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Finally a Smoking Gun?

Compensating Wage Differentials and the Introduction of Smoking Bans in Germany

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Abstract

Using the staggered introduction of smoking bans in the German hospitality industry over 2007/08, I find a robust 2.4% decline in the daily earnings of workers in bars and restaurants associated with the most comprehensive smoking ban. This effect is unlikely to be driven by a decline in hospitality revenues or hours worked, but is consistent with a simple model of compensating differentials.

Keywords: Occupational Wage Differential, Working Conditions, Smoking, Regulated Industries

JEL Codes: I18, I12, J28, J31, K230

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1. Introduction

> *"The whole of the advantages and disadvantages of the different employments of labour and stock must, in the same neighborhood, be either perfectly equal or continually tending to equality. If in the same neighborhood, there was any employment evidently either more or less advantageous than the rest, so many people would desert it in the other, that its advantages would soon return to the level of other employments."*
>
> Adam A. Smith (1776, p. 111)

How do workers' wages react to a sudden improvement in working conditions? The theory of compensating differentials (A. Smith 1776; Rosen 1986) suggests that in a competitive labor market workers must be offered a wage premium to offset any disutility associated with unpleasant attributes of a given job relative to another otherwise comparable one. These negative attributes may comprise non-standard working hours, unpleasant tasks, or health related hazards such as being exposed to secondhand smoke. This idea also becomes important when understanding inequality: Earnings inequality might significantly overstate utility inequality precisely because some jobs need to pay compensating differentials to compensate for certain disamenities. In line with this reasoning, Sorkin (2018) estimates that about 15% of the variation in US earnings can be explained by compensating differentials.

Compensating differentials in Sorkin (2018) are essentially a residual. However, establishing *direct* empirical evidence for compensating differentials and labeling what they represent has proven to be challenging. Existing studies are mostly plagued by confounding selection effects that cannot be separated from the effect of interest and the lack of appropriate identifying variation. Thus, it seems as true today as some thirty years ago when Duncan and Holmlund (1983, p. 367) noted that "[Adam Smith's] intuitive statement has [...] shown surprising resistance to empirical confirmation".

In this paper, I exploit a natural experiment that can help to overcome some of the previous challenges in estimating compensating differentials. I use the introduction of smoking bans in restaurants, bars and clubs in the German federal states in 2007/08 to study their effect on the daily earnings of workers in these businesses. I argue that this setting has several appealing features that facilitate identification. First, smoking bans were highly effective at reducing the amount of harmful airborne particles, a claim backed by representative indoor air quality measurements taken before and after the implementation of smoking bans by the German Cancer Research Center. Second, smoking bans were rolled out over a thirteen-month period across all German states with the introduction dates of individual states being uncorrelated to a host of potential predictors and thus creating arguably exogenous variation in treatment status across time and space. Third, smoking bans also varied in their intensities thereby creating additional variation that

I exploit by constructing an index capturing the strictness of different smoking bans. What is more, the intensities of smoking bans in some states were altered from one day to another due to a rather unexpected ruling by the Constitutional Court, thus creating additional, arguably exogenous variation along the intensity dimension. Fourth, the smoking bans of 2007/08 were targeted at restaurants, bars, and dancing clubs while leaving smoking regulations for other occupation groups unaffected. This adds yet another layer of variation which enables me to not only rely on variation across time, space and intensity but also across occupations strengthening the credibility of my findings.

To estimate the effect of smoking bans on the daily earnings of hospitality workers, I use high-quality, large sample administrative labor market data. In most of my analysis, I will focus on workers in so-called *mini jobs* – a flexible part-time contract ubiquitous in the German low wage sector in which workers are exempted from most social security and tax payments – as I argue that in this less rigid segment of the labor market a new equilibrium can emerge more quickly. Employing either a conventional difference-in-differences (DD) strategy across states and time or a triple difference-in-differences (DDD) approach using unaffected occupations as an additional control group, I find that the most comprehensive smoking ban in the sample led to a 2.4% decline in daily earnings of these workers.

I address several concerns that could potentially cast doubt on a causal interpretation of my findings. Performing a battery of robustness checks including several placebo and permutation exercises, I find no violation of the parallel trends assumption or any evidence that the effect would be confounded by seasonal effects, a specific choice of the time period, outliers, foreign or domestic tourist demand, election cycles, or coincidental variation in weather variables. These results suggest that the effect can indeed be interpreted as causal.

I then set out to study alternative explanations for the decline in the daily earnings of waiters. One commonly proposed channel is that smoking bans resulted in lower revenues of bars and restaurants which consequently led to lower daily earnings. Using official revenue data from the German Statistical Office, I find no evidence for that claim once properly accounting for seasonal variation in the data. The effect also remains virtually unchanged when I control for revenues directly in my earnings regressions. Nevertheless, the revenue data might be too noisy or – since only available at the state level – too coarse to accurately reflect changes in demand. Therefore, I test the revenue channel from two different angles. First, a decline in revenues would plausibly also affect the daily earnings of other workers in the hospitality industry. The robustness of the smoking ban coefficient on waiters' daily earnings in my DDD strategy thus provides further evidence against a revenue decline acting as the main channel. Second, if demand after the implementation of smoking bans indeed went down because patronage by smokers went down, we should see a larger decline in states with an initially higher share of smokers. However, including the initial share of smokers in a state interacted with time effects leaves the effect of interest quantitatively unchanged. Further evidence also suggests that there is no increased closure or start-up activity of hospitality establishments associated with the introduction of smoking bans. Taken together, it thus seems unlikely that the effect is driven by a decline in revenues or a change in the business landscape.

Another potential explanation for the decline in daily earnings is a decrease in the hours worked. Since earnings in the administrative labor market data are only reported as *daily* earnings and the hours worked are not observable, I draw on a compulsory labor market survey – the German Microcensus – in which the hours worked are reported.

Using either a triple difference-in-differences approach exploiting variation between states and occupation groups or a synthetic control group approach with other occupation defining the donor pool of potential control units, I find no support for a decline in the hours worked of workers in bars and restaurants. If anything, the evidence points to an increase in hours worked by waiters.

I argue that my findings are consistent with a simple compensating differentials model. If the marginal worker – all other amenities remaining equal – positively values a smoke-free environment, economic theory suggest that she should be willing to give up part of her earnings in exchange. Additionally, individuals who previously were not willing to work in smoke-allowed restaurants or bars might now be induced to look for a job in the hospitality sector. Both effects will unambiguously result in lower earnings and – depending on the elasticity of labor demand – in an increase in the hours worked.

My setting provides an ideal testing ground for these predictions. Due to many non-unionized workers and low qualification requirements on the supply side and many small firms and low entry barriers on the demand side coupled with the absence of a minimum wage, relatively high turnover rates and rather flexible employment regulations, the labor market for most waiters in Germany comes close to the textbook case of perfect competition in which the new equilibrium is expected to emerge quickly. In line with the prediction of such a simple compensating differentials model, I find a significant decline in daily earnings that is highest for states that introduced the strictest smoking bans. A back-of-the-envelope calculation suggests that the WTP for working in a smoke-free establishment implied by my estimates is consistent with the valuation of the increase in health related quality of life associated with an elevated secondhand smoke exposure.

This paper contributes to at least two strands of the literature. First and most importantly, I contribute to the literature related to the empirical measurement of compensating differentials. Evidence for compensating differentials have been found in some (specific) cases including shift-work (Kostiuk 1990; Lanfranchi et al. 2002), employer-sponsored health insurance (Kolstad and Kowalski 2016), and fatal and non fatal injury risks (Leeth and Ruser 2003; Galick 2014). However, many studies find insignificant or "wrong-signed" estimates such that Sorkin (2018, p. 1) concludes that the "conventional view is that... it is hard to find robust evidence that non-pay characteristics are priced in the labor market". In a similar vein, Lavetti (2018) describes the estimation of compensating differentials "a classic topic in labor economics that has long been considered notoriously difficult to solve".[1]

The most prominent issue complicating the empirical estimation of compensating differentials is self-selection of workers into different jobs as noted by Duncan and Holmlund (1983) and Galick (2014).[2] In this paper, I exploit a panel of workers which enables me to control for unobserved fixed worker characteristics, a feature I share with a few other papers (e.g. Duncan and Holmlund 1983; Galick 2014). Identification in these panel studies relies on within-worker job changes. However, as Lavetti (2018) shows, job changes themselves and amenities offered by firms are endogenous which can exacerbate bias in panel studies.[3] In contrast to these papers, I can rely on an arguably exogenous variation in amenities *within* jobs (smoking bans "shocked" existing firm-worker pairs) while still controlling for worker fixed effects. To the best of my knowledge, Lavetti (2018) is the only other paper to apply a similar research design. Relative to Lavetti (2018) who studies compensating differentials related to fatal risks of commercial fishing deckhands in the Alaskan Bering Sea based on survey data, I can rely on a large administrative data set to exploit a relatively broad, economy wide natural experiment studying the compensating differential of a non-fatal health amenity.

A further issue that complicates the empirical establishment of compensating differentials noted by Bonhomme and Jolivet (2009) is the existence of labor market frictions. In particular if job search is costly and plagued by incomplete information related to job-specific (dis-)amenities, compensating differentials might be small or non-existent even when workers exhibit a non-zero marginal willingness to pay for these amenities. In my setting, these concerns are likely to be of less importance. First, as I focus on workers in mini jobs, regulatory frictions and wage rigidities should be less prevalent than in the case of full-time jobs which most previous studies are based on. Second, the existence of smoking bans in bars and restaurants are a very salient and commonly

[1] For similar assessments see R. S. Smith (1979), Brown (1980), Bonhomme and Jolivet (2009), and Hornstein et al. (2011).

[2] Another important issue are (non-classical) measurement errors of disamenities, e.g. resulting from survey data or low probability events (Black and Kniesner 2003). This issue does not arise in my setting, however, since treatment status is perfectly observable and applies to a large group of workers.

[3] Workers who change jobs in (frictional) labor markets tend to move to jobs that both pay more and offer better non-wage amenities (Lavetti 2018). In this context, one might wonder whether the decision to stay at a certain job is also endogenous. However, as Lavetti (2018, 12f) argues, using within-job variation holds latent fixed firm wage effects (the potentially omitted variable) constant and thus – mechanically – fixed firm wage effects cannot be correlated with within-job variation in amenities. The decision to leave a job thus affects the representativeness of the sample but not identification.

known job feature for any existing or potential worker and thus incomplete information does not constitute a major impediment in the estimation.

Second, my study also relates to a series of papers that evaluate the impacts of smoking bans on various health and economic outcomes. Most epidemiological studies largely agree on the positive impact of smoking bans on air quality and health outcomes of hospitality workers. For instance, Repace et al. (2006) find that Boston's 2003 smoke-free workplace law (granting no exceptions) led to a 95% reduction in respirable particle pollution in bars and pubs while in case of Germany's smoking bans implemented over 2007/08 (granting some exceptions), the German Cancer Research Center (DKFZ 2010) finds a reduction by up to 82% (I will discuss these results in more detail in section 2). Goodman et al. (2007), studying Ireland's 2004 complete smoking ban, find that the air quality improvements associated with smoking bans also translate into large and sustainable health improvements of nonsmoking bar staff in terms of the pulmonary function and respiratory and irritant symptoms in the short and longer run. Carton et al. (2016) and Anger et al. (2011) find that smoking bans in the US and Germany, respectively, significantly reduced smoking prevalence among specific subgroups of the population such as young or low-income individuals. Kuehnle and Wunder (2013) also find significant health externalities for young non-smokers while Adda and Cornaglia (2010) highlight that public smoking bans may *increase* children's and other non-smokers' exposure to tobacco smoke as smokers shift cigarette consumption to their private homes. Finally, Adams and Cotti (2008) exploit geographic variation in local and state smoke-free bar laws in the US finding that alcohol-related fatal accidents increased as smokers drive longer distances to bars that still allow smoking.

The evidence related to the effect of smoking bans on revenues of bars and restaurants is mixed (Adda et al. 2007; Pakko 2008; Adda et al. 2009; Ahlfeldt and Maennig 2010; Kvasnicka and Tauchmann 2012), with some studies finding negative and some finding insignificant or positive impacts on revenues.[4] More often than not, however, these studies are based on research designs that render a causal interpretation of the findings difficult, for instance by relying on self-reported data by business owners, pure time-series before-after comparisons, or inadequate accounting for seasonal variation in sales data. Finally, Adams and Cotti (2007) find a significant decrease in employment related to the introduction of smoking bans in the US while Thompson et al. (2008) find a significant short-term decrease in employee turnover. To the best of my knowledge, my paper is the first to look at the effect of smoking bans on earnings of hospitality workers.

The rest of the paper is organized as follows. Section 2 provides background information regarding the introduction of smoking bans in the German hospitality industry. Section 3 describes the data, explains the identification strategies, and presents estimation results as well as robustness checks. Section 4 discusses alternative explanations before section 6 concludes.

[4] See Scollo and Lal (2008) for a survey. I will review the related evidence for Germany in more detail in section 4.1.

2. Background

According to the American Cancer Society, secondhand smoke contains at least seventy substances that can cause cancer and that carry the risk of heart attacks, strokes, and chronic lung diseases. Harmful particles from tobacco smoke stay in rooms and remain a hazard even without anyone present and smoking. Employees working in hospitality establishments not covered by smoking bans are among the most exposed occupation groups and are estimated to have a 50% higher risk of dying from lung cancer even if they are non-smokers themselves (Siegel 1993).[5] According to Jamrozik (2005), 1.4% of all British non-smoking hospitality workers are estimated to die in the long run due to their exposure to secondhand smoke.

In 2007, the European Cancer League published a report reviewing tobacco control activities in Europe taking into account the price of tobacco products, the protection from secondhand smoke via smoking bans, the regulation of advertising, and other indicators. In this report, Germany ranked 27 out of 30 countries and was described as "the biggest problem for tobacco control in Europe [due to its] well established connections with the tobacco industry" (Joossens and Raw 2007, p. 12). In the wake of such reports and a growing number of Western countries implementing smoking bans, anti-smoking sentiment in the general population was growing in Germany. According to a survey conducted by the German Cancer Research Center, in 2006 a majority of 59% was in favor of smoking bans in bars and restaurants (DKFZ 2006). Against this backdrop, in early 2007 the federal states decided to implement smoking bans in public places including bars and restaurants "within the next months" (Bundesrat 2007, p. 4).[6] In doing so, the states had some leeway in deciding *when* and *how strict* a ban they would implement. Subsequently, between August 2007 and August 2008, 16 different smoking bans became effective.[7]

The smoking bans differed along four components: whether or not (i) restaurants and bars could install a separate smoking room, (ii) dancings clubs could install a separate smoking room, (iii) small pubs could choose to be smoke free or not, and (iv) smoking was allowed in party tents. All states but Bavaria granted larger bars and restaurants the possibility to install a separate smoking room. 10 out of 16 states allowed dancing clubs to install a separate smoking room. Only Rhineland-Palatinate gave small single-room pubs the opportunity to opt out of implementing a smoking ban. A complete overview of the initial regulations in the different states along with the introduction dates is given in Table A2 in the Online Appendix.

[5] According to data from the Microcensus, in 2005 about 49% of waiters aged 17 to 62 years were regular or occasional smokers while the same share was about 35% among the general population. See Table A1 in the Online Appendix for more details and Figure A1 for a map of the share of smokers in 2005 in the German states.

[6] Anti-smoking regulation in public places is a matter of the states except for regulations concerning public transportation, the workplace and federal buildings which are at the discretion of the federal government.

[7] Throughout, I rely on the *effective* introduction of smoking bans, i.e. the date when a ban was officially enforced by sanctions. In robustness check 1 of Table 4, I show that my results remain robust when using the *legal* start of a ban instead.

Owners of small bars and dancing clubs challenged some of these regulations claiming they were treated unequally compared to owners of larger bars and restaurants who had the possibility to install separate smoking rooms. In line with their argumentation, on July 30, 2008 Germany's Federal Constitutional Court revoked the respective parts of the smoking ban laws. Minutes after the ruling was made public, restaurants and bars in ten states[8] could return to be smoke-allowed and many did so, creating an arguably exogenous variation in the intensity of smoking bans.[9]

To exploit the bans' variation in both time and intensity, I construct an *intensity*-index that aggregates the strictness of the different regulations at different points in time. Specifically, the index is constructed as follows:

$$\text{intensity}_{st} = \mathbf{1}_{st}^{ban}\left[\frac{2 - \omega_{LR}LR_{st} - \omega_{DC}DC_{st} - \omega_{SB}SB_{st} - \omega_{PT}PT_{st}}{2}\right] \in [0,1] \tag{1}$$

where s refers to state, t to time, and $\mathbf{1}_{st}^{ban}$ is an indicator that is one if a smoking ban is in operation and zero otherwise. $LR_{st}, DC_{st}, SB_{st}, PT_{st}$ are dummies indicating whether or not state s at time t allowed for a separate smoking room in large bars and restaurants (LR_{st}), in dancing clubs (DC_{st}), an opt-out possibility for small pubs (SB_{st}), and party tents (PT_{st}). The ω's denote the corresponding index weights which are derived from the employee shares in the respective establishments in the base year 2007.[10] These index weights are listed in Table A3 in the Online Appendix. The bulk of workers are employed in large (66% in 2007) and small (30%) bars and restaurants while the share of employees in dancing clubs (3%) and party tents (1%, estimated) is small. Thus, the index will put most weight on the indicators referring to separate smoking rooms and exemptions for small bars. This weighting is in line with a series of robustness checks in which I regress daily earnings on the single, mutually exclusive components of the smoking ban intensity index or jointly on all of its components. It turns out that a ban in separate side rooms in restaurants and larger pubs has the largest and most significant impact on daily earnings, precisely in line with the reasoning and motivation for the index construction which gives most weight (2/3) to this indicator. Also in line, smoking bans in party tents and separate smoking rooms in dancing clubs have a far less important impact which is reflected in their small weights (1 and 3%, respectively, see Online Appendix Table A4 for the corresponding DD and Tables A5 and A6

[8] The remaining were either not affected (Bavaria and Rhineland-Palatinate) or waited for pending rulings of their respective state courts.

[9] See, for instance, a newspaper article by Der Spiegel (2016) which describes how the owner of a pub in Berlin called his employee shortly after the Constitutional Court's decision and told her to put out a "smoking allowed" sign at the pub's door.

[10] For instance, ω_{LR} corresponds to the share of employees in food and beverage serving establishments with six or more employees in all employees in food and beverage serving establishments and dancing clubs, see Table A3 in the Online Appendix.

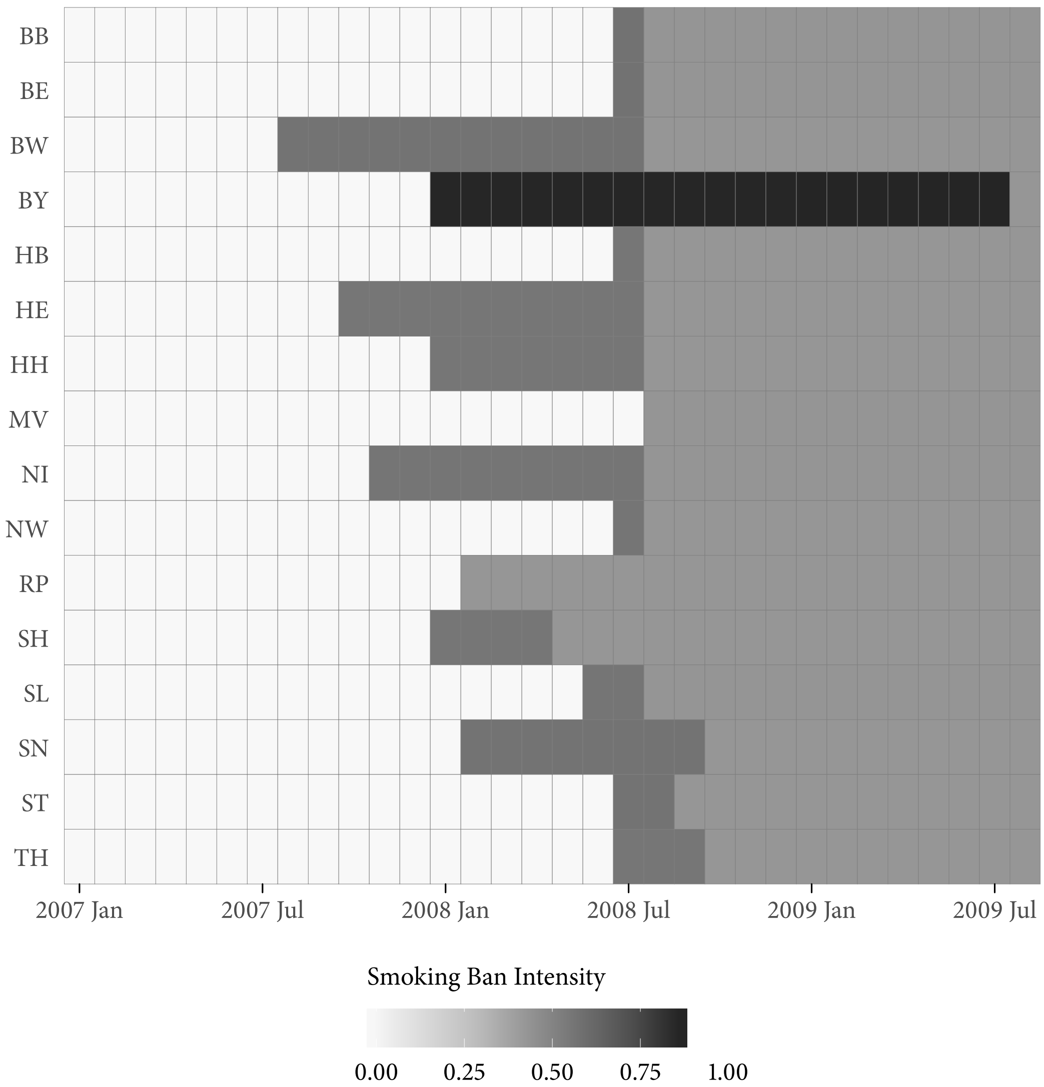

Figure 1: Illustration of Variation in Smoking Ban Policies Across States

Notes: This figure illustrates the two dimensions of identifying variation (time and intensity) across states between January 2007 - August 2009. Each tile corresponds to a state-month combination and the coloring of a tile illustrates the intensity of the respective smoking ban regulation in that state and month. For details on the construction of the intensity index see Section 2. BB = Brandenburg, BE = Berlin, BW = Baden-Wuerttemberg, BY = Bavaria, HB = Bremen, HE = Hesse, HH = Hamburg, MV = Mecklenburg Western Pomerania, NI = Lower Saxony, NW = North Rhine-Westphalia, RP = Rhineland-Palatinate, SH = Schleswig-Holstein, SL = Saarland, SN = Saxony, ST = Saxony-Anhalt, TH = Thuringia. *Source:* Own illustration based on respective state laws.

for the DDD results).[11] By construction, this index is zero if no smoking ban is in operation, 0.5

[11]These regression results should be taken with a grain of salt since the indicators are highly collinear and in some cases reflect only a narrow state-time combination of observations. For instance, only Bavaria initially did not allow

if a state grants exception in all four categories (Rhineland-Palatinate, weakest ban), and 1 if no exceptions are granted (Bavaria, strictest ban). The intensities of each state's initial smoking ban are tabulated in the last column of Table A2 and are visualized in a map in Figure A2 in the Online Appendix. Note that the intensity index is time-varying, i.e. it reflects any changes that occur due to court rulings or the amendments of laws.[12]

Figure 1 illustrates the two dimensions of identifying variation (time and intensity) across states. For instance, Baden-Wuerttemberg (BW) introduced a moderate smoking ban already in August 2007 and due to the Constitutional Court's ruling on July 30, 2008 had to weaken it in August 2008. Bavaria (BY) implemented its strict smoking ban in January 2008, five months after Baden-Wuerttemberg, and its ban remained unaffected by the Court's ruling. Finally, North Rhine-Westphalia's (NW) ban had to be weakened just a month after its implementation in July 2008.

As Adams and Cotti (2007) point out, the introduction of smoking bans might be endogenous in the sense that states with a stronger anti-smoking sentiment and a lower prevalence of smokers pass smoking bans earlier in time and choose bans that are stricter. I argue, however, that in my setting these arguments are less of a concern. First, in terms of timing all states agreed to pass a ban within the next months and actually did so over the course of twelve months between August 2007 and August 2008. In fact, as Table A7 in the Online Appendix shows, the introduction date is not systematically related to the ban's intensity, the initial share of smokers, the trend in hospitality revenues or hospitality earnings, or other potential determinants. Second, as Table A8 in the Online Appendix suggests, the intensity of a state's smoking ban, too, does not seem to be systematically correlated with the same set of potential determinants, i.e. in particular states with a higher share of smokers in 2005 did not implement stricter bans.[13] Thus, it seems that the timing as well as the strictness of a smoking ban was rather determined by idiosyncratic factors such as the patterns of parliamentary sessions, administrative concerns, or personal preferences of state legislators.

Were smoking bans in Germany effective? That is, did they indeed improve air quality in bars, restaurants, and dancing clubs? The German Cancer Research Center measured air quality in a representative sample of hospitality establishments in Germany before and after smoking bans

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for separate smoking rooms in restaurants and lager pubs and only Rhineland-Palatinate allowed for smaller pubs to opt out of imposing a smoking ban, while after the Federal Courts ruling all states allowed for a separate smoking room except for Bavaria.

[12] For instance, Bavaria introduced a strict smoking ban in January 2008 and was not affected by the Federal Constitutional Court's ruling in July 2008. However, on August 1, 2009 the Bavarian government weakened parts of the initial ban allowing for separate smoking rooms in restaurants and exempting small pubs altogether as was the case in most other states at that time. This, however, triggered a referendum in favor of an even stricter ban than the initial one which was approved by a 60:40 majority and came into effect August 1, 2010.

[13] Even if they were, the inclusion of state fixed effects in the subsequent regressions would absorb any of such time-invariant state-specific variables.

were implemented. The measurements were carried out using an inconspicuous small aerosol monitor during times when clients would typically visit the respective type of establishment. A total of 98 (2005) and 81 (2009) establishments surveyed in 10 cities in 9 states. As Figure A3 in the Online Appendix shows, the amount of harmful particles was reduced dramatically by some 70-80% after the introduction of smoking bans. As smoking was still allowed in (parts of) some establishments, there is still a positive amount of harmful particles left on average. Figure A4 in the Online Appendix compares the particle concentration in selected establishments that implemented comprehensive smoking bans granting no exceptions (like Bavaria). In these cases, the amount of harmful particles was virtually completely eliminated. Summarizing, smoking bans in Germany in fact led to a very substantial improvement in the indoor air quality of hospitality establishments and thus effectively reduced the exposure of hospitality workers to harmful particles from secondhand tobacco smoke.

3. The Effect of Smoking Bans on Waiters' Earnings

3.1. Theoretical Motivation

A simple compensating differentials model assuming a competitive labor market and a positive valuation of working in a smoke-free environment by the the marginal worker predicts a negative impact of smoking bans on the wages of waiters in bars and restaurants. Given that labor demand is not completely inelastic, one would also expect an increase in the hours worked. The assumption of a competitive labor market for waiters seems warranted in Germany as (i) competition between hospitality businesses is fierce (as indicated by a very low Herfindahl index, see Statistisches Bundesamt 2001), (ii) qualification requirements for hospitality workers are generally low and firms can draw on a large pool of suitable workers such as students or second-income earners, and (iii) most of these jobs are contracted as flexible mini jobs as discussed above. Furthermore, during the period studied, there was no minimum wage in Germany and unionization rates among hospitality workers were low.[14] Furthermore, individual productivities of workers must be held constant. In the empirical estimations, I will therefore include worker fixed effects to hold all time invariant characteristics constant, including in particular unobserved traits such as motivation, friendliness, or sales talent that are likely to influence individual wages (apart from tips) and hours worked. Including worker fixed effects thus helps to control for unobserved selection of more able or productive individuals into waiter jobs as a consequence of smoking bans.[15]

[14] According to Vogt (2007), the unionization rate in 2007 in the entire hospitality sector was below 10% which includes full-time employees in hotels and catering firms. Focusing on the employees working as waiters, the percentage is likely to be (much) lower, as most of the employees are mini job workers working in small firms (Frese 2015).

[15] It could also be the case that the *same* individual becomes more productive after the introduction of smoking bans. Although I have no way to control for such a time-varying unobserved effect, this effect should lead to *higher* earnings and would thus work against me by biasing my estimates upwards.

3.2. Data

My prime data source to study the effect of smoking bans on the earnings of waiters is the *Sample of Integrated Labour Market Biographies* provided by the Institute for Employment Research (IAB). The IAB earnings sample is a 2% random sample of the official records of all employees subject to social security and provides data on daily earnings and employment status (full-time, part-time, mini job, unemployed, in vocational training) as well as a number of individual characteristics such as age, gender, education, German nationality, region, occupation, and industry. My baseline samples comprise between 155,000 to 350,000 worker-month spells of about 15,000 to 31,000 different employees aged 17 to 62 years in East and West Germany between August 2006 and February 2009. Section B1 in the Online Appendix provides more details on the construction of the sample and its variables.

When it comes to studying the wages or earnings, respectively, of hospitality workers it is important to understand the role of tipping. Unlike in the US where the wage of a typical waiter almost exclusively depends on tips, tips are less important in Germany. Customers would typically round up their bill resulting in tips of about 5 to 10%. Whether waiters can keep these tips or share (parts of) them with their colleagues (e.g. the cooks) varies from establishment to establishment. A common rule seems to be that waiters keep 75% of their tips and share the rest with their non-tipped colleagues. No precise estimate of the share of tips relative to the baseline wage exists, but some 20 to 30% seems reasonable.[16] In any case, tips are not subject to taxation or social security contributions and thus are thus not recorded in the IAB earnings data.[17]

Ideally, I would only select workers who work inside establishments in which smoking is or was formerly allowed (typically these are bars, restaurants, and dancing clubs). However, the data resolution is not fine enough for such an exercise. Therefore, I identify as "waiters" those workers who are employed in the hospitality industry *and* work in "guest attending" occupations. However, this subsample contains some workers such as guest attending workers in youth hostels, ice cream parlors, open-air beer gardens, caterers, or canteens who were likely not exposed to secondhand smoke even before the introduction of smoking bans. Similarly, the data does not allow to separately identify hotel and restaurant owners and managers, receptionists, or staff in charge of housekeeping or back office related tasks who are all part of the guest attending occupation group but are unlikely to be affected by the introduction of smoking bans.[18] This will likely result in the attenuation of the estimated treatment effects and thus my coefficients

[16] Here, I mainly rely on personal conversations with waiters and restaurant managers, and on information found on the web such as `https://gehaltsreporter.de/gehaelter-von-a-bis-z/hotellerie-gastronomie/Kellner.html` (in German).

[17] Compare Art. 3 Nr. 51 *Gesetz zur Steuerfreistellung von Arbeitnehmertrinkgeldern* as of 08/08/2002.

[18] For instance, the German occupation "Hotelfachmann/ -frau" contained in occupation group 115 (waiters) is related to a wide range of guest attending tasks in hotels including book keeping and other presumably non-secondhand-smoke-exposed tasks.

are to be understood as lower bounds. However, many of the untreated workers such as hotel managers or back office staff are more likely to work full-time or regular part-time (as opposed to mini jobs) and thus excluding these workers should mitigate the problem of including many untreated workers. Based on official employment statistics (Bundesagentur für Arbeit 2015) which break down employment at finer occupation levels, in 2013 (earliest year available with this finer resolution) about 84% of mini job workers in guest attending occupations worked in occupations likely affected by the introduction of smoking bans. Considering that some 5 to 10% of these workers might have been employed in "untreated" establishments such as ice cream parlors or outdoor cafes, it seems reasonable to assume that some 75 to 80% of waiters in mini jobs in the hospitality industry were actually affected by the introduction of smoking bans.

Consistent with this line of reasoning , I find much larger and more significant effects when focusing on mini job employees only, the typical contract for waiters in Germany (as Table 1 suggests, about 60% of all months worked by workers in guest attending occupations in the hospitality sector are employed in mini jobs.) Employees in mini jobs are exempted from regular social security contributions and income taxation while employers only pay lump sum tax and social security contributions of about 25%. During the time of my analysis, workers in mini jobs were allowed to earn up to 400 euros per month (summed up over all mini jobs of a wokers) on a regular basis.[19] Firing of mini job waiters is relatively easy for at least two reasons. First, notice periods are usually two (within the first six months) to four weeks (for employment relationships between six months and two years). In my baseline daily earnings data set, the median mini job waiter has a tenure of 14 months compared to 18 and 26 months in case of regular part-time and full time waiters, respectively. 31% (66%) of all mini job waiters have a tenure of less than 6 (24) months, while this is the case for only 25% (58%) and 20% (49%) of regular part-time and full time waiters, respectively. This points to shorter contract durations of mini job workers compared to their regularly employed colleagues.[20] Second, given that more than 70% of all companies in the hospitality sector have less than 10 employees (DEHOGA 2017), the stricter dismissal rules according to Employment Protection Act do not apply even for longer-term employment relationships and small employers can lay off workers without the duty to give reasons within the narrow scope of the Employment Protection Act. All this and the fact that there was no minimum wage in place at the time of the analysis suggests that there is ample flexibility to adjust wages in

[19] Some exceptions from this rule are possible, e.g. for employees in short-term contracts or when smoothing seasonal peaks obeying an annual earnings cap. Only about 3% of mini job employees in my baseline sample earn more than 400 euros per month. All my results are robust to excluding these observations. During the time of analysis, no changes in the income threshold occurred (from April 2003 to January 2013 the threshold remained at 400 euros. On July 1, 2006 the employer contribution to the health and pension insurance was increased to 13% and 15%, respectively. I choose to start my baseline analysis in August 2006 which is also 12 months before the start of the first smoking ban.

[20] Note that the reported tenure variable cumulates the durations that a worker has worked for a specific company irrespective of interruptions in between. Contract durations are not observable in the IAB data.

Table 1: Summary Statistics of Individual Earnings Data

	(1) Full-Time Jobs		(2) Regular Part-Time Jobs		(3) Mini Jobs	
Real Monthly Earnings (in 2010 euros)	1594.0	[848.3]	675.6	[470.7]	238.2	[162.9]
Real Earnings Change 2005-07 (in %)[a]	-0.029	–	-0.080	–	-0.043	–
Real Earnings Change 2007-09 (in %)[a]	-0.016	–	0.049	–	-0.037	–
Nominal Earnings Change 2005-07 (in %)[a]	0.009	–	-0.044	–	-0.006	–
Nominal Earnings Change 2007-09 (in %)[a]	0.011	–	0.078	–	-0.010	–
Low Skilled (share)	0.18	[0.39]	0.29	[0.45]	0.33	[0.47]
Medium Skilled (share)	0.79	[0.41]	0.67	[0.47]	0.64	[0.48]
High Skilled (share)	0.031	[0.17]	0.038	[0.19]	0.036	[0.19]
Age (in years)	35.5	[10.8]	35.3	[11.2]	33.0	[11.5]
Female (share)	0.62	[0.48]	0.72	[0.45]	0.76	[0.43]
German (share)	0.75	[0.43]	0.74	[0.44]	0.85	[0.36]
East Germany (share)	0.23	[0.42]	0.21	[0.41]	0.11	[0.31]
Tenure (in months)	46.1	[53.9]	36.0	[44.7]	24.3	[27.3]
Exp. in Hospitality Sector (in months)	77.1	[63.3]	52.6	[53.1]	34.6	[36.4]
Share	0.33		0.09		0.57	
Persons	5,746		2,052		13,366	
Observations	89,491		23,121		153,840	

Note: This table presents summary statistics of individual daily earnings data. Standard deviation in brackets. The sample is restricted to workers aged 17-62 years, employed in the hospitality sector and working in guest attending occupations between August 2006 and February 2009. The unit of observation is a worker and time is running in monthly intervals. Real euro values are deflated to 2010 using the consumer price index of the German Bundesbank. Censored earnings are imputed following Gartner (2005). [a]Aggregated change by job types.
Source: Author's calculations based on IAB earnings data.

case of waiters in mini jobs. In most of the empirical analyses I will therefore restrict my attention to this group of workers.

As mentioned above, I only observe *daily* earnings, which are derived from total payments to an employee in a given period divided by the number of calender days (not the days worked) in that period. In Section 4 when discussing alternative explanations, I will come back to this point. Based on Microcensus data I will try to shed more light on whether the observed effect could also be explained by a change in the hours worked.

The individual earnings data are summarized in Table 1. Employees in the hospitality industry are among the lowest-paid occupations in Germany. The gross monthly real earnings of employees in full-time jobs amount to some 1,600 euros. Nearly 60% of all months worked in the sample accrue to waiters in mini jobs, who on average make some 240 euros a month. In the years around the introduction of smoking bans, earnings have declined in real terms for full-time and mini-job workers, mini job waiters have seen their earnings also decline in nominal terms over 2005 and 2009. Waiters are relatively young and predominantly female.

To complement my analysis, I use waves 2004 to 2011 of the German Microcensus, an official yearly survey similar to the US Current Population Survey (CPS). The German Microcensus is based on a 1% random cross-section of German households. Participation is compulsory and non-compliance can be fined. Most population and many labor market statistics are based on the Microcensus. In particular, I observe the state, occupation, sector, smoking behavior (only in 2005 and 2009), full- or part-time status and – unlike in the IAB earnings data – the usual hours worked per week along with a broad set of socio-economic characteristics. Section B2 in the Online Appendix contains more details on the sample construction and variables derived from the Microcensus.

Finally, I include several state level controls. The data on monthly revenues (separately for bars and restaurants) is based on a monthly compulsory survey among an 8% random sample of all establishments in the hospitality sector (about 10,000 businesses per month) with yearly revenues exceeding 50,000 euros and is taken from Ahlfeldt and Maennig (2010).[21] Population figures, election data, and unemployment rates at the state level are taken from the Federal Statistical Office. Weather data are derived from the German Weather Service (DWD) and comprise the monthly state mean temperature, rain, and hours of sunshine). Table A9 in the Online Appendix summarizes these variables, all of which vary at the state and month level.

3.3. Identification Strategies

In my main approach, I will exploit variation between states and over time in a difference-in-differences (DD) fashion. Specifically, to study the effect of smoking bans on the daily earnings of workers in guest attending occupations in the hospitality industry ("waiters" in the following), I estimate variants of the following OLS specification:

$$\ln \text{earnings}_{its} = \beta_1 \text{smoking\_ban}_{ts} + \alpha_i + \alpha_t + \alpha_s + \alpha_{s,m(t)} + \delta \mathbf{X}_{ts} + u_{its} \qquad (2)$$

where earnings_{its} denotes the log average daily earnings of individual i at (monthly) time t (e.g. July 2008) in state s. smoking_ban_{ts} refers to either a binary indicator of whether any kind of smoking ban is in place in state s at time t or to the value of the smoking intensity index in state s at time t as introduced above. α_i,α_t, α_s, and $\alpha_{s,m(t)}$ capture worker, time, state, and state-month

[21] Revenue data is not available for Berlin and Brandenburg due to its underrepresentation in the underlying survey (see Ahlfeldt and Maennig 2010, p. 509).

fixed effects, respectively. The state-month fixed effects account for different seasonal demand patterns in each state (e.g. Bavaria-October estimated across all October observations in different years from Bavaria). $\mathbf{X}_{ts}$ contains further controls that vary at the state-month level such as the current or lags of the state unemployment rate or state specific linear pre-trends that project the trend in daily earnings from the previous 36 months before treatment into the post-treatment period.[22] The parameter of interest is β_1. It indicates by how much percent the daily earnings of a waiter change when a smoking ban (in case of the binary ban indicator) or a strict smoking ban (equivalent to the intensity of Bavaria's initial smoking ban in case of the ban intensity index) is introduced.

The baseline estimation period starts in August 2006, i.e. twelve months before the first smoking ban became effective in August 2007 in Baden-Wuerttemberg and ends six months after the last ban became effective in August 2008 in Mecklenburg-Western Pomerania. In a robustness check, I show that my results do not depend on this specific time choice and are robust to choosing a longer pre- and post-period or a balanced time windows around the treatment (see Table 4). Throughout, I cluster standard errors at the state level, the unit at which the treatment varies.[23]

A potential threat to the common trends assumption underlying the DD identification strategy would be an unobserved policy or demand shock unrelated to smoking bans that affects a state's hospitality sector post treatment. A triple difference-in-differences (DDD) approach tackles such a concern by controlling not only for secular changes in the earnings of waiters *across* states (as in the DD approach), but also for changes in the earnings of other comparable employees in the hospitality industry in the *same* state. This can be seen more easily in the following regression equation:

$$\begin{aligned} \ln \text{earnings}_{itso} = &\beta_1 \text{smoking\_ban}_{ts} + \beta_2 \text{smoking\_ban}_{ts} \times \text{waiter}_o \\ &+ \alpha_i + \alpha_{to} + \alpha_{so} + \alpha_{s,m(t)} + \boldsymbol{\delta}\mathbf{X}_{tso} + u_{itso} \end{aligned} \tag{3}$$

where o indexes different occupation groups, waiter_o is an indicator variable that is one if worker i belongs to the occupation group of waiters, α_{to} and α_{so} denote occupation specific time and occupation specific state fixed effects, while α_i refers to worker fixed effects, $\alpha_{s,m(t)}$ to state-month fixed effects, and $\mathbf{X}_{tso}$ to occupation-state specific pre-trends and further state-level controls as before. β_1 corresponds to the effect of smoking bans on the daily earnings of all occupations except

[22] Specifically, I recover the coefficient ϕ_s for each state from a regression $\ln \text{earnings}_{its} = \sum_{s=1}^{16}(\text{state}_s \times \phi_s \text{time}_t) + u_{its}$ and include $\hat{\phi}_s \times \text{time}_t$ as a new control variable in the main specification thereby projecting pre-treatment trends in the post-treatment period following Repetto (2018). Employing quadratic pre-trends or choosing different lengths of the pre-period leaves my estimates virtually unchanged. My difference-in-differences- and triple difference estimates are also robust to using standard state specific linear trends instead of state specific linear pre-trends.

[23] In Table A10 in the Online Appendix, I report p-values of the treatment coefficients using alternative inference methods including the wild cluster bootstrap (Colin Cameron et al. 2008) and a permutation exercise in which smoking ban policies are randomly shuffled across states (without replacement).

for the group of waiters. The coefficient of interest is now β_2. It refers to the effect of smoking bans on the daily earnings of waiters *net of* secular changes in the hospitality industry within the same state and secular changes in the daily earnings of waiters in non-treated states. As additional control groups, I choose cooks in mini jobs working in the hospitality sector or all other mini job workers in the hospitality sector. In case of cooks, the DDD approach thus compares the evolution of the difference in daily earnings between waiters and cooks in a treated state with the same difference in an untreated state. The underlying identifying assumption states that the difference in daily earnings between waiters and cooks in more or less treated states would have evolved similarly in the absence of smoking bans.

3.4. Estimation Results

Table 2 reports estimates of equation 2 for workers with different types of work schedules (columns) and using different treatment indicators (panels). In odd columns, I present estimation results using a reduced set of controls only (worker-, state-, time, and state-month fixed effects) while in even columns I include additional controls (linear pre-trends and the current and six lags of the monthly state unemployment rate). Irrespective of using a binary smoking ban indicator (Panel A) or the smoking ban intensity as defined above (Panel B), the effect of smoking bans on the daily earnings of full-time workers is close to zero (columns 1 and 2) and only slightly more pronounced but still insignificant for the daily earnings of waiters working regular part-time (columns 3 and 4).[24] In contrast, for the group of waiters in mini jobs the negative effect of smoking bans on daily earnings is highly significant, robust in both specifications and sizable (columns 5 and 6).[25] Not accounting for its strictness, the average smoking ban is estimated to reduce the daily earnings of mini job waiters by about 1.3% (Panel A), while taking into account the intensity of the different smoking bans yields a decrease of 2.4% (Panel B).[26] This is in line with the reasoning outlined above, i.e. the group of waiters in mini jobs supposedly makes up the overwhelming majority of treated workers and that due to their flexible contracts are less affected by wage rigidities. Against the backdrop of the results presented in Table 2 and the reasoning presented above, in the following I will focus on waiters in mini jobs.

[24] It could be that the effect in case of full-time and regular part-time workers materializes more slowly due to the higher rigidities of these employment contracts. However, the impact of smoking bans on the daily earnings of these workers remains insignificant even when extending the length of the post treatment period even up to some two years after the last ban came into effect.

[25] Against the backdrop of a nominal earnings decline of 1% over 2007-2009 documented in column 3 of Table 1, the effect corresponds to nominal cuts in earnings of mini jobs waiters in the hospitality industry. Note that this is not unusual for the German labor market and declines in earnings for lower income percentiles have been documented before, for instance, by Dustmann et al. (2009).

[26] When jointly including the smoking ban indicator and the intensity index in a regression, both coefficients remain significant thus indicating that it is important not only to account for the existence of a ban by itself but also for its intensity.

Table 2: The Effect of Smoking Bans on Waiters' Earnings

	Full-Time Jobs		Regular Part-Time Jobs		Mini Jobs	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Smoking Ban vs. No Ban Indicator						
Smoking Ban Indicator	-0.001 (0.003)	-0.003 (0.002)	-0.010 (0.009)	-0.002 (0.008)	-0.013 (0.003)	-0.013 (0.004)
Adj. R^2	0.948	0.948	0.952	0.952	0.868	0.868
Panel B: Smoking Ban Intensity Index						
Ban Intensity	-0.000 (0.003)	-0.004 (0.002)	-0.012 (0.010)	0.000 (0.008)	-0.023 (0.006)	-0.024 (0.006)
Adj. R^2	0.948	0.948	0.952	0.952	0.868	0.868
Worker, Time, State FEs	✓	✓	✓	✓	✓	✓
State-Month FEs	✓	✓	✓	✓	✓	✓
Extended DD Controls		✓		✓		✓
Start	Aug 2006	Aug 2006	Aug 2006	Aug 2006	Aug 2006	Aug 2006
End	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009
Clusters	16	16	16	16	16	16
Individuals	5,746	5,746	2,052	2,052	13,366	13,366
Observations	89,491	89,491	23,121	23,121	153,840	153,840

Note: This table shows regression results of the impact of smoking bans on log average daily earnings of different sets of workers working as waiters in the hospitality sector. The unit of observation is a worker and time is running in monthly intervals. The set of extended DD controls include state specific linear pre-trends specific to each estimation sample (full-time, regular part-time, and mini job workers) as well as the current and six lags of the monthly state unemployment rate. Standard errors clustered at the state level.
Source: Author's calculations based on IAB earnings data.

Figure 2a plots an event study graph based on a dynamic version of the specification in column 5, Panel A of Table 2. Although I control for state-month fixed effects, there is still considerable volatility left in the earnings data. The figure shows, however, that there are no trends and that on average, earnings are significantly lower after the introduction of a smoking ban (the gray horizontal line and the box indicate the corresponding point estimate of the ban indicator and the 95% confidence interval.) Figure 2b depicts the same event study graph for the group of all mini job workers except for waiters in the hospitality industry. Earnings of this group (conditional on controls) are similarly volatile, but are not significantly lower after the introduction of smokings bans. The two figures also jointly illustrate the idea of the DDD approach in which the daily earnings of non-waiter mini job workers serve as the counterfactual for the daily earnings of waiters in the absence of smoking bans.[27]

As outlined in the identification strategy, the DD approach may be confounded if states exhibit diverging unobserved trends such that the evolution of the outcome in control states alone is not an appropriate counterfactual. A DDD approach helps to overcome this concern by adding a further control group. In Table 3, I estimate variants of equation 3 using cooks (columns 1 to 3) and all other workers (columns 4 to 6) as additional controls groups, both restricted to mini job workers in the hospitality sector.[28]

The DDD estimation results presented in table 3 corroborate the findings using the simpler DD approach from above. Analogously to the DD regression Table 2, the DDD regressions include for each set of regressions a specification with a reduced set of controls (columns 1 and 4), a specification with extended controls (columns 2 and 5), and a specification including state-occupation-month fixed effects instead of state-month fixed effects only to allow for different seasonal patterns at the finer occupation-state level (columns 3 and 6). In both sets of regressions, the coefficients of interest remain very similar across the different specifications. When using cooks as the additional control group in columns 1 to 3, the estimation results indicate a significant decline in the daily earnings of waiters related to the introduction of smoking bans. However, the baseline ban and intensity coefficients are marginally significant and positive indicating that smoking bans might have increased daily earnings for cooks. This might be caused by an increase in food demand in now smoke-free restaurants leading to higher earnings for cooks. Thus, a broader control group including all other mini job workers in the hospitality industry might be a better control group. Indeed, in columns 4 to 6 the impact of the smoking ban indicator or the intensity index on the daily earnings of waiters are virtually identical to the corresponding DD estimates presented above (compare estimates in columns 7 and 8 of Table 2) while the baseline ban indicator and intensity coefficient are practically zero indicating that smoking bans did not have a significant impact on the daily earnings of other mini job employees working in bars and restaurants. That fact that the DD and the DDD estimates coincide makes is unlikely that the DD estimates suffer from a confounding unobserved counterfactual trend and thus reinforces the credibility of the estimation strategy.

[27] Figure A5 in the Online Appendix overlays the two plots and shows that the two follow a similar pattern previous to the introduction of smoking bans.

[28] A list of the occupations used in the DDD analyses and their frequencies can be found in Table A11 in the Online Appendix. Apart from waiters and cooks, these include other guest attending occupations (e.g. event managers or hotel service staff), salespersons, motor vehicle drivers, office workers, as well as housekeeping and cleaning occupations. Table A12 in the Online Appendix shows that waiters, cooks and the group of all other workers are broadly similar with respect to their main characteristics in particular regarding their mean earnings and the usual hours worked.

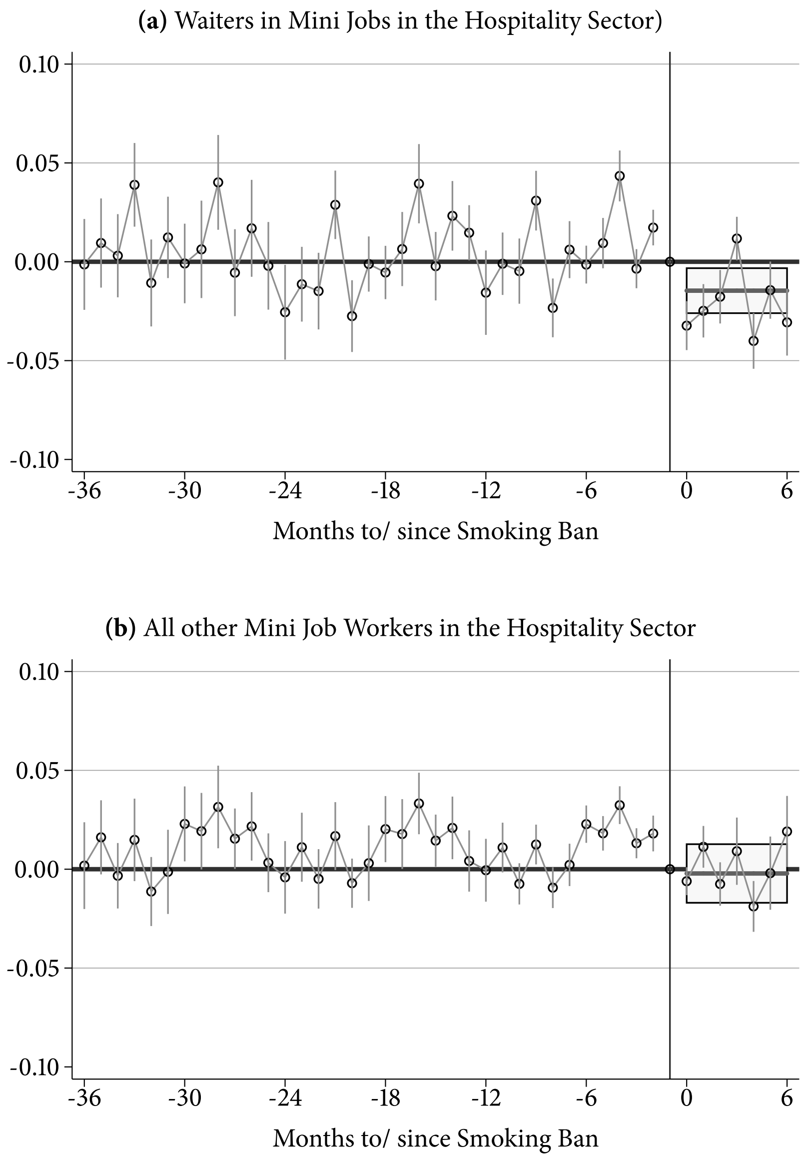

Figure 2: Event Study Graphs Related to the Introduction of Smoking Bans

Notes: This figure plots the regression coefficients of dummies indicating the months to or since the introduction of a smoking for the period 36 months prior and up to 6 months after the introduction of a smoking ban. The period one month prior to the introduction is the reference period. Regressions are based on a dynamic version of the specification in column 5 in Panel A of Table 2. The point estimate and 95% confidence interval of a regression using a smoking ban indicator corresponding to this sample is shown in row 4 of Table 4 and is marked in the figure with a horizontal line and gray box, respectively. The vertical gray lines indicate the 95% confidence intervals of each point estimate. Standard errors clustered at the state level.
Source: Author's calculations based on IAB earnings data.

Table 3: Triple Difference Estimates

	Cooks			All Other Occupations		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Smoking Ban vs. No Ban Indicator						
Smoking Ban × Waiters Indicator	-0.017 (0.005)	-0.017 (0.005)	-0.024 (0.007)	-0.011 (0.004)	-0.011 (0.004)	-0.011 (0.005)
Smoking Ban Indicator	0.010 (0.005)	0.009 (0.005)	0.014 (0.006)	0.001 (0.003)	0.000 (0.003)	0.000 (0.004)
Adj. R^2	0.869	0.869	0.869	0.873	0.873	0.873
Panel B: Smoking Ban Intensity Index						
Ban Intensity × Waiters	-0.032 (0.009)	-0.032 (0.009)	-0.041 (0.009)	-0.024 (0.009)	-0.024 (0.009)	-0.025 (0.008)
Ban Intensity	0.014 (0.006)	0.013 (0.006)	0.019 (0.007)	0.003 (0.004)	0.002 (0.004)	0.003 (0.004)
Adj. R^2	0.869	0.869	0.869	0.873	0.873	0.873
Worker, Occupation-State, Occupation-Time FEs	✓	✓	✓	✓	✓	✓
State-Month FEs	✓	✓		✓	✓	
Occupation-State-Month FEs			✓			✓
Extended DDD Controls		✓	✓		✓	✓
Start	Aug 2006	Aug 2006	Aug 2006	Aug 2006	Aug 2006	Aug 2006
End	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009
Clusters	16	16	16	16	16	16
Individuals	19,716	19,716	19,716	28,393	28,393	28,392
Observations	229,433	229,433	229,433	342,854	342,854	342,847

Note: This table shows regression results of the impact of smoking bans on log average daily earnings of different sets of workers in mini jobs working in the hospitality sector. The unit of observation is a worker and time is running in monthly intervals. The set of extended DDD controls include state-occupation specific linear pre-trends as well as the current and six lags of the monthly state-specific unemployment rate. Standard errors clustered at the state level.
Source: Author's calculations based on IAB earnings data.

3.5. Robustness Checks

In Table 4, I test the robustness of the baseline DD-intensity effect for mini job workers. Each row represents a separate regression. In the first row, the intensity measure turns on already at the time of the *legal* introduction of a smoking ban which in some states did not coincide with the time a smoking ban was enforced by sanctions.[29] The estimate – in line with expectations – is slightly lower but remains highly significant and very similar in magnitude. The intensity coefficient also remains unchanged when I include a dummy that indicates whether establishments in a certain state could avoid imposing a smoking ban by declaring themselves as "smokers clubs" (row 2).[30] Alternatively, in row 3, I reduce the intensity measure by 0.3 for states where smoking clubs could be installed (the same reduction in the index as if smoking was allowed in small bars and restaurants) and again the results remain robust. Furthermore, I show that my results are also not driven by a specific state (Table A13 and Figure A6 in the Online Appendix).

The baseline estimate also holds up to a battery of additional robustness checks presented in Panel B and C of Table 4 including using a longer pre- and post period (from August 2004 to July 2009); a balanced time window equal for all states (including the 36 months before and 6 months after the introduction of a smoking ban in each state); excluding observations from states that introduced smoking bans first (in 2007) or in January or July of 2008, i.e. months where the treatment might be particularly prone to be confounded by seasonal effects; relying on observations from West Germany only; excluding the largest and smallest 5% of daily earnings; including weather controls (temperature, rain, and hours of sunshine); controlling for the monthly state level consumer price index, the initial share of smokers (as a way to control for a potential decline in patronage by smokers), the months to the next state-level elections, the pre-treatment party vote shares interacted with time fixed effects, proxies for the share of foreign visitors (to control for a change in the composition of tourists from countries like the US where tipping is more common and generous), tourism demand, or region and region-month fixed effects to allow for more disaggregated patterns in demand. All of these alternative specifications leave my baseline results virtually unchanged.

Taken together, the DD and DDD estimates and the robustness checks provide strong evidence in favor of a *causal* interpretation of the impact of smoking bans on the daily earnings of waiters..

[29] This was the case in Berlin, Brandenburg, Bremen, Mecklenburg-Western Pomerania, Lower Saxony, Saarland, and Saxony-Anhalt. It was non uncommon to see people smoking in bars in restaurants in Berlin during January and June 2008 when a smoking bans was *legally* in place but violations did not carry any (financial) consequences for establishment owners or guests. This was also the case in Lower-Saxony where smoking was still prevalent in the transition period as mentioned in this newspaper article (in German) `http://www.tagesspiegel.de/berlin/berlin-bis-juli-2008-keine-bussgelder-beim-rauchverbot/1088506.html`.

[30] As Kvasnicka and Tauchmann (2012, p. 4541) point out, this kind of relabeling only developed into a major loophole in Bavaria and North Rhine-Westphalia, consequently the dummy is one for Bavaria between January 2008 and July 2009 and for North Rhine-Westphalia between July 2008 and April 2011, the end dates marking the time when the loopholes were shut down.

Table 4: Robustness Checks

	Intensity Coefficient	Adj. R^2
Panel A: Institutional Features		
1. Legal instead of Effective Introduction Date	-0.022 (0.006)	0.868
2. Controlling for Smoker Clubs	-0.022 (0.008)	0.868
3. Ban Intensity Adjusted for Smoker Clubs	-0.025 (0.007)	0.868
Panel B: Different Sample/ Time Periods		
4. Balanced Time Windows (36 Months before, 6 after)	-0.025 (0.007)	0.847
5. Longer Pre- and Post Period (August 2004 - July 2009)	-0.023 (0.008)	0.830
6. Excluding States with Bans Introduced in 2007	-0.032 (0.007)	0.871
7. Excluding States with Bans Introduced in January 2008	-0.020 (0.006)	0.870
8. Excluding States with Bans Introduced in July 2008	-0.024 (0.008)	0.867
9. Excluding Spells from East Germany	-0.021 (0.005)	0.866
10. Trimming Top/ Bottom 5% of Wages	-0.021 (0.006)	0.851
Panel C: Additional Controls		
11. Weather Controls (Temperature, Rain, Sunshine Hours)	-0.026 (0.005)	0.869
12. Monthly state level CPI	-0.022 (0.006)	0.869
13. Time to Next State-Level Elections	-0.022 (0.005)	0.868
14. Initial Party Vote Shares × Time FEs	-0.025 (0.011)	0.868
15. Initial Share of Smokers in 2005	-0.020 (0.006)	0.868
16. Monthly Share of Foreign Overnight Stays[a]	-0.025 (0.007)	0.868
17. Monthly Index of Foreign and Domestic Overnight Stays[b]	-0.023 (0.006)	0.868
18. County and County-Month FEs	-0.024 (0.006)	0.870

Note: This table shows additional robustness checks of the impact of the smoking ban intensity on log average daily earnings of workers in mini jobs working as waiters in the hospitality sector. The unit of observation is a worker and time is running in monthly intervals. Each row represents a separate regression using a using extended DD controls in addition to the indicated specification. The sample covers August 2006 to February 2009 as in the baseline if not indicated otherwise. Standard errors clustered at the state level. [a]Not available for Hamburg and Schleswig-Holstein. [b]lags 0-6.
Source: Author's calculations based on IAB earnings data.

4. Alternative Explanations

4.1. Revenues

In the previous section, I established a negative impact of smoking bans on the daily earnings of waiters in mini jobs working in hospitality establishments. I argued that this is in line with a simple compensating differential model. An alternative explanation for the decline in earnings would be a decline in hospitality revenues driven by a decrease in patronage of smokers. In that case, the decline in waiters' earnings just reflected a pass-through of firm revenues to earnings.

To evaluate this possibility, I perform a difference-in-differences analysis at the state level, separately for the index of real revenues of restaurants (Panel A) and bars (Panel B) in Table 5. Note that the revenue data vary at the state-month level and are only available for 14 out of the 16 German federal states (data for Berlin and Brandenburg are not available for this period).[31] All regressions are weighted by state population and control for state- and time fixed effects, the monthly state-specific unemployment rate and the monthly state-specific consumer price index.[32]

Hospitality revenues show a high level of seasonal variability. The results show that if one does not properly account for the seasonal variation by including state-month fixed effects, the estimations yield spuriously negative point estimates. Once properly accounting for seasonal effects, the point estimates, if anything, suggest a positive effect of smoking bans on revenues. Even though my preferred specifications include state-month fixed and explain more than 80% of the variation in the data, all coefficients are estimated imprecisely. A power calculation based on the change in R^2 for the models in column 3 shows that this is not necessarily due to a lack of statistical power. Given the estimated effect sizes in column 3, a power analysis yields values of 0.77 (0.85) and 0.64 (0.75) at a significance level of 5% (10%) for restaurants and bars, respectively. All in all, these results make me reasonably confident that smoking bans did not lead to a significant decrease in revenues of bars and restaurants and that my setting is sufficiently powered to detect a potential decrease.

My estimates are in line with a host of studies from the US and other countries that do not find any robust evidence for a negative effect of smoking bans on hospitality revenues (see for instance the review articles by Scollo et al. 2003; Scollo and Lal 2008). They are also in line with Ahlfeldt and Maennig (2010) who perform a similar difference-in-differences analysis based on the same official German revenue data as I do here covering January 2005 and December 2009 and who do not find any statistically significant decline in revenues neither for bars nor restaurants.

[31] For more details regarding the revenue index data see Ahlfeldt and Maennig (2010).

[32] Hamburg and Schleswig-Holstein do not report their own state-specific CPI and are thus not included in the analyses reported in Table 5 resulting in 12 clusters. Table A14 in Online Appendix shows that regressions not controlling for the monthly state-specific CPI and including observations from these two states or using the German-wide CPI for these states are very similar to the ones reported in Table 5.

Table 5: Impact of Smoking Bans on Revenues of Restaurants and Bars

	Dependent Variable: *Log Real Revenue Index*				
	(1)	(2)	(3)	(4)	(5)
		+	+	+	+
	Simple DD	Linear State Trend	State-Month FEs	Weather Controls	Tourism Demand
Panel A: Restaurants					
Ban Intensity	-0.041	0.013	0.045	0.045	0.051
	(0.035)	(0.045)	(0.058)	(0.054)	(0.056)
Adj. R^2	0.751	0.837	0.850	0.852	0.853
Panel B: Bars					
Ban Intensity	-0.044	-0.002	0.052	0.055	0.056
	(0.057)	(0.047)	(0.074)	(0.069)	(0.069)
Adj. R^2	0.617	0.757	0.801	0.805	0.804
State & Time FEs	✓	✓	✓	✓	✓
Unemp. Rate & State CPI	✓	✓	✓	✓	✓
Linear State Trends		✓	✓	✓	✓
State-Month FEs			✓	✓	✓
Weather Controls				✓	✓
Index of Domestic and Foreign Overnight Stays					✓
Start	Jan 2005	Jan 2005	Jan 2005	Jan 2005	Jan 2005
End	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009
Clusters	12	12	12	12	12
Observations	576	576	576	576	576

Note: This table presents regressions of the monthly state-level log real revenue index of restaurants (Panel A) and bars (Panel B) on the smoking ban intensity index and further controls. The unit of observation is a state and time is running in monthly intervals. Revenue data is not available for Berlin and Brandenburg. Weather controls include the monthly state mean temperature, rain amount, and hours of sunshine. CPI refers to the monthly state consumer price index (not available for Hamburg and Schleswig-Holstein). The index of domestic and foreign overnights stays refers to the number of overnights stays by tourists of domestic or foreign origin. Standard errors clustered at the state level. All regressions are weighted by population size.
Source: Author's calculations based on Ahlfeldt and Maennig (2010).

They suggest that their findings might be explained by increased spending of non-smokers that compensated the reduced spending by smokers or that smokers did not reduce their spending in the first place.[33]

Another issue could be that the average effect masks important heterogeneous effects between different sorts of establishments. For instance, old-school corner bars with a majority of their customers being smokers might be hit harder and thus are forced to go out of business while modern, more food oriented establishments or trendy coffee shops might benefit from a shift in demand due to smoking bans.[34] However, Kvasnicka and Tauchmann (2012) do not find any significant effect of smoking bans neither on the number of business closures nor business start ups up until December 2008 (i.e. between 5 and 17 months after the introduction of smoking bans).

The DDD estimates presented earlier (Table 3) also provide evidence that declining revenues are unlikely to serve as the main explanation for declining daily earnings of waiters. In particular, if the decline in waiters' daily earnings was caused by a general decline in revenues of bars and restaurants, other occupations would likely also see their daily earnings falling after the introduction of smoking bans. Additionally, row 15 of Table 4 shows that the baseline DD coefficient is robust to controlling for the initial share of smokers (measured in 2005) interacted with time dummies as a way of controlling for a decline in patronage by smokers. Thus, in light of these results and the revenue regressions of Table 5 it seems unlikely that a major part of the relative decrease in waiters' daily earnings caused by smoking bans was driven by a decline in hospitality revenues.

4.2. Hours Worked

The analyses so far showed that the introduction of smoking bans was associated with lower average daily earnings. However, lower earnings could either be the result of lower hourly wages or fewer hours worked (or a combination of the two). Ideally, I would like to decompose the total effect into the part that is due to a change in hours worked and the part that is due to a change in the hourly wage. Unfortunately, in the IAB administrative labor market data I do not observe the hours worked (apart from the distinction between different forms of full- or part-time schedules).

[33] In contrast, Kvasnicka and Tauchmann (2012) use revenue data for the *entire* hospitality industry (thus also including revenues from accommodation businesses such as hotels or camping sites likely unaffected by smoking bans). They find a statistically significant decline in revenues of at some 2%. However, they use data only covering the period January 2007 to September 2008 and thus use considerably less pre- and post-treatment observations than Ahlfeldt and Maennig 2010 and than I do here. They also do not include the monthly state-level consumer price index and do not account for state-month specific seasonal effects which I argue are particularly important in the context of such volatile and seasonal data. For instance, 5 out of 16 states introduced smoking bans in January or February, two months characterized by particularly low demand in some states but not all (e.g. due to winter sports tourism).

[34] Kvasnicka and Tauchmann (2012) note that in the sales data some of the very small bars with revenues of less than 50,000 euros per year are excluded. In the IAB [earnings] data, there is no such lower limit censoring and employees of small bars are also sampled proportionally.

Therefore, I use data from the Microcensus, a 1% compulsory population survey. Amongst others, individuals in the Microcensus are asked about their usual hours worked per week.[35] I construct a sample as similar to the IAB earnings sample as possible by selecting all individuals with a mini job between 17 and 62 years in East and West Germany.[36] The key drawback of using Microcensus data for the purpose at hand is that it comes in yearly and not in monthly intervals and is available in cross sections only and not as panel data such that the inclusion of worker fixed effects as before is not possible. Another caveat of the Microcensus data is that most variables and in particular the hours worked are self-reported and – given the official nature of the survey – it is unlikely that individuals will report all of their informal hours worked (Boockmann et al. 2010, 43f).[37] Bearing these limitations in mind, the Microcensus is still the best data set available to analyze the effect of smoking bans on hours worked by waiters in mini jobs in Germany.[38]

To go beyond the yearly variation of the Microcensus data, I exploit the time when an individual was interviewed. This variable is available in quarterly time (e.g. Q1 of 2008). I construct a pooled cross section data set where the time record of an observation is set to the quarter of the interview. To estimate the impact of smoking bans on the usual hours worked I then run variants of the following DDD regression model:

$$\begin{aligned} \ln \text{hours}_{itso} = &\beta_1 \text{smoking\_ban}_{ts} + \beta_2 \text{smoking\_ban}_{ts} \times \text{waiter}_o \\ &+ \alpha_{to} + \alpha_{so} + \alpha_{soq} + \gamma \mathbf{Z}_{ts} + \boldsymbol{\delta} \mathbf{X}_{it} + u_{itso} \end{aligned} \tag{4}$$

where $\ln \text{hours}_{itso}$ denotes the log of the usual hours worked reported by individual i at (quarterly) time t working in state s and working in occupation o. The variable smoking_ban_{ts} refers to either a smoking ban indicator or the smoking ban intensity as before. Since the dependent variable now runs in quarterly time (as opposed to monthly time in the IAB earnings data), smoking_ban_{ts} is computed as the proportion of a quarter a smoking ban is in place or as the average smoking ban intensity in a given quarter (for instance, Rhineland Palatinate introduced a smoking with intensity 0.5 in February 2008; thus the ban indicator in 2008 Q1 equals 2/3 and the intensity index 1/3). α_{to} denotes occupation-time, α_{so} occupation-state, and α_{soq} state-occupation-quarter fixed effects. The latter are intended to account for state-occupation specific seasonal effects as before. $\mathbf{Z}_{ts}$ contains time-varying state level controls including the quarterly state-specific unemployment rate, weather controls, and state-occupation specific linear trends. Since individuals cannot be linked

[35] The exact wording of the question is (translated from German): "How many hours (potentially rounded) do you usually work (including regular overtime hours)?"

[36] See Section B2 in the Online Appendix for more details on the construction of the sample and the variables.

[37] At the same time, earnings in the IAB data only refer to formal employment relations. To the extent that proportion of informal to formal hours worked did not change, this should thus not constitute an issue.

[38] The German Socio-Economic Panel (GSOEP) would be an alternative data set where (self-reported) hours worked are also available and the data is organized as panel data. This data set, however, suffers from sample sizes far too small to conduct analyses at the level of waiters in mini-jobs across different states.

across different years, no worker fixed effects can be included. Therefore, I control for a broad set of individual characteristics denoted by $\mathbf{X}_{it}$ that were reported at the time of the interview.[39] All regressions are weighted by survey weights (available from 2005 onwards). Standard errors are clustered at the state level.[40]

The results of estimating the impact of smoking bans on the usual hours worked using a DDD regression approach are presented in Table 6.[41] The coefficients of interest - the interaction between smoking bans and the indicator for waiters – are all positive and significant implying that the usual hours worked by mini job waiters have increased as a results of smoking bans. This is in line with the predictions of a simple compensating differentials model. Relative to the control group, the estimates imply an increase in the usual hours worked by 10% or about 1.2 hours more based on the average hours worked by waiters in mini jobs in 2006.

The cross sectional nature of the data set raises concerns about potential selection. Two pieces of evidence contribute to alleviating such concerns. First, as mentioned above I include a broad set of individual characteristics ($\mathbf{X}_{it_q}$). The inclusion of these controls barely changes the coefficients of interest but raises the R^2 considerably. To get a more formal assessment of this intuition, I follow Oster (2019) in applying the method of Altonji et al. (2005). As the results presented in Table A16 in the Online Appendix imply, selection on unobservables based on the DDD results would have to be at least three times (or 22.8 times in the median case) greater than selection on observables to attribute the entire effect of smoking bans on the hours worked to selection effects. This makes it less likely that the impact of smoking bans on the usual hours worked is entirely driven by unobservables.

Second, I perform a synthetic control approach that relies on different underlying identifying assumptions.[42] The results and further details including inference tests are presented in Figures A7 to A9 in the Online Appendix. They corroborate the findings of the DDD regressions: if anything,

[39] Specifically, $\mathbf{X}_{it_q}$ contains dummy variables for being female, having a partner, having children under 18 years of age in the household, having a German citizenship, and whether the main source of income is from own work (as opposed to transfers or capital income). Furthermore, I include dummies for each of eight age categories, nine city size categories, three education categories, five categories referring to the years passed since migrated to Germany, and five household size categories along with tenure and tenure squared at the current employer.

[40] To ensure a sufficient number of observations per cell, I require a minimum of 15 observations in each state-occupation-time combination which results in observations from the City of Bremen dropping out resulting in a total of 15 clusters. Allowing even more sparsely populated cells increases the variance of the estimates but also does not yield negative point estimates and thus does not change the main conclusions.

[41] The results using a DD approach are reported in Table A15 in the Online Appendix; they are in line with the DDD results and also indicate a positive effects of smoking bans on the hours worked.

[42] While the DDD regression identify the treatment effect based on the parallel trends assumption, the synthetic control approach requires that the post-treatment outcomes of the treatment and control group would be the same conditional on past-outcomes and the observed covariates in the absence of treatment (independence conditional on past outcomes).

Table 6: DDD Regression of the Impact of Smoking Bans on Hours Worked

	(1) Simple DDD	(2) + Ind. Controls	(3) + Trends	(4) + Occupation -Quarter FEs
Panel A: Ban vs. No Ban Indicator				
Smoking Ban × Waiters Indicator	0.097	0.095	0.102	0.102
	(0.055)	(0.043)	(0.042)	(0.019)
Smoking Ban Indicator	0.020	0.024	0.023	0.014
	(0.009)	(0.010)	(0.009)	(0.011)
Adj. R^2	0.067	0.131	0.132	0.126
Panel B: Smoking Ban Intensity Index				
Ban Intensity × Waiters	0.075	0.078	0.121	0.099
	(0.023)	(0.026)	(0.025)	(0.037)
Ban Intensity	0.009	0.014	0.032	0.028
	(0.019)	(0.018)	(0.010)	(0.012)
Adj. R^2	0.067	0.131	0.132	0.126
Occupation-State, Occupation Time FEs	✓	✓	✓	✓
State Level Controls Z	✓	✓	✓	✓
Individual Controls X		✓	✓	✓
Occupation-State Specific Linear Trends			✓	✓
Occupation-State -Quarter FEs				✓
Start	2005	2005	2005	2005
End	2009	2009	2009	2009
Cluster	15	15	15	15
Observations (Individuals)	41,302	41,302	41,302	41,302

Note: This table shows regression results of the impact of smoking bans on the log usual hours worked per week. The sample is restricted to individuals in mini-jobs. The unit of observation is a worker and time is running in quarterly intervals. Time refers to the running time variable, quarter to one of the four quarters of any year. The set of individual controls X include dummy variables for being female, having a partner, having children under 18 years of age in the household, having a German citizenship, and whether the main source of income is from own work (as opposed to transfers or capital income), dummies for each of eight age categories, nine city size categories, three education categories, five categories referring to the years passed since migrated to Germany, and five household size categories along with tenure and tenure squared at the current employer. Regressions weighted by survey weights. Standard errors clustered at the state level.
Source: Author's calculations based on Microcensus.

the evidence points to an increase in the usual hours worked by mini job waiters relative to a synthetic control group.[43]

A further piece of evidence based on Microcensus data also supports the predictions of a compensating differentials model and are in line with the previous earnings regressions: If anything, smoking bans are associated with a decrease in individual net incomes.[44]

Summarizing, based on the best data available and using different methodologies, I find no evidence that smoking bans had a negative impact on the hours worked by waiters that could explain the decline in their daily earnings. If anything, the evidence points to an increase in the hours worked by waiters. Supporting results regarding individual incomes and employment corroborate these findings.

5. Valuating the Health Benefits of Smoking Bans

In the framework of a compensating differentials models, my preferred estimate (column 8 Panel B of Table 2) implies that mini job waiters are willing to trade off 2.4% of their earnings for the improved working conditions. Does this actually reflect the value of the underlying health benefits? A back-of-the-envelope calculation based on the improvement in quality adjusted life years (QALYs) can help to shed some light on this question.

Panel A of Table 7 uses the empirical findings of the previous sections to compute the revealed willingness to pay (WTP) of waiters. Summed up over an employment duration of about three years implied by the average experience in the hospitality industry (column 3 of Table 1) and based on the average earning of 240 euros per month, a waiter's revealed WTP for the improved working conditions amounts to a total of 207 euros (Panel A).

A multitude of medical studies have examined the effect of smoking on health conditions. However, to the best of my knowledge no study has quantified the impact of working in a smoke allowed bar or restaurant on the health-related quality of life (HRQoL). The measurement of the effect in terms of HRQoL is necessary to infer the valuation of the health improvement associated with imposing a smoking ban. To inform my back-on-the-envelope calculation, I use a range of estimates of the impact of exposure to secondhand smoke and of firsthand smoking on HRQoL. Kim et al. (2015) find that exposure to SHS significantly reduces HRQoL by 0.007 years. Regular SHS exposure of 2 hours and more per day increases that difference to 0.011. This means that

[43] It would be interesting to confirm this finding by looking at employment related outcomes. Table A17 in the Online Appendix presents evidence on the turnover, on probabilities to start or end a job in a given month as well as (on data aggregated at the state level) on the number of months worked and state-wide turnover. The results do not provide evidence in support of significant changes in these variables, but unfortunately, the setup very underpowered.

[44] See Tables A18 and A19 in the Online Appendix for results and further details. Note that in the Microcensus, one can only observe the net income of a person (or household) including transfers and other income sources but not the or earnings associated with a specific job.

Table 7: Comparison of Different Valuations of Health Benefits of Smoking Bans

Panel A: Revealed WTP Among Waiters in Mini Jobs

$\hat{\beta}_{\text{intensity}}$	2.4%
Average earnings of mini job waiter	240€
Average duration of exposure	3 years
Implied WTP over three years	**207€**

Panel B: Valuation of the Increase in QALYs

	Δ HRQoL of...		
	average SHS exposure[a]	elevated SHS exposure[a]	light FH smoker[b]
Assumed value of one QALY	0.007	0.011	0.021
100.000 €	2,100 €	3,300 €	6,300 €
30.000 €	630 €	990 €	1,890 €
5,760 €	121 €	190 €	363 €

Panel C: Implied Value of one QALY given a WTP of 207 €

	9,874 €	6,284 €	3,291 €

Note: This table presents back-on-the-envelope calculations of the implied willingness-to-pay (WTP) for the health improvement associated with smoking bans for hospitality workers, see text for further details. HRQoL = health-related quality of life, QALY = quality adjusted life year, SHS = secondhand smoke, FH = firsthand. Elevated SHS exposure refers to a regular exposure of ≥ 2 hours per day.[a] Kim et al. (2015), compared to an individual unexposed to SHS. [b] Vogl et al. (2012), compared to a never smoker.
Source: Author's calculations, Kim et al. (2015), Vogl et al. (2012), National Institute for Health and Care Excellence (2013) and Anderson et al. (2014).

an individual regularly exposed (≥2h/ day) to SHS lives 0.007 (0.011) quality-adjusted life-years (QALYs) less per year of exposure compared to an unexposed individual with the metric of QALYs running from 0 (=being deceased) to 1 (=perfect health). Vogl et al. (2012) study the effect of firsthand-smoking and estimate that the difference in HRQoL of a never smoker compared to a light smoker (one who smokes under 10 cigarettes a day) is 0.021. If one is willing to assume that the health improvement of a waiter affected by a strict smoking ban is comparable to one of those differences in HRQoL, then the valuation of the health improvement can be inferred by multiplying the improvement in HRQoL with the value of a QALY. Based on a range of common values used in the literature and by public health authorities, I choose 100,000€, 30,000€ and 5,760€ (two times the yearly earnings of a mini job waiter) as possible values for one QALY.[45]

[45] Estimates of a QALY range from 183,000 to 264,000 USD (Braithwaite et al. 2008); 129,090 USD (Lee et al. 2009); 50,000 to 100,000 USD (Report of the American College of Cardiology by Anderson et al. 2014); 1 to 3 times the per capita GDP following the recommendation of the WHO, 20,000 - 30,000 GBP used by the United Kingdom's

Panel B of Table 7 shows the valuations of the increase in QALYs associated with the introduction of smoking bans when using different parameter combinations. At the upper bound, assuming one QALY is worth 100.000 euros and that working in a bar or restaurant without a smoking ban is comparable to being a light smoker results in a valuation of 6,300 euros. At the lower bound, assuming that working as a waiter in a smoke-allowed bar is comparable to regular SHS exposure implies a valuation of just 121 euros. The WTP implied by my estimates (207 euros) falls within a plausible middle ground between an increased SHS-exposure and being a light firsthand smoker. A different way to assess the valuation of health benefits is to calculate the implied valuation of a QALY given a WTP of 207 euros and the various differences in HRQoL from the medical literature (Panel C). As it turns out, the observed WTP is consistent with the health effects of an average to elevated SHS exposure and a QALY-valuation at the lower bound but larger that twice the yearly earnings as a waiter. This seems plausible as a mini job waiter is likely to draw on additional income apart from her mini job earnings such as social or family transfers. The back-of-the-envelope calculations thus show that the observed decrease in waiters' earnings associated with the introduction of smoking bans can be rationalized by estimates of the health impact of an elevated SHS exposure.

With at most 9,900€, the implied valuation of one life year in perfect health (QALY) is at the lower end of corresponding valuations suggested in the literature or applied in actual cost-benefit analyses of health interventions. A potential explanation for this seemingly low valuation might be heterogeneity in the WTP for the health benefits of a smoking ban. Studying the WTP for alternative work arrangements, Mas and Pallais (2017) find that while the average WTP is low, some workers have a much higher WTP for some amenities. Given heterogeneity in the valuations of health benefits and that my estimated earnings decline might not fully reflect the marginal WTP (due to frictions etc.), the above calculations would understate actual market compensating differentials for smoking bans in the earnings of waiters.

6. Concluding Remarks

The classical topic of compensating differentials has re-emerged on the agenda of the economics profession, mainly driven by the availability of new and better worker and firm level microdata. This is despite, or because, so far it has been hard to find compelling empirical evidence for this classical concept first put forward by Adam Smith.

Exploiting a policy experiment, I find evidence that strongly suggests that exposure to secondhand smoke is priced in the labor market. Under the plausible assumption of a competitive

National Institute for Health and Care Excellence (NICE 2013) to assess health technology value for the UK's National Health Service (NHS). Finally, summarizing various studies of the individual willingness to pay by trading off salary for additional years of life, the Institute for Clinical and Economic Review (2019) concludes that "many [of these studies] are in the range of two times the individual's salary.

labor market, employers in the hospitality sector have to pay their workers an earnings premium to compensate for the exposure to secondhand smoke. When a smoking ban is introduced, it is no longer necessary to pay such a premium and we should *ceteris paribus* see a drop in earnings that exactly equals the compensating differential of bearing the exposure to secondhand smoke. My setting offers three main advantages relative to the previous literature: (i) an arguably exogenous, within job, salient and effective variation in amenities that overcomes the endogeneity of amenities and jobs and their and opaqueness which plagued many of the previous analyses; (ii) the use of panel data which allows holding individual productivities constant; and (iii) a competitive and rather flexible segment of the labor market that allows the equilibrium to emerge quickly and also makes an adjustment via prices more likely than via quantities.

My baseline estimates indicate that introducing a complete smoking ban that grants no exceptions leads to a decrease in the daily earnings of waiters of 2.4%. I rule out several confounding factors including seasonal, political or weather effects. To interpret the estimated effects as a compensating differential, I present evidence that refutes two main alternative explanations, namely a decrease in revenues of bars and restaurants and a reductions of hours worked. A back-of-the-envelope calculation suggests that the WTP for working in a smoke-free establishment implied by my estimates is consistent with the impact on health related quality of life associated with an elevated secondhand smoke exposure.

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APPENDIX

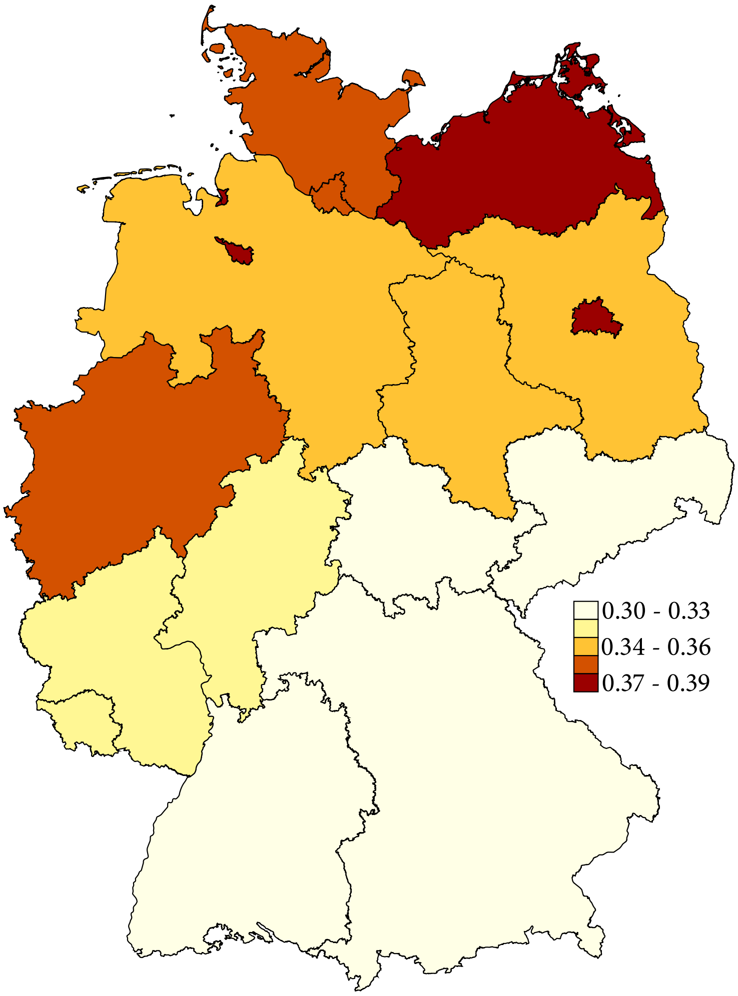


Figure A1: Map of Share of Population Smoking

Notes: This map shows the share of smokers in each state in 2005 based on Microcensus data. The sample is based on Microcensus waves 2005 and 2009 and is restricted to individuals aged 17-62 not in civil service (*Beamte*) and with non-missing values the control variable values used in Table 6. Statistics are weighted by survey weights.
Source: Author's calculations based on the German Microcensus.

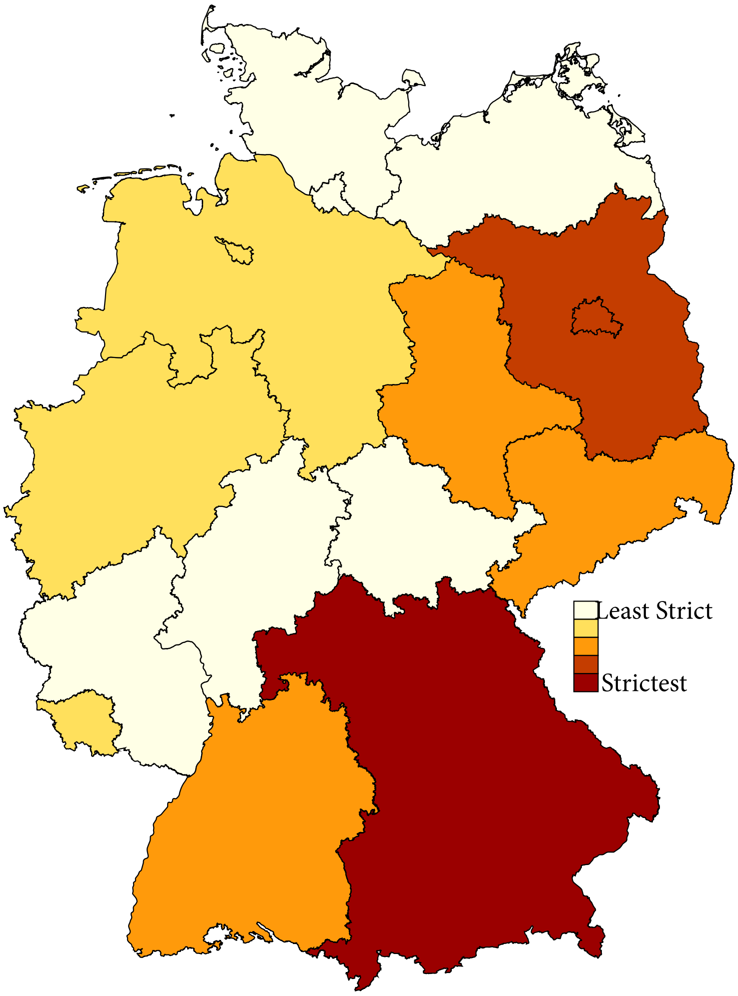


Figure A2: Map of Initial Smoking Ban Intensities in Germany

Notes: This map shows the initial intensity of smoking bans according to the index specified in equation 1. "Strictest" refers to the strictest ban (corresponding to Bavaria's initial smoking ban, index value 1) and "least strict" to the least strict ban observed (corresponding to Rhineland-Palantinate, index value 0.5).
Source: Author's calculations based on respective state regulations.

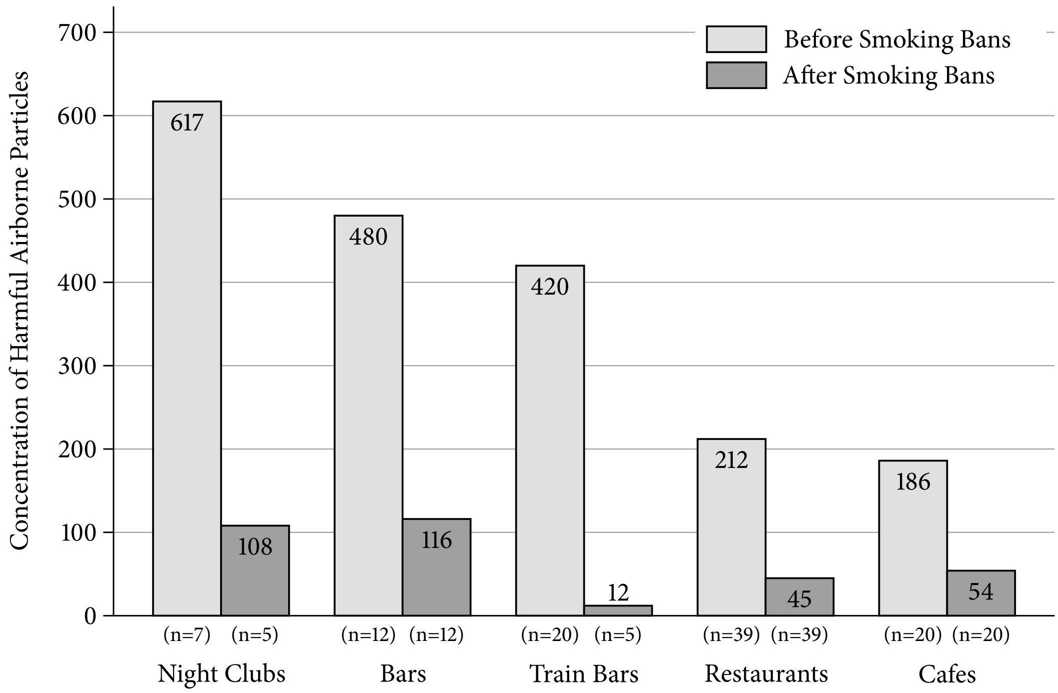


Figure A3: Air Quality Measurements Before and After the Introduction of Smoking Bans

Notes: This figure compares the average concentration of particle matter (PM) up to 2.5 μm per m^3 measured in the indoor air of five different types of hospitality establishments in Germany before (2005) and after (2009) the introduction of smoking bans. The post measurement for train bars was taken in 2007.

Notes:[Source:] DKFZ (2010, 24ff)

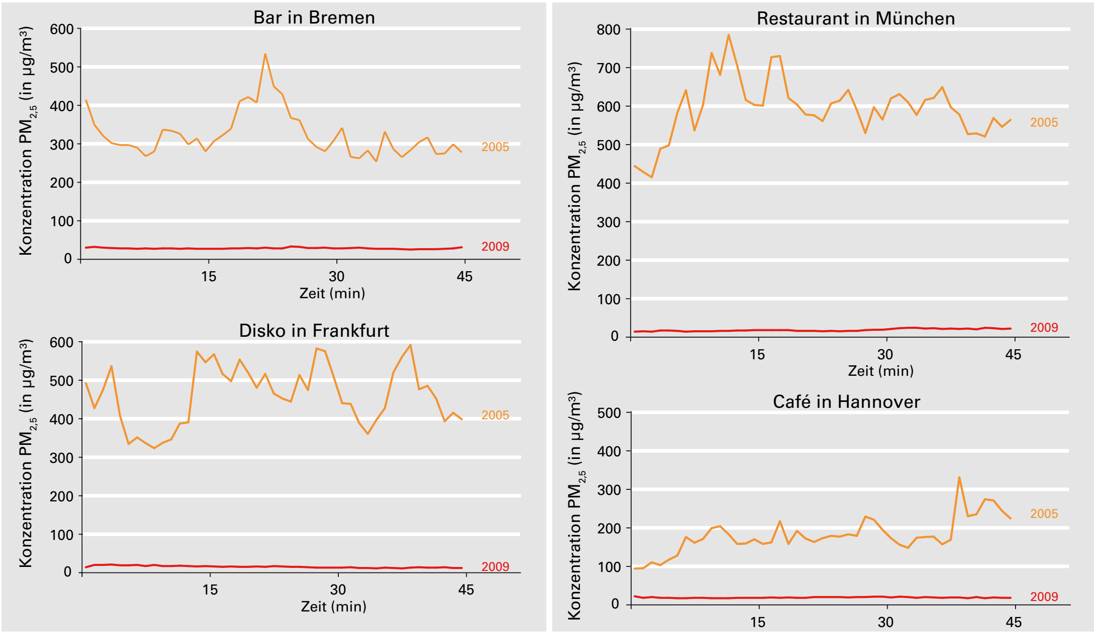


Figure A4: Air Quality Before and After the Introduction of Smoking Bans in German Hospitality Establishments with a Comprehensive Ban

Notes: This figure compares the times series of the average concentration of particles up to 2.5 µm in the indoor air before (dark gray/ red) and after (light gray/ orange) the introduction of smoking bans in hospitality establishments in Germany with a comprehensive smoking ban.
Source: DKFZ (2010, 25ff)

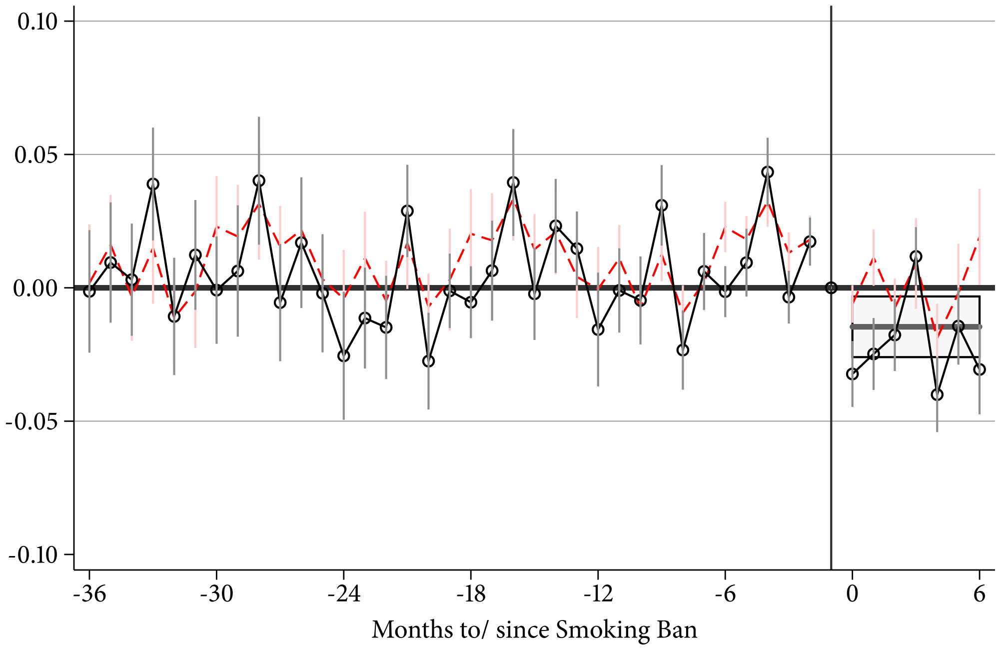


Figure A5: Illustration of DDD Approach

Notes: See notes for Figure 2.
Source: Author's calculations based on IAB earnings data.

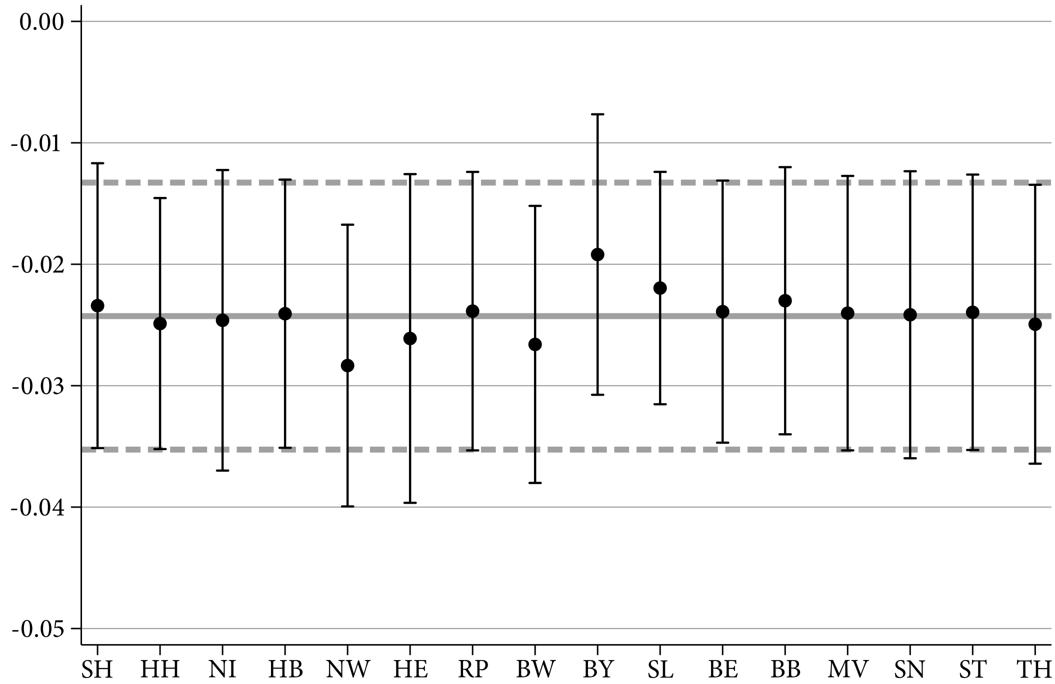


Figure A6: Leave One State Out at a Time

Notes: This figure plots the coefficients (filled black dots) and corresponding 95% confidence intervals (dashed lines) from regressions using extended controls of the smoking ban intensity on waiters' log daily earnings where observations from the state indicated on the x-axis are left out. The solid thick gray line (dashed gray lines) refers to the baseline estimate (95% confidence interval) including observations from all 16 states.
Source: Author's calculations based on IAB earnings data.

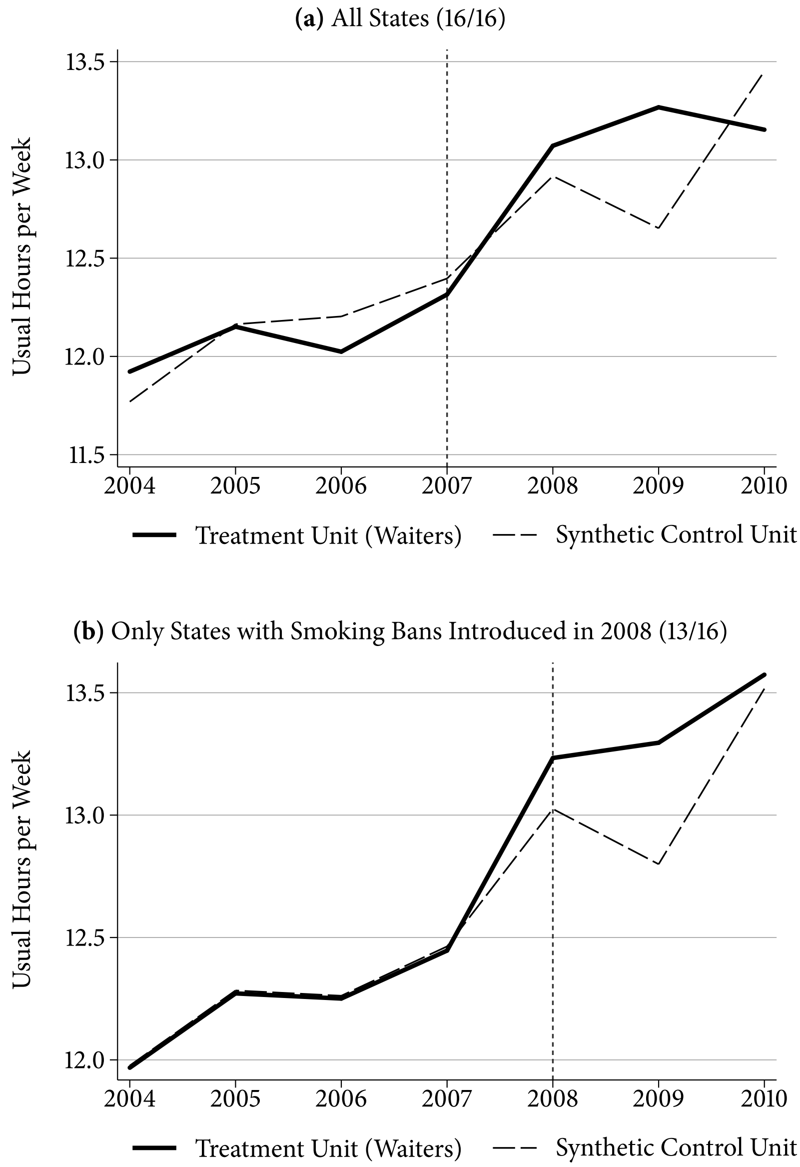
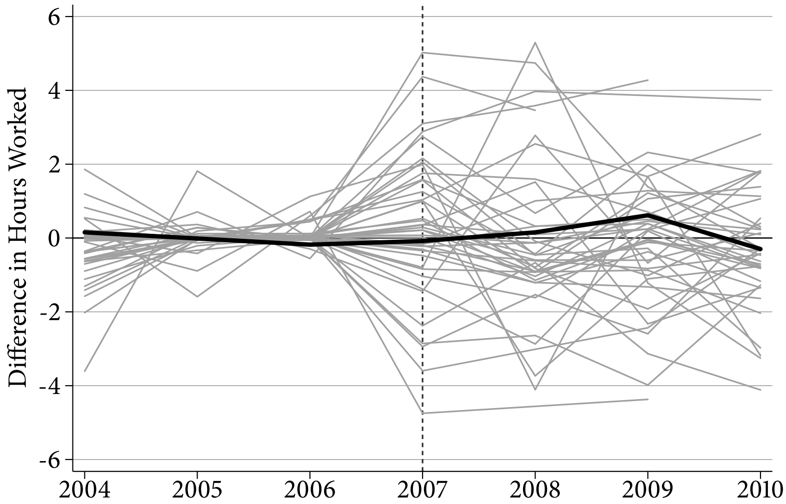


Figure A7: Evolution of Hours Worked (Synthetic Control Group Approach)

Notes: This figure compares the evolution of the usual hours worked per week of mini job workers employed as waiters to a synthetic control group constructed from a pool of all other mini job workers in occupations with at least 15 observations per state. The predictor variables are averaged over the entire pre-treatment period and include age, the share of females, and the share of workers in East Germany along with the hours worked in 2005 and 2006. Fully nested and fully robust (global) optimization procedure of Hainmueller, Abadie, and Diamond's `synth` package applied. A complete list of donor pool occupations and the according synthetic control weights is provided in Table A20 (only in German).
Source: Author's calculations based on the German Microcensus.

(a) Treatment and Placebo Differences in Hours Worked



(b) Treatment and Placebo Pre/Post MSPE Ratios

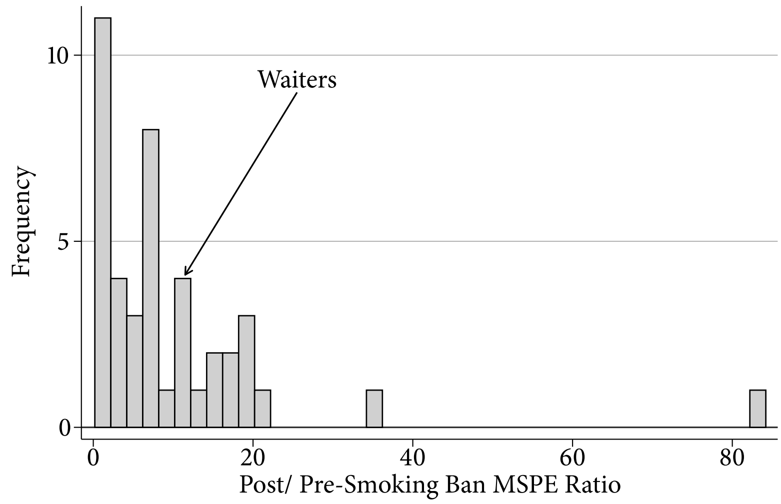


Figure A8: Synthetic Control Inference Graphs
(All States)

Notes: This figure presents two approaches commonly used for inference in a synthetic control approach. Figure A8a, shows the result of a placebo exercise in which all occupations in the donor pool are iteratively assigned to be treated while waiters are moved into the control group. For four occupation groups no synthetic control group could be constructed, they remain, however, in the donor pool. Figure A8b plots the ratios of the pre- and post mean squared prediction errors (MSPE). Occupations with pre-smoking ban MSPE ten times higher than that of waiters discarded. When including observations from all 16 state and setting 2007 to be the first treatment year, neither inference approach indicates that the hours worked would significantly differ between the group of waiters and a synthetic control group in the period after the introduction of smoking bans.
Source: Author's calculations based on the German Microcensus.

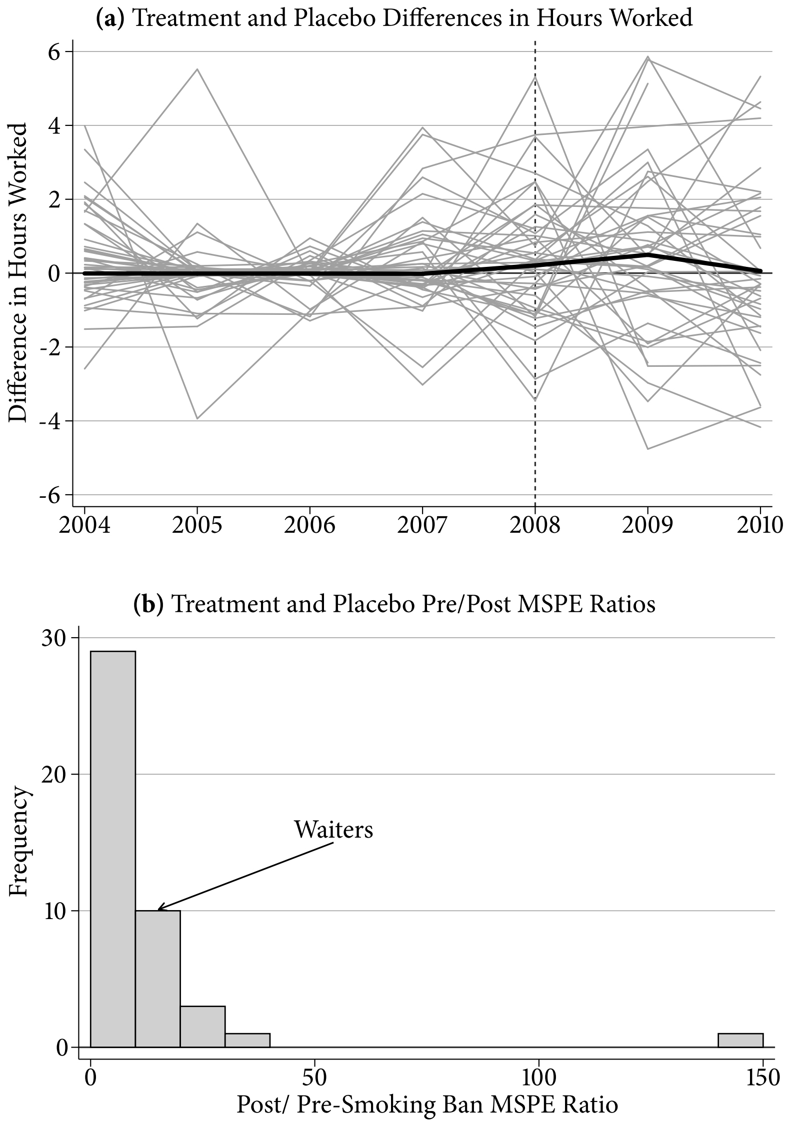


Figure A9: Synthetic Control Inference Graphs
(Only States with Ban Introduction in 2008)

Notes: This figure presents two approaches commonly used for inference in a synthetic control approach. Figure A9a shows the result of a placebo exercise in which all occupations in the donor pool are iteratively assigned to be treated while waiters are moved into the control group. Figure A8b plots the post/pre-ratio of the mean squared prediction errors (MSPE). Occupations with pre-smoking ban MSPE ten times higher than that of waiters discarded. When including observations from only the 13 states that introduced smoking bans in 2008 and setting 2008 to be the first treatment year, the post/pre-ratio of MSPEs indicates that no other control state achieves such a large ratio as the group of waiters implying that the hours worked significantly increases for waiters in comparison to a synthetic control group in the period after the introduction of smoking bans.
Source: Author's calculations based on the German Microcensus.

Table A1: Smoking Behavior among the Population and Waiters

	2005			2009		
	(1) Population	(2) Waiters	(3) Waiters (Mini Jobs)	(4) Population	(5) Waiters	(6) Waiters (Mini Jobs)
How Often do you Smoke?						
Regularly	29.7	43.3	42.4	29.6	39.2	38.5
Sometimes	4.8	5.6	8.1	4.8	6.0	6.9
Never	65.5	51.1	49.5	65.6	54.8	54.6
Observations	140,513	1,919	428	188,809	2,207	503
How many Cigarettes do you Smoke per Day? (if Smoking)						
1 to 5	13.9	10.6	14.1	14.3	12.7	16.5
5 to 20	70.8	72.9	75.3	72.7	74.2	75.0
12 to 40	14.4	15.3	10.2	12.3	12.4	7.1
41 and more	0.9	1.3	0.4	0.7	0.6	1.3
Observations	45,594	888	209	61,303	962	213

Note: This table shows descriptive statics regarding the smoking behavior of the general population, waiters, and waiters in mini jobs in 2005 and 2009. The sample is restricted to individuals aged 17-62 not in civil service (*Beamte*). Waiters are defined as those working in occupation groups 911 and 912. Mini job holders are those indicating that their main current job is a mini job. The questions regarding smoking behavior are not compulsory in the Microcensus. Statistics are weighted by survey weights.
Source: Author's calculations based on the German Microcensus.

Table A2: Initial Smoking Ban Regulations in German States (until August 2008)

State	Ban Introduction (Legal)	Ban Introduction (Enforced) *if different*	(i) Separate Smoking Room Allowed? (Restaurants & Bars) SR1	(ii) Separate Smoking Room Allowed? (Dancing Clubs) SR2	(iii) Exception for Small Bars? SB	(iv) Exception for Party Tents? PT	**Intensity** (Baseline)
BB	2008 - 01	2008 - 07	✓				0.67
BE	2008 - 01	2008 - 07	✓				0.67
BW	2007 - 08		✓			✓	0.665
BY	2008 - 01					[a]	0.995
HB	2008 - 01	2008 - 07	✓	✓		✓	0.65
HE	2007 - 10		✓	✓		✓	0.65
HH	2008 - 01		✓	✓		✓	0.65
MV	2008 - 01	2008 - 08	✓	✓			0.505
NI	2007 - 08	2007 - 11	✓	✓			0.655
NW	2008 - 07		✓	✓			0.655
RP	2008 - 02		✓	✓	✓	✓	0.50
SH	2008 - 01		✓	✓	✓	✓	0.65
SL	2008 - 02	2008 - 06	✓	✓			0.655
SN	2008 - 02		✓		✓	✓	0.665
ST	2008 - 01	2008 - 07	✓			✓	0.665
TH	2008 - 07		✓	✓		✓	0.65

Note: Ticks in light gray indicate that exception was only granted for owner-operated bars without employees and thus was not considered in the empirical analysis. [a]Bavaria granted an exception to the smoking ban in party tents from January - December 2008, the intensity index value during this period for Bavaria is thus 0.995.
Source: Respective state laws from *beck-online*, Ahlfeldt and Maennig (2010, 516ff, table A.1)

Table A3: Index Weights used to Construct the Intensity Index

Type	Employees	WZ 2008	Weight ω
Restaurants & Bars, large (LB)[a]	567,900	56.1, 56.301, 56.303, 56.304, 56.309	0.66
Dancing Clubs (DC)	26,982	56.302	0.03
Restaurants & Bars, small (SB)[b]	250,428	56.1, 56.301, 56.303, 56.304, 56.309	0.30
Party Tents (PT)[c]	11,590	56.1, 56.301, 56.303, 56.304, 56.309	0.01
Total	856,900	56.1, 56.3	1.00
Other Food Services	91,132	56.2	–
Accomodation	408,599	55	–
Total Hospitality Industry	1,356,631	55, 56	–

Note: [a] 6 or more employees. [b] up to 5 employees. [c] estimated as 1% of employees in large restaurant and bars.
Source: Data refer to the year 2007 and are taken from the Yearly Statistics in the Hospitality Industry (*Jahresstatistik im Gastgewerbe*) published by the Federal Statistical Office (Statistisches Bundesamt 2011).

Table A4: DD Regression Models: Individual Intensity Index Components

	Dependent Variable: *Log Wage*				
	(1)	(2)	(3)	(4)	(5)
Ban in Side Rooms[a]	-0.024				-0.022
	(0.005)				(0.006)
Ban in Small Pubs[b]		-0.013			-0.011
		(0.004)			(0.005)
Ban in Party Tents			-0.000		0.001
			(0.009)		(0.010)
Ban in Side Room (Dancing Clubs)				-0.012	0.007
				(0.007)	(0.004)
Worker, Time, State FEs	✓	✓	✓	✓	✓
State-Month FEs	✓	✓	✓	✓	✓
Extended DD Controls	✓	✓	✓	✓	✓
Start	Aug 2006	Aug 2006	Aug 2006	Aug 2006	Aug 2006
End	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009
Clusters	16	16	16	16	16
Individuals	13,366	13,366	13,366	13,366	13,366
Observations	153,840	153,840	153,840	153,840	153,840
Adj. R^2	0.868	0.868	0.868	0.868	0.868

Note: Sample restricted to waiters in mini jobs working in the hospitality sector. The unit of observation is a worker and time is running in monthly intervals. The set of extended DD controls include state specific linear pre-trends as well as the current and six lags of the monthly state unemployment rate. Standard errors clustered at the state level.[a] in larger restaurants and pubs larger than $75m^2$. [b] up to $75m^2$.
Source: Author's calculations based on IAB earnings data.

Table A5: DDD Regression Models (Cooks): Individual Intensity Index Components

	Dependent Variable: *Log Wage*				
	(1)	(2)	(3)	(4)	(5)
Ban in Side Rooms[a] × Waiters	-0.039				-0.035
	(0.007)				(0.008)
Ban in Side Rooms[a]	0.013				0.010
	(0.006)				(0.008)
Ban in Small Pubs[b] × Waiters		-0.015			-0.001
		(0.008)			(0.009)
Ban in Small Pubs[b]		0.004			-0.004
		(0.005)			(0.007)
Ban in Party Tents × Waiters			-0.007		-0.008
			(0.008)		(0.010)
Ban in Party Tents			0.008		0.008
			(0.007)		(0.008)
Ban in Side Room × Waiters (Dancing Clubs)				-0.019	-0.003
				(0.010)	(0.008)
Ban in Side Room (Dancing Clubs)				0.008	0.007
				(0.004)	(0.006)
Worker, Occupation-State, Occupation-Time FEs	✓	✓	✓	✓	✓
State-Month FEs	✓	✓	✓	✓	✓
Extended DDD Controls	✓	✓	✓	✓	✓
Start	Aug 2006	Aug 2006	Aug 2006	Aug 2006	Aug 2006
End	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009
Clusters	16	16	16	16	16
Individuals	19,716	19,716	19,716	19,716	19,716
Observations	229,433	229,433	229,433	229,433	229,433
Adj. R^2	0.869	0.869	0.869	0.869	0.869

Note: Sample restricted to waiters and cooks in mini jobs working in the hospitality sector. The unit of observation is a worker and time is running in monthly intervals. The set of extended DDD controls include state-occupation specific linear pre-trends as well as the current and six lags of the monthly state unemployment rate. Standard errors clustered at the state level. [a]in larger restaurants and pubs larger than 75m^2. [b]up to 75m^2.
Source: Author's calculations based on IAB earnings data.

Table A6: DDD Regression Models (All Other Mini Job Workers): Ban Indicators

	Dependent Variable: *Log Wage*				
	(1)	(2)	(3)	(4)	(5)
Ban in Side Rooms[a] × Waiters	-0.034				-0.038
	(0.005)				(0.005)
Ban in Side Rooms[a]	0.008				0.013
	(0.004)				(0.004)
Ban in Small Pubs[b] × Waiters		-0.012			-0.004
		(0.007)			(0.004)
Ban in Small Pubs[b]		0.000			-0.001
		(0.003)			(0.005)
Ban in Party Tents × Waiters			-0.007		-0.007
			(0.007)		(0.007)
Ban in Party Tents			0.005		0.004
			(0.008)		(0.009)
Ban in Side Room × Waiters (Dancing Clubs)				-0.013	0.006
				(0.010)	(0.004)
Ban in Side Room (Dancing Clubs)				-0.000	-0.005
				(0.004)	(0.004)
Worker, Occupation-State, Occupation-Time FEs	✓	✓	✓	✓	✓
State-Month FEs	✓	✓	✓	✓	✓
Extended DDD Controls	✓	✓	✓	✓	✓
Start	Aug 2006	Aug 2006	Aug 2006	Aug 2006	Aug 2006
End	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009
Clusters	16	16	16	16	16
Individuals	28,393	28,393	28,393	28,393	28,393
Observations	342,854	342,854	342,854	342,854	342,854
Adj. R^2	0.873	0.873	0.873	0.873	0.873

Note: Sample restricted to mini job workers working in the hospitality sector. The unit of observation is a worker and time is running in monthly intervals. The set of extended DDD controls include state-occupation specific linear pre-trends as well as the current and six lags of the monthly state unemployment rate. Standard errors clustered at the state level. [a]in larger restaurants and pubs larger than 75m^2. [b]up to 75m^2.
Source: Author's calculations based on IAB earnings data.

Table A7: Potential Determinants of the Introduction Time of a State's Smoking Ban

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Ban Intensity	-1.345										77.784
	(1.169)										(55.268)
Share Smokers in 2005 (%)		0.122									-3.682
		(0.191)									(3.810)
Share Foreign Tourists			-4.543								-133.606
			(8.884)								(139.689)
Months to Election				-0.004							-0.190
				(0.049)							(0.111)
ln(Population)					-0.411						-7.389
					(1.121)						(11.201)
Conservative Index						-1.244					-19.822
						(1.019)					(20.673)
Trend Unemployment Rate 2005-07							-0.310				3.799
							(0.294)				(2.940)
Trend Hospitality Wages 2005-07								0.923			-16.773
								(1.295)			(23.014)
Trend Bar Revenues 2005-07									0.547		9.352
									(1.283)		(11.278)
Trend Restaurant Revenues 2005-07										1.220	5.244
										(1.362)	(4.950)
Observations	16	16	16	16	16	16	16	16	14	14	14
R^2	0.002	0.013	0.015	0.000	0.016	0.039	0.059	0.008	0.020	0.063	0.557
Adj. R^2	-0.069	-0.057	-0.055	-0.071	-0.055	-0.029	-0.008	-0.063	-0.062	-0.015	-0.918

Note: This table shows correlations between potential determinants of the introduction date of a state's smoking ban. The dependent variable is the introduction time of a state's smoking ban (measured in Stata's monthly date format, e.g. 571 refers to August 2008). The ban intensity refers to the intensity of the smoking ban in the month it first became effective. The conservative index is defined as the vote shares of CDU/ CSU and FDP over the the shares of SPD, Greens and the Left. The trend variables refer to coefficient from a regression of the state level unemployment rate, the revenues of bars, restaurants, and in the unemployment rate, respectively, on time. Robust standard errors in parentheses.
Source: Author's calculations based on respective state laws, German Microcensus 2005, Federal Statistical Office, Ahlfeldt and Maennig (2010).

Table A8: Potential Determinants of the Intensity of a State's Smoking Ban

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Ban Intro Date	-0.002										0.006
	(0.001)										(0.002)
Share Smokers in 2005 (%)		-0.014									0.040
		(0.011)									(0.025)
Share Foreign Tourists			0.270								1.419
			(0.296)								(0.975)
Months to Election				-0.003							0.002
				(0.002)							(0.001)
ln(Population)					0.044						0.079
					(0.036)						(0.082)
Conservative Index						0.145					0.262
						(0.073)					(0.110)
Trend Unemployment Rate 2005-07							-0.001				-0.046
							(0.011)				(0.017)
Trend Hospitality Wages 2005-07								0.001			0.188
								(0.038)			(0.158)
Trend Bar Revenues 2005-07									-0.024		-0.103
									(0.038)		(0.083)
Trend Restaurant Revenues 2005-07										-0.051	-0.045
										(0.044)	(0.041)
Observations	16	16	16	16	16	16	16	16	14	14	14
R^2	0.002	0.150	0.043	0.161	0.145	0.439	0.001	0.000	0.031	0.090	0.973
Adj. R^2	-0.069	0.090	-0.025	0.101	0.083	0.399	-0.071	-0.071	-0.049	0.014	0.884

Note: This table shows correlations between potential determinants of the intensity of a state's smoking ban. The dependent variable is the intensity of a state's smoking ban at the month it first become effective. The conservative index is defined as the vote shares of CDU/ CSU and FDP over the the shares of SPD, Greens and the Left. The trend variables refer to coefficient from a regression of the state level unemployment rate, the revenues of bars, restaurants, and in the unemployment rate, respectively, on time. Robust standard errors in parentheses.
Source: Author's calculations based on respective state laws, German Microcensus 2005, Federal Statistical Office, Ahlfeldt and Maennig (2010).

Table A9: Summary Statistics of State Level Data

	Mean	SD	Min	Max
Monthly				
Unemployment Rate (in %)	11.5	[4.23]	4.10	21.2
Revenue Index Restaurants (2005=100)	103.4	[20.6]	57.3	173.7
Revenue Index Bars (2005=100)	91.2	[21.5]	45.1	174.2
Share of Foreign Arrivals (in %)[a]	16.1	[8.13]	2.84	39.1
Temperature (in Degrees Celsuis)	9.46	[6.03]	-3.90	19.1
Rain Amount (in l/m^2)	67.9	[33.9]	1.10	179.2
Sunshine Hours	127.7	[75.5]	18.3	351.3
Yearly				
Population (in Millions)	5.13	[4.70]	0.66	18.0
Share of Smokers in 2005 (in %)	28.5	[2.75]	24.5	33.7
With Election Cycles				
Turnout in State-Level Elections (in %)	58.6	[5.62]	44.4	70.6
Conservative Index	1.05	[0.44]	0.36	2.31

Note: This table presents summary statistics of state level data between August 2006 and February 2009. Standard deviation in brackets. [a]Data not available for Berlin and Brandenburg. [b]Share of registrations of tourists of foreign nationality in all touristic registrations at accommodation establishments.
Source: GENESIS online, German Microcensus, German Weather Service (DWD).

Table A10: *p*-Value Results from Alternative Inference Methods

	(1) Ban vs. No Ban Indicator	(2) Smoking Ban Intensity Index
$\widehat{\beta}$	-0.013	-0.024
p-values:		
1. Analytical (clustered at state level)[1]	0.0038	0.0006
2. Wild Cluster Bootstrap (clustered at state level)[2]	0.0015	0.0010
3. Permutation based (shuffling policies across states)[3]	0.0080	0.0028

Notes: [1]Based on analytically derived standard errors and *t*-values evaluated against a Student-*t* distribution with 15 (16 states-1) degrees of freedom.
[2]Wild cluster (at the state level) bootstrap following Colin Cameron et al. (2008) with the null hypothesis imposed ($\beta = 0$), using Rademacher weights and 65,536 repetitions (2^{16} = the universe of Rademacher weights). The *p*-value is calculated as the two-tailed symmetric *p*-value following the suggestions in Roodman et al. (2018) and implemented via their `boottest` command in Stata.
[3]Two-tailed symmetric *p*-value based on 10,000 permutation placebos coefficients resulting from randomly shuffling smoking ban policies across states (without replacement) using a specification with extended controls.
Source: Author's calculations based on IAB earnings data.

Table A11: Selected Occupation Groups of Mini Job Workers in the Hospitality Sector

Occupation Group (KldB 1988)	Observations	Percent
40 Cooks until ready-to-serve meals, fruit, vegetable preservers, preparers	75,810	21.9
56 Unskilled laborer/ assistants (no further specification)	6,860	2.0
73 Salespersons	20,380	5.9
81 Motor vehicle drivers	8,904	2.6
86 Stowers, furniture packers until stores/transport workers	2,333	0.7
93 Office specialists	5,924	1.7
97 Doormen, caretakers until domestic and non-domestic servants	4,378	1.3
116 Others attending on guests (non-waiters, e.g. event management,...)	32,272	9.3
117 Housekeeping managers until employees by household cheque procedure	10,852	3.1
119 Household cleaners until glass, buildings cleaners	23,006	6.6
115 Restaurant, inn, bar keepers, hotel proprietors, catering trade dealers until waiters, stewards	**155,561**	**44.9**
Total	346,280.0	100.0

Note: Occupation group identifiers and labels refer to the classification of occupations (version 1988). Occupations groups required to have at least 20 observations per state.
Source: Author's calculations based on IAB earnings data.

Table A12: Summary Statistics of DDD Occupation Groups

	(1) Waiters		(2) Cooks		(3) All Other Mini Job Workers	
Panel A: All Workers						
Full Time (share)	0.34	[0.47]	0.46	[0.50]	0.34	[0.47]
Regular Part Time (share)	0.087	[0.28]	0.16	[0.37]	0.13	[0.34]
Mini Job (share)	0.58	[0.49]	0.38	[0.48]	0.53	[0.50]
Panel B: Mini Job Workers						
Real Monthly Earnings (in 2010 euros)	238.2	[162.9]	275.5	[188.5]	268.2	[194.9]
Low Skilled (share)	0.33	[0.47]	0.41	[0.49]	0.35	[0.48]
Medium Skilled (share)	0.64	[0.48]	0.57	[0.50]	0.63	[0.48]
High Skilled (share)	0.036	[0.19]	0.019	[0.14]	0.028	[0.17]
Age (in years)	33.0	[11.5]	36.3	[12.2]	37.4	[12.4]
Female (share)	0.76	[0.43]	0.63	[0.48]	0.69	[0.46]
German (share)	0.85	[0.36]	0.70	[0.46]	0.81	[0.39]
East German (share)	0.11	[0.31]	0.15	[0.35]	0.14	[0.35]
Usual Weekly Hours Worked (Microcensus)	12.3		14.1		11.9	

Note: This table presents summary statistics of individual earnings data and Microcensus data (hours). Standard deviation in brackets. The sample is restricted to individuals aged 17-62 years, employed in the hospitality sector between August 2006 and February 2009 (not restricted to the hospitality industry in case of the usual hours worked taken from the Microcensus). Real euro values are deflated to 2010 using the consumer price index of the German Bundesbank. Censored earnings are imputed following Gartner (2005).
Source: Author's calculations based on IAB earnings data and Microcensus.

Table A13: Leave one State out at a Time

	Intensity
Schleswig-Holstein	-0.023**
	(0.006)
Hamburg	-0.025***
	(0.005)
Niedersachsen	-0.025**
	(0.006)
Bremen	-0.024***
	(0.006)
NRW	-0.028***
	(0.006)
Hessen	-0.026**
	(0.007)
Rheinland-Pfalz	-0.024**
	(0.006)
Baden-Wuerttemberg	-0.027***
	(0.006)
Bayern	-0.019**
	(0.006)
Saarland	-0.022***
	(0.005)
Mecklenburg-Vorpommern	-0.024***
	(0.006)
Sachsen	-0.024**
	(0.006)
Sachsen-Anhalt	-0.024**
	(0.006)
Thueringen	-0.025***
	(0.006)
Berlin	-0.024***
	(0.006)
Brandenburg	-0.023**
	(0.006)

Note: All regressions replicate the baseline specification using extended controls but leave out observations from the state indicated in the corresponding row. The sample is restricted to waiters in mini job in the hospitality sector. Standard errors are clustered at the state-level.
Source: Author's calculations based on IAB earnings data.

Table A14: Impact of Smoking Bans on Revenues of Restaurants and Bars (*Robustness Checks*)

	Dependent Variable: *Log Real Revenue Index*			
	(1) Baseline Full Controls	(2) No State CPI	(3) No State CPI + HH, SH	(4) Germany's CPI for HH, SH[a]
Panel A: Restaurants				
Ban Intensity	0.051	0.052	0.047	0.046
	(0.056)	(0.056)	(0.055)	(0.056)
Adj. R^2	0.853	0.850	0.864	0.866
Panel B: Bars				
Ban Intensity	0.056	0.057	0.053	0.052
	(0.069)	(0.068)	(0.065)	(0.066)
Adj. R^2	0.804	0.802	0.814	0.815
State & Time FEs	✓	✓	✓	✓
Unemp. Rate	✓	✓	✓	✓
State CPI	✓			✓
Linear State Trends	✓	✓	✓	✓
State-Month FEs	✓	✓	✓	✓
Weather Controls	✓	✓	✓	✓
Index of Domestic and Foreign Overnight Stays	✓	✓	✓	✓
Start	Jan 2005	Jan 2005	Jan 2005	Jan 2005
End	Feb 2009	Feb 2009	Feb 2009	Feb 2009
Clusters	12	12	14	14
Observations	576	576	676	676

Note: This table presents regressions of the monthly state-level log real revenues index of restaurants (panel A) and bars (panel B) on the smoking ban intensity index and further controls. All controls vary at the state-month level. Weather controls include the monthly state mean temperature, rain amount, and hours of sunshine. CPI refers to the monthly state consumer price index. The index of domestic and foreign overnights stays refers to the number of overnights stays by tourists of domestic or foreign origin. [a]Hamburg and Schleswig-Holstein are assigned the CPI of Germany since these two states do not report their own state-specific CPI. Standard errors clustered at the state level. All regressions are weighted by population size.
Source: Author's calculations based on Ahlfeldt and Maennig (2010).

Table A15: DD Regression of the Impact of Smoking Bans on Hours Worked

	(1) Simple DD	(2) + Ind. Controls	(3) + Trends	(4) + State-Quarter FEs
Panel A: Ban vs. No Ban Indicator				
Smoking Ban Indicator	0.064	0.091	0.083	0.063
	(0.044)	(0.035)	(0.031)	(0.027)
Adj. R^2	0.012	0.109	0.108	0.098
Panel B: Smoking Ban Intensity Index				
Ban Intensity	0.042	0.056	0.108	0.051
	(0.018)	(0.047)	(0.035)	(0.071)
Adj. R^2	0.012	0.109	0.108	0.098
Time, State Time FEs	✓	✓	✓	✓
State Level Controls Z	✓	✓	✓	✓
Individual Controls X		✓	✓	✓
State Specific Linear Trends			✓	✓
State-Quarter FEs				✓
Start	2005	2005	2005	2005
End	2009	2009	2009	2009
Cluster	6	6	6	6
Observations (Individuals)	1,483	1,483	1,483	1,483

Note: This table shows regression results of the impact of smoking bans on the log usual hours worked per week. The sample is restricted to individuals in mini-jobs. The unit of observation is a worker and time is running in quarterly intervals. Time refers to the running time variable, quarter to one of the four quarters of any year. The set of individual controls X include dummy variables for being female, having a partner, having children under 18 years of age in the household, having a German citizenship, and whether the main source of income is from own work (as opposed to transfers or capital income), dummies for each of eight age categories, nine city size categories, three education categories, five categories referring to the years passed since migrated to Germany, and five household size categories along with tenure and tenure squared at the current employer. Regressions weighted by survey weights. Standard errors clustered at the state level.
Source: Author's calculations based on the German Microcensus.

Table A16: Hours worked:
assessing the bias from unobservables following the approach by Oster (2019)

	DD Table A15		DDD Table 6	
Panel A: Ban vs. No Ban Indicator				
$\hat{\beta}^R$	0.064^a		0.097^a	
$\hat{\beta}^F$	0.091^b	0.063^c	0.095^b	$0{,}102^c$
Ratio	3.4	63.0	47.5	20.4
Panel B: Smoking Ban Intensity Index				
$\hat{\beta}^R$	0.042^a		0.075^a	
$\hat{\beta}^F$	0.056^b	0.051^c	0.078^b	$0{,}099^c$
Ratio	3.0	4.7	25.0	3.1
Median Ratio	4.02		22.7	

Note: Own calculations based on the estimates of Table A15 for the DD estimates and Table 6 for the DDD estimates of the impact of smoking bans on the usual hours worked. [a] column 1 of respective table, [b] column 2, [c] column 4. $\hat{\beta}^R$ refers to the coefficient from the restricted regression and $\hat{\beta}^F$ to the coefficient from the regression using the full set of controls. The ratio is then calculated as $\mid \frac{\hat{\beta}^F}{\hat{\beta}^R - \hat{\beta}^F} \mid$

Table A17: Turnover and Employment Effects of Smoking Bans

	Probability to ...a Job (Individual Level Data)			Employment (State Level Data)	
	(1) Start or End	(2) Start	(3) End	(4) ln(Months Worked)	(5) ln(Turnover)
Panel A: Ban vs. No Ban Indicator					
Smoking Ban Indicator	-0.001	-0.002	0.001	-0.009	0.005
	(0.004)	(0.003)	(0.003)	(0.012)	(0.019)
Adj. R^2	0.217	0.113	0.119	0.997	0.970
Panel B: Smoking Ban Intensity Index					
Ban Intensity	0.002	0.005	-0.003	-0.006	0.041
	(0.004)	(0.005)	(0.004)	(0.015)	(0.028)
Adj. R^2	0.217	0.113	0.119	0.997	0.970
Worker FEs	✓	✓	✓		
Time, State FEs	✓	✓	✓	✓	✓
State-Month FEs	✓	✓	✓	✓	✓
Extended DD Controls	✓	✓	✓	✓	✓
Start	Aug 2004	Aug 2004	Aug 2004	Aug 2004	Aug 2004
End	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009
Clusters	16	16	16	16	16
Individuals	18,711	18,711	18,711		
Observations	264,548	264,548	264,548	880	880

Note: This table shows regression results of the impact of smoking bans on various employment outcomes of waiters in mini jobs working in the hospitality sector. The unit of observation in columns 1-3 is a worker and in columns 4-5 these are aggregated at the state-month level. Time is running in monthly intervals. ln(Months Worked) is defined as the natural logarithm of the number of (person-month) spells in a given state-month cell +1. ln(Turnover) is defined as the total number of spells starting and ending in a given state-month cell +1. State level regressions are weighted by the number of underlying observations from with the data was aggregated. Standard errors clustered at the state level. The set of extended DD controls include state specific linear pre-trends specific to each estimation sample as well as the current and six lags of the monthly state unemployment rate. Standard errors clustered at the state level.
Source: Author's calculations based on IAB earnings data.

Table A18: MZ DD Income

	(1) Simple DD	(2) + Ind. Controls	(3) + Trends	(4) + State-Quarter FEs
Panel A: Ban vs. No Ban Indicator				
Smoking Ban Indicator	0.003	0.017	0.005	-0.042
	(0.057)	(0.040)	(0.037)	(0.072)
Adj. R^2	0.014	0.211	0.210	0.203
Panel B: Smoking Ban Intensity Index				
Ban Intensity	-0.065	-0.060	-0.052	-0.117
	(0.043)	(0.040)	(0.030)	(0.079)
Adj. R^2	0.014	0.211	0.210	0.203
Time, State Time FEs	✓	✓	✓	✓
State Level Controls Z	✓	✓	✓	✓
Individual Controls X		✓	✓	✓
State Specific Linear Trends			✓	✓
State-Quarter FEs				✓
Start	2005	2005	2005	2005
End	2009	2009	2009	2009
Cluster	6	6	6	6
Observations (Individuals)	1,427	1,427	1,427	1,427

Note: This table shows regression results of the impact of smoking bans on the log real net household income. Income is measured in intervals and set to the midpoint of a given income bracket. The sample is restricted to individuals in mini-jobs. The unit of observation is a worker and time is running in quarterly intervals. Time refers to the running time variable, quarter to one of the four quarters of any year. The set of individual controls X include dummy variables for being female, having a partner, having children under 18 years of age in the household, having a German citizenship, and whether the main source of income is from own work (as opposed to transfers or capital income), dummies for each of eight age categories, nine city size categories, three education categories, five categories referring to the years passed since migrated to Germany, and five household size categories along with tenure and tenure squared at the current employer. Regressions weighted by survey weights. Standard errors clustered at the state level.
Source: Author's calculations based on the German Microcensus.

Table A19: MZ DDD Income

	(1) Simple DDD	(2) + Ind. Controls	(3) + Trends	(4) + Occupation -Quarter FEs
Panel A: Ban vs. No Ban Indicator				
Smoking Ban × Waiters Indicator	0.040	0.023	0.002	-0.039
	(0.069)	(0.037)	(0.044)	(0.055)
Smoking Ban Indicator	-0.008	-0.017	-0.018	-0.018
	(0.012)	(0.008)	(0.006)	(0.019)
Adj. R^2	0.044	0.197	0.197	0.192
Panel B: Smoking Ban Intensity Index				
Ban Intensity × Waiters	-0.049	-0.021	-0.021	-0.081
	(0.054)	(0.031)	(0.027)	(0.052)
Ban Intensity	0.009	-0.008	-0.026	-0.026
	(0.020)	(0.013)	(0.008)	(0.029)
Adj. R^2	0.044	0.197	0.197	0.192
Occupation-State, Occupation Time FEs	✓	✓	✓	✓
State Level Controls Z	✓	✓	✓	✓
Individual Controls X		✓	✓	✓
Occupation-State Specific Linear Trends			✓	✓
Occupation-State -Quarter FEs				✓
Start	2005	2005	2005	2005
End	2009	2009	2009	2009
Cluster	15	15	15	15
Observations (Individuals)	39,768	39,768	39,768	39,768

Note: This table shows regression results of the impact of smoking bans on the log real net household income. Income is measured in intervals and set to the midpoint of a given income bracket. The sample is restricted to individuals in mini-jobs. The unit of observation is a worker and time is running in quarterly intervals. Time refers to the running time variable, quarter to one of the four quarters of any year. The set of individual controls X include dummy variables for being female, having a partner, having children under 18 years of age in the household, having a German citizenship, and whether the main source of income is from own work (as opposed to transfers or capital income), dummies for each of eight age categories, nine city size categories, three education categories, five categories referring to the years passed since migrated to Germany, and five household size categories along with tenure and tenure squared at the current employer. Regressions weighted by survey weights. Standard errors clustered at the state level.
Source: Author's calculations based on the German Microcensus.

Table A20: Weights in Synthetic Control Approach

Occupation Group	Description	Synth Weight (All)	Synth Weight (Only 2008)
1	Landwirtschaftliche Berufe	0	0
2	Tierwirtschaftliche Berufe	0	0
5	Gartenbauberufe	0	0
17	Druck- und Druckweiterverarbeitungsberufe	0	0
25	Metall- und Anlagenbauberufe	0	
26	Blechkonstruktions- und Installationsberufe	0	0
27	Maschinenbau- und -wartungsberufe	0	
28	Fahr-, Flugzeugbau- und -wartungsberufe	0	0
30	Feinwerktechnische und verwandte Berufe	0	0
31	Elektroberufe	0	0
32	Montierer/Montiererinnen und Metallberufe, a.n.g.	0	0
35	Berufe in der Textilverarbeitung	0	0
39	Berufe in der Back-, Konditor-, Süßwarenherstellung	0	0
41	Köche/Köchinnen	0	0
44	Hochbauberufe	0	0
47	Bauhilfsarbeiter	0	0
48	Ausbauberufe	0	0
50	Berufe in der Holz- und Kunststoffverarbeitung	0	0
51	Maler/Malerinnen, Lackierer/Lackiererinnen und verwandte Berufe	0	
52	Warenprüfer/Warenprüferinnen, Versandfertigmacher/Versandfertigmacherinnen	0	0
53	Hilfsarbeiter/Hilfsarbeiterinnen ohne nähere Tätigkeitsangabe	0	0
60	Ingenieure/Ingenieurinnen, a.n.g.	0	0
62	Techniker/Technikerinnen, a.n.g.	0	0
66	Verkaufspersonal	0	0
67	Groß- und Einzelhandelskaufleute, Ein- und Verkaufsfachleute	0	0
68	Warenkaufleute, a.n.g., Vertreter/Vertreterinnen	0	0
69	Bank-, Bausparkassen-, Versicherungsfachleute	0	0
70	Andere Dienstleistungskaufleute und zugehörige Berufe	0	0
71	Berufe des Landverkehrs	0	0
73	Berufe des Nachrichtenverkehrs	0	0
74	Lagerverwalter/Lagerverwalterinnen, Lager-, Transportarbeiter und -arbeiterinnen	0	0
75	Berufe in der Unternehmensleitung, -beratung und -prüfung	0	0
77	Rechnungskaufleute, Informatiker/Informatikerinnen	.497	.649
78	Büroberufe, Kaufmännische Angestellte, a.n.g.	0	0
79	Dienst-, Wachberufe	0	0
82	Publizistische, Übersetzungs-, Bibliotheks- und verwandte Berufe	0	0
83	Künstlerische und zugeordnete Berufe	.053	.066
84	Ärzte/Ärztinnen, Apotheker/Apothekerinnen	0	0
85	Übrige Gesundheitsdienstberufe	.359	.213
86	Soziale Berufe	0	0
87	Lehrer/Lehrerinnen	0	.064
88	Geistes- und naturwissenschaftliche Berufe, a.n.g.	0	0
89	Berufe in der Seelsorge	0	
90	Berufe in der Körperpflege	0	0
92	Haus- und ernährungswirtschaftliche Berufe	0	0
93	Reinigungs- und Entsorgungsberufe	0	0
99	Arbeitskräfte ohne nähere Tätigkeitsangabe	0	.009
100	Sonstige Berufe in der Gästebetreuung	.092	0

Note: This table provides the weights attached to each occupation group in the donor pool used in the synthetic controls approaches based on a sample that includes all stated (column 1) or only those which introduced smoking bans in 2008 (column 2).

A. Sample Restrictions and Data Preparation

B1. IAB Earnings data

Sample Construction: Following common practice when working with the IAB earnings data, I drop spells with missing location information (after imputation, see below), spells of doctors and pharmacists (due to corrupted and missing records, see vom Berge et al. 2013), spells that last only one day, spells with statuses "seeking for employment but not registered unemployed", "without status", and "seeking advice", zero daily earnings spells, spells with missing employment status, full-time spells with daily earnings below the marginal earnings threshold, unemployment spells that overlap with non-unemployment spells and unemployment spells that overlap with other unemployment spells (and keep only one of them).

Daily Earnings: I impute censored earnings above the upper earnings threshold for compulsory social insurance (66,000 euros per year in 2010) using the "no heteroskedasticity" approach by Gartner (2005) and Dustmann et al. (2009). Specifically, I consider earnings as censored that were up to two euros below the maximum earnings value observed in each year and then estimate for each year and for males and females separately a censored regression of log daily earnings on indicators of eight age groups, three skill groups and all their possible interactions, assuming that the error term is normally distributed and has the same variance across age and skill groups.

Education: I impute missing education information following Fitzenberger et al. (2006) and group individuals in three categories (low, medium, and high). Low comprises those with at most a *Realschule* degree, missing education, and those who have not completed any vocational training, Abitur, or a tertiary degree. Medium contains those with vocational training or Abitur. High refers to all those with a completed tertiary degree (*Fachhochschule* or *Universität*).

Location: If missing, location information is imputed with the last non-missing location.

Tenure: For each individual, the number of months at the same employer as observed from his/ her IAB labor market biography are summed up (potentially since 1985).

Experience in Hospitality Industry: For each individual, the number of months in the hospitality sector as observed from his/ her IAB labor market biography are summed up (potentially since 1985).

B2. Microcensus Data

Sample Construction: I restrict the sample to individuals interviewed at their main place of residence (to avoid double counting) living in private households (as opposed to community accommodations such as prisons), the years 2004 to 2010, to workers between 17 and 62 years of age who are not civil servants (*Beamte*) or self-employed and with their main or first job being a mini job. I then set the time variable of an observation to the quarter when the Microcensus was conducted. Finally, I restrict the sample to occupation-state-time cells with at least 15 observations across the sample period (balanced panel needed for the synthetic control approaches).

Occupation Groups: To ensure sufficiently large cells, occupations are aggregated from three-digit to two-digit level occupation groups according to the classification of occupations (*Klassifikation der Berufe*) version 1992.

Individual and Household Income: Income variables are set to the mean of the nominal income bracket in a given Microcensus wave and are then deflated to real net incomes.

Other Variables: Other variables used as controls include dummies for being female, having a partner, having children under 18 years of age in the household, having a German citizenship, and whether the main source of income is from own work (as opposed to transfers or capital income), dummies for each of eight age categories, nine city size categories, three education categories defined as in the IAB earnings data, five categories referring to the years passed since migrated to Germany, and five household size categories (1, 2, 3, 4, and 5 and more); tenure and tenure squared at the current employer.