

Three Essays in Applied Microeconomics

A Microeconomic Study of Snacks, Beer, and Real
Estate Agents

Berlin, Fall 2023

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Dissertation submitted to the Hertie School
in partial fulfilment of the requirements for the degree of

Doctor rerum politicarum (Dr. rer. pol)

in the

Doctoral Programme in Governance

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List of Papers

The Cost of Honesty: Field Evidence.

Published in the *Journal of Behavioral and Experimental Economics*, Volume 101, December 2022

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Pages: 1–24

How not to Reduce Commission Rates of Real Estate Agents

Published as a Working Paper in the *BSE Discussion Paper Series*, #0026

Authors: Julius Stoll

Pages: 25–58

Beer and Crime: Evidence from Germany, 1882-1913

Authors: Julius Stoll, Christian Traxler, and Carsten Burhop

Pages: 59–90

HERTIE SCHOOL

*Abstract***Three Essays in Applied Microeconomics**

by Julius STOLL

This dissertation comprises three essays. Although their titles make clear their self-contained nature, all essays share a common motivation: to combine economic theory with modern tools of causal inference to understand applied problems.

The first chapter studies honesty when the costs to behave honestly change. Using field data from a snack delivery company that employs an honesty payment system, the chapter presents an event study to analyze how price increases affect pay rates. The honesty payment system expects customers to pay a listed price for each consumed snack. This allows me to measure honesty with the pay rate that compares consumption to payments. The results, which draw on deliveries from several thousand firms, show that price increases that make honest behavior more costly cause more cheating. Price increases of 15% trigger a fall in pay rates of 11%.

The second chapter studies a recent legal reform in Germany, which aims to lower commission rates of real estate agents by raising the cost salience of sellers. I find that the reform has backfired and real estate agents have exploited the transition to increase their commission rates. The findings document that in some regions real estate agents increase their commission by up to 2 percentage points, adding over €6,000 in transaction cost to the average home sale. As explicit collusion is unlikely in this setting, I argue that this arbitrary increase points to seller ignorance instead. To verify if and why sellers fail to induce price competition, I run a pre-registered survey experiment with 1,062 real estate agents. Although commission rates should be negotiated independently for each sale, the survey confirms that 85% of sellers do not attempt to negotiate lower commission rates. The randomized experimental questions suggest that real estate agents may cater to the low willingness of sellers to negotiate by providing misleading reference commission rates and shrouding the economic incidence for sellers.

The third chapter is co-authored with Christian Traxler and Carsten Burhop and examines the causal effect of beer on crime based on unique panel data from Germany between 1882 and 1913. Using exogenous variation in the yield of spring barley, a key input in beer production, we identify a quantitatively and qualitatively significant effect of beer on violent crimes. This effect is mainly driven by a positive effect on assaults, where our findings suggest that a 1% increase in beer production raises assault rates by the same percentage. These findings are corroborated using a complementary empirical design that exploits a brewing tax reform in Prussia.

Acknowledgements

I am immensely grateful to my first supervisor Christian Traxler whose feedback guided many aspects of this dissertation. His rigorous comments on my methods, writing, and thinking pushed me to produce the best dissertation possible within my capabilities. In the same breath, I thank my other supervisor Egon Tripodi, whose good-hearted patience proved invaluable for the success of my first-off survey experiment. My dissertation has benefited greatly from his illuminating comments, the foresight of which I could sometimes only appreciate in retrospect. Likewise, I want to thank my next supervisor Dorothea Kübler. Her clear feedback pierced to the core of my work, providing me with a critical perspective to produce a well-rounded dissertation. Furthermore, I want to say thank you to my co-author Carsten Burhop for stimulating my work in economics and history alike.

Furthermore, I want to thank all those who inspired me with their refreshing kindness, be it by expressing their support after showing them my work or simply by sending me a helpful reference via e-mail on their own initiative. This rings especially true as the discipline of economics is not always welcoming to beginners. At my first presentation, a tenured academic thought that the best mode of feedback was to interrupt my talk by shouting that he disliked the wording on my second slide. Consequently, I am particularly thankful to all those who encouraged me with their well-articulated ideas and considerate suggestions along this journey.

While fun group pictures often grace the websites of PhD programs, doing a PhD is an inherently solitary exercise. As a result, I owe immense gratitude to my office colleagues Silke, Elena, and Anina, and all my other Berlin friends, like Julian and Christian, who always kept me on the bright side of life. In the same way, I am grateful to my parents who instilled in me the necessary assurance in my own capabilities that allowed me to pursue a doctorate.

Lastly, I thank my fiancée Lotti for her radiating support. Even after three years, she never fainted to show excitement when I presented her with a childish joy some freshly plotted graph. Being the best critic and supporter I could hope for, you provide the keystone to making these PhD years wholesome.

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Preface

What drives human behavior? This simple question underpins almost all policy challenges. Without a comprehensive understanding of how people perceive, interpret, and respond to different incentives and circumstances, policies risk unintended consequences or, as I demonstrate in this dissertation, even failure.

However, the scientific study of human behavior is complex. Surprisingly few certainties exist when trying to predict the perplexing variety of how people respond to the slightest changes in their environment. This already limited capacity diminishes even further when considering human reactions at a policy scale. The key reason for this high uncertainty arises from the nature of the subject. Most human activity unfolds within a complex web of individual preferences, societal norms, and sophisticated institutions. Unlike in most natural sciences, treatments and their effects are often particularly heterogeneous, conditional, and difficult to observe. For example, the positive impact of a new regulation might be magnified by a general shift in people's attitudes, while the negative consequences of another policy could remain completely hidden behind the veil of a global pandemic. As a result, researchers struggle to isolate cause and effect.

This used to make it harder to test a theory rather than to come up with a new one. Consequently, the economic discipline assumed a lot to predict human behavior but tested comparatively little over the past two centuries. However, recent advances in data availability and statistical techniques have tilted the scale, allowing us to empirically assess the predictions derived from economic theory. In that sense, the present work is a child of this trend. Culminating in the sub-discipline of Applied Microeconomics, this field has now brought forward a powerful toolkit, which I leverage in this dissertation. By extending economic theory with large-scale data and modern econometric techniques, all chapters in this dissertation examine causal relations within a wide set of socioeconomic settings: To what extent remain individuals honest when given the opportunity to gain a material incentive? Why do people go to multiple supermarkets to save 5 cents on a liter of milk but do not

appear to bother to compare prices when hiring a real estate agent? What are the long-term consequences of drugs on society?

The questions that I deal with illustrate that, at this point, theory alone can often only provide a framework to start considerations, but seldom provide definite answers for validating and quantifying causal relations. To fill this gap and establish conclusive evidence from there, the use of data is instrumental. This dissertation marks no exception, whether by utilizing large-scale contemporary data collected in a business environment (Chapters 1 and 2) or by exploiting newly digitized data from historical records (Chapter 3). On this foundation, the present dissertation provides new insights into how people react to changing incentives in different settings. Already published (Chapter 1) or close to the submission for publication in international economics journals (Chapters 2 and 3), these findings aim to add to the current academic literature.

Investigating a snack delivery company that employs an honesty payment system in Chapter 1 allows me to study honesty in a real-world setting. My results on the effects of incentives on honesty complement other findings from studies involving lab experiments. Showing that price increases trigger strong decreases in pay rates, I offer evidence in support of more recent experimental reviews illustrating that incentives may matter more than what most lab studies suggested in the past. Chapter 2 brings a new perspective to the literature on price competition among real estate agents. Studying a legal reform that partially shifts the payment of the real estate agent from buyers to sellers, I show that the legal reform missed its original goal of lowering commission rates by raising the cost salience of sellers. Furthermore, my results indicate that housing prices around the reform do not adequately adjust to the shift of the commission payment, indicating that the traditional assumption on the irrelevance of a tax-like incidence may not hold as unconditional as described in economics textbooks. Chapter 3 extends the surprisingly scant literature on the causal effect of alcohol on crime. Exploiting the lack of economic integration in the German Empire between 1882 and 1913, we use natural variation in barley yield to test if exogenous changes in beer production produce changes in crime. Besides supplying an entirely new data set on Prussia and the German Empire, our work emphasizes that alcohol predominantly affects rates of violent crime. By contributing to debates on the economics of honesty, the influence of behavioral consumers on price competition, and the causal effects of alcohol on crime, this dissertation offers new implications and methods that provide fertile ground for future research.

Moreover, the present work strives to contribute beyond the academic discourse. Attending academic seminars and following the current academic literature can leave the impression that the claim for policy relevance is partly downgraded to a footnote. However, the goal of my doctorate was not to pursue a self-referencing past time but to constantly link all findings to their potential applications in the real world. Showing that small material incentives can encourage dishonesty (Chapter 1) holds an important lesson for practitioners who design regulations that require

a trade-off between investing in monitoring and the costs of fraud. Demonstrating that a reform which neglects best practices to lower commissions of real estate agents backfires (Chapter 2), my work highlights an easy-to-implement policy opportunity to improve affordable homeownership. Illustrating the crime-inducing effects of beer production in the context of an emerging country around the turn of the 20th century, Chapter 3 showcases how restricting the availability of alcohol represents a potentially underestimated tool to reduce violent crime.

Taken together, the chapters in this dissertation strive to fill in some of the gaps that remain for a thorough understanding of human behavior and its implication for policy. Since each research stage is subject to natural limits of data availability, time constraints, and the specific requirements of the problem in question, the methodological plurality used in this dissertation exemplifies the benefits of utilizing different identification strategies for each research process.

While the event study in Chapter 2 uses over 500,000 listings to provide additional insights, this analysis also provoked new questions that I could not have come close to answering without the subsequent survey experiment on a much smaller scale. Similarly, pairing the instrumental variable framework in Chapter 3 with a Diff-in-Diff study of a brewing tax reform enabled us to validate the findings in a way neither approach could have achieved by itself. Leveraging these diverse empirical techniques across chapters not only extends the academic scope but also helps to probe individual research questions with greater depth. Amid the uncertainties that accompany the transfer of research findings into successful policies, understanding human behavior even better is paramount. The present work marks an attempt towards this goal.

Chapter 1

The Cost of Honesty: Field Evidence

A version of this chapter was published in the *Journal of Behavioral and Experimental Economics*, 101 in December 2022. This version is embargoed for publication until December 2024 and therefore not included in this dissertation version.

Chapter 2

How not to Reduce Commission Rates of Real Estate Agents

The survey experiment in this chapter was pre-registered in the OSF registry (doi: 10.17605/OSF.IO/C7WDH) and reviewed and approved in advance by the Associate for Research Ethics and Integrity at Hertie School (ID:20220920-03).

2.1 Introduction

The preferences for homeownership are unbroken around the world. For example, respondents in the US rank owning a home as a higher gauge for prosperity than a career, having children, or a college education.¹ Even in countries with a long tradition of renting, such as Germany, approximately 70% of tenants would prefer owning their homes.² High transaction costs pose a direct challenge to this desire for homeownership. Aside from real estate transfer taxes and stamp duties, commission fees for real estate agents represent a large portion of these transaction costs.

The level of commission fees is typically not regulated. Assuming that real estate agents operate in a competitive market, one would therefore expect the commission to approximate the marginal production cost of a real estate agent to fulfill a given transaction. Considering the technological advancements of the past two decades, these innovations should have brought down cost and, in turn, the commission fees of real estate agents: large databases help to determine a reasonable asking price, artificial intelligence and smartphones assist in producing object descriptions and images, and online tools can automate the scheduling for viewings.

The developments in several countries affirm that such cost-lowering innovations can translate into lower commission rates. For example, Dutch real estate agents now charge around 1.25% of the selling price, and some online real estate agents even offer a flat fee full-service package for less than €2,000.³ However, in other countries, the commission rates of real estate agents refuse to decrease: often adding 4% to 6% to the final price of a property, commission rates in France, Germany, or the US remain three to four times higher than in the Netherlands, the UK, or most of Scandinavia (Table 2.1).

¹The YouGov survey was based on a sample of 2,530 respondents representative for the U.S. and commissioned by the financial provider *Bankrate*.

²The survey was based on a sample of 2,180 respondents from a representative online panel in Germany. The sample was collected in 2022, and the survey was commissioned by the financial provider *Interhyp*.

³Based on an offer from the Dutch agency *maakelarsland.nl* in April 2023. The flat fee of €1,950 includes the agency analyzing and pricing the home, a professional photographer taking pictures, listing the home on the relevant platform, conducting viewings, as well as assisting with negotiations and legal matters. In sum, the flat fee includes all the service components that most real estate agents provide around the world.

Table 2.1: Average Total Commission Rates per Transaction Around the World

Country	Commission rate (2022)	Commission rate (2002)	Housing prices to income ratio (2020)	Buying Agent	Commission paid by
Netherlands	1.25	1.75	11.4	No	Seller
Norway	1.5	2.5	8.7	No	Seller
Sweden	1.5	5	9.0	No	Seller
U.K.	1.5	1.5	12.7	No	Seller
Ireland	1.75	1.75	8.5	No	Seller
Australia	2.5	2.5	12.7	No	Seller
Denmark	2.5	3	9.2	No	Seller
Finland	3	4	6.2	No	Seller
Belgium	3	3	8.2	No	Seller
Indonesia	3	5	-	No	Seller
Spain	3	5	8.4	No	Seller
Mexico	3.5	7.5	-	No	Seller
Greece	4	4	-	No	Seller or buyer
Israel	4	4	-	No	Split
Russia	4	7.5	-	No	Seller
Canada	5	4.5	11.5	Partially	Seller
France	5	-	9.2	No	Seller or buyer
Japan	5	3	-	Yes	Split
Germany	5.5	4.5	12.0	No	Split
Italy	6	5	8.1	No	Seller or buyer
U.S.	6	6	7.4	Yes	Seller

Note: This table displays average total commission rates per transaction. The data from 2002 are reproduced from Delcoure and Miller (2002). Commissions from 2022 and information on the national systems are collected using web queries from at least three local websites for each country that provided information on customary commission rates no older than two years. Buying agent ‘Yes’ indicates widespread use of additional buying agents. Housing price-to-income ratios are calculated by dividing 2020’s average transaction prices by median incomes. Average housing prices are sourced from national statistical offices or, if not available, other web sources. Median incomes are taken from Eurostat (2023). The price-to-income ratios seek to illustrate that national housing prices are comparable across countries and are unlikely to explain the much larger gaps in commission rates.

This divergence adds to long-standing doubts⁴ if commission rates in such high-commission countries are determined competitively. As technological innovation combined with low entry barriers and the decentralized market structure of real estate agents should drive down rates, their persistence in some regions motivates a closer inspection.

In this paper, I focus on Germany, which has one of the consistently highest commission rates in the world. My study leverages a legal reform that explicitly aims

⁴These concerns are old (Crockett, 1982) and typically derived from the unresponsiveness of commission rates to changes in residential real estate prices, the resulting excess entry (Hsieh & Moretti, 2003; Han & Hong, 2011; Barwick & Pathak, 2015), and the uniformity of commission rates (Owen & Kickbacks, 1977; Miceli, 1992; Hatfield, Kominers, & Lowery, 2019). Adequate explanations exist only for the US, indicating barriers for low-commission real estate agents to access the relevant listing platforms (Hahn, Litan, & Gurman, 2006) and collusive steering (Levitt, Syverson, & Ferreira, 2008; Barwick, Pathak, & Wong, 2017), where the practice of using a buying agent who is compensated with a share of the selling agent’s commission incentivizes buying agents to steer customers away from low-commission listings.

to increase price competition. As in most countries, real estate agents in Germany are predominantly selling agents, i.e. they are initially contracted by the seller and incentivized by the commission to work in the interest of the seller. However, before the reform, the commission for the selling agent was often charged only from the buyer. As this practice allowed real estate agents to claim that the service for sellers was ‘free of charge’, this custom raised concerns about sellers failing to exert sufficient competitive pressure on commission rates.⁵ To change this, the reform required sellers who commission a selling agent to pay at least half of the statutory commission fee. Echoing recent findings on the incomplete salience of taxes and tax-like costs (e.g. Chetty, Looney, & Kroft, 2009; Bradley & Feldman, 2020), German policy makers reckoned that forcing sellers to partially pay the commission explicitly would make the cost more visible:

“[With the reform,] everyone, who commissions [a real estate agent] becomes aware, that he can maximally pass through half [of the commission]. The seller has an incentive that prices and thus commission rates decrease. This creates competition.”⁶

I study this policy by examining half a million listings around the reform. My results demonstrate that the policy not only failed to lower commission rates but even backfired. Using an event study design, I reveal that total commission rates have strongly increased in response to the reform. The documented increase is robust to controlling for a wide set of property characteristics to ensure that the finding is not driven by a changing composition of the listed properties around the reform.

A heterogeneity analysis shows that this increase follows a simple strategy that is tied to the regionally typical commission rates. Pre-reform, the commission rates paid by buyers were most frequently 4% or 5% of the property price in low-commission regions, and 6% in regions with the highest commissions. Post-reform, many agents switched to charging 3% from buyers and sellers each, often increasing the total commission rate from 4% or 5% to 6%. These increases are substantial: applied to the average home price in the sample, raising commission rates of 4% or 5% to 6% corresponds to respective additional transaction cost of approximately €6,000 or €3,000, almost overnight.

I provide complementary evidence that this increase does not follow conventional explanations. The reform merely shifted the statutory payment but should have left other market conditions unchanged. Using a traditional incidence framework, I illustrate that standard economic theory would render the shifting statutory incidence irrelevant for all parties. The evident increase also rejects the idea underpinning the legal reform: partially increased cost salience for sellers does not appear

⁵I return to discussing the economic incidence of the commission below.

⁶Karsten Möring, member of the German parliament for the CDU party at the 160th session of the federal parliament on 14 May 2020 when debating the reform. Original: *“Es ist die Tatsache, dass wir nicht das Bestellerprinzip in Reinkultur einführen, sondern jeden, der bestellt, davon in Kenntnis setzen, dass er maximal die Hälfte davon weitergeben kann. Er hat ein Interesse daran, dass die Preise, die Provision sinken. Damit entsteht Wettbewerb.”*

to stimulate price competition in my setting. Lastly, the data indicate that neither the industry structure nor other empirical observations support explicit collusion of real estate agents as a convincing alternative explanation.

Instead, my study emphasizes the passive role of home sellers: sellers can negotiate the commission freely with the agent and my results indicate that sellers bear most of the economic commission cost; nevertheless, the evidence suggest that sellers appear surprisingly inelastic to the price increase. I offer two complementary interpretations to explain sellers' indifference. First, sellers could underestimate their burden of the commission incidence. The incidence framework illustrates that if sellers believe that real estate agents can pass all the commission costs to buyers in the form of a higher selling price, then sellers might underestimate the economic commission cost. Second, sellers' inattention to commission rates may be fueled by the influence of reference rates. I show that the reference commission rate used by real estate agents is not only inflated but can also explain the path of the commission rate increases which cascade from high- to low-commission regions.

I validate these hypotheses in a survey experiment among 1,062 real estate agents. First and foremost, responses indicate that sellers appear perplexingly unwilling to seek lower commissions: 85% of sellers do not attempt to negotiate lower rates. Failing to demand a lower commission is most prevalent among female and older home sellers. Randomized experimental questions further indicate that real estate agents inflate the reference commission rate and expect their ability to fetch a higher price to transcend any incidence effects. Incentivizing accurate answers deflates both estimates, suggesting that real estate agents communicate inaccurate information by default.

My paper offers three contributions to the literature. First, my findings speak to the specific research on real estate agents. By emphasizing the low willingness of home sellers to seek lower commissions, the present study provides a new angle to the old puzzle of constant and unresponsive commission rates. Providing evidence on a price increase without changes in market conditions, my paper substantiates concerns that also outside the US, rates are not determined competitively (e.g. Crockett, 1982; Barwick et al., 2017; Barwick & Wong, 2019). Furthermore, comparing listing prices to owners selling directly supports the findings on the underwhelming performance of real estate agents in general (Rutherford, Springer, & Yavas, 2005; Levitt & Syverson, 2008; P. Jia & Pathak, 2010).

Second, the comparison of listing prices to offers without a commission also contributes to measuring the neutrality of the statutory incidence (Kopczuk, Marion, Muehlegger, & Slemrod, 2016; Benzarti, Carloni, Harju, & Kosonen, 2020). The commission rate for real estate agents partially resembles a tax by creating a wedge between prices for buyers and sellers. However, the statutory commission rate shift studied in this paper is not fully offset by higher listing prices. Thus, my results also speak to the mounting evidence of incomplete tax salience (Chetty et al., 2009; Finkelstein, 2009; Bradley & Feldman, 2020).

Third, I demonstrate that markets prevail in which many consumers leave serious money on the table. Hereby, the study intersects with the literature on consumer inattention (e.g. Gabaix & Laibson, 2006; Finkelstein, 2009; Bhargava, Loewenstein, & Sydnor, 2017; Bordalo, Gennaioli, & Shleifer, 2019) and the problematic behavior of commission-motivated agents (Levitt et al., 2008; Anagol, Cole, & Sarkar, 2017). The present paper supports the notion that such problems are especially acute in the domain of real estate (Simonsohn & Loewenstein, 2006; Woodward & Hall, 2012; Bradley, 2017; Repetto & Solís, 2019; Agarwal & Karapetyan, 2022) and among vulnerable consumer groups, such as women and the elderly. However, the reform was introduced with the ulterior motive that home sellers better respond to salient cues. One would have expected that forcing sellers to explicitly pay part of the commission would have made cost more salient (Bradley & Feldman, 2020). However, my research shows that this logic did not materialize in the specific context of splitting the commission payment. Although the results point to inattentive home sellers, the unexpected consequences of the reform offer a caveat about predicting the interaction of the statutory incidence with salience on a policy scale.

Ultimately, the perplexing commission increase following the reform relates the commission rates in Germany to a policy choice. This finding aligns with international observations, where high-commission countries either have confusing commission systems where both sellers and buyers may pay the selling agent or, alternatively, use systems that incentivize the involvement of an extra buying agent, like in the US (Table 2.1). By contrast, other global examples point to simpler and less bureaucratic policy solutions: countries with low commissions all share systems with just a selling agent who is only paid for by the seller.

2.2 Institutional Background

2.2.1 Market Characteristics

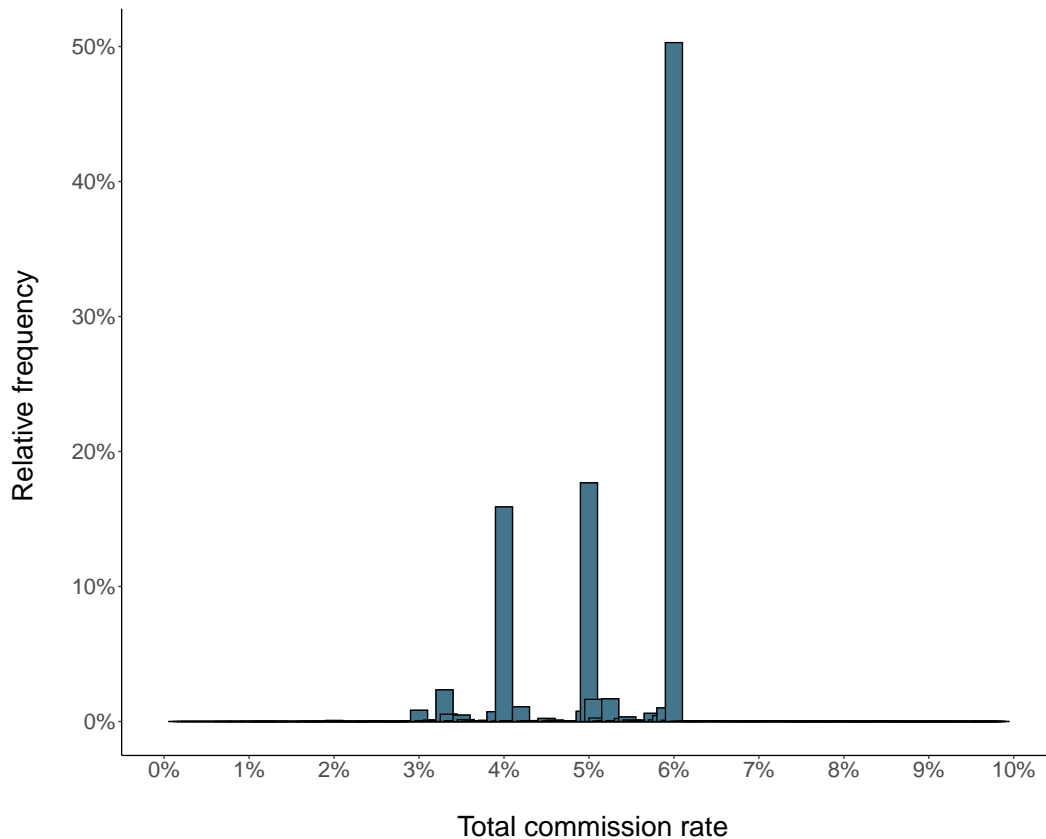
Real estate agents play an important role in German housing markets (Figure B.4). As in most countries, real estate agents in Germany are generally selling agents, meaning that they are initially hired by sellers to list and show the property to potential buyers. Unlike in the US, buyers in Germany search for properties on their own and almost never hire a buying agent. Real estate agents and private sellers alike use independent third-party platforms to offer properties.

Selling agents receive a commission for their service. The commission is a percentage value, henceforth referred to as ‘commission rate’, of the final selling price.⁷ Although all contracts are purportedly negotiated independently, nearly all total commission rates are either 4%, 5%, or 6% of the final selling price (Figure 2.1). These

⁷Fixed price commissions are unusual in Germany.

rates exclude an additional 19% (16%)⁸ VAT. For ease of presentation, all the commission rates are displayed excluding the respective VATs throughout the paper.

Figure 2.1: Histogram of Total Commission Rates in the Sample



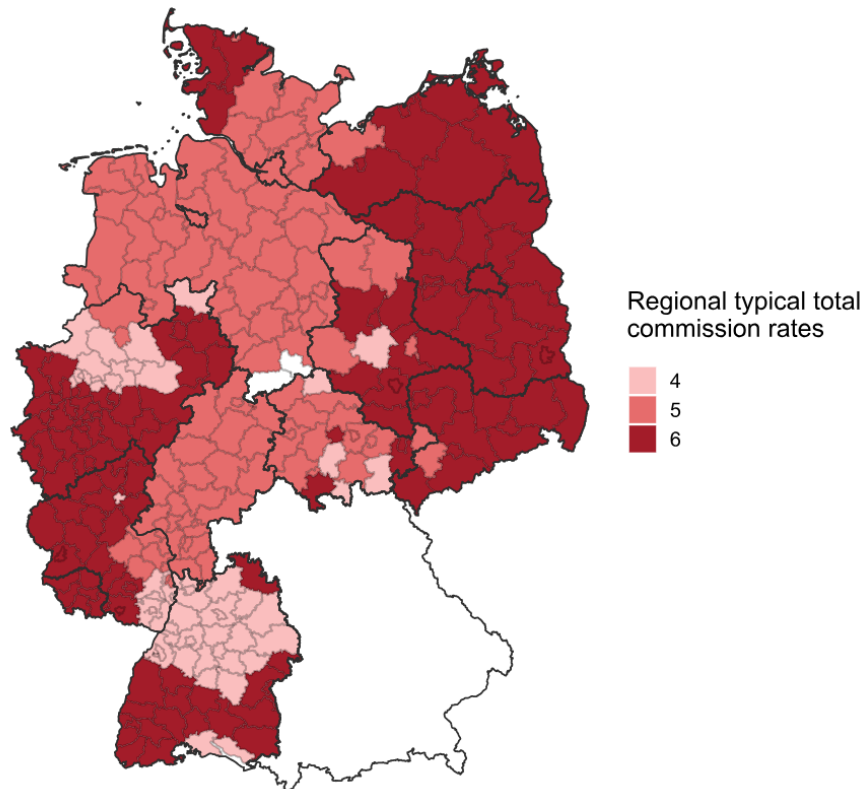
Note: This figure displays the distribution of total commission rates in Germany over the entire sample period. While the majority of real estate agents either offers 4%, 5%, or 6% commissions, a small share of real estate agents offers lower rates.

These values are often communicated as the respective *regionally typical* commission rate ('ortsübliche Provision'). However, the meaning of this *regionally typical* rate is atypical: the *regionally typical* commission rate seems to refer to the most frequent commission rate (modal value) rather than the arithmetic mean. Furthermore, the different *regionally typical* commission rates do not appear to be tied to any market characteristics; for instance, although one would expect that regions with higher real estate prices would feature lower commission rates, these or similar correlations are not reflected in the data (Figure 2.2). Interestingly, the empirical distribution of commission rates is left-skewed: as several real estate agents offer rates below the

⁸As part of the second COVID-19 relief package, Germany temporarily reduced the VAT rate from 19% to 16% between July and December 2021, that applies to the commission rates of real estate agents. Theoretically, this may affect the demand for real estate agents and housing. However, the empirical analysis shows that the former remains constant, whereas the impact of the latter is marginal: reducing the commission rate of on average 5% by the change in VAT of 3% will unlikely translate into a noticeable price effect for housing.

typical rate, the communicated *regionally typical* rate is almost always higher than the actual average commission rate.

Figure 2.2: District Variation of the Most Frequent Pre-Reform Commissions Rates



Note: The map depicts the *regionally typical* commission rates based on all the listings before the reform in December 2020. Commission rates for the sellers in Bavaria cannot be observed and are therefore excluded.

By international standards, commission rates in Germany are high (Table 2.1). Compared to structurally similar countries such as the Netherlands and the UK, or countries in northern Europe, commission rates in Germany are two to four times as large for equivalent service offerings, despite similar housing price-to-income ratios (Table 2.1). This notable disparity raises questions about whether typical commission rates represent an equilibrium of a perfectly competitive market and how policy could influence this outcome.

2.2.2 Legal Reform and Statutory Incidence

While most countries around the world require the seller, who typically hires the selling agent, to also pay the commission, Germany is an outlier. Before the studied reform, sellers could hire their selling agent but could make the buyer pay for the selling agent's entire commission.⁹ Although a few sellers paid the commission

⁹Bavaria followed a special path: Although the payment of the commission was also not regulated, even before the reform commissions were regularly split. Nonetheless, the seller's share was not required to match the commission rate of the buyer. This custom made the seller's commission private

themselves, most sellers made buyers pay the entire commission of the selling agent.

This practice was considered problematic for several reasons. First, the setting may undermine the buyers' efforts to demand a lower commission rate, as real estate agents can exclude potential buyers who seek to negotiate the initially asked commission rate. Second, the system was believed to make the commission cost not salient enough to sellers who are in a better position to demand lower rates. In response, the German federal government passed a reform with the explicit aim of "protect[ing] buyers from exploitation of this [contractual] predicament" (BMJ, 2019) and raising the cost salience for sellers.

The reform defines who pays the statutory commission cost; it was enacted in May 2020 and came into effect as of late December 2020. The original intent of the law was to require only sellers who commission the real estate agent to pay the entire commission (*'Bestellerprinzip'*); however, a compromise was passed to prohibit sellers from making buyers pay more than half of the total commission rate. When establishing a contract with the real estate agent, sellers are now restricted to pay the entire commission themselves or split the commission with the buyer.¹⁰ In practice, sellers now predominantly split the commission equally with the buyer.¹¹ How would this reform affect typical commission rates and selling prices? In the following, I outline a theoretical framework to provide a baseline prediction before empirically studying the impact of the reform in Section 2.4.

2.3 Theoretical Framework

2.3.1 Prices for Buyers and Sellers

As a starting point, consider the listings for sale by owner, which are not subject to any commission. Let $D(p)$ denote the buyers' demand, and let $S(p)$ signify the supply as a function of price p . $D(p) = S(p)$ yields the equilibrium selling price $p = p'$ when an object is sold directly by an owner.

Introducing a real estate agent adds a commission rate that applies to the final selling price. The total commission rate can be treated as an *ad valorem* tax τ_t , where $t \in \{pre, post\}$ denotes the period before or after the reform. For the moment, I hold the commission rate τ_t fixed around the reform, $\tau_{pre} = \tau_{post}$. To allow for the shifting of the statutory incidence of the total commission rate τ_t between buyers and sellers, the total commission rate τ_t may be split up between buyers τ_t^B and sellers τ_t^S , such that $\tau_t = \tau_t^B + \tau_t^S$.

information, thus deterring the reliable estimation of the pre-reform level of the total commission rate in Bavaria.

¹⁰Theoretically, other splits are possible whereby sellers pay a higher share than buyers. However, in practice, this approach is virtually never used.

¹¹To enforce that agents do not offer hidden discounts to sellers, buyers must pay their share only after being presented a receipt of the seller's payment. Some real estate agents claim that this additional bureaucracy leads to later payments. However, as this delay concerns merely a few weeks, the additional financial cost can be considered negligible.

Prior to the reform, the seller formally made the buyer pay the entire commission, or $\tau_{pre}^S = 0$ and $\tau_{pre}^B = \tau_{pre}$. Note that the commission rate creates a wedge between the price that a buyer must pay $p_{pre}^B = (1 + \tau_{pre}^B)p_{pre}$ and the price that a seller receives $p_{pre}^S = p_{pre}$. Setting $D((1 + \tau_{pre}^B)p_{pre}) = S(p_{pre})$ yields the market clearing price with a real estate agent $p = p_{pre}^*$ before the reform. Assuming that the conditions of a competitive market hold, the distribution of the economic incidence of the commission in equilibrium depends only on the respective elasticities of buyers $\eta^B = (\partial D / \partial p_{pre}^B)(p_{pre}^B / D)$ and sellers $\eta^S = (\partial D / \partial p_{pre}^S)(p_{pre}^S / S)$. Although the buyer formally pays the commission, part of the burden may be passed through to sellers even before the reform.

After the reform, the statutory incidence of the commission rate partially shifts from buyers to sellers. In practice, this requirement means that the statutory payment of the commission rate is split equally. The commission rate consequently changes to $\tau_{post}^B = \tau_{post}^S = \frac{1}{2}\tau_{post}$, which transforms the prices of buyers and sellers to represent the commission-inclusive prices $p_{post}^B = (1 + \frac{1}{2}\tau_{post})p_{post}$ and $p_{post}^S = (1 - \frac{1}{2}\tau_{post})p_{post}$ after the reform.

From a traditional incidence perspective, this partial shift of the statutory incidence is irrelevant: the economic incidence between the parties should be independent of who pays the commission. Although the statutory shift is predicted to alter the commission-exclusive equilibrium price with a real estate agent, that is, the listing price, such that $p_{post}^* \neq p_{pre}^*$, the commission-inclusive prices that home sellers receive and buyers pay should remain unaffected.

PREDICTION 1: Commission-inclusive prices around the reform remain equal or $p_{pre}^{*S} = p_{post}^{*S}$ and $p_{pre}^{*B} = p_{post}^{*B}$.

A possible complication arises from the nature of the commission rate. The model assumes a fixed rate thus far. However, in contrast to a tax rate that is traditionally set exogenously, the commission rate τ_t is a market outcome shaped by the interaction between the seller and the real estate agent. This requires a more in-depth analysis of the role of home sellers and real estate agents.

2.3.2 The Incidence of the Commission

In a simple framework, the home seller values the real estate transaction according to a utility function that is quasi-linear around the value of the property,

$$U_{pre}^S = p'_{pre} - a \quad (2.1)$$

where p'_{pre} again denotes the equilibrium price when not using a real estate agent and a represents the transaction cost to the seller without assistance.¹² Potential

¹²Assume that a follows some distribution F that reflects the different transaction cost to different sellers. For example, sometimes a seller might want to list an object inherited from a relative living next door, but another seller might have moved to a location afar from the object for sale, which makes

buyers are expected to be indifferent to an object presented by the owner directly or through a real estate agent. Before the reform, involving a real estate agent replaces the transaction cost a with the commissioning cost of the real estate agent. This changes the seller's calculation to

$$U_{pre}^S = p_{pre}^{*S} \quad (2.2)$$

where p_{pre}^{*S} denotes the commission-inclusive price received by the seller. Even though the buyer formally pays the commission, the seller must account for this indirect commission cost. Assuming that neither the supply from sellers is perfectly elastic ($\eta^S < \infty$) nor that the buyers' demand is completely inelastic ($\eta^B > 0$), sellers should expect their commission-inclusive selling price with a real estate agent to be lower than when selling alone, $p_{pre}^{*S} < p'_{pre}$. The difference between the equilibrium price without a real estate agent p'_{pre} (when $\tau = 0$) and the equilibrium price with a real estate agent p_{pre}^{*S} represents the effective commission cost borne by the seller, or $p'_{pre} - p_{pre}^{*S}$. To highlight how this commission incidence is composed, one can express the seller's calculation also as

$$U_{pre}^S = p_{pre}^{*S} \equiv p'_{pre} - p_{pre}^* \tau_{pre} \gamma^S \quad (2.3)$$

where $p_{pre}^* \tau_{pre}$ represents the total commission cost of which the seller bears the pass-through rate denoted as γ^S , with $\gamma^S = 1$ if the entire commission is passed through to the seller and $\gamma^S = 0$ if the seller bears none of the commission. Thus, Equation 2.3 illustrates that γ^S is critical to the utility of the seller.

Under a rational choice framework, a seller would commission a real estate agent when the cost a of selling alone is larger than the total commission cost induced by a real estate agent $a > p'_{pre} - p_{pre}^S$. Note that the commission burden to the seller should only depend on the economic incidence. After the reform that shifts half of the statutory incidence toward the seller, the pass-through of the economic incidence of a seller γ^S should remain the same. Similarly, as long as the commission cost $p_{pre}^* \tau_t$ remain constant, the decision of a seller to commission a real estate agent should be independent of the reform.

2.3.3 Commission Rates

Next, we must acknowledge that the commission rate τ_t is a market outcome. This raises the question of whether τ_t remains constant around the reform. In simple terms, the utility of the real estate agent can be denoted as the revenue, $p_t \tau_t$, minus the cost c of the transaction:

$$U_t^A = p_t \tau_t - c \quad (2.4)$$

conducting viewings prohibitively costly. A heterogeneous distribution of a in the seller population predicts why some sellers list objects on their own instead of using an agent.

Assuming that the pre-reform commission rate τ_{pre} reflects the equilibrium rate, the incidence analysis suggests that the utility of buyers and sellers should not change around the reform. Since the commission burden is determined only by the elasticity of sellers and buyers, the sellers' commission burden and their demand for the services of real estate agents remain the same after the reform. Moreover, the statutory shift leaves the cost of real estate agents unaffected, implying that their supply remains unaffected. Even though the incidence framework is primarily established in the context of an exogenously set *ad valorem* tax, the statutory reform should not affect any of the endogenous forces that determine the interaction between buyers, sellers, and real estate agents. Thus, the subsequent analysis follows the principle of the liability side equivalence and assumes that no one is better or worse off as a result of the reform.

However, note that a real estate agent's revenue relies on the equilibrium price p_{pre}^* to which the commission rate τ_{pre} applies. Pre-reform, sellers did not bear any statutory commission cost, making the equilibrium price with a real estate agent p_{pre} equal to the price that a seller receives, $p_{pre} = p_{pre}^S$. Post-reform, sellers pay half of the statutory commission. Assuming that the elasticities for buyers and sellers remain constant, the additional statutory commission cost are added to the commission-exclusive selling price. This leads to the new equilibrium selling price p_{post}^* that ensures that sellers receive the same commission-inclusive price as before. In other words, while the framework predicts commission-inclusive prices for sellers to remain the same around the reform $p_{pre}^{*S} = p_{post}^{*S}$, the new equilibrium selling price of an object p_{post}^* must increase by the fraction of the statutory commission rate $\frac{1}{2}\tau_t$ that sellers now pay.

The adjustment of the commission-exclusive selling price p_{post}^* ensures that buyers and sellers receive the same prices post-reform. However, note that this increase would affect the earnings of a real estate agent, as the commission rate τ_{pre} applies to the higher equilibrium price $p_{post}^* = (1 + \frac{1}{2}\tau_{pre})p_{pre}^*$. This increase by $1 + \frac{1}{2}\tau_{pre}$ would raise the earnings of a real estate agent by the same proportion. Yet, within the assumptions of a traditional incidence framework, the nature of the reform should not affect the market power of sellers, buyers, or real estate agents. Consequently, the market conditions and nominal revenues of real estate agents are expected to remain the same.

For this case to hold, the commission rate τ_{post} must decrease to offset the expected higher selling price p_{post}^* to which the commission rate applies. Since the commission-exclusive selling price increases to $p_{post}^* = (1 + \frac{1}{2}\tau_t)p_{pre}^*$, the post-reform commission rate must decrease proportionately, that is $\tau_{post} = \frac{\tau_{pre}}{1 + \frac{1}{2}\tau_{pre}}$. This prediction upholds the liability side equivalence, ensuring that a statutory shift, which should not affect the market conditions for any party, leaves the outcome for all parties unaffected. Nevertheless, in Germany, where the size of τ_{pre} typically ranges

between 4% to 6%, the expected change in τ_{post} is relatively minor.¹³

PREDICTION 2: Nominal commission rates are slightly lower after the reform or

$$\tau_{post} = \frac{\tau_{pre}}{1 + \frac{1}{2}\tau_{pre}}.$$

The incidence framework and its predictions provide a benchmark from the perspective of standard economic theory. In the next section, I empirically test the commission rate and the commission-inclusive prices around the reform to examine the predictions of the standard model against alternative interpretations.

2.4 Evidence From a Legal Reform in Germany

2.4.1 Data

To analyze the reform, I use data from the leading real estate platform in Germany. The data are provided by ImmobilienScout24 and kindly published by the RWI Essen (Schaffner, 2022). This sample offers the largest base of detailed listings, which also maintains consistent records of seller types and commission rates. Another strength of the data is the mixed-user base. The platform is popular among real estate agents and for sale by owners.¹⁴ Nonetheless, listings offered by real estate agents dominate and do so consistently (Figure B.4). Featuring around half a million listings between January 2018 and June 2022, the data provide a comprehensive overview of real estate listings for the 18 months before and after the reform (Table 2.2).

¹³For instance, assuming that selling prices p_{post}^* fully adjust to the statutory shift, a pre-reform commission of $\tau_{pre} = 6\%$ would be expected to decrease to $\tau_{post} = 5.825\%$. Table B.1 provides a numerical example to illustrate the incidence framework in more detail.

¹⁴Opposed to e.g. the Netherlands, a property listed by an owner instead of a real estate agent is not perceived as suspicious and considered normal. This is important for the later analysis where I compare prices of objects listed by a real estate agents to those listed by owners directly.

Table 2.2: Summary Statistics of Listing Characteristics

	Full Sample				Houses	Flats
	Mean	Std. Dev.	Min	Max	Mean	Mean
Price (€)	336,484	233,137	20,001	2,499,000	402,333	280,291
Floor space	108.50	49.49	15.00	250.00	144.07	78.15
Price per m ²	3,210	1,670	97.51	11,995	2,830	3,535
Construction year	1961	40.48	1000	2018	1958	1963
Rooms	4.00	1.88	1	15	5.38	2.82
Time on market (in days)	31.36	37.30	1	365	31.90	30.90
Listing offered by real estate agent (0/1)	0.87	0.34	0	1	0.87	0.86
Commission rate* (in %)	5.27	0.92	0	12.56	5.27	5.28
Observations	560,160				257,895	302,211

Note: *Commission rate only considers listings by real estate agents ($n = 484,964$) and depicts the net total commission rate excluding VAT as percentage value of the final selling price.

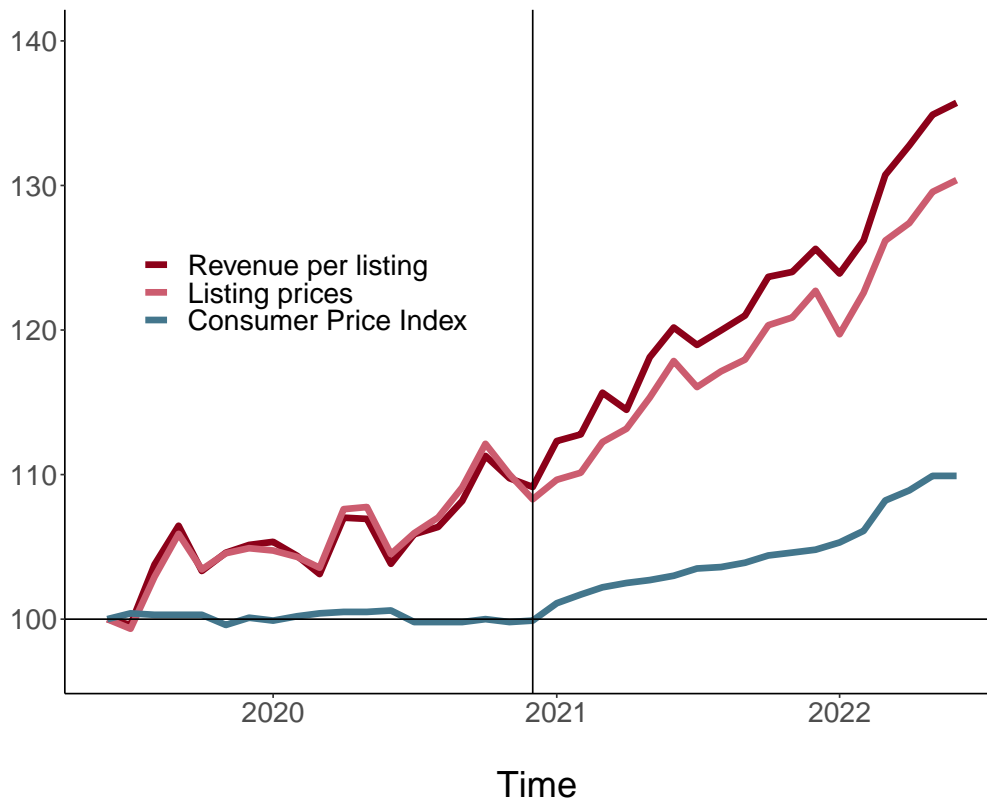
Other variables used in the analysis include the type of property (house or flat), numerous categories for each type (e.g. semi-detached or single-family for houses; ground-floor apartment or penthouse), multiple levels of furnishing and condition, and other individual characteristics, e.g. availability of a balcony or a parking space.

The purpose of the analysis naturally implies certain restrictions on the data. As the reform only applies to non-commercial transactions of residential property between private individuals, offers by commercial actors and developers are omitted. Thus, I only consider constructed apartments and houses. For the same reason, I exclude the small share of listings featuring specific property types unaffected by the reform, such as entire apartment buildings with multiple flats. Listings where sellers pay the entire commission and the level of commission rates is unknown are excluded. For similar reasons, Bavaria is not included in the sample: even before the reform, commissions were split but not always equal. Since this makes the share of sellers private information, total rates cannot be observed before the reform. Furthermore, one must note that the detailed property characteristics of each listing are not obtained from official records but are entered manually. Therefore, some listings display improbable values, typos, or plain errors. These require further processing described in Appendix I.

The resulting sample appears to represent the real estate market in Germany well. Although the platform is marketed toward more urban and premium customers, this imbalance is only partially reflected in the sample. A comparison of average prices in the sample to the notarized prices of aggregated real estate transactions in Germany indicates proportional price levels across regions (Figure B.1).

Descriptively plotting the trend of prices and commission revenues ($\tau \times p$) of each listing in Figure 2.3 provides first indications of increasing commission rates.

Figure 2.3: Commission Revenue per Transaction Over Time



Note: The figure describes the approximate revenues ($p \times \tau$) of real estate agents per listing in the sample against the trajectory of housing prices and the official consumer price index for Germany (Destatis, 2023). All trends have July 2019 as starting point (= 100). Next to the windfalls from rising housing prices, revenues begin to decouple after the reform, pointing to an increase in average commission rates beyond the rate of inflation.

Even in the short period before the reform, the increase in housing prices exceeds the rate of inflation, generating large windfall revenues per object for real estate agents.¹⁵ Moreover, from the reform onward, commission revenues outpace the increase in housing prices, pointing to rising commission rates. Whereas commission revenues averaged €16,786 in the beginning of the sample in June 2019, they increased disproportionately stronger to €20,873 by June 2022. Taken together, these initial observations provide a strong motivation for a closer examination of commission rates around the reform. Moreover, the descriptive nature of these observations requires careful inspection. For example, the evidence in Figure 2.3 cannot rule out that the increase in commission rates is caused by a change in the characteristics of the offered housing stock which might correlate with commission rates. To address this shortcoming, I perform an event study in the next section.

¹⁵Note that my figure only displays the tail of a long trend. Real house prices in Germany increased by 30% since 2010.

2.4.2 Method

Beginning with the commission rate as dependent variable, I estimate the equation:

$$\tau_{i,t} = \sum_{t=-18}^{-8} \delta_t m_t^{pre} + \sum_{t=-6}^{18} \delta_t m_t^{post} + \alpha X_i + \lambda_r + \epsilon_{i,r,t} \quad (2.5)$$

where the m_t^{pre} and m_t^{post} respectively represent the key dummies for the 18 months before and after the reform. The coefficients δ_t are the key estimates, capturing changes in the total commission rate τ over time. To account for anticipation before the reform came into effect, I define the enactment in May 2020 ($t = -7$) as the reference period. The commission rate τ could depend on the characteristics of properties i , which may change around the reform; thus, vector X_i controls for relevant characteristics.¹⁶ Lastly, I include λ_r to control for regional fixed effects on the zip-code level, the latter is also used to cluster standard errors.

Next to potential changes in the commission rate, I study how prices respond to the reform. Therefore, I modify Equation 2.5 in order to exploit the context that the platform is used not only by real estate agents but also by owners for direct sale. These listings provide an interesting benchmark for two reasons.

First, an object listed for sale by owner is ‘untaxed’ by the commission rate of a real estate agent. Assuming that, aside from the commission, buyers are indifferent if an object with given characteristics X_i is presented by a real estate agent or the owner directly, then these listings serve as references unaffected by the reform. Any remaining price difference would reflect the effect of the commission rate on prices.

Second, comparing listings from real estate agents to the ones for sale by owner can reveal the economic incidence. Price differences between these listing types would indicate the distribution of the economic incidence between buyers and sellers.

I empirically estimate the effects around the reform on the commission-inclusive prices separately for buyers ($j = B$) and sellers ($j = S$) with

$$\log(p_{i,t}^j) = \sum_{t=-18}^{18} \beta_t m_t \times A_i + \alpha X_i + \lambda_r + \epsilon_{i,r,t} \quad (2.6)$$

where $m_t \times A_i$ indicates dummies for month m_t with an interaction term for a listing i when offered through a real estate agent ($A_i = 1$) or through the homeowner directly ($A_i = 0$). This interaction term captures the within month difference to listings

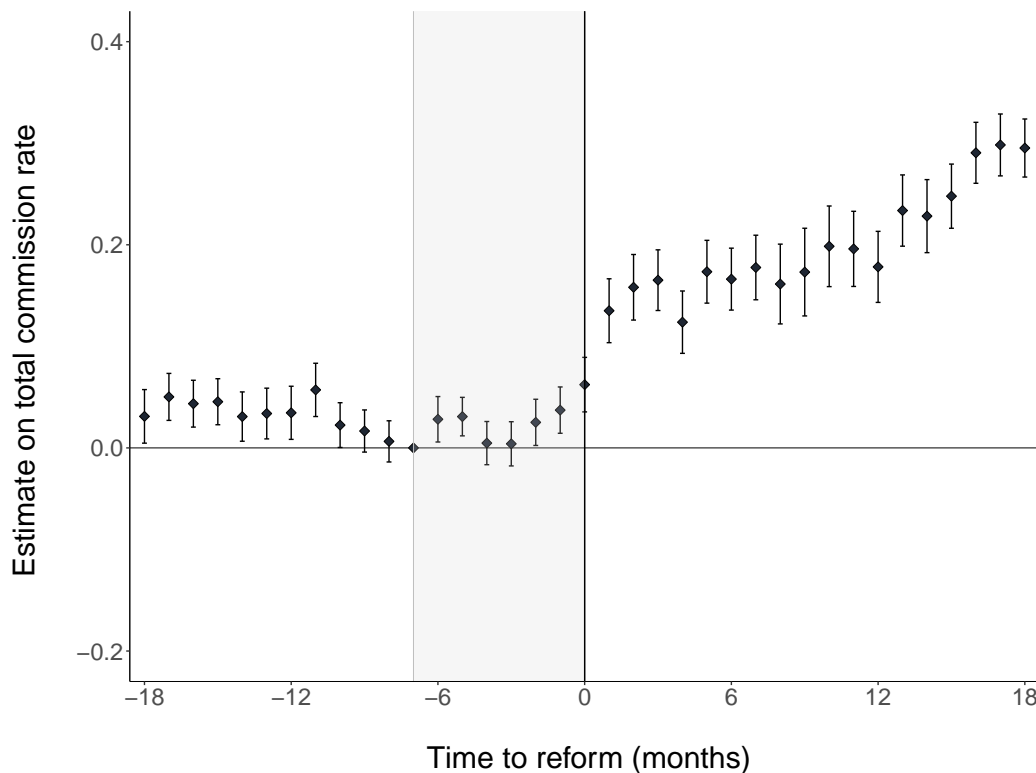
¹⁶The characteristics include month of the listing; (squared) floor and lot size; furnishing (e.g. deluxe or simple); condition (e.g. needs renovation or is modernised); number of rooms; heating type. Furthermore, I include dummies for the availability of a balcony; parking space; garden; kitchen; or basement. Since the effect of the construction year appears nonlinear, I create construction year dummies for 10-year strata. Furthermore, vector X_i includes an indicator for whether a property i is rented or not with a state interaction term to capture differences in tenant protection laws that vary by state. Regional variations in price trends are captured by interacting monthly dummies with the degree of urbanization (*‘Siedlungsstrukturelle Kreistypen’*) following Dolls, Fuest, Krolage, and Neumeier (2021).

for sale by owner that serve as the reference group. Otherwise, the right-hand side is identical to Equation 2.5.

2.4.3 Results

I begin with the analysis of commission rates around the reform. Recall that the incidence framework predicts that commission rates remain approximately neutral. The empirical estimates displayed in Figure 2.4 clearly reject this notion.

Figure 2.4: Event Study Estimates for Total Commission Rates Around the Reform



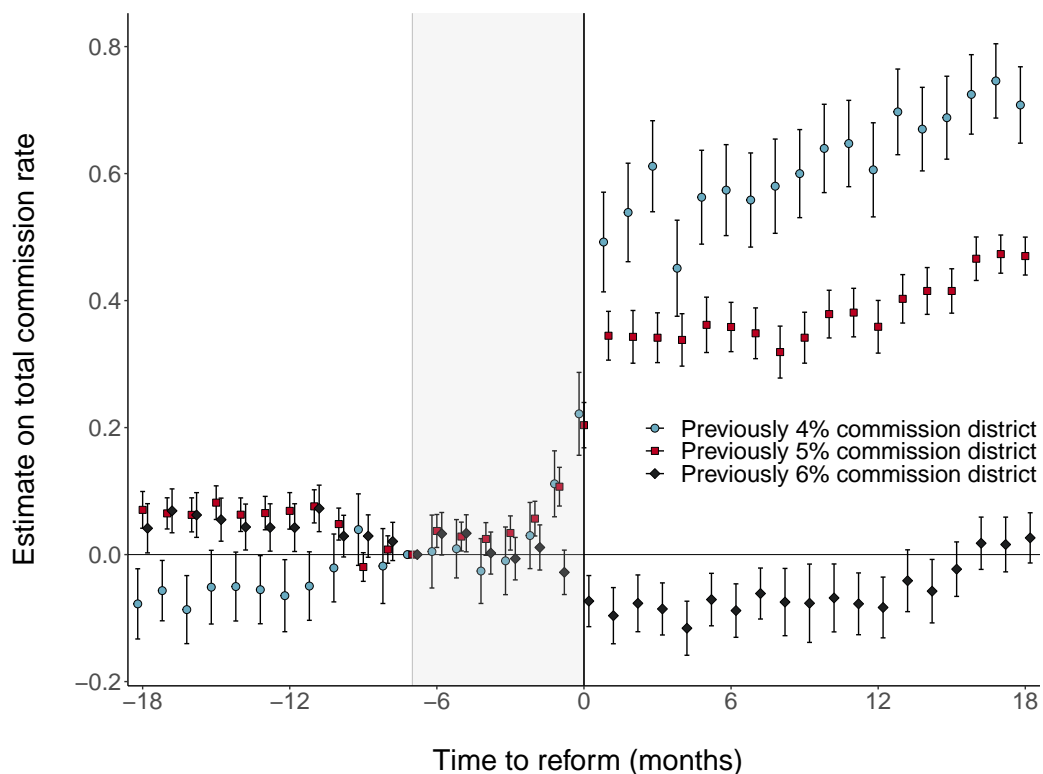
Note: The figure plots the event study estimates for the total commission rate. The underlying model controls for relevant property characteristics and uses regional (zip) fixed effects. Standard errors are clustered on zip codes and represent 95% confidence intervals. As the law was enacted in May 2020 and came into effect in December 2020, I use May 2020 ($t = -7$) as the reference period. The total net commission rate displayed is adjusted for the temporary VAT decrease in Germany six months before the reform between July and December 2020.

Commission rates are stable before the reform. However, after the enactment of the law in May 2020 ($t = -7$), commission rates begin to increase and rise sharply when the law came into effect in December 2020 ($t = 0$). I find that within one year, the total commission rate τ_{post} is on average 0.2 percentage points larger. After 18 months, the commission rate τ_{post} is 0.3 percentage points above pre-reform levels τ_{pre} . On average, this increase corresponds to an additional €956.32 per transaction.¹⁷

¹⁷Based on the average property price of €318,775 in the sample throughout the enactment month.

Figure 2.4 displays the average effect of the reform. Studying heterogeneity reveals important details. As Figure 2.2 illustrates, total commissions rates differ regionally before the reform. Although regions with previously either 4%, 5%, or 6% as their typical commission rate also feature listings with lower or higher rates, exploiting this pre-reform variation reveals the strategy of the increase. For this purpose, Figure 2.5 presents the estimates when the sample is split by regions and their most frequent commission rates before the reform.

Figure 2.5: Event Study Estimates for Total Commission Rates Split by Pre-Reform Regions



Note: The figure shows the event study estimates for total commission rates using the sample split by regions according to the most frequent commission rates as shown in Figure 2.2. The underlying models control for relevant property characteristics and use regional (zip) fixed effects. Standard errors are clustered on zip codes and represent 95% confidence intervals. The law was enacted in May 2020 and came into effect in December 2020; hence, I use May 2020 ($t = -7$) as the reference period. The total net commission rate displayed is adjusted for the temporary VAT decrease in Germany six months before the reform between July and December 2020.

The trajectory of commission rates in the different regions is similar before the reform, but it diverges right before the reform came into effect in $t = 0$ and strongly separates thereafter. Regions with a total commission rate of typically 4% before the reform see the strongest increase of 0.7 percentage points after 18 months. Similarly, districts with a predominant commission rate of 5% are subject to an increase of 0.4 percentage points. Although numerically small, this value translates into raw increases for revenues of commission services by 18% and 10%, respectively. In absolute terms, this corresponds to additional revenues of €2,231 and €1,275. However,

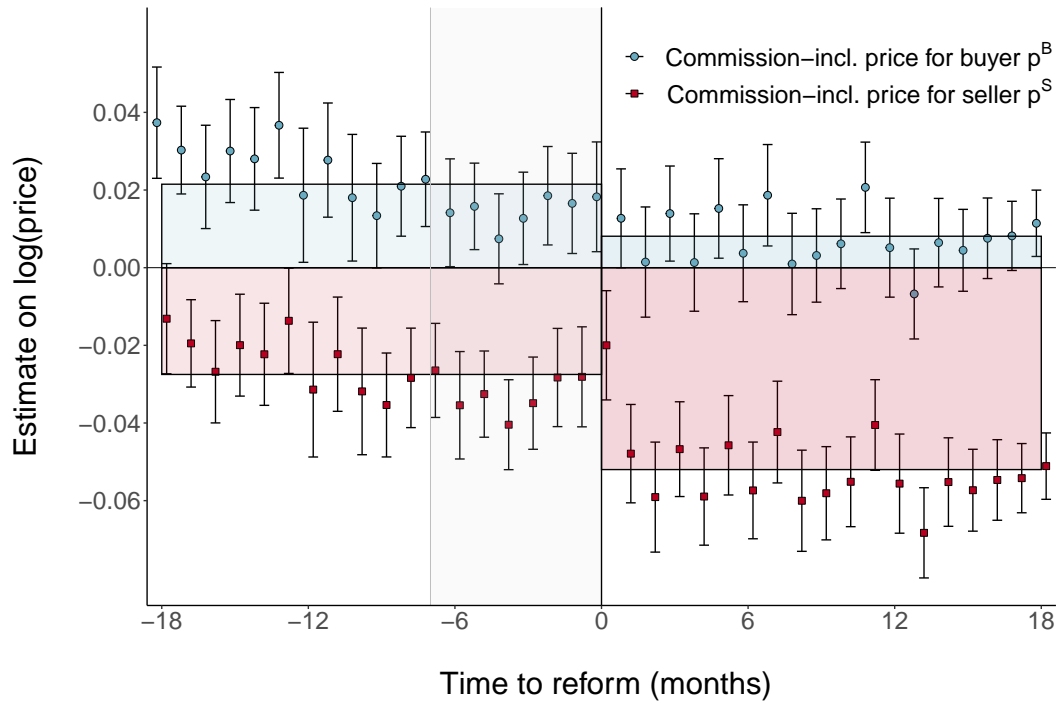
note that even this raise does not yet consider the general increase in housing prices depicted in Figure 2.3.

By contrast, commission rates in districts that previously featured the highest values remain relatively stable. The heterogeneous response to the statutory shift hints at the strategy of most price increases: in regions where buyers previously paid 4% or 5% as the entire commission, several real estate agents switched to asking 3% from both parties after the reform, allowing the total commission rate to converge to the 6% level that already persisted in several other regions. In these cases, the commission fee for the transaction of an averaged priced home increased by approximately €6,000 and €3,000, respectively.

This structure is supported by descriptive evidence (Figure B.2 and B.3). Interestingly, sellers' demand for real estate agent services appears unaffected by this. The probability of using a real estate agent does not appear to change around the reform (Figure B.7).

I continue with the estimation of commission-inclusive prices. Figure 2.6 presents the estimates of Equation 2.6 with logged commission-inclusive prices for buyers and sellers. Recall that the estimates display the effect of listings offered by a real estate agent compared to the reference of listings for sale by owner. The distance between the estimates for buyers' and sellers' prices corresponds to the total commission τ_i . Consequently, the ratio of the estimates to the distance between the estimates of both parties can be interpreted as the share of the commission economically borne by buyers and sellers.

Figure 2.6: Event Study Estimates for Commission-Inclusive Prices for Buyers and Sellers Around the Reform



Note: This figure presents the event study estimates on the (log) commission-inclusive prices for buyers and sellers. The colored boxes display the average effect before and after the reform. The underlying model controls for relevant property characteristics and uses regional (zip) fixed effects. Standard errors are clustered on zip codes and represent 95% confidence intervals. To account for anticipation effects, the model uses $t = -7$ (May 2020) as the reference period when the reform was announced and was passed by the German Parliament. The reform came into effect in $t = 0$ (December 2020).

I find that commission-inclusive prices for buyers $p^B = (1 + \tau_t^B)p_t$ (blue) are only modestly impacted by the reform. Although p^B appears to be slightly decreasing in the beginning of the sample, prices seem stable around the months when the reform came into effect. Compared to the enactment month $t = -7$, buyers' prices are on average 0.8% lower than before, bringing them closer to the price of listings that do not involve any real estate agent.

By contrast, the commission-inclusive prices p^S (red) that sellers receive diminish notably. Post-reform, sellers exhibit an approximately 2% lower price than before. The incidence model predicts that the statutory shift of the commission from buyers to sellers would be fully offset by a higher listing price but the results do not support this adjustment. The raw listing prices only partially compensate the statutory shift of the reform (Figure B.5). Initially, sellers appear to receive lower prices with a real estate agent, which diminish further after the reform. This trend is robust to a wide scope of control variables (Figure B.6).

The event study also supports the previous assumption that a non-trivial share of the economic incidence of the commission is on the sellers. Note that the seller's pass-through rate can be expressed as the ratio of the seller's commission cost and the total commission cost, or $(p_t^I - p_t^S)/(p_t^B - p_t^S)$. This pass-through is provided

by the empirical estimates in Figure 2.6, which display the seller's commission cost (red boxes) in relation to the total commission cost illustrated by the wedge between sellers' and buyers' prices (red and blue boxes). These estimates suggest that sellers bear between 56% and 87% of the economic incidence of the commission. This corroborates the assumption that sellers bear a non-zero commission share, even before the reform.

These results should be interpreted with care as the data reflect listed prices and the interpretation assumes objects for sale by owner to approximate the untaxed equilibrium price p'_t . Nonetheless, they align well with the theoretical considerations of the fixed quantity of supply in housing markets, and empirical research on real estate transfer taxes: recent evidence in developed countries suggests that tax increases on real estate transactions mostly fall on sellers (Kopczuk & Munroe, 2015; Dolls et al., 2021).

Overall, the key findings highlight a quantitatively and statistically significant increase in the total commission rates following the reform. Comparing listings by real estate agents to objects without any commission further indicates that prices largely fail to adjust to the statutory shift and that it is sellers who bear most of the commission incidence. Both findings contradict the theoretical predictions from a standard incidence framework. Thus, I consider alternative ways to interpret my findings next.

2.4.4 Interpretation

The results suggest that the policy reform not only failed to lower commission rates, but also even backfired. Parallel to continuously rising real estate prices, several real estate agents increased their commission rates after the reform. What triggered this response? Home sellers commission the real estate agent, negotiate the commission rate with the agent, and, as the analysis of prices that sellers receive indicates, home sellers bear most of the commission cost. As a result, sellers should have an incentive to induce competitive pressure by contracting real estate agents offering lower rates.

The results point to inattentive home sellers. Recent findings suggest that individuals may underreact to taxes (e.g. Chetty et al., 2009; Bradley, 2017) or cost (e.g. Gabaix & Laibson, 2006; Heiss, Ornaghi, & Tonin, 2023) with lower salience. Since the commission rate of real estate agents represents both, limited salience of the commission could explain inattention. However, within my setting, increasing salience does not unfold as planned. Assuming a limited visibility of the commission previously, the reform specifically aimed to raise salience by forcing sellers to pay real estate agents explicitly. Yet the results indicate that this increased salience did not increase attention.

The incidence framework marks two alternative gateways to interpret my results. As illustrated in Equation 2.3, the commission rate τ_t and the pass-through rate γ^S are essential to the seller's commission cost. Focusing on these components,

I suggest two other mechanisms to interpret the increase and reconcile the findings with other peculiarities of the German market for real estate agents.

Incidence Neglect. The first interpretation focuses on sellers' perception of their economic incidence. Before seeking lower commission rates, a seller must expect a benefit from reducing the commission rate. However, the difficulties of the real estate market, namely limited experience, insufficient learning, and information asymmetries, fully extend to the market for real estate agents. As a result, sellers do not have information on their share of the commission incidence γ^S . If sellers believe that buyers bear all commission costs anyway through a higher selling price, then a seller would not expect a benefit from a lower commission rate.

The empirical evidence suggests that the incidence borne by home sellers is not trivial (Figure 2.6). This aspect sharply contrasts to the marketing efforts by real estate agents. Recurring lawsuits¹⁸ show that real estate agents regularly claim to fetch the highest possible prices for sellers, although stating such claims is prohibited by law. Highlighting the ability to fetch higher prices may signal to sellers that their incidence burden is close to zero.

Considering how limited sellers are in their ability to infer the true pass-through rate, overlooking the economic incidence could explain why sellers underreact to commission fees. I hypothesize that real estate agents cater to this neglect by concealing information that would indicate that the commission cost is partially passed-through to sellers in the form of lower selling prices. However, the incidence neglect of sellers can only explain a generally low level of attention to the commission cost. To interpret the commission rate increase, I argue that attention to the commission rate is influenced by the reference commission rate.

Reference Commission Rates. Many home sellers commission a real estate agent only once every few decades. To judge the commission rate τ_t , I suppose that sellers compare the offered rate against what they perceive as the current typical rate $\bar{\tau}_t$. Recall that the industry term of the *regionally typical* commission rate encapsulates this idea. Building on previous work on inattention (e.g. Bordalo et al., 2019) and non-binding retail prices (Puppe & Rosenkranz, 2011; Bruttel, 2018), I hypothesize that attention to the offered commission rate depends on the distance between the offered rate τ_t and the currently perceived typical rate $\bar{\tau}_t$. In other words, if sellers have no historical benchmark and see that the offered commission rate τ_t is equal to what others currently seem to pay $\bar{\tau}_t$, then why should they negotiate?

The influence of the reference rate can also account for the nature of the increase (Figure 2.5): the statutory shift forced real estate agents to adjust their commission rates that they display in listings.¹⁹ As a result, several offers listed the adjusted

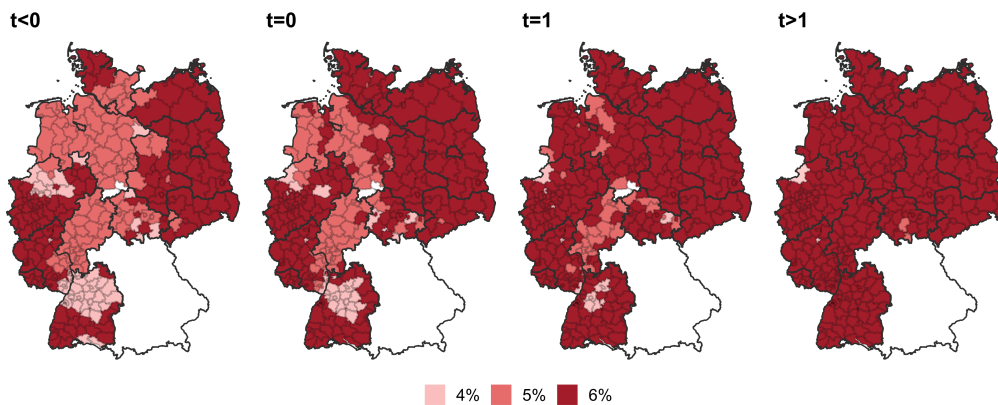
¹⁸See State Court Berlin (2020) Az. 52 O 125/19 or State Court Hamburg (2020) Az. 312 O 367/19.

¹⁹Recall that previously, these rates were mostly 4%, 5%, or 6% for the buyer only. Afterward, these values should have halved. Yet, Figure 2.5 indicates that many real estate agents switched from a 4% or 5% commission rate paid by the buyer to a 3% rate paid by both.

commission rates, whereas others still displayed old rates meant to be only paid by buyers around the date of the reform. A seller that observes the market only at this point in time cannot identify a dominant reference rate. Instead, sellers could perceive both, lower and higher rates, as *regionally typical*.

Furthermore, the idea of reference rates can explain the apparent upper bound of rates at 6%. While a real estate agent charging a 6% rate can always point to a region where 6% *regionally typical* rates already existed before the reform, a higher rate of e.g. 7% would be highly conspicuous with no reference rate to refer to. However, if an adjacent region already had 6% as the *regionally typical* rates, then real estate agents raising their rates from 4% or 5% could more easily refer to this higher level. This is consistent with the cascading path of the increase (Figure 2.7).

Figure 2.7: Regional Spread of the Commission Increase

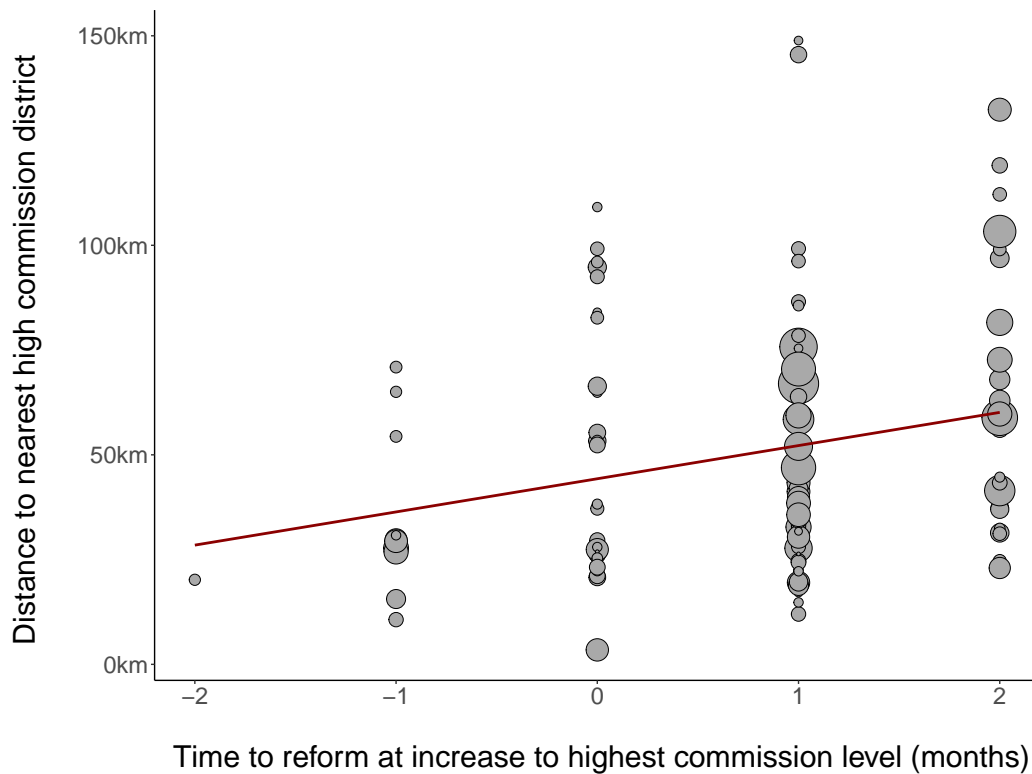


Note: The maps display the *regionally typical* commission rates around the reform in December 2020 ($t = 0$) based on three-month moving averages. Rate increases spread from high- to low-commission regions.

The increases gradually spread from high- to low-commission regions. Low-commission regions close to high-commission regions are the first to increase commission rates, whereas those in the center of larger low-commission areas are the last (Figure 2.8).²⁰

²⁰As an example, imagine two adjacent regions where one has a high typical commission rate (i.e. 6%), while the other has a lower typical rate (e.g. 5%). The *regionally typical* rate implies that in an area overlapping the border of the regions, the frequency of both, high and low rates, is similar. I argue that in such bordering regions, only a few real estate agents in the lower region must switch to higher commission rates to establish these higher commissions as the new *regionally typical* rate.

Figure 2.8: Timing of Increases vs Distance to High-Commission Districts



Note: The figure illustrates the time when the regionally typical commission rate of a previously low-commission district of 4% or 5% increased to the highest rate of 6% in relation to the distance to the nearest high-commission district. Each dot corresponds to one district, where the size of the dot is proportionate to the number of listings per region. On average, regions that are farthest from high-commission districts are among the latest to establish the highest level of 6% as *regionally typical* rate.

Similar to exploiting the confusion about the economic incidence, I hypothesize that real estate agents could act upon the influence of the reference rate by attempting to inflate the *regionally typical* rate that is often used for reference.

In the next section, I examine both interpretations and test if real estate agents respond strategically to sellers' incidence neglect and the influence of reference rates.

2.5 Evidence From a Randomized Survey Experiment

The findings in the first part of this paper suggest that sellers could be inattentive to the commission cost. I interpret this inattention as a result of sellers' neglect about their economic incidence and the influence of reference commission rates.

In the following, I present a survey experiment to further validate these accounts. First, I seek to verify if home sellers are inattentive to the commission cost. Second, I examine my previous interpretation in more detail: if sellers are uncertain about their economic incidence and substantially influenced by reference rates, real estate agents may respond strategically to these channels. As a result, I hypothesize that real estate agents inflate their capacity to fetch higher prices and the level of reference commission rates.

2.5.1 Subject Pool and Design

The survey experiment was run with currently active real estate agents from the three largest real estate platforms in Germany.²¹ I contacted the candidates by email over two weeks in November 2022. In return for participation, subjects received an exclusive report of the survey results to e.g. optimize their marketing strategies. Out of 1,315 respondents who started the survey, 1,146 finished it with 1,062 completing all the questions.²² To test for selection effects, I estimate the participation of subjects conditional on measurable characteristics (Figure B.11). The results indicate that responses appear uncorrelated with the size of a firm and seem regionally balanced.

In the first section, the survey inquired about factors which real estate agents believe to be important to their customers. Although mainly asked to spark interest, one question also asked to estimate the current *regionally typical* commission rate in their state. Later, this estimate serves as a baseline for one of the incentivized experimental questions.

In the second section, real estate agents had to consider their most recent home seller only.²³ Next to demographics, this part is critical to understand the role of sellers in the commission rate increase: subjects were asked whether or not the home seller tried to demand a lower commission rate when signing them as a real estate agent.

The third section contained the randomized experiment. Two incentivized questions seek to test if real estate agents strategically exploit sellers' potential incidence neglect and the influence of the reference commission rate.

For the incidence neglect, one question asked real estate agents' to estimate the average price difference between the listings of real estate agents and statistically similar listings for sale by owner. In essence, this question assesses real estate agents' perception of the incidence effects measured in the first part of this paper.

For the influence of reference rates, another question asked for an estimate of the average (arithmetic mean) commission level in their region. Comparing this value to their estimate for the *regionally typical* commission level aims to detect whether real estate agents are aware of the gap between the oft-communicated industry term and the empirical average.

To detect whether real estate agents may withhold information strategically, one of the two experimental questions was randomly incentivized: the survey offered €5 in the form of an Amazon gift card if their estimate matched the outcome of

²¹Although I originally planned to survey home sellers who directly commissioned a real estate agent, reaching this population was not possible without either deceiving real estate agents or violating the German interpretation of the European General Data Protection Regulation.

²²The survey achieved a response rate of 7.8%, considering that 5% of the invitations bounced back due to expired addresses, full mailboxes, or spam protection and that I collected 15,441 e-mail addresses from active listings to begin with.

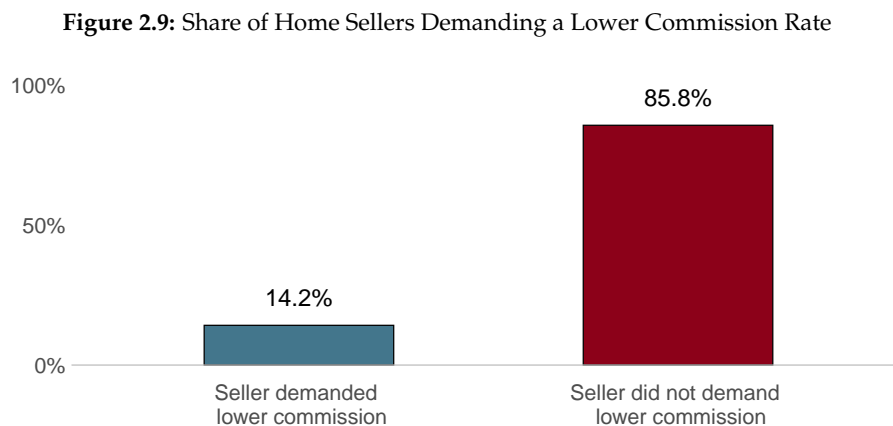
²³Limiting the focus to only the most recent seller has two advantages. First, this approach reduces distortions by particularly memorable customers and mitigates social desirability concerns. Second, a question asking for the general share of customers who demand a lower commission may trigger concerns of revealing illicit competitive practices; by contrast, the behavior of a single home seller cannot be traced back to the potentially distorting behavior of the real estate agent.

my empirical analysis. To mitigate any spillover effects, the experimental questions were also randomized in order. Appendix II provides the translated copies of the invitation e-mail, the welcome page, and the questionnaire.

2.5.2 Results

The results are presented in two parts: I begin with descriptive evidence on the frequency of home sellers demanding lower commission rates. Verifying that most home sellers do not seek lower commissions, I subsequently present real estate agents' beliefs for the underlying reasons. Although these results are descriptive, they are consistent with the findings from the randomized experimental questions, which are presented in the final part of the results.

Descriptive Evidence: Low Demand for Low Commissions. To test if sellers fail to demand lower commission rates from real estate agents, the survey asks whether the most recent home seller of the real estate agent tried to negotiate the commission. I find that less than 15% of home sellers demand lower commission rates (Figure 2.9).



Note: This plot illustrates the distribution of responses for the survey item “Did your last customer negotiate the commission rate, or not?”. Most sellers do not appear to seek lower commission rates.

The surprisingly low willingness to negotiate merits a reassessment. Although answers can be misreported, real estate agents have no foreseeable incentive to provide biased estimates in the observed direction: if real estate agents were attempting to create the impression that the observed commission rates are a natural market outcome, then they could have simply stated that home sellers did negotiate but failed to achieve a lower rate by doing so.

One imprecision arises from the fact that some real estate agents offer lower rates at the outset. As a small share indicates later, their sellers do not negotiate because they already offer lower rates. Yet as this matter concerns less than 5% of responses, this limitation does not affect the main finding: Most sellers do not try to demand lower rates when commissioning a real estate agent.

Descriptive Evidence: Explaining Negotiation Behavior. To seek potential explanations, I explore more descriptive evidence on home sellers' characteristics and real estate agents' beliefs on the negotiation behavior. In the following, I provide a brief summary of these supplementary findings, which are discussed in more detail in Appendix II.

Examining the propensity to negotiate based on demographic traits yields two main insights (Table B.2). First, I find that women appear half as likely to negotiate with a real estate agent than men. Second, the willingness of sellers to negotiate decreases substantially with age.

Studying the beliefs of real estate agents about sellers' negotiation behavior further motivates the survey experiment. Real estate agents state that the reference commission rate and sellers' perception of real estate agents' ability to fetch a higher price play the most role in affecting sellers' negotiating behavior (Figure B.12). However, although indicative, these findings cannot verify that home sellers low willingness to negotiate is a result of biased reference rates and underestimated incidence effects. To overcome this issue, I provide experimental evidence that tests whether real estate agents respond strategically to these limitations of sellers.

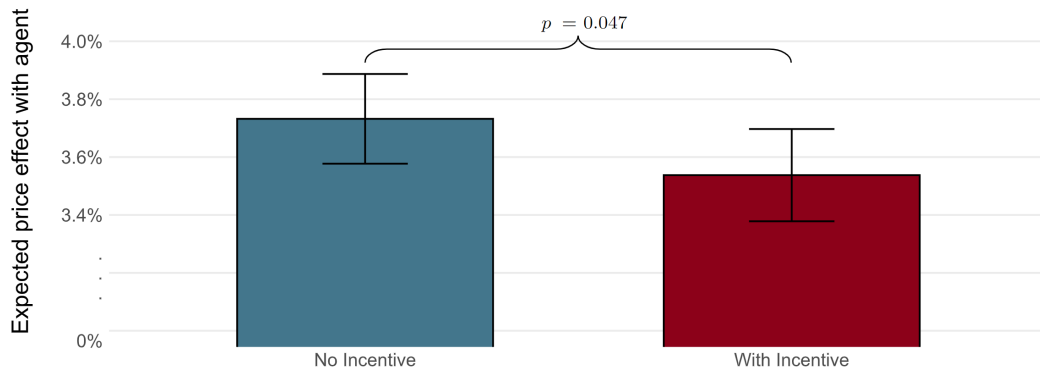
Experimental Evidence: Misguiding Real Estate Agents. Sellers are potentially unaware of the incidence of the commission and influenced by reference commission rates. As a result, I hypothesize that real estate agents may strategically inflate their ability to fetch higher prices for sellers and the reference commission rate which they communicate to sellers. To test this, the experimental questions ask real estate agents to guess the average price difference between listings offered by real estate agents and listings for sale by owner as well as to provide estimates on the average commission rate. Both questions are presented in random order with one of the questions being randomly incentivized: if the estimates provided by real estate agents are approximately correct, then they received a €5 Amazon gift card in the succeeding weeks. This design assumes that the information which real estate agents communicate by default is also what they communicate to potential sellers. Consequently, I hypothesize that randomly incentivising accurate answers deflates the estimates in both questions.

Selling Price and Incidence. The subjects are asked to estimate the price difference between listings made by real estate agents and those for sale by owner. For reference, recall the analysis of the incidence in Section 2.4 which indicates that offers by real estate agents are consistently priced below those listed for sale by owners (Figure 2.6).

Half of the subjects are randomly incentivized to provide accurate answers. The results are illustrated in Figure 2.10. In the unincentivized group, I find that real estate agents estimate to list objects at a 3.73% higher price than similar listings offered by the owners themselves. When adding the incentive, their estimate decreases to

3.54% ($p = 0.047$).²⁴ This negative effect induced by the incentive denotes that several real estate agents seem to know that their actual listing prices are lower or, at least, not as high than initially suggested.

Figure 2.10: Price Effect as Estimated by Real Estate Agents



Note: The figure illustrates the mean values stated by real estate agents when asked to estimate the price difference of listings offered by real estate agents compared to statistically similar objects offered for sale by owner when offered an incentive (right) and when not (left). Note that the y-axis is truncated for ease of presentation. The error bars display 95% confidence intervals.

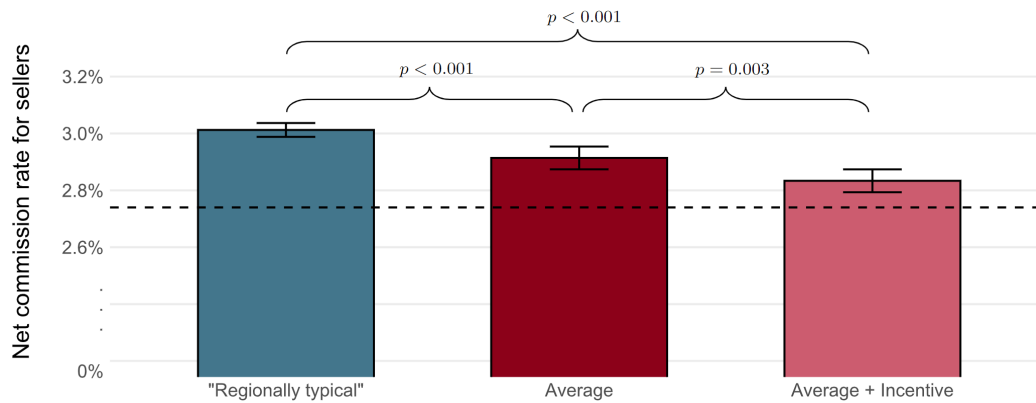
Between the treatment groups, I find that real estate agents severely overestimate their ability to offer listings at a higher price than objects not taxed by a commission. The findings suggest that real estate agents communicate biased information by default. The incentivized survey item suggests that real estate agents seem to be aware of this aspect. Although this result needs to be interpreted with care given the weaker statistical significance,²⁵ the direction of the effect supports the notion that real estate agents may conceal the economic incidence of the commission to home sellers.

Reference Commission Rate. Recall from the first part of this study that the empirical average of the commission rate is often lower than the *regionally typical* commission rate (Figure 2.1). This gap motivates two goals when asking real estate agents about the average commission rate. First, I intend to test whether subjects are aware that the average commission rate is lower than what they declared as the *regionally typical* commission rate in the beginning of the survey. Second, my objective is to verify whether real estate agents withhold information on the prevalence of lower commission rates by incentivizing true reporting on the estimated average commission rate. The results are shown in Figure 2.11.

²⁴Response times in both groups are statistically the same. Mean response times for the unincentivized and the incentivized group are 35.46 seconds and 36.48 seconds, respectively ($p = 0.39$).

²⁵The significance level likely underestimates the true difference. For technical reasons, I had to limit the answer options to an 11-point scale ranging from -5% to +5%. As the distribution of answers indicates, this likely biases the results: several real estate agents apparently would have selected an even higher value if given the opportunity, particularly in the unincentivized group. With around half of participants selecting the maximum value on the effect of a real estate agent on the listed price (Figure B.13), I clearly underestimated the tendency of real estate agents to overestimate their ability.

Figure 2.11: Commission Rate Estimates by Real Estate Agents



Note: This figure shows the mean of the values stated by real estate agents when asked about the typical regional commission rate (left), the average commission rate (middle) and the average commission rate when incentivizing correct answers (right). Note that the y-axis is truncated for ease of presentation. The dashed horizontal line presents the actual average commission rate observed in the post-reform listing data. The error bars display 95% confidence intervals.

To mitigate any misunderstandings, I explicitly ask real estate agents to provide estimates for the sellers' commission share and not the total commission for both, buyers and sellers. For reference, Figure 2.11 includes the empirical average commission level as the dashed line.

Unsurprisingly, real estate agents correctly guess the industry term of the *regionally typical* rate; with 3.01%, this proportion perfectly corresponds to the most frequently used commissions for sellers seen in the micro data.

Turning towards the average commission rate yields more interesting results: even the group without an incentive for this question appears to be aware that the average commission rate is lower than the *regionally typical* commission rate. When asked about the average rate, real estate agents indicate a lower estimate of 2.91% ($p < 0.001$). Although the effect size appears small, this change is driven by a large extensive margin response. Only 18% of subjects indicate a rate less than 3% as the *regionally typical* commission, but this share nearly doubles to 34% when asked for the average commission rate. This result implies that several real estate agents are aware of the inconsistency between the *regionally typical* rate and the empirical distribution.

In the incentivized treatment arm, this effect is even more pronounced. With the opportunity to gain €5, real estate agents stated an average commission rate of 2.83%, significantly lower ($p = 0.003$) than in the unincentivized group. The decrease is again primarily driven by an increased share of respondents (44%) reporting a lower value than the typical 3% rate. This indicates that real estate agents are not only aware of the lower average commission rate but also withhold this information when not encouraged otherwise. As the item for the unincentivized group did not feature the additional note informing about the incentive, lower salience might explain this effect rather than the word 'average', which can be overlooked.

However, analyzing response times indicates that respondents in both groups paid similar attention to the question.²⁶

Overall, the higher rates among unincentivized subjects appear to measure the real estate agents' concealment of their knowledge of lower commission rates. Assuming that real estate agents communicate similar information in the survey and to home sellers, communicating inflated rates would corroborate the hypothesis that sellers are influenced by the reference rate.

2.6 Discussion

2.6.1 Policy Implications

The results hold several implications. Above all, the findings signal that the reform failed to lower commission rates. The evident price increase even suggests that the reform backfired, adding significant cost to buyers and sellers. Assuming an annual transaction volume of €130 billion with real estate agents,²⁷ buyers and sellers approximately lose €390 million every year from the commission rate increase of 0.3% following the reform alone.²⁸

The survey experiment extends the insight around this failure. Showing that only 15% of sellers demand lower commission rates confirms that unresponsive sellers may explain why the reform failed to lower commission rates. Furthermore, the experiment indicates that sellers' unwillingness to negotiate may be maintained by the real estate agents' tendency to communicate obscure reference commission rates and the downplaying of the economic incidence of the commission cost.

The German system of sharing the commission cost with the buyer represents an outlier internationally and a puzzle internally: the system of splitting the commission between sellers and buyers is frequently advertised as fair; however, real estate agents in the current system are selected by the sellers, their service portfolios are determined by the sellers, and they are incentivized through a commission on the selling price to work in favor of the sellers only. My findings extend these doubts by offering new evidence that this unusual policy facilitates rent seeking by real estate agents. The fact that all countries with low total commission rates have systems

²⁶Both groups spent a statistically similar amount of time to give their estimates. More precisely, the incentivized group averaged 34.42 seconds, whereas the unincentivized respondents spent 32.23 seconds ($p = 0.24$) on average. Considering that the incentivized item featured slightly more text, this small difference becomes even less of a convincing explanation for insufficient attention in the unincentivized group.

²⁷The transaction volume of residential real estate in 2020 was €217 billion (AK OGA, 2021), of which I assume that 60% of residential sales were being handled by real estate agents. This estimate is deliberately lower than what my sample suggests: note that transactions also transpire without an intermediary, for example, when objects are offered by property developers directly or among family members and friends.

²⁸When calculated against real estate commissions in low-commission countries with similar housing prices, where total commission rates average approximately 2% rather than 5.5%, the yearly loss to buyers and sellers is nearly €3 billion.

with only selling agents who are solemnly paid for by the sellers provides a coherent indication that such a clear system maximizes price competition.

2.6.2 Limitations and Alternative Explanations

The results must be interpreted considering the limitations of the study. Foremost, data availability restrains my research. Germany does not provide access to large-scale micro data for research purposes on housing prices. Although this limitation should not influence the findings on the commission rate increase, the precision of incidence effects may be affected. Being restricted to the last posted listing price interacts with the assumption that prices of listings offered for sale by owner or through a real estate agent are comparable. However, after a listing is taken offline, the bargaining process between the buyer and a real estate agent or an owner selling directly may differ. Although my results align well with previous findings in direction and magnitude (Rutherford et al., 2005; Levitt et al., 2008; P. Jia & Pathak, 2010), the estimates involving listing prices should be interpreted with care. Beyond data restrictions, other explanations could play a role.

First, poor information on the true cost and abilities of real estate agents may lead home sellers to infer quality from price. Low commission rates may signal low quality, possibly hindering real estate agents' incentive to compete through lower commission rates. Similarly, sellers may fear moral hazard following a demand for lower commission rates. However, as this does not stop consumers from comparing multiple offers and prices in other service industries, this argument likely plays a minor role.

Second, the way by which the reform was communicated might explain part of the increase: advocates framed the effective split as a fair compromise between buyers and sellers. Even though this argument ignores the fact that the commission incentivizes real estate agents to work in favor of the seller only and fully disregards the economic incidence, sellers may see less necessity to negotiate a commission rate that is presented to them as fair.

Finally, the possibility of collusion must be examined. At first glance, the uniformity of commission rates might suggest that real estate agents fix commission rates. However, the real estate agent industry in Germany is highly decentralized. Approximately 70,000 real estate agents work in 32,000 firms, of which only 6,500 firms generate annual revenues larger than €250,000 (Destatis, 2020). Large cities offer hundreds of real estate agencies to choose from, and even rural markets exhibit little concentration (Figure B.8).²⁹ The decentralized organization reflects the low entry barriers: real estate agents in Germany only require a standard business license³⁰ but no formal qualification, training, or membership access. Unlike the

²⁹Low HHI scores corroborate this conjecture, ranking below 1,000 in rural areas and around 50 to 300 in larger cities. As a sanity check, I also calculated HHI scores for the capitals of the states where the districts in Figure B.8 are located, namely Mainz (133), Hannover (75), and Magdeburg (276).

³⁰Costing between €20 and €60.

US, for example, where the Multiple Listing Service is exclusively operated by real estate agents, the central platforms in Germany are run independently. Other collusive practices such as ‘steering’ are not possible, whereby buying agents might steer customers away from selling agents who advertise a lower overall commissions (Barwick et al., 2017).

Yet one feature of the real estate agent market in Germany may be conducive to price coordination: the commission rates are transparent. As buyers continue to pay some of the commission after the reform, real estate agents must publicly display the rate in each listing. One would assume that this practice serves price competition. However, recent evidence suggests that transparency may also facilitate price coordination among competitors (Luco, 2019). Such tacit price coordination based on the commission rates of other agencies would add to the potentially problematic influence of reference dependence induced by the obscure *regionally typical* commission rate.

2.7 Conclusion

This paper relates a perplexing increase of commission rates to home sellers not demanding lower rates. Exploiting a reform which shifts the obligation to pay the commission fees from buyers to sellers, I find that commission rates increase sharply while the demand for real estate agents remains constant. The results show that the reform, which attempted to lower commission rates by raising cost salience, backfired.

However, I demonstrate that the commission increase also contradicts standard incidence predictions. Although the results indicate that sellers bear most of the economic incidence, they appear to underreact to the statutory shift and do not seem to demand lower commissions from real estate agents. I attribute these observations to seller inattention to the commission cost. I argue that this inattention is potentially fueled by biased reference commission rates and confusion about the pass-through to sellers. A follow-up survey experiment verifies that 85% of home sellers do not attempt to demand lower commission rates. Furthermore, the experimental questions illustrate that real estate agents may strategically exploit sellers’ ignorance. Randomly incentivizing accurate answers suggests that real estate agents knowingly inflate reference commission rates and conceal the empirical incidence.

Although my results are based on national evidence, they speak internationally. By structuring the idiosyncrasies of the global practices of real estate agents, the overview in Table 2.1 may suggest that all low-commission markets are alike, and every high-commission system is limited in its competitiveness in its own way; more specifically, all low-commission countries forgo a buying agent and oblige the seller to pay the entire commission. By contrast, most high-commission countries add complexity by either incentivizing the use of an additional buying agent or, alternatively, by splitting the obligation to pay the selling agent (or leave it entirely unclear).

Germany is a case in point: my findings imply that the current split policy adds complexity to obscure reference rates and the neglect of home sellers' incidence.

Considering the rising cost of housing, my research highlights a policy opportunity to reduce the cost of homeownership. Linking inattentive consumer behavior to potential mechanisms, my work not only extends ongoing studies on consumer inattention and tax salience but also identifies the current limits of predicting their effects at a policy scale.

If 85% of consumers in any other industry were to indicate that they do not care about the price for a service, then one would scarcely believe such pronouncement. Yet in the market for real estate agents, such behavior is almost inconspicuous. Ultimately, this paper aims to challenge the conception that the market for real estate agents is bound to follow a different set of economic principles than other industries.

Chapter 3

Beer and Crime: Evidence From Germany, 1882–1913

This chapter represents joint work with Christian Traxler from Hertie School and Carsten Burhop from the University of Bonn.

3.1 Introduction

Although individual alcohol consumption has plateaued in most high-income countries over the past decades, the ongoing industrialization of developing countries is drastically reshaping alcohol markets. For example, compared with the levels of the 1970s, per capita alcohol consumption in India and China has increased three- and five-fold, respectively (World Health Organization, 2019). This surge has in part been driven by booming beer markets. Requiring efficient means of production and distribution, beer brewing benefits strongly from industrialization. The worldwide trends of drinking reflect this aspect of economic progress: in contrast to 1980 when alcohol was consumed mainly in the form of spirits, beer represents the dominant source of alcohol consumed today (Figure C.1).

As industrialization continues to drive increases in beer accessibility through more efficient production and rising income levels, understanding its potential impact on society becomes critical. Modern research on the social costs of alcohol focuses primarily on public health outcomes. Although these studies provide vital information on the direct and indirect health effects of alcohol, such as the increased risk of cardiovascular disease or the excess mortality from drunk-driving accidents, comparatively few studies have been devoted to studying the causal effects of alcohol on crime.

A key obstacle to empirical research on alcohol and crime is their inherent endogeneity: loss of inhibition and increased aggression could encourage crimes,¹ but one can similarly argue that the soothing and relaxing effects of alcohol might also be particularly attractive for criminals. In addition, estimations of the effect of alcohol on crime could suffer from unobserved confounders. Certain socioeconomic characteristics could drive both alcohol consumption and crime independently of one another, making it difficult to isolate alcohol as a cause of crime. These challenges limit the scope of empirical setups that allow the relationship between alcohol and crime to be studied. As a result, most studies use short-term, locally confined, or age-specific changes in legal access to alcohol to solve the identification problem. Possibly due to the individual contexts of these approaches, the causal relationship between alcohol and crime remains ambiguous.

Whereas Grönqvist and Niknami (2014) find no effects on violent crimes and a positive effect on property crimes from experimentally rolled-out Saturday weekend sales in Sweden, Heaton (2012) only observes an increase in minor offenses, such as disorderly conduct, but no property crime effects from liberalized Sunday weekend sales in the US. Other studies have suggested a stronger effect on violent crimes. Studying the adoption of mandatory closing hours for bars and restaurants in Brazil's São Paulo Metropolitan Area, Biderman, De Mello, and Schneider (2010) demonstrate that states that adopted the dry law saw a 10% reduction in homicide

¹For a more detailed discussion on the pharmacological effects of alcohol on crime, see Carpenter and Dobkin (2011)

rates. Similarly, Barron et al. (2022) observe a sharp drop in homicides, assaults, and rapes in weeks with alcohol bans during curfews throughout the COVID-19 pandemic in South Africa.

These empirical designs have been used to provide valuable insights by studying specific measures for restricting alcohol consumption; yet, they may not fully capture the broader, long-term implications of a fundamental shift in the accessibility of alcohol. Considering that crime consistently induces large social costs, it is critical to understand the impact of alcohol on society at large. Thus, the present study examines the link between alcohol and crime using a newly compiled historical data set. Specifically, we digitized rich data on convictions for more than 20 different types of crimes in Prussia and the rest of the German Empire. Offering consistent records over more than three decades, we merge these crime data with a variety of other data sources to study the role of beer consumption in crime in a rapidly industrializing economy.

To identify the causal link between beer and crime, we draw on two unique empirical strategies. First, we employ a novel instrument that uses exogenous variation in local barley yield. Barley was almost exclusively reserved for beer production, and its yield was sensitive to local weather conditions during its sowing and harvest periods. Using data on beer production from the official brewing tax records, we first verify the impact of the local barley yield on local beer production. Then, exploiting variation in barley yield while controlling for other agricultural outputs and prices, we run two-stage least-squares (2SLS) and reduced-form regressions. Our results document a quantitatively and statistically significant effect of barley yield on violent crimes, driven mainly by assault rates. We find that a 1% reduction in per capita beer production is associated with a 1% reduction in violent crime rates. On the contrary, property crime rates do not appear to be subject to changes in barley and beer output.

Our second research strategy builds on a tax reform. In 1906, Prussia increased its brewing tax while other German states kept their tax levels unchanged. Exploiting this quasi-experimental setting, we run a traditional difference-in-differences (DID) design that compares crime rates in Prussia with those in the untreated southern states before and after the reform. Although with a magnitude smaller than the IV estimates, the results indicate a significant negative effect of the brewing tax increase on assault rates. The DID estimates imply a beer-assault elasticity of between 0.16 and 0.64. A robustness check using a synthetic control approach adds weight to these findings.

Our research contributes to the emerging causal literature that investigates how alcohol consumption affects crime. By demonstrating a positive effect of beer output on assault rates in both empirical strategies, our results support the hypothesis that alcohol predominantly affects violent crime. Yet, we do not find sufficient evidence to establish a link with property crimes. Complementary to studies that have examined the effects of alcohol using methods that are predominantly local (Rossow

& Norström, 2012; Biderman et al., 2010), temporary (Heaton, 2012; Grönqvist & Niknami, 2014; Barron et al., 2022), or specific to a sub-population (Carpenter, 2007; Carpenter & Dobkin, 2015; Hansen & Waddell, 2018), our empirical work adds new methods for studying the long-term consequences of alcohol for society as a whole (Cook & Durrance, 2013). Furthermore, our findings hold insights for policy. Offering evidence based on the availability of brewing inputs and a tax reform, our work suggests that alcohol-induced crime not only responds to bans and curfews but also appears responsive at the margins. Although historically unique, the context of our study makes these findings relevant today. The German Empire around 1900 can be characterized as a low-income country with a rapidly growing beer market. With a GDP per capita of between \$3,000 and \$5,000 (2020 US\$), this backdrop is not unlike the trajectory of several emerging countries in Asia, South America, and Africa today.

Moreover, we contribute to a debate pertaining to the history of alcohol and crime. Although scholars in the 19th and 20th centuries argued that alcohol was a likely cause of immoral and criminal activity, most of these debates were limited to the role of spirits in the early part of the 19th century. Focusing on a time when the population experienced a strong increase in purchasing power while technological advancements simultaneously reduced the cost of brewing beer, our study draws attention to a dimension of the Industrial Revolution that has not yet been fully illuminated. Therefore, our focus on alcohol extends the set of previously studied reasons that could explain long-term patterns of crime next to poverty (R. Jia, 2014; Bignon, Caroli, & Galbiati, 2017) or the sociodemographic shifts induced by urbanization (Johnson, 2002). By digitizing new data on crime and compiling this information with a variety of other sources, we also aim to facilitate future studies' use of historical data about the German Empire. Combining several methods of causal inference, our study further contributes to the growing literature that applies modern statistics to historical crime data (R. Jia, 2014; Pinotti, 2015; Bignon et al., 2017; Bindler & Hjalmarsson, 2021; Melander & Miotto, 2023).

The remainder of this paper is organized as follows. Section 2 describes the institutional background and the new data that we digitized for this study. Section 3 presents the first identification strategy using an instrumental variable approach. We complement these findings with a second empirical approach exploiting a brewing tax reform in Section 4. Section 5 concludes with a discussion of our findings and their implications for policy and research.

3.2 Institutional Background & Data

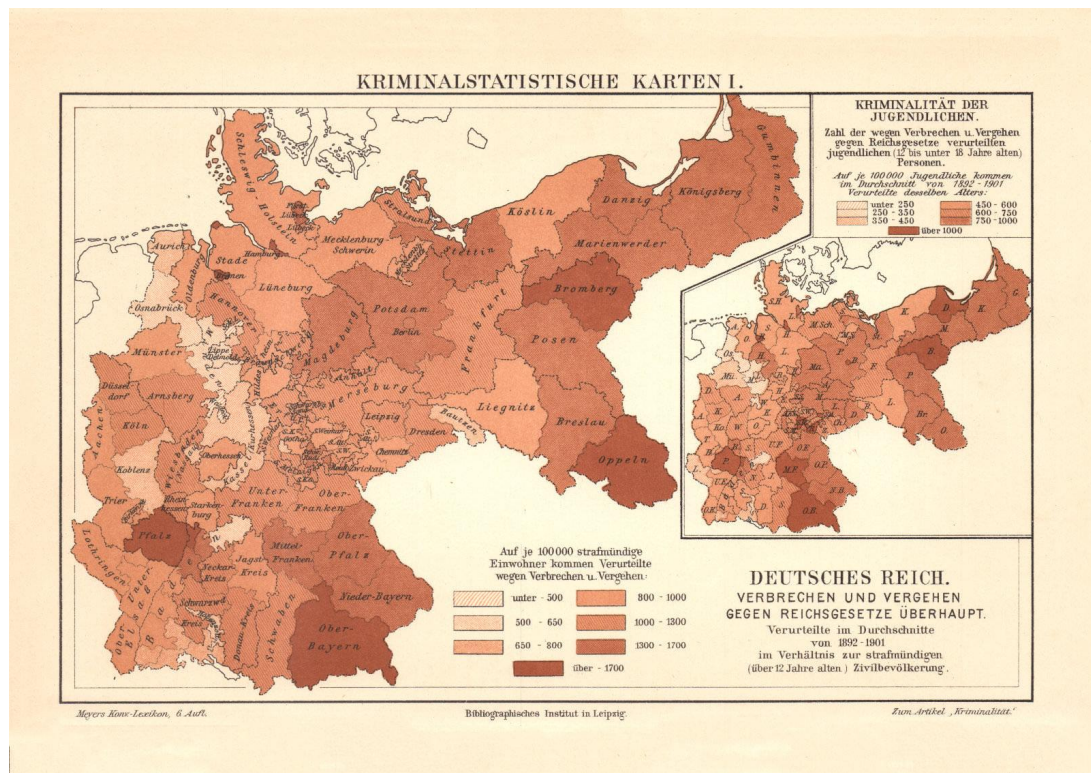
We study the effect of beer on crime and other outcomes in the context of late 19th-century Germany, during a massive boom of the beer industry. Our analyses exploit unique characteristics of the justice system and the brewing industry in the German Empire. Founded in 1871, the German Empire integrated the previously loose confederations of kingdoms, duchies, and cities to form a federal German nation-state. The Kingdom of Prussia formed its heart, providing the chancellor for the entire German Empire and representing two-thirds of its population. This central government oversaw the rapid transition from an agricultural society to an industrial one. Fueled by modern factories and novel production technologies, the GDP more than doubled from its founding to the outbreak of World War I. This transformation was accompanied by a modern public administration that included a rigorous collection of statistical information. The works published by the Royal Prussian Statistical Office served as a blueprint for the statistical work established in the other states of the German Empire.

3.2.1 Crime

The introduction of a unified criminal code in 1872 and a common code for criminal procedure in 1879 created a homogeneous legal space throughout Prussia and the German Empire. This standardization of the legal system was reflected in the statutory law itself. By specifying offenses and punishments in detail, the codified nature of the criminal code set uniform guidelines for criminal proceedings and sentences in each jurisdiction.²

²A similar statement can be made regarding police forces. Note also that, compared with the situation in other European countries at the time (see, e.g., Bindler & Hjalmarsson, 2021), the powers of the police and the scope of their functions were wide in the German Empire (Fosdick, 1915). The thorough training of legal officials and police promised common implementation across its territory: “Virtually all police officers from the rank of lieutenant up, all attorneys, all judges, and all government bureaucrats held university degrees, [and] had passed arduous governmental examinations [...]” (Johnson, 2002, p. 18f.).

Figure 3.1: Original Crime Statistics per District



Note: This image displays a map published originally around 1910. Based on data from the imperial statistical office, the map illustrates average crime rates per 100,000 (of population above the age of 12) for each district, aggregated between 1892 and 1901.

From 1882, the national statistical office (*Kaiserliches Statistisches Amt*) began publishing harmonized crime statistics. These statistics were primarily based on all convicted criminal cases and were aggregated using counting cards (*Zählkarten*; see Figure C.2), which were filled out by the local courts following a judgment. Even today, many judicial processes follow the basic structure of the 1879 imperial code of criminal procedure (*Reichsstrafprozessordnung*). Summarizing the extensive description of Johnson (2002, p. 43f.), the typical process was as follows: In response to a potential offense, a state attorney would informally begin to collect evidence. If the evidence substantiated the accusation, the state attorney would formally initiate a preliminary examination overseen by a judge and independent tribunal. Once concluded, the judge would then decide whether to proceed to trial. Typically, cases that had a relatively high chance of conviction were brought to court. For every 100 individuals convicted, approximately 18 were acquitted considering all courts; 90% of trials were concluded in the first instance at the local court (*Amtsgericht*).³

We collect and digitize the yearly crime statistics at the district level.⁴ For the 37 Prussian districts, our data cover the period from 1882 to 1913, i.e., all years available

³See *Justiz-Statistik des Reichs-Justizamts 1889*, p. 191 and p.197.

⁴These represent the finest regional level available. Although some scattered statistical data exists on a county level, these are either limited to 5-year aggregates or restricted to selected cities.

in the Prussian crime statistics.⁵ For these years, we observe the 25 most common crimes, covering violent crimes (among others, murder and manslaughter, infanticide, simple and aggravated assault, sexual assault, and robbery), property crimes (e.g., simple and aggravated theft, embezzlement, and property damage) and other crimes (e.g., perjury or arson). For each crime, we observe the number of criminals convicted as well as the total number of crimes for which they were convicted. These different crime counts are also broken down by gender and by age (distinguishing offenders below and above 18 years). Moreover, for the years 1882 to 1890, we also observe the total number of trialed crimes/criminals.

Complementing the Prussian crime data, we also compile crime data for the years 1899 to 1913 for the 19 governmental districts of Bavaria, Baden, Württemberg, and Alsace–Lorraine. For these districts, which comprised most of the rest of the German Empire, the crime data are limited to the total numbers of crimes and criminals, and omit some of the less frequent crime types.⁶ Below, we will present all crime data as crime rates, i.e., criminals or crimes per 100,000 of the total district (or province) population.

The nature of crimes in the German Empire was not unlike that in societies today. The leading number of convictions were related to assault, battery, and theft. The scale of convictions ranks in a magnitude comparable to modern-day Germany. While most districts (*Regierungsbezirke*) in the German Empire registered between 650 and 1,300 convictions per 100,000 of the population for all crimes in general (see Figure 3.1), current data for Germany indicate approximately 770 convictions.⁷

3.2.2 Beer and Taxation

Beer production in the German Empire boomed during the second half of the 19th century (Tappe, 1994, p. 242ff.). Until the 1850s and 1860s, beer brewers concentrated on top-fermented ales (Schanz & Manicke, 1906, p. 259). Starting with the 1870s, however, the brewing industry underwent a radical transformation that led it to almost exclusively produce bottom-fermented Lager and Pilsener-style beers by 1914. This transformation was fueled by a series of scientific and technological innovations (Wischermann, 1985). Firstly, the understanding of the biochemistry of the fermentation process (yeast) and brewing increased dramatically. Secondly, a novel cooling technology became available. Traditionally, German brewers relied on cold winters and ice from frozen lakes or rivers to resort to the necessary cooling

⁵Starting with 1906, the district of Allenstein was formed by parts of the two prior districts Gumbinnen and Königsberg and was established as a new, third district within the province of East Prussia. Throughout our analysis, we proportionally assigned all data recorded for Allenstein to the two 'old' districts. For the years 1906 to 1912 we thus compute values that maintain the original partitioning of East Prussia and thus allow us to compare the data for Gumbinnen and Königsberg over the entire sample period. We also examined alternative approaches and none of the results reported below is qualitatively sensitive to this data transformation.

⁶This concerns perjury, sexual assault of the unconscious and children, kidnapping, active and passive bribery, and embezzlement.

⁷This accounts for the difference in the age of criminal responsibility, which is 12 years in the German Empire and 14 years in modern-day Germany.

required for bottom-fermented brews (which require temperatures between 4°C and 9°C). Carl von Linde’s invention of an industrial-scale refrigeration machine in the 1870s disrupted the reliance on natural ice and allowed for more consistent cooling conditions year-round:

“If mechanical refrigeration was, in a sense, the keystone among the fundamental technical innovations, the introduction of the steam engine marks the beginning of industrial beer production.” (Tappe, 1994, p. 220)

Together with the invention of the chiller and other tools to better control the brewing process, these innovations made it possible to produce bottom-fermented beers of consistently high quality.⁸ This supply was met by an increasingly affluent population with a demand for more refreshing beers compared with traditional ales. In response, the amount of beer produced increased dramatically from 1870 onward (Figure 3.2). At the turn of the century, total beer output reached peak levels – which remained unmatched until the 1960s. Between 1871 and 1900, annual beer production almost doubled from 65 to 126 liters per capita. Note that other sources of alcohol intake did not follow this trend: per capita consumption of spirits and wine (which only played a role in some German regions) remained largely constant (Hoffmann, 1965).

One part of our empirical analyses uses local beer production as a proxy for local beer consumption in a given year. This approach is motivated by several institutional features. First, similar to modern days, the shelf life of beer was limited (to, at best, one or two months). Second, beer production was highly decentralized. At the beginning of our sample period, there were approximately 9,000 commercial brewers in Prussia alone, which corresponds to nearly one brewery for every 3,000 inhabitants or every larger village.⁹ One rationale behind this level of decentralization was the high weight-to-value ratio as well as the relatively complex cooling requirements, which made beer particularly costly to transport (Adams, 2006).¹⁰ As a result, the long-distance trade of beer in the German Empire was rare.¹¹ This reflects that despite significant improvements in the road and railway networks (Hornung, 2015), the German Empire’s economic integration remained limited prior to 1914 (Wolf, 2009). Finally, note that high import tariffs further limited beer trade with other countries. In 1900, the German Empire imported only 564,000 hl beer (Schanz & Manicke, 1906), which represented less than 1% of the beer produced within its borders.

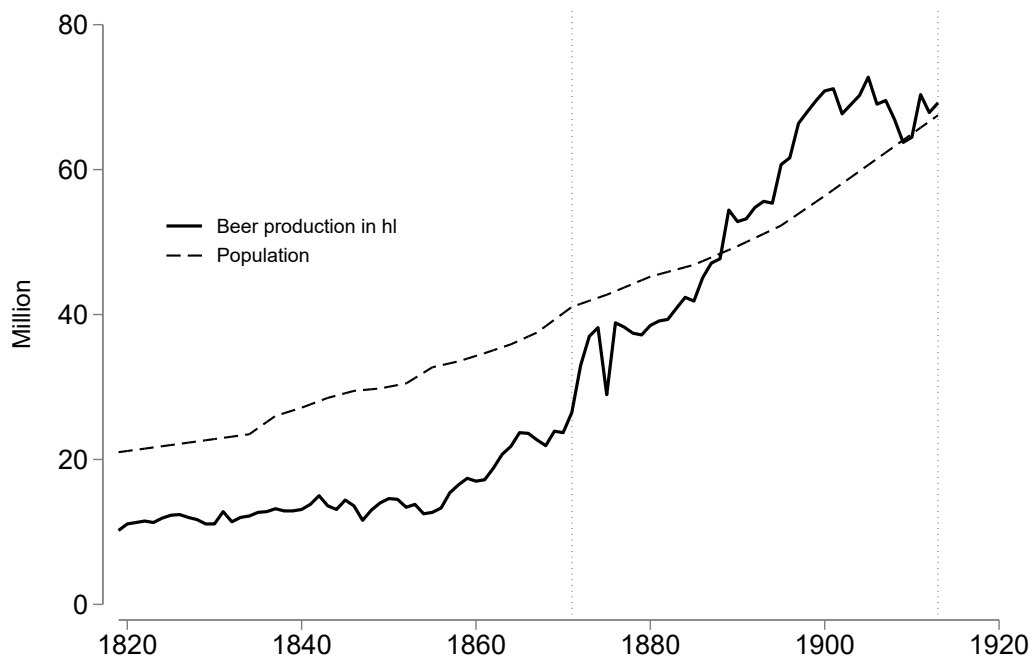
⁸These bottom fermented beers were often referred to as “bavarian style” beers (*nach baierischer Art*), as in the advertisement displayed in Figure C.3.

⁹Monatshefte zur Statistik des Deutschen Reichs, 2. Teil, Juli-Dezember 1883 Band 4.

¹⁰Despite widely known anecdotes about a small beer delivery from Nuremberg to Furth in 1836, the capacity of refrigerated wagons remained limited and severely constrained the transportation of beer via the railroad network for most of our sample period.

¹¹For example, even in the most extreme case of Bavaria, exports to the North German Confederation accounted for only 3.3% of beer produced within the North German Confederation at the beginning of our sample. Improved modes of transport only partly raised this figure later to 6.9% in 1913 (see *Monats-/ Vierteljahreshefte zur Statistik des Deutschen Reichs*, for the years 1884 and 1913).

Figure 3.2: A Century of Beer Production on German Territory



Note: This figure displays the amount of beer produced (solid line) and the population (dashed line) on the German territory. Dotted lines mark the beginning of the German Empire in 1871 and the outbreak of WWI in 1914. Industrial and scientific brewing led to a rapid increase in beer production that outpaced population growth towards the end of the 19th century. Based on data from Mitchell (1998).

We collected information on local beer production from beer tax records. The taxation of beer was an important source of revenues: it contributed approximately 10% of all federal and state taxes in the German Empire.¹² Beer taxes were thus meticulously recorded. Tax inspectors regularly visited breweries to collect information on the amount of beer brewed and the (taxable) raw materials used in the brewing process. We digitized the annual amount of beer brewed for all Prussian states (*Provinzen*¹³) for all available tax years (1882 – 1912).¹⁴

In contrast to the brewing volumes and taxes, beer prices were not systematically collected. Official statistical records only provide broad indications of the wholesale price of certain beer types (e.g., 7-12 *Mark* per hl). Final selling prices of beer are not available; yet scattered information from old restaurant menus, advertisements (see

¹²The exact tax rate and schedule differed across five major brewery tax territories within the German Empire (the North German Brewery Tax Confederation, Bavaria, Baden, Württemberg, and Alsace-Lorraine). For total beer taxes in each brewery tax region, see the respective years in *Statistisches Jahrbuch für das Deutsche Reich*. For data on total taxes on a state and federal level, see Sensch (2013).

¹³While Prussian states were referred to as provinces (*Provinzen*), they were more similar in size and structure to other states of the German Empire (such as e.g. Bavaria or Baden). Hence, we refer to them as states.

¹⁴There are no comprehensive data available at the district level. The Prussian Statistical Office published more fine-grained beer tax data only for one year but then stopped this work because “the examination and sifting of the material exclusively occupied the forces of the officer concerned and an additional official for four months” (*Festschrift des Königlich Preussischen Büros 1905, p. 148*).

Figure C.3), and the reference of wholesale prices provides a rough range of 8–20 cents (*Pfennige*, with 1 Mark corresponding to 100 *Pfennige*) for the most common types of local beer (0.33–0.5 liters).¹⁵ At this time, the daily wage of an unskilled worker on a construction site was around 2.50 Mark in 1882 and around 5 Mark in 1913 (Kuczynski, 2015).

3.2.3 Other Data and Panel Structure

In addition to data on beer and crime, we collected several additional variables. We gathered district-level harvest data for Prussia from the monthly (1882–1891) and quarterly (1892–1913) publications of the Imperial Statistical Office (*Monats-/Vierteljahreshefte zur Statistik des Deutschen Reichs*). Next to a limited set of wholesale prices, these data include rich information on the harvest yields for the crops of wheat, rye, and – most critically – barley. Our analysis will exploit variation in the barley harvest, based on a variable that compares the current year’s barley yield relative to the average yield over the past 5 years (see below and Appendix C).

Furthermore, we use comprehensive sociodemographic data on Prussia. These data were partially digitized by ourselves and partially derived from the Galloway Prussia Database (Galloway, 2007), which offers statistical information from major volumes of the statistical publications of Prussia (*Preussische Statistik*) and the German Empire (*Statistik des Deutschen Reichs*). As complementary outcome variables, we also compiled data on deadly accidents and suicides at the district level. The latter data cover the years 1883–1900 (where the year 1886 is missing for the accident data).

For data on the German Empire besides Prussia, we mostly employ district-level data provided by Thome (2006), who refined and extended previous work by Johnson (2002).¹⁶ To compensate for the absence of more fundamental economic data for the German Empire panel at the district level, we supplement information on the share of the employed population that worked in industry jobs from the occupation and firm census of 1895 (*Berufs- und Gewerbezahl von 1895*).

Based on the different data sources, we pursue two research designs. Our first approach exploits exogenous variation in barley yield. While we will use barley yield as an instrument for beer production (in a two-stage least squares approach), our main strategy will consist of reduced form estimations. Our second research design zooms in on a tax reform that only increased beer taxes in Prussia but left taxation in the southern states of the German Empire unaffected. We will examine this variation using a canonical DID design.

¹⁵One consistent record of beer prices comes from the *Oktoberfest* in Munich, where prices are available from its beginning in 1810 until today. In 1910, a liter cost 38 *Pfennig*.

¹⁶Johnson (2002) and Thome (2002) both study crime trends and reject theories of modernization but do not provide causal explanations for the long-term crime trends in the German Empire.

Table 3.1: Overview of Panel Data

Data	Prussia		German Empire
	(A) District panel	(B) State panel	(C) District panel
Crime (crimes & criminals)	1882–1913	1882–1913	1899–1913
Crime (youth & other crimes)	1882–1913	1882–1913	-
Accidents & Suicides	1883–1900*	1883–1900*	-
Beer (production)	-	1882–1912	-
Harvest	1882–1913	1882–1913	-
Sociodemographics	1882–1913	1882–1913	1900**
Panel structure (region \times years)	36 \times 32	14 \times 31	55 \times 15

Note: *The accident data are missing for the year 1886. **The set of socio-demographic variables available at the district level for the German Empire panel is restricted to the 1900 census, which contains fewer variables than what is available data for Prussia.

To implement these research designs, we construct three different sets of panel data (see Table 3.1 for an overview): (A) A panel of 36 Prussian districts (see footnote 5) covers the years 1882–1913. These data, which cover barley harvest and crime rates, will serve as the main basis for our primary identification strategy. (B) Accounting for the fact that beer production is only available at the state (*Provinz*) level, we also construct a Panel with the 14 Prussian states for any inference involving beer directly. (C) Finally, in our second research design, which will examine the impact of the beer tax reform of 1906, we augment the district-level panel for Prussia with data for the 19 districts of Alsace-Lorraine, Baden, Bavaria, and Württemberg. This ‘Imperial Panel’ is limited to the years 1899–1913 (see Table 3.1). Summary statistics for the most critical variables in each panel are presented in Table 3.2.

Table 3.2: Summary Statistics

	Prussia				German Empire	
	(A) District panel		(B) State panel		(C) District panel	
	Mean	St. dev.	Mean	St. dev.	Mean	St. dev.
All crimes rate	466.16	165.41	500.47	165.25	453.31	138.74
Violent crime rate	169.62	67.39	174.15	59.75	189.07	77.68
Property crime rate	205.36	95.24	226.39	100.07	180.39	59.75
Rel. barley harvest	1.04	0.19	1.05	0.17	-	-
Beer production p.c.	-	-	84.89	43.17	-	-
Observations	1,152		434		825	

Note: Crime rates display the number of crimes per 100,000 of the population.

3.3 Barley Yield and Beer: An IV Approach

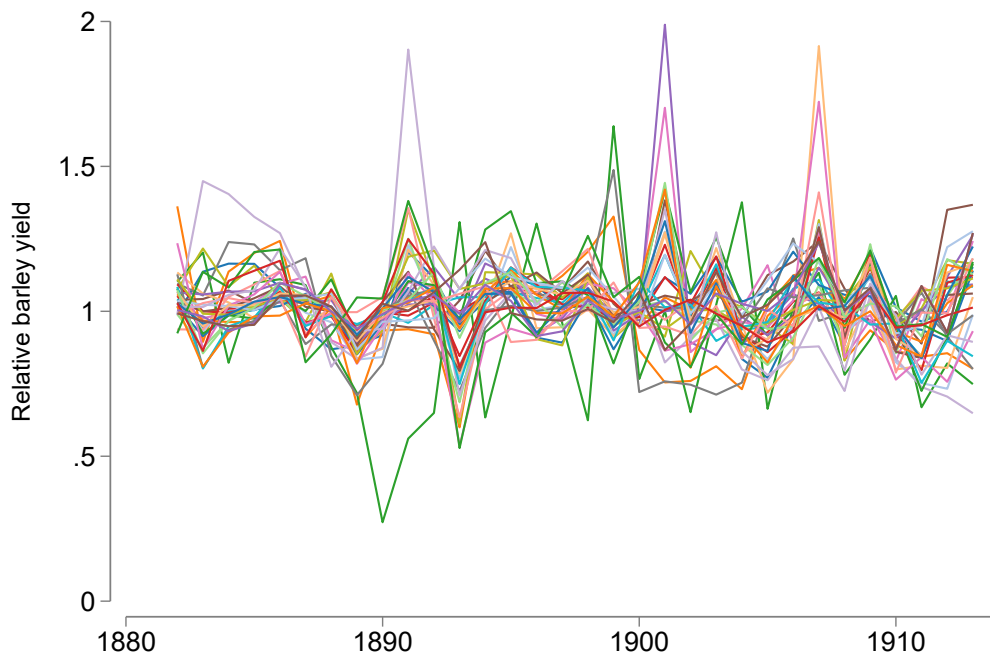
Our primary research design employs an instrumental variable (IV) strategy. The IV approach aims to address potential endogeneity concerns regarding beer consumption. Unobserved factors – such as changes in risk preferences or attitudes – may

simultaneously alter people’s willingness to drink and their propensity to commit a crime (or their risk of a deadly accident). An additional source of endogeneity, which can be equally important in a panel data setting (Griliches & Hausman, 1986), might be a problematic measurement error in the dependent variable.¹⁷ To overcome these endogeneity concerns and to identify the causal effect of beer, we exploit variation in barley yield.

3.3.1 Instrument Motivation

We use the ‘relative’ spring barley yield – more specifically, the average barley harvest in region i and year t relative to the average harvest during the past 5 years in that region – as our instrumental variable. Almost exclusively grown for the purpose of brewing beer, the spring barley harvest in the German Empire was subject to substantial variation (Figure 3.3). Given that spring barley was a key input for the brewing process, the variation should affect the regional output of beer.

Figure 3.3: Variation of the Relative Barley Yield



Note: The graph depicts the barley yield in each year relative to the 5-year past average at a district level.

Reflecting that Prussia’s geography spanned from the borders of the Benelux

¹⁷If, conditional on a crime occurring, the crime is more likely to result in a conviction when the offender was drunk, this would induce a bias in panel estimates.

states in the west to modern-day Kaliningrad in the east, the variation in the barley harvest was not uniform across Prussia.¹⁸ Yet, the cross-sectional variation was relatively modest compared with the intertemporal variation. Although barley was traded within Prussia, we argue that this trade was imperfect in compensating for local variation in the harvest yield of barley. In particular, at the beginning of our sample period, when motorized transportation was restricted to the railway, this likely affected barley's availability. Still, even in later periods, natural variation in the barley harvest would likely have affected brewers. Exporting or importing barley from remote regions following a good or bad harvest locally would induce additional transport costs to the production process. Ultimately, this is an empirical question; our analysis will assess whether barley yield provides a sufficiently strong instrument for beer production.

Our definition of the instrument is motivated by our attempt to capture 'good' vs 'bad' harvesting years of barley. Using the barley yield from a given year relative to a moving average from past years allows us to measure the harvest on a scale that is comparable between states or districts that are more and less agrarian (Figure 3.3). Moreover, this variable is less prone to simply reflecting long-run trends in agricultural productivity. The specific functional form also delivers the strongest first stage (and, unsurprisingly, a higher precision in the main stage). Alternative definitions of the IV (based on the logged level of spring barley yield) give us similar but less precise estimates as those reported below.

The main concern with any instrument is the exclusion restriction. One might argue that variation in barley yield correlates with the harvest of other crops, such as wheat and rye (i.e., the input for rye bread, which can be considered a staple). This correlation could, in turn, influence crime and other outcomes via numerous channels (Miguel, Satyanath, & Sergenti, 2004). To account for such indirect effects, our main specifications will control for other major crop yields in each year; some specifications will also control for wheat and rye prices.¹⁹

To identify a local average treatment effect, we must also consider whether the monotonicity assumption holds. Theoretically, one could argue that if barley correlates with inputs for other types of alcohol, such as wine or spirits, and the increased availability of their inputs increases their production quantity disproportionately more than beer, an increase in the barley yield may not raise beer consumption everywhere: regions with a tradition of other alcohol types might see a reduction in beer consumption with additional barley yield due to the substitution of other types

¹⁸In complementary work, which is not presented in this thesis, we document that a non-trivial share of the variation in barley yield can be explained by variation in weather conditions and its interactions with the local soil quality.

¹⁹A further concern might be related to income losses of barley farmers during bad harvests. While the point is in principle valid, one has to keep in mind that revenues from spring barley were small relative to those derived from wheat and rye. We nevertheless assessed this point by running reduced form estimates that split our sample into the most and least agrarian districts (in terms of occupational characteristics). Observing that the resulting estimates were very similar between those samples is reassuring and suggests that variation in barely related profits and incomes played a minor role in shaping crime.

of drinks. However, considering that the main inputs of wine (grapes) and spirits (fruits, potatoes) follow different growth and harvest cycles compared with barley, we consider this theoretical possibility to be practically limited.

3.3.2 First-Stage Results

To test whether the instrument is sufficiently strong, we examine the impact of barley yield on beer output. More specifically, we estimate the first-stage equation

$$\log(\text{Beer})_{i,t} = \beta_1^{\text{fs}} \log(\text{Barley})_{i,t} + \mathbf{f}^{\text{fs}} \mathbf{X}_{i,t} + \sum_i \rho_i^{\text{fs}} \mathbb{1}_i + \sum_i \tau_i^{\text{fs}} (\text{Trend}_t \times \mathbb{1}_i) + \epsilon_{i,t}^{\text{fs}} \quad (3.1)$$

where $\log(\text{Beer})$ represents the per capita beer production in state i in year t and $\log(\text{Barley})$ depicts state i 's spring barley harvest yield relative to the average yield in the past 5 years. The estimates also include $X_{i,t}$, a set of control variables. Our baseline specifications will only include sociodemographic and economic covariates.²⁰ In augmented specifications, we will also control for the rye and wheat harvest or for rye and wheat prices (in state i and year t). All models include state-level fixed effects (ρ_i) and state-specific time trends (τ_i) as in, e.g., Biderman et al. (2010). Lastly, we also allow for a general trend break in the year 1906 to account for any possible disruption induced by the brewing tax reform in 1906 (see Section 3.4). Below we will also discuss year fixed effects.

Keep in mind that information on beer production is only available at the state level. The first-stage estimates are thus restricted to the state-level panel and are reported with clustered standard errors on the state level. To account for the small number of cluster units (states), we also present wild cluster bootstrapped t statistics (Cameron, Gelbach, & Miller, 2008).

²⁰These include the population density, the share of males in the population, the share of individuals employed in industry vs agriculture/forestry, the infant mortality rate, the average wages, the (state's) share of individuals born in the municipality they live in, and the share of children receiving primary education.

Table 3.3: First-Stage Estimation Results (Prussia State Panel)

Dep. variable (log):	Beer				Violent crime	
	(1)	(2)	(3)	(4)	(5)	(6)
log(Barley)	0.079*** (0.014) [5.461]	0.078*** (0.010) [7.584]	0.070*** (0.013) [5.293]	0.077*** (0.011) [6.587]	0.019 (0.020)	0.078*** (0.025) [3.118]
Observations	434	434	434	408	434	434
F statistic	30.79	59.43	28.96	44.95	0.976	
Covariates	No	Yes	Yes	Yes	Yes	Yes
Crop yield controls	No	No	Yes	No	Yes	Yes
Crop price controls	No	No	No	Yes	No	No
Time controls	Trends	Trends	Trends	Trends	Year FEs	Trends

Note: All specifications include state fixed effects and state-specific linear time trends. Robust standard errors, clustered at the state level, are in parentheses. To account for the small number of cluster units, wild cluster bootstrapped t statistics (Cameron et al., 2008) are reported in brackets. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3.3 presents the results from the first-stage estimates. Overall, there is a strong link between the barley harvest and beer production. The estimates from Specification (1), which only includes state fixed effects and state-specific time trends, indicate that a 1% increase in the relative barley harvest is associated with a 0.079% increase in the per capita beer production. The coefficient is highly statistically significant, with an F statistic of over 30. In Column (2) we add covariates. The point estimates remain virtually unchanged. To tackle concerns about the exclusion restriction, Column (3) controls for the harvest yield, and Column (4) for the (logged) prices of the key crops rye and wheat. The estimates as well as the F statistic remain similarly stable. In contrast to the harvest data, the price data include several missing state/year cells. As a result, we focus on models with yield data for our main specifications (as in Specification 3). We can run these models with the full set of observations from our different panels.

While the results point to a strong and stable link between barley and beer output, we must note that these results are sensitive to including year fixed effects (instead of state-specific trends): Column (5) documents that the coefficient on barley becomes much smaller and loses statistical significance. This seems to reflect that the year fixed effects absorb too much of the data's inter-temporal variation. Considering the panel structure, which features a small number of states but a relatively high number of years, this observation does not seem too surprising. Consequently, our further estimates will only account for state- or district-level trends but not for year fixed effects.

Complementing the first-stage estimates, Specification (6) provides a first, reduced-form estimation of the model

$$\log(\text{Crime}_{it}^j) = \beta_1^{\text{rf}} \log(\text{Barley})_{i,t} + \mathbf{f}^{\text{rf}} \mathbf{X}_{i,t} + \sum_i \rho_i^{\text{rf}} \mathbb{1}_i + \sum_i \tau_i^{\text{rf}} (\text{Trend}_t \times \mathbb{1}_i) + \epsilon_{i,t}^{\text{rf}} \quad (3.2)$$

where $\log(\text{Crime}_{it}^j)$ is the crime rate per 100,000 for crime category j . Focusing on violent crimes, Specification (6) indicates that a 1% higher barley yield implies an 0.078% increase in violent crimes. As the point estimate is almost identical to the one reported in columns (1) to (4), this would suggest an elasticity of violent crimes w.r.t. beer of about unity (i.e., $\beta^{\text{rf}}/\beta^{\text{fs}} \approx 1$).

In summary, the first-stage results corroborate the idea that local barley yield is a strong instrument for local per capita beer production. We obtain meaningfully high F statistics. Furthermore, the wild bootstrapped t statistics are reassuring and suggest that the limited number of cluster units is not a major issue. The first-stage estimates are also encouraging concerning the exclusion restriction: controlling for rye and wheat harvest yields or prices does not substantially affect the estimated link between barley and beer production. This finding is consistent with the notion that the harvest timing of spring barley differs from that of other major crops (with barley being the first crop harvested each summer). In the next subsection, we build on these findings with the two-stage least squares (2SLS) estimates at the state level, before we turn to reduced-form estimates of the finer district panel.

3.3.3 Two-Stage Least Squares Results

Based on the first-stage estimates of Equation (3.1), we now estimate the main stage of our 2SLS model,

$$\log(\text{Crime}_{it}^j) = \beta_1^{\text{tsl}} \log(\widehat{\text{Beer}})_{i,t} + \mathbf{f}^{\text{tsl}} \mathbf{X}_{i,t} + \sum_i \rho_i^{\text{tsl}} \mathbb{1}_i + \sum_i \tau_i^{\text{tsl}} (\text{Trend}_t \times \mathbb{1}_i) + \epsilon_{i,t}^{\text{tsl}} \quad (3.3)$$

where $\log(\widehat{\text{Beer}})$ is the instrumented version of our main beer variable. We first consider the violent crime rate and its major subcategories. 2SLS estimates for this outcome are presented in Table 3.4.

Table 3.4: IV Estimations: Violent Crime (Prussia State Panel)

Dep. variable (log):	Violent crime				Minor assaults	Major assaults	Murder/homicide
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log(Beer)	0.570** (0.234) [12.56]	0.998*** (0.319) [12.68]	1.129** (0.401) [12.63]	0.523* (0.272) [12.40]	1.365* (0.752) [6.934]	1.738** (0.639) [10.16]	-1.530 (1.445) [-2.635]
Observations	434	434	434	408	434	434	434
F statistic (first stage)	30.79	59.43	28.96	44.95	28.96	28.96	28.96
Covariates	No	Yes	Yes	Yes	Yes	Yes	Yes
Crop yield controls	No	No	Yes	No	Yes	Yes	Yes
Crop price controls	No	No	No	Yes	No	No	No
Time controls	Trends	Trends	Trends	Trends	Trends	Trends	Trends

Note: The table presents 2SLS estimates together with the corresponding first-stage F statistic. All specifications include state fixed effects and state-specific linear time trends. Robust standard errors, clustered at the state level, are in parentheses. To account for the small number of cluster units, we present wild cluster bootstrapped t statistics (Cameron et al., 2008) in brackets. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In the baseline Specification (1), we observe a significantly positive effect of beer on our measure of violent crimes. A 1% increase in the amount of beer produced per capita is linked to a 0.6% increase in the rate of violent crimes. As a sensitivity check, we mirror the step-wise inclusion of covariates and additional controls for other crop yield and prices from the first stage (see Table 3.3). Except for Specification (4), which drops a non-trivial number of observations due to missings, the point estimates increase slightly and remain significant at the 5%- and 1%-level. Our main Specification (3) corroborates the unit elasticity of beer and violent crime observed in the ratio of the coefficients from the first stage and the reduced form (see above).

Specifications (5) to (7) decompose this effect into the subcategories of violent crimes. These results illustrate that the positive effect of beer on violent crimes is largely driven by assaults. Both minor and major assaults display estimates that surpass the coefficients seen in the equivalent specification for all violent crimes, revealing elasticities of 1.4 and 1.7, respectively. However, these estimates are less precise. Finally, Column (7) shows that we find no significant effects of beer on murder and homicide rates.

Table 3.5: IV Estimations: Other Crimes (Prussia State Panel)

Dep. variable (log):	All	Property crimes	
	crimes	Theft	Vandalism
	(1)	(2)	(3)
log(Beer)	0.164 (0.402) [0.407]	-0.684 (0.513) [-1.332]	0.027 (0.408) [0.0655]
Observations	434	434	434
F statistic (first stage)	28.96	28.96	28.96

Note: The table presents 2SLS estimates together with the corresponding first-stage F statistic. All specifications include state fixed effects and state-specific linear time trends as well as our basic covariates and controls for crop yields. Robust standard errors, clustered at the state level, are in parentheses. To account for the small number of cluster units, wild cluster bootstrapped t statistics (Cameron et al., 2008) are reported in brackets. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Turning to the 2SLS estimates for the other crime categories in Table 3.5, the effects are similarly nondescript. While there is a moderately negative coefficient for theft and a small positive coefficient for vandalism, both estimates are highly imprecise, with neither one being statistically significant.

The crime data are not restricted to the number of crimes but also feature the number of criminals. Studying the rate of criminals in the population, we find that the extensive margin results echo our previous findings on crimes (Table C.1). For property crimes, we find an insignificant negative coefficient. For violent crimes, the results indicate a weakly significant positive effect of local beer production on the number of criminals convicted for violent crimes and assaults. The estimates indicate an elasticity of 1.5, which is slightly larger than what we find for the coefficients for crimes. The results suggest that beer had an intensive and extensive margin effect on violent crimes: larger quantities of beer produced not only lead to more violent crimes and assaults but also to a greater number of criminals involved in these crimes. However, this finding remains limited to violent crimes, as we do not observe extensive margin effects for other types of crime (Table C.1).

To summarize, the 2SLS results indicate a positive effect of beer consumption on violent crime but not on property crime. The effect for violent crime occurs both at the extensive as well as at the intensive margin. Both findings are mainly driven by minor and major assaults. In the next step, we use panel data at the finer district level to validate these results. As data on beer production is not available at this regional level, the following analyses are confined to the reduced-form estimates.

3.3.4 Reduced-Form Results

The reduced-form estimations at the district level are based on the same specification as described in Equation (3.2). Naturally, we now use district-level fixed effects as well as district-specific time trends.²¹ Beginning with violent crimes, Table 3.6 presents the reduced-form results.

Table 3.6: Reduced-Form Estimates for Violent Crimes (Prussia District Panel)

Dep. variable (log):	Violent crime	Min. assaults	Maj. assaults	Murder+hom.
	(1)	(2)	(3)	(4)
log(Barley)	0.068** (0.028)	0.079*** (0.029)	0.076** (0.031)	0.003 (0.099)
Observations	1,152	1,152	1,152	1,152

Note: The table presents reduced-form estimates. All specifications account for district-level fixed effects and include district-specific linear time trends, our basic covariates and controls for rye and wheat yields. Robust standard errors, clustered at the district level, are in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The reduced-form results at the district level reveal a positive effect of beer on violent crimes. For the general violent crime rate, we observe that a 1% increase in the relative barley yield raises violent crimes by 0.068%. Similar to the state-level results, this effect is mainly driven by minor and major assaults (see Columns 2 and 3). For these outcomes, we observe coefficients close to 0.08, which are significant at the 1%- and the 5%-level, respectively.

The effect size aligns well with the earlier state-level results. Recall from the first-stage results in Table 3.3 that a 1% increase in the relative barley yield is associated with a 0.07% larger per capita beer output. The reduced-form coefficients observed for assault rates in Table 3.6 are of a similar size. Assuming that the relation between relative barley yield and beer output also holds at a district level, the reduced-form estimates would again imply a unit beer-assault elasticity ($0.08/0.07 \approx 1.14$). Although this result is slightly lower than the elasticity suggested by the 2SLS results in Table 3.4, the magnitude appears reasonably close. As in our 2SLS estimates, we do not observe any effect on murder and homicides nor on other violent crimes. This consistency also extends to property crimes and further crime categories (Table C.2) that do not suggest any effect either.

It is worth noting that the results from the reduced-form estimations overlap with the magnitudes observed in the literature. Although the effects on violent crimes and assaults are comparably strong, their dimensions appear plausible in light of other contexts. For example, Heaton (2012) observes that allowing Sunday

²¹Standard errors are now clustered at the district level. We again examined whether our inference is sensitive to the limited number of cluster units. Computing wild cluster bootstrapped confidence intervals indicates that this is never an issue in this larger panel.

liquor sales in Virginia increases minor and major crimes by 5% and 10% respectively. On a more refined level, Rossow and Norström (2012) find that reducing bar opening times in Norway by 1 hour reduces assault rates by about 16%. Similarly, studies on alcohol restrictions in other contexts find notable effects on violent crimes (Biderman et al., 2010; Cook & Durrance, 2013; Barron et al., 2022). While null results for murders and homicides are also common in the literature (Heaton, 2012; Cook & Durrance, 2013), we must critically reflect on the precision of this null result in our study. Considering the elementary state of criminalistics at the end of the 19th century, which heavily relied on witnesses, a smaller clearance rate for murders and homicides is likely to affect our estimation which uses conviction rates.

We also apply the reduced-form specification to study the extensive margin using the number of criminals. Similar to the 2SLS estimators, the results suggest that an increase in the relative barley yield increases the number of criminals involved in violent crimes and assaults (Table C.3). Here, the results for crimes and criminals are even closer than in the IV specification, producing almost the exact same coefficients for the (logged) rate of criminals involved in violent crimes and assaults as for the rate of the respective crimes. Likewise, there is no effect on the number of criminals for other crimes (Table C.3).

Accidents and Suicides

While a higher level of beer consumption appears to cause more individuals to commit violent crimes, the consequences of beer consumption might extend beyond crime. Without blurring the focus of our study, we want to consider two related outcomes: deadly accidents and suicides. Accidents are a relevant outcome as beer was often consumed before work or during breaks. Suicides might be sensitive to beer consumption, too, as a higher alcohol intake can inhibit self-control.

We examine these two outcomes in reduced-form estimates at the district level. Our data, which are limited to a smaller set of years, allow us to consider all accidents and suicides but also gender-specific cases. Motivated by the fact that beer was predominantly consumed by males (Roberts, 1980), we focus on male suicides and accidents. For the latter, rates are computed relative to the male population in a district/year. Replacing the (logged) crime rates with (logged) suicide and accident rates, we maintain all controls used before. Table 3.7 presents our findings.

Table 3.7: Reduced-Form Estimates for Other Outcomes (Prussia District Panel 1882-1900)

Dep. variable (log):	Suicide rate		Accident rate	
	All	Males	All	Males
	(1)	(2)	(3)	(4)
log(Barley)	0.038 (0.031)	0.056* (0.031)	-0.033 (0.043)	-0.029 (0.038)
Observations	648	647	612	540

Note: The table presents reduced-form estimates. All specifications account for district-level fixed effects and include district-specific linear time trends, our basic covariates and controls for rye and wheat yields. Robust standard errors, clustered at the district level, are in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

We observe an insignificantly positive effect of beer on all suicides (Column 1). When we examine suicide rates of males only, this effect becomes stronger and weakly significant (Column 2). The estimates would suggest that a 1% increase in the barley yield, which the first stage links to a 0.07 increase in the beer output, triggers an increase in male suicides by 0.056%. This would imply an elasticity of nearly 0.8. Keep in mind, however, that the estimate for this effect is relatively imprecise.

Looking at accident rates, we do not find any meaningful effects. In fact, the coefficients reported in Columns (3) and (4) of Table 3.7 are negative and, moreover, imprecisely estimated. Considering that traffic-related accidents were not yet relevant in our sample, these results are consistent with those of Cook and Durrance (2013).

3.4 The 1906 Beer Tax Reform: A Diff-in-Diff Approach

3.4.1 Empirical Framework

Our second identification strategy focuses on a brewery tax reform enacted in 1906. Then, the German Empire was divided into five different brewing tax territories. The tax reform only affected the northern tax territory of the brewing tax confederation (*Brauststeuerbund*), which largely resembled Prussia. Following rising debt levels in the German Empire (Schanz & Manicke, 1906, p. 255), the financial reform of 1906 sought to increase revenues by raising the brewing tax. Although widely rejected by the brewing industry, the reform was perceived as long overdue; the increased use of surrogates²² and the greater purchasing power of common workers made the brewing tax appear to be progressively lower compared to other consumption taxes, such as the salt or sugar tax (Schanz & Manicke, 1906). Before the reform, the brewing tax levels approximated 1 cent per liter of the final selling price to consumers, at a time when the price of a typical beer (0.33 – 0.5 liters) started at about 10 cents

²²The surrogates partially substituted the higher taxed barley malt and thus decreased the effective tax rate per liter of beer produced.

(Schanz & Manicke, 1906, p. 261). After the reform, the brewing tax rate in the North German Confederation was increased by approximately 50%, to about 1.5 cents per liter.

This tax increase coincided with the long-term goal, also set out in the constitution, of getting closer to harmonizing taxation throughout the entire jurisdiction of the German Empire. The other four brewing tax territories, namely Bavaria, Baden, Württemberg, and Alsace–Lorraine, already had slightly different²³ yet generally higher taxes in place that were not changed by the financial reform of 1906. Conducive to a comparative framework, Prussia and the southern states that make up most of the rest of the German Empire are highly comparable. They not only shared the same criminal justice system but also had a common culture and similar characteristics as observed by Johnson (2002, p. 131):

“Prussia was a large and representative part of Germany as a whole. It made up roughly 60 percent of the population and land area; it had roughly the same mix of demographic, social, and economic characteristics as the rest of Germany; and the court records demonstrate that its crime trends mirrored the crime trends for the entire Reich.”

We exploit this similarity to study the impact of the tax increase in the brewing tax confederation on crime. For this exercise, we extend the District Panel of Prussia (Table 3.1) to also include the districts within the southern states of Bavaria, Baden, Württemberg, and Alsace–Lorraine. Using the latter as an untreated control group, we begin by estimating a simple 2×2 DID model as a baseline:

$$\text{Crime}_{it}^j = \beta^{\text{did}} \text{Post} \times \text{Prussia} + \sum_i \rho_i^{\text{did}} \mathbb{1}_i + \sum_t \tau_t^{\text{did}} \mathbb{1}_t + \epsilon_{i,t}^{\text{did}} \quad (3.4)$$

where ρ_i and τ_t denote a full set of district and time fixed effects, respectively.²⁴ In the next step, we specify a traditional event study around the reform to gain a more detailed view of the effect trajectory:

$$\text{Crime}_{it}^j = \sum_{t=1899}^{1905} \beta_t^{\text{es}} \text{Prussia}_t^{\text{Pre}} + \sum_{t=1907}^{1909} \beta_t^{\text{es}} \text{Prussia}_t^{\text{Post}} + \sum_i \rho_i^{\text{es}} \mathbb{1}_i + \sum_t \tau_t^{\text{es}} \mathbb{1}_t + \epsilon_{i,t}^{\text{es}} \quad (3.5)$$

where Prussia_t represents dummies for treated Prussia in the years before and after the reform to illustrate possible changes in crime rates j . Due to further increases in the brewing tax with differing magnitudes in the southern states and Prussia from 1908–09 onward, we limit the sample period up to this point in all specifications.

²³For example, taxes in the North German tax territory were based on the weight of brewery inputs, whereas Bavaria followed a volumetric approach.

²⁴ $\mathbb{1}_i$ and $\mathbb{1}_t$ are indicator functions for a given district i or a year t , respectively.

3.4.2 DID Results

Table 3.8 summarizes the 2×2 DID results for violent crimes and the largest subcategories. These results serve as a benchmark for the subsequent event study estimates.

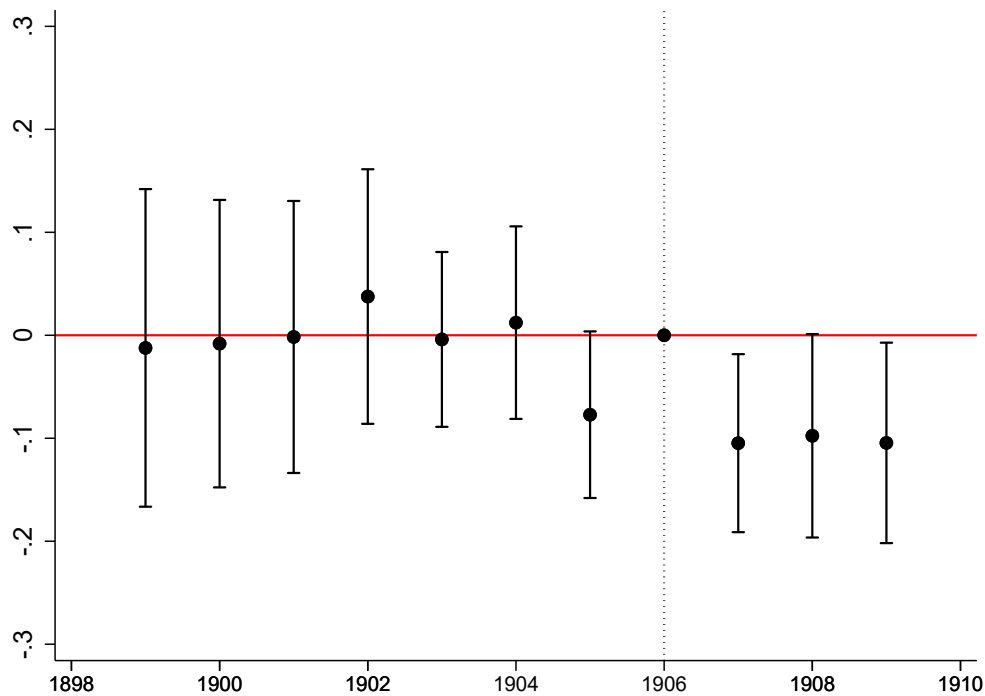
Table 3.8: DID Estimates for Violent Crimes (Extended District Panel)

Dep. variable (log):	Violent crimes	Min. assaults	Maj. assaults	Murder+hom.
	(1)	(2)	(3)	(4)
Post-reform \times Prussia	-0.014 (0.030)	-0.096* (0.052)	-0.012 (0.030)	0.105 (0.119)
Observations	605	605	605	605
District FEs	Yes	Yes	Yes	Yes
Time FEs (yearly)	Yes	Yes	Yes	Yes

Note: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Robust standard errors are clustered at the district level in parentheses.

Whereas the tax reform does not appear to have a pronounced effect on violent crimes in general, we do observe a weakly significant negative effect on minor assaults. In the 3 years following the tax increase in Prussia, rates for minor assaults were around 0.1% lower than in the southern states that comprised most of the rest of the German Empire. Figure 3.4 presents the estimates of the event study, which allow us to explore the trajectory of minor assault rates in more detail.

Figure 3.4: Event Study With Minor Assault Rates



Note: This figure displays the coefficient estimates on logged violent crime rates from a two-way fixed effects estimates with robust standard errors clustered at the district level. Error bars show the 90% confidence intervals.

We observe that prior to the reform, minor assault rates remained relatively stable. However, we must acknowledge an outlier in 1905 immediately before the tax increase. Although not statistically significant, this observation imposes some reservations on the parallel pre-trend assumption in our context. Still, we find that in the years after the reform, minor assault rates are consistently 0.1% below pre-reform levels. Note that the statutory tax increase corresponds to circa 0.5 cents per liter when the price for a liter of typical beer was approximately 20 cents for end consumers. Assuming a pass-through of the brewing tax to consumers between 20% to 80%, we can estimate a range of how the expected price increase of 0.5% to 2% affected beer demand and relates to the observed minor assault rates. With a price elasticity of beer of -0.3 (Nelson & Moran, 2019), the expected reduction in demand would correspond to between -0.015% and -0.6%. For the decrease observed in minor assault rates, this range would imply a beer-assault elasticity of between 0.16 and 0.64. This approximation is smaller than the IV estimate which suggested an elasticity closer to 1. Also, the practical implications of this effect are modest. Bearing in mind that Prussia recorded approximately 24,000 convicted minor assaults in the year 1906, the reduction would imply that the reform avoided 24 minor assaults each year after the reform. Similarly, as with the IV approach, repeating the exercise with the rate of criminals instead of crimes yields highly similar coefficients; however, they do not produce any additional insights.

Turning to other crime categories, we find that the 2×2 DID estimates suggest null results similar to the IV estimates, with the exception of property crimes.

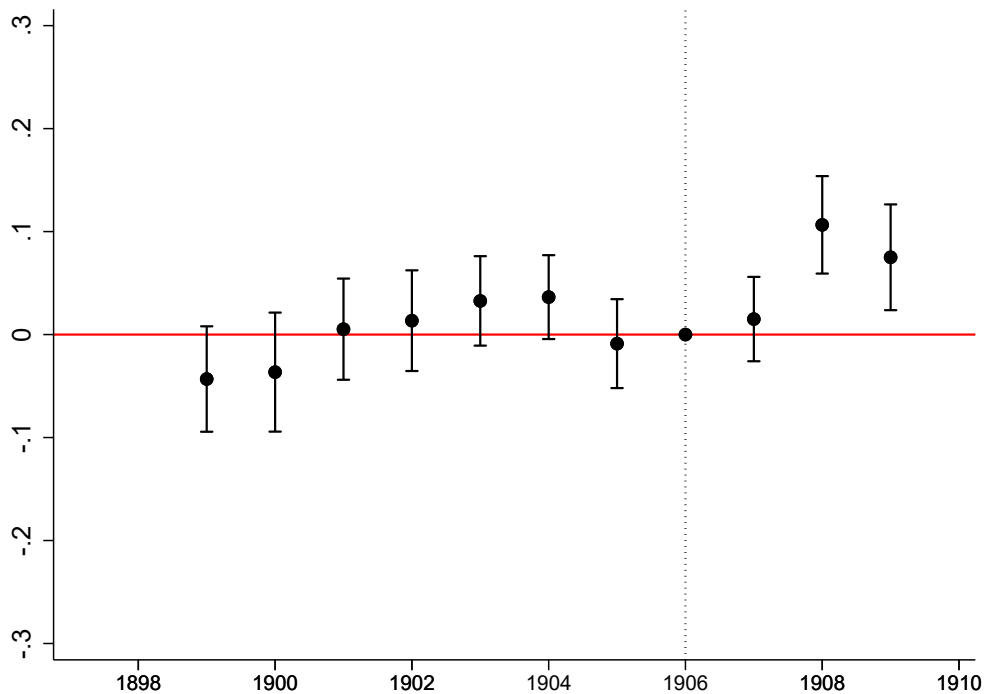
Table 3.9: DID Estimates for Other Crimes (Extended District Panel)

Dep. variable (log):	All crimes	Property crimes	Vandalism
	(1)	(2)	(3)
Post-reform \times Prussia	0.034 (0.023)	0.066*** (0.024)	0.043 (0.039)
Observations	605	605	605
District FEs	Yes	Yes	Yes
Time FEs (yearly)	Yes	Yes	Yes

Note: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Robust standard errors are clustered at the district level in parentheses.

Here, we notice a relatively small yet significantly positive estimate for the period in Prussia after the increase. When we study this effect on property crimes more closely using the event study estimates presented in Figure 3.5, we spot two crucial caveats.

Figure 3.5: Event Study With Property Crime Rates



Note: This figure displays the coefficient estimates on logged property crime rates from a two-way fixed effects estimates with robust standard errors clustered at the district level. Error bars show the 90% confidence intervals.

First, the pre-treatment period appears to be subject to a small but steady positive

trend that we cannot rationalize. Possibly extending into the period after the reform, this limits any inference that would attribute an increase in the property crime rate after 1906 to the tax reform. Second, the positive coefficient visible in the 2×2 DID estimates seems to reflect a trajectory that unfolds only in later years. Immediately after the reform came into effect, the estimate remains near zero. Considering the problematic pre-trends and the unexplained temporal offset, we must interpret this small effect on property crimes carefully. This notion is further underscored by a subsequent robustness check, which is described in the following subsection.

3.4.3 Robustness Check: Synthetic Control Method

As a robustness check, we complement the DID results with a synthetic control approach. This strategy aims to mitigate the drawback of the DID approach, namely its inability to utilize information from additional covariates. The reason for this limitation comes from natural data constraints. In contrast to Prussia, district-level data on sociodemographics for the rest of the German Empire are only available for 1900. Whereas these covariates cannot be exploited in a traditional event study design, we can use this information from the pre-treatment period to calibrate synthetic control units from the donor pool of southern states.

In short, the synthetic control approach employs the same data as the event study, but with the modification that all Prussian districts are aggregated²⁵ to become one treated unit subject to the brewing tax reform. Since crime levels have notable differences between districts, this would potentially bias the effect estimates (Abadie & L'hour, 2021; Ben-Michael, Feller, & Rothstein, 2021). Hence, we follow the bias-correction procedure proposed by Wiltshire et al. (2021). As the synthetic control approach reduces the sample size further by assigning certain districts a weight of zero, we only consider this approach suitable for aggregated crime categories that exhibit less variance.

To create a synthetic Prussia, we include all districts from the southern states as potential donors. The weights of the southern districts are chosen to minimize the root mean squared prediction error (RMSPE) in the pre-intervention period and are based on the covariates of the population density (1900), share of employed workers in industry (1900[1895]), share of the population with German as their mother tongue (1900), share of married men (1900), and share of the population aged between 20–49 years (1900). Finally, we include the respective crime rate for 1906 as a predictor.

Beginning with a focus on violent crime rates, the synthetic control units appear to represent a wide combination of districts from the southern regions of the sample. The districts in the state of Alsace–Lorraine at the French border represent a slight outlier, receiving a more pronounced weight (Table 3.10).

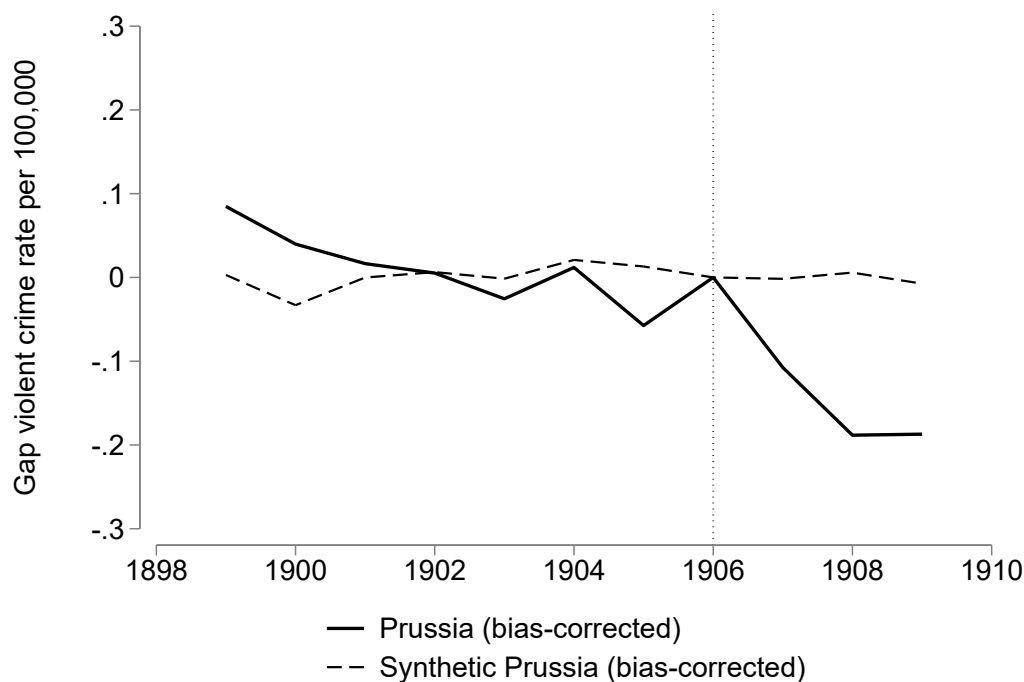
²⁵All variables are aggregated by first adding up absolute values, such as the number of crimes or the size of the district, before calculating any of the relative measures like crime rates or population density.

Table 3.10: District Weights for Synthetic Prussia

State	District	Weights	
		Violent crime model	Property crime model
Bayern	Niederbayern	0.19	0.43
Bayern	Oberbayern	0	0.15
Elsass-Lothringen	Lothringen	0.27	0.16
Elsass-Lothringen	Oberelsass	0.37	0.26
Württemberg	Jagstkreis	0.17	0

This selection must be noted with caution since the districts in Alsace-Lorraine are characterized by a relatively large share of foreigners with French as their native language. However, bearing in mind that the eastern parts of Prussia also feature districts where large segments of the population are not German native speakers, this balance appears plausible for constructing a synthetic Prussia.

Figure 3.6 depicts the trajectory of the violent crime rate for Prussia, where brewing taxes increased as of 1906, and compares it to the trajectory of synthetic Prussia, where brewing taxes remained constant.

Figure 3.6: Prussia vs Synthetic Prussia for Violent Crime Rates

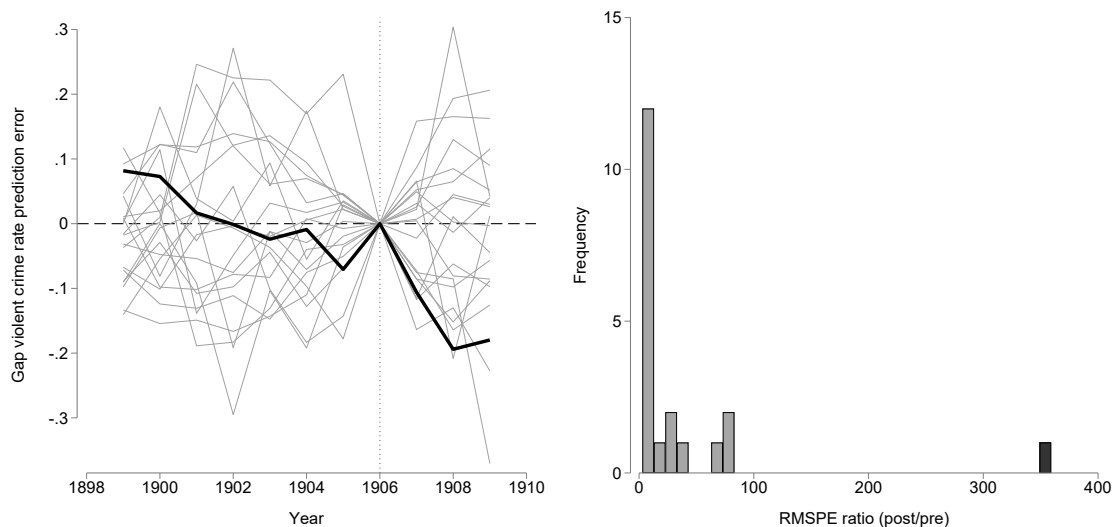
Note: This figure displays the gap of the (logged) violent crime rates for Prussia (solid line) and Synthetic Prussia (dashed line). Following the tax reform in 1906, violent crime rates exhibit a notable decrease.

Before the reform, Prussia and synthetic Prussia exhibit comparable trends. However, both trajectories diverge sharply thereafter. Whereas violent crimes remain

constant in the synthetic control group, the violent crime rate in Prussia follows a similar trajectory as in the event study. The (bias-corrected) gap suggests a decrease of -0.11% for violent crimes per 100,000 in 1907. Then, the gap increases further to approximately -0.18% in 1908 and 1909. Encouraged by these results, we further test whether these effects hold when using aggregated assault rates only. Figure C.4 shows that the findings are almost identical, suggesting that the drop in violent crime convictions is predominantly driven by a drop in assaults.

Note that the synthetic control estimates exceed the event study estimates. Using the same assumptions as in Subsection 3.4.2, the synthetic control estimates suggest a beer-assault rate elasticity of between 0.3 to 1.2 which encompasses the magnitude observed under the IV approach. These estimates remain stable even when individual predictor variables are omitted for constructing the synthetic control weights (Figure C.5). Likewise, removing individual donor districts does not diminish the effect of violent crimes (Figure C.6). We show next that the bias-corrected synthetic control estimation appears robust to an in-space placebo test as well as a comparison of pre- and post-reform RMSPE values (Figure 3.7).

Figure 3.7: Placebo Plot and Post/Pre RMSPE Ratios for Violent Crime Rates



Notes: The left side of the figure displays the violent crime trend of Prussia (solid line) compared to the trends of individual placebo estimations for all donor units. The right side of the figure illustrates the ratio of the post-treatment RMSPE and the pre-treatment RMSPE of the main specification. The decrease of Prussia marks the strongest outlier.

Prussia experienced one of the most consistent drops in violent crime after the 1906 tax reform. The robustness of this decline is visible in the histogram of the post-/pre-reform RMSPE ratio for each placebo unit. By normalizing the differences between the model fit before and after the intervention, this ratio can be used as a test statistic to guide inference (Abadie & L'hour, 2021). Prussia shows the largest

RMSPE ratio, which suggests that the deviation of Prussia from the model fit post-reform is unlikely driven by chance: Ranking first out of the placebo pool of the 19 other districts, Prussia's RMSPE ratio suggests a p value of $1/20 = 0.05$. The outcome of the synthetic control method seems to affirm the robustness of the previous results for violent crimes.

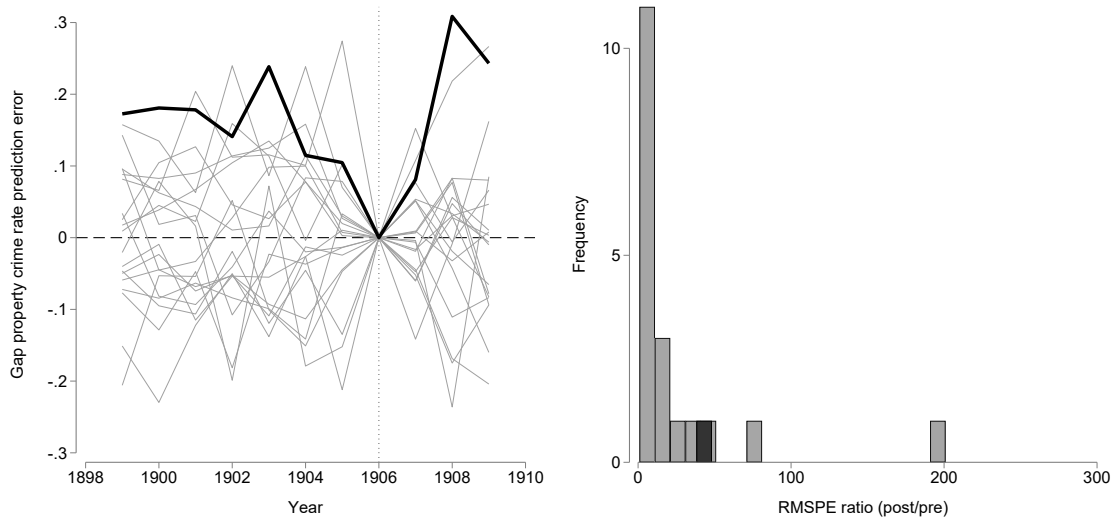
However, replicating the exercise for property crimes casts further doubt on a compelling effect of the reform on property crime rates. Figure 3.8 displays the trends in property crime rates in Prussia and synthetic Prussia.

Figure 3.8: Prussia vs Synthetic Prussia for Property Crime Rates



Note: This figure displays the gap of the (logged) property crime rates for Prussia (solid line) and Synthetic Prussia (dashed line). Even before the reform, Prussia and Synthetic Prussia exhibit a poor fit.

Despite reasonably balanced donor weights (Table C.4), property crime rates exhibit a weak fit to the model even before reform. Although property crimes do increase thereafter, this increase pales in comparison with the weak pre-reform fit. Hereby, the robustness check mirrors the ambiguous trajectory observed in the event study. Evaluating the estimations against the placebo estimates (Figure 3.9) further suggests that the deviation of Prussia after the reform is not unique.

Figure 3.9: Placebo Plot and Post/Pre RMSPE Ratios for Property Crime Rates

Note: The left side of the figure displays the property crime trend of Prussia (solid line) compared to the trends of individual placebo estimations for all donor units. The right side of the figure illustrates the ratio of the post-treatment RMSPE and the pre-treatment RMSPE. The property crime trend in Prussia after the reform does not stand out.

The RMSPE ratios substantiate this notion (Figure 3.9). With Prussia ranking fourth overall ($p = 0.2$), the gap as measured by the post-intervention RMSPE seems unassertive of a causal effect of the tax reform on property crimes.

3.5 Conclusion

This study examines the effect of beer availability on crime rates. Making use of newly digitized data on crime statistics from Prussia and the German Empire between 1882 and 1913, we develop two empirical strategies to identify a causal effect.

In our first approach, we use barley yield as a new instrument to exploit exogenous variation in the amount of beer brewed. After demonstrating that regional barley yield is a strong predictor of beer production, we use this instrument to estimate the effect of beer on various crimes. Consistent estimators across our 2SLS and reduced-form specifications suggest that barley-induced changes in beer production drive crime rates. Our results indicate that violent crimes appear close to unit-elastic: Increasing beer production by 1% increases violent crime rates to the same extent. This effect on violent crimes appears to be primarily caused by assault rates. Noteworthy, we do not find an impact on murder or homicide rates, nor do our results validate an effect on property crimes.

Our second empirical approach complements these findings by studying a brewing tax increase in Prussia. We use a traditional DID setup to compare Prussian crime rates around the reform to the southern states that left their brewing tax unchanged. While less pronounced, the results support the notion that making beer less affordable affects violent crimes, primarily by reducing the rate of assaults. A subsequent robustness check employing a synthetic control approach corroborates these findings.

Overall, the present study supports the evidence that suggests that alcohol predominantly affects violent crime (Biderman et al., 2010; Heaton, 2012; Barron et al., 2022), and particularly assaults (Cook & Durrance, 2013), while contributing new empirical approaches and an original context. Still, we must note that our setting introduces several limitations. First and foremost, being restricted to convicted crimes prevents us from verifying whether an alcohol-induced increase in convictions is a result of more criminal behavior, a higher propensity for criminals to get caught, or an increased risk of victimization. Our findings are further confined to justiciable crimes in the German Empire; hence, important harmful behaviors are not measurable in our data, such as sexual abuse and domestic violence. As beer was largely consumed outside the home, we cannot rule out the possibility that increased violence from alcohol consumption reflects, in part, increased social contact at bars and restaurants.

In addition, both empirical methods are not exempt from constraints. Despite a robust first stage, the IV strategy has a weaker theoretical base toward the end of our sample period, when improved modes of transportation would have made grain and beer markets more integrated (Wolf, 2009). Other concerns must also be acknowledged in the analysis of the tax reform. The DID estimation around the reform lacks vital yearly controls on important sociodemographics, which the synthetic control approach cannot fully overcome due to the comparatively few periods

observed.

Although our study is located in a historically unique setting, it has modern implications. Currently representing the main global source of alcohol consumption, the ever-greater popularity of beer extends beyond Germany (World Health Organization, 2019). Paying tribute to this aspect of global industrialization, our study highlights the potential social costs of increased alcohol availability by emphasizing its effect on violent crime. Furthermore, we extend the range of settings to study the consequences of alcohol. Corroborating that individuals respond to alcohol at the margin, our results contribute to discussions on regulatory options and efficient alcohol regulation (Cnossen, 2007; Cook, 2007; Cawley & Ruhm, 2011). Lastly, our study demonstrates that historical records harbor valuable insights into criminal behavior that can be leveraged using modern econometrics (R. Jia, 2014; Pinotti, 2015; Bignon et al., 2017; Bindler & Hjalmarsson, 2021; Melander & Miotto, 2023). While acknowledging the challenges associated with historical data analysis, we hope to encourage further exploration of historical archives to gain a deeper understanding of past and contemporary economic issues.

Appendix A

Appendix for The Cost of Honesty: Field Evidence

This publication and its appendix is embargoed for publication until December 2024.

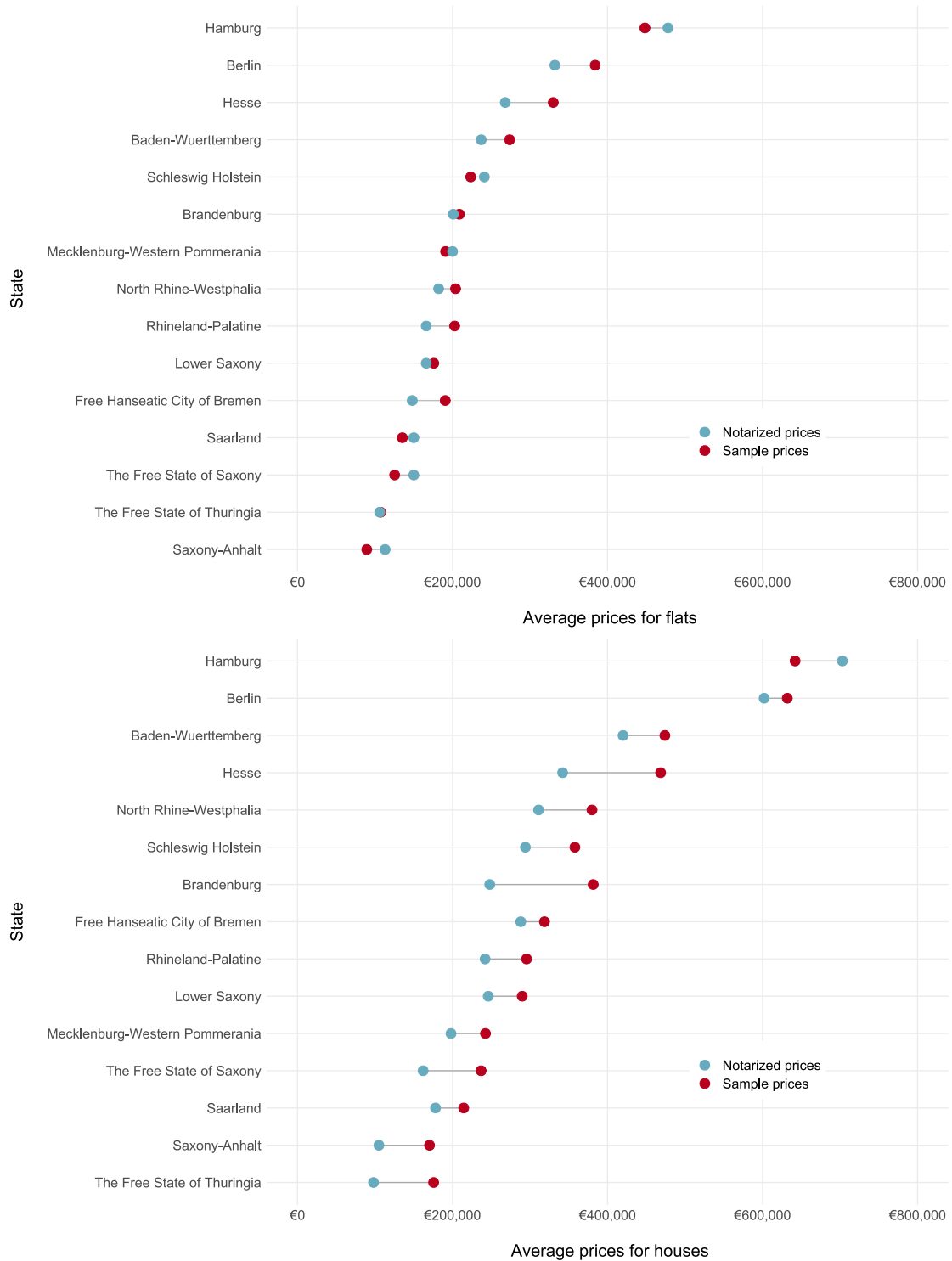
Appendix B

Appendix for How not to Reduce Commissions

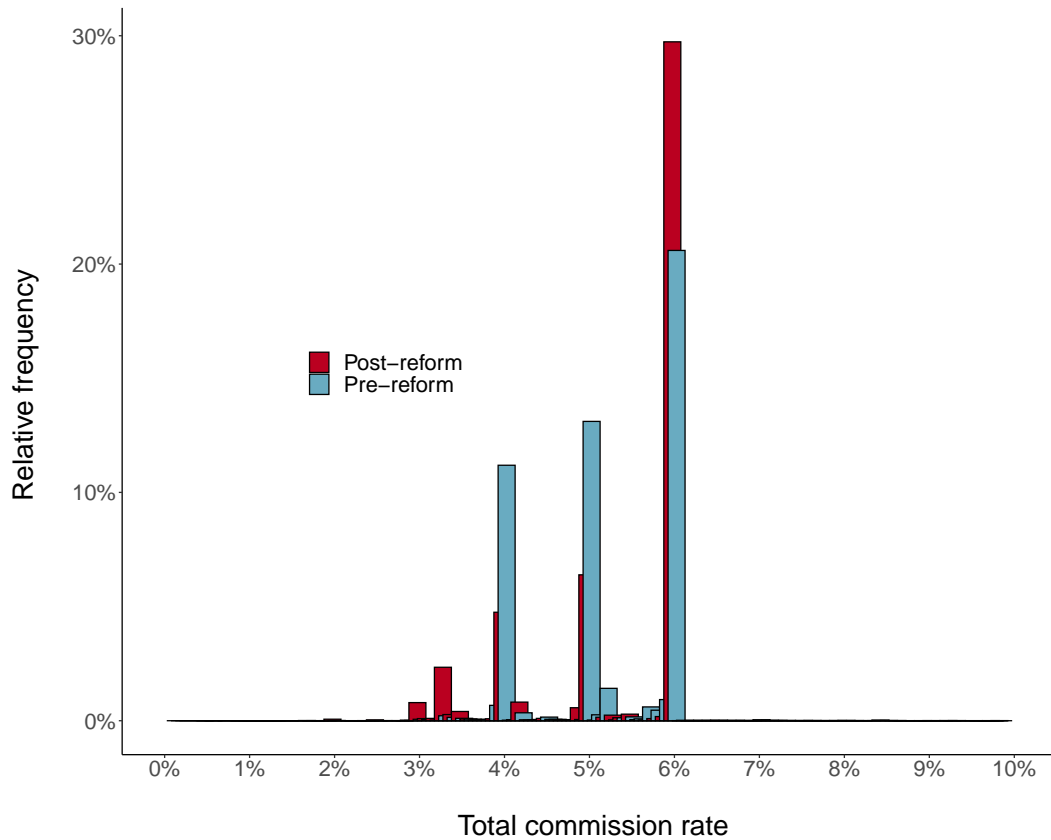
Part I: Data Processing, Supplementary Tables and Figures

Data Processing. To correct for errors that mostly stem from typos or classifications errors, I begin by omitting listings with implausible values considering their declared floor space, price, or their combination. In a second step, properties with highly unusual characteristics or at the extreme end are discarded. This typically concerned properties with a value below €20,000, which often represent misclassified parking spaces or undeveloped plots of land. On the upper end of the spectrum, properties with prices above €2,500,000 are omitted, which either display luxury properties with highly sophisticated features that the available variables cannot adequately control for, or entire apartment buildings listed incorrectly as a single object. For the same reason, objects with a price per m² above €12,000 are omitted. I set the minimum floor space to 15 m² and exclude flats larger than 200 m² or houses larger than 250 m², as these regularly include hotels or restaurants indicating a commercial use. Similarly, the sample is limited to objects with 1 to 15 rooms. Naturally, duplicates of the same property are not considered.

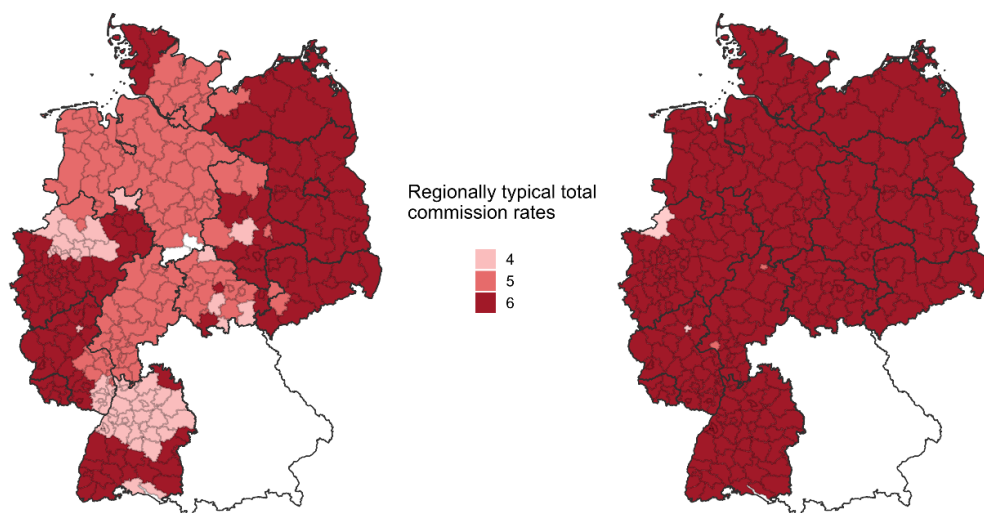
Figure B.1: Comparing Sample Housing Prices With Notarized Data



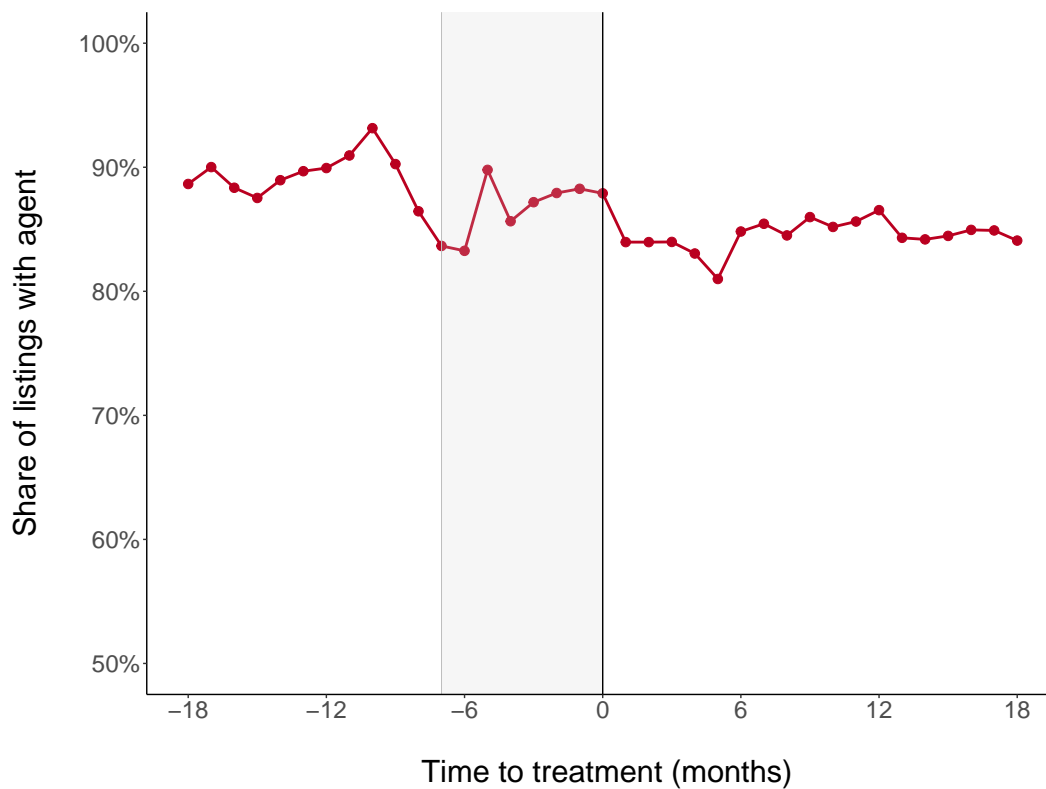
Note: This figure compares the average housing prices in each state within the listing price sample and the official transaction data. The official data is provided by the German committee on land and property evaluation ('Gutachterausschuss') in its national report for the year 2020.

Figure B.2: Histogram of Total Commission Rates Before and After Reform

Note: This figure shows the distribution of total commission rates split by the periods before and after the legal reform. Although reflecting the general increase of commission rates, a notable share of agents still advertises lower rates after the reform.

Figure B.3: District Variation of the Most Frequent Commissions Rates

Note: The maps display the most frequent total commission rates for all listings by real estate agents before the reform in December 2020 (left) and thereafter (right).

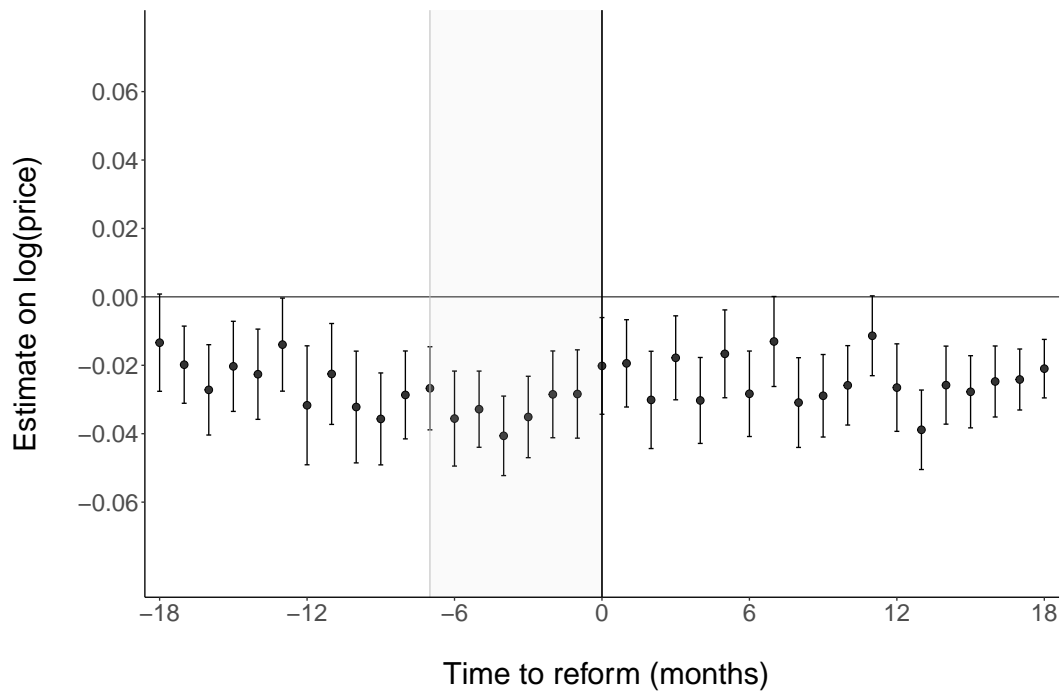
Figure B.4: Descriptive Mean Share of Listings With Real Estate Agents

Note: The figure displays the share of listings offered by real estate agents over time. The data covers all listings that are either listed by an agent or by the owner directly. Note that the sample in this study does not include other seller types, such as commercial developers, which is why the data overestimates the use of real estate agents in Germany.

Table B.1: Illustrative Numerical Example for the Theoretical Incidence Analysis

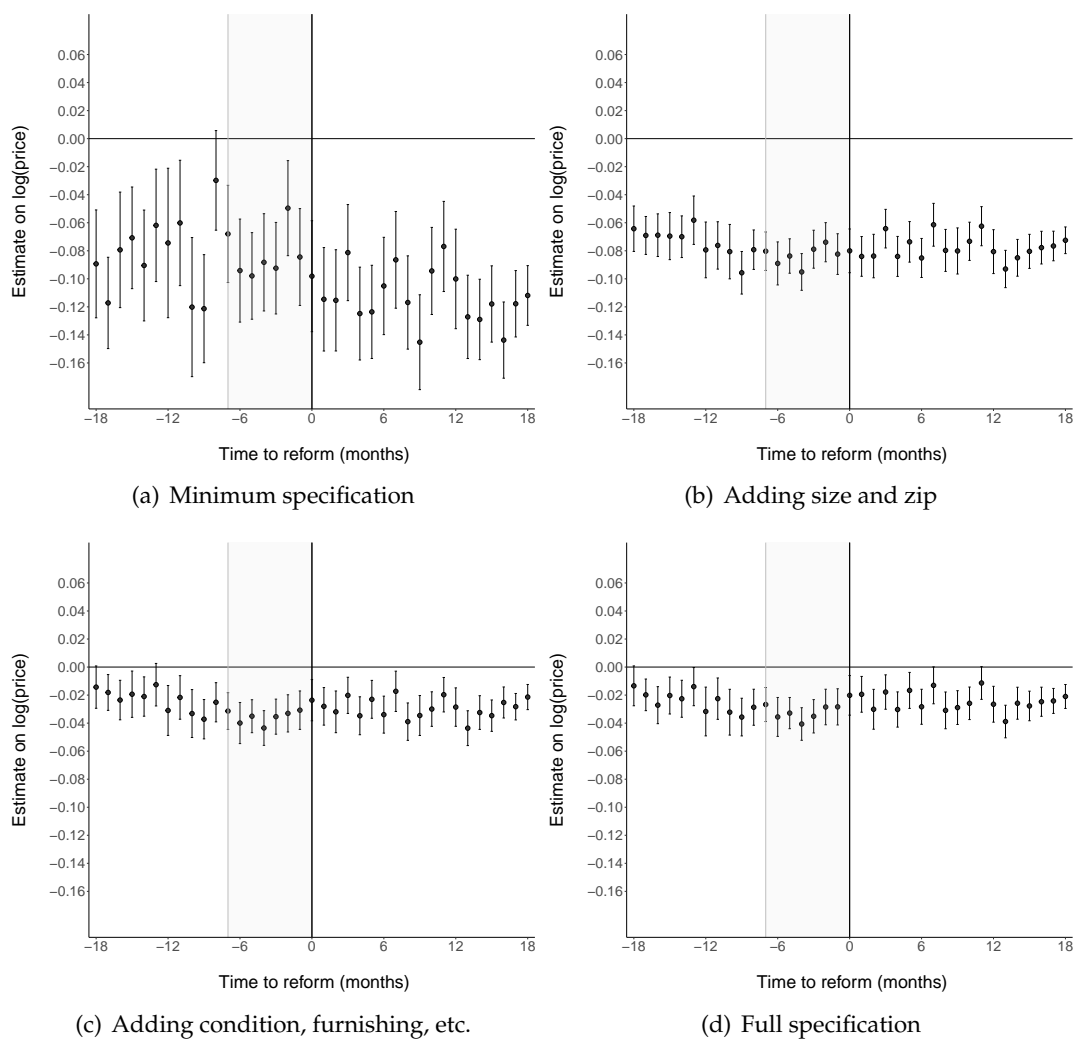
	Pre-reform	Post-reform
Statutory incidence buyer (relative)	100%	50%
Statutory incidence seller (relative)	0%	50%
Eq. price w/o agent (p')	103,000	103,000
Eq. selling price w/ agent (p_t^*)	100,000	103,000
Commission rate (τ_t)	6%	5.825%
Earnings of real estate agent ($\tau_t \times p_t^*$)	6,000	6,000
Economic incidence buyer (relative)	50%	50%
Economic incidence seller (relative)	50%	50%
Economic incidence buyer (nominal)	3,000	3,000
Economic incidence seller (nominal)	3,000	3,000
Commission-inclusive price buyer (p_t^{*B})	106,000	106,000
Commission-inclusive price seller (p_t^{*S})	100,000	100,000

Note: This example serves to illustrate the concept of the incidence framework presented in Section 2.3. For ease of presentation, I assume a hypothetical economic incidence for buyers and sellers of 50% and an equilibrium price without a real estate agent of 103,000. The reform only shifts the statutory incidence of buyers to sellers. The equilibrium price without a real estate agent remains the same throughout (103,000). The lower pre-reform equilibrium selling price with a real estate agent (100,000) reflects the economic incidence of the seller, while the increase after the reform (103,000) displays the inclusion of the statutory shift, reflecting the principle of the liability side equivalence. For the same reason, the commission-inclusive prices for the buyer p_t^{*B} and the seller p_t^{*S} remain constant. The statutory shift should not alter the market conditions of buyers, sellers, or real estate agents. Consequently, the post-reform commission rate τ_{post} adjusts so that all parties receive the same outcome as before.

Figure B.5: Event Study Estimates for Raw Listing Prices Around the Reform

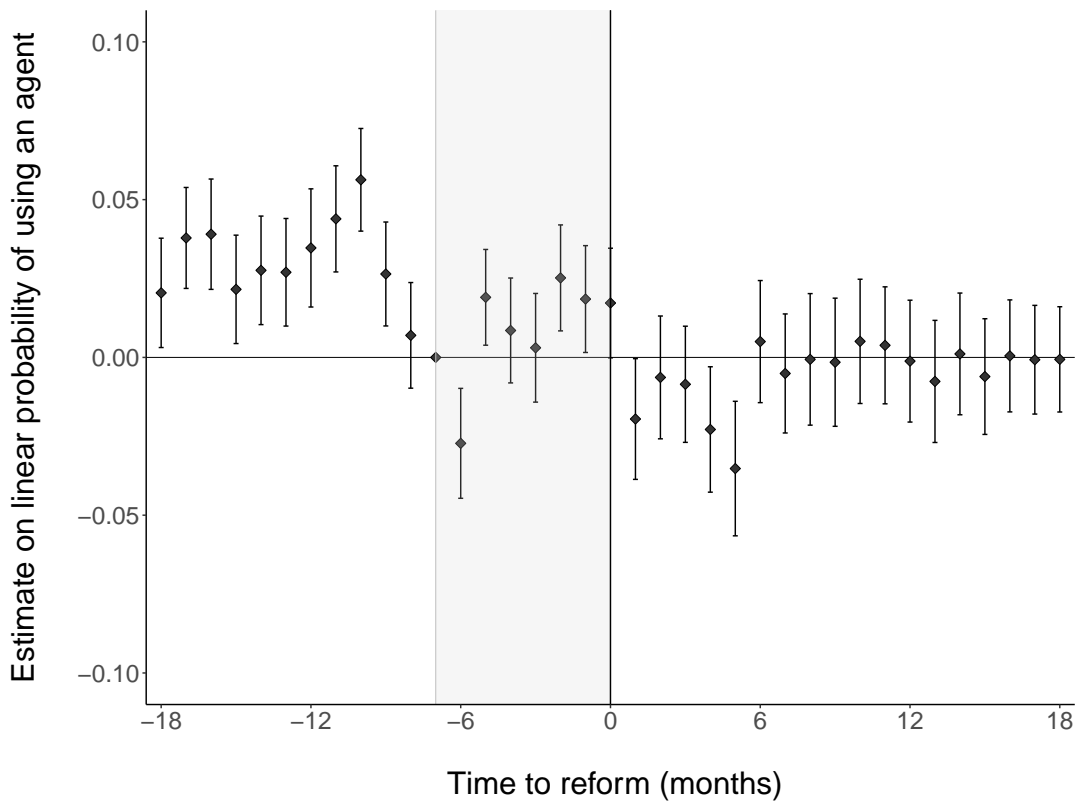
Note: The figure presents event study estimates for raw listing prices around the reform. The underlying model controls for relevant property characteristics and uses regional (zip) fixed effects. Standard errors are clustered on zip codes and represent 95% confidence intervals. To account for anticipation effects, the model uses $t = -7$ (May 2020) as the reference period when the reform was announced and was passed by the German Parliament. The reform came into effect in $t = 0$ (December 2020). Listing prices hardly adjust to the statutory shift of the legal reform.

Figure B.6: Robustness Check With Step-Wise Price Estimations



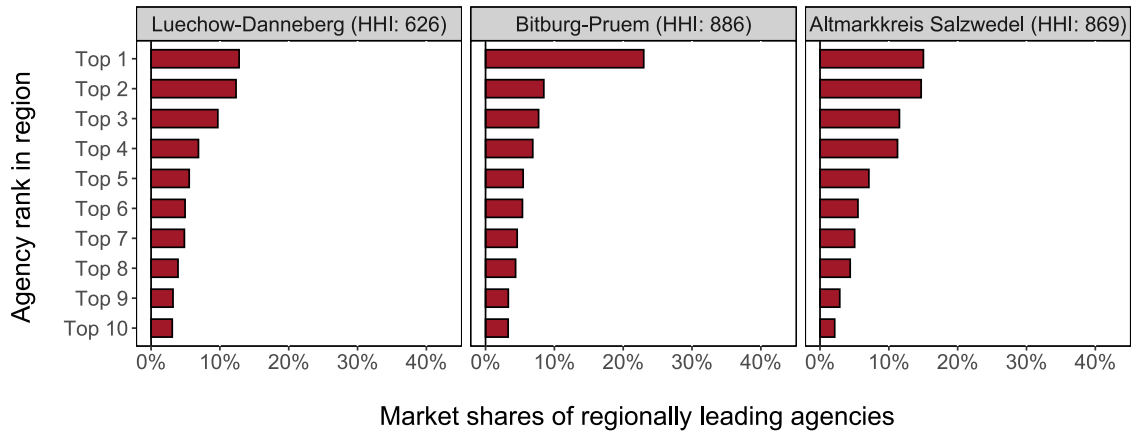
Note: These figures display the event study estimates on raw (logged) listing prices as in Figure B.5, successively adding additional covariates: (a) Minimum model only controlling for $m_t \times A_i$ and monthly FEs; (b) Adding (squared) object/lot size and regional FEs; (c) Adding furnishing, condition, category, and heating type; (d) Full specification. Although the price gap between listings offered by real estate agents and those for sale by owner is in part driven by different characteristics, a substantial gap remains even when controlling for all available characteristics in the full specification.

Figure B.7: Linear Probability Model of Using a Real Estate Agent



Note: The plot shows the coefficient estimates for a linear probability model with the usage of a real estate agent as dependent variable over time. Apart from the dependent variable, the model is identical to the event study on the commission rate (Equation 2.5) and uses regional (zip) fixed effects. Standard errors are clustered on zip codes and represent 95% confidence intervals. As the law was enacted in May 2020 and came into effect in December 2020, I use May 2020 ($t = -7$) as reference period. Splitting the sample according to pre-reform commission levels does not produce qualitatively different results.

Figure B.8: Market Shares in Three Exemplary Rural Districts



Note: These plots show the market shares of regionally individual top 10 agencies based on the listing volume from a snapshot for houses on the largest real estate platform in February 2023. The Herfindahl-Index (HHI) for each region is provided in brackets. To reflect the mobility constraints of real estate agents, listings were selected if they fell within a radius of 20 kilometers within the center of each district. The displayed regions are randomly sampled from the 20 most rural regions in Germany as defined by Thünen (2023). Even in the most rural regions, market concentration appears low.

Part II: Survey Appendix

Survey Materials

Figure B.9: Translated Copy of the Survey Invitation

Dear Mrs _____,

We are researchers from the Hertie School in Berlin and would like to offer you the opportunity to participate in our study on the motives of home sellers. The survey takes approximately three minutes.

In return for participation, you will receive exclusive access to a report of the study which will not be published in this bundled form otherwise. The report may help you to gain a competitive edge compared to other real estate agents in your region by showing how e.g. other real estate agents acquire new sellers most successfully.

If interested, please follow this link: [To the survey](#)

Thank you for your support!

With kind regards,

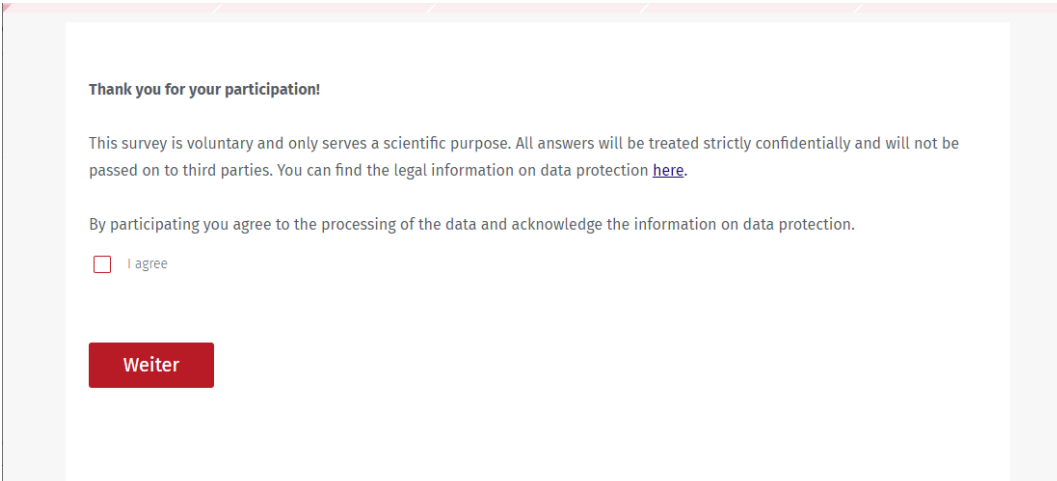
Julius Stoll

Julius Stoll
PhD Student | MSc. Economics

Hertie School
Friedrichstraße 180
10117 Berlin · Germany

Mobile: +49 162 208999

Figure B.10: Translated Copy of the Welcome Page



Thank you for your participation!

This survey is voluntary and only serves a scientific purpose. All answers will be treated strictly confidentially and will not be passed on to third parties. You can find the legal information on data protection [here](#).

By participating you agree to the processing of the data and acknowledge the information on data protection.

I agree

Weiter

Survey

[Welcome Page]

1. How do most sellers learn about you?

- Real estate platform (e.g. ImmobilienScout24)
 - Internet search (e.g. Google)
 - Cold call
 - Physical shop/office
 - Personal contact (e.g. friends, family)
 - Referral from other sellers
 - Other: (please indicate)
-

2. What do you believe are services that current sellers find particularly convincing?

- Modern exposé (e.g. virtual viewings, drone images)
 - Creation of professional floor plan
 - Variety of platforms used
 - International marketing
 - Permanent reachability
 - Other: (please indicate)
-

3. What is the current regionally typical seller commission rate in your state? (incl. VAT)

[Slider] Percent (incl. VAT) 2 [- - - - - • - - - - -] 5

For statistical purposes, we now ask you to consider your most recent seller only.

- Understood
-

4. What do you believe, what is the most important reason that your most recent customer chose you as their agent?

- High selling price
 - Fast sale
 - Extensive service portfolio
 - Trust
 - Low commission
 - Visibility of physical office/shop
 - Other: (please indicate)
-

5. What do you believe, what is the most important reason that your most recent customer chose you as their agent?

- High selling price
 - Fast sale
 - Extensive service portfolio
 - Trust
 - Low commission
 - Other: (please indicate)
-

6. Did your most recent seller negotiate the commission, or not?

- Yes
 - No
-

6.-No If No, what do you believe, why not?

- Seller expects offered service to lead to comparably higher selling price that compensates for commission cost
 - Seller is guided by typical regional commission level or the commission rate of other listings
 - Seller is not explicitly aware that commission can be negotiated
 - Other: (please indicate)
-

6.-Yes If Yes, what do you believe, why yes?

- Seller expects a lower selling price, as buyer pays for higher buying commission
 - Seller observed other listings that had lower commissions
 - Seller received advice to negotiate commission
 - Other: (please indicate)
-

7. How much did your last seller know about the current legal framework concerning the commissioning of real estate agents?

[7-point scale] My last seller knew the framework...

[Very imprecisely] [imprecisely] [somewhat imprecisely] [more or less] [somewhat precisely] [precisely] [very precisely]

8. Please describe the profile of your latest seller

- Less than 40 years old
- 41-50 years old
- 51-60 years old
- 61-70 years old

- 71-80 years old
 Over 80 years old

Home value

- Below 200,000 Euro
 200,001 to 400,000 Euro
 400,001 to 600,000 Euro
 600,001 to 800,000 Euro
 800,001 to 1,000,000 Euro
 1,000,001 to 1,200,000 Euro
 1,200,001 to 1,400,000 Euro
 over 1,400,000 Euro


Sex

- Male
 Female

Compared to other sellers: How much experience did your most recent seller have in the real estate market?

[7-point scale] My last seller had ... experience.


[much less] [less] [somewhat less] [average] [somewhat more] [more] [much more]

 **If your estimate in this question matches the outcome of a separate analysis with listings, you will receive a 5-Euro Amazon gift card via email.** [Only in incentivized group; order of this and next question is random]

(Your Answer is correct when it matches the analysis to +/- 0.03 percent)

9. What do you estimate: What is the current average seller commission rate in your state? (incl. VAT)

[Slider] Percent (incl. VAT) 2 [- - - - - ● - - - - -] 5

 **If your estimate in this question matches the outcome of a separate analysis with listings, you will receive a 5-Euro Amazon gift card via email.** [Only in incentivized group; order of this and previous question is random]

(Your Answer is correct when it matches the analysis to the nearest percentage point)

10. What do you estimate: Do real estate agents achieve a higher or lower price than comparable listings for sale by owner?

[Discrete slider] Real estate agent achieve percent less/more -5 [- - - - - ● - - - - -]
+5

11. I want to receive the short report of this study via e-mail once it is completed
- Yes, to the e-mail address I have been initially contacted with
 - Yes, but to another e-mail address: [please indicate]
 - No
-

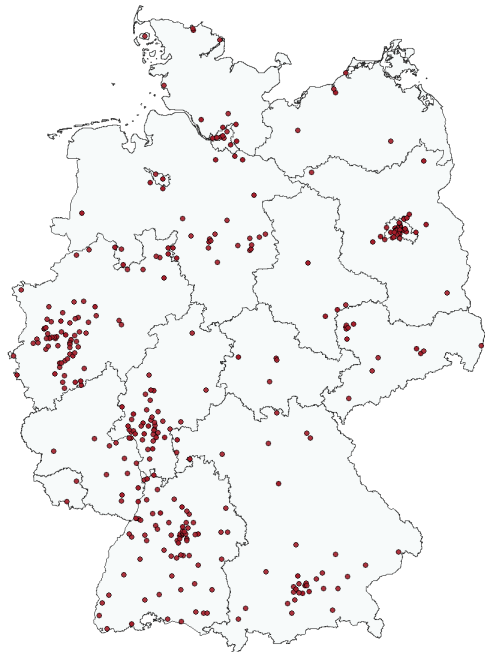
Thank you for participating in this survey.

If indicated, you will receive the short report with the results of this survey within the next 3 months, after all the data has been collected and analyzed. If your answer was correct, you will receive the Amazon gift card to your preferred e-mail address.

Survey Sample

Figure B.11 displays the results from testing the participation conditional on observable characteristics. I limit this test to the subject pool recruited from the largest online platform because this platform also provided information on the number of listings each agency had on offer when collecting the data. Furthermore, I construct a proxy for the size of each real estate agency¹ based on unique e-mail domains. Note that the number of listings exhibits a slightly negative coefficient. As the survey incentivized respondents with information to improve marketing, this effect appears natural but for the present research, negligible. Similar considerations explain the weak effects for certain states representing Germany's most contracting real estate markets.

Figure B.11: Survey Sample Selection



	Survey Participation	
	Estimate	Std. Error
Firm size	0.003	(0.004)
Firm size ²	-0.000	(0.000)
Number of listings	-0.003*	(0.001)
Brandenburg	0.456*	(0.214)
Berlin	0.356	(0.195)
Bremen	-0.749	(0.607)
Baden-Wurttemberg	0.165	(0.184)
Hamburg	-0.035	(0.242)
Hesse	0.012	(0.198)
Mecklen. Wes.-Pom.	0.536*	(0.261)
Lower Saxony	0.189	(0.217)
North Rhine-West.	-0.013	(0.179)
Rhineland-Palat.	-0.074	(0.229)
Saxony	0.529*	(0.269)
Schleswig-Holstein	-0.089	(0.247)
Saarland	-0.056	(0.540)
Saxony-Anhalt	0.317	(0.234)
Thuringia	0.229	(0.339)
Constant	-2.475***	(0.152)
Observations	8,220	

Note: The table gives the regression estimates for participation conditional on the collected covariates. The sample represents the pool of the invited candidates from the largest platform where the number of listings that a real estate agent offered was available when collecting the data. The map depicts the locations of all the responses from this subsample. Overall, participation appears regionally balanced and largely independent of firm size. Significance levels: ***: 0.001, **: 0.01, *: 0.05.

¹Although most real estate agencies are run by fewer than 2-3 agents, some larger firms exist with offices across Germany.

Additional Exploratory Evidence

Demographics

Several survey items ask real estate agents about a seller's age, gender, experience on real estate and the regulation of real estate agents, and the corresponding price of the object. Given the limitation of sampling real estate agents, some variables are collected as coarse intervals. Table B.2 presents the estimates for the propensity to negotiate the real estate agent's commission conditional on the characteristics of the home seller.

Table B.2: Correlates With the Negotiation Decision of Sellers

	Dependent variable: Negotiated (1 = Yes)
Female	-0.672** (0.217)
Age 41-50	-0.009 (0.342)
Age 51-60	-0.367 (0.338)
Age 61-70	-0.832* (0.379)
Age 71-80	-1.027 (0.566)
Age 80+	-1.562 (1.098)
Experience real estate	0.051 (0.065)
Knowledge regulation	-0.157** (0.054)
Price 200,001 - 400,000	-0.220 (0.354)
Price 400,001 - 600,000	-0.146 (0.361)
Price 600,001 - 800,000	0.708 (0.389)
Price 800,001 - 1,000,000	1.084* (0.435)
Price 1,000,001 - 1,200,000	1.233* (0.571)
Price 1,200,001 - 1,400,000	0.914 (0.639)
Price greater 1,400,001	1.360** (0.429)
Sending wave FE	Yes
Observations	1,062

Note: Significance levels: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

The model suggests that female home sellers negotiate significantly less. Compared to men, they are only half as likely to demand a lower commission rate. Although in line with findings from similar contexts in which the option to negotiate is not explicit (e.g. Leibbrandt & List, 2015), the magnitude is notable. Similarly, I find that sellers beyond retirement age are less likely to negotiate.

Interestingly, experience on the real estate market fails to predict the home sellers' tendency to negotiate, whilst the regulatory knowledge of real estate agents does. However, the direction of this effect is unexpected: when home sellers appear² to be more knowledgeable about how real estate agents are regulated, they exhibit less inclination to demand a lower commission rate. Although speculative,

²Specifically, the survey item asked: "How much did your last seller know about the current legal framework concerning the commissioning of real estate agents?"

this effect may reflect a common misconception about the reform, in that splitting the obligation to pay is communicated as if the reform also mandated a fixed 3% commission rate for both. As a result, home sellers may be considered well informed about the regulation, despite misunderstanding that the commission rate is not fixed but freely negotiable. Finally, higher priced homes are associated with a significantly higher propensity for sellers to negotiate with the real estate agent. This correlation appears natural: individuals selling a higher value property have higher stakes when demanding a lower commission and will likely have a higher socio-economic status.

Real Estate Agents' Beliefs

Next to demographic traits, real estate agents are also questioned about their beliefs regarding why home sellers demand a lower commission or not. The options are based on the conjectures from the findings of the price increase and reflect the seller's perception of the incidence, and the perceived level of reference commission rates. For sellers who did not negotiate, real estate agents had the following choices:

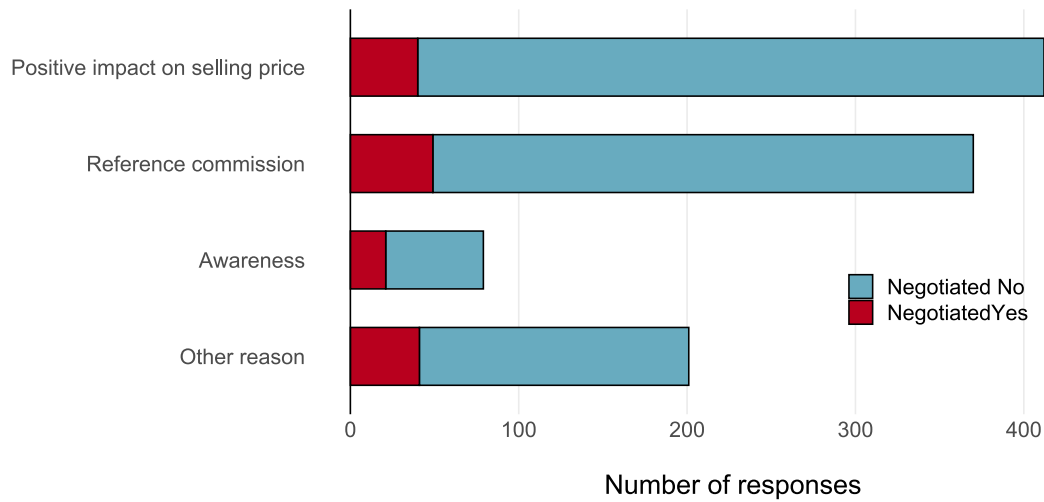
- The seller expects a higher selling price with the agent, which exceeds the commission cost [Incidence]
- The seller is guided by the regionally typical commission rate or similar listings [Reference commission]
- The seller is unaware that the commission rate can be negotiated [Knowledge]
- Other reasons

If sellers did negotiate, an inverse set of options is provided:

- The seller expects a lower selling price with the agent, as the buyer also pays more commission [Incidence]
- The seller observed other listings with a lower commission rate [Reference commission]
- The seller received a recommendation to negotiate the commission rate [Knowledge]
- Other reasons

The results suggest that real estate agents believe that the reference commission and the ability of a real estate agent to fetch a higher (or insufficiently high) price are important explanations for the negotiation behavior of home sellers (Figure B.12). These dimensions are even the most prominent when real estate agents select other reasons. When a seller did not negotiate, approximately 2/3 of real estate agents selecting other reasons answered that customers are willing to pay for a higher commission due to the surplus value added by the service.³ Similarly, when a seller did negotiate, the other most prominent reason cited was that the seller wanted to save money. This justification implies that the seller did not believe that a real estate agent fetches a higher selling price that offsets the commission cost.

³Typical answers included "The seller sees the value of our service" and "We offer a worry-free full-service portfolio which the customer acknowledged."

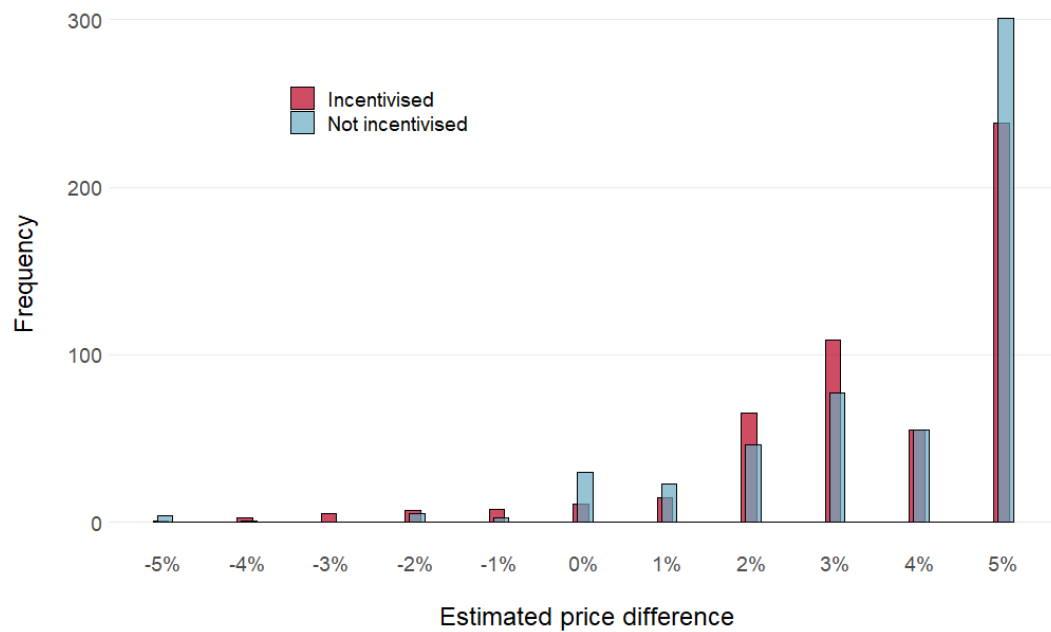
Figure B.12: Estimated Reasons for Negotiation Behavior

Note: This figure shows the proportion of the reasons that real estate agents selected to explain why their most recent seller negotiated or not. Real estate agents selecting "Other reason" for why a home seller decided to negotiate most often provided a variation of "The home seller wanted to save money."

All the inferences drawn from this section are contingent on the subjects' truthful reporting. Some options may be more prone to social desirability concerns than others: stating that a home seller is unaware of the opportunity to negotiate may be less socially desirable than indicating that the offered service quality is so exceptional that a seller would not dare to demand a lower commission rate. Nonetheless, the results support the explanatory role of the variables highlighted in the theoretical considerations. Home sellers' attention to negotiate may be related to the reference level of commission rates and the perceived incidence.

Supplementary Figures

Figure B.13: Estimated Price Difference to Listings for Sale by Owner



Note: The histogram depicts the frequency of estimations given by real estate agents which price difference real estate agents achieve compared to listings for sale by owner.

Appendix C

Appendix for Beer and Crime

Data Preparation

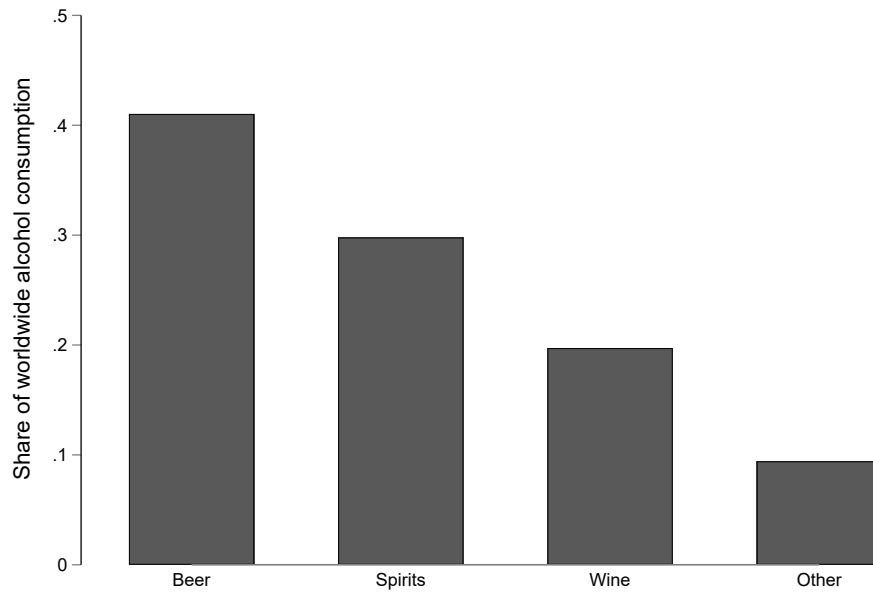
This section provides details of the preparation of the different data used in the analysis.

Beer data. The beer tax records have two significant limitations. First, for the two provinces of Berlin and Brandenburg, only the aggregated volume of annual beer production was published. Our first-stage estimates thus use the same (per capita) values of beer in those states. A sensitivity analysis that pooled all data for Berlin and Brandenburg yielded similar results.

Second, for the province of *Hohenzollern/Sigmaringen*, no beer volumes were recorded for the years 1907 to 1912. To produce a balanced panel, we imputed these missing data points. In step one, we estimated the correlation in the beer production between the province of *Hessen-Nassau* and *Hohenzollern* during the years 1882–1906 (where data for both provinces are available). Based on this estimate and the full *Hessen-Nassau* data, the second step then predicted beer output in *Hohenzollern* for the missing years 1907–1912. The first-stage results hardly change if we omit *Hohenzollern* for the years 1907–1912.

Figures

Figure C.1: Worldwide Sources of Alcohol



Note: This figure depicts the global share of drink types for the consumption of alcohol. Calculated using data by World Health Organization (2019) on absolute per capita consumption of pure alcohol among the population 15+ in 2019.

Figure C.2: Counting Cards Used by Courts

Rückseite der Zählkarte:

8. Strafbare Handlung(en).
 Art der strafbaren Handlung(en), zu bezeichnen nach der Urtheilsformel bezw. dem Strafbefehl, mit Hinweis auf den oder die entsprechenden Paragraphen des Strafgesetzbuchs oder anderer Reichsgesetze (bei gemeinschaftlich begangenen Handlungen auch auf die angewendeten §§ 47, 48, 49 Str.-G.-B.). — Verjahrt ist als solcher zu bezeichnen. Bei Identifizierung (§ 73 Str.-G.-B.) ist die Handlung nur gemäß dem zur Räumung gebrauchten Strafgesetz zu bezeichnen. Sollen mehrere strafbare Handlungen in Frage (§§ 74 ff. a.o.C.), so sind dieselben einzeln unter einander aufzuführen, gleichartige Handlungen jedoch nur unter einem Buchstaben mit Angabe ihrer Gesamtzahl.

Der nebenbezeichneten strafbaren Handlung(en) — zu a), b), c) u. f. w. je bejonders —
 Ort, bezogen in welchem

Ort	Nicht oder entprechenden Verwaltungsbezirk (Regierungs, Kreis-, Hauptmannschaft, District, Kreisamt u. s. w.)	Staat?	Zeit (wenn nicht genau bekannt, ungefähre Zeit)		
			Tag	Monat	Jahr
a)					
b)					
c)					
d)					
e)					

9. Nichtsträfliches Urtheil, bezw. Strafbefehl.

A. Bezüglich jeder der zu Ziffer 8 benannten strafbaren Handlungen durch „Ja“ angegeben ob:

und zwar zu:	verurtheilt?	Verfahren eingestellt?	freigesprochen?
a)			
b)			
c)			
d)			
e)			

B. Erkannte Strafe (Gesamtstrafe. — Bei a), d) und f) bezw. durch „Ja“ zu beantworten.

a) Todesstrafe? _____
 b) Freiheitsstrafe, und zwar:
 Zuchthaus
 Gefängniß
 Festung
 Haft
 c) Ehrverlust
 d) Zulässigkeit der Polizeiaufsicht? _____
 e) Geldstrafe: Wie viel Mark? _____ f) Verweis? _____

Zeitdauer der Strafe
 (Jahre) | Monate | Wochen | Tage

10. Für Verurtheilte: Währungsge*) Verurtheilungen wegen Verbrechen und Vergehen (nicht Nebenstrafen) gegen Reichsgesetze (nicht Landesgesetze) durch wie viele Entscheidungen? _____ In welches Jahr fällt die letzte derselben? 18__ und zwar wegen welcher Straftat (von gleichzeitig abgeurtheilten nur die schwerste)? _____ Falls auf Grund einer der Entscheidungen Freiheitsstrafe verhängt wurde*, Dauer der letzten Strafverbüßung: _____; Verbüßung derselben am _____ 18__

*) bei Verhängung der Straftat zu Ziffer 8; bei mehreren Straftaten, bei Verhängung der Zeit nach letztem Vertheilten.
 Außerdem sind bei Rückfällen nach §§ 244, 250*, 261, 264 des Strafgesetzbuchs die Nummern der betreffenden Paragraphen hier so viel mal zu unterzeichnen, als den Rückfall bedingende Strafungen (auch Reichsgesetzen) stattgefunden haben.

Datum: _____ Name des ausfüllenden Beamten: _____

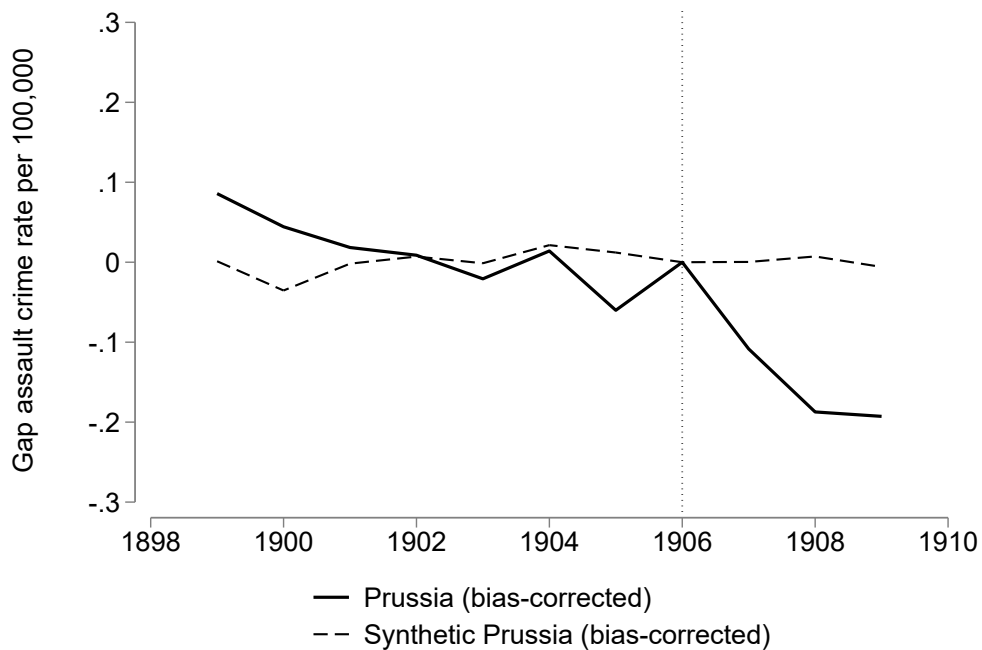
Note: This figure displays an original counting card used by courts across the German Empire.

Figure C.3: Advertisement from Brewery Germania in Berlin Around 1900, Resurfaced 2023



Note: This picture shows an advertisement sign which was uncovered in 2023 after demolition works in the (then) Usedomstrasse 8. The advert reads “Green beer daily fresh for 10 cents (*Pfennige*) per litre. Bavarian [style] beer for 10 cents (*Pfennige*) per 1/2 litre.”

Figure C.4: Prussia vs Synthetic Prussia: Assault Crime Rates



Note: This figure displays the gap of (logged) assault rates between Prussia (solid line) and Synthetic Prussia (dashed line). The trends are almost identical to the trends observed for violent crime rates displayed in Figure 3.6, exhibiting a notable decrease following the tax reform in 1906.

Tables

Table C.1: IV Estimates for Rate of Criminals (Prussia State Panel)

Dep. variable (log):	All crimes	Violent crime	Assaults	Property crime
	(1)	(2)	(3)	(4)
log(Beer)	0.486 (0.422) [1.153]	1.533* (0.716) [2.141]	1.672* (0.796) [2.100]	-0.544 (0.457) [-1.190]
Observations	434	434	434	434
F statistic	28.96	28.96	28.96	28.96

Note: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the state level in parentheses and wild bootstrap t statistics are provided in brackets. All specifications control for state-level FEs, general covariates, crop yields, and time trends.

Table C.2: Reduced-Form Estimates for Other Crimes (District Panel)

Dep. variable (log):	All crimes	Property crimes	Vandalism
	(1)	(2)	(3)
Barley yield	-0.017 (0.027)	-0.036 (0.033)	0.011 (0.030)
Observations	1,152	1,152	1,152

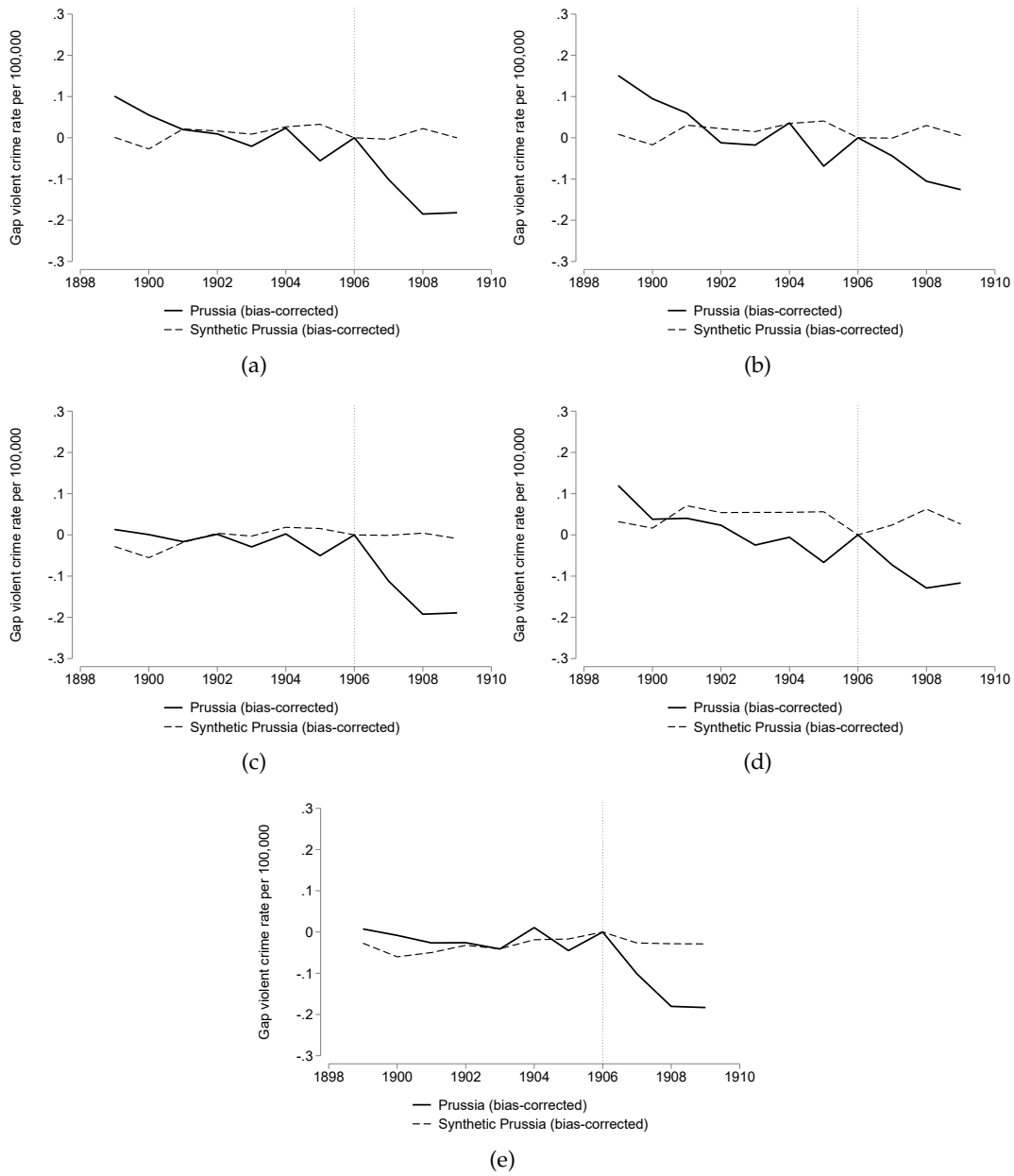
Note: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the state level in parentheses. All specifications control for state-level FEs, general covariates, crop yields, and time trends.

Table C.3: Reduced-Form Estimates for Rate of Criminals (Prussia District Panel)

Dep. variable (log):	All crimes	Violent crime	Assaults	Property crime
	(1)	(2)	(3)	(4)
Barley yield	0.019 (0.018)	0.074** (0.027)	0.077** (0.028)	-0.021 (0.024)
Observations	1,152	1,152	1,152	1,152

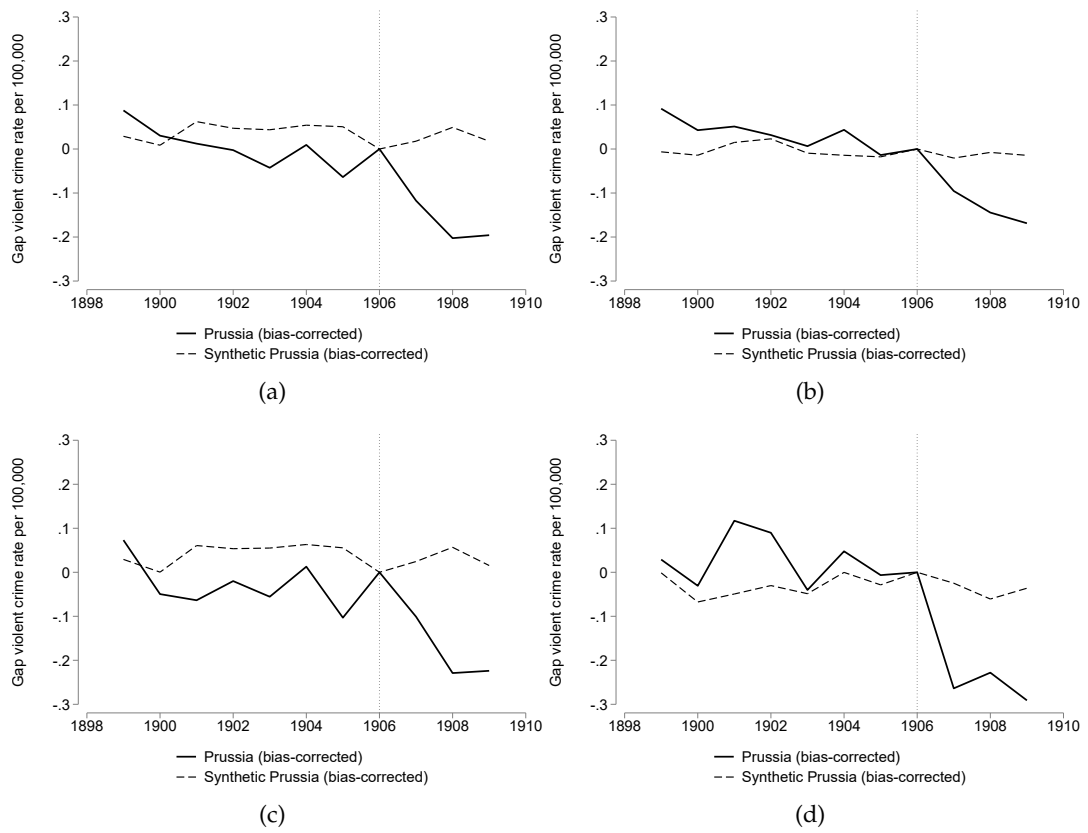
Note: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the district level in parentheses. All specifications control for state-level FEs, general covariates, crop yields, and time trends.

Figure C.5: Prussia vs Synthetic Prussia: Violent Crimes Excluding Individual Predictors



Note: These figures display the synthetic control results for violent crime rates shown in Figure 3.6 when estimated without the predictor variables (a) Population density; (b) Industry; (c) Share German mother tongue; (d) Share married men; (e) Share aged 20-49.

Figure C.6: Prussia vs Synthetic Prussia: Violent Crimes Excluding Individual Donor Units



Note: These figures display the synthetic control results for violent crime rates shown in Figure 3.6 when the synthetic control is constructed without (a) Niederbayern; (b) Jagstkreis; (c) Oberelsass; (d) Lothringen.

Table C.4: Covariate Balance

Variable	Violent crime model		Property crime model	
	Prussia	Synth. Prussia	Prussia	Synth. Prussia
Share age 20–49	0.40	0.40	0.40	0.40
Pop. density (per sqkm)	98.87	101.76	98.87	89.9
Share married males	0.35	0.36	0.35	0.36
Share empl. in industry	0.44	0.39	0.44	0.34
Share native speakers	0.88	0.90	0.88	0.93
log(Violent crime rate 1906)	5.27	5.27		
log(Property crime rate 1906)			5.28	5.28

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