The Costs of Unenforced Laws: A Field Experiment

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ABSTRACT

Unenforced laws are controversial. Admonished by some for undermining public respect for the law and violating the separation of powers, symbolic laws are supported by others because they can change people`s attitudes and behavior and set goals that society may one day achieve. In this article we identify a hidden cost of laws that the state does not intend or lacks resources to enforce. Based on evidence from a controlled field experiment with random treatment assignment involving public smoking bans, we show that right violations impose psychological costs on right holders even if they are indifferent about the behavior targeted by the law. Our results also shed new light on social enforcement.

1. INTRODUCTION

Not all laws are enforced equally.[1] Some laws remain unenforced because the executive branch purposely turns a blind eye to legal violations.[2] At other times, under-enforcement is fully anticipated by lawmakers. International treaties, for instance, often include social and economic rights without providing mechanisms to ensure their actual implementation or public enforcement. Many other laws, such as public smoking bans, littering laws and traffic regulations, are often too difficult or expensive to enforce comprehensively.

Legal scholars have long debated the wisdom of enacting laws that the state

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[1] Historical and current examples of laws that receive varying and sometimes very limited public enforcement attention include Prohibition-era alcohol regulation, gun control laws, seat belt laws, distracted driving laws, compulsory voting laws, certain anti-dumping laws, etc.

[2] For discussions of the legal and political implications of executive non-enforcement, see Price, 2013; Delahunty & Yoo, 2013. Scholars disagree whether the Executive branch's enforcement discretion is a legitimate policy instrument or instead violates the constitutional Take Care Clause, which provides that the President "shall take Care that the Laws be faithfully executed".

will not or cannot enforce. Unenforced laws are deemed problematic because non-compliance may undermine the rule of law. When individuals observe law breaking without repercussion, the perceived lawlessness may erode the authority of the law and public respect for legal rules in general.[3] Others claim that unenforced laws can serve valuable symbolic purposes by setting goals that society can aspire to (Harvey, 2004; Starr, 2008; Hurwitz, 2003). For instance, when international treaties declare universal living standards, they create legal rights that embody ideals society hopes to implement someday (Fallon, 2006; Kagan, 2007). A strand of scholarship has studied the interaction between laws and social norms, showing that symbolic laws may help set expectations about socially acceptable behavior (McAdams, 2015) or cause normative adherence to the rules expressed in laws and regulations (Cooter, 1996, 1998; Goldstein, 2006).[4]

Our article contributes to this debate by providing empirical evidence for a hereto unidentified *negative* effect of unenforced laws. We analyze how people respond to an unenforced public smoking ban in a controlled field experiment with random treatment assignment.[5] Our results show that when individuals are allocated a passive right, they react adversely to violations of this right even if they are indifferent to the behavior that causes the violation and unaffected by the material harm it may inflict. We conducted our study in a German bar because public smoking laws are scarcely enforced and often violated in Germany. We analyze the impact of non-enforcement on guests that experience violations of their right to a smoke free environment. The bar that we selected for our study utilizes an exception in German public smoking law that allows public establishments to permit smoking in one of their rooms. The bar has two rooms and strictly enforces the smoking prohibition in the main room, but alternates the smoking regime in the second room: sometimes the bar decides to use the legal exception and designates the second room as a smoking area. When the bar does not utilize the exception, the public smoking law applies and smoking is prohibited in the entire bar. Even at times when smoking is forbidden in the second room, the staff never enforces the ban there. As a result, guests in the second room regularly smoke in violation of the prohibition. With this regime the bar seeks to encourage smoke-tolerant guests to take a seat in the second room when the main room is crowded.

The bar gave us permission to conduct a field experiment and allowed us to randomly assign treatments in its rooms that build on the bar's own smoking regime. We implement two treatments in the second room *Smoking Allowed* & *Smoking Forbidden* and one *No Smoking* condition in the main room. In the *Smoking Allowed* condition we designate the second room as a smoking area (legal exception applies=smoking is allowed) while in the *Smoking Forbidden* treatment public law forbids smoking, but the prohibition remains unenforced. Finally, in the *No Smoking* condition we observe the behavior of non-smokers in the main room where smoking is forbidden and the ban is strictly obeyed. The treatments allow us to measure the effect a violation of their legal

[3] This intuition is captured in the words of Justice Brandeis in *Olmstead v. U.S.,* 277 U.S. 438, 485 (1928). "Our government teaches the whole people by its example. If the government becomes the lawbreaker, it breeds contempt for law; it invites every man to become a law unto himself; it invites anarchy."

[4] Empirical evidence includes Goodman, 2001 who reports on the basis of interviews that unenforced sodomy laws in South Africa created a climate of public and social surveillance and Bhattacharya & Daouk, 2009 finding that unenforced insider trading law increases the costs of equity in developing countries; Fisman and Miguel, 2007 finding corruption norm variance among diplomats in the context of unenforced parking laws.

[5] Although we use the term *bar*, the establishment best fits the profile of a European bistro or brasserie where guests visit not exclusively for drinking, but rather to spend time drinking refreshments, dining, working, and socializing with friends.

right not to be exposed to secondhand smoke has on non-smokers. In the *Smoking Forbidden* treatment smoking is banned but since the prohibition is never enforced, smokers violate the rights the law grants to non-smokers. In *Smoking Allowed* people also smoke but smoking is permitted, such that no rights of guests are violated. In *No Smoking* guests are granted the rights in the main room, but since the prohibition is always strictly enforced there, the rights are not infringed. To summarize, our treatments systematically vary whether 1) the public smoking prohibition is in place; 2) whether it is enforced; 3) whether the rights of guests are violated; and 4) whether they are exposed to smoke. Comparing the treatments enables us to show that guests adversely experience smoking when it violates their rights but not when smoking is allowed.

We measured as dependent variables a) the consumption of non-smokers and b) the duration of their stay. We hypothesize that non-smokers respond to the active violation of their right by leaving earlier and consuming less in the *Smoking Forbidden* treatment, even though they are exposed to less smoke than when smoking is allowed (assuming that some smokers will comply with the prohibition). We expect to observe a negative reaction to infringements in comparison to both other treatments: In *Smoking Allowed* guests are exposed to more intensive smoke but they are not granted rights that could be violated, because the prohibition is not in place; in the *No Smoking* condition guests are never subjected to either right violations or smoke.

The results support our main hypothesis: non-smokers leave the bar earlier and consume less in the *Smoking Forbidden* treatment, as compared to both the *Smoking Allowed* and the *No Smoking* conditions. Since the exposure to smoke is even stronger in *Smoking Allowed* than in *Smoking Forbidden*, the treatment comparison demonstrates that the non-smoking patrons react adversely not to secondhand smoke but to the violation of their rights. We conclude that the unenforced smoking ban imposes a cost on the non-smokers. This utility loss is psychological in nature since it relates to the experience of the violation of the right itself. It is distinct from the material harm that the infringing behavior inflicts, such as the odor and health risks associated with secondhand smoke – which all guests accepted who decided to sit in the second room.

As a robustness check, we used a particle counter to *directly* measure the concentration of smoke that individuals are exposed to. The device allows us to elicit and compare the concentration of smoke across treatments and to determine the individual dose of smoke each guest was exposed to during the time they spent in the room.[6] As expected, since some smokers complied with the prohibition and others reduced their smoking, the exposure to smoke was significantly lower in the *Smoking Forbidden* than in the *Smoking Allowed* treatment. Additionally, the individual level of smoke exposure had no explanatory power in our regressions for both outcome variables the time guest`s spent in the bar and their consumption. This direct evidence underlines that the adverse reaction to smoking we observe in the *Smoking Forbidden* treatment is not caused by smoke exposure.

We exclude that our findings are caused by a potential selection bias. In *Smoking Forbidden* guests may have expected that people would abstain from smoking. Under the *Smoking Allowed* treatment, by contrast, it may have been apparent that people will

[6] We are grateful to the occupation cooperative for hospitality (Berufsgenossenschaft Nahrungsmittel und Gastgewerbe) in Mannheim for providing us with the TSI 3700 free of charge and in particular Frank Thiele who instructed us how to use the device.

smoke. Since guests self-select in the two rooms of the bar, guests who are more sensitive to second-hand smoke may thus decide to sit in the second room when smoking is forbidden compared to being allowed. Assuming that guests who are more sensitive to smoke will respond more strongly to smoke exposure, the process of non-smokers self-selecting in the two rooms may bias the sample we observe. Our results show, however, that guests in both treatments *Smoking Allowed* and *Forbidden* realize that they will be exposed to smoke, when they enter the room. As the prohibition is either not in place or not enforced, many people smoke and heavy smoke fills the air (details about the intensive exposure below). The heavy smoke makes it salient to anyone entering the second room that the bar does not enforce the ban. Indeed, we surveyed guests when they left the bar and their responses confirm that they were fully aware that they would be exposed to smoke in the second room. We expected a guest`s decisions to sit in the second room to reflect his or her preference regarding smoke exposure. If the guest prefers avoiding second-hand smoke, he or she can be expected to take a seat in the main room, where always was ample space. If, on the other hand, a guest is indifferent about smoke exposure, he or she may decide to sit in the second room. Since guests are equally aware in both treatments that people smoke and the prohibition is not enforced, we expect that their decision is not affected by our treatment.

To support our assumption we elicit self-reported data on smoke tolerance and track whether non-smokers make the same room choice across treatments. As we expected, in both treatments *Smoking Allowed* and *Forbidden* guests indicate an equally high tolerance of secondhand smoke consistent with that guests base their room choice on actual smoke exposure and not on the legal smoking regime that we assigned[7]. By contrast if our treatment would have induced a selection bias, guests in the *Smoking Forbidden* treatment should reveal a comparatively lower smoke tolerance. Furthermore we show that the likelihood that a non-smoker decides to sit in the second and not the main room where the prohibition is enforced is the same in both treatments. We conclude that our assigned treatments, whether smoking is allowed or forbidden, does not bias the sample of guests that we observe and compare across treatments.

Our study has several policy implications. First, legal rights are not costless options to right holders. Our findings suggest that being allocated a right that remains unenforced and is violated can make the right holder worse off compared to not having the right at all. Right holders negatively experience infringements even if they are indifferent about what the right protects.[8] Thus when lawmakers consider enacting laws with little prospect of enforcement, they should weigh the benefits the symbolic rights may produce against the cost that violations may impose on right holders. Second, our study provides a new insight into the causes of social enforcement. Right holders in our experiment resent noncompliance not because of any material harm inflicted by the unlawful behavior, but merely because of the psychological cost they experience when their rights are infringed. As a result, right holders might engage in social enforcement

[7] We did not observe the behavior of non-smokers who visited the bar together with smokers; these non-smokers might prefer to join a friend despite being bothered by the smoke. We also considered observations only, when there was ample seating space in the main room and when non-smokers either sat alone or in a group of non-smokers. We discuss the controls we used in detail, see *supra* III.C and III.D.

[8] Legal rights are active when they entitle individuals to perform an action (sign a contract, take residence) or passive, when they entitle the right holder to have others perform an action or to abstain from an action (Hohfeld, 1919; Lyons, 1969). For instance, smoking bans, littering laws, and scoop-a-poop regulations have in common that they relieve the right holder from certain actions imposed by others.

even when they do not support the objectives of the law and have no material benefits from enforcing it. The independent infringement costs that we identify suggest that even controversial laws may induce social enforcement. For example, the mechanism may help explain the sudden equilibrium shifts from public smoking to abstention in countries that passed controversial and widely unenforced public smoking laws.[9]

This article proceeds as follows. In the next section we describe the general design of our study. Part 3 explains our treatments, the dependent and control variables that we measured and addresses potential biases that may affect our results. Part 4 presents our main hypothesis, and 5 explains the experimental procedure. We report our results in Part 6. Part 7 discusses the results and explores their policy implications. Part 8 concludes.

2. LEGAL BACKGROUND AND PRACTICE IN THE BAR *BEFORE* OUR EXPERIMENT

We conducted our study in a bar in Lower Saxony, Germany. German federal law prohibits smoking in public places. But many states permit exceptions for bars and restaurants. One such exception is a central element of our study: Lower Saxon state law permits bars to allow smoking in one room if the establishment has at least two separate rooms and the bar explicitly designates the second room as a smoking area.[10] The bar can decide whether it wants to fulfill the requirements of this exception. If the bar meets the requirements, smoking is legal within the limits imposed by the smoking ban. Otherwise, the exception does not apply and federal law prohibits smoking in the entire bar. For example, should a guest light up in the second room when the bar did *not* designate it as a smoking area, then the guest violates the, state imposed smoking prohibition.

This legal framework helps understand the policy that was in place at the bar *before* we conducted our study. As the law requires, the bar operated two separate rooms. In the main room, smoking was prohibited at all times and the smoking ban was enforced. The staff was instructed to intervene instantly if a guest lighted up there. In the second room, however, the bar alternated the smoking regime. On some days the management put up a sign that designated the second room as a smoking area, making

[9] In New York City, for instance, since the CIAA was implemented, exposure from all sources of SHS dropped by 94%. Matthew Farrelly & Andrew Hyland, The Health and Economic Impact of New York's Clean Indoor Air Act, July 2006, http://www.health.state.ny.us /prevention/ tobacco_control/docs/ciaa_impact_report.pdf at 12. In the United Kingdom after the first month of the smoking ban being in effect in the United Kingdom, there were strong figures showing compliance in the UK. Smoking Ban: Initial Survey Reveals High Level Of Compliance And Public Support, Action on Smoking and Health, August 4, 2007, http://www.ash.org.uk/media-room/press-releases/smoking-ban-initial-survey-reveals-high-level-of-compliance-and public-support. Local figures ranged from 94% to 99%, but average compliance by businesses to the law was 97%.

[10] Lower Saxony's law for the protection from the dangers of Secondhand Smoke July 12th 2007. §1, 10 NiRSG. § 2, 6 define exceptions from the general prohibition under particular conditions. If a bar has two rooms, smoking can be permitted in the second room if (1) the second room is smaller and does not have a counter; (2) the room is specifically designated as a smoking room. Note without the specific designation as a smoking room, the exception of rule § 2, 6 is not fulfilled and the general public smoking ban of rule § 5, 1 (1), 2 applies throughout the bar.

use of the exception of Lower Saxony's smoking law. At other times, the bar removed the sign so that smoking was prohibited by the public smoking ban.

In contrast to the main room, where the ban was always in place and enforced, the bar never enforced the public smoking law in the second room when the public ban applied because the room was designating as a non-smoking area.[11] The bar provided ashtrays on all tables in the second room regardless of the smoking regime and staff members never stopped guests from smoking. If a guest inquired about the smoking ban, staff members responded that they would not enforce the prohibition and they invited guests to sit in the main room, where the ban was strictly obeyed. Due to this policy, guests regularly smoked in the second room even when it was forbidden. Consequently, non-smokers were at all times subjected to high levels of secondhand smoke there.

By designating the second room as a non-smoking area without enforcing the prohibition the bar tried to solve a conflict of interests: The staff expected that with the prohibition in place some smokers would abstain from smoking or reduce their consumption in order to comply with the law, even though it remained unenforced.[12] The bar hoped that the reduced smoke concentration would be tolerable for non-smokers who could not be seated in the main room when it was crowded. At the same time, the bar wanted to appease the most fervent smokers by not enforcing the ban.

3. Experimental Design

3.1. The Treatments

In order to identify the effect of infringement of the guests` right to an environment free of smoke, we implemented three treatments that systematically vary the four factors prohibition, enforcement, smoke exposure and right violation. In *Smoking Forbidden* lighting up was forbidden, but the prohibition was not enforced, such that guests were exposed to smoke and their rights were violated. In *Smoking Allowed*, guests were exposed to smoke, but as smoking was not prohibited no rights were granted and could be violated. Finally in the *No Smoking* condition smoking was forbidden and the staff strictly enforced the ban, such that guests were not exposed to smoke and their rights were respected. By comparing the three treatments, we disentangle the impact of each of these factors. For example, comparing *Smoking Forbidden* with the other two treatments reveals the effect of right violations, since rights are violated in *Smoking Forbid-*

[11] The Bar can be fined by public authorities for not enforcing the smoking ban whenever the second room is not explicitly designated as a smoking area. Inspections by public authorities are rare, and fines are seldom imposed, however. In order to appease smokers, public smoking bans are often not enforced by owners in Germany. More than 60% of the bars are reported to violate the law. See *Carry on Lighting Up: Smoking Ban not Enforced in Parts of Germany*, DER SPIEGEL, July 2, 2008. Available at: http://www.spiegel.de/international/ germany/carry-on-lighting-up-smoking-ban-not-enforced-in-parts-of-germany-a-563424.html.

[12] This assumption was indeed correct. As we describe in more detail below, the concentration of smoke decreased when the prohibition was in place. We measured the smoke concentration in the room using a particle counter machine. *Infra*__.

den but not in the other two treatments. Comparing *Smoking Forbidden* with *Smoking Allowed* and *No Smoking* reveals that secondhand smoke does not cause the effects we observe. The results are the same for the treatment with the highest smoke exposure and the one with no smoke exposure at all, while we find a strong effect in *Smoking Forbidden*, where smoke concentration is less intense.

Table 1: Treatments

	Smoking Forbidden	Smoking Allowed	No Smoking
Prohibition	Yes	No	Yes
Enforcement	No	×	Yes
Smoke Exposure	Yes	Yes	No
Right Violation	Yes	×	No

In order to make our results robust against spontaneous variation in behavior, we collected observations over three full days for each treatment - a test day running from the opening of the bar until midnight closing time. We compare the data cross-sectionally between treatments. Treatments were not changed during the day. As a result, a single public event that might influence the decisions of more than just one individual guest – a soccer match in the evening that people may want to watch for example – can influence only a few observations and likely cancels out over the full three-day observation period and across the randomly assigned treatments. In addition this design enables us to perform robustness checks by splitting up the data and comparing the observations of days separately (see below). The bar allowed us to *randomly* assign our treatments. We conducted the study on consecutive weekends (Friday-Sunday) following a balanced schedule, which alternated the order of the treatments using placeholders. We drew a lot to randomly assign the treatments to the placeholders.

Note that except for our treatment manipulation we held the experimental set-up constant across conditions. In the second room, the staff never requested guests to stop smoking. Ashtrays were provided on each table during all observations that we collected. In the main room by contrast, the smoking prohibition was always strictly enforced. The only difference between treatments was the randomly assigned smoking regime, i.e. whether smoking was allowed or forbidden in the second room.

3.2. Dependent and Control Variables

We collected two dependent variables: First, the time non-smoking guests spent at the bar, measured in minutes from getting seated until departure. Second, the money guests spent on food and beverages. Both dependent variables may be influenced by latent factors that remain unobserved. For instance, guests might leave the bar because

they have another appointment, or because their budget restricts their consumption. Eliciting (fragments of) this information in the exit interviews would not have been useful. Due to its subjective and incomplete nature, the self-reported information would have been very difficult to classify. Assume for example that a customer stated that his work obligations made him leave the bar. It would be impossible to determine with confidence that this observation should be classified as a necessary departure or whether the customer could in fact have stayed longer in spite of this obligation. We also needed to keep the list of questions short enough to motivate guests to complete the interviews.

To ensure that our observations are independent, we treat groups of guests as one observation regardless of the number of guests in the group. This is necessary since members of a group will usually decide together when they leave the bar. As a result, also the consumption of the members of the group will not be independent of one another. Since we could not observe how group members reach their collective decisions, we treat the group as one observation and calculate the mean *consumption* of the group and take the mean of the time they spent at the bar in the rare case they did not leave together. When a smoker was part of the group, we did not include the group in our observations. A smoker might affect how long the other members of the group stayed and how much they consumed. Finally, since groups may generally stay longer than guests who visited the bar alone, we control for the size of groups in our regressions.[13]

We also observed whether a non-smoker's right was violated during the time they spent in the second room, i.e. whether someone had smoked in his or her presence. This variable enables us to attribute non-smokers' behavior to the violation of their personal right as opposed to responding to the bar's omission to enforce the smoking ban.

Additionally, we elicited a set of control variables. In order to control for the potential impact that smoke exposure might have on the behavior of customers, we installed a device in the second room that measured the concentration of particles in the air. We attach a detailed description of how the TSI 3007 functions in the appendix[14]. The device measures particles ranged in size from 0.01 to >1.0 µm. In city environments the air outside usually contains around 4000/cm^3 particles (depending on city size and environmental pollution); for inside non-smoking office space the typical concentration is as low as 2500/cm^3 particles[15]. Guests could not see or hear the device. The particle counter elicited the smoke concentration every 30 seconds. The measurements allowed us to determine the dose of smoke that each individual guest was exposed to during his or her visit at the bar. For example, if a guest stayed 30 minutes, we collected 60 sepa-

[13] We also measured for every eighth guest, the time between a meal order and it being served. The results indicate that guests did not have to wait systematically longer in one condition than in the other, for example because the bar might have been busier on some days.

[14] http://www.tsi.com/uploadedFiles/_Site_Root/Products/Literature/Spec_Sheets/3007_1930032.pdf

[15] http://www.tsi.com/uploadedFiles/_Site_Root/Products/Literature/Case_Studies/ptrak-case4.pdf

rate measurements for him or her. We calculated the mean of these values constructing an individual score. On average we observed a particle concentration of 32117/cm^3 in the *Smoking Forbidden* treatment which is 800% higher than in average outside environments and almost 1300% as much as is typical for office space.

The results show that the exposure to smoke was only 25.2% higher when guests were allowed to smoke than when smoking was forbidden (*Smoking Allowed:* 40230/cm^3 v. *Smoking Forbidden*: 32117/cm^3; two-sided Mann-Whitney *p*-value <0.01): The difference is consistent with the expectation that some smokers would feel obliged to comply with the smoking ban or reduce their smoking. Since the mean concentration of particles was lower when smoking was forbidden, our main effect – non-smokers leave earlier and consume less when smoking is forbidden – cannot be caused by a higher level of smoke exposure. We also use *Smoke Exposure* as a control variable in our regressions. The variable allows us to control for the individual dose of smoke each guest was exposed to. For example, guests in whose presence no one smoked, were exposed to a somewhat lower level of secondhand smoke while other guests have been around multiple smokers and accordingly were exposed to a higher dose of smoke. Our regression analysis confirms that smoke exposure did not affect the behavior of guests. We report the results below along with the main effects[16].

We conducted a brief exit interview when guests left the bar. First, we asked whether the guest is a smoker or a non-smoker. Second, we inquired how sensitive guests are to secondhand smoke (Likert item ranging on an ordinal scale from 1=very tolerant to 7=not tolerant at all). Third, we queried non-smokers whether they fully realized that they would be exposed to second-hand smoke prior to entering the second room.

3.3. Testing for Selection Bias

If non-smokers would have expected the staff to enforce the prohibition in the second room, our treatment may bias how guests self-select into the two rooms of the bar. Non-smoking guests who are smoke sensitive and expect the prohibition to be obeyed may decide to sit in the second room under the *Smoking Forbidden* treatment, while they may be more likely to favor a table in the main room under the *Smoking Allowed* treatment. As a consequence, when smoking is forbidden, we may observe a sample of guests in the second room that is on average less smoke tolerant than when smoking is allowed. Since less smoke intolerant patrons are more likely to feel disturbed when other guests smoke they may leave the bar earlier and consume less than a more smoke tolerant sample of patrons who may choose to sit in the room under the *Smoking Allowed* treatment. The process of guests selecting in which room they want to sit may thus bias our findings and may be the actual cause behind the treatment effect we aim to observe.

[16] This variable also enabled us to test for potential interaction effects with the treatment; for example, subjects may be more sensitive to smoke exposure when smoking was forbidden. We did not find such effects.

However, we designed the experiment such that our treatment manipulation (varying the smoking regime) should not affect people`s decision where to sit. Recall that the staff does not enforce the prohibition in the *Smoking Forbidden* treatment. The staff leaves the ashtrays on the tables and never requests guests to stop smoking. As a result, the ban is regularly violated during the experiment and the air is always filled with smoke. Upon entering the room, non-smokers immediately become aware that some guests smoke even when smoking is forbidden. Since people smoke independently of the assigned smoking regime, we expect all guests who are *not* indifferent about smoke exposure to take a seat in the main room (where is ample space), irrespectively of which treatment we implement in the second room, *Smoking Allowed* or *Smoking Forbidden*. Only guests who are truly *indifferent* about second-hand smoke should get a table in the second room. Since these guests are indifferent about smoke exposure, we do not expect their choice of room to be influenced by which treatment is assigned either, such that we should see the same sample of guests in both rooms irrespective of the treatment we implement.

The data that we collected in the exit-surveys allow us to directly test our assumption that that our treatment design does not induce a selection bias which may distort our results. On the basis of the survey data, we can distinguish between smokers and non-smokers.[17] Using this data, we can determine the odds ratio for a non-smoker deciding to sit in the second room instead of the main room. We then compare the likelihood across the two treatments *Smoking Forbidden* and *Smoking Allowed* that we implemented in the second room. Under the *Smoking Forbidden* treatment the likelihood that a non-smoker decides to take a seat in the second room is 0.29, while under the *Smoking Allowed* treatment it is 0.37. Notice, if our treatment indeed had induced a selection bias, the effect should point in the opposite direction. If the likelihood that a non-smoker decides to sit in the second room would be exactly the same under both treatments the odds ratio would be 1. In our study the odds that a non-smoker sits in the second room during the *Smoking Allowed* treatment are 1.32 times higher than the odds of a non-smoker showing up in the second room during the *Smoking Forbidden* treatment. Since the unbiased odds ratio of 1 lies within the 95% confidence interval of the odds (1.32) we determined in our study (lower bound= 0.95<1) we can reject that a selection bias is present. This result also holds if we rely on our own observations to distinguish smokers from non-smokers. In this case any guest is treated as a non-smoker, who we did not observe smoking during his or her visit at the bar.

As a robustness check we use a second approach to rule out a selection bias. We asked guests to indicate their tolerance of smoke using a Likert item with an ordinal scale ranging from 1-7 (1=very tolerant; 7=not tolerant at all). First we directly compare the values we measured in the treatments *Smoking Allowed* and *Smoking Forbidden.* Overall, non-smokers in both treatments reported a score of around 2 suggesting that the guests did not feel strongly disturbed by the secondhand smoke. We obtained an average of 2.04 for customers in the *Smoking Forbidden* treatment (95% confidence

[17] We also matched the survey data with the behavior of guests at the bar and did not find inconsistencies.

interval lower bound 1.83, upper bound 2.19), while we elicited a very similar average of 1.89 (95% confidence interval lower bound 1.73, upper bound 2.05) in the *Smoking Allowed* treatment. The difference is insignificant (two-sided Mann-Whitney *p*-value 0.35). Also the standard deviations in the two treatments are not significantly different from each other, ruling out that our results are distorted by accidental differences in variance.

In the main room we distributed surveys on each table for guests to indicate their smoke tolerance. As expected, guests in the main room are significantly more sensitive to secondhand smoke (2.68) compared to the non-smokers in both treatments in the second room (two-sided Mann-Whitney, *p*-value <0.01)[18]. As a robustness check we split the data in observations we collected while in the second room a) the *Smoking Allowed* treatment b) the *Smoking Forbidden* condition was implemented. If more smoke sensitive guests would have taken a seat in the second room instead of the main room under the *Smoking Forbidden* treatment, the values we elicit in the main room may change as well. However, we do not find a difference between the two values (two-sided Mann-Whitney *p*-value 0.31; *Smoking Forbidden* 95% confidence interval lower bound 2.35, upper bound 3.07, *Smoking Allowed* 95% confidence interval lower bound 2.14, upper bound 2.73) supporting that the treatment does not affect people`s choice of room.[19]

We can also use regression analysis to rule out selection bias by including the variable *Smoke Tolerance* in our regressions. If the treatment (smoking forbidden) would have induced guests with a lower tolerance of smoke to get seated in the second room and if this lower tolerance would have caused guests to leave earlier and consume less, the variable should have an impact in the regressions. Yet, we find the variable *Smoke Exposure* to be insignificant across all our regressions (see tables below). The variable does not even change the R^2, suggesting that it resolves almost no variance in our data. This result strongly shows that no selection bias is present and the decision where to sit was not influenced by the treatment we assigned.

Finally the measurements of our particle counter confirm the assumption we used to design the experiment: we assume guests are well aware that people smoke in the second room under both treatments. Indeed, the results of the particle counter indicate how evident it was to non-smokers that people were smoking in the second room during both treatments – even if no-one was smoking at the moment when they happened to enter the room. In the *Smoking Forbidden* treatment the concentration of smoke was on average with 32.117/cm^3 800% higher than typically in outside environments and almost 1300% higher than in non-smoking office space. These extreme values explain why almost all guests indicated in the exit interviews that they expected some patrons to smoke in the second room even when the ban was in effect and smok-

[18] Generally the values that subjects indicated are surprisingly low. However, the absolute values should not concern the treatment effects that we are interested in.

[19] The smoke tolerance does also not differ in any of our treatments substantially across the three days of observation (two sided Kruskal-Wallis: *Smoking Allowed p*-value 0.77; *Smoking Forbidden p*-value 0.40; *No Smoking p*-value 0.93).

ing was forbidden.[20] By contrast, the difference between the values measured in the *Smoking Forbidden* and the *Smoking Allowed* condition (40230/cm^3) is comparatively small (25.2%).[21]

The second assumption of our design has already been shown valid before: Once guests understand that people smoke in the second room independent of whether smoking is allowed or forbidden, then guests choose the room they want to sit in, according to whether they want to avoid being exposed to smoke or not. The smoking regime does not influence their choice of room.

We also considered a second potential cause that might have induced a selection bias: space constraints. If the main room was crowded, people may have decided to sit in the second room even if they otherwise would have preferred a table in a smoke free area. Space could have confounded our results if, due to coincidence, the main room happened to be crowded during the *Smoking Forbidden* treatment but had ample space during the *Smoking Allowed* treatment. Then more sensitive guests might have moved to the second room under the *Smoking Forbidden* treatment. To rule out this potential confound, we always verified that there was sufficient space in the main room such that guests always could freely decide where they wanted to sit. It turned out that the main room was never crowded while we conducted our study.

4. Main Hypothesis

We expect that non-smokers experience disutility when their right to a smoke free environment is violated. We hypothesize that infringements impose a psychological cost on non-smokers, reducing the utility they derive from their stay at the bar. Since we cannot observe this disutility directly, we use two dependent variables as proxies: *Time* spent in the bar and *Consumption*. As we want to measure the same outcome with both variables (utility derived from visiting the bar) we expect the variables to be highly correlated. Whether the treatment primarily influences the time the guests spent at the bar and *through* time the amount they consume, or whether guests want to consume less, because they do not enjoy their visit and therefore leave earlier makes no difference for our study. We do not aim to identify the actual effect channel. We only elicit both variables as a robustness check, allowing us to measure the *utility* derived from the visit in two different currencies: time and money.

We predict that guests will consume less and stay at the bar for shorter periods if their rights are violated during their stay at the bar. We assume that the non-smokers in the treatments *Smoking Allowed* and *Smoking Forbidden* are not affected by the secondhand smoke; otherwise they would have preferred to get seated in the uncrowded main room. In the *Smoking Forbidden* treatment smoking imposes a psychological

[20] Over the course of the entire experiment, we observed very few customers that decided to move over to the non-smoking area once they observed others lighting up.

[21] While the smoke tolerance measure is more fine-grained, determining the odds ratio of non-smokers that take a seat in the second room has the advantage that the status of smoker/non-smoker cannot be influenced by the treatment.

cost on guests, since they have been allocated rights in this treatment that smoking violates. By contrast in *Smoking Allowed* no rights to a smoke free environment are granted such that guests do not experience a disutility when they are exposed to smokers. Thus we expect guests to stay shorter (*Hypothesis 1a*) and consume less (*Hypothesis 2a*) when smoking is forbidden, as compared to the *Smoking Allowed* condition. We expect to find the same treatment effect when we compare the behavior of non-smokers in the *No Smoking* with the *Smoking Forbidden* condition. Since the smoking law is strictly enforced in the main room, non-smokers in the *No Smoking* condition experience no right violations that may impose a psychological cost on them. Therefore we expect that guests in the in the *Smoking Forbidden* treatment will stay shorter (*Hypothesis 1b*) and consume less (*Hypothesis 2b*) than the guests in *No Smoking* in the main room. Finally, since we assume that exposure to smoke does not affect guests in the second room, guests in the *Smoking Allowed* treatment who are exposed to the most intense concentration of smoke should not behave differently from guests in the *No Smoking* condition who sit in the smoke free main room. Comparing these two treatments, we do not expect to find a difference with respect to the time guests spend at the bar (*Hypothesis 1c*) and the amount they consume (*Hypothesis 2c*).

5. Experimental Procedure

Our observations are based on the natural behavior of the guests. The experimental protocol ensured that customers at the bar were not aware that a study was being conducted until they were surveyed upon exiting the bar.[22] Therefore we can exclude that guests changed their behavior (deliberately or subconsciously) due to being observed.

We obtained 244 independent observations in total. In the *Smoking Forbidden* treatment we collected 76 observations of non-smokers, in *Smoking Allowed* 81 and finally in *No Smoking* 87 observations. Additionally, we observed whether someone was smoking during the time a guest stayed in the second room of the bar. In the *Smoking Forbidden* treatment we obtained 22 observations of guests whose rights were not actively violated by a smoker and 54 observations of non-smokers whose rights were infringed. In *Smoking Allowed* 73 guests faced a smoker while they stayed at the bar, while 8 did not. At all times during the experiment, we measured the concentration of smoke in the second room with the TSI 3007 particle counter that was hidden inside the room.

Finally, we conducted brief exit-interviews with the non-smoking guests after they left the second room. By registering the time when they left their seats and when they completed the interview, we matched the data of the exit interviews with the observations on time and consumption. Customers did not have to reveal their identity or any demographic information during the interviews. The staff provided us with the

[22] Given that patrons often were studying or did other work in the bar, the experimenters taking notes would not raise suspicion. The exit-interviews confirmed that the guests did not suspect that an experiment was conducted.

payments of the guests. Some visitors refused to be interviewed; either because they had privacy concerns or because they did not want to invest the time.

For each treatment we observed the behavior of the guests for three full days. In order to hold the conditions as constant as possible, the experiment was conducted on the same days in consecutive weeks. This ensured that the same waiters worked at the bar during the whole experiment. We did not disclose the purpose of the study to the staff until the study was completed. The bar management endorsed the project because they wanted to find out whether the alternating smoking regime negatively affects their business.

6. RESULTS

We first compare the two dependent variables *Time* and *Consumption* across the three treatments: *Smoking Allowed, Smoking Forbidden*, and *No Smoking*. Second, to further support our hypothesis that the psychological cost is caused by right infringement we distinguish in the *Smoking Forbidden* and the Smoking Allowed treatment between guests who were exposed to a smoker and guests who were not. We should find an effect between these two groups in the *Smoking Forbidden* treatment, where smoking presents a right violation; by contrast there should be no difference between the groups in *Smoking Allowed,* since no rights to a smoke free environment are granted in this treatment. As customary in experimental economics, we present non-parametric treatment comparisons first to transparently report our findings without relying on distributive assumptions[23]. Then we show regression models that include all data and control variables.

6.1. Effect on Time

Hypothesis 1: Non-smokers spend less time in the bar in the Smoking Forbidden *compared to (a) the* Smoking Allowed *and (b) the* No Smoking *treatment. We expect to find no difference between the* Smoking Allowed *and the* No Smoking condition *(c).*

We measured the time from when a customer took a seat until he or she left the room. The data indicates that non-smokers in the *Smoking Forbidden* treatment stayed significantly shorter than the guests in the *Smoking Allowed* treatment (*H1a*: 53.89 v. 66.09 min.; two-sided Mann Whitney *p*-value <0.01).

Table 2: Non-Parametric Treatment Comparisons

	Time Min	Consumption €
Smoking Forbidden	53.89	6.78

[23] Our data would also justify using t-tests. The rationale of presenting non-parametric tests in experimental economics is to increase transparency as these tests do not rely on assumptions about the sample distribution.

(N=76)		
Smoking Allowed (N=81)	66.09 p <0.01*** (v. *Smoking Forbidden*)	10.15 p <0.01** (v. *Smoking Allowed*)
No Smoking (N=87)	69.31 p =0.31 (v. *Smoking Allowed*) p <0.01*** (v. *Smoking Forbidden*)	9.67 p =0.39 (v. *Smoking Allowed*) p =0.02** (v. *Smoking Forbidden*)

All *p*-values are two-sided Mann-Whitney tests.
*, **, *** indicate significance at the 10%, 5%, and 1% level.

We also find a significant difference when comparing the behavior of non-smokers in the *Smoking Forbidden* treatment with the guests in the *No Smoking* condition (*H1b*: 53.89 vs. 69.31 min; two-sided Mann Whitney *p*-value <0.01). Finally, we compare the results between the *Smoking Allowed* and the *No Smoking* condition. As expected (*H1c*), the difference is insignificant (two-sided Mann-Whitney *p*-value 0.31; 95% confidence interval lower bound 64.1 & upper bound 74.56 for *Smoking Allowed* and 62.54 & 69.63 for *Smoking Forbidden*). The results support our assumption that the guests in the second room are affected by infringements of their rights, but not by secondhand smoke: In the treatment with the highest smoke exposure we find the same results as in the treatment with no smoke exposure at all. Only the behavior in the treatment, where the guests` rights are violated, strongly differs from the other two on both of our outcome variables. The results are summarized in table 2.

Additionally, we perform an OLS regression including all data of the three conditions. We include the level of smoke exposure and the self-reported tolerance of smoke as individual level data for each guest (group) into the regression. To account for potential events that might have influenced the choices of more than a single guest (or group of guests that we treat as one observation) and which took place on a particular day when we conducted the study (a football match on tv for example), we construct a separate dummy variable for each weekday. A second dummy controls for the *Room* in which the guests were seated (main room=0 or second room=1). Finally, we control for group size as the observations consist of a varying number of guests.

We measure the main treatment effects with two dummy variables. The first dummy *Treatment* captures the difference that the treatment *Smoking Forbidden* makes in comparison to the two other conditions *Smoking Allowed* and *No Smoking*. Thus the coding of the variable is 1=smoking forbidden; 0=other. Our second variable is a dummy for *Room* (Second room=1; main room=0) which picks up if guests who sit in the main room behave differently from guests in the second room as long as this difference is not caused by the treatment *Smoking Forbidden*, which is controlled for by the *Treatment* dummy. Thus the *Room* dummy allows us to measure whether the guests in the two treatments *Smoking Allowed* and *No Smoking* behave differently. While our regression treats the guests in these conditions as being assigned to the same treatment (*Other code=0),* they sit in different rooms. If they would behave different from one

other, this difference would not be picked up by the *Treatment* dummy, but the room dummy would reveal it.

As we hypothesized the *Treatment* dummy is significant that is guests stay significantly shorter at the bar under the *Smoking Forbidden* compared to the pooled two other treatments (-13.57min). The *Room* dummy by contrast is insignificant and demonstrates that the guests` behavior does not differ depending on the room in which they sit as long as the treatment is the same. The result thus confirms that the guests` who sit in different rooms under the *Smoking Allowed* compared and the *No Smoking* treatment do not behave differently. The main effect is induced by the treatment: Guests only leave the bar earlier, when smoking is forbidden and their rights are violated.

The *Room* dummy also controls for unobservable factors that might lead guests to prefer one room over the other (the interior in one room might appear more beautiful etc.) and shows that room preferences do not affect the guests' behavior. The other covariates, the *Smoke Tolerance* and individual *Smoke Exposure* also remain insignificant in the regression, underlining that exposure to smoke did not cause guests to leave earlier. All results are reported in table 3.

Table 3: OLS Regressions – Main Effects

Dependent Variable	Time	Consumption
Dummy Treatment	13.575***	-3.612***
	(3.215)	(1.207)
Dummy Room	-3.387	0.321
	(4.558)	(1.207)
Smoke Tolerance	-0.402	-0.118
	(1.236)	(0.327)
Smoke Exposure	-0.000	-0.000
	(0.000)	(0.000)
Dummy Day1	-3.927	-1.218
	(4.616)	(1.610)
Dummy Day2	-10.005	-3.209***
	(6.077)	(1.437)
Dummy Day3	-0.989*	-3.612***
	(1.545)	(0.851)
Group Size	-0.123	0.002
	(1.065)	(0.282)
Constant	82.838	13.801
	(8.233)	(2.181)
R^2	0.12	0.10

Standard errors are reported in parentheses.
*, **, *** indicate significance at the 10%, 5%, and 1% level.

We check the robustness of our findings by splitting our data set in three separate parts: day1 (Friday), day2 (Saturday) and day3 (Sunday). We analyze the effects for each of the three parts separately across treatments. Our main treatment effect, the difference between *Smoking Forbidden and Allowed* is independently significant for each of the three

data sets (two-sided Mann Whitney: set1 *p*-value <0.01; set2: *p*-value <0.01; set3: *p*-value <0.01) strongly underlining the robustness of our results. The regression results, which use the same model including control variables that we presented above, confirm the non-parametric tests and are reported in table 5 in the appendix.

6.2. Effect on Consumption

As we use both dependent variables *time* and *consumption* as proxies to measure the same value - the utility guests derive from their visit at the bar - we test the same set of hypothesis for the variable *Consumption*.

Hypothesis 2: *We expect non-smokers to consume less in the* Smoking Forbidden *compared to (a) the* Smoking Allowed *and (b) the* No Smoking *treatments. Finally, we do not expect to observe any difference between the* Smoking Allowed *and the* No Smoking *condition (c).*

When dealing with a group of customers we calculate their mean spending and treat this value as one observation. If one guest paid for the others, we calculated the per capita spending and included this value as one observation in our analysis.

We find that guests in the *Smoking Forbidden* treatment spent €6.79 on food and beverages while the non-smokers in the *Smoking Allowed* treatment consumed €10.15 (see results table 1). As hypothesized, a comparison yields a significant treatment effect (two-sided Mann Whitney *p*-value <0.01; *H2a*). The consumption in the *Smoking Forbidden* treatment (€ 6.79) is also significantly different from the *No Smoking* condition (€ 9.67; two-sided Mann-Whitney *p*-value 0.02; *H2b*). Finally, we compare the results of the *Smoking Allowed* treatment with the *No Smoking* condition finding no substantial deviation (two-sided Mann-Whitney *p*-value 0.83; *H2c;* 95% confidence interval lower bound 8.98 & upper bound 11.32 for *Smoking Allowed* and 8.46 & 10.88 for *Smoking Forbidden*).

Additionally, we run the same OLS regression model that we employed for the first outcome variable *Time*. The *Treatment* dummy is again significant, showing that the unenforced smoking law reduces the amount guests spent in the bar on meals and beverages by an estimated -3.61€. As found for the regression on *Time* the *Room* dummy is insignificant, indicating that guests consume the same amount in both rooms as long as they are not assigned to the *Smoking Forbidden* treatment. Thus the results suggest that there is no difference in spending under the *Smoking Allowed* and *No Smoking* treatments; guests only consume less when their rights are violated when smoking is forbidden. The other covariates are insignificant as they have been for *Time* (see table 2).

We again check the robustness of our findings by splitting up the data in three separate sets day1, day2 and day3. In each of the three data sets guests consume significantly less under the *Smoking Allowed* v. the *Smoking Forbidden* treatment (two-sided Mann Whitney set1, set2, set3: all *p*-values <0.01). The regression results confirm these findings. Only the treatment difference in set3 does not reach a conventional level of significance in the regression. The full regression results of the robustness check are reported in table 5 in the appendix.

As expected, our two outcome variables *Time* and *Consumption* are highly correlated (Spearman $r_{s=}$ 0.616, *p*-value <0.01) and lead to collinearity in the regressions. For the purpose of our study, however, it is not relevant to identify the dominant effect channel. Both outcome variables serve as proxies to measure the loss of utility induced by the violation of the smoking ban. We consistently find similar effects on both proxy variables, presenting robust evidence that the treatment, the unenforced smoking ban indeed reduces the utility that guests derive from their visit at the bar.

6.3. Active Violation of Personal Rights

The comparison of the treatments *No Smoking*, *Smoking Forbidden* and *Allowed* enabled us to carve out the main effect that we observe is caused by the violation of personal rights that the smoking ban confers to guests, since our conditions systematically vary prohibition, non-enforcement, smoke exposure and right violation.

In this section we provide further support for our hypothesis that the disutility guests experience is caused by infringements. The analysis below exploits the fact that people did not at all times smoke in the second room. This allows us to split the sample of observations in both treatments *Smoking Forbidden* and *Smoking Allowed* according to whether someone smoked in the presence of the guest or whether the guest did not face a smoker during his or her stay. In the *Smoking Forbidden* treatment smoking infringes the right for a smoke free environment, while in *Smoking Allowed* guests were not granted rights that smoking could violate. Thus if the effect is driven by right violations as we claim, comparing the two groups - guests who faced a smoker and those who did not –should reveal a difference in the *Smoking Forbidden* but not in the *Smoking Allowed* treatment. The infringement should impose a psychological cost only on those guests in *Smoking Forbidden*, whose rights were violated.

In line with our expectation the N=54 non-smokers whose rights were actively violated by a smoker left the bar significantly earlier than the N=22 guests who were not subjected to a direct infringement (51.98 < 58.59 min; two-sided Mann-Whitney *p*-value 0.03; OLS reg beta=5.962 *p*-value 0.06). We also observe that the guests whose rights were violated consumed a smaller amount (€6.07 < €8.52; two-sided Mann-Whitney *p*-value 0.13, OLS reg beta=2.588 *p*-value <0.01).

As expected in the *Smoking Allowed* condition the same analysis does not reveal a difference on both of our outcome variables. The guests in whose presence someone smoked (N=73), and those patrons who have not been exposed to a smoker (N=8) stayed equally long at the bar and consumed a similar amount. In fact, the trend even points in the opposite direction than in the *Smoking Forbidden* treatment with guests being exposed to a smoker staying slightly longer and consuming more: *Time*: 66.55_{exposed} v. $61.88_{\text{not-exposed}}$ Mann-Whitney *p*-value 0.21; and *Consumption*: € 10.22_{exposed} v. € $9.55_{\text{not-exposed}}$ Mann-Whitney *p*-value 0.96).

In addition we perform an OLS regression with the data of the *Smoking Forbidden* and *Smoking Allowed* treatments to estimate the interaction term *Treat-*

*ment*Smoking.* We include control variables for days, smoke exposure and tolerance. The interaction turns out significant on the 10 percent level for both outcome variables *Consumption* (beta=3.345, stderr. 2.023) and *Time* (beta=11.750, stderr=6.519) - even though we naturally could only collect a few observations of guests who did not face a smoker while they stayed at the bar when smoking was allowed. Thus we can conclude that the effect of being exposed to a smoker on *Time* and *Consumption* is significantly larger in *Smoking Forbidden* than in *Smoking Allowed* that is the effect is significantly stronger when a smoker violates the guests' assigned rights.

Table 4: Right Violations – Non-Parametric Treatment Comparisons

	Time Min	Consumption €
Smoking Forbidden *Right Violation* (N=54)	51.98	6.07
Smoking Forbidden *No Right Violation* (N=22)	58.59 p=0.03** (v. *Smoking Forbidden Right Violation*)	8.52 p=0.13 (v. *Smoking Forbidden Right Violation*)
Smoking Allowed *Smoking during Visit* (N=74)	66.42 p<0.01*** (v. *Smoking Forbidden Right Violation*)	10.20 p<0.01*** (v. *Smoking Forbidden Right Violation*)
Smoking Allowed *No Smoking during Visit* (N=7)	62.57 p=0.34 (v. *Smoking Forbidden No Right Violation*)	9.61 p=0.94 (v. *Smoking Forbidden No Right Violation*)
No Smoking (N=87)	69.31 p<0.01** (v. *Smoking Forbidden Right Violation*) p=0.91 (v. *Smoking Allowed Smoking during Visit*)	9.67 p=0.02* (v. *Smoking Forbidden* Right Violation) p=0.41 (v. *Smoking Allowed Smoking during Visit*)

All *p*-values are two-sided Mann-Whitney tests.
*, **, *** indicate significance at the 10%, 5%, and 1% level.

As a further robustness check for our findings, we directly compare the behavior of guests who faced a smoker during their stay under the *Smoking Forbidden* treatment with guests in the *Smoking Allowed* treatment who also have been exposed to smokers. If our main effect is indeed caused, by right violations, non-smokers should leave the bar earlier and consume less under the *Smoking Forbidden* treatment. Additionally, we should observe a similar difference in relation to guests in the *No Smoking* condition, because they also were not subjected to an infringement. As expected, guests in the *Smoking Forbidden* treatment whose rights were violated, leave the bar significantly earlier than the guests in the *Smoking Allowed* treatment (51.89 v. 66.09 min; two-sided Mann-Whitney *p*-value <0.01) and the *No Smoking* condition (51.89 v. 69.31 min; two-sided Mann-Whitney *p*-value <0.01). We find the same effect for consumption: guests under the *Smoking Forbidden* treatment spend significantly less on food and beverages

compared to the *Smoking Allowed* condition (6.07 v. 10.15 min; two-sided Mann-Whitney *p*-value <0.01) as well as compared to the *No Smoking* treatment (6.07 v. 9.67 min; two-sided Mann-Whitney *p*-value 0.02). The similarity of the *No Smoking* and *Smoking Allowed* results demonstrates yet again that smoke exposure does not cause our findings.

Finally, we confirm our findings in an OLS regression using the full data set, including the *No Smoking* condition. We create two dummy variables in order to estimate the effect of infringements. The first dummy measures the impact of whether during the time a visitor was present at the bar he or she was exposed to a smoker (smoking in presence=1; otherwise=0). The second dummy estimates the influence of the treatment and distinguishes smoking that occurred when the prohibition was in place from smoking that occurred when it was allowed (*Smoking Forbidden*=1; *No Smoking* & *Smoking Allowed*=0). As table 5 indicates the results show the same pattern for both outcome variables *Time* and *Consumption*.

Table 5: OLS Regressions – Rights Violation

Dependent Variable	Time	Consumption
Right Violation Yes/No	14.028***	-3.730***
	(3.545)	(0.933)
Smoking Yes/No	-3.697	-0.154
	(4.436)	(1.167)
Room Main/Second	-9.723	-0.361
	(4.764)	(1.173)
Smoke Tolerance	-0.584	-0.179
	(1.241)	(0.326)
Smoke Exposure	-0.000	-0.000
	(0.000)	(0.000)
Dummy Day1	-3.797	-0.353
	(4.638)	(0.412)
Dummy Day2	-9.277**	-2.846
	(6.133)	1.641
Dummy Day3	-8.803*	-2.984
	(5.478)	(1.442)
Group Size	0.669	0.024
	(1.005)	(0.265)
Constant	62.248	6.368
	(7.466)	(7.466)
R^2	0.11	0.09

Standard errors are reported in parentheses.
*, **, *** indicate significance at the 10%, 5%, and 1% level.

The variable *Right Violation* is strongly significant, showing that smoking in violation of the ban does indeed reduce the time and the money that guests spend in the bar. By contrast, the dummy for *Smoking* remains insignificant, indicating that whether a guest is exposed to smoking during his or her visit or not does not have a substantial influence on his or her behavior as long as smoking does not violate the non-smokers' rights. The dummy for *Room* is also insignificant showing that the behavior of guests does not differ depending on whether they sit in the main room under the *No Smoking* treatment or in the second room, as long as they are not exposed to a smoker, when smoking is forbidden. Finally the covariates *Smoke Tolerance* and *Smoke Exposure* are

insignificant as well as the controls for *Group Size* and the three dummy variables for *Days* of Experiment. The regression thus confirms our main result: right violations cause a utility loss among right holders.

6.4. Reaction to the Bar's Omitted Enforcement

So far we analyzed whether guests respond to the *active* violation of their rights by other smoking guests. But patrons might also react to the bar's omission to enforce the public smoking law and its failure to protect their rights. For instance, the bar was legally obligated to remove the ashtrays and to instruct smokers not to light up. Our measures and the exit surveys reveal that guests who did not face a smoker during their stay were also aware that the bar neglected to enforce the public smoking law. They were exposed to an almost equally high concentration of particles with 27.209/cm^3, a concentration 700% higher than in typical outside environments and almost 1100% higher than in regular non-smoking office space. In comparison, for guests whose rights were actively violated by smokers we measured an exposure with 34.116/cm^3 particles, a difference of only 25.3%. The extremity of these values explains why almost all guests who did not face a smoker during their stay at the bar nevertheless indicated in the exit interviews that they understood that people smoke and the bar does not enforce the ban before they entered the room.[24]

We expect the guests' response to the bar`s noncompliance with the smoking law to be less pronounced than their reaction towards smokers. The smokers *actively* violate the guests' rights, while the bar only c*ommits* an omission by not enforcing the ban. Experiments from multiple disciplines have established that people generally ascribe substantially less outcome responsibility to omissions than to actions (see instead of many Baron & Ritov, 1994). Thus guests can be expected to attribute the responsibility for the infringements primarily to the smokers.

To test how guests react to the bar`s omission, we compare the behavior of guests in the *Smoking Forbidden* condition whose rights were *not* actively violated by smokers with the guests in the *Smoking Allowed* and the *No Smoking* conditions. Interestingly, we find significant evidence that the guests in the *Smoking Forbidden* treatment who did not face a smoker, stay on average for shorter periods (58.59 min) than the non-smoking guests in the *No Smoking* (69.31 min; two-sided Mann-Whitney *p*-value <0.01) and the *Smoking Allowed* conditions (66.09 min; two-sided Mann-Whitney *p*-value 0.04). While the results for *Consumption* show the same tendency, they do not reach significance: in the *Smoking Forbidden* treatment non-smokers who did not face a smoker consume € 8.52, in *Smoking Allowed* we measured € 10.15 (two-sided Mann-Whitney *p*-value 0.21) and in the *No Smoking* treatment € 9.67 (two-sided Mann-Whitney *p*-value 0.39). The findings are consistent with the expectation that the guests

[24] By contrast, the difference of the values measured in the *Smoking Forbidden* treatment and the *Smoking Allowed* condition is very small: only 25% (40230/cm3).

primarily respond to the *active* violation of their personal right and only to a lesser degree to the bar's neglect of its legal duty to enforce the smoking ban.

7. DISCUSSION

The smoking ban we analyzed provided non-smokers with a right to be free from secondhand smoke. Our findings suggest that individuals suffer a psychological cost when these rights that are granted by the prohibition are infringed even though individuals did not care about the behavior the legal right targets to protect them against. The cost is psychological, because it refers exclusively to the disutility caused by the violation of the right and is independent of any physical inconvenience imposed by the secondhand smoke.

7.1. Internal Validity

The evidence we presented in Section 3.3. and in the regression analysis shows that our treatment – the alternated smoking regime - does not induce a selection bias. We compare the odds ratio of a non-smoker deciding to sit[25] in the second room in the *Smoking Forbidden* treatment with the probability he or she makes the same choice in the *Smoking Allowed* treatment; the likelihood is equivalent. We also compare the smoke tolerance that guests report across the two treatments; again we find that smoke tolerance does not differ in the *Smoking Forbidden* and *Allowed* treatments. Additionally, *Smoke Tolerance* did resolve almost no variance in all the regressions we performed. If the treatment would indeed have caused guests who are less smoke tolerant to sit in the second room, these guests could be expected to react stronger to smoking and smoke exposure. Yet, in that case we should have observed *Smoke Tolerance* influencing the R^2 in our regressions.

The dummies for *Room* and *Days (1, 2, 3)* control for that neither a preference for the choice of room nor extraordinary events on a particular weekday (a Friday nights party, for example) distorted our results. Indeed, the main effect on both outcome variables remains unaffected by including the dummies. While we did not collect demographic data of the guests, some demographical factors may correlate with smoke tolerance, drinking and eating habits and the guests` time management. Yet, if demographics would systematically affect our results, their influence should be picked up by one of the regression dummies. For example, if people who have a higher tolerance for smoke would tend to drink and consume more or like to stay out longer, the *Room* dummy should reveal this tendency: since the guests in the main room have a comparatively lower smoke tolerance the room dummy should have indicated that guests in the second room stay out longer and drink more.

[25] Note that in Germany the staff of the bar does not seat guests. Guests are free to sit at any unreserved table.

As we explained above, the smoking ban is public law. Nevertheless, since the bar decides whether it utilizes the law's exception to allow smoking in one of its rooms, the prohibition may appear to some guests as being merely a private rule of the bar. Their response to a violation may differ depending on whether they perceive their personal entitlement as being granted by public law or privately by the bar. We cannot rule out this misperception, but it appears to be unlikely as the enactment of the public smoking ban was prominently debated in Germany. More over even if some guests may nevertheless have misunderstood the nature of the smoking prohibition, this misunderstanding should not undermine our results: the effect of the violation is likely only more intense if the right is perceived as being assigned by State law, rather than by the bar; thus a potential misunderstanding should only have weakened the effect we found.

Our main findings indicate that guests suffer a disutility when their rights are actively violated by smokers. As reported above, patrons were fully aware when they entered the second room that the ban would be violated by smokers and remain unenforced by the bar.[26] Even though non-smokers may have been better off sitting in the main room where their rights would not have been infringed, they voluntarily took a seat in the second room. While this behavior seems counterintuitive at first glance, it is in line with judgment and decision-making research. Individuals often fail to anticipate their emotional responses and reactions to events that occur in a different affective state (Loewenstein, 1999; Van Boven et al., 2004). Guests who took a seat in the second room plausibly had the expectation that they would be indifferent about smoking because they had a high tolerance for smoke exposure. Then the entitlement with the right granted by the smoking prohibition put them in a different situation and changed their response to smoking: Smoking did not merely expose them to smoke, what they anticipated, but it actively violated their right. Loewenstein refers to this failure of anticipation as an empathy gap.

The right violations that we observe are endogenous to the experiment as they are caused by the behavior of the smokers instead of being directly manipulated by our experimental treatment. Therefore, we might pick up interaction effects of treatment, smoker and non-smoker behavior that potentially may confound a clear interpretation of our results. However, all potential interaction effects we see would have worked against our findings. For instance, smokers in the *Smoking Allowed* treatment may be more likely to smoke because they expect that other guests do not mind if they light up. Indeed, the particle counter indicates that smokers light up more often when smoking is allowed. In turns their increased smoking may affect the behavior of non-smokers. However, if this effect was indeed at play, we would expect guests to leave earlier and consume less in the *Smoking Allowed* and not in the *Smoking Forbidden* treatment as we find where fewer people smoke and the exposure to smoke is lower.

This study does not aim to analyze the psychology of why people experience utility losses from the violation of their rights. Laboratory experiments that allow for a large

[26] This assumption is confirmed by the environmental factors (smoke, ashtrays) as well as the exit-survey. *Infra*.

set of manipulations are better suited than field studies to tease out the driving mechanisms. One plausible interpretation for the observed effect might be that rights are central to an individual's social integrity. Actively violating a right may thus disrespect or harm the social integrity of a person and induce discomfort even if no other substantial interests of the individual are affected. Our study is also not designed to predict what social equilibrium may evolve in a particular case when public law remains unenforced. The social outcome will likely depend on many contextual factors, for example whether potential violators or right holders are in the majority or how burdensome it is for right holders to engage in self-help to halt current and prevent future violations. In our study the bar's (illegal) policy might have induced protest if guests would not have had the option to avoid right infringements by sitting in the main room. While unenforced laws may cause multiple social equilibria, because of the complex interplay of actors, the cost of right violations that we identify may likely be present whenever rights are violated in social settings.

A potential alternative explanation for our results might be that the increased exposure to smoke when another patron started smoking in their presence may have made the bar's omission to enforce the prohibition salient to the guests. If so, non-smokers might have become annoyed with the lack of enforcement at the bar, making them leave earlier and consume less. However, as reported above, our data suggests that the bar`s omission to enforce the smoking ban was salient to all guests - whether a patron smoked in their presence or not - as soon as they entered the second room. The concentration of particles in the *Smoking Forbidden* treatment was 800% as high as in typical outside environments and even 1300% higher as in normal office space with a smoking prohibition in place. Indeed almost all guests in the exit interviews indicated that they understood that the bar did not enforce the ban and that they would be exposed to smoke when they took a seat in the second room. Exit interviews show that the lack of enforcement was just as salient to guests whose rights were not actively violated by smokers: these guests were exposed to a similarly high concentration of smoke ($27.209/cm^3$) and their answers in the exit survey did not differ from those of guests who were directly exposed to a smoker.

Notice as we measure the level of smoke each individual guest was exposed to we capture the increase in smoke exposure when someone started smoking in the presence of a guest. We account for this increase by including the variable *Smoke Exposure* in our regressions. As reported above, all regressions reveal that individual smoke exposure has no explanatory power for our two outcome variables. Guests respond to the violation of their right, but not to a difference in smoke exposure.

7.2. External Validity

Our field design avoids several methodological problems. Because our guests were not aware that their behavior was observed and subject to a scientific study, we avoid social or experimenter demand effects. In the context of a laboratory study participants' attention naturally focuses on the study's objective and participants may become more aware of their rights perceive them rights as being more important and respond with

greater sensitivity when they are violated. Under observation, guests may also feel an obligation to stop the illegal practice of the bar or may be less willing to support the bar with their consumption. Additionally, the natural sample of bar visitors (a mix of professionals, university and high school students) increases the reliability of our results.

The experimental design makes our study a hard test for the costs of unenforced laws and right violations that we analyze. Guests who decided to sit in the second room (without being accompanied by a smoker) reveal with their choice that protection from secondhand smoke is not an important priority for them. Indeed, exit interviews confirm that they were highly tolerant of smoke exposure. Many non-smokers however, approve smoking bans. It seems likely, that people who support the underlying rationale of a law will experience even stronger psychological disutility when this law is violated compared to the effect we found in our study. Second, a virtue of our experimental set-up is that it allows us to disentangle the physical harm smoking inflicts from the guests` reaction to the violation of their right itself. But many non-smokers will dislike being exposed to smoke and resent the physical harm it exposes them to as well. People, who resent this harm on top of the right infringement, can be expected to experience a more severe disutility from the unlawful behavior. Finally, in our study guests were aware that the bar would not enforce the smoking ban and that smokers would likely not respect the prohibition. By contrast, in many social contexts, right holders expect that their rights will be enforced and respected. Plausibly, the frustration with violations increases when people have a firm expectation of compliance that is disappointed. We therefore expect that our study results provide a rather conservative estimate of the disutility that people may experience in many social contexts when facing infringements of their rights.

8. POLICY IMPLICATIONS

8.1. Social Costs of Unenforced Laws

Scholars have long debated the costs and benefits of enacting rights States cannot or do not want to enforce effectively (Bickel, 1955; Fallon, 2006; Kagan 2007). For instance, when international treaties provide social and economic rights, such as a right to education, to rest and leisure, a right to housing, they set out universal living standard goals without providing mechanisms to ensure actual implementation or public enforcement. Other laws, such as public smoking bans, littering laws or speeding regulations, are difficult or expensive to enforce pragmatically.

These legal rights are often considered as “symbolic” or “aspirational”. They are valuable because they set goals the society can strive towards (Harvey, 2004; Starr, 2008; Hurwitz, 2003) even though States may not (yet) be able or willing to rigorously implement them. They also influence actual behavior: social norm scholarship has highlighted that unenforced laws can change people`s attitudes and promote compliance (Cooter, 1996, 1998). They set focal points and coordinate expectations of socially appropriate behavior people tend to correspond with (McAdams, 2000; Fisman and Miguel, 2007; Goldstein 2006).

While the literature has focused on the benefits of unenforced laws, less is known on potential costs. Our study explores a downside that has remained unnoticed both in theoretical as well as empirical scholarship. In the rights-as-options framework (Ayres, 2005) a legal entitlement makes the right holder better off or can leave him indifferent at worst. For instance, smoke-intolerant individuals can invoke public enforcement when a public smoking ban gives them a right that protects them from smoking. Even if the smoking ban would be widely ignored and remained unenforced, smoke-sensitive individuals are no be worse off than if no legal prohibition had been enacted in the first place. Our findings show however that rights are more than "options" right holders are free to exercise. Right holders can be negatively affected when the State assigns unenforced rights. Violations of these rights impose psychological costs that are distinct from the material harm the prohibited behavior itself may inflict. Therefore the entitlement with a right can make its holder worse off. When passing laws they are not able or willing to effectively enforce, lawmakers should carefully weigh off these potential costs to right holders against the benefits the unenforced laws may have.

8.2. Self-Help and Compliance as a Long Term Equilibrium

It is widely recognized that unenforced laws may alter the equilibrium of social behavior. Unenforced laws may change preferences or attitudes (Cooter, 1996, 1998) and adjust social expectations (Geisinger, 2002; McAdams, 2000; Bohnet & Cooter, 2003). In doing so, unenforced laws may set incentives for social enforcement and reduce its potential costs. For instance, if a law is widely supported private enforcement can earn social approval and esteem; also people may expect Infringers to give in more easily and stop their conduct when prompted, because the law stands against them (McAdams, 1997; Posner, 2002; Ellickson, 2001).

Beyond demonstrating the costs of infringements, our findings may also provide first insights into a novel mechanism of social enforcement. We show that individuals suffer a psychological cost when a right they are entitled to is violated. This cost is independent of the material consequences the infringing behavior may impose on them; right holders incur this cost even when they are indifferent about the material consequences like the health risk and odor of secondhand smoke. In our study guests did not confront smokers, because they knew the ban was not enforced when they chose to sit in the second room; also they could have easily moved to a smoke-free room.[27] In other situations however when right holders cannot as easily avoid violations, unenforced laws may cause a stronger demand for compliance. Right holders might engage in social enforcement merely to prevent their legal entitlement from being violated, independently of the material consequences the prohibited behavior has for them. Unenforced laws may thus tend to be self-fulfilling: By creating a right the law also classifies behavior as infringement. Since individuals incur a cost when their rights are disrespect-

[27] Individuals have a preference to avoid inconsistency, *see* Simon et al., 2004. Non-smokers might also realize that the bar would not have been supportive of confrontations and would prefer non-smokers to move to the main room when annoyed with secondhand smoke or violations of the prohibition.

ed, the law generates sensitivity towards this behavior prompting social pressure towards compliance and enforcement. This supports the perspective that unenforced laws can serve as useful starting points for social change, as emphasized by advocates of aspirational laws.

The psychological costs of infringement may also increase social enforcement by third parties. Third parties might intervene in order to solve the conflict between right holders and offenders. For example, the bar management endorsed the study because it wanted to learn how customers respond to that the bar does not enforce the smoking prohibition to appease smokers and non-smokers in the second room. When we shared our findings that even smoke-tolerant guests reacted negatively when smoking violated their rights, the management changed the policy and began enforcing the prohibition whenever it designated the second room as smoke-free. Similarly, public officials may be prompted to enforce the law when social conflicts occur. The psychological costs of unenforced laws that we identify may thus explain how laws can induce social enforcement even though the public has not (yet) internalized the values embodied in the law. Even without public support, unenforced laws may become self-fulfilling, causing pressure towards compliance and nudging the equilibrium of social behavior towards a desired outcome, as advocates of aspirational laws argue.[28]

9. CONCLUSION

Unenforced laws can be useful instruments of social change. Symbolic laws can change attitudes, coordinate social expectations and trigger third-party enforcement. But unenforced laws also impose hidden costs. Our field study shows that individuals are aggrieved and incur psychological costs when their personal rights remain unenforced and are violated by others. The disutility is independent of the material or physical harm that infringements may impose on the right holders. Rights are not merely costless options that can increase personal freedom. They can make individuals worse off, when they are disrespected. When legislators enact laws without providing resources to ensure enforcement or when policy-makers use executive discretion to leave rights unprotected, they should weigh the costs of unenforcement against the benefits the symbolic law may generate.

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[28] *Supra*__.

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APPENDIX -

ROBUSTNESS CHECKS

Table 5: OLS Regressions – Main Effects by Day

Dependent Variable	Time Day1	Consumption Day1	Time Day2	Consumption Day2	Time Day3	Consumption Day3
Dummy Treatment	-9.451**	-4.355***	-13.873***	-3.436***	-16.152***	-2.994
	(4.831)	(1.413)	(5.697)	(1.670)	(7.605)	(2.192)
Dummy Room	1.768	3.588	-3.614	0.186	-2.372	-1.199
	(6.647)	(1.945)	(8.717)	(1.938)	(11.306)	(3.259)
Smoke Tolerance	-4.173**	0.185	3.337	0.454	0.748	-0.922
	(2.127)	(0.622)	(2.067)	(0.459)	(2.402)	(0.692)
Smoke Exposure	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Group Size	0.077	-0.134	1.201	0.291	0.466	-0.219
	(1.669)	(0.488)	(1.757)	(0.390)	(2.014)	(0.580)
Constant	80.474	8.923	54.338	8.812	78.273	12.242
	(7.519)	(2.200)	(9.120)	(2.028)	(10.495)	(3.025)
R^2	0.11	0.11	0.18	0.16	0.16	0.09

Features and Functions of the TSI 3007

The TSI 3007 is a hand-held particle counter intended for measuring ultrafine particles for outdoor and indoor air quality monitoring and nanoparticle work area surveys. General applications are: Filter and air cleaner testing, atmospheric and climate studies, inhalation or exposure and health effects studies (for example exposure to second hand smoke),. This is the data sheet of the device:

Particle Size Range
Min. Detectable Particle (D_{50}) 10 nm
Max. Detectable Particle >1 µm

Concentration Range
0 to 100,000 particles/cm³

Minimum Displayable Concentration Value
1 particle/cm³

Concentration Accuracy
±20%

False Background Counts
<0.01 particles/cm³

Response Time
<9 sec for 95% response

Environmental Operating Conditions
Ambient Temperature 10 to 35°C (50 to 95°F)
Storage Temperature -40 to 70°C (-40 to 160°F)

Flow Rate
Detected Aerosol 100 cm³/min
Inlet 700 cm³/min (nominal)

Aerosol Inlet Diameter
¼-in. O.D.

Power Requirement
Battery Type 6 AA alkaline or rechargeable
Battery Life 5 hours (alkaline batteries at 21°C)

Alcohol Requirement
Type 99.5%+ reagent-grade isopropyl alcohol
Hours Per Fill 6 hours at 21°C (70°F)

RS-232 Output
9600 Baud rate

Software
Supplied with TSI Aerosol Instrument Manager® software, CPC Module

Calibration check
Recommended annually

Dimensions (L x W x H)
CPC 29.2 cm x 14 cm x 14 cm (11.5 in. x 5.5 in. x 5.5 in.)
Carrying Case 53 cm x 36 cm x 21 cm (21 in. x 14 in. x 8.3 in.)

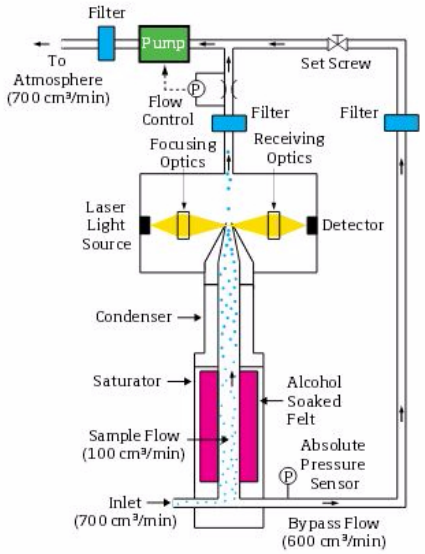
Weight
CPC with Batteries 1.7 kg (3.8 lbs)
Instrument with Accessories in Case 7.7 kg (16.8 lbs)

Software

Every Model 3007 is supplied with Aerosol Instrument Manager® software designed for use with Microsoft® Windows® operating systems. The software is used for instrument control and provides data collection, management, and export capabilities, as well as several choices for data display.

Operation

In general, laminar-flow CPCs operate by drawing an aerosol sample continuously through a heated saturator, in which alcohol is vaporized and diffuses into the sample stream. Together, the aerosol sample and alcohol vapor pass into a cooled condenser where the alcohol vapor becomes supersaturated and ready to condense. Particles present in the sample stream serve as condensation sites for the alcohol vapor. Once condensation begins, particles grow quickly into larger alcohol droplets and pass through an optical detector where they are counted easily.



Specifications are subject to change without notice.