporate taxes in order to stay attractive as location for foreign direct investment. At least this would be the prediction of tax competition models with increasing returns to scale, see Haufler and Wooton (1999).

**Figure 3.1:** Corporate Tax Rates 1979-2017 (%)
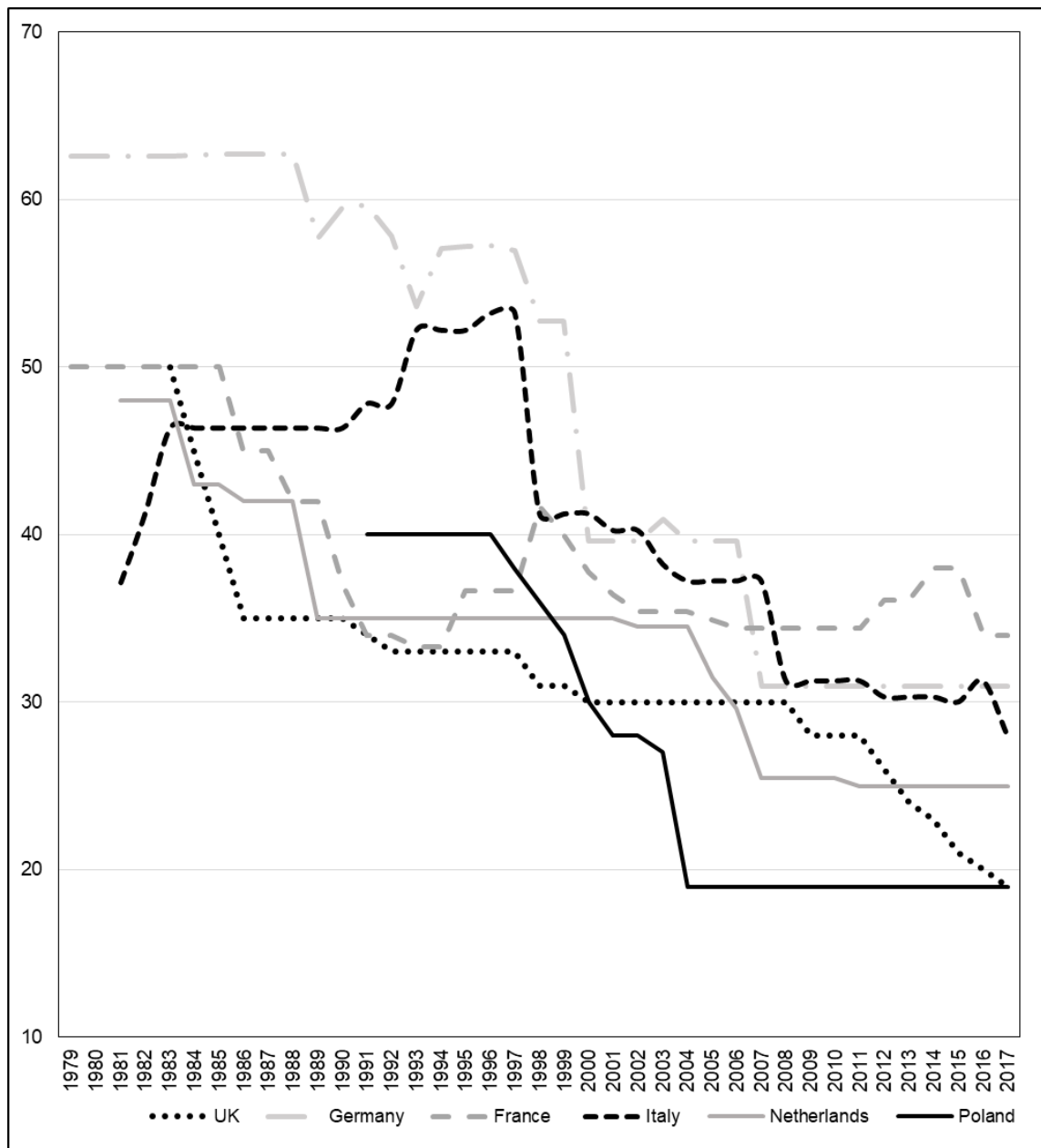
Notes: This figure shows the development in corporate tax rates across Europe for the time period 1979-2017. The data is retrieved from various sources such as EY Tax Reports and the OECD.

The trouble with the second prediction is that UK membership does not prevent the other EU member states to engage in tax harmonization if they want to do so. For

instance, they could use the instrument of enhanced cooperation, which allows a subset of EU member states to act jointly in different policy areas, including taxation. In addition, it is certainly correct that a change in EU membership may also change incentives for harmonization, but whether harmonization becomes more or less likely is far from trivial, as the analysis in this paper shows.

In this paper we focus on another aspect of the tax policy implications of Brexit: The fact that after leaving the EU, the UK will no longer be subject to EU state aid regulations established in Articles 107 to 108 of the Treaty on the Functioning of the European Union and the EU code of conduct for business taxation.[2] While these regulations do not prevent national governments from cutting headline tax rates, they do prevent them from offering different tax rates to different companies or sectors. This form of tax discrimination plays an important role in international tax policy. The relevance of the issue is exemplified by the recent opening of in-depth investigations by the EU Commission into the UK tax scheme for multinationals.[3] Since 2013 the UK allows for an exemption to its Controlled Foreign Company (CFC) rules, the *Group Financing Exemption*. It states that financing income received by the offshore subsidiary of a UK-based multinational from a foreign group company is exempted from reallocation to the UK. Hence, the UK parent company is able to pay little or no tax on the financing income generated via that scheme. The EU Commission has raised doubts as to whether the Group Financing Exemption complies with EU state aid rules, as an exemption to an anti-avoidance provision, such as CFC rules, can amount to a selective advantage for certain companies (EU Commission; 2017). If the UK leaves the EU and starts to target its corporate tax policy to specific firms or sectors even more aggressively, this may have consequences for corporate tax competition throughout Europe.[4]

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[2]This obviously also depends on the kind of Brexit deal the UK strikes with the EU. In case of a membership to the European Economic Area, the UK would have to replicate all EU state aid rules, while in case of a trade agreement, the impact on state aid regulations is less clear. For instance, in the CETA deal between the EU and Canada, there are no state aid rule provisions (Mor; 2017).

[3]Similarly, the EU Commission has concluded in 2016 that Ireland's tax benefits to Apple were illegal under EU state aid rules and recently has reached the same conclusion about tax benefits granted to Amazon in Luxembourg.

[4]The UK is not only subject to EU state aid regulations, but also to the WTO agreement on subsidies and countervailing mechanisms. However, the WTO system is considered much less stringent than the EU system (Mor; 2017). The most important difference between the two systems is that the WTO rules do not provide for any *ex ante* control, but only *ex post* control. This allows a member state to challenge another member state's subsidy before the WTO dispute settlement body, where it has to prove that the subsidy causes harm to itself. Instead, the EU system allows for both ex ante and ex post control. Thus, a member state planning to grant aid must notify the EU Commission as implementation of the measure depends on its clearance decision. Moreover, the EU Commission reviews aids and can request modification to previously approved aid schemes. This means that EU rules do not require an official complaint from

To investigate the implications of a policy shift towards tax discrimination we set up a simple three-country model with competition for internationally mobile capital. There are three sectors in the economy, each of which connects two of the three countries. This implies that each country hosts two sectors. Since these sectors are different, for example regarding size, countries have incentives to treat them differently for tax purposes. We start by analyzing a situation where the three countries form a 'union' that bans discrimination within countries. We compare this situation, in terms of tax competition outcomes and tax harmonization incentives within the union, to a situation where one of the three countries leaves the union and introduces discriminatory taxation. We also discuss how the stylized setup of our model links back to the situation in the EU.

Our analysis leads to three key results. First, the introduction of tax discrimination in one country, while the two other countries stick to uniform taxation but set their tax rates independently, leads to increasing tax heterogeneity regarding rates as well as revenue within the union. Second, if the two countries remaining in the union harmonize their tax rates, the introduction of tax discrimination in the third country redistributes tax revenue between the countries remaining in the union. The country with lower taxes before harmonization loses while the high tax country benefits. Third and most importantly, the incentives for tax harmonization among the countries remaining in the union decline as the third country introduces discriminatory taxation. This also holds if countries can use transfers to share gains from harmonization.

These results are important for several reasons. First, the fact that a country leaves the union does not generally intensify tax competition, it puts the low tax countries remaining in the union under stronger competitive pressure and eases pressures on high tax countries. What does that imply for tax policy? For instance, currently multinational firms use low tax jurisdictions like Ireland and the Netherlands to shift profits out of the United Kingdom. The relatively large domestic tax base as well as lower competitive pressures from other high tax countries like Germany or France, combined with EU rules against tax discrimination, prevent the UK from competing more aggressively with the Netherlands and Ireland. Our model predicts that, after leaving the EU, the UK will offer more attractive tax regimes targeted to those firms and sectors which currently shift profits to the Netherlands and Ireland. Second, the result that exit by one country makes harmonization among the other countries less attractive does not rely on the argument that tax coordination or harmonization becomes less effective if the number of participating countries falls and

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a member state to be triggered. Other differences between the two systems are that the default position in WTO rules is that subsidies not targeted at exports and/or imports substitution are generally allowed, while they are generally considered illegal under EU rules. Moreover, WTO rules do not apply domestically but only apply to international trade. For a more complete discussion of the two systems, see Ehlermann and Goyette (2006).

leakage effects grow (Konrad and Schjelderup; 1999): in our analysis, we compare situations where there is an outsider who does not participate. More generally, the intuition that the deviation of one country from a common tax policy norm – uniform tax rates – increases the willingness to harmonize taxes within the group of the remaining countries complying with the norm does not hold in our model. The opposite is true. This questions the widespread view that Brexit will make tax coordination or harmonization among the remaining EU countries more likely.

This paper is related to the literature on the economic consequences of Brexit.[5] This literature focuses on the implications of the rising trade costs for international trade and welfare in the United Kingdom and the EU (Aichele and Felbermayr; 2015; Dhingra et al.; 2017; Felbermayr et al.; 2017), on the effect of Brexit anticipated by financial markets (Davies and Studnicka; 2018; Schiereck et al.; 2016), on the effect on foreign direct investment (Dhingra et al.; 2016), and on consequences for the EU Budget (Felbermayr et al.; 2017), where the United Kingdom is a net contributor.

More specifically this paper is related to the literature on tax competition, coordination and preferential regimes, which started to develop in the late 80s as the internationalization of economic activity strained national tax structures (Zodrow and Mieszkowski; 1986).[6] Keen (2001) analyzes a situation of two symmetric countries and concludes that the introduction of preferential tax regimes can hamper tax competition. He assumes that both countries are able to discriminate. In contrast to Keen (2001), Janeba and Peters (1999) conclude that a uniform tax regime is preferable to tax discrimination as it allows governments to exploit the mobile tax base. In a model with home bias of investors, Haupt and Peters (2005) show that a partial restriction of preferential tax regimes increases tax revenues. Janeba and Smart (2003) show that the benefits of tax discrimination depend on the elasticity of the aggregate tax bases, reconciling the seemingly contradictory findings of Keen (2001) and Janeba and Peters (1999). In a model with imperfect competition and trade costs, Gaigné and Wooton (2011) compare the Nash equilibrium tax regimes. I trade costs are high enough, a uniform tax regime is preferred. Otherwise countries will choose to discriminate between mobile and immobile firms.

We also contribute to the literature on asymmetric tax competition. In a simple two-country model, Kanbur and Keen (1993) analyze how tax competition and coordination are affected by differences in country size. They conclude that the small country loses in terms of tax revenue from harmonization. But they also find that both countries benefit from the introduction of a minimum tax rate. Bucovetsky and Haufler (2007) extend Keen (2001)'s model to the case of asymmetric countries. They arrive at similar results

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[5]Sampson (2017) provides a very useful survey of this literature.
[6]See Keen and Konrad (2013) for a complete discussion of the literature.

as in the symmetric case. Similarly, Bucovetsky (1991) and Stöwhase (2005) analyze tax competition when countries differ in size.

Our analysis is also related to the literature on partial tax coordination, where only a subset of countries participates in tax coordination or harmonization agreements (Konrad and Schjelderup; 1999; Conconi et al.; 2008; Haufler and Lülfesmann; 2015).

Finally, this paper can be linked to the literature on tax havens (Desai et al.; 2004; Hong and Smart; 2010; Slemrod and Wilson; 2009; Johannesen; 2010; Becker and Fuest; 2012), where one of the central issues is whether tax havens mitigate tax competition among non-haven countries.

This paper extends the literature by analyzing the effects of tax discrimination on tax competition and harmonization in a three-country model. We consider a situation where some countries are allowed to discriminate while others are not and where only a subset of countries harmonizes taxes.

The rest of the paper is set up as follows. In Section 3.2 we present the model. In Section 3.3 we analyze various tax competition equilibria with and without discrimination. Section 3.4 focuses on tax harmonization and Section 3.5 concludes.

3.2   The Model

There are three countries A, B and C. Each country hosts two sectors. Each of these sectors links the country to one of the two other countries. The companies in all sectors operate under perfect competition in input and output markets. The number of firms per sector and the output price are normalized to unity. Each firm is endowed with a sector specific factor of production which is mobile across borders but not across sectors (Keen; 2001). We refer to this factor as capital. Sector A has its headquarter in country A and employs firm specific but internationally mobile capital either in country A or in its subsidiary in country B, sector B has its headquarter in country B and a subsidiary in country C. Accordingly, sector C resides in country C and has a subsidiary in A. The profit of the representative firm in sector A is given by

$$P_A = F_A^A(K_A) + F_A^B(S_A - K_A) - T_A K_A - T_{BA}(S_A - K_A), \qquad (3.1)$$

where $F_A^A(K_A) + F_A^B(S_A - K_A)$ is the firm's revenue generated in countries A and B, S_A is the firm's endowment of sector specific capital and K_A is investment in country A. Thus, the only choice the firm makes is to allocate its capital stock across the two production locations in countries A and B. T_A is the corporate tax country A levies per unit of capital employed in sector A and T_{BA} is the corporate tax on sector A investment levied by country B. This setup implies that countries use territorial corporate taxation.

Figure 3.2: The Model
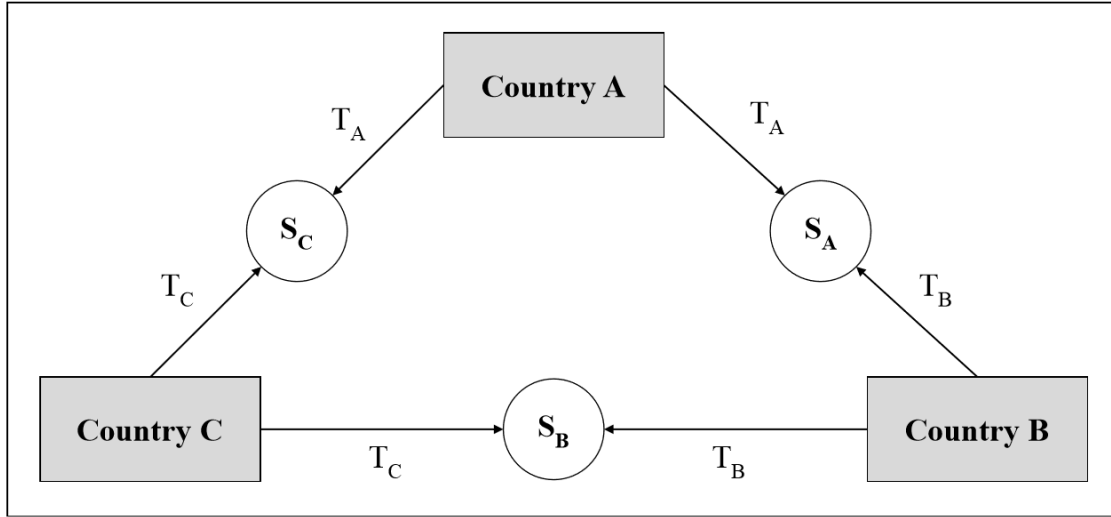
Notes: This figure shows how countries A, B and C are connected by the different sectors S_A, S_B and S_C
and the respective tax rates in the initial situation of non-discrimination in the model.

This is in line with existing tax regimes in the EU.[7] Profit maximizing investment is given
by the first order condition

$$F_A^{A'} - T_A = F_A^{B'} - T_{BA}. \tag{3.2}$$

Figure 3.2 visualizes how countries A, B and C are connected by the different sectors.

The tax revenue of country A is given by

$$R_A = T_A K_A + T_{AC}(S_C - K_C).$$

Accordingly, the tax revenue of countries B and C is given by

$$R_B = T_B K_B + T_{BA}(S_A - K_A)$$

and

$$R_C = T_C K_C + T_{CB}(S_B - K_B).$$

We consider two types of tax regimes for each country: uniform taxation, where both
sectors operating in the countries are taxed at the same rate, i.e. for country A this would
imply $T_A = T_{AC}$, and discriminatory taxation, where the two tax rates may differ.

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[7]With the exception of Greece, Ireland and Poland, all EU members currently follow the territorial
approach to corporate taxation. The UK switched from worldwide to territorial taxation in 2009 (Matheson
et al.; 2013).

Following Keen (2001), we assume that countries maximize their tax revenue and take the behavior of the companies and the tax rates of the other countries as given. The focus of our analysis is how the ability of countries to tax the two sectors differently affects the tax competition equilibria and the incentives for tax harmonization between two of the three countries.

To make the following analysis tractable, we follow Bucovetsky (1991) as well as Bucovetsky and Haufler (2007) in assuming that the production technology is quadratic:

$$F_i^j(K_i^j) = \alpha K_i^j - \frac{1}{4}(K_i^j)^2, \qquad i, j = A, B, C. \tag{3.3}$$

Note that the factor determining asymmetries between the three countries is the endowment of sector specific capital $S_i, i = A, B, C$.

The first-order condition for firm A's optimal investment can be written as

$$K_A = \frac{S_A}{2} + T_{BA} - T_A \tag{3.4}$$

and the tax revenue of country A is

$$R_A = T_A(\frac{S_A}{2} + T_{BA} - T_A) + T_{AC}(\frac{S_C}{2} + T_C - T_{AC}). \tag{3.5}$$

For B and C we get, accordingly

$$R_B = T_B(\frac{S_B}{2} + T_{CB} - T_B) + T_{BA}(\frac{S_A}{2} + T_A - T_{BA})$$

and

$$R_C = T_C(\frac{S_C}{2} + T_{AC} - T_C) + T_{CB}(\frac{S_B}{2} + T_B - T_{CB}).$$

3.3   Tax Competition

As mentioned above, we assume that governments use the available tax instruments to maximize their tax revenue. We consider two types of equilibria. First, we assume that all countries tax the two sectors uniformly, that is they operate under uniform taxation. Second, we consider the situation where A and B do not discriminate but C does.[8]

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[8]The equilibrium tax rates and revenues in the case where all countries discriminate can be found in the Appendix.

3.3.1 Uniform Taxation in All Countries

Denote the uniform tax rate of country j by T_j, j=A,B,C. Under uniform taxation in all countries the tax rate which maximizes tax revenue of country A, given the tax rates of B and C, is

$$4T_A - T_B - T_C = \frac{S_A + S_C}{2}.$$

Accordingly, the 'reaction functions' of countries B and C are given by

$$4T_B - T_A - T_C = \frac{S_A + S_B}{2}$$

and

$$4T_C - T_B - T_A = \frac{S_B + S_C}{2}.$$

These three equations can be solved for the equilibrium tax rates which emerge under a regime of uniform taxation in all countries. This leads to

$$T_A^u = \frac{1}{10}(2S_A + 2S_C + S_B), \tag{3.6}$$

$$T_B^u = \frac{1}{10}(2S_A + 2S_B + S_C), \tag{3.7}$$

$$T_C^u = \frac{1}{10}(2S_B + 2S_C + S_A). \tag{3.8}$$

Substituting into the revenue functions yields

$$R_j^u = 2(T_j^u)^2, \qquad j = A, B, C. \tag{3.9}$$

Unsurprisingly, the tax rates emerging in the tax competition equilibrium depend on the capital endowment of the different sectors. For instance, country A hosts activities of sectors A and C but not of B. This is why the capital endowments of sectors A and C play a greater role for its tax rate than the capital endowment of sector B. The latter is relevant for country A only indirectly because it determines the tax rates of the other countries, which do compete with country A for activities of sectors A and C.

3.3.2 Country C Discriminates While A and B Employ Uniform Taxation

We now consider the asymmetric situation where countries A and B levy uniform tax rates but country C discriminates between the two sectors. In this case, the tax competition equilibrium changes. Country C's 'tax reaction functions' are now given by $T_C = \frac{S_C}{4} + \frac{T_A}{2}$

and $T_{CB} = \frac{S_B}{4} + \frac{T_B}{2}$. Reaction functions for countries A and B are the same as in the previous subsection.

Inserting the reaction functions of country C and solving for the tax rates of A and B yields:

$$T_A^{udc} = \frac{1}{30}(7S_C + 6S_A + 2S_B), \tag{3.10}$$

where the superscript *udc* stands for the equilibrium in which A and B have uniform tax rates and country C discriminates. For country B we get

$$T_B^{udc} = \frac{1}{30}(7S_B + 6S_A + 2S_C). \tag{3.11}$$

With these results, one can now derive the equilibrium values of T_C as well as T_{CB}:

$$T_C^{udc} = \frac{1}{30}(S_B + 3S_A + 11S_C), \tag{3.12}$$

$$T_{CB}^{udc} = \frac{1}{30}(11S_B + 3S_A + S_C). \tag{3.13}$$

For tax revenue we get

$$R_j^{udc} = 2(T_j^{udc})^2, \qquad j = A, B \tag{3.14}$$

and

$$R_C^{udc} = (T_C^{udc})^2 + (T_{CB}^{udc})^2. \tag{3.15}$$

For the following analysis note that C will discriminate only if $S_B \neq S_C$.

3.3.3 How Does Discrimination in C Affect Equilibrium Tax Rates and Revenues?

A key issue is how the introduction of discrimination in country C affects tax rates and revenues in the other countries. In this section we compare the tax rates and revenues in countries A and B for the situation of uniform taxation in all countries to the scenario where only country C discriminates. For country A the relevant tax rates are given by equations (3.6) and (3.10). Subtraction leads to

$$T_A^u - T_A^{udc} = \frac{1}{30}(S_B - S_C). \tag{3.16}$$

For country B the same procedure leads to:

$$T_B^u - T_B^{udc} = \frac{1}{30}(S_C - S_B). \tag{3.17}$$

Note also that, under uniform taxation, the difference between the tax rates of A and B is given by[9]:

$$T_A^u - T_B^u = \frac{1}{10}(S_C - S_B). \tag{3.18}$$

These results may be summarized as

Result 1: Tax Rates

If $S_B \neq S_C$, the tax rates of countries A and B differ under the regime of uniform taxation within all countries. If country C switches to discriminatory taxation while A and B stick to uniform taxation, the tax rates of A and B always move into opposite directions. Thus, the tax rate difference between A and B increases.

How can this be explained? Consider for example the case where $S_C > S_B$, so that country A levies a higher tax rate than B: $T_A^u > T_B^u$, as the capital endowment that connects the respective country with C, is larger for country A than for country B. In this case, country C would like to levy a higher tax on sector C than on B but non-discrimination forces country C to choose a uniform rate. The equilibrium uniform tax rate in country C will be between the rates that would be optimal for sectors B and C. When country C switches to tax discrimination, it will compete less fiercely for sector C investment and more fiercely for sector B investment. This in turn will induce country A to increase its tax rate as well while country B responds by cutting its rate. The growing tax rate divergence between A and B mirrors the tax rate divergence within country C. Therefore discrimination in country C will drive the tax rates in countries A and B further apart.

As the previous results have shown, C will use its ability to discriminate if and only if $S_B \neq S_C$. For the following analysis we therefore make

Assumption 1: Sector Sizes

Sector C is strictly larger than sector B, that is, $S_C > S_B$.

It is important to note that, given the setup of our model, Assumption 1 has two implications. The first is that country A will want to levy higher taxes than country B. The second is that, if country C discriminates, it will want to tax sector C at a higher rate than B because rents in sector C are larger. This drives many of the results in the following sections. Theoretically it would be possible to use a different setup, where the tax rate difference between countries A and B is not determined by the size of the rents in the sector which links these two countries to country C. For instance, there could be a

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[9]Note also that under discriminatory taxation in C, the tax rate difference between A and B is given by $T_A^{udc} - T_B^{udc} = \frac{1}{6}(S_C - S_B)$, which is larger than the tax rate difference under uniform taxation

very profitable and immobile fourth sector operating in country B only, which drives up taxes there. We will come back to this issue in the discussion of the results.

Consider next the impact on tax revenue. It follows directly from equations (3.9) and (3.14) that the regime switch of country C will increase tax revenue for the high tax country and reduce the tax revenue for the low tax country among A and B:

$$R_A^{udc} - R_A^u = \frac{1}{450}[5(S_C^2 - S_B^2) + (12S_A + 8S_C)(S_C - S_B)], \qquad (3.19)$$

$$R_B^{udc} - R_B^u = \frac{1}{450}[5(S_B^2 - S_C^2) + (12S_A + 8S_B)(S_B - S_C)]. \qquad (3.20)$$

However, *aggregate* tax revenue of A and B will increase due to the regime switch as adding up equations (3.19) and (3.20) yields

$$(R_A^{udc} - R_A^u) + (R_B^{udc} - R_B^u) = \frac{4}{225}(S_B - S_C)^2 > 0. \qquad (3.21)$$

How about country C? It is straightforward to show that the following holds

$$R_C^{udc} - R_C^u = (T_C^{udc})^2 + (T_{CB}^{udc})^2 - 2(T_C^u)^2 = \frac{1}{18}(S_B - S_C)^2 > 0. \qquad (3.22)$$

We may thus state the following:

Result 2: Tax Revenue

Given Assumption 1, a switch from uniform to discriminatory taxation by country C, while A and B continue to levy uniform rates, increases the tax revenue of country C. Country A's tax revenue increases while B's tax revenue decreases. Aggregate tax revenue of A and B increases.

The impact on tax revenue in the different countries is a consequence of the tax rate changes explained in the context of Result 1. It is interesting to note that the low tax country, B, is negatively affected by the regime change in C, not the high tax country, A. The parameters that determine this result are the relative capital endowments of the different sectors. First of all the size of the respective sectors that connect countries A and B with country C matters as discussed above. Moreover, the capital endowment of sector A, which connects countries A and B, matters by scaling up the revenue effects of the regime switch. Thus, if country B is forced to lower its tax rate on sector B due to the competition from country C, uniform taxation will also force it to lower its tax rate on sector A, thereby aggravating country B's tax revenue loss due to the regime switch. On the other hand, country A's tax revenue will increase even further due to its higher uniform tax rate on the two sectors it hosts. The result that tax revenue of C as well as the aggregate tax revenue of A and B increases can be seen as an extension of Keen (2001)

and Bucovetsky and Haufler (2007). While these papers show that the introduction of tax discrimination in all countries mitigates tax competition with symmetric and asymmetric countries, our analysis shows that the introduction of tax discrimination in *one* country only also mitigates tax competition in terms of revenue raised in a setting with three asymmetric countries.

3.3.4 Discussion

How can these results contribute to our understanding of the tax policy implications of Brexit? Our results are derived from a highly stylized model, with restrictive assumptions. Three assumptions are particularly important: the first is that capital is mobile within but not across sectors. The second is that the supply of capital to the economy is fixed. The third is that each of the three sectors is present in only two countries.[10] Regarding the first assumption, it is important to note that introducing perfect capital mobility across sectors removes the direct bilateral links between countries hosting firms from the same sector. The only link between countries is the international capital market. In such a framework tax discrimination in C will still occur but it will no longer make the tax policies of countries A and B more heterogeneous. The link of country C to A and B, the interest rate in the international capital market, is the same for B and C. As a result, the tax rates in countries A and B will not diverge in reaction to tax discrimination in C. Moreover, aggregate investment and capital demand in C stay constant after the regime switch. Therefore the assumption of imperfect capital mobility across sectors is crucial.[11]

So how can the assumption of imperfect capital mobility across companies or sectors be justified? There is no doubt that multinational companies have firm-specific capital like managerial capacity which is mobile across countries at low costs but less easily transferred to other firms. Our model should be interpreted as referring to this type of capital, rather than capital in general. Moreover, links between entities of multinational firms but not across firms are created through tax planning and the shifting of book profits. Adding profit shifting to our model would make the analysis cumbersome but the economic forces at work would be similar. To prevent profit shifting to the low tax country B, country C would cut its tax in sectors where it is linked to that country and it would raise its tax in sectors linking it to the high tax country A.

The relevance of both firm specific capital moving across borders and international profit shifting depends strongly on how countries are linked through bilateral foreign direct investments. Note that bilateral links between the UK and other EU countries in the form

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[10]It has been shown in the literature that these assumptions may affect the impact of tax discrimination in models of tax competition, see in particular Janeba and Smart (2003) and Janeba and Peters (1999).

[11]A formal analysis of this case is available from the authors upon request.

of foreign direct investment differ considerably across EU countries (see Tables 3.B1 and 3.B2 in the Appendix). These asymmetries drive the results in our model.[12]

What are the implications for tax policy? For instance, it is plausible that multinational firms with a presence in both the UK and Ireland currently shift significant profits to Ireland. After leaving the EU, the UK will be able to offer these companies targeted tax regimes with a lower tax burden, intensifying tax competition between the UK and the low tax country Ireland. In contrast, for sectors or companies with a presence primarily in the UK and Germany, the UK may levy higher rates than today in reaction to the relatively high tax rates in Germany.

How about the assumption that the supply of capital to the economy is fixed? Relaxing this assumption seems most plausible in a framework where capital is mobile across sectors as well.[13] Empirically savings are not very elastic but the EU is integrated into world capital markets. As discussed previously introducing capital mobility across sectors and a common capital market changes the results of introducing tax discrimination. If we additionally assume that capital is perfectly mobile globally, the case for coordinating or harmonizing taxes vanishes, at least if it is driven by the desire to raise more revenue and if the EU or a subset of EU countries has no market power in the capital market. All of the results on tax harmonization, discussed in the next section, depend on the assumption that the remaining EU countries have at least some market power in capital markets. Again, if our model is interpreted as a model of competition for firm or sector specific resources, it is plausible to assume that these tax bases are at least not fully elastic in supply.

The third key assumption in our model is that each country hosts only two of the three sectors. This particular assumption is not critical but if each country hosted every sector, the countries would be symmetric in that respect. For our analysis we need sectoral relations to be asymmetric between countries. This is in line with asymmetries in bilateral

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[12]Blonigen and Piger (2014) show that important determinants of foreign direct investment include gravity variables, cultural distance factors and relative labor endowments, all of which motivate asymmetries in bilateral foreign direct investment relations.

[13]In the literature the effect of relaxing the assumption of fixed capital in Keen's (2001) model has been addressed by Janeba and Smart (2003). In their analysis they differentiate between a *tax base* effect and a *strategic* effect of a restriction on tax preferences. The base effect captures the implications of restrictions on tax preferences on the revenues from each tax base. Using the elasticities of tax bases they show that these restrictions tend to increase equilibrium tax revenue through the tax base effect. The strategic effect captures how the intensity of tax competition for the different tax bases between countries is affected by the restrictions. Depending on the net impact of the two effects, which tend to work in opposite directions, total revenues can either fall or rise with the restrictions. As the tax base elasticity is zero in our analysis, the tax base effect is absent, and any restriction on tax preferences will reduce revenues and hence increase tax competition through the strategic effect in our model. This corresponds to Proposition 2 in Janeba and Smart (2003).

economic relations among EU member states as illustrated by Tables 3.B1 and 3.B2 in the Appendix, which show that bilateral FDI stocks between the UK and selected partner countries in the EU vary substantially.

Finally, our analysis raises the more general question of why the union with its rule of uniform taxation was formed in the first place and why one country decides to leave. Clearly, in the model country C benefits from leaving as its tax revenue increases (see Result 2). This is because by leaving country C gains additional tax policy options and loses nothing. Of course, after leaving the EU many things will change for the UK, and explaining why the UK was an EU member state for so long and why the country leaves now is beyond the scope of this paper. It seem though that factors other than tax policy were key drivers of the referendum vote, in particular opposition to immigration (Goodwin and Milazzo; 2017; Sampson; 2017). Moreover, socioeconomic characteristics of the voting population such as low educational attainment, economic deprivation, unemployment and conservatism are associated with voting Leave in the referendum (Becker et al.; 2017; Alabrese et al.; 2018).[14] These findings corroborate the aggregate trend found by Goodwin and Heath (2016) that the feeling of socioeconomic marginalization and of being left behind were key drivers. In so far, analyzing tax policy implications of Brexit in a model where the exit decision is exogenous seems acceptable.

3.4   Tax Harmonization

What are the implications of introducing tax discrimination in country C for corporate tax harmonization? We consider tax harmonization of the following type: Countries A and B set a common tax rate to maximize the sum of their tax revenues.[15] The focus of our interest is whether the incentives for countries A and B to engage in tax harmonization change as a result of introducing tax discrimination in country C.

3.4.1   Tax Harmonization with Uniform Taxation in Country C

Assume that C levies a uniform tax rate and countries A and B choose the harmonized tax rate which maximizes their aggregate tax revenue. By focusing on the case where the harmonized tax rate is the one that maximizes aggregate tax revenue of A and B, we implicitly assume that side payments between the two countries are possible in order to

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[14]Exit polls by ICM/Guardian (2016) find that 'to take back powers from Brussels' and immigration were the main reason why people voted to leave the EU but also that these responses greatly vary by age, social class, voting intention, region and ethnic group.

[15]One could think of alternative forms of tax harmonization or tax coordination. For instance, countries A and B could bargain over the harmonized tax rate or they could coordinate their tax rates without harmonizing them.

reach an agreement on harmonization. In this Section we take this agreement as given, but will return to this issue in Sections 3.4.4 and 3.4.5. Moreover, both sides take the tax rate set by the other side as given. The tax rates emerging under these assumptions are given by

$$T_H^{huc} = \frac{1}{12}(4S_A + 3(S_B + S_C)) \tag{3.23}$$

and

$$T_C^{huc} = \frac{1}{12}(2S_A + 3(S_B + S_C)), \tag{3.24}$$

where T_H^{huc} is the harmonized tax rate levied by countries A and B, given that C has a uniform tax rate (T_C^{huc}).

Tax revenues are now given by

$$R_A^{huc} = (T_H^{huc})^2 + \frac{T_H^{huc}}{12}3(S_C - S_B), \tag{3.25}$$

$$R_B^{huc} = (T_H^{huc})^2 + \frac{T_H^{huc}}{12}3(S_C - S_B), \tag{3.26}$$

and

$$R_C^{huc} = 2(T_C^{huc})^2. \tag{3.27}$$

3.4.2   Tax Harmonization with Discriminatory Taxation in Country C

Consider next the equilibrium where country C discriminates. Here the emerging tax rate for A and B is:

$$T_H^{hdc} = \frac{1}{12}(4S_A + 3(S_B + S_C)). \tag{3.28}$$

The tax rates of country C are

$$T_C^{hdc} = \frac{1}{24}(4S_A + 6S_C + 3(S_B + S_C)) \tag{3.29}$$

and

$$T_{CB}^{hdc} = \frac{1}{24}(4S_A + 6S_B + 3(S_B + S_C)). \tag{3.30}$$

Note that the switch of C to tax discrimination leaves the harmonized tax rate of A and B unchanged, i.e. $T_H^{hdc} = T_H^{huc} \equiv T_H$. The reason is that C cuts its tax on sector B and increases its tax on sector C. The optimal response for A and B, given that they are not able to discriminate, is to do nothing.

For tax revenues, we get

$$R_A^{hdc} = \frac{T_H}{24}(8S_A + 12S_C + 3(S_C - S_B)), \tag{3.31}$$

$$R_B^{hdc} = \frac{T_H}{24}(8S_A + 12S_B + 3(S_B - S_C)) \tag{3.32}$$

and

$$R_C^{hdc} = (T_C^{hdc})^2 + (T_{CB}^{hdc})^2. \tag{3.33}$$

3.4.3 How Does the Regime Switch in C Affect Tax Revenues in the Presence of Tax Harmonization?

Analyzing the effect of the regime switch on tax revenue under harmonization for A and B separately, we get

$$R_A^{hdc} - R_A^{huc} = \frac{T_H}{8}(S_C - S_B) \tag{3.34}$$

and

$$R_B^{hdc} - R_B^{huc} = \frac{T_H}{8}(S_B - S_C). \tag{3.35}$$

Given Assumption 1, i.e. $S_C > S_B$, a switch to discrimination increases the tax rate and revenue of A and reduces those of B. The reason is that country C has incentives to increase its tax on the larger sector C and compete more aggressively for investment of the smaller sector B. This will increase investment and, hence, tax revenue in country A. The impact on country B is the opposite. Equations (3.34) and (3.35) also show that the aggregate tax revenue of countries A and B does not change. Discrimination in C only redistributes revenue from B to A.

What happens to the tax revenue in country C due to its switch to tax discrimination? Comparing tax revenue in the two equilibria for country C using equation (3.27) and (3.33) shows

$$R_C^{hdc} > R_C^{huc}. \tag{3.36}$$

This means that country C benefits in terms of tax revenue by discriminating. We again compare these results to the findings in Keen (2001). We extend on them not only by showing that introducing tax discrimination in one country only also mitigates tax competition (see Result 2) but that this also holds under tax harmonization in a subset of countries. While tax competition is lowered in the sense that aggregate tax revenue is increased, the distribution of these gains between the three countries in our model is highly asymmetric. While C and A gain revenue, B loses.[16]

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[16]Note also that $R_C^{hdc} > R_C^{udc}$. Thus, C gains more from discrimination if A and B set a harmonized tax rate instead of independently setting uniform tax rates. Proof is available from the authors upon request.

We summarize these findings in

Result 3: One-sided Tax Discrimination

If country C switches from uniform taxation to discriminatory taxation, and given that countries A and B levy a harmonized tax rate, countries A and C gain tax revenue while country B loses. Aggregate tax revenue of A, B and C increases.

3.4.4   How Does the Regime Switch in C Affect Incentives for Countries A and B to Harmonize Taxes?

If country C gives up the rule of uniform taxation and introduces discrimination, how does this affect the incentives for A and B to engage in corporate tax harmonization? To investigate this, we compare the gains from tax harmonization between A and B under uniform taxation in all countries to the gains from tax harmonization between A and B in the case where C discriminates. It continues to hold that C does not enter the tax harmonization agreement in either case. We start by considering countries A and B individually.

Formally, by defining

$$\Delta R_j^{uc} = R_j^{huc} - R_j^{u}, \qquad j = A, B, \tag{3.37}$$

and

$$\Delta R_j^{dc} = R_j^{hdc} - R_j^{udc}, \qquad j = A, B, \tag{3.38}$$

the differences in the revenue gains from harmonization for countries A and B can be expressed as

$$\Delta R_A^{dc} - \Delta R_A^{uc} = \frac{1}{72}[0.17(S_C^2 - S_B^2) + (1.08S_A + 1.28S_B)(S_C - S_B)] \tag{3.39}$$

and

$$\Delta R_B^{dc} - \Delta R_B^{uc} = \frac{1}{72}[0.17(S_B^2 - S_C^2) + (1.08S_A + 1.28S_C)(S_B - S_C)]. \tag{3.40}$$

As one would expect, the harmonization gains are equal with and without discrimination (i.e. the right hand side of equations (3.39) and (3.40) is equal to zero) if $S_B = S_C$, as tax discrimination by C is irrelevant in that case.[17]

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[17]Note also that the tax revenue effect of harmonization for A and B is positive both if C levies a uniform tax rate (i.e. $\Delta R_j^{uc} > 0$) and if C discriminates (i.e. $\Delta R_j^{dc} > 0$) as long as they are not too asymmetric regarding sector sizes. The proof is available from the authors upon request.

But in the presence of asymmetries, things are different. If $S_C > S_B$, as stated in Assumption 1, the switch to discrimination in C increases the benefit of country A ($\Delta R_A^{dc} - \Delta R_A^{uc} > 0$) but decreases B's gains from harmonization ($\Delta R_B^{dc} - \Delta R_B^{uc} < 0$). The reason is that discrimination increases the heterogeneity in tax policy between countries A and B (see Result 1), where, given Assumption 1, the initial tax rate in A is higher than in B . This means that, in the absence of harmonization between A and B, B's tax rate declines even further and A's tax rate increases even further as a result of discrimination in C. In this situation harmonization means that B increases its tax rate considerably while A actually lowers it. This has the effect that B loses capital to the other two countries. For country A the opposite holds, which is why A gains more from harmonization in the regime where C discriminates.

This can be summarized as

Result 4: Benefits from Tax Harmonization for Individual Countries
Introducing tax discrimination in C reduces the benefits from tax harmonization in country B and increases the benefits from harmonization in country A.

As mentioned at the beginning of this Section, choosing the harmonized tax rate which maximizes joint tax revenue only makes sense if transfers across countries are possible. Result 4 implies that country A would have to pay higher transfers to B than in a situation wihtout discrimination in C.

3.4.5   Are Transfers Between A and B Sufficient to Maintain Incentives for Tax Harmonization?

Clearly the impact of the regime change in C on tax harmonization incentives in A and B depends on its effect on *aggregate* tax revenue gains of A and B. To maintain incentives it would suffice that the regime change at least does not reduce those gains.

Under uniform taxation in all countries the aggregate tax revenue gains from tax harmonization for countries A and B are given by

$$\Delta R_{A+B}^{uc} = (R_A^{huc} + R_B^{huc}) - (R_A^u + R_B^u). \tag{3.41}$$

In case country C discriminates, the gains from tax harmonization for A and B are given by

$$\Delta R_{A+B}^{dc} = (R_A^{hdc} + R_B^{hdc}) - (R_A^{udc} + R_B^{udc}). \tag{3.42}$$

Combining equations (3.41) and (3.42) and using the revenue equations for the different equilibria yields

$$\Delta R^{uc}_{A+B} - \Delta R^{dc}_{A+B} = \frac{4}{225}(S_B - S_C)^2 > 0. \tag{3.43}$$

We may thus state the following:

Result 5: Aggregate Benefits from Tax Harmonization for Countries
A and B

If country C switches from uniform taxation to discriminatory taxation and countries A and B levy a uniform tax rate, the aggregate revenue gains for countries A and B from harmonizing their tax rates decline.

What is the economic explanation for Result 5? The reason that discrimination undermines the incentives for A and B to harmonize their taxes is that, in the absence of harmonization, the introduction of tax discrimination in C mitigates tax competition in the sense that the tax revenue gain in country A exceeds the loss in country B, so that aggregate tax revenue in A and B increases. In the equilibrium with harmonized taxes, revenue for countries A and B is the same with and without discrimination in C. Therefore, the revenue gain that can be achieved through harmonization is smaller if C discriminates.[18]

The result that the incentives for countries A and B to harmonize their corporate tax rates will be smaller if country C discriminates compared to a situation where country C levies a uniform tax is noteworthy for two reasons. First, it does not rely on the fact that tax coordination or harmonization becomes less attractive if the number of participating countries falls, because leakage effects grow. We compare situations where there is always an outsider, country C, who does not participate. Second, the intuition that the deviation of one country from a common tax policy norm – uniform tax rates – increases the willingness to harmonize taxes in the group of remaining countries complying with the norm, does not hold in our model. Instead, the opposite is true.

3.4.6　Discussion

Our result that tax rate harmonization between countries A and B becomes less attractive once C discriminates rests on the assumption that tax revenue is the decisive factor in driving tax harmonization. The fear that tax competition may erode corporate tax revenue is a widely discussed motive for governments to support tax coordination or harmonization initiatives (Bond et al.; 2000). Also, it seems unlikely that countries will agree

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[18]More formally, from the derivation of equations (3.34) and (3.35), we know that the regime switch of C, given that A and B have harmonized their tax rate, does not change the aggregate tax revenue of A and B. Thus, it follows from equations (3.41), (3.42) and (3.43) that $(R^{udc}_A + R^{udc}_B) > (R^u_A + R^u_B)$ must hold.

to tax harmonization if they lose revenue as a result. However, we do not claim that tax revenue is the only factor driving tax harmonization or the resistance against it. There is an extensive literature exploring the welfare effects of tax coordination and harmonization (Edwards and Keen; 1996; Zodrow; 2003), but the literature investigating why harmonization happens or does not happen is limited. Exceptions are Cardarelli et al. (2002) and Itaya et al. (2008) who study the endogenous formation of groups of countries harmonizing their taxes in repeated interactions models. They show that two factors are important: the degree of regional asymmetries and preferences for public goods. Osterloh and Heinemann (2013) show empirically that ideology and as well as individual and national interests of Members of the European Parliament are important factors in determining their stance towards a European agreement on minimum tax rates. They also emphasize the critical role of sovereignty and the shift of competencies regarding any agreement on corporate tax harmonization. Thus, the emphasis on tax revenues in our model abstracts from a number of relevant factors.

Finally, our analysis also abstracts from the question of why countries form tax unions and join or leave harmonization agreements. Country C's decision to exit the union is exogenously determined in our model. We think this is a reasonable assumption given that Brexit was not primarily motivated by tax policy but rather by opposition to immigration (Goodwin and Milazzo; 2017; Sampson; 2017) and the feeling of socioeconomic marginalization by part of the voting population (Becker et al.; 2017; Alabrese et al.; 2018; Goodwin and Heath; 2016).

3.5 Conclusion

In this paper we use a stylized model of tax competition to study the implications of a regime change where one of three countries gives up a common tax policy norm, the norm of levying uniform tax rates on all sectors in the economy. It turns out that this regime change increases tax policy heterogeneity among the remaining countries which continue to comply with the norm. We also show that the regime change discourages tax harmonization among the remaining countries. If applied to the case of the EU, these results question the widely held view that the remaining EU countries will be more likely to take steps towards enhanced corporate tax harmonization. Interestingly, our model predicts that the low tax countries will be those who lose as the country leaving the union introduces discriminatory taxation. This holds both for the case of harmonization and non-harmonization in the union.

Of course, the results of our theoretical analysis should be seen in the context of the highly stylized nature of the model from which they have been derived. Most importantly,

our model implies that tax rate differences within the union are determined by size differences in the sectors which link countries A and B to country C, which leaves the union. These size differences are critical for the result that tax discrimination in C increases tax rate heterogeneity within the union. A related and important assumption is that of sector specific capital. With perfect capital mobility across sectors the direct bilateral links between countries hosting firms from the same sector play no role anymore and the key link between countries is the international capital market with a uniform interest rate. While tax discrimination in C would still occur in such a setting, it would no longer make the tax policies of countries A and B more heterogeneous because the link of country C to A and B, the interest rate in the international capital market, is the same. This implies that the optimal tax policies of the two countries would no longer diverge in reaction to discrimination in C. Moreover, the finding that incentives for tax harmonization are reduced is driven by the effect of tax discrimination on the intensity of tax competition. Essentially, this is the effect first described in Keen (2001). As mentioned in the Introduction and in Section 3.3.4, the literature on tax discrimination has pointed out that models can be constructed where tax discrimination intensifies tax competition (Janeba and Smart; 2003; Janeba and Peters; 1999). Therefore our model should not be interpreted as showing *generally* that tax harmonization becomes less likely as a result of one country introducing discrimination. Rather, the contribution of our analysis is to show that there are economic forces pushing into this direction.

There are more limitations of our model which should be considered. Countries focus on revenue maximization but the analysis abstracts from issues like profit shifting and other types of tax avoidance. Adding profit shifting to the analysis would create another channel where different bilateral links between countries would play a role. We would therefore expect our analysis to hold in such a setting.

Brexit will not just free the United Kingdom from the restrictions of EU state aid, it will also most likely increase trade costs between the United Kingdom and the rest of the EU. This will have an impact on location decisions and, hence, on corporate tax competition. This aspect is entirely absent from the model considered here, which focuses on tax discrimination.

While we have discussed the implications of relaxing some of the assumptions made in our analysis, it is clear that more work needs to be done to improve our understanding of how Brexit will change corporate tax competition in Europe and beyond.

3.A   Appendix: Discriminatory Taxation in All Countries

If all countries discriminate, country A's tax rates are given by

$$T_A = \frac{S_A}{4} + \frac{T_{BA}}{2}$$

and

$$T_{AC} = \frac{S_C}{4} + \frac{T_C}{2}.$$

Accordingly, the tax rates for B and C are given by
$T_B = \frac{S_B}{4} + \frac{T_{BC}}{2}, T_{BA} = \frac{S_A}{4} + \frac{T_A}{2}$ and $T_C = \frac{S_C}{4} + \frac{T_{AC}}{2}, T_{CB} = \frac{S_B}{4} + \frac{T_B}{2}.$

Inserting leads to:
$T_A^d = \frac{S_A}{2}; T_B^d = \frac{S_B}{2}; T_C^d = \frac{S_C}{2}$
$T_{AC}^d = \frac{S_C}{2}; T_{BA}^d = \frac{S_A}{2}; T_{CB}^d = \frac{S_B}{2}.$

For tax revenue we get
$R_A^d = \frac{S_A^2 + S_C^2}{4};$
$R_B^d = \frac{S_B^2 + S_A^2}{4};$
$R_C^d = \frac{S_C^2 + S_B^2}{4}.$

Under discrimination each country can target its tax rates to the two specific sectors, which explains why the capital endowments of other sectors play no role. Put differently, the number of policy instruments for each country equals the number of targets. Interestingly, the result of discrimination by all, is a form of tax harmonization by sectors.

3.B   Appendix: Additional Tables

Table 3.B1: UK Inward FDI Stock by Partner Country

(in Million US $)

Country	2008	2009	2010	2011	2012	2013	2014	2015	2016
Denmark	12,692	8,677	4,916	5,743	9,312	6,378	10,518	11,135	7,823
France	75,570	119,103	105,261	94,793	123,733	116,369	118,638	109,081	81,818
Germany	104,605	109,789	80,897	77,471	102,801	83,491	78,145	82,783	81,710
Ireland	14,875	17,632	14,909	17,048	22,006	17,732	21,431	23,520	14,128
Italy	9,123	7,862	1,179	3,260	13,909	13,406	6,925	9,520	6,223
Netherlands	200,080	179,589	175,784	196,153	227,295	246,487	274,585	231,568	260,914
Poland	111	26	41	152	n.a.	171	161	584	186
Spain	16,295	43,377	52,198	62,612	66,172	88,127	71,279	39,566	37,244

Notes: The table shows UK inward FDI stock by partner country for the time period 2008-2016 in Million US $. The data has been retrieved from the OECD.

Table 3.B2: UK Outward FDI Stock by Partner Country

(in Million US $)

Country	2008	2009	2010	2011	2012	2013	2014	2015	2016
Denmark	15,757	11,330	12,430	11,071	12,759	12,533	10,939	9,388	8,641
France	61,389	64,697	85,850	82,754	85,864	62,218	59,663	87,971	88,253
Germany	44,536	43,563	35,185	31,706	30,262	34,346	37,061	36,426	25,709
Ireland	44,505	56,461	64,069	69,418	65,364	52,069	52,679	52,712	67,093
Italy	15,648	18,933	18,740	18,124	24,246	16,971	15,495	15,793	14,983
Netherlands	233,499	236,519	227,425	224,406	199,114	202,564	185,281	175,969	149,963
Poland	4,248	6,030	5,709	5,876	6,374	7,879	7,778	8,789	7,621
Spain	54,092	52,724	56,099	57,807	58,287	21,670	25,356	62,361	68,820

Notes: The table shows UK outward FDI stock by partner country for the time period 2008-2016 in Million US $. The data has been retrieved from the OECD.

Chapter 4

What Drives Chinese Overseas M&A Investment?
Evidence From Micro Data

4.1 Introduction

In recent years, Chinese investors have significantly increased their foreign investment activities especially in the form of cross-border mergers and acquisitions (M&As). In many European countries and the United States, Chinese M&As arouse suspicion.[1] Critics claim that Chinese acquisitions lead to undesirable technology transfers, that Chinese acquirers enjoy unfair advantages because of government subsidies, or that their acquisitions are motivated strategically with the objective to gain market dominance or to increase China's political influence in the target countries.[2] There are also concerns that Chinese takeovers may have adverse consequences for the employees of the target firms. According to a recent survey by the ifo Institute, for example, economists from 74% of the countries surveyed are more critical of foreign investment from China than of that from other countries (ifo Institute; 2019). At the same time, there are legitimate reasons for the surge in Chinese investment abroad. China has invested the revenue from its trade surplus primarily in US

This chapter is based on joint work with Clemens Fuest, Felix Hugger, and Jing Xing (Fuest, Hugger, Sultan and Xing; 2019)

[1]Of course investment is not the only channel via which China spreads its outreach, but it can also have a strong impact via trade as for instance Autor et al. (2016) and Hsieh and Ossa (2016) have shown.

[2]In a recent report the European Commission has analyzed challenges and prospects in the relationship with China (EU Commission; 2019).

government bonds for a long time. Thus, diversifying its foreign investment through cross-border M&As seems perfectly rational. For many Chinese firms, foreign acquisitions are also a way to ensure access to customers or key suppliers, in particular of raw materials.[3] This debate on Chinese foreign M&A activities, however, is mostly based on speculations and anecdotes. Despite a growing number of studies on Chinese overseas investment, there is surprisingly little systematic evidence on whether Chinese cross-border M&As differ from investment coming from other countries. As Buckley et al. (2018) conclude, "the degree to which China is truly different from the advanced economies, or from other emerging economies, is worthy of debate" (Buckley et al.; 2018, p. 18).

The objective of this paper is to fill this gap by examining whether Chinese cross-border acquisitions differ from foreign acquisitions of investors from other countries. Specifically, we address three questions: First, how do Chinese acquirer select their targets? Second, do Chinese acquirer pay more in a takeover? Third, what are the effects of a Chinese acquisition on the target firm?

Unlike previous quantitative studies that mostly use aggregate data, we use an extensive deal-level dataset, which allows us to analyze not only how target country-level factors but also how target-level characteristics affect cross-border transactions. Instead of focusing solely on Chinese cross-border M&As, we use a logit model to directly compare the drivers of Chinese foreign acquisitions with those of non-Chinese investments. Such an approach has not been carried out before on a comparable scale.

On the question of target selection, we find that Chinese overseas M&As are distinct from non-Chinese cross-border investments in several dimensions. For example, Chinese acquirers seem less concerned about market size, and conduct more deals in tax havens and offshore financial centers. Chinese companies also tend to acquire targets with lower profitability, more assets, higher levels of debt, and more patents. In contrast, we do not find that target countries' institutional qualities, such as political stability and the rule of law, play a different role in determining Chinese cross-border acquisitions than they do for non-Chinese investors.

At the same time, we uncover rich differences between private and state-owned Chinese acquirers (SOEs), which appear to be attracted to distinctive sets of target-level and target-country-level characteristics. For example, Chinese SOEs and private investors are differently affected by recent Chinese government policies like the *Belt and Road Initiative* (BRI) or *Made in China 2025*. While we find these government initiatives to have a significant impact on the location and industry choices of Chinese SOEs' overseas acquisitions, they do not appear to influence those of Chinese private investors.

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[3]For Japanese firms Raff et al. (2018) provide evidence of the link between financial frictions and foreign direct investment.

Whether the prevailing critical attitude towards Chinese cross-border M&As is justifiable also requires a comparison between Chinese SOEs and state-owned investors from elsewhere, which has not been conducted before. Based on a sample of government-led acquisitions, we find no significant difference between Chinese and non-Chinese SOEs in seeking natural resources or industry diversification. Chinese SOEs, however, do tend to purchase larger targets with poorer financial performance.

Another critique of Chinese acquirers is that they systematically outbid other investors, benefiting from government support. This in turn may distort the global M&A market, with potentially negative economic effects on the target countries. However, such criticism is largely based on anecdotal evidence. To shed light on this debate we use our detailed deal-level dataset to examine whether Chinese investors pay higher acquisition prices. In contrast to the view that Chinese companies pay premiums to win bids, we do not find that Chinese investors pay more for similar target firms compared to non-Chinese acquirers.

Finally, a key question is whether Chinese acquisitions have a different impact on the development of target firms or their employees. We find that post-merger performance differs in two dimensions. First, growth in capital productivity is lower in the short-run, mostly because Chinese acquirers seem to invest more after the takeover. Second, the growth of employee compensation is higher. Since most Chinese foreign acquisitions happened relatively recently, the number of cases where we can observe post-merger performance is limited, though.

The rest of the paper is structured as follows. In the next section, we provide a brief review of the related literature. In Section 4.3, we describe our data and sample construction. We provide descriptive statistics in Section 4.4 and present our empirical analysis in Sections 4.5, 4.6, and 4.7. Section 4.8 concludes.

4.2   Related Literature

This paper is related to several strands of literature. Our first research question focuses on the determinants of Chinese acquisitions. Determinants for foreign investment have been the topic of extensive academic research, resulting in a long list of factors suggested to influence cross-border M&As. This list includes access to resources and technology, entry to the foreign market, diversification, geographic proximity, bilateral trade, and relative valuation in currencies and stock markets (Erel et al.; 2012); domestic financial market development (Di Giovanni; 2005); accounting disclosure and accounting standards (Erel et al.; 2012; Rossi and Volpin; 2004); shareholder protection and corporate governance (Kim and Lu; 2013); cultural differences (Ahern et al.; 2012); and social attitudes (Dinc and Erel; 2013); target and home countries' institutional qualities such as political stability

and the rule of law (Brockman et al.; 2013; Erel et al.; 2012; Jandik and Kali; 2009); regulatory arbitrage (Alimov; 2015; Karolyi and Taboada; 2015); and taxes (Huizinga and Voget; 2007). Some argue that no new theoretical framework is needed to explain Chinese foreign investment (Alon et al.; 2009; Rugman; 2010). That is, the same list of economic and institutional factors should similarly affect both Chinese and non-Chinese cross-border M&As. Others believe that Chinese multinational companies are distinctive (Boisot and Meyer; 2008; Buckley et al.; 2009; Child and Rodrigues; 2005). Despite these contrasting views, there is a lack of systematic quantitative analysis that compares the patterns of Chinese cross-border M&As with those of other investors.

Following the seminal work by Buckley et al. (2009), there has been an increasing number of studies on the determinants of Chinese outward greenfield investment (e.g., Kolstad and Wiig 2012; Lu et al. 2011). However, there are much fewer quantitative studies on Chinese cross-border M&A activities, even though they have become the main form of outward foreign direct investment by Chinese firms and may be influenced by a different set of factors (Buckley et al.; 2016). Using aggregate-level data for the years 1985-2011, Buckley et al. (2016) examine country-level factors that affect the location and scale of Chinese overseas M&As. According to Buckley et al. (2016), institutional rather than economic factors make cross-border acquisitions by companies from emerging market distinct. Consistent with this view, they find that Chinese acquirers are attracted to countries with higher risks, proxied by a poorer record of law and order. Nonetheless, Buckley et al. (2016) do not compare Chinese acquirers with investors from other countries. Therefore, their study does not answer the question whether economic and institutional features affect Chinese investors differently.

One distinct feature of Chinese investors is that many of them have close government ties. Studies on Chinese foreign greenfield investment have compared SOEs with private firms (Amighini et al.; 2013; Duanmu; 2012; Ramasamy et al.; 2012; Luo et al.; 2017), and uncovered significant differences. For example, SOEs are less concerned about political risk in the target country, less market oriented, and more resource-seeking in their investment decisions. Nevertheless, the contrast between state-owned and private acquirers may not be China-specific. Comparing cross-border M&As by both private and government-led acquirers around the globe, Karolyi and Liao (2017) find that pursuing targets in countries with rich natural resources and a high potential to diversify industrial structures are common features of government-backed acquirers in general. Therefore, to answer the question whether Chinese cross-border M&As are different, it is important to compare Chinese state-owned acquirers with other government-led acquirers. To the best of our knowledge, we are the first to conduct such a comparison on a larger scale.

Furthermore, this paper is related to the literature on determinants of the take-over

premium, and the post-take-over performance of target firms. In particular, our study is closely related to studies on the impact of acquisitions on targets' productivity (e.g., Arnold and Javorcik 2009; Bertrand and Zitouna 2008; Fons-Rosen et al. 2013; Schiffbauer et al. 2017. There also exists a small number of studies that examine the effects of foreign acquisitions in China on firms' productivity (Wang and Wang; 2015; Zhang et al.; 2019). Nevertheless, to our best knowledge, we are the first to investigate the impact of recent Chinese foreign takeovers on the performance of the target firms.

4.3   Data and Sample Construction

We combine data from a number of sources to construct our samples. To obtain deal-level information, we use Bureau van Dijk's (BvD) Zephyr database, which contains information on worldwide M&A transactions. We only consider cross-border deals and exclude deals with multiple acquirers.[4] If a firm acquires several targets in one deal, we regard each acquirer-target pair as a single transaction. Our full sample contains 157,985 completed cross-border M&A deals of which 3,283 are conducted by Chinese investors. The sample period covers the years from 2002 to 2017, and, therefore, only includes years after China's accession to the World Trade Organization. We differentiate between three types of acquirers: Chinese private acquirers, Chinese state-owned acquirers, and non-Chinese acquirers. A Chinese acquirer is regarded as an SOE if its global ultimate owner is state-owned or state-controlled. Following this definition, 1,279 deals of our full sample are conducted by Chinese SOEs.

As financial information about the targets and acquirers is limited in Zephyr, we use the BvD Orbis database to obtain financial and ownership information on both targets and acquirers. Each target or acquirer is assigned a unique identifier by BvD, which allows us to link Zephyr with Orbis. Table 4.A2 in the Appendix provides the definitions of target-level variables in our analyses.

For analyses and estimations, we restrict the sample further. First, we focus on deals where at least 50% of the target's shares are purchased and exclude deals where the target country is unknown. We drop deals where the target reports non-positive total assets, turnover, or employees, and where the target's intangible fixed assets are greater than its total assets. To ensure comparability, we only keep targets acquired once during our sample period. This leaves us with a total of 72,056 deals, of which 1,168 are conducted

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[4]Cross-border deals are those where the target and the acquirer are located in different countries. To identify the origin of the acquirer, we use the location of the acquirer's global ultimate owner. Frequently, the location of the acquirer is the same as that of its global ultimate owner, but in some cases relying on the location of the acquirer would be misleading due to intricate ownership structures.

by Chinese private investors and 732 by Chinese SOEs (see Table 4.1).

Table 4.1: Summary Statistics by Acquirer Type Based on the
Estimation Sample

	Number of Deals		Mean deal value (in million EUR)	Mean deal value (in million EUR)
Acquirer type	All	With deal value		
CN private	1,168	577	159.0	20.0
CN SOE	732	391	394.3	54.6
Non-CN	70,156	21,038	263.8	32.1
Total	72,056	22,006	263.4	23.0

We augment the deal-level data with target country-level variables from various sources. From the World Bank's World Development Indicators (WDI), we obtain general macroeconomic variables like GDP, exchange rate, population, and the share of resource rents in GDP. We use CEPII data for a population weighted distance measure from the target country to China. The UN Comtrade database provides us with the trade volume between the target country and China. To identify tax havens, we rely on an OECD definition[5]. To measure institutional quality, we use the World Bank's World Governance Indicators (WGI) on the rule of law, control of corruption, political stability, and regulatory strength. Table 4.A1 in the Appendix provides more details about the definitions and sources for these country-level variables.

4.4 Descriptive Statistics

This section first provides some general descriptive statistics on Chinese and non-Chinese cross-border acquisitions. We then describe target country-level characteristics in more detail before looking at target firm-level characteristics.

Figure 4.1 shows the number and value of cross-border acquisitions by type of acquirer for 2002-2017.[6] For non-Chinese acquisitions (Panel A), we observe a peak in both number of deals and transaction volume around 2006-2007 and a significant drop during the 2008-2009 financial crisis. There is a gradual recovery of global cross-border M&As since around 2012. These patterns are consistent with observations made elsewhere (EU Commission; 2018). Panel B shows that the evolution of Chinese cross-border acquisitions is rather different from the global trend. In particular, there was a spike in the number of Chinese cross-border transactions in 2008, in contrast to the dip in global M&A activities. Over

[5]See Table 4.A3 in the Appendix for the list of countries.
[6]Deals are assigned to years depending on their date of completion.

the full sample period, both the number and the volume of Chinese overseas acquisitions increase substantially.

Figure 4.1: Number and Value of Deals by Deal Category

Notes: This figure shows the development of the number and value of deals over the sample period 2002-2017. We differentiate between different deal categories depending on the nature of the acquirer: non-Chinese acquirers (Panel A) and Chinese acquirers (Panel B). We furthermore decompose Chinese acquirers into private firms (Panel C) and SOEs (Panel D). The number of deals is reported in the right hand scale and the value of deals (in constant billion Euros with base year 2015) is reported in the left hand side.

In Panels C and D, we distinguish between Chinese private and state-owned acquirers. This reveals that while there are fewer acquisitions by Chinese state-owned acquirers, they tend to conduct larger deals. The spike in the number of deals in 2008 shown in Panel B is largely driven by the activities of Chinese private acquirers. For both private and state-owned acquirers, the total value of acquisitions rises sharply over time. Since 2011, however, the rise is more prominent for acquisitions by SOEs. Table 4.1 summarizes the number of deals, and the mean and median deal values by acquirer types. Deal value data is available for about half of the Chinese transactions and for about one third of non-Chinese acquisitions. Table 4.1 confirms that Chinese SOEs are involved in larger

deals than other acquirers, which is reflected by substantially higher mean and median deal values. In contrast, Chinese private acquirers tend to conduct deals of similar sizes to non-Chinese acquirers.

Figure 4.2: Types of Deals by Percentage of Shares Acquired
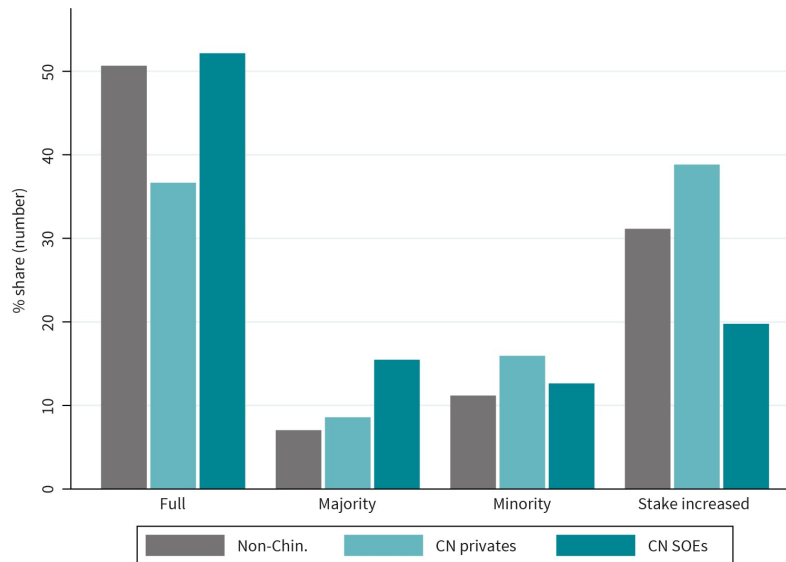
Notes: This figure shows the share of different types of deals for the three types of acquirers. Full means that 100% of the target firm were acquired. Majority means that at least 50% but less than 100% were acquired. Minority means that less than 50% were acquired. Stake increased means that the acquirer already owned a share of the target firm and increased this share.

There are also notable differences across the three types of investors in terms of the target share they acquire. Figure 4.2 shows that Chinese SOEs predominantly engage in full or majority acquisitions. In contrast, a larger percentage of acquisitions by Chinese private or non-Chinese investors takes the form of gradual increases in stakes. This could reflect that Chinese SOEs follow a less cautious investment strategy or are less financially constrained than other investors.

Next, we look at the geographical distribution of M&A deals by acquirer group. Figure 4.3 shows that a major share of global cross-border M&As takes place in Europe, which amounts to 66.6% of transactions by non-Chinese acquirers, 47.5% by Chinese SOEs, and 38.2% by Chinese private acquirers. Around 15-20% of global cross-border acquisition targets are located in North America. Significant differences emerge in other regions between Chinese and non-Chinese acquirers. There are more transactions by Chinese acquirers in the East Asia and Pacific region, as well as in Latin America and the Caribbean.

Table 4.2 offers a more detailed look at the distribution of deals by target countries

Figure 4.3: Distribution of Cross-Border M&As by Target Regions
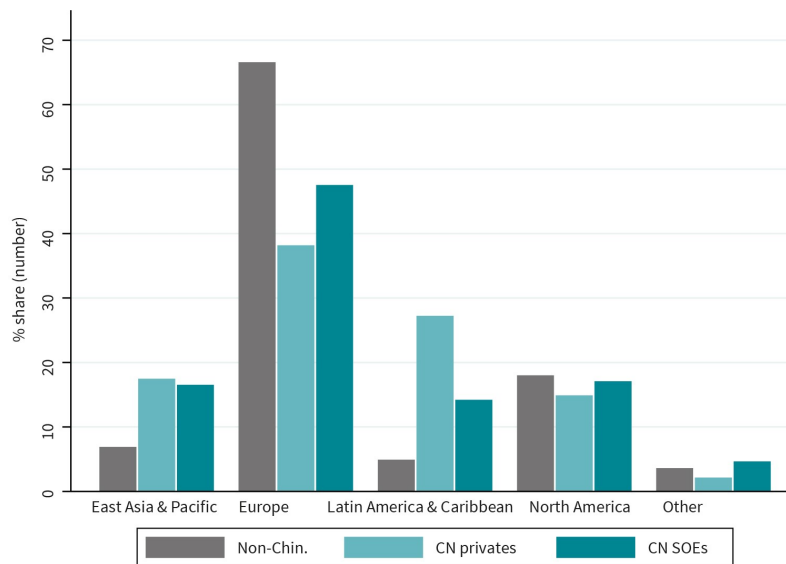
Notes: This figure shows the distribution of cross-border M&As in different regions. The category "Other" includes countries in Central Asia, Sub-Saharan Africa, Middle East & North Africa, and South Asia.

and acquirer types. Target countries are ranked based on the number of Chinese private acquisitions. For each target country, we provide the number of deals, the total deal value, and the corresponding sample percentages. A large share of Chinese private acquisitions occurs in tax havens and offshore financial centers. In terms of the number of deals, the British Virgin Islands lead the list for Chinese private acquirers. Chinese SOEs also have substantial M&A activities in the British Virgin Islands, Cayman Islands, and Bermuda. In contrast, tax havens and offshore financial centers are less popular with non-Chinese acquirers. In addition, Table 4.2 again shows the geographic preference of Chinese acquirers for Asia and Pacific countries. Based on the total value of deals, a much higher share of Chinese acquisitions happens in Australia, Japan, Malaysia, and Singapore, relative to non-Chinese acquisitions. There is no indication that Chinese acquirers invest more in BRIC countries (excluding China), as their investment pattern in Brazil, Russia, and India is not widely different from that of non-Chinese acquirers.

Table 4.2: Number of Deals and Deal Value by Target Countries and Acquirer Types

	Number of Deals						Total deal value (in million EUR)					
	Non-Chinese		CN private		CN SOE		Non-Chinese		CN private		CN SOE	
Target Country	Count	Percent	Count	Percent	Count	Percent	Value	Percent	Value	Percent	Value	Percent
Virgin Isl.	553	0.79	223	19.09	54	7.38	22,340	0.38	12,898	13.76	8,474	5.41
US	9,885	14.09	138	11.82	90	12.30	2,061,414	34.89	25,790	27.50	4,938	3.15
Gr. Britain	10,105	14.40	104	8.90	61	8.33	967,109	16.37	4,758	5.07	18,992	12.13
Germany	4,897	6.98	84	7.19	87	11.89	212,247	3.59	1,519	1.62	2,923	1.87
Cayman Isl.	271	0.39	76	6.51	24	3.28	47,186	0.8	4,535	4.84	9,968	6.36
Singapore	682	0.97	47	4.02	24	3.28	36,608	0.62	4,023	4.29	6,441	4.11
Australia	2,118	3.02	46	3.94	46	6.28	163,839	2.77	4,909	5.24	15,561	9.94
France	3,032	4.32	34	2.91	19	2.60	185,400	3.14	118	0.13	2,314	1.48
Italy	1,720	2.45	23	1.97	22	3.01	83,315	1.41	2,229	2.38	359	0.23
Japan	280	0.40	23	1.97	8	1.09	26,175	0.44	1,865	1.99	565	0.36
Netherlands	3,234	4.61	23	1.97	27	3.69	315,270	5.34	3,185	3.40	1,675	1.07
Spain	3,144	4.48	22	1.88	41	5.60	111,175	1.88	2,937	3.13	2,175	1.39
Malaysia	475	0.68	21	1.80	19	2.60	7,483	0.13	2,533	2.70	489	0.31
Bermuda	142	0.20	20	1.71	12	1.64	57,152	0.97	8,156	8.7	3,721	2.38
Canada	2,602	3.71	16	1.37	23	3.14	256,285	4.34	361	0.38	17,999	11.49
Belgium	1,482	2.11	14	1.20	5	0.68	78,600	1.33	229	0.24	1,890	1.21
India	239	0.34	13	1.11	6	0.82	35,634	0.60	39	0.04	48	0.03
Switzerland	1,296	1.85	12	1.03	8	1.09	113,653	1.92	4,355	4.64	37,432	23.90
Russia	1,727	2.46	11	0.94	4	0.55	106,778	1.81	78	0.08	3,155	2.01
Brazil	1,005	1.43	8	0.68	15	2.05	63,985	1.08	914	0.98	2,033	1.30
RoW	21,267	30.32	210	17.99	137	18.7	955,940	16.19	8,336	8.90	15,474	9.87
World	70,156	100	1,168	100	732	100	5,907,588	100	93,767	100	156,626	100

Notes: This table shows the number of deals and total deal value by target country and acquirer type. Total deal value is reported in constant million EUR with base year 2015.

Last, we consider target-level determinants of M&A activities. Table 4.3 compares the different groups of acquirers regarding the means of assets, leverage, return on assets (ROA), share of intangibles, assets growth, and patents of targets.[7] We also conduct a t-test to formally examine whether the sample means of target-level factors are equal between different types of acquirers. The descriptive statistics reveal some interesting heterogeneities. Relative to non-Chinese investors, Chinese investors purchase larger targets in terms of total assets. Moreover, Chinese private firms tend to purchase targets with significantly lower ROA but with more patents than non-Chinese investors.[8]

Table 4.3: Target-Level Characteristics by Acquirer Types

Variable	Non-CN	All CN	T-test of equal means (p-value)	CN SOE	T-test of equal means (p-value)	CN Private	T-test of equal means (p-value)
Total assets	101,189	702,026	0.0000	858,103	0.000	581,966	0.000
Leverage	0.191	0.256	0.7779	0.252	0.862	0.260	0.824
ROA	0.003	-0.045	0.0317	0.002	0.982	-0.080	0.005
Intangibles %	0.050	0.056	0.3705	0.055	0.570	0.056	0.483
Asset growth	14.258	4.942	0.8462	1.003	0.853	8.142	0.925
Patents	4.927	22.357	0.0003	8.927	0.568	30.819	0.000

Notes: This table reports the sample means of target size, leverage, return on assets (ROA), the share of intangibles in total assets, asset growth, and number of patents. Sample means are reported for each variable for targets acquired by non-Chinese, all Chinese, Chinese state-owned, and Chinese private investors, separately. The t-tests test the null hypothesis of equal sample means between targets acquired by non-Chinese and Chinese investors, between targets acquired by non-Chinese and Chinese state-owned investors, and between targets acquired by non-Chinese and Chinese private investors. P-values from the associated t-tests are reported. For definitions of target-level variables listed here, see Table 4.A1 in the Appendix.

4.5 How Do Chinese Acquirers Select Their Targets?

The first question we address in this study is whether Chinese overseas acquisitions have different rationales and patterns than non-Chinese investments. To shed light on this issue, we employ the deal-level data and estimate the following logit regression model:

$$Pr(CN_{i,j,t} = 1) = F(\beta_0 + \mu X_{i,t}^{TF} + \gamma Z_{j,t}^{TC} + Deal\,Year\,FE + \epsilon_{i,j,t}) \qquad (4.1)$$

where the dependent variable is a dummy indicating whether target i in country j in year t is purchased by a Chinese acquirer. In some estimations, we also differentiate between

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[7]Variable definitions are provided in Table 4.A2 in the Appendix.

[8]In unreported exercises, we compare Chinese and non-Chinese acquirers in terms of their sizes and profitability at the time of the acquisition. We do not find any significant differences between the two types of acquirers and hence, the observed differences in target features are unlikely to be driven by acquirers' size and profitability.

private Chinese firms and SOEs.[9] $X_{i,j,t}^{TF}$ is a set of target firm-level characteristics, and $Z_{j,t}^{TC}$ is a set of target country-level characteristics. The coefficients of interest are μ and γ, which indicate how various target firm-level and target country-level characteristics influence the probability of a target being acquired by a Chinese firm. If a coefficient is not statistically significant, the corresponding characteristic is either not important for all investors or equally important for Chinese and non-Chinese investors. We include deal year fixed effects in all specifications to control for general time trends. In some specifications, we also control for industry and target country fixed effects. Standard errors are robust and clustered at the target firm level.

4.5.1   Effects of Target Country Characteristics

We first examine how target country characteristics affect the probability of a target being acquired by a Chinese company as opposed to a non-Chinese investor. We consider a set of country-level economic indicators frequently employed in the literature.[10] We use three variables to proxy for market size: $Ln(GDPPC)$ is the log of real gross domestic product (GDP) per capita in the target country; $GDPgrowth$ is the target country's annual real GDP growth rate; $Ln(Population)$ is the log of population of the target country. Cross-border M&As may also be influenced by geographic proximity and trade volume. Distance measures the population-weighted distance between China and the target country; $Ln(Trade)$ measures the log of the bilateral trade volume between China and the target country. We construct a dummy variable $Tax\ Haven$ that equals 1 if a target country is regarded as a tax haven. To investigate whether Chinese cross-border M&As are more attracted to countries abundant in natural resources, we use the variable $Resource\ rents$ measuring total resource rents relative to the target country's GDP. We further consider two variables associated with economic risks in the target country: $Inflation$ is the annual inflation rate in the target country; and $\Delta Exchange\ rate$ is the rate of appreciation of the target country's currency against the Chinese Renminbi (RMB), where a negative value stands for an depreciation of the RMB. Institutional qualities in target countries are often considered to influence cross-border M&As. Using the World Bank's World Governance Indicators, we control for four institutional quality indicators: $Political\ Stability$, $Regulatory\ Quality$, $Rule\ of\ Law$, and $Control\ of\ Corruption$. A larger value in these variables indicates better institutional qualities in the target country.

Table 4.4 reports the estimated marginal effects based on the logit model, focusing

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[9]In the regression for Chinese private companies, acquisitions by Chinese SOEs are excluded from the sample and vice versa.

[10]Table 4.A1 in the Appendix provides on overview of definitions and summary statistics for these variables.

Table 4.4: Target Country Characteristics and Probability of Chinese Acquisitions

Probability of being acquired by	All CN					CN Private	CN SOE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Ln(GDPPC)	-0.009***	-0.008***	-0.007***	-0.008***	-0.009***	-0.005***	-0.004***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
GDP growth	-0.001*	-0.001*	-0.000	-0.001*	-0.001*	-0.000	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Ln(Population)	-0.009***	-0.009***	-0.009***	-0.009***	-0.009***	-0.005***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Distance	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.001***	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Ln(Trade)	0.013***	0.014***	0.013***	0.013***	0.013***	0.008***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Tax haven	0.016***	0.015***	0.014***	0.015***	0.016***	0.014***	0.000
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)	(0.005)
Resource rents	-0.010	-0.014	-0.020	-0.018	-0.010	-0.039***	0.021**
	(0.018)	(0.019)	(0.020)	(0.020)	(0.019)	(0.015)	(0.010)
Inflation	-0.001**	-0.001**	-0.001**	-0.001**	-0.001**	-0.000*	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Δ Exchange rate	-0.003	-0.004	-0.002	-0.003	-0.003	0.012**	-0.014***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.006)	(0.004)
Political stability		-0.002					
		(0.002)					
Regulatory quality			-0.003				
			(0.002)				
Rule of law				-0.002			
				(0.002)			
Control of Corruption					-0.000		
					(0.001)		
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	63,085	63,085	63,085	63,085	63,085	62,536	62,373

Notes: This table considers how target country-level economic and institutional characteristics affect the likelihood of a Chinese cross-border acquisition. Marginal effects from logit estimations based on Equation 1 are reported. Standard errors are robust and clustered at the target firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

on target country characteristics. Column (1) of Table 4.4 shows that relative to non-Chinese acquirers, Chinese acquirers tend to conduct acquisitions in countries with lower GDP per capita, lower GDP growth, and a smaller population. This suggests that Chinese cross-border M&As do not seem to be motivated by market size. As expected, being geographically closer to China and having a larger trade volume with China both increase the probability of Chinese cross-border acquisitions. Chinese investors also show a strong

preference for targets in tax havens, consistent with the previous descriptive analysis. A potential explanation for this result is the existence of capital controls in China. Successfully bidding for firms requires the ability to make large international payments at short notice. This may require Chinese companies to set up holding companies abroad. Tax havens and offshore financial centers may offer the easiest way to do so. We do not find that the estimated marginal effect on $Resource\,rents$ is statistically significant. Relative to other investors, Chinese acquirers tend to avoid inflation risks, as the estimated marginal effect on $Inflation$ is negative and significant. However, they do not seem to be particularly concerned about currency risks. We control for $Political\ Stability$ in Column (2), $Regulatory\,Quality$ in Column (3), $Rule\,of\,Law$ in Column (4), and $Control\,of\,Corruption$ in Column (5) of Table 4.4. However, we find no evidence that these institutional qualities of target countries affect decisions on Chinese overseas acquisitions differently, as the estimated marginal effects on all four indicators are insignificant.

In Columns (6) and (7), we differentiate between Chinese private and state-owned acquirers. The investment strategy of the two types of Chinese acquirers appears similar in many dimensions. However, some target country characteristics have different effects on the two. First, the strong preference for tax havens is unique to Chinese private acquirers, while the difference between Chinese SOEs and non-Chinese acquirers is not significant in this dimension. Second, while Chinese private acquirers are less likely to purchase targets in resource-rich countries, the opposite is true for Chinese SOEs. Thus, seeking natural resources is a motivation for Chinese state-owned acquirers only. Third, Chinese private investors tend to invest in countries where the currency depreciates against the RMB, but the reverse holds for Chinese SOEs. This suggests that Chinese SOEs may be less concerned about costs due to exchange rate risks. In unreported exercises, we include the four indicators of institutional qualities and compare the two types of Chinese acquirers with international acquirers. We continue to find that institutional qualities of target countries do not affect Chinese acquirers in a distinct way, regardless of their ownership type.

4.5.2   Effects of Target Industry

Using deal-level information, we are able to investigate whether Chinese acquirers are drawn to specific industries. Table 4.5 includes a set of target industry dummies based on the NACE industry classification[11], in addition to a basic set of macroeconomic control

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[11]We use NACE industries 77 to 99 as the reference group. This reference group includes administrative and support service activities, public administration and defense, compulsory social security, education, human health and social work activities, arts, entertainment and recreation, and other service activities.

variables.[12] Column 1 in Table 4.5 reveals that Chinese acquisitions are more likely to occur in certain industries. For example, Chinese investors are keen on targets in certain manufacturing industries, such as manufacturing of electronics, machinery, and vehicles. Consistent with resource seeking motives, Chinese acquirers are also more likely to conduct deals in the mining sector. In contrast, firms in the information and communication industry are less likely to be targeted by Chinese acquirers.

Table 4.5: Target Industries and Probability of Chinese Acquisitions

Probability of being acquired by	(1) All CN	(2) CN Private	(3) CN SOE
Agriculture	0.010	-0.009*	0.019**
	(0.009)	(0.005)	(0.008)
Construction	0.001	-0.003	0.005*
	(0.004)	(0.003)	(0.003)
Energy, water, gas.	0.004	-0.003	0.006**
	(0.005)	(0.004)	(0.003)
Finance & Insurance	0.004	-0.001	0.005***
	(0.003)	(0.002)	(0.002)
Information & Communication	-0.008***	-0.007***	-0.001
	(0.002)	(0.002)	(0.001)
Manufacturing of chemicals/oil, pharma	-0.002	-0.007**	0.005**
	(0.003)	(0.003)	(0.002)
Manufacturing of electricity & machinery	0.023***	0.007**	0.017***
	(0.004)	(0.003)	(0.003)
Manufacturing of metal products	0.011**	-0.006*	0.017***
	(0.005)	(0.003)	(0.004)
Manufacturing of vehicles	0.048***	0.017**	0.033***
	(0.010)	(0.008)	(0.008)
Mining	0.016***	-0.007**	0.025***
	(0.006)	(0.003)	(0.005)
Professional/scientific/techno-logical activities	-0.004	-0.008***	0.003**
	(0.003)	(0.002)	(0.001)
Macro Controls	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
No. of observations	62,353	61,723	61,373

Notes: This table considers how target industries affect the likelihood of a Chinese cross-border acquisition. Classification of industries is based on NACE industry classification. NACE industries 77 to 99 are used as the reference group. Marginal effects from logit estimations based on Equation 1 are reported. Standard errors are robust and clustered at the target firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Columns (2) and (3) of Table 4.5 differentiate between private and state-owned Chinese companies. Chinese SOEs, for example are more likely to acquire agricultural firms, but

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[12]This includes *GDPPC*, *GDP growth*, *Population*, *Distance*, and *Trade*.

the opposite is true for Chinese private firms. A similar pattern holds for targets in the utility, construction, and the mining sector. These results are consistent with the previous finding that Chinese SOEs are particularly attracted to natural resources abroad. Additionally, a comparison between Columns (2) and (3) reveals that even within the manufacturing sector, the two types of Chinese acquirers display different preferences for specific industries.

Generally speaking, we find that there are some notable differences between Chinese and non-Chinese investors in terms of their industry preferences. However, the distinction is even greater between Chinese SOEs and non-Chinese investors.

4.5.3   Effects of Target Firm Characteristics

Next, we consider target firm-level characteristics that may affect the probability of Chinese cross-border acquisitions. These include: *Industry Diversity* (a dummy that equals 1 if the target and the acquirer are active in different industries), the log of total assets of the target firm, as well as return on assets (*ROA*), *Leverage*, *Asset Growth*, *Intangibles* %, and *Patents*. All variables except *Patents* are measured one year before the acquisition and are winsorized at the 1% level.

We formally analyze whether Chinese acquisitions are different from other investments by including these target-level characteristics in the logit model, as specified by Equation (1). Table 4.6 reports the corresponding results. Columns (1) to (3) suggest that Chinese acquirers prefer targets in industries different from their own, with more assets, lower profitability, higher levels of debt, and more patents. Based on our preferred specification in Column 3 where we control for both target country and target industry fixed effects, the probability of this target being acquired by a Chinese investor increases by 0.7 percentage points if the target is from a different industry than the acquirer. A 10-percentage point reduction in ROA would increase the probability of a Chinese acquisition by around 0.2 percentage points. A 10-percentage point increase in target leverage leads to a 0.13 percentage point increase in the probability of a Chinese acquisition. Consistent with the view that Chinese cross-border M&As are particularly driven by technology transfer and know-how, we find a positive marginal effect associated with the number of patents the target holds. If the number of patents held by the target firm increases by one standard deviation, this increases the probability of acquisition by Chinese investors by around 0.2 percentage points.[13] Considering that only 2.6% of cross-border acquisitions are made by Chinese investors in our sample, these estimated marginal effects from ROA, leverage, and

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[13]The distribution of patents held by target firms is highly skewed. One standard deviation equals to around 200 patents.

patents are rather large.[14] There is also a positive and significant marginal effect of target size measured by total assets, but the magnitude of the effect is smaller. Based on the estimation result in Column 3, a 100-log points increase in target size raises the probability of a Chinese acquisition by 0.6 percentage points. Neither *Asset Growth* nor the share of intangibles matter differently for Chinese acquirers relative to non-Chinese acquirers.[15]

Table 4.6: Target-Level Characteristics and Probability of Chinese Acquisitions

Probability of being acquired by	All CN			CN Private			CN SOE		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Industry Diversity	0.008**	0.006	0.007*	0.002	0.001	0.001	0.006**	0.006*	0.006*
	(0.003)	(0.004)	(0.004)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Ln(Total Assets)	0.007***	0.006***	0.006***	0.002***	0.001*	0.001	0.006***	0.005***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
ROA	-0.018***	-0.020***	-0.020***	-0.006*	-0.008*	-0.009*	-0.013***	-0.014***	-0.013***
	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.003)	(0.004)	(0.004)
Leverage	0.007	0.014**	0.013**	0.006	0.009**	0.007	0.001	0.006	0.008
	(0.005)	(0.006)	(0.006)	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)
Asset Growth	-0.004	-0.004	-0.004	-0.002	-0.002	-0.001	-0.003	-0.002	-0.002
	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
Intangibles %	-0.005	-0.001	0.001	-0.003	0.001	0.005	-0.002	-0.003	-0.003
	(0.013)	(0.015)	(0.015)	(0.009)	(0.011)	(0.011)	(0.010)	(0.013)	(0.014)
Patents	0.001**	0.001***	0.001***	0.000***	0.001***	0.001***	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Macro controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Target country FEs		Yes	Yes		Yes	Yes		Yes	Yes
Target industry FEs			Yes			Yes			Yes
No. of observations	8,786	7,509	7,509	8,459	6,918	6,787	8,410	6,947	6,849

Notes: This table considers how targets' financial characteristics affect the likelihood of Chinese cross-border acquisition. We report the marginal effects from logit estimations based on Equation (1). Standard errors are robust and clustered at the target firm level. * p < 0.10, ** p < 0.05, *** p < 0.01.

We focus on Chinese private acquirers in Columns (4) to (6) and Chinese state-owned acquirers in Columns (7) to (9). We uncover several differences between SOEs and private firms. The preference for industry diversification is mainly driven by Chinese SOEs. This corroborates the results from Columns (1) to (3). Chinese SOEs favor larger targets. While the estimated marginal effect on $ln(Total\,Assets)$ is also positive for Chinese private acquirers, the effect becomes insignificant when we control for target-industry fixed effects. These results could imply that Chinese SOEs have financial support from the state-owned banking system which allows them to engage in large-scale cross-border M&As. The ten-

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[14]We also use the Stata command firthlogit to correct for potential bias due to the low probability of Chinese acquisitions in our sample (results available upon request), and the results are very similar to logit estimation results.

[15]Results are similar when we use a matched sample where Chinese and non-Chinese acquisitions are more comparable in terms of target size and profitability.

dency to buy highly leveraged targets and those which hold patents is mainly driven by Chinese private acquirers. These results indicate that Chinese private acquirers are more likely to purchase targets in financial distress, and that access to technology and knowledge may be a particularly important consideration for them. Relative to non-Chinese investors, both Chinese private and state-owned acquirers tend to purchase targets with lower ROA. Chinese acquirers may be less motivated by short-run profits, less cautious of investing in poorly-performing targets, or more optimistic about improving the performance of such targets after the acquisition.

4.5.4   Effects of Recent Chinese Policy Initiatives

Table 4.7: The *Belt and Road Initiative* and
Probability of Chinese Acquisitions

Probability of being acquired by	(1) All CN	(2) CN Private	(3) CN SOE
PostBRI	-0.115	-0.182	0.103
	(0.254)	(0.327)	(0.392)
BRI	-0.223*	-0.0118	-0.539**
	(0.132)	(0.163)	(0.220)
BRI×PostBRI	0.0320	-0.181	0.386*
	(0.142)	(0.182)	(0.230)
Macro controls	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
No. of observations	69,269	68,574	68,186

Notes: This table analyzes the impact of the *Belt and Road Initiative*, BRI, on Chinese cross-border acquisitions. We report the point estimates from logit estimations. PostBRI is a dummy that equals to 1 if the deal took place in or after 2013. BRI is a dummy variable that equals to 1 if the target country is one of the 65 BRI countries (see Table 4.B1 in the Appendix for the list of countries). Standard errors are robust and clustered at the target firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The Chinese government announced the *Belt and Road Initiative* in 2013 and *Made in China 2025* in 2015, which both reinforce the *Go Out* policy from 2000. Do these policy initiatives have a material impact on Chinese overseas acquisitions?

The initial aim of the *Belt and Road Initiative* is to improve trade, infrastructure, and investment links between China and 65 countries in Central, South, and South East Asia, Europe, the Middle East, and North Africa.[16] We use a differences-in-differences (DiD) approach to test whether the BRI has changed the regional focus of Chinese overseas acquisitions. To do so, we construct a dummy *PostBRI*, which equals 1 if the cross-

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[16]The list of BRI countries is provided in Table 4.B1 in the Appendix.

border deal happened in or after 2013 and 0 otherwise. We also construct a dummy BRI, which equals 1 if the target is located in one of the 65 BRI countries narrowly defining the outreach of the BRI initiative according to the China International Trade Institute. The interaction term between BRI and $PostBRI$ measures the effect of BRI on the location decisions of Chinese acquirers.

Column (1) of Table 4.7 shows that before 2013, Chinese acquirers were less likely to pursue targets in BRI countries, as the estimated coefficient on BRI is negative and statistically significant. For Chinese private acquirers, the *Belt and Road Initiative* fails to encourage acquisitions in BRI countries since 2013, as the estimated coefficient on $BRI \times PostBRI$ is insignificant in Column (2). In contrast, the estimated coefficient on $BRI \times PostBRI$ is positive and statistically significant for Chinese state-owned acquirers in Column (3). These results suggest that the BRI only influences the location choice of cross-border M&As by Chinese SOEs.

Table 4.8: *Made in China 2025* and Probability of Chinese Acquisitions

Probability of being acquired by	(1) All CN	(2) CN Private	(3) CN SOE
PostCN2025	0.0116	-0.100	0.343
	(0.299)	(0.374)	(0.484)
CN2025	-0.0166	0.0960	-0.185
	(0.0868)	(0.107)	(0.148)
CN2025×PostCN2025	0.0815	-0.218	0.402*
	(0.143)	(0.198)	(0.214)
Macro controls	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
No. of observations	62,353	61,723	61,373

Notes: This table analyzes the impact of the *Made in China 2025* policy on Chinese cross-border acquisitions. We report the point estimates from logit estimations. $PostCN2025$ is a dummy that equals to 1 if the deal took place in or after 2015. $CN2025$ is a dummy variable that equals to 1 if the target belongs to the industries defined in the *Made in China 2025* strategy. Standard errors are robust and clustered at the target firm level.* p < 0.10, ** p < 0.05, *** p < 0.01.

Made in China 2025 announced in 2015, defines ten industries in which the Chinese government wants Chinese companies to become globally competitive.[17] One way to reach that goal is through takeovers of foreign firms in these industries. To investi-

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[17]These include new generation information technology, high-end computerized machines and robots, space and aviation, maritime equipment and high-tech ships, advanced railway transportation equipment, new energy and energy-saving vehicles, energy equipment, agricultural machines, new materials, and bio-pharma and high-tech medical devices (Conrad et al.; 2016).

gate whether the policy influences the industry focus of Chinese overseas acquisitions, we again use a DiD estimator. We construct a dummy variable $CN2025$ that equals 1 for targets active in industries that are related to the *Made in China 2025* strategy. A second dummy, $PostCN2025$, equals 1 for acquisitions since 2015. We then interact $CN2015$ with $PostCN2025$ for the DiD estimations.

Table 4.8 reports the corresponding estimation results. There is no evidence that Chinese acquisitions occurred more frequently in industries targeted by *Made in China 2025* before 2015, relative to non-Chinese acquisitions. However, targets in these industries become significantly more likely to be purchased by Chinese SOEs after the policy was introduced. Again, the policy does not change the investment pattern of Chinese private acquirers.

4.5.5   Comparison between Chinese and Non-Chinese State-Owned Acquirers

An open question is whether Chinese SOEs are different from state-owned acquirers in other countries. We identify 619 non-Chinese state-owned acquirers in our sample. However, only for 522 of the basic country-level characteristics are available. We then run a logit estimation where the dependent variable equals 1 if a target is acquired by a Chinese SOE, and 0 if it is purchased by a non-Chinese SOE. We report the marginal effects based on this estimation in Table 4.9. We use alternative specifications to control for year and target-country fixed effects in the different columns.

Columns (1) and (2) consider target country characteristics that were previously found to matter for Chinese state-owned acquirers. In Columns (3) and (4), we add three target level characteristics: the indicator Industry diversity, the log of targets' pre-acquisition total assets, and ROA. These are the target firm characteristics we found to matter most for Chinese SOEs compared to non-Chinese investors (see Section 5.3). This reduces the sample size in the last two columns of Table 4.9, since we do not observe target-level characteristics for many acquisitions by state-owned investors.

While the estimated marginal effects on some factors vary across teh different columns due to changes in specifications and sample sizes, two robust results emerge: Chinese SOEs are more likely to acquire larger targets and those with lower pre-deal profitability. These patterns are consistent with previous findings when we use a broader set of non-Chinese acquirers as the control group. The estimated marginal effects are generally larger in Table 4.9, relative to those in previous tables, since the sample size becomes much smaller and the share of Chinese acquisitions increases. Relative to non-Chinese SOEs, Chinese state-owned acquirers no longer appear to be particularly focused on seeking natural resources, and there is only weak evidence in Column (3) that they are especially keen on industry

Table 4.9: Comparison between Chinese and Non-Chinese
State-Owned Acquirers

Probability of being acquired by CN SOEs	(1)	(2)	(3)	(4)
Ln(GDPPC)	-0.050	1.724**	-0.063	1.365
	(0.033)	(0.714)	(0.053)	(1.300)
GDP growth	-0.010	-0.030**	-0.008	-0.021
	(0.008)	(0.013)	(0.017)	(0.026)
Distance	-0.008	-2.592	-0.017	0.902
	(0.007)	(2.043)	(0.015)	(4.129)
Ln(Population)	-0.052*	-0.023	-0.040	-1.346
	(0.027)	(1.940)	(0.053)	(4.638)
Ln(Trade)	0.107***	-0.050	0.068	0.017
	(0.027)	(0.218)	(0.051)	(0.501)
Resource rents	0.027	0.941	-1.303	-11.382
	(0.480)	(2.669)	(1.027)	(12.552)
Δ Exchange rate	-0.554**	-0.670**	-0.412	-0.382
	(0.261)	(0.294)	(0.460)	(0.564)
Industry Diversity			0.116**	0.054
			(0.055)	(0.060)
Ln(Total Assets)			0.063***	0.064***
			(0.013)	(0.015)
ROA			-0.264**	-0.301***
			(0.106)	(0.112)
Year FEs	Yes	Yes	Yes	Yes
Target country FEs		Yes		Yes
No. of observations	928	828	271	233

Notes: This table compares Chinese and non-Chinese state-owned acquirers. The dependent variable is a dummy that equals 1 if a target is purchased by a Chinese SOE and 0 if it is purchased by an SOE from other countries. We report the marginal effects from logit estimations. Standard errors are robust and clustered at the target firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

diversification. Our findings thus echo Karolyi and Liao (2017) that state-owned acquirers in general are more oriented towards targets in resource-rich countries and aim to diversity their industry portfolio. In these dimensions, Chinese SOEs are not different from other SOE acquirers.

4.6   Do Chinese Acquirers Pay More?

In this section, we investigate whether Chinese acquirers pay different prices compared to non-Chinese investors for targets with similar observable characteristics. The motivation

for this analysis is the anecdotal evidence that Chinese acquirers, backed by cheap financing from domestic banks or direct government subsidies, overpay relative to other investors to win bids. As around 95% of target firms in our sample are unlisted, we do not observe the share prices of target firms, which is the most common variable used in the literature for pricing analyses. Instead, we calculate the variable $Price_{i,j,t}$ which is the amount an acquirer pays for 1% of the equity of target firm i in country j in year t.[18] We then estimate Equation (2) as follows:

$$
\begin{aligned}
lnPrice_{i,j,t} = {} & \beta_0 + \beta_1 CN_{i,j,t} + \beta_2 lnEquity_{i,t} + \beta_3 ROA_{i,t} + \beta_4 Leverage_{i,t} \\
& + \beta_5 Full\,AC_{i,j,t} + \beta_6 Any\,Patent_{i,t} + \gamma Z_{j,t}^{TC} + Deal\,Year\,FE \\
& + Industry\,FE + Target\,country\,FE + \epsilon_{i,j,t}.
\end{aligned} \tag{4.2}
$$

In Equation (2), CN is a dummy that equals 1 if the acquirer of target firm i in country j in year t is a Chinese firm. To differentiate between Chinese private and state-owned acquirers, we include in some specifications a dummy $CNpriv_{i,j,t}$ that equals 1 if the acquirer is a Chinese private firm, and a second dummy $CNSOE_{i,j,t}$ that equals 1 if the acquirer is a Chinese SOE. We include several target firm-level characteristics: ROA and $ln(Equity)$ are the average value of return on assets and the average book value of equity over the three years prior to the deal;[19] $Leverage$ is the sum of long-term debt and short-term loans as a ratio to total assets also averaged over the three years prior to the deal; $Full\,AC$ is a dummy variable indicating whether 100% of the target's equity is acquired; $Any\,Patent$ is a dummy indicating whether the target firm holds any patents. $Z_{j,t}^{TC}$ is a set of target country-level controls. Furthermore, we control for industry and target-country fixed effects and allow for different time fixed effects across specifications.

Table 4.10 shows the estimation results based on Equation (2). Larger $ln(Equity)$ or ROA increases the payment for the target among all types of acquirers. Similarly, investors pay more to purchase targets with patents. Surprisingly, all types of acquirers tend to pay more for highly-leveraged firms. This may reflect potential tax advantages associated with debt or unobserved factors improving both borrowing capacity and value. These results are robust throughout the different specifications shown in Table 4.10. Whether the deal is a full acquisition has no significant impact on the prices paid for 1% of a target's equity.

Controlling for these observable characteristics, we do not find any tendency of overpayment by Chinese acquirers relative to non-Chinese investors as the estimated coefficient on CN is not statistically significant across specifications. When we distinguish between

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[18]This is to account for the fact that not all acquirers in our sample bought 100% of the target firm.

[19]We control for book equity value instead of total assets because acquirers purchase the equity of the target firm, which is different from asset acquisition. Our results are robust to controlling for total assets instead of equity.

Table 4.10: Prices Paid for Targets by Chinese Acquirers

Dep. variable: **ln $(Price_{i,j,t})$**	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CN	0.127		0.065		0.001		0.053	
	(0.095)		(0.098)		(0.110)		(0.165)	
CNpriv		0.084		0.002		-0.067		0.025
		(0.137)		(0.142)		(0.157)		(0.243)
CNSOE		0.164		0.119		0.060		0.077
		(0.117)		(0.125)		(0.139)		(0.213)
Ln(Equity)	0.742***	0.742***	0.745***	0.745***	0.736***	0.735***	0.758***	0.758***
	(0.014)	(0.014)	(0.013)	(0.013)	(0.014)	(0.014)	(0.019)	(0.019)
ROA	0.289***	0.289***	0.252**	0.252**	0.331***	0.331***	0.346***	0.346***
	(0.102)	(0.102)	(0.101)	(0.101)	(0.105)	(0.105)	(0.133)	(0.133)
Leverage	1.182***	1.182***	1.190***	1.189***	1.126***	1.125***	1.212***	1.211***
	(0.097)	(0.097)	(0.095)	(0.095)	(0.099)	(0.099)	(0.135)	(0.135)
Full AC	0.016	0.016	0.022	0.021	-0.059	-0.059	-0.131	-0.131
	(0.055)	(0.055)	(0.054)	(0.054)	(0.058)	(0.058)	(0.088)	(0.088)
Any Patent	0.340***	0.340***	0.368***	0.368***	0.258***	0.258***	0.422***	0.422***
	(0.048)	(0.048)	(0.050)	(0.050)	(0.047)	(0.047)	(0.070)	(0.070)
Macro controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes			Yes	Yes
Target country FEs	Yes	Yes			Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FEs			Yes	Yes				
Target country- year FEs					Yes	Yes		
Target country- industry-year FEs							Yes	Yes
No. of observations	5,166	5,166	5,159	5,159	5,131	5,131	3,350	3,350

Notes: This table analyzes whether the prices paid by Chinese acquirers are different those paid by non-Chinese investors. The dependent variable $Price_{i,j,t}$ is the natural logarithm of price paid for a 1% share of the target firm (in thousand EUR). CN is a dummy that equals 1 if the acquirer is a Chinese firm. $CNpriv$ is a dummy that equals 1 if the acquirer is a Chinese private firm, and $CNSOE$ is a dummy that equals 1 if the acquirer is a Chinese state-owned or state-controlled firm. ROA, $Ln(Equity)$ and $Leverage$ are the average value of return on assets, the natural logarithm of the book value of equity and total debt in total assets over the three years prior to the deal. $Full\ AC$ is a dummy variable indicating whether 100% of the target were acquired. $Any\ Patent$ is a dummy indicating whether the target firm holds any patent. Standard errors are robust and clustered at the target firm level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Chinese SOEs and private investors, neither appears to pay more for similar targets as compared to non-Chinese investors (Columns (2), (4), (6), and (8) of Table 4.10). These results question the view that Chinese investors systematically outbid others in the global M&A market. One should also note that we do not find any systematic difference between Chinese SOEs and non-Chinese acquirers, even though in particular the former might receive government subsidies and support in other forms. As a robustness test, we estimate Equation (2) using acquisition prices instead of logarithms of prices as dependent variable.

In these estimations we even find slightly lower prices paid by Chinese investors, confirming the result that they do not overpay.[20]

4.7   What Are the Effects of Chinese Acquisitions on Target Firms?

As last step of the analysis, this section investigates how a takeover by a Chinese company affects the target firm and its employees. Again, the focus is on the question of whether these effects differ from the changes triggered by a non-Chinese acquisition.

4.7.1   Empirical Approach

When estimating the effects of a Chinese acquisition, a simple OLS-estimation is likely to yield biased results as the selection of targets by Chinese investors is endogenous. For instance, the selection of targets is likely to depend on their pre-acquisition financial performance. To address this issue, we use a differences-in-differences approach. The treatment group consists of targets acquired by a Chinese company, and the control group comprises targets acquired by non-Chinese investors. Following the approach of Wang and Wang (2015), the dependent variable in all regressions is the accumulated change in the target firm's financial variables since the deal. Accordingly, only targets for which financial information from pre- and post-deal periods is available are used for this analysis. This leaves us with 14,243 targets that were acquired by non-Chinese companies and 351 targets with Chinese acquirers. Most targets in this sample are from Europe. This is not surprising, as a large share of acquisitions in the sample took place in Europe.

We focus on the effects of Chinese acquisitions on target firms' labor and capital productivity. We use turnover per employee and value-added per employee to proxy labor productivity. Capital productivity is proxied by the ratio of turnover or value-added to total assets. We also investigate changes in targets' leverage, ROA, average cost of labor, and the share of intangible assets in total assets. Summary statistics of these variables are provided in Table 4.A4 of the Appendix.

The results presented in Section 5 suggest that targets of Chinese acquirers are different in a number of dimensions as compared to other acquisition targets. Targets of Chinese acquirers tend to be larger, have lower profitability and tend to have higher levels of debt before acquisition. In the baseline OLS regressions, we therefore control for total assets, return on assets, and leverage. We also control for the deal year. To account for the specific industry distribution of Chinese acquisitions across industries, industry group dummies are

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[20]The corresponding estimation results are reported in Table 4.C1 in the Appendix.

added. We control for the target country characteristics that affect the probability of a target being acquired by a Chinese firm as opposed to a non-Chinese investor (see Table 4.3).[21] The estimation equation takes the following form:

$$
\begin{aligned}
\Delta Dep.Var_{i,j,t} = \beta_0 &+ \beta_1 CN_i + \mu X_{i,t}^{TF} + \gamma Z_{j,t}^{TC} \\
&+ Deal\,Year\,FE + Industry\,FE + \epsilon_{i,j,t}
\end{aligned}
\tag{4.3}
$$

where $\Delta Dep.Var_{i,j,t}$ measures the relative performance of target firm i in country j in period t. We run separate regressions for the acquisition year and each of the two subsequent years.[22] CN is a dummy that takes the value of 1 if the acquirer is a Chinese firm. $X_{i,t}^{TF}$ comprises the target firm controls and $Z_{j,t}^{TC}$ is a set of target country-level controls.

Since the differences-in-differences estimator crucially relies on the suitability of the control group, we employ two re-weighting strategies to improve the comparability of the treatment and control groups: entropy balancing (Hainmueller; 2012)[23] and propensity score matching. Entropy balancing re-weights observations in the control group such that the distribution of a set of pre-specified covariates matches that of the treatment group. The weights are chosen in a way that the balancing constraints are met, but remain as close as possible to uniform weights. The weights are calibrated based on the variables used as controls in the baseline OLS estimation, including target country-level and target firm-level controls, industry and deal year dummies. Parts A and B of Table 4.A5 illustrate the effects of the re-weighting based on entropy balancing.[24] Part A of Table 4.A5 summarizes the mean and variance for a number of variables in treatment and control group without reweighting. As discussed above, on average, the leverage of target firms in the treatment group is higher, while their return on assets is lower than for the control group. Firms in the treatment group also tend to be larger in terms of total assets. The target country characteristics are also distributed differently across treatment and control group. As shown in Part B of Table 4.A5, the treatment and control groups become much more comparable with respect to these parameters after re-weighting using entropy balancing.

To test the robustness of the estimation results, we use propensity score matching as a

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[21] These include target countries' GDP per capita, GDP growth, population, inflation, corporate income tax rate, distance to and trade volume with China.

[22] As the coverage of the different dependent variables varies, individual samples are constructed for each of them. To ensure that results are comparable across time, we only keep observations that we observe in each of the three periods. For most dependent variables, the treatment group contains between 100 and 150 target firms.

[23] For the implementation of this approach, we rely on the Stata package described in Hainmueller and Xu (2013).

[24] The re-weighting shown in Table 4.A5 reports the data used for the estimations on capital productivity 1 as an example. Results for the samples for all other dependent variables look very similar.

second re-weighting strategy (see Abadie and Imbens 2016 on this approach). The propensity score reflects the conditional probability of assignment into the treatment group. Here, the propensity score is calculated using a logit regression based on the same independent variables as described above. In a second step, all observations in the treatment group are matched with three observations from the control group based on their propensity score.[25] Part C of Table 4.A5 summarizes the effects of this re-weighting technique.

4.7.2   Results on Acquisition Effects

Table 4.11 summarizes the estimation results on the effects of a Chinese acquisition as compared to an acquisition by an investor from another country. Each cell of Table 4.11 reports the treatment effect for an individual estimation. Columns (1) to (3) present the results from the basic differences-in-differences estimation without re-weighting for the acquisition year (Column (1)) and the two years following the acquisition (Columns (2) and (3)). According to these estimations, the effects of an acquisition by a Chinese company are similar to those of a non-Chinese acquisition in terms of labor productivity, leverage, ROA, and the share of intangible assets. In contrast, both measures of capital productivity are negatively affected by a Chinese acquisition in the acquisition year. This effect fades out over the subsequent years. The treatment effect for the average compensation of employees shows the opposite pattern. While there is no significant effect in the treatment year and the first year after the acquisition, the respective coefficient is positive and highly significant in the second year. On average, wages in firms bought by Chinese investors grow by roughly 7,000 Euro over three years after acquisition compared to firms acquired by other investors.

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[25]We use nearest neighbor matching with three matches per observation in the control group. Using a lower number of matches per observation does not change the results substantially.

Table 4.11: Estimation Results on Acquisition Effects

	Baseline OLS			Entropy Balancing			Propensity Score Matching		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Acquisition Year	One year after	Two years after	Acquisition year	One year after	Two years after	Acquisition year	One year after	Two years after
Labor Productivity 1	-13.27	-16.93	65.46	-25.37	-24.81	44.54	1.431	-46.48	45.35
	(25.42)	(29.04)	(60.73)	(27.82)	(32.08)	(64.68)	(26.96)	(34.23)	(63.27)
Labor Productivity 2	-0.004	4.326	6.579	-0.151	1.407	6.356	11.63	5.598	12.14
	(16.23)	(12.78)	(13.28)	(17.76)	(13.22)	(13.50)	(18.27)	(15.07)	(15.23)
Capital Productivity 1	-0.177***	-0.046	-0.086	-0.177***	-0.012	-0.077	-0.160**	0.001	-0.065
	(0.059)	(0.080)	(0.095)	(0.061)	(0.080)	(0.097)	(0.074)	(0.092)	(0.105)
Capital Productivity 2	-0.064***	-0.051*	-0.055	-0.064***	-0.046*	-0.060	-0.030	0.018	0.002
	(0.025)	(0.030)	(0.041)	(0.025)	(0.028)	(0.039)	(0.029)	(0.032)	(0.040)
Leverage	0.004	0.045	0.025	-0.003	0.021	0.000	-0.011	0.031	0.004
	(0.021)	(0.032)	(0.031)	(0.027)	(0.038)	(0.040)	(0.028)	(0.038)	(0.040)
Return on assets	-0.031	-0.010	-0.007	-0.039	-0.021	-0.015	0.018	0.007	0.017
	(0.036)	(0.021)	(0.025)	(0.039)	(0.035)	(0.037)	(0.043)	(0.039)	(0.041)
Avg. Compensation of Employees	0.308	2.416	8.849**	-0.599	2.181	7.513**	-0.979	1.102	7.373*
	(1.751)	(2.304)	(3.624)	(2.033)	(2.548)	(3.766)	(2.349)	(2.644)	(3.971)
Intangibles %	0.000	0.000	0.001	0.002	0.001	0.001	0.005	0.004	0.008
	(0.005)	(0.007)	(0.008)	(0.005)	(0.007)	(0.008)	(0.006)	(0.008)	(0.009)

Notes: This table summarizes the effects of Chinese cross-border acquisitions on target firms. We report the marginal effects of difference-in-difference estimations using simple OLS (Columns 1-3), entropy balancing (Columns 4-6) and propensity score matching (Columns 7-9). Dependent variables are the differences in the respective variable between the pre-acquisition year and the year noted. The baseline OLS estimations contain target firm-level controls (return on assets, ln(total assets), return on assets) and target country-level controls (GDPPC, GDP growth, Inflation, Population, Distance, Trade) and as well as industry and deal year dummies. Entropy balancing and propensity score matching are based on the same variables. Standard errors are robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The control groups for the estimations of Columns (4) to (6) of Table 4.11 are re-weighted using entropy balancing. The results from the corresponding estimations confirm the findings of the baseline regressions in Columns (1) to (3) of Table 4.11. Again, the treatment effect is negative and statistically significant for both measures of capital productivity immediately after the acquisition, while the coefficient on average compensation of employees becomes positive and significant in the second year after the acquisition. The negative effect on capital productivity seems to be due to an increase in assets shortly after the acquisition. While turnover also increases after the acquisition, this sets in a little later (see Table 4.C2 in the Appendix). In Columns (7) to (9) of Table 4.11, the control group is constructed using propensity score matching. Results are similar but of slightly lower statistical significance. This is mainly due to a smaller sample.

To check whether an acquisition by Chinese private firms and SOEs have different effects on the targets, all estimations are also conducted including two different dummy variables indicating whether a company is a Chinese private company or a Chinese SOE, similar to the approach used in Section 4.6. About half of the treatment group was acquired by an SOE. Table 4.C3 in the Appendix shows the results of the corresponding estimations for the two measures of capital productivity, average compensation of employee and the share of intangible assets.[26] The negative short-run effect on capital productivity is similar between Chinese private acquirers and SOEs. In contrast, the increase in average compensation of employees is mainly driven by Chinese SOEs. Lastly, the effect on the share of intangible assets differs between the two acquirer types. While the corresponding coefficient is positive for Chinese private acquirers and statistically significant in the acquisition year and two years after the acquisition, it is negative and insignificant for Chinese SOEs.

How can these findings be explained? There is anecdotal evidence that Chinese investors place a larger impact on the 'footprint' of their investment and less on profitability, at least in the short term. This would be compatible with the observation that Chinese investors inject more assets into target firms, even at the cost of declining capital productivity immediately after the acquisition. The fact that employee compensation is higher than in targets firms bought by Chinese investors may have various explanations. More investment in the target may raise the bargaining power of employees. It is also possible that employees are more hostile towards takeovers by Chinese investors, especially Chinese SOEs, than towards other investors. Given this, Chinese investors may decide to offer higher salaries to get employees to cooperate or prevent them from leaving.

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[26]For all other dependent variables from Table 4.11, all coefficients of $CNpriv$ and $CNSOE$ are not statistically significant. The control group in the estimations shown in Table 4.C2 is re-weighted based on entropy balancing.

4.8   Conclusion

The growing importance of Chinese cross-border mergers and acquisitions in recent years has given rise to a debate about the motivations and the likely impact of these investments. In this debate, the idea is widespread that Chinese investors differ from other international investors. This idea is related to the far reaching influence of the Chinese government on economic activities of Chinese companies both domestically and abroad. A direct indicator for this influence is the large number of SOEs, which also play an important role as acquirers of foreign companies.

The aim of this paper is to investigate determinants, pricing, and effects of Chinese cross-border acquisitions. First, regarding the question of target selection, our analysis suggests that Chinese investment does indeed differ from investment coming from other countries in several aspects. Our findings support the view that Chinese acquirers tend to be less concerned about market size and economic risks of target countries. Instead, they are more interested in factors like natural resources. Acquisitions by Chinese SOEs are also significantly influenced by government policies like the *Belt and Road Initiative* and *Made in China 2025.*, With respect to target firm-level characteristics, we find that Chinese companies acquire less profitable and more indebted targets. This suggests that Chinese acquirers might have easier access to finance than other investors, which allows them to pursue less cautious investment strategies. Alternatively, it may be the case that Chinese acquirers are more optimistic about the improving targets' post-acquisition performance, have a longer time horizon or pursue objectives other than profitability, such as a large 'footprint' of investment. The latter seems plausible in particular for Chinese SOEs, where the managers are often politicians who intend to pursue a career in politics.

Second, on the question of acquisition prices, we do not find that Chinese acquirers pay higher prices than other investors for targets with comparable characteristics. This contradicts the view that government support enables Chinese companies to outbid other investors in the global M&A market.

Third, we have also analyzed the impact of a Chinese acquisition on the target firms. In many dimensions, the effects of a Chinese acquisition are similar to those of non-Chinese takeovers. However, we do find that the capital productivity of Chinese acquisition targets declines in the short run, while average employee compensation gradually rises compared to other targets. Thus, our results do not support the concern that a Chinese takeover leads to lower wages for employees compared to other acquisitions.

Drawing policy conclusions from these findings is difficult though, for a number of reasons. First, some of our findings are based on a small number of observations, mostly because the increase in Chinese acquisitions abroad is a relatively recent phenomenon.

The impact of takeovers by Chinese investors on target firms and target countries may also only become more visible in the longer run, which would require a longer sample period. Second, we are unable to investigate important issues such as technology transfer due to data limitations. These questions should be addressed in future research.

4.A   Appendix: Variable Definitions and Summary Statistics

Table 4.A1:   Country-Level Control Variables

Variable	Definition	Source	Obs	Mean	Std. Dev
Ln(GDPPC)	Ln of GDP per capita (USD)	WDI	69,540	10.36	0.815
GDP growth	GDP growth rate (%)	WDI	69,540	2.24	2.65
Distance	Population weighted distance to China (1000 km)	CEPII	71,783	8.87	2.42
Ln(Population)	Ln of No. of inhabitants	WDI	69,656	3.52	1.73
Ln(Trade)	Ln of the sum of exports and import in goods with China (USD)	UN Comtrade	69,543	3.53	1.54
Inflation	Annual inflation of consumer prices (%)	WDI	68,306	2.49	3.03
Tax haven	Dummy=1 if the target country is defined as a tax haven according to the OECD (see Table A3 for the list of countries)	OECD	72,056	0.028	0.164
Resource rents	Share of resource rents in GDP	WDI	63,653	0.021	0.020
ΔExchange rate	Annual growth rate of target country currency relative to Chinese Yuan	WDI and own calculations	66,687	0.021	0.259
Political stability	Measure for political stability on a scale from -2.5 to 2.5	WGI	68,918	0.538	0.609
Regulatory quality	Measure for regulatory quality on a scale from -2.5 to 2.5	WGI	68,894	1.284	0.633
Rule of law	Measure for rule of law on a scale from -2.5 to 2.5	WGI	68,917	1.287	0.750
Control of corruption	Measure for control of corruption on a scale from -2.5 to 2.5	WGI	68,896	1.288	0.880

Table 4.A2: Firm-Level Control Variables

Variable	Definition	Source	Obs	Mean	Std. Dev
Industry Diversity	Dummy=1 if the target and the acquirer are in different industries	Orbis	58,385	0.543	0.498
Ln(Total Assets)	Natural logarithm of total assets of the target firm	Orbis	21,999	8.51	2.12
ROA	(Profit or loss before taxes) / Total assets	Orbis	21,907	0.027	0.339
Book equity	Natural logarithm of Total assets - (loans + long-term debt)	Orbis and own calculation	23,589	71,019	806,934
Patents	Number of patents the target firm holds as reported in 2017	Orbis	71,525	5.39	204.3
Any patent	Dummy variable indicating whether the target firm holds any patent	Orbis	71,525	0.133	0.339
Leverage	(Short-term loans + long term debt) / Total assets	Orbis	18,591	0.313	6.91
Asset Growth	Annual growth rate of total assets	Orbis	23,783	14.04	1,114
Intangibles %	Intangible fixed assets / Total assets	Orbis	20,550	0.050	0.140

Notes: All variables are winsorized at the 1%-level.

Table 4.A3: List of Tax Haven Countries

Andorra	Gibraltar	Netherlands Antilles
Anguilla	Grenada	Niue
Antigua and Barbuda	Guernsey	Panama
Aruba	Isle of Man	Samoa
The Bahama	Jersey	San Marino
Bahrain	Liberia	Seychelles
Bermuda	Liechtenstein	St. Lucia
Belize	Malta	St. Kitts & Nevis
British Virgin Islands	Marshall Islands	St. Vincent and the Grenadines
Cayman Islands	Mauritius	Turks & Caicos Islands
Cook Islands	Monaco	US Virgin Islands
Cyprus	Montserrat	Vanuatu
Dominica	Nauru	

Source: OECD (2000).

Table 4.A4: Variable Description and Summary Statistics for Effects Estimations

Variable	Definition	Source	Obs	Mean	Std. Dev
ΔLabor Productivity 1	Δ (Turnover / No. of Employees)	Orbis	7,273	21.24	276.7
ΔLabor Productivity 2	Δ (Added Value / No. of Employees)	Orbis	4,918	12.97	145.1
ΔCapital Productivity 1	Δ (Turnover / Total assets)	Orbis	10,226	0.012	0.941
ΔCapital Productivity 2	Δ (Added Value / Total assets)	Orbis	6,308	-0.012	0.342
ΔLeverage	Δ ((Short-term loans + long term debt) / Total assets)	Orbis	10,389	-0.020	0.213
ΔROA	Δ ((Profit / loss before taxes) / Total assets)	Orbis	11,078	-0.018	0.387
ΔAverage Cost of Employees	Δ (Total compensation of employees / No. of employees)	Orbis	6,595	2.57	19.14
ΔIntangibles %	Δ (Intangible fixes assets / total assets)	Orbis	10,977	-0.004	0.058

Notes: All information on the number of observations, mean and standard deviation of variables refer to the difference between the pre-acquisition year and the acquisition year. All variables are winsorized at the 1%-level.

Table 4.A5: Reweighting

	Treatment		Control	
Variable	**Mean**	**Variance**	**Mean**	**Variance**
Part A: Without weighting				
Leverage	0.295	0.195	0.203	0.093
ROA	-0.050	0.142	0.037	0.094
Ln(Total Assets)	9.813	4.403	8.794	3.926
GDPPC	32,593	2.95E+08	33,677	4.94E+08
GDP growth	1.956	5.56	1.695	11.16
Inflation	2.189	5.197	3.062	18.81
Population	47.66	2,135	51.56	10,773
Distance	7,634	5.40E+06	3,482	1.31E+07
Trade	99.32	5,410	83.79	1,868
CIT rate	26.49	43.53	26.02	45.05
Part B: After re-weighting based on entropy balancing				
Leverage	0.295	0.195	0.294	0.194
ROA	-0.050	0.142	-0.050	0.142
Ln(Total Assets)	9.813	4.403	9.788	4.392
GDPPC	32,593	2.95E+08	32,509	2.94E+08
GDP growth	1.956	5.556	1.951	5.542
Inflation	2.189	5.197	2.184	5.189
Population	47.66	2,135	47.54	2,130
Distance	7,634	5.40E+06	7,615	5.39E+06
Trade	99.32	5,410	99.07	5,396
CIT rate	26.49	43.53	26.42	43.43
Part C: After re-weighting based on propensity score matching				
Leverage	0.295	0.195	0.252	0.117
ROA	-0.050	0.142	-0.048	0.173
Ln(Total Assets)	9.813	4.403	9.553	4.263
GDPPC	32,593	2.95E+08	29,645	3.29E+08
GDP growth	1.956	5.556	2.064	6.584
Inflation	2.189	5.197	2.254	5.080
Population	47.66	2,135	52.70	10,262
Distance	7,634	5.40E+06	8,065	1.67E+7
Trade	99.32	5,410	94.99	4,293
CIT rate	26.49	43.53	26.51	49.62

Notes: All variables as in the year before the acquisition. In addition to the variables reported here, reweighting is also based on deal year and industry. The samples shown here are used for the estimations on capital productivity 1, but look very similar for the other estimations on the effects of Chinese acquisitions. The unweighted sample shown here contains 113 treated units and 6,083 control units. Apart from the variables listed here, re-weighting is also based on deal year and industry. For Part C, nearest neighbor matching with three matches per observation in the control group is used.

4.B   Appendix: The *Belt and Road Initiative*

Table 4.B1: Countries of the *Belt and Road Initiative*

Region	Countries
East Asia	China, Mongolia
South East Asia	Brunei, Cambodia, Indonesia, Laos, Malaysia, Myanmar, Philippines, Singapore, Thailand, Timor-Leste, Vietnam
South Asia	Afghanistan, Bangladesh, Bhutan, India, Maldives, Nepal, Pakistan, Sri Lanka
Central Asia	Kazakhstan, Kyrgyzstan, Tajikistan, Turkmenistan, Uzbekistan
Middle East and North Africa	Bahrain, Egypt, Iran, Iraq, Israel, Jordan, Kuwait, Lebanon, Oman, Qatar, Saudi Arabia, Palestine, Syria, United Arab Emirates, Yemen
Europe	Albania, Armenia, Azerbaijan, Belarus, Bosnia and Herzegovina, Bulgaria, Croatia, Czech Republic, Estonia, North Macedonia, Georgia, Hungary, Latvia, Lithuania, Moldova, Montenegro, Poland, Romania, Russia, Serbia, Slovakia, Slovenia, Turkey, Ukraine

Source: Steer Davies Gleave (2018).

4.C   Appendix: Additional Estimations

Table 4.C1: Prices Paid for Targets by Chinese Acquirers - Using the Level of Prices as the Dependent Variable

Dep. variable: $Price_{i,j,t}$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CN	-2424.5*		-2203.6		-2960.9**		0.053	
	(1417.9)		(1523.2)		(1474.8)		(0.165)	
CNpriv		-4653.2**		-4218.2**		-5013.6**		-5116.8*
		(1997.4)		(1973.6)		(2362.1)		(2982.7)
CNSOE		-535.9		-511.0		-1228.7		-2104.9
		(1535.7)		(1821.9)		(1291.0)		(1698.1)
Ln(Equity)	0.016***	0.016***	0.016***	0.016***	0.016***	0.016***	0.015***	0.015***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
ROA	3463.6***	3463.3***	2793.1***	2788.6***	3757.7***	3760.8***	4846.7***	4867.7***
	(926.7)	(926.5)	(751.1)	(750.9)	(1066.5)	(1066.2)	(1507.1)	(1509.4)
Leverage	3908.2***	3879.6***	3619.3***	3587.9***	3740.5***	3714.8***	3275.3**	3260.3**
	(1123.5)	(1121.6)	(1113.4)	(1112.8)	(1077.1)	(1073.5)	(1655.4)	(1654.7)
Full AC	306.1	291.0	670.9	646.6	-106.0	-119.0	54.4	32.6
	(396.6)	(396.4)	(411.3)	(411.2)	(422.1)	(421.3)	(783.4)	(784.8)
Any Patent	728.1	719.5	1034.2	1026.3	1199.5**	1188.7**	1730.3*	1729.7*
	(621.8)	(620.4)	(682.2)	(680.9)	(571.6)	(570.9)	(975.3)	(974.2)
Macro controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes			Yes	Yes
Target country FEs	Yes	Yes			Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year FEs			Yes	Yes				
Target country-year FEs					Yes	Yes		
Target country-industry-year FEs							Yes	Yes
No. of observations	5,315	5,315	5,307	5,307	5,288	5,288	3,485	3,485

Notes: This table analyzes whether the pricing of targets by Chinese acquirers is different from non-Chinese investors. The dependent variable $Price_{i,j,t}$ is what the acquirer paid for a 1% share of the target firm (in thousand EUR). CN is a dummy that equals 1 if the acquirer is a Chinese firm. $CNpriv$ is a dummy that equals 1 if the acquirer is a Chinese private firm, and $CNSOE$ is a dummy that equals 1 if the acquirer is a Chinese state-owned or state-controlled firm. ROA, $Ln(Equity)$ and $Leverage$ are the average value of return on assets, the natural logarithm of the book value of equity and total debt in total assets over the three years prior to the deal. $FullAC$ is a dummy variable indicating whether 100% of the target were acquired. $AnyPatent$ is a dummy indicating whether the target firm holds any patent. Standard errors are robust and clustered at the target firm level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 4.C2: Acquisition Effects of Chinese Acquirers

Dep. variable	Total Assets			Turnover		
	(1)	(2)	(3)	(4)	(5)	(6)
	Acquisition Year	One year after	Two years after	Acquisition Year	One year after	Two years after
CN	0.303*	0.121	0.108	0.277	0.376*	0.150
	(0.161)	(0.080)	(0.069)	(0.243)	(0.211)	(0.136)

Notes: This table contains estimation results on acquisition affects for Chinese companies using a difference-in differences approach. Dependent variable is the difference in the growth rate between treatment and control group in the respective variable. CN is a dummy that equals 1 if the acquirer is a Chinese firm. The control group is re-weighted using entropy balancing based on target firm characteristics (return on assets, ln(total assets), return on assets and industry group) and deal year. Standard errors are robust. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 4.C3:   Acquisition Effects of Chinese Private Firms and SOEs

Dep. variable	Capital Productivity 1			Capital Productivity 2			Av. CoE			Intangibles %		
	(1) Acquisition Year	(2) One year after	(3) Two years after	(4) Acquisition Year	(5) One year after	(6) Two years after	(7) Acquisition Year	(8) One year after	(9) Two years after	(10) Acquisition Year	(11) One year after	(12) Two years after
CNpriv	-0.215**	-0.054	-0.244	-0.065*	-0.021	-0.086	-2.371	0.66	1.383	0.013*	0.0137	0.018*
	(0.098)	(0.154)	(0.185)	(0.038)	(0.039)	(0.060)	(2.713)	(3.492)	(5.186)	(0.007)	(0.00835)	(0.010)
CNSOE	-0.146**	0.023	0.060	-0.063**	-0.062*	-0.042	1.122	3.657	13.46***	-0.008	-0.010	-0.0132
	(0.066)	(0.065)	(0.075)	(0.029)	(0.035)	(0.045)	(2.671)	(3.377)	(5.01)	(0.007)	(0.010)	(0.012)
Balanced on target firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced on target country controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table contains estimation results on acquisition affects for Chinese private firms and SOEs. CNSOE is a dummy that equals 1 if the acquirer is a Chinese state-owned or state-controlled firm. The control group is re-weighted using entropy balancing. Standard errors are robust. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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