

Aspirational Laws in Action: A Field Experiment

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This article examines aspirational laws in a randomized field experiment. We analyze the impact of an unenforced public smoking ban on individual behavior and attitudes. The findings indicate that aspirational laws, like public smoking bans, can make rights holders sensitive to behavior that violates their rights, irrespective of the material consequences of infringements and their personal views about the law. The results present a mixed position in the debate between rights-based social movement lawyering and critics of hollow rights. On the one hand, aspirational laws can create unforeseen social frictions when rights are declared, but their implementation and enforcement are ineffective. On the other hand, aspirational laws may also have self-fulfilling potential. Due to the adverse experience of rule breaking, rights holders may seek enforcement and compliance even if the law fails to influence public beliefs.

The role of aspirational law and the pursuit of legal rights in achieving social change have been the subject of intense debate in the law and society literature (see the symposium in Schmidt 2017). Some herald law reform strategies for their symbolic significance and potential to mobilize public and political forces for a cause. Similarly, a rich body of scholarship indicates that seeking legal rights via litigation can foster aspirations and legal awareness, promote empowerment and social mobilization, and bring attention to a cause by conveying a legal and political agenda (Scheingold 1974; McCann 1994, 1998; Lobel 2003, 2004; NeJaime 2011). Supporters of aspirational, symbolic laws claim that, even if a legal right remains unenforced, it may serve valuable aspirational purposes by setting goals that society may one day achieve (Harvey 2004; Fallon 2006; Kagan 2007). In other words, laws can have important symbolic effects even if a law remains without implementation or public enforcement. Similarly,

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expressive theories of the law suggest that norms may serve as focal points influencing expectations about socially desirable behavior (McAdams 2000a, 2000b) or tipping the social equilibrium into a better state (Becker 1968; Cooter 1996; Bohnet and Cooter 2003). Aspirational laws may cause the public to internalize the values expressed in laws and judicial opinions (Muir 1967; Cooter 1998).

Others admonish rights-based policy strategies for failing to provide effective reform (Berry, Nelson, and Nielsen 2017). Aspirational rights created by the courts are said to be ineffective if the other branches do not effectively support, implement, or enforce these legal rights (Handler 1978; Edelman 1992; Rosenberg 1992, 2004; Stefan 2000). Some argue that unenforced laws are also problematic because, when individuals observe widespread law breaking, the perceived lawlessness may erode the authority of, and public respect for, legal rules more generally (Friedman 1977; Schauer 2015, 2017). Although aspirational rights are a topic of feverish debate, empirical evidence on the actual effects of unenforced laws is scant (Goodman 2001; Bhattacharya and Daouk 2009).¹ In particular, we know very little about the impact unenforced laws have on rights holders. Proponents of aspirational law suggest that, even if a legal right is symbolic and unenforced, rights holders should be better off or at least be in the same position than if they had not been assigned a right in the first place (Lyons 1969, 1970). But how do individuals experience unenforced laws that are violated? In the absence of compliance, is being assigned unenforced rights the same as having no rights?

Our study helps to fill this empirical gap. In a novel randomized field experiment, we measure the effect of violations of an unenforced smoking ban on rights holders. We conducted our study in Germany during a time when public smoking laws had just been introduced but were still scarcely enforced and regularly violated. We chose a typical European bistro that, like many other German establishments, took advantage of an exemption in the German public smoking law. The law provides that, if bars, bistros, and restaurants have a main room where smoking is always prohibited, such an establishment can designate a separate room where smoking is permitted. The bistro, in which we conducted our study, sometimes utilizes this legal exception in its regular operations, designating the second room as a smoking area with a clarifying sign on the door as required by the law. At other times, the bistro does not provide this possibility, and the standard public smoking ban applies to the second room. However, the establishment never enforces the smoking law in the second room.

To conduct our study, the bistro allowed us to randomly assign the smoking regime in the second room. In the first treatment, we instructed the bistro to use the exception and designate the second room as a smoking area with the explanatory sign on the door. In the second treatment, the exception was not applied so that the public law banned smoking in the entire bistro. We requested the bistro to adhere to its standard practice of not enforcing the smoking ban in the second room. Patrons constantly violated the ban. Since the second room was dense with smoke during both treatments, we assumed, and our data show that, patrons who decided to sit in the second room were tolerant of

1. Ryan Goodman (2001) presents ethnographic research conducted in South Africa before and after the abolition of sodomy laws, examining the influence of the criminalization of homosexuality on norms and institutional discourses, including religion and medicine. Utpal Bhattacharya and Hazem Daouk (2009) show, theoretically and empirically, how unenforced prohibitions on insider trading may create an equilibrium in which violators cause law abiders more harm than in the absence of a ban on insider trading.

second-hand smoke.² This setting allows us to examine the effect of right infringements: since non-smoking patrons in the second room were smoke tolerant, comparing their reactions to smoking when the ban was in place to when smoking was allowed, would reveal the impact of infringements of the smoking ban on their behavior.

The results show that non-smokers in the second room reacted adversely to smoking but only when it violated the public smoking ban. An electronic air particle counter indicated that there was less second-hand smoke in the air when the unenforced smoking ban was in place and was violated by smokers. However, non-smokers left the bistro earlier and consumed less food and beverages when the public smoking ban was in effect compared to when smoking was allowed. We support our findings with several measures. We conducted exit interviews to confirm whether guests were aware if the ban was in place or not and whether guests were aware that the bistro did not enforce the ban and to examine the opinions of guests about public smoking. As mentioned, we used a particle counting device to verify that the non-smoking patrons in the second room were subjected to high levels of second-hand smoke during both treatments. We used these measures in our regression analysis. The findings are presented in the sections on experimental design and results.

Our results show that non-smoking patrons leave earlier and consume less in the treatment where smoking was forbidden and their right to a smoke-free environment was violated compared to the treatment in which smoking was allowed. The findings yield mixed conclusions in the debate about aspirational laws. On the one hand, our findings indicate that being allocated a symbolic right can make beneficiaries worse off in certain situations. Once assigned a legal right, individuals may become sensitive to violations of that right even if they are indifferent to the prohibited behavior itself. The non-smokers in our study, for instance, were insensitive to second-hand smoke, but they reacted adversely when the public smoking ban assigned the right to be free from smoke and that right was violated. Having an unenforced right that is frequently violated can burden individuals with an adverse experience of violations that would be absent without the aspirational law. On the other hand, our findings point to a psychological mechanism that might spur a self-fulfilling potential of unenforced laws. Given how infractions of symbolic rights are experienced negatively by rights holders, aggrieved rights holders might mount pressure to have the public smoking ban more widely enforced. Additionally, the prospect of being reprimanded by aggrieved rights holders might induce voluntary compliance with the law. These forces may help tip behavior to an equilibrium where compliance with an unenforced law becomes commonplace even if the public does not fully embrace the law and its policy objectives. This aspect of our findings is encouraging to proponents of aspirational laws and social movement lawyering.

Our article engages with several related areas of scholarship. First, our field study contributes to the understanding of law as an instrument of social control. Public smoking laws are difficult and costly to enforce while also being, at least initially, quite controversial in many countries. Interestingly, however, compliance with public smoking laws has been strong, even in countries where public smoking was common

2. As described below, we confirmed this assumption by measuring smoke concentrations in the room and conducting exit interviews that reveal patrons' smoke tolerance and health concerns.

before the new laws. While much of the literature views compliance as resulting primarily from deterrence (Becker 1968; Stigler 1970; Schauer 2015, 2017) or legitimacy (Tyler 2006; Wenzel and Jobling 2006; Tyler and Jackson 2014; Nadler 2017), others have explained that laws may have an expressive effect by coordinating behavior, informing beliefs (McAdams 1997, 2000a, 2000b, 2015; McAdams and Nadler 2005, 2008; see also Lessig 1995; Kahan 1997), or even shaping preferences (Sunstein 1996; Cooter 1998, 2000), leading people to eschew the prohibited conduct, even in the absence of a credible sanction. We expand on this work by pointing to an alternative social mechanism. Even where aspirational laws lack expressive power due to their controversial nature, the negative experience of infringements when the law is violated, may lead rights holders to prefer that the laws are effectively implemented and enforced. Accordingly, our study also contributes to the debate on the mobilizing effect of rights.

FIELD SETTING

Legal Background

Not all laws are enforced equally. Some laws remain unenforced because the executive branch purposely turns a blind eye to legal violations.³ Sometimes under-enforcement is fully anticipated by legislators. International treaties, for instance, commonly include social and economic rights without providing mechanisms to ensure implementation or public enforcement (Chilton and Versteeg 2017). Other laws, such as public smoking bans, littering laws, or speeding regulations, are simply too difficult or expensive to enforce comprehensively. We conducted our study in a bistro in Lower Saxony, Germany. German federal law prohibits smoking in public places, but many states permit exceptions for bistros, restaurants, and other public establishments. One such exception is central to our study: Lower Saxony's state law permits smoking in explicitly designated smoking rooms. This exception is reserved only for businesses that operate two separate rooms, one where smoking may be allowed and one where it is strictly and always prohibited.⁴ The establishments can decide whether they want to meet the requirements of this exception. If they do, smoking is lawful within the limits prescribed by the smoking ban. Otherwise, the exception does not apply, and German federal law prohibits smoking in the entire establishment. For example, if a guest lights

3. For example, during the 2009–12 term, the Obama administration announced a policy of not investigating and prosecuting certain federal marijuana crimes, postponed enforcement of several key provisions of the Affordable Care Act, and suspended enforcement of removal statutes against certain undocumented immigrants. See Delahunty and Yoo 2013; Price 2013.

4. The smoking ban is regulated in Lower Saxony's law for the protection from the dangers of Secondhand Smoke, Nds. NiRSG Rule 2(6), defines exceptions to the general prohibition under certain conditions. If a bistro has two rooms, smoking can be permitted in the second room if (1) the second room is smaller and has no counter and (2) the room is specifically designated as a smoking room. Note that without the specific designation as a smoking room, the exception of Rule 2(6) is not met, and the general public smoking ban of Rule 1(1) applies throughout the bistro.

up in the second room when the bistro or the restaurant did not designate it as a smoking area, the guest violates the state law prohibition.

The Bistro's Practice before the Experiment

As required by law, the bistro operated two separate rooms. In the main room, smoking was always prohibited, and the staff strictly enforced the smoking ban. In the second room, the bistro alternated the smoking regime. On some days, the management would designate the second room as a smoking area, making use of the exception of Lower Saxony's smoking law, while, at other times, the bistro would remove the sign such that smoking was prohibited by the public smoking ban.⁵ In contrast to the main room, however, the bistro never enforced the public smoking law in the second room whenever management designated the room as a non-smoking area.⁶ The bistro provided ashtrays in the second room regardless of the smoking regime, and staff members never confronted patrons about smoking. If a guest inquired about the smoking ban, the staff explained that they would not enforce the prohibition and pointed out to patrons that the ban was strictly enforced and obeyed in the main room. Due to the absence of enforcement, guests regularly smoked in the second room, even when smoking was forbidden. Consequently, non-smokers were always subjected to second-hand smoke in the second room, independently of the experimental treatment. As we describe below, many guests were quite tolerant of second-hand smoke. Likely because the smoking ban had just been introduced in Germany, patrons were still accustomed to being exposed to smoke in public places.

Prior to the study, the bistro designated the second room as a non-smoking area to accommodate guests who could not be seated in the main room when it was very crowded.⁷ The bistro expected that most smokers would abstain from smoking or reduce their consumption to comply with the unenforced law. Meanwhile, the bistro sought to appease the smokers in the second room by not enforcing the ban there.

Natural Venue

The establishment fits the typical profile of any typical European bistro that serves alcohol, coffee, and typical food. Guests visit the establishment for refreshments, to eat a light meal with companions, or to work or study. Patrons can easily move from one room to the other. Both rooms are comparable in size, height, decoration, and distance

5. The separation of rooms is one of the major exemptions in the German smoking law, and it was common at the time for public establishments to make use of it—to soften the sudden impact of the law on their smoking guests as they reorganized to adapt to the new law. Alternating the smoking regime in the room was also commonplace to be flexible and accommodate capacity issues.

6. The bistro 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 were rare in Germany, however, and, therefore, owners rarely enforced the public smoking ban. More than 60 percent of establishments in Germany were reported to have violated the law (*Der Spiegel* 2008).

7. The main room was never crowded during our study.

from the bar.⁸ The service was identical in both rooms, and the menu stayed the same throughout the study. There was little interaction between guests who were not sitting at the same table. People ate and drank with their companions at their own table.

EXPERIMENTAL DESIGN

Treatments and Manipulation Check

Treatments

Our study is a field experiment with a randomized treatment manipulation. The bistro permitted us to apply an experimental protocol where we randomly alternated two experimental treatments. During the *No Prohibition* treatment, the bistro designated the second room as a smoking area and, as required by law, posted a sign on the door to indicate that an exception to the ban was in place. In this treatment, smoking did not violate any (unenforced) right of guests to be free from second-hand smoke. In the *Violated Prohibition* treatment, the bistro took down the sign, and, therefore, smoking was prohibited by law. We instructed the bistro to never prevent guests from smoking in the second room, irrespective of the treatment assigned to the room. Since the ban was never enforced, many guests were smoking in the second room in both treatments. When the ban was in place, in the *Violated Prohibition* treatment, guests' rights to a smoke-free environment were violated.

By comparing the two treatments we can examine the effect rights violations have on patron's behavior in the bistro. To support the robustness of our findings, we also compared our two treatments to a third treatment in the front room. In this room, guests were never exposed to smoke, and their rights were upheld as the staff strictly enforced the ban. If guests in the second room dislike smoking only because it violates their rights granted by the ban, guests in the *No Prohibition* treatment should behave like the guests in the main room because neither group experienced violations of their rights. By contrast, subjects in the second room in the *Violated Prohibition* treatment should respond adversely compared to the guests in the main room, as their rights were infringed (see [Table 1](#)).

Manipulation Checks

To ensure that the patrons were aware of the treatment, we performed two manipulation checks in our study. First, in the planning stage before the experiment and then in exit interviews after the study. Both in the pre-experimental check as well as in the exit interviews, the patrons indicated (1) that they were aware of whether smoking was allowed or prohibited in the second room and (2) that, since the bistro did not

8. In the exit interviews, main room guests pointed out that they chose the main room because of their sensitivity to second-hand smoke. The aesthetics or the décor of the rooms did not influence the decision to sit in one room over the other.

TABLE 1.
Treatments and robustness checks

	<i>Violated Prohibition</i>	<i>No Prohibition</i>	<i>Enforced Prohibition (main room)</i>
Prohibition	Yes	No	Yes
Enforcement	No	×	Yes
Smoke exposure	Yes	Yes	No
Right violation	Yes	×	No

enforce the general prohibition law in the second room, guests would smoke there. Patrons understood that the sign utilized an exception to the general ban.⁹

Main Observations

Time and Consumption

We measured two dependent variables: (1) the time non-smoking guests spent at the bistro, measured in minutes from getting seated until departure, and (2) the amount of money guests spent on food and beverages. For calculating consumption, the staff provided us with the guests' payments.

Right Violation and Facing a Smoker

As our independent variable, we measured whether a guest in the second room was in the room with someone who smoked. Depending on the treatment, smoking is either a breach of legal rights when prohibited by the bistro, or it is a legally permissible activity when smoking is permitted. Thus, the interaction term "Smoking*Legal_Regime" allows us to compare non-smoking guests who were exposed to smoking and whose rights were violated in the *Right Violation* treatment with those guests who were exposed to smokers who did not violate their rights in the *No Prohibition* treatment.

Groups of Guests: Independence of Observations

To ensure that our main observations were independent, we treated a group of guests as one single observation regardless of the number of guests in the group.¹⁰ The

9. Note that, even if patrons had not seen the sign or were otherwise unclear about the nuances of the law, the exit interviews enabled us to confirm that patrons were aware whether smoking was allowed or prohibited under the two treatments. The exact understanding of the law among patrons is not relevant to our study.

10. As a further robustness check, we also measured for every eighth guest, the time between a meal order and the meal being served to ensure that patrons did not systematically wait longer in one condition than in another.

reason for this decision was that members of a group usually decide together when to leave. As a result, the time they spent at the bistro and their consumption of food and beverages were highly correlated and not independent of one another.¹¹ In addition, if a smoker was part of a group, we did not include that group in our sample as the smoker may have caused the group to take a table in the second room even if individual members were not indifferent about smoke exposure. Also, the smoker may have asked for the non-smoking companion's consent before lighting up. The non-smoker's consent could have influenced their attitude toward violations of the smoking ban. We excluded about the same number of groups that involved at least one smoker in both treatments. To confirm the robustness of our results, we also controlled for group size in our regression analysis.

Control Variables

We elicited a set of control variables to ensure that the behavior of patrons was driven by the violations of the smoking ban and not for other reasons.

Exit Interviews

First, we conducted an exit interview as guests left the bistro (see Appendix B, which had an 80 percent response rate). In the interview (in addition to the manipulation check), we inquired about:

- (1) individual sensitivity to second-hand smoke (measured using a Likert item ranging on an ordinal scale from 1 = not sensitive at all to 7 = very sensitive) to rule out the possibility that, in the *Right Violation* treatment, guests with a higher smoke sensitivity moved to the second room;
- (2) attitudes toward smoking and the health protection provided by the smoking prohibition (Likert item ranging on an ordinal scale from 1 = not important at all to 7 = very important) to check whether attitudes were expressively changed by the smoking ban;
- (3) preferences for features of the second room that may have made guests choose to sit there (Likert 1–7) even though they would otherwise have stayed in the smoke-free front room.

We used these three variables (sensitivity, protection, room preference) in our regression analysis, and none of them explains why guests leave earlier or consume less in the *Violated Prohibition* treatment.

11. The data within the groups of non-smokers is highly correlated. This is most obvious for the variable time: most guests who visit the bistro together also leave together. The within-group correlation is also very high for the data on spending. Groups typically have a common purpose, like having a drink or consuming a meal, when they come to the bistro. When a new round is ordered, usually everyone is asked what to drink and eat. And consumption is highly correlated also with time spent at the bistro. Group members are likely to stay in the bistro for about the same time. Because of the high within-group correlation, these observations are also not independent, and we use each group as one observation. If we treated these guests as individual observations, we would increase our *N* by a factor of about two, while the data points would entail very little variance. Using individual-level data instead of group-level data would therefore work in our favor, boosting the power of the analysis.

Measuring Individual Smoke Exposure

To control for the impact of smoke exposure on the behavior of customers, we installed a device in the second room that measured the concentration of particles in the air.¹² Guests could not see or hear the device. The device is very sensitive and measures particles ranging in size from 0.01 to 1.0 micrometer (μm). By comparison, in urban environments, the air outside typically contains about 4,000 particles per cubic centimeter of particles (depending on environmental pollution). Conversely, the typical particle concentration for non-smoking indoor office space is as low as 2,500 particles per cubic centimeter particles. The particle counter elicited the smoke concentration every thirty seconds. The measurements allowed us to determine the smoke concentration that each guest was exposed to during their visit to the bistro. For example, if a guest stayed for thirty minutes, we collected sixty measurements for that patron. We calculated the average of these values by constructing a score that captures each patron's individual exposure to smoke. On average, we observed a particle concentration of 32,117 per cubic centimeter in the *Violated Prohibition* treatment—that is, almost thirteen times more measurable particles than present in a typical office space.

The results show that smoke exposure was 25.2 percent higher when guests were allowed to smoke than when smoking was forbidden (*No Prohibition*: 40,230 particles per cubic centimeter versus *Violated Prohibition*: 32,117 per cubic centimeter; t-test p-value < 0.01). The difference is consistent with the bistro's expectation that some smokers would feel obliged to comply with the smoking ban or reduce their smoking frequency while the smoking ban was in effect in the room. Since the mean concentration of particles was lower when smoking was forbidden, a higher level of smoke exposure cannot cause our main effect.

Difference in Smoke Exposure across Treatments Does Not Motivate Room Choice

Although smoke concentration marginally differed across treatments, smoke-sensitive patrons are likely to stay in the first room in both treatments. While the level of smoke exposure in the main room was always low (we measured only about 4,000 particles per cubic centimeter in the main room), smoke exposure in the second room was at least eight times higher than in the main room irrespective of the treatment. Thus, the level of smoke concentration is always so bothersome in the second room (even when smoking is prohibited) that sensitive non-smokers will always avoid the second room. Our sensitivity data also confirms that the marginal difference in smoke exposure across treatments does not cause guests to leave earlier or consume less.

We used smoke exposure as a control variable in our regressions to show that smoke exposure does not cause guests in the second room to leave the bistro earlier or

12. We utilized a TSI CPC 3007 particle counter. 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 on how to use the device (details of the device can be found at "Condensation Particle Counter 3007," <https://tsi.com/products/particle-counters-and-detectors/condensation-particle-counters/condensation-particle-counter-3007/>).

spend less money there. This variable allowed us to directly control for the exact concentration of smoke that each guest was individually exposed to at the bistro. Our regression analysis confirmed that the level of smoke exposure did not change the behavior of guests. We report the results below, along with the main effects.¹³

Ruling Out Selection Bias

Our study's design focuses on the non-smoking guests of the bistro who, by sitting in the second room, revealed that they were not concerned about second-hand smoke. These guests are of particular interest because, if we can show that they are aggrieved by a violation of the smoking ban, this must be due to the infringement of their rights and not due to sensitivity to second-hand smoke or health concerns.¹⁴ For that reason, we filtered guests according to their sensitivity to smoke. Only the guests who are relatively insensitive to smoke were expected to take a seat in the second room of the bistro where they fully expect to be exposed to smoking. However, we had to rule out a potential selection bias: the *No Prohibition* and *Violated Prohibition* treatments were in place when the guests decided whether to enter the second room. As a result, more smoke-sensitive guests may take a seat in the second room if they expect that guests will abstain from smoking when the prohibition is in effect there (*Violated Prohibition* treatment). Therefore, it is possible that the treatments, and not only the guests' sensitivity to smoke, may have influenced the decision of the guests to take a seat in the second room.

Our data of the particle counter and the exit interviews allow us to reject this potential selection bias. Since the bistro never enforced the prohibition in the second room, the air was always filled with smoke. The visibility of the smoke and its smell help to explain why almost all guests stated in the exit interviews that they were fully aware that patrons would smoke in the second room even when smoking was forbidden.¹⁵ The particle counter measurements demonstrate that it was very evident to non-smokers that people were smoking in the room: the smoke concentration averaged 32,117 particles per cubic centimeter, which is eight times higher than in typical outdoor environments and almost thirteen times higher than in a ventilated office space. By contrast, the difference between the values in the *Violated Prohibition* condition and the *No Prohibition* treatment (40,230 per cubic centimeter) is much smaller in comparison (25.2 percent).¹⁶ Therefore, only guests indifferent to smoke exposure should have taken a seat in the second room, regardless of the implemented treatment: *Violated*

13. 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 found no such effects.

14. Our approach can be compared to a laboratory experiment that is interested in prosocial participants. In such studies, potential participants are screened and filtered, and only those with prosocial preferences are enrolled.

15. Over the course of the entire experiment, we observed only a few customers moving to the non-smoking area once they had observed other guests smoking in the second room.

16. This result also holds when we rely on our observation to distinguish smokers from non-smokers. In this case, we treated any guest as a non-smoker that we did not observe smoking during their visit at the bistro.

Prohibition or *No Prohibition*. In turn, a person who is somewhat less tolerant of smoke exposure should prefer to sit in the main room where smoking is strictly forbidden.

Next, we used the self-reported smoke sensitivity data from the exit interviews to rule out selection bias. As reported above, we asked guests about their tolerance of second-hand smoke using a Likert item with an ordinal scale ranging from 1 to 7 (1 = not sensitive at all; 7 = very sensitive). We obtained an average of 2.04 for customers in the *Violated Prohibition* treatment (95 percent confidence interval lower bound 1.83, upper bound 2.19). In comparison, we elicited a similar average of 1.89 (95 percent confidence interval lower bound 1.73, upper bound 2.05) in the *No Prohibition* treatment. The difference is not significant (t-test p -value = 0.35). Finally, the standard deviations in the two treatments are not significantly different from each other, rejecting the notion that random differences in variance are biasing our results. As expected, guests in the main room are significantly more sensitive to second-hand smoke (2.68) compared to the non-smokers in both treatments in the second room (t-test p -value < 0.01).¹⁷ As a robustness check, we split the data collected in the main room into two samples: (1) the observations we collected while the *Violated Prohibition* treatment was implemented in the second room and (2) the observations we collected while the *No Prohibition* treatment was implemented in the second room. If more smoke-sensitive guests had taken a seat in the second room instead of the main room while the *Violated Prohibition* treatment was implemented, then the values we elicited in the main room should have changed as well. We did not find a difference between the two values (t-test p -value = 0.31; *Violated Prohibition* 95 percent confidence interval lower bound 2.35, upper bound 3.07, *No Prohibition* 95 percent confidence interval lower bound 2.14, upper bound 2.73) supporting the finding that the treatment does not affect patrons' room choice.

By examining the decision of guests to sit in the first or second room, we found that the treatment did not influence the sample of guests who decided to sit in the second room. We determined the odds ratio for a non-smoker to sit in the second room. We then compared the odds ratio under the two treatments that we implemented in the second room. If the odds ratios were the same, it would suggest that the treatment did not affect the guest's decision. We learned whether guests were smokers by observing their behavior in the bistro and by collecting information in the exit interviews. Under the *Violated Prohibition* treatment, the likelihood that a non-smoker would sit in the second room was 0.29, while under the *No Prohibition* treatment, the likelihood was 0.37. If non-smokers had refrained from sitting in the second room because they expected it to be smoke-free in the *No Violation* treatment, then the odds for non-smokers to sit in the second room when smoking was forbidden would have to be higher. However, the odds ratio does not differ between treatments. If the odds of a non-smoker sitting in the second room were the same in both treatments, the odds ratio would be 1. The odds measured in the *No Prohibition* treatment were 1.32 times higher than the odds of a non-smoker appearing in the second room during the *Violated Prohibition* treatment. As the odds ratio of 1 lies within the 95 percent confidence

17. The smoke sensitivity of guests does not differ in any of our treatments substantially across the three days of observation (two sided Kruskal-Wallis (KW): *No Prohibition* KW p -value 0.77; *Violation Prohibition* KW p -value 0.40; *No Smoking* KW p -value 0.93).

interval of the odds (1.32) that we measured in our study (the lower bound is $0.95 < 1$), we can reject the idea that the treatments influenced the guests' choice of room.¹⁸

We also included the variable concerning smoke sensitivity in our regressions. If the treatment (*Violated Prohibition*) had induced more smoke-sensitive guests to get seated in the second room, and if this higher sensitivity had caused them to leave earlier and consume less, this variable should have an impact on the regressions. Yet smoke sensitivity does not change the coefficient of determination (R^2) in our regressions, suggesting that it resolves almost no variance in our data. The results suggest that the decision on where to sit was not influenced by the treatments we assigned. This finding aligns with the exit interviews: patrons in both treatments stated that they were fully aware that there would be smoking when they decided to get seated in the second room.¹⁹

We also verified whether seating was always available in the main room during study observation times. This helps to reject a potential bias in the sample of guests treated in the second room. If the main room had been crowded, patrons may have decided to sit in the second room even if they otherwise would have preferred a table in the smoke-free area.²⁰ Finally, guests in the first and the second room did not experience wait times for a table. Our data show that guests were served about as quickly in both rooms.

Hypothesis

Our two main treatments alter the smoking regime in the second room: smoking is allowed or legally prohibited by the public ban. Additionally, the control group in the front room helps serve as a robustness check. We hypothesize that non-smokers adversely experience violations of their legal entitlement, even when they do not care about the behavior from which the law protects them from. Thus, in the context of our study, if non-smokers adversely experience violations of their right to be free from smoke, we should observe an adverse reaction to smoking in the second room when the smoking ban is in effect in the second room and violated (*Violated Prohibition*) but not when smoking is allowed. By contrast, in the *No Prohibition* treatment, smoking should not affect the experience of guests in the bistro since they are not entitled to a smoke-free environment. Conversely, if the infractions of the unenforced smoking ban had left rights holders unaffected, we should not notice a difference in behavior among non-smokers across both *Violated Prohibition* and the *No Prohibition* treatments.

We used two dependent variables as proxies for the behavior of guests at the bistro: time spent at the bistro and consumption of beverages and food. As we try to measure the same outcome with both variables (utility derived from visiting the bistro), we

18. In general, the values that subjects indicated seem surprisingly low. However, the absolute values should not concern the treatment effects that we are interested in.

19. Non-smokers did not anticipate their own adverse experience of violations of their rights when smoking was forbidden in the second room. We discuss this empathy gap below in the Internal Validity section.

20. If the first room had become overcrowded, we would have paused collecting data and resumed once (1) space became available in the first room and (2) all guests who had taken a seat in the second room, while the first room was full, had left the second room.

expected the variables to be highly correlated. The two variables allowed us to measure the utility derived from the visit along two dimensions: time and money. We expected that guests would leave the establishment prematurely (*Hypothesis 1a*) and consume less (*Hypothesis 2a*) when smoking was forbidden compared to the *No Prohibition* treatment. As a robustness check, we expected to see the same effect when comparing the behavior of non-smokers in the *Violated Prohibition* treatment with the guests in the *Enforced Prohibition* condition of the main room. Since the smoking prohibition was strictly enforced in the main room, non-smokers experienced no rights violations. Therefore, if our hypothesis holds, guests seated in the *Violated Prohibition* treatment should have left the bistro earlier (*Hypothesis 1b*) and consumed less (*Hypothesis 2b*) than the guests in the *Enforced Prohibition* condition.

Finally, we conducted a robustness check on the impact of smoke exposure. We expected that smoke exposure would not affect the guests' behavior in the second, smoky room. Guests in the *No Prohibition* treatment were subjected to high smoke concentrations but did not experience rights violations. Therefore, as these guests did not experience any rights violations, they should behave no differently than the guests in the main room in the *Enforced Prohibition* condition, who were not exposed to any smoke but whose rights were not violated either. Thus, when comparing the guests in the *Enforced Prohibition* condition and the *No Prohibition* treatment, we did not expect to find any differences concerning the time guests spent at the bistro (*Hypothesis 1c*) and the amount consumed (*Hypothesis 2c*).

Methods

Demand Effects and Field Design

Customers were unaware that a study was conducted until they were surveyed upon exiting the bistro. Therefore, we can exclude the possibility that guests changed their behavior, either deliberately or subconsciously, because of being observed. We did not disclose the purpose of the study to the staff until after the study was completed. The bistro's management endorsed the project because it wanted to examine the commercial impact of the alternating smoking regime.

Repeat Guests

We asked patrons in the exit interviews if they had visited the bistro on any of the days where we collected observations. The question in the questionnaire was as follows: "Did you visit this bistro on any of these preceding days (observation days)?" Patrons who answered yes were excluded from the sample. We wanted to control for repeat visitors because their prior experience may have changed their behavior compared to the other guests in ways unobservable to us.²¹

21. A patron's prior experience may affect the results in unobservable ways. For example, it would be difficult to collect information about the experience of repeat patrons on previous visits (including which room they sat in, what regime was in place, how many smokers were present, and so on). In our setting,

Robust Data Collection

To support the robustness of our results, we collected observations for each treatment on Friday, Saturday, and Sundays on three consecutive weekends. We alternated the order of the treatments such that we collected for each treatment observations on a Friday, a Saturday, and a Sunday from different weekends (Weekend 1: Friday - *Violated Prohibition*; Saturday - *No Prohibition*; Sunday - *Enforced Prohibition*; Weekend 2: Friday - *No Prohibition*; Saturday - *Enforced Prohibition*; Sunday - *Violated Prohibition*; Weekend 3: Friday - *Enforced Prohibition*; Saturday - *Violated Prohibition*; Sunday - *No Prohibition*). We randomly assigned the treatments to the experiment days, generating a separate data set each weekend. As we will see in the regression analysis, the results did not differ across the three weekends.

We collected our observations from the bistro's opening in the late morning until closing time. We did not alternate treatments during the day. No major public events (elections or sporting events, for example) took place during the days of the experiment. Nothing out of the ordinary occurred to affect the behavior of many guests simultaneously.²² The experimental setup remained constant across conditions. The same service staff was working in the bistro throughout the experiment, and, in the second room, the staff never requested guests to stop smoking. Conversely, the smoking prohibition was always strictly enforced in the main room. The only difference in the location between treatments was the random assignment of the public smoking regime—that is, whether public law allowed or prohibited smoking in the second room.

Observations

We obtained 244 independent observations of non-smokers in total. We collected seventy-six observations in the *Violated Prohibition* treatment, eighty-one observations during the *No Prohibition* treatment, and eighty-seven observations in the *Enforced Prohibition* treatment in the main room. Importantly, we also elicited direct evidence on the influence of rights violations by observing whether someone smoked when a guest stayed in the second room. In the *Violated Prohibition* treatment, we obtained twenty-two observations of guests whose rights were not violated by a smoker and fifty-four observations of non-smokers whose rights were infringed. In the *No Prohibition* treatment, seventy-three guests or groups of guests were exposed to another guest who smoked in their presence; eight guests or groups of guests were not exposed to smoking. We continuously measured the smoke concentration in the second room with the particle counter.

repeat guests who are insensitive to smoke and who had experienced right violations by sitting in the second room, may be more likely to stay in the front room on their next visit. To answer this separate research question, however, we would need to collect more data than is feasible in a field experiment that relies on the cooperation of guests and a bistro that is concerned with disturbing the natural venue.

22. For example, events such as election days, major concerts, or sporting events on television may bring many patrons in or out of the bistro at the same time on one particular day (remember, only one treatment was implemented per day) influencing their time of stay and consumption and thus biasing the treatment comparison.

Exit Interviews

The exit interviews (Appendix B) were conducted after the non-smoking guests left the second room. By keeping the interview brief and focused, we achieved a participation rate of 85 percent of the observations in our sample.²³ By recording the time when they left their seats and the time when they completed the interview, we matched the exit interview data with the observations of the main variables. Some visitors declined to be interviewed because of privacy concerns or because they did not want to invest the time.

RESULTS

We compared the two dependent variables—time and consumption—across the *No Prohibition* and *Violated Prohibition* treatments to measure the main treatment effect. As a robustness check, we also compared the results in both treatments to the behavior of guests in the main room in the *Enforced Prohibition* condition. In addition, to provide direct evidence that rights infringements impose a psychological cost, we distinguished in our *Violated Prohibition* treatment between guests whose rights were violated by smokers and those guests who did not encounter a smoking patron whilst staying in the bistro. We present direct treatment comparisons and then report regression models that include all data and control variables.

Treatment Effects

We measured the dependent variables - time and consumption - as proxies for the utility that guests derive from their visit to the bistro. In this section, we report the results of the main treatment comparisons in Hypothesis 1:

Non-smokers spend less time and money in the bistro in the Violated Prohibition treatment compared to the No Prohibition treatment (Hypothesis 1a) and the guests in the main room in the Enforced Prohibition condition (Hypothesis 1b). We expect to find no difference between the No Prohibition treatment and the guests in the Enforced Prohibition condition in the main room (Hypothesis 1c).

Treatment Effects on Time

We measured the time between when a customer took a seat and when they left the room.²⁴ The data indicated that non-smokers in the *Violated Prohibition* treatment

23. In order to ensure an effective participation rate, we limited the survey to obtaining the most important information for the study. Because “subjects” did not choose to participate in our experiment and were asked to complete a questionnaire without payment, we did not obtain information that might otherwise have been useful. For example, we did not ask participants for demographic information or the reason for leaving the bistro. We were also concerned that personal questions might be perceived as an invasion of privacy. We balanced the collection of additional data against the risk of reducing participation.

24. As indicated above, we also measured the time it took for a guest to be served in both rooms and observed no difference.

TABLE 2.
Treatment comparisons

	Time (minutes)	Consume (Euros)	Sensitivity (Likert 1–7)	Protection (Likert 1–7)
<i>Violated Prohibition</i> (<i>N</i> = 76)	53.89	6.78	1.89	2.57
<i>No Prohibition</i> (<i>N</i> = 81)	66.09 <i>p</i> < 0.01***	10.15 <i>p</i> < 0.01**	2.04 <i>p</i> = 0.25	2.64 <i>p</i> = 0.58
<i>Enforced Prohibition</i> (<i>N</i> = 87)	69.31 <i>p</i> < 0.01***	9.67 <i>p</i> < 0.01***	2.61 <i>p</i> < 0.01***	2.91 <i>p</i> = 0.03**
<i>Enforced Prohibition</i> (<i>N</i> = 87)	versus <i>No Prohibition</i> <i>p</i> = 0.33	versus <i>No Prohibition</i> <i>p</i> = 0.57	versus <i>No Prohibition</i> <i>p</i> = 0.01	versus <i>No Prohibition</i> <i>p</i> = 0.07*

Notes: All *p*-values are two-sided *t*-tests. *, **, *** indicate significance at the 10 percent, 5 percent, and 1 percent level.

remained at the bistro for a significantly shorter amount of time than the guests in the *No Prohibition* treatment (*Hypothesis 1a*: 53.89 versus 66.09 minutes; *t*-test *p*-value < 0.01) (see Table 2). We also compared the behavior of non-smokers in the *Violated Prohibition* treatment to that of the guests in the main room (*Enforced Prohibition*). Guests in the *Enforced Prohibition* condition in the main room whose rights were not violated stayed for a significantly longer time at the bistro (*Hypothesis 1b*: 53.89 versus 69.31 minutes; *t*-test *p*-value < 0.01).

Finally, we compared the results of the *No Prohibition* treatment with the behavior of the guests in the main room (*Enforced Prohibition*). Neither group experienced a violation of a legal right (either because the ban was enforced or because smoking was allowed). The difference between the two groups is their actual exposure to smoke. As expected (*Hypothesis 1c*), smoke exposure did not cause a treatment difference: we found an insignificant *t*-test result with a *p*-value of 0.31 and values for *No Prohibition* of 66.09 and 69.63 for *Enforced Prohibition*. The results support our hypothesis that the guests in the second room were affected by the legal regime in place but not by the second-hand smoke. In fact, guests in the *No Prohibition* treatment stayed significantly longer, even though they were exposed to significantly more smoke than guests in the *Enforced Prohibition* treatment. Furthermore, guests in the *No Prohibition* treatment stayed as long in the second room as they did in the main room, even though they were exposed to heavy smoke in the former. Descriptive results are summarized in Table 2.²⁵

25. To keep the exit survey brief, we did not ask patrons in the exit interview whether they were in a hurry when they left the bistro. This decision should not cost us explanatory power as external constraints that affect the length of time a patron spends in the bistro would cancel each other out across treatments. And, importantly, as mentioned above, there was always plenty of room in the first room, so one should not have to trade off a preference for a smoke-free stay against getting a (quick) table. If anything, patrons in a hurry may have preferred the first room simply to save time walking to the second room. But if that was the case, they would have done so regardless of the treatment in the second room.

Treatment Effects on Consumption

When observing a group of customers, we calculated their average spending and treated the result as one observation. For example, if one guest paid for the other group members, we calculated the per capita spending and included this value as one observation in our analysis. We found that non-smoking guests in the *Violated Prohibition* treatment spent €6.79 significantly less on food and beverages than the non-smokers in the *No Prohibition* treatment who spent €10.15 (t-test p -value < 0.01 ; *Hypothesis 2a*). The consumption in the *Violated Prohibition* treatment (€6.79) was also significantly lower than in the *Enforced Prohibition* condition (€9.67; t-test p -value < 0.01 ; *Hypothesis 2b*). Finally, we compared the results of the *No Prohibition* treatment with the *Enforced Prohibition* condition and found, as expected, that the results did not substantially deviate (t-test p -value 0.57; *Hypothesis 2c*). All comparisons are reported in [Table 2](#).

Results of the Exit Interviews

We have previously reported how the exit interview data on smoke sensitivity and patrons' views on smoking bans help to rule out selection bias. Here, we report on the potential impact of the smoking ban on patrons' sensitivity to smoke exposure and need for protection from smoke. If so, this may have caused patrons to leave earlier and consume less in the *Violated Prohibition* treatment. As shown in [Table 2](#), the results do not indicate the presence of such an effect. Non-smoking patrons in the *Violated Prohibition* treatment reported a sensitivity to smoke of 1.89, which is not significantly different from guests in the *No Prohibition* treatment (2.04; t-test p -value = 0.25). We found the same pattern for the preference for health protection with a score of 2.57 for guests in the *Violated Prohibition* treatment and 2.64 for the guests in the *No Prohibition* treatment (t-test p -value = 0.58).²⁶ All comparisons are reported in [Table 2](#).

Regressions

To further investigate the robustness of our results, we performed an additional ordinary least squares (OLS) regression that includes several controls. First, we used the data of the exit interviews on (1) smoke sensitivity; (2) concern about health protection; and (3) whether guests prefer one or the other room because of some hidden feature of that room. Second, we included our individual-level exposure from the particle counter machine. We controlled for the group size of our observations as they can represent the data of an individual guest or any number of guests that entered the bistro together and therefore form a group. We used dummy variables to control for the

26. The results of the *Enforced Prohibition* condition show significantly higher sensitivity and health protection concerns compared to both the *No Prohibition* and *Violated Prohibition* treatments. This suggests that patrons with higher sensitivity and concern about the health effects of second-hand smoke stayed in the first room where they were protected from smoke exposure. This comports with our finding reported on selection bias above.

day that the observation was collected as well as the room in which the observation was collected.

Since there are no right violations in either treatment (coded = 0), we collapsed the *No Prohibition* and the *Enforced Prohibition* treatments and compared them to the *Violated Prohibition* treatment (coded = 1). The regression analysis supports the results we have shown above: while the control variables do not turn out to be significant, we observed a negative effect on both dependent variables, time, and consumption. The results are reported in Table 4 in Appendix A. As expected, our outcome variables time and consumption are highly correlated (Spearman $r_s = 0.657$, p -value < 0.01) and lead to collinearity in the regressions. As both outcome variables serve as proxies to measure the loss of utility induced by the violation of the smoking ban, the similar effects on both proxy variables provide robust evidence that the *Violated Prohibition* treatment indeed reduced guests' utility from their visit to the bistro.

Active Violation of Rights

The analysis below further examines evidence that the downward effects on the time of guests at the bistro and their consumption result from right violations. In the *Violated Prohibition* treatment, smoking infringed the guests' right to a smoke-free environment, while in *No Prohibition*, guests were not granted such a right. Our analysis exploits the fact that some non-smokers stayed in the second room while other guests were smoking, while other non-smokers did not witness smoking while they were in the second room. This scenario allows us to compare the behavior of non-smoking guests whose rights were violated to the behavior of other guests whose rights were not violated and leads us to the following hypothesis: If the treatment effect is driven by right violations, then guests who faced smoking in the *Violated Prohibition* treatment should leave earlier and consume less than guests (1) who did not experience smoking in the *Violated Prohibition* treatment and (2) all guests in the *No Prohibition* treatment where non-smokers did not have the right to a smoke-free environment.

In line with this hypothesis, the non-smokers in the *Violated Prohibition* treatment ($N = 54$) whose rights were violated by a smoker left the bistro significantly earlier than guests ($N = 22$) who were not directly subjected to an infringement in the *Violated Prohibition* treatment ($50.98 < 58.59$ minutes; t -test p -value = 0.02). We also observed that the guests whose rights were violated consumed less on average ($\text{€}6.07 < \text{€}8.52$; two-tailed t -test p -value < 0.01). Second, the non-smokers in the *Violated Prohibition* treatment ($N = 54$) whose rights were violated by a smoker also left the bistro significantly earlier and consumed significantly less than guests in the *No Prohibition* treatment, irrespective of whether they (1) encountered a smoker during their stay ($N = 73$; time: $51.98 < 58.59$ minutes; t -test p -value = 0.02; consumption: $N = 73$; $\text{€}6.07 < \text{€}10.20$; t -test p -value < 0.01) or (2) when they did not encounter a smoker ($N = 8$; time: $51.98 < 62.57$ minutes; t -test p -value = 0.05; consumption: $N = 8$; $\text{€}6.07 < \text{€}9.61$; t -test p -value < 0.01). By contrast, the comparison of non-smokers in the *No Prohibition* treatment who either encountered a smoker or did not face a smoker is not significant (time: $62.5 < 66.42$ minutes; t -test p -value = 0.43; consumption: $\text{€}9.61 < \text{€}10.20$; t -test p -value 0.76), demonstrating, of course, that, while statistically

TABLE 3.
Right Violations

	Time (minutes)	Consumption (Euros)
<i>Violated Prohibition Smoking</i> (<i>N</i> = 54)	51.98	6.07
<i>Violated Prohibition No Smoking</i> (<i>N</i> = 22)	58.59 <i>p</i> = 0.02** (versus <i>Violated Prohibition - Smoking</i>)	8.52 <i>p</i> < 0.01*** (versus <i>Violated Prohibition -Smoking</i>)
<i>No Prohibition Smoking</i> (<i>N</i> = 73)	66.42 <i>p</i> < 0.01*** (versus <i>Violated Prohibition -Smoking</i>)	10.20 <i>p</i> < 0.01*** (versus <i>Violated Prohibition -Smoking</i>)
<i>No Prohibition No Smoking</i> (<i>N</i> = 8)	62.57 <i>p</i> = 0.05* (versus <i>No Prohibition - Smoking</i>)	9.61 <i>p</i> < 0.01*** (versus <i>No Prohibition -Smoking</i>)
<i>Enforced Prohibition</i> (<i>N</i> = 87)	69.31 <i>p</i> < 0.01*** (versus <i>Violated Prohibition -Smoking</i>)	9.67 <i>p</i> < 0.01*** (versus <i>Violated Prohibition -Smoking</i>)
<i>Enforced Prohibition</i> (<i>N</i> = 87)	<i>p</i> = 0.97 (versus <i>No Prohibition - No smoking</i>)	<i>p</i> = 0.44 (versus <i>No Prohibition - No smoking</i>)

Notes: All *p*-values are two-sided *t*-tests. *, **, *** indicate significance at the 10 percent, 5 percent, and 1 percent level.

significant and consistent with our hypothesis, the observations with *N* = 8 for guests in the *No Prohibition* treatment who were not exposed to smoking limit this part of the analysis (see [Table 3](#)).

Our results indicate that public smoking caused a negative effect in our study only when smoking violated the rights of the non-smokers but not when smoking was allowed. Using the full data set, we confirm our findings in an OLS regression, including the observations of the guests in the main room. We created two dummy variables to estimate the effect of infringements. Smoking, the first dummy variable, measures whether exposure to a smoking patron during the stay at the bistro had an effect (yes = 1; no = 0). The second dummy variable is the interaction of the regime (prohibition = 0; no prohibition = 0) with smoking. We refer to it as “right violation.” It estimates the influence of the treatment (= smoking regime) and distinguishes between smoking that occurred in violation of the prohibition, on the one hand, and smoking when it was allowed, on the other hand (right violation = 1; no right violation = 0). The outcome variables—time and consumption—yield the same results (see [Appendix A, Table 5](#)). The outcomes show that guests’ behavior does not differ depending on whether guests were seated in the main or in the second room unless they

were exposed to a smoker and the smoker violated the smoking prohibition. Thus, the regressions support our central result: rights holders suffer pure infringement costs when their rights are violated.

Internal Validity

Selection Bias: Awareness of Legal Regime and Manipulation Check

As discussed in the Experimental Design section, our treatment manipulation (altering the smoking regime) does not induce a selection bias where more smoke-sensitive guests take a seat in the second room when smoking is forbidden. The exit interview data further confirmed that almost all patrons were aware that guests would smoke even if the ban was in place. They also were aware whether the ban was in place or not (manipulation check). In addition, since there was always ample seating available in the front room during our observations, guests were not forced to move to the second room when they would have preferred to be in the smoke-free front room.

Cognitive Dissonance and Exit Interviews

Another potential alternative explanation for our results relates to cognitive dissonance (Simon, Snow, and Read 2004). Patrons may not have understood that people would smoke in the second room when smoking was banned there (*Right Violation* treatment). If so, a greater number of smoke-sensitive guests may have entered the room when the ban was in place than when smoking was allowed (*No Prohibition* treatment). Subsequently, during the exit interviews, guests in the *Right Violation* treatment who had been exposed to smoking may have downplayed their smoke sensitivity to reduce cognitive dissonance. Along the same lines, to reduce cognitive dissonance, guests may have indicated that they were aware that others were smoking.²⁷ Cognitive dissonance is a challenge for self-reported data. For that reason, our study also gauges the smoke sensitivity of guests more directly. With the aid of the particle counter, we examined the impact of smoke concentration levels on the actual behavior of guests to estimate their sensitivity to second-hand smoke. Additionally, a cognitive dissonance interpretation is inconsistent with our core finding that patrons whose rights were violated in the *Right Violation* treatment leave earlier and consume less than guests whose rights were not violated in this treatment. It is unlikely that patrons who chance encountered a smoker in the second room were, on average, more smoke sensitive than those who happened to not encounter smoking during their stay and therefore left earlier (and subsequently downplayed their smoke sensitivity to reduce cognitive dissonance).²⁸

27. We are grateful to the reviewer for raising this excellent point.

28. The same rationale applies to guests' awareness that other patrons would be smoking in the second room. The exit interviews show that all patrons were aware of the smoke exposure in the second room of the bistro, irrespective of the treatment they were subjected to and the room they sat in.

Room Preference

When we selected our experimental location, we searched for an establishment with two rooms that share the same characteristics. We asked subjects whether they preferred one room over the other. Most guests said they did not. Including this data in our regression analysis (variable = room preference), we see that preferences for rooms did not affect the patrons' length of stay at the bistro or the magnitude of their consumption.²⁹

Effect of the Ban on Public Attitudes

The smoking ban might have made salient the health hazards of second-hand smoke, potentially influencing patrons' attitudes towards smoking and their sensitivity to second-hand smoke. This increased awareness of the risks associated with second-hand smoke or reduced tolerance for smoke might have caused guests to leave the second room earlier and consume less. However, the exit interview data, does not support this interpretation. Respondents' attitudes about protection from public smoking and sensitivity to smoke did not differ across the assigned treatment. Additionally, our regression analysis shows that attitudes toward protection (variable: legal protection) and smoke sensitivity did not impact our findings (see [Tables 4 and 5](#) in [Appendix B](#)).

Hidden Variables

Some individual factors may be correlated with smoke sensitivity, drinking, and eating habits as well as with the guests' time management and, therefore, could confound our results. We were not able to collect this demographic data in our field setting. Yet if such factors would have systematically affected our results, their impact should have been picked up by one of the regression dummies. For example, suppose that individuals who have a high tolerance of smoke also tend to drink more or stay in the bistro for longer periods. If so, the room dummy would have picked up this effect and indicated that guests in the second room stay longer and drink more.

Data from Different Dates

The data may have strongly varied and, incidentally, may have been driven by the results of one particular day. To ensure the robustness of our results across the whole test period, we split our data set into three separate parts, each part containing all the treatments and conditions collected over one weekend. To test for differences across the

29. Note, however, that, while we decided to elicit this data to ensure our findings are robust, our study design does not require controlling for patrons' room preferences unless such a preference (for a quieter room or a room with more sunlight for example) was affected by the treatment. Thus, for a room preference to affect our results, non-smokers would need to prefer a quieter environment or a room with more sunlight only if smoking was forbidden but not if it was allowed (or the other way around). This seems implausible. Instead, if room preferences are independent of the treatment, such room preferences should be equally present in both treatments and are likely to cancel out.

data sets, we included dummy variables in our regression. Results show that including the dummies in the analysis did not affect either of the two outcome variables. The treatment effects were robust and did not differ across the data sets.

Independent Observations

We selected a bistro for our study because the guests there did not interact with one another unless they were sitting at the same table. We might not have independent observations if the behavior of some guests in the bistro had influenced other guests. Additionally, the bistro offers no entertainment (television or live music) that could affect the behavior of guests and their collective experience at the establishment. To preserve the independence of the observations, we classified guests seated together as a group. We also avoided conducting our study on days when major public events (elections, for example) were taking place.

Public or Private Smoking Ban

The influence of the smoking ban on the behavior of guests may be impacted if guests mistakenly believe that the ban was imposed by the bistro instead of the government. The smoking ban is imposed by public state law. The bistro management applies the legal smoking ban or relies on the legal exception by creating a separate, closed-off smoking area as prescribed by the public smoking law. The exit interviews confirmed that guests were fully aware that the smoking ban is imposed by public law,³⁰ as was to be expected considering the widespread media attention to the topic of public smoking bans.

Lack of Forecasting

We found that guests in the second room adversely experienced violations of their right to a smoke-free environment. The exit interviews indicate that patrons were aware that when the bistro was not enforcing the ban, smokers would violate the prohibition.³¹ This suggests that the guests in the second room, who left earlier and consumed less, may have failed to anticipate their negative experience of violations of the ban. There are different explanations for this lack of anticipation. First, guests may have little experience with violations. Consider that observing violations of a smoking ban is not the same as experiencing rights violations. Our findings show that the negative experience of rights violations occurs only when one's own rights are violated (like the patrons who stayed in the second room when the smokers did not comply with

30. This is not surprising as the smoking law was widely debated in Germany before it was enacted and applied to almost all public areas.

31. Our exit interviews demonstrate that almost all patrons were aware that the smoking ban was not enforced in the second room. This is to be expected given the presence of second-hand smoke in the second room at all times, as confirmed by our particle counter. The exit interviews also enabled us to identify patrons that may have visited the bistro more than once during our study. For these patrons, we only considered the data of their first visit to the bistro.

the ban) as opposed to when individuals merely witnessed violations of a law (like the guests in the front-room of the bistro who observe guests violating the ban, but do not experience an infringement of their own right as our data show). Similarly, when people see others smoking in a public building like an airport or office building without being in a zone confined to smoking, they understand that smokers are violating the prohibition and that the prohibition is not enforced, but they are not exposed to a violation of their own rights if they are not subjected to the smoke. Many individuals have observed instances where the smoking ban goes unenforced and is violated, yet they have not personally experienced such violations or had the opportunity to reflect on their own response to infringements on their rights.

Second, the psychological effect of empathy gaps that is reported in the literature shows that predicting one's own behavior and attitudes is bound to one's own visceral state (Loewenstein 2005; Van Boven et al., 2000). In a cold state, individuals have difficulty predicting their own actions, attitudes, and preferences in a hot state.³² In our study, patrons who took a seat in the second room were indifferent about smoke exposure, and they indicated that they did not care strongly about being protected from smoke exposure. When patrons decided to sit in the second room, they were in a cold and calm state and did not consider the extent to which smoking might bother them. Once seated in the room, however, when their right to a smoke-free environment was violated, the emotional response to violations of the ban was more intense than anticipated. Such empathy gaps are reported to be especially strong in situations where there is a potential for social embarrassment, as in our experiment (Van Boven et al., 2005).

Research on empathy gaps also shows that prior experience typically does not substantially improve affective forecasting. George Loewenstein (2005) and others report that even extensive experience from everyday life may not enable people to correctly forecast their own behavior in a hot state. For example, robust gaps were observed in a study that presented college students with a social performance task (Van Boven et al., 2005). Despite their prior experience with such tasks, the students failed to forecast that they would not be willing to participate because they feared social embarrassment. In other studies, respondents failed to forecast that they would not be willing to ask for help or that they would reject a request for help (Flynn and Lake 2008; Bohns and Flynn 2010). Finally, it is important to note that non-smoking patrons who have prior experience with violations of their personal rights and who forecast their own response to violations more accurately than most of the population, apparently, may have taken a seat in the front room—a decision that does not affect our result.

Indirect Effects

We cannot directly manipulate whether the rights of guests are violated or not. Therefore, we may pick up interaction effects between the treatment, the smoker, and

32. A classic example is drug addiction: for someone who is not craving a drug, it is virtually impossible to anticipate the grip that such a craving will have on their behavior (Loewenstein 1999). Relating the example to our study, as long as your rights are not violated and you are indifferent about a law that protects you from others smoking in your presence, it is difficult to anticipate your reaction to violations of the law.

the non-smoker's behavior. For instance, smokers in the *No Prohibition* treatment may smoke more not only because it is permitted but also because they expect that other guests will not mind them smoking when it is allowed. Their increased smoking, in turn, may cause non-smoking guests to leave earlier. Such an interaction effect could confound a clear interpretation of our results. Suppose, however, that this indirect effect was indeed at play. Then it should cause only the guests in the *No Prohibition* treatment to leave earlier and consume less but not the guests in the *Violated Prohibition* treatment. We did not see any interaction effects that would confound our findings.

Limitations

This is a field study, which necessarily has inherent limitations. Unlike laboratory experiments, we did not have a high degree of control over the setting and cannot rule out factors that may have impacted our results in ways that our measures did not pick up. Without compromising participation in the exit interviews, it would not have been possible to elicit all the types of demographic and personal data that are commonly obtained in a laboratory or survey study.

External Validity

Demand Effects

Our field design avoids social or experimenter demand effects. In a laboratory study, participants often try to figure out the study's objective. In doing so, participants in our study may become more aware of their rights and perceive them as being more important than they would in a normal situation. Additionally, the natural sample of bistro visitors (a mix of professionals and students from university and high school) increased our results' reliability.

Indifference About the Objective of the Smoking Ban

We focused on a particular subsample of guests in our study: guests for whom the legal protection from smoking had no personal or material value. Their decision to sit in the second room instead of the smoke-free main room suggested that they were not interested in the protection provided by the smoking prohibition. We focused on this sample of guests for methodological reasons: to isolate the psychological costs of the right violation. While our study tested the effect of unenforced rights and their violation with a specific set of rights holders to cleanly isolate the psychological cost of rights infringements, the effect can also be expected to occur in other social contexts and with different types of rights holders. For instance, individuals who support the smoking ban and value the right to be free from smoke exposure are likely to feel even more harmed by infringements of the smoking ban. In addition to the frustration caused by a disrespect of their rights (as identified in our study), they may feel that public smoking is objectionable and/or be concerned about the material harm that it imposes.

Finally, note that right violations imposed psychological costs on guests even though these guests were aware that smokers would not comply with the smoking ban. In many other social contexts, rights holders have even stronger expectations about the respect of others for their legal rights. In such instances, a right holder's frustration with violations likely exceeds what we observed in our study.

DISCUSSION

Our study reveals the psychological costs involved with the violation of symbolic rights. Teasing out the underlying cognitive and social mechanisms that cause the adverse response to the violation of symbolic rights is an interesting avenue for further research. Laboratory experiments will help explore this topic in more detail. However, our data provides several cues about potential underlying mechanisms that may explain the negative experience of rights violations, as we documented above.

Ownership of Legal Rights in Social Settings

Our findings show that non-smokers in the bistro's second room adversely experienced smoking but only when smoking was prohibited by the unenforced ban. In other words, non-smokers adversely experienced smoking not because of the material effects of second-hand smoke but because smoking violates their right to be free of smoke. The literature on the psychology of ownership of legal rights offers a potential explanation for this phenomenon (Pierce et al. 2002; Spellman and Schauer 2009). Legal rights may create a sense of psychological ownership. As a result, legal rights are perceived as entitlements and become part of the rights holder's identity and self-image, affecting their relationships with others (Dittmar 1992; Pierce et al. 2002). Violations of the smoking ban may have been objectionable because they took away the "legal entitlement" of guests to be free from public smoking. In this manner, non-smokers in the second room might have felt that infractions of the ban showed a lack of respect for their rights as non-smokers. This interpretation may also help explain the finding that latent smokers (guests who indicated they were smokers in the exit interviews but who did not smoke in the bistro) were not bothered by infractions of the ban. Perhaps smokers in the second room did not feel entitled to the smoking ban because they felt that, as smokers, they were not the intended beneficiaries of the law.

Shaping Preferences

According to the strong version of the expressive function of the law, laws and regulations can shape preferences by designating conduct as socially undesirable (Sunstein 1996; Cooter 1998, 2000). In this regard, the public smoking ban, when in effect, might have influenced preferences and attitudes toward smoking and second-hand smoke and may have caused non-smokers to leave earlier and consume less when smokers violated the ban in the second room. In our study, however, the smoking ban

did not have an observable shaping effect on the attitudes toward smoking among non-smokers who were in the room when smoking was prohibited. First, our exit interviews show that the smoking ban did not make non-smokers more sensitive to second-hand smoke: their reported sensitivity to smoke exposure did not differ based on whether smoking was allowed or forbidden during their stay at the bistro. Accordingly, our particle counter readings show that non-smokers did not leave earlier or spend less because smoke concentrations were higher (or lower) in the room. Second, the exit survey data also indicate that the treatment did not enforce attitudes among non-smokers that public smoking should be prohibited. Customers who had visited the bistro when the smoking ban was in effect did not hold a stronger belief that guests should be protected from second-hand smoke.³³ Thus, a potential shaping effect of the ban on attitudes does not explain our results.

Methodologically, we selected a German bistro as the venue for our study because we did not expect a strong preference-shaping effect of the law to take hold in this context. German customers are very familiar with the public smoking ban, which is in effect in all public places and at all other establishments. For that reason, it is likely that most guests already had formed attitudes toward the public smoking ban before entering the experimental setting at the bistro. Also, this setting at the bistro (alternating smoking regime, lack of enforcement, constant violations of the smoking ban) may have further undermined the potential expressive power of the smoking law.³⁴

The Expressive Function of the Law: Information and Coordination

Richard McAdams's (2000a, 2000b, 2015, 2017) expressive theory of the law provides a powerful explanation for compliance with unenforced laws. Legislation can reveal the prevailing public attitudes about an issue and coordinate expectations about socially acceptable behavior. The law may empower rights holders to engage in social enforcement, while legally disfavored parties are more likely to back down. For instance, a public smoking ban might embolden non-smokers to confront smokers, and the resulting compliance may further bolster the legitimacy of the smoking ban (Kagan and Skolnick 1993). Similarly, in the context of our study, the smoking may have encouraged non-smokers to confront smokers when the ban was in effect in the second room. Additionally, if smokers feared public disapproval when violating the smoking ban, this may have given them pause about lighting up in the presence of non-smokers.

We did not observe any non-smokers chastising smokers (coughing, frowning, making comments) for violating the smoking prohibition at the bistro. Perhaps non-smokers were reluctant to engage in social enforcement because they either knew (as indicated in the exit interviews) or could have noticed (due to the smoke in the air) that the ban remained unenforced when they entered the second room. Additionally, non-smokers could always have moved to the smoke-free front room and may have

33. The measure for the approval of legal protection shows similar scores: 2.69 versus 2.58 for both treatments in a Likert scale of 1–5 (where 1 = legal protection is not important at all; 5 = legal protection is very important).

34. Research indicates that partial smoking bans are relatively ineffective with regard to ban awareness and support, compliance, and enforcement (Ravara et al. 2013).

anticipated that a smoker could remind them of this option.³⁵ Of course, the lack of visible private enforcement in our setting does not rule out that the coordinating effect of the law plays a role in bringing about compliance with smoking bans in many other situations (McAdams 2015, 2017).

Rule-of-Law Preference

Infractions of the smoking ban might also be objectionable to non-smoking patrons in the second room because they generally consider rule breaking to be irresponsible as a violation of civic duty or immoral.³⁶ Patrons might object to the breach of the smoking ban at the bistro regardless of whether they are bothered by being exposed to second-hand smoke or their viewpoints on the health effects of second-hand smoke and smoking bans. Such a “rule-of-law” preference might be consistent with the finding that non-smokers felt perturbed by infractions of the smoking ban even though they are insensitive to second-hand smoke. Several of our findings, however, seem inconsistent with a general, cultural, or principled aversion to rule breaking. Non-smokers do not generally disapprove of the lack of enforcement at the bistro. Instead, we found that non-smokers in the second room reacted adversely only when someone smoked in their presence—that is, when their personal right was violated.

Latent smokers (smokers who did not smoke while in the second room) also did not seem disturbed by smoking ban violations. We recorded no significant differences in the time and money they spent in the bistro³⁷ when smoking was prohibited versus when it was allowed.³⁸ If a rule-of-law preference was driving their behavior, we would have expected to see them being bothered by violations and leave earlier. Furthermore, guests in the front room stayed much longer than the guests whose rights were violated in the second room. In total, 90 percent of the patrons who sat in the front room indicated in the exit interviews that they were aware of the rule-breaking in the second room but were not bothered by it. By contrast, if their behavior was driven by a rule-of-law preference, then we would have expected them to respond to the violations of the smoking ban that they observed through the glass door of the second room. Generally, a psychological ownership explanation seems to be more consistent with our evidence than a rule-of-law preference since guests did react adversely only to the violations of their personal rights but not to violations of the ban as such. In general, the rule-of-law preference might have been stronger if Germany had passed a strict public smoking ban. The partial smoking ban’s many exceptions and the hybrid smoking regime at the

35. As reported above, we only made observations when there was ample seating available in the front, non-smoking room.

36. In light of certain stereotypes, this effect might be more pronounced in Germany (see Kulish 2009). Note, however, a strong preference for the rule of law should prevent customers from lighting up in the first place when smoking is banned in the second room.

37. For the variable spending, we found the following means in US dollars: \$9.937 (*No Prohibition*) and 9.940 (*Violated Prohibition*). Conducting a *t*-test, we get a $p = 0.996$ rejecting H_0 that the means differ significantly since the result is not within a 10 percent range of equivalence (1.454; -1.447). Still, the rejection of the *t*-test above 0.9 is strong evidence that the two means are close.

38. In the regressions, the legal protection measure is our main interest. To increase the robustness, we also included the controls for smoke exposure, smoke tolerance, and aesthetics.

bistro created a more ambivalent context that likely undercut the salience of the smoking ban.

POLICY IMPLICATIONS

Hidden Costs of Symbolic Rights

Conventional wisdom holds that legal rights will always leave rights holders no worse off than if no rights had been created, even if the rights are poorly implemented or enforced (Lyons 1969, 170). Legal rights enable citizens to challenge prohibited behavior by invoking the state's enforcement powers. In this respect, a legal right can be conceived as an option that aggrieved rights holders may or may not exercise (Hohfeld 1913, 1917; Corbin 1919; Ayres 2005). Rights holders stand to benefit even if the right is not enforced. For instance, non-smokers stand to benefit when the state enacts a public smoking ban. If the ban is unenforced, it still may reduce smoking by altering public attitudes and social expectations about smoking. And even if the ban remains completely ineffective in reducing public smoking, rights holders do not seem to be any worse off than if no legal protection against smoking had been enacted.

Our study questions this intuition and provides an important nuance to the understanding of the effect of unenforced laws. Our findings suggest that right violations may aggrieve individuals even when they are indifferent about the protection or material benefits that the legal right provides. In our study, non-smokers in the second room objected to smoking only when the ban was in effect: they were not bothered by the exposure to second-hand smoke and the associated health risks. Yet these non-smokers suffered psychological costs from infringements of the right that the smoking ban granted them. Additionally, the negative experience of infringements may also bring about social friction. The mere declaration of a passive right (to be free from second-hand smoke, litter, bicyclists in a pedestrian zone) may induce rights holders to resent individuals who do not respect their rights (Kelley et al. 2003; Janoff-Bulman and Werther 2008), even though they would not be bothered by the same behavior had they not been allocated a passive right. Importantly, the social friction that violations of unenforced rights may cause is distinct from any ideological conflicts that symbolic laws often bring about, such as whether smoking is part of everyone's freedom or a disturbance and health concern. Lawmakers should be aware of this potential social price of aspirational, unenforced laws. But, as we discuss next, the psychological costs of rights violations may also have an inadvertent, positive effect on compliance.

The Self-fulfilling Potential of Aspirational Laws

Empirical scholarship has widely recognized that laws and regulations can alter social equilibria by creating social norms and expressive effects (Posner 2000). Laws can set focal points and provide information about community preferences (McAdams 2000a, 2000b, 2015, 2017; Goldstein 2006; Fisman and Miguel 2007). Especially if a law is perceived to be widely supported or coordinates expectations about socially

appropriate behavior (Geisinger 2002), compliance with, and social enforcement of, unenforced laws can provide social approval and esteem to individuals (McAdams 1997; Posner 2000; Ellickson 2001, 2017). Our field study suggests an additional mechanism that may cause unenforced symbolic laws to be self-enforcing. As evidenced by our data, the symbolic law effectively increased guests' sensitivity to public smoking without changing their attitude toward smoke exposure. By triggering their frustration with smoking violating their rights, the smoking ban influenced guests to avoid the rights violations thereby substantially reducing their exposure to smoke, which is one of the policy goals of the smoking ban.³⁹

Additionally, the frustration with violations may induce rights holders to engage in social enforcement. Non-smokers can complain outright to a patron who lights up or provide subtle indications of disapproval, such as glancing, changing seats to increase the distance to smokers, coughing, and so on, and rights holders might organize protests to encourage bureaucratic implementation and effective enforcement. Overall, unenforced laws have a self-fulfilling potential by inducing sensitivity to violations of rights granted by the law without requiring rights holders to internalize or change their attitudes toward the law's objective. In this manner, a symbolic law may tip the social dynamics toward greater compliance, even in the absence of public enforcement.

Our study offers an interesting avenue for future research on social norms and enforcement. An open question is what factors determine the equilibrium that may emerge when a public law remains unenforced. The outcome will likely depend on many contextual factors—for example, whether potential violators or rights holders are in the majority or how burdensome it is for rights holders to engage in self-help to counter infringements. For instance, in our field setting, the availability of a separate, smoke-free front room may explain why non-smoking patrons in the second room were reluctant to openly signal their disapproval of violations of the smoking ban. In a different context, social enforcement may have tipped the social equilibrium toward compliance with the prohibition.

This self-enforcing mechanism may also interact with other mechanisms of social enforcement. As the unease with violations increases the demand for compliance, intermediaries may step up to remove social tension. For instance, after we shared with the bistro our findings that guests responded negatively to violations of the smoking ban by leaving early and consuming less, management began to enforce the ban in the second room. Ultimately, by organizing protests and public engagement, rights holders may push for bureaucratic implementation and effective public enforcement. In this regard, our study reinforces the potential of symbolic laws as useful starting points in the mobilization and pursuit of social change, as emphasized by advocates of aspirational laws.

CONCLUSION

This article extends the literature on the role of law as an instrument of social control and change. We have empirically examined the impact of aspirational laws in a

39. We are grateful to our reviewers for pointing us to this indirect effect.

natural field environment where the influence of deterrence, expressive, and legitimacy-based drivers of compliance is modest. The results indicate that unenforced laws can cause rights holders to be more sensitive to rule breaking in public settings. The results present a mixed position in the debate between social movement lawyers and critics of rights-based reform strategies. Aspirational laws may create significant costs by burdening rights holders with rights violations and the ensuing social conflict. At the same time, however, even if a law does not initially alter public attitudes, frustration with violations may induce rights holders to engage in social enforcement and mobilize support for the implementation and public enforcement of the law.

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APPENDIX A: REGRESSIONS

We performed an OLS regression, including all data from our two treatments and the condition in the main room. We included smoke exposure and the self-reported tolerance of smoke as individual-level data for each guest (group) in the regression. We also constructed a dummy variable controlling for group size as the observations consisted of varying numbers of guests. Finally, we included dummy variables for each of the separate data sets that we collected over the three different weekends to analyze whether they differed from each other with regard to our main effects.

We measured the main treatment effect with two dummy variables. The first dummy “treatment”⁴⁰ measured the difference caused by the *Violated Prohibition* treatment versus the differences caused by the other two conditions, *No Prohibition* and *Enforced Prohibition*.⁴¹ Our second variable was a dummy for “room” (second room = 1; main room = 0), which picked up if guests who sat in the main room behaved differently from guests sitting in the second room as long as this difference was not caused by the *Violated Prohibition* treatment, the latter effect being controlled for by the “treatment” dummy. That said, the “room” dummy allowed us to measure whether the guests in the *No Prohibition* and *Enforced Prohibition* treatments behaved differently. While our regression treated the guests in these treatments as being assigned to the same treatment (treatment code = 0), they sat in different rooms. If their behavior had differed from each other, the difference would have been picked up by the “treatment” dummy.

As hypothesized, the “treatment” dummy was significant. Guests left the bistro prematurely under the *Violated Prohibition* compared to the pooled *No Prohibition* and *Enforced Prohibition* treatments (−13.57 minutes). Ultimately, the “room” dummy was insignificant and showed that the guests’ behavior was not altered by the room in which they sat as long as the treatment (= the smoking regime) remained the same. The results support the finding that the guests in the *No Prohibition* treatment and guests in the smoke-free main room (*Enforced Prohibition*) do not behave differently. As assumed, the strong smoke exposure in the second room during the *No Prohibition* treatment does not seem to have affected the guests’ behavior. The treatment induces the main effect guests only leave the bistro earlier when smoking is forbidden and their rights are violated.

The “room” dummy also controls for potential factors that might lead guests to prefer one room over the other. For example, some people may have a general preference for sitting in front rooms. However, as the “room” dummy remains insignificant, the analysis shows that potential room preferences do not affect the guests’ behavior. The other covariates—“smoke tolerance” and individual “smoke exposure”—also remain insignificant in the regression, supporting the conclusion that exposure to smoke did not cause guests to leave earlier. [Table 4](#) presents the results.

40. In what follows, we use quotation marks to highlight the names of the variables that we will use in the regression analysis.

41. By contrast, the difference of the values measured in the *Smoking Forbidden* treatment and the *Smoking Allowed* treatment is small.

TABLE 4.
OLS regressions: main effects

Dependent variable	Time	Consumption
Treatment	-13.062*** (3.213)	-3.432*** (0.855)
Room	-1.287 (4.403)	0.415 (1.171)
Smoke tolerance	-0.458 (1.241)	-0.138 (0.330)
Smoke exposure	-0.000 (0.000)	-0.000 (0.000)
Data Set 1	1.876 (3.095)	0.820 (0.823)
Data Set 2	-1.094 (3.095)	-0.081 (0.806)
Group size	-0.007 (1.067)	0.048 (0.284)
Constant	71.858 (5.074)	9.947 (1.350)
R ²	0.12	0.09

Notes: Standard errors are reported in parentheses. All *p*-values are two-sided *t*-tests. *, **, *** indicate significance at the 10 percent, 5 percent, and 1 percent level.

The “treatment” dummy indicates that the violation of the smoking prohibition significantly reduced the amount that guests spent on food and beverages by an estimated -€3.61. The “room” dummy was insignificant, indicating that guests consumed the same amount in both rooms when not assigned to the *Violated Prohibition* treatment. Thus, guests only consumed less when smoking was forbidden and their rights were violated. The other covariates are insignificant as they have been for our first main variable: “time” (see Table 2). As expected, the outcome variables—“time” and “consumption”—are highly correlated (Spearman $r_s = 0.616$, *p*-value < 0.01) and lead to collinearity in the regressions. Both outcome variables serve as proxies to measure the loss of utility induced by the smoking ban’s violation. We consistently found similar effects on both proxy variables, providing robust evidence that the *Violated Prohibition* treatment reduced the utility that guests derived from their visit to the bistro.

We tested the same OLS regression model for rights violations. We created two dummy variables to estimate the effect of infringements. “Smoking,” the first dummy variable, measures whether being exposed to smoking during the bistro stay had an effect (smoking = 1; no smoking = 0). The second dummy variable, “right violation,” estimated the influence of the treatment (= smoking regime) and distinguishes between smoking that violated the prohibition and smoking when it was allowed (right violation = 1; no right violation = 0).

Our outcome variables, “time” and “consumption,” yield the same results (see Table 5). The dummy variable “right violation” is significant, showing that smoking in

TABLE 5.
OLS regressions: rights violation

Dependent variable	Time	Consumption
Right violation	-15.198***	-3.730***
Yes/No	(3.516)	(0.933)
Smoking	-8.223	-1.389
Yes/No	(4.434)	(1.176)
Room	-9.553**	-0.361
Main/Second	(4.343)	(1.173)
Smoke tolerance	-0.502	-0.162
	(1.2439)	(0.328)
Smoke exposure	-0.000	-0.000
	(0.000)	(0.000)
Data Set 1	1.915	0.816
	(3.089)	(0.819)
Data Set 2	-1.162	-0.036
	(6.133)	0.805
Group size	0.050	0.032
	(1.072)	(0.284)
Constant	64.726	7.030
	(7.054)	(1.870)
R ²	0.11	0.10

Notes: Standard errors are reported in parentheses. All *p*-values are two-sided *t*-tests. *, **, *** indicate significance at the 10 percent, 5 percent, and 1 percent level.

violation of the ban reduced the time and the money guests spent in the bistro. By contrast, the dummy for “smoking” remains insignificant, indicating that being exposed to smoking during the visit does not substantially influence behavior whenever smoking does not violate guests’ rights. The dummy variable “room” is also insignificant. The results show that the behavior of patrons does not differ depending on whether they sat in the main or in the second room unless they were exposed to a smoker and the smoker violated their rights. Finally, the covariates “smoke tolerance” and “smoke exposure” are insignificant alongside the control for “group size.” The regression supports our central result that rights holders negatively experience right violations.

APPENDIX B: EXIT INTERVIEW

We obtained additional information in our exit interview with patrons. We presented guests with questions that were partly conditioned on their answers (for example, are you a smoker or non-smoker).⁴² We report the wording and sequence of questions of the exit interviews below:

42. In the context of our study, a post-experimental survey was likely the most feasible way to identify latent smokers who decided not to smoke.

Patrons that exited the second room

1. Prior to this interview, did you realize a study was being conducted in the bistro (Yes/No)
2. Did you visit this bistro on any of these preceding days (observation days) (Yes/No)

If Yes on either question ⇒ excluded

Are you a smoker (Yes/No)

⇒ If No. Further questions: When entering the second room . . .

1. were you aware of the public smoking ban and that smoking was allowed /prohibited in the second room?
2. did you expect other guests to smoke when you got seated in the room?
3. how much did the interior or aesthetics of the room influence your decision to sit in the second room? (1-7 scale)
4. how much does smoke exposure disturb you? (1-7)
5. how important is the legal protection against second-hand smoke to you? (1-7)

⇒ If Yes.

Did you smoke? (Yes/ No)

Questions for guests in the main room:

Prior to this interview, did you realize a study was being conducted in the bistro?
(Yes/No)

Did you visit this bistro on any of these preceding days (observation days)?
(Yes/No)

⇒ If Yes. Excluded

⇒ If No.

1. Was smoking allowed or prohibited in the second room?
2. Were you aware that people would smoke in the second room?
3. Were you bothered by the smoking in the second room?