The Effects of Local Workplace Smoking Laws on Smoking Restrictions and Exposure to Smoke at Work

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ABSTRACT

We provide new evidence on the effects of workplace smoking restrictions by studying more than 100 local smoking ordinances in Ontario, Canada from 1997–2004. We advance the literature by examining local (as opposed to state or provincial) laws in a quasi-experimental framework and by explicitly testing for effects on worksite compliance and exposure to environmental tobacco smoke (ETS). We show that the local laws significantly increased workplace smoking restrictions for blue collar workers, and among this group the laws (and, by implication, workplace smoking bans) reduced ETS exposure by 28–33 percent. We find smaller and insignificant estimates for other workers.

I. Introduction

California recently became the first U.S. state to classify environmental tobacco smoke (ETS) as a toxic air pollutant, with potentially wide-ranging policy implications. This recent activity, however, is part of a much more longstanding trend in recognizing the potential health risks associated with ETS. One of the most important ways this sentiment has been exhibited in the United States is though the steady increase in restrictions on smoking at private worksites; by

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1999, almost 70 percent of adult workers reported the presence of a complete work area smoking ban (Shopland et al. 2001). A large literature in the United States has examined the effects of these workplace restrictions; multiple reviews of the evidence find that workplace smoking bans are associated with reduced exposure to ETS and lower rates of own smoking and per capita cigarette consumption (Fichtenberg and Glantz 2002; Levy and Friend 2003; Brownson, Hopkins, and Wakefield 2002; and others).

An increasingly common yet contentious policy tool aimed at achieving these improved outcomes is the adoption of laws and regulations at the state, province, and/or local levels that require smoke-free workplaces. These policies are widespread in the United States. As of October 1, 2007, 23 states and 507 municipalities have adopted 100 percent smoke-free workplace laws (Americans for Nonsmoker's Rights Foundation 2007), and if one includes less restrictive policies, the number of smoke-free ordinances is well over 1,000 (Brownson et al. 2002). Statistics are similar for Canada: Eight provinces and territories have adopted 100 percent smoke-free workplace policies covering more than 80 percent of Canada's population (Physicians for a Smoke-Free Canada 2007).

The existing research in economics and public health on the effects of these public laws restricting workplace smoking suffers from a few key limitations, however. First, the majority of the research on the effects of smoking laws follows a cross-location research design, in which outcomes for residents of places with smoking laws are compared to outcomes for residents in untreated areas. This approach may be problematic, however, if there exist unobserved characteristics about people that are correlated both with the decision to adopt a smoking ordinance and with individual preferences regarding smoke-free worksites and/or own smoking behavior. Since the usual set of control variables available to researchers in the data sets used in these studies is fairly limited, these biases generally cannot be ruled out. Second, most studies examining smoking laws in the United States focus on statewide policies, despite that—as noted above—most of the variation is at the local level. In fact, many state policies arise as an explicit response to preexisting strong local laws; as such, it is important to examine the effects of local policies.[1] Third, most previous research studies smoking as the outcome of interest, despite that the explicit goal of these laws has been to reduce exposure to environmental tobacco smoke.[2] Finally, existing work has not adequately studied the effects of smoking laws on compliance—that is,

1. See Shipan and Volden (2006) for a model of local/state diffusion of antismoking laws. Some state policies in the United States are intentionally written to weaken the strong local laws that precede them through "preemption" clauses that prohibit localities within the state from adopting ordinances that are more stringent than the state law, thus reducing local policy activity after a strong state preemption law. Tobacco companies, for example, have focused their efforts on state legislatures, since the costs of fighting each local ordinance can be much higher (Walls et al. 1994). Notably, this is not the case in Ontario. Localities were fairly late (relative to the United States) in adopting smoking bylaws, and Ontario did not have a province-wide workplace smoking law until 2006, after our sample period.

2. Canada's Non-Smokers' Rights Association (the country's advocacy group counterpart to Americans for Nonsmokers' Rights Foundation in the United States) notes that "[t]he creation of a smoke-free bylaw has a single purpose: to protect people form the known health hazards of exposure to SHS [second-hand smoke]." The 2006 province-wide Ontario law (the Smoke-Free Ontario Act) "prohibits smoking in enclosed workplaces and enclosed public places in Ontario *in order to protect workers and the public from the hazards of second-hand smoke*" [emphasis added].

whether actual worksite smoking policies have been affected by the laws. Since we know that many private firms in the United States and Canada instituted smoke-free policies well before government intervention, it not obvious that local laws should be expected to have large effects on actual smoking restrictions faced by workers. And, if smoking laws do not affect smoking restrictions faced by individuals, they should not be expected to affect own-smoking behavior.

In this paper we contribute to the literature by addressing each of the limitations described above. Specifically, we consider the effects of more than 100 strong local workplace smoking bylaws in Ontario, Canada (the most populated province in that country) using a quasi-experimental framework. Our restricted-use repeated cross-section individual level data (covering 1997–2004) span a period of rapid and widespread adoption of local smoking bylaws and contain detailed information on worksite smoking policies, ETS exposure, and own-smoking behaviors for a large sample of adults. These data also identify the worker's detailed location of residence, which we use to match the local workplace smoking restrictions to the individual observations. This allows us to explicitly test for worksite compliance with the local laws and to provide evidence on a range of relevant outcomes in addition to smoking behavior. Our preferred approach estimates the effects of local smoking restrictions in the presence of controls for demographic characteristics, survey year dummies, and county fixed effects.

To preview, we find that adoption of local smoking bans in Ontario significantly increased reported workplace smoking restrictions. In the full sample, a local bylaw is estimated to increase the likelihood a worker reports a complete work area smoking ban by about 8 percent. We also find large and statistically significant reductions in the likelihood of reporting that smoking is allowed "anywhere" at work (that is, no workplace restrictions). These aggregate effects, however, mask important differences across occupations. Specifically, we show that the effects of bylaws on ban presence are driven entirely by increases in smoking restrictions reported by blue collar workers. Among blue collar workers we find that adoption of a local bylaw cuts the likelihood of reporting no workplace policy by more than half and increases complete ban presence by 25 percent. We also find a significant reduction in ETS exposure of about 28-33 percent and a large (but statistically insignificant) estimated reduction in own-smoking rates experienced by blue collar workers. Importantly, our estimates for outcomes of white collar and sales/service workers—whose worksites were much more likely to have privately initiated smoking bans without government intervention—are plausibly smaller and statistically insignificant.

Taken together, our results confirm the beneficial effects of local clean indoor air laws—and by implication workplace smoking bans—on respiratory health, but we uncover substantial occupation-related heterogeneity in their effects. From a policy perspective, this suggests that recent movements toward occupation-specific bans focusing on bars and restaurants may have overlooked factories and warehouses as equally important targets of reform. And from a methodological standpoint, our results are important because they provide initial insight into the potential effects of a province-wide smoking ban that went into effect in Ontario in May 2006. Since other states and provinces have similarly adopted wide-ranging smoking ordinances that were preceded by many strong local laws, it is particularly important to understand the extent to which preexisting local bylaws have already improved worker outcomes. Failing to do so could result in biased estimates of the effects of state or province-wide policies.

The paper proceeds as follows. Section II provides a brief review of relevant literature. In Section III we present the data and outline the empirical approach, and Section IV offers the main results. Section V concludes.

II. Previous Research

A large literature has considered the effects of workplace smoking bans and associated policies mandating smoke-free workplaces.[3] The earliest research used a cross-locality research design in which outcomes for individuals in places with smoking bans are compared to outcomes for individuals in places without smoking bans. Moskowitz, Lin, and Hudes (2000), for example, use a single cross-section of the 1990 California Tobacco Survey and find that smokers in areas with strong ordinances were significantly more likely to report quitting behavior compared to smokers in areas with no local ordinance. Emont et al. (1993) perform a similar exercise using variation in the strength of state level restrictions and the 1989 Cardiovascular Disease Supplement to the Current Population Survey. They find that residents of states with more restrictive laws had lower cigarette consumption and higher quit rates than residents of states with less restrictive laws. Wasserman et al. (1991) use the National Health Interview Survey from 1970-85 and a state-specific index that is larger if smoking is restricted in private worksites and smaller if smoking is restricted, say, only in restaurants or elevators. Results indicated that higher state smoking restrictions were negatively related to tobacco consumption. Keeler et al. (1993) control for a similar local smoking regulations index in California and find some evidence that local ordinances were negatively related to cigarette consumption. Chaloupka (1992) uses data from the Second National Health and Nutrition Examination Survey (NHANES II) and finds that state clean indoor air laws are associated with lower cigarette consumption, with results driven mostly by males. These cross-sectional studies generally do not address the possibility that other unobserved factors may contribute both to the presence of smoking restrictions and to outcomes such as cigarette consumption.

Economists have used a variety of approaches to account for nonrandom adoption of smoking bans. Evans, Farrelly, and Montgomery (1999) use multiple U.S. sources of data on privately initiated workplace smoking bans in the early 1990s and find a strong negative relationship between a workplace smoking ban and smoking participation and intensity. To address concerns about unobserved third factors such as

3. We do not review here a series of studies that have examined internal documents from tobacco companies from the late 1980s into the mid 1990s that clearly indicate the tobacco companies believed that clean indoor air laws and workplace smoking bans reduced cigarette consumption (see, for example, Muggli et al. 2001). We also do not review here several studies that have examined the effects of laws banning smoking in bars and restaurants (which, strictly speaking, are also workplace bans for bar and restaurant workers). That literature has mainly focused on whether such policies affect employment in the ban-adopting area (see, for example, Adams and Cotti 2007). We do not have sufficient sample sizes to meaningfully estimate effects on bar and restaurant workers, and our institutional setting does not allow us to credibly identify the separate effects of local workplace smoking laws from local bar/restaurant smoking laws.

preferences for risk or health that may bias the cross-sectional relationships, these researchers use firm size as an instrumental variable and find that the IV estimates of the effect of smoking bans on own-smoking are slightly larger than the OLS estimates, suggesting a causal effect of bans at reducing smoking. Another common approach is to consider outcomes before and after smoking ban adoption in a quasi-experimental setting. Yurekli and Zhang (2000) use state panel data on cigarette sales in the United States from 1970–95 and control for clean indoor air laws through the use of an index similar in spirit to those described above. Tauras (2005) applies a similar framework to microdata from the Tobacco Use Supplements of the Current Population Survey over the period 1992–99. Importantly, both Yurekli and Zhang (2000) and Tauras (2005) estimate models with unrestricted state and year fixed effects, and both studies find that the clean indoor air laws are significantly and negatively related to cigarette consumption.[4]

To summarize, there is a large body of evidence on the question of whether smoking bans improve worker outcomes. Despite this, several gaps in the literature remain. First, most studies of smoking laws focus on state policies—indeed, all of the quasi-experimental research considers only state laws. This is problematic because the vast majority of the "action" in adoption of smoking restrictions in both the United States and Canada is at the local level, not the state or province. Moreover, states and provinces that did adopt wide-scale laws were almost always preceded by the presence of strong local laws; as such, state policies are not the appropriate level of aggregation. Second, the studies that do consider local ordinances do not account for third factors that may influence both the adoption of a local law and outcomes. We use the adoption of numerous local ordinances in a quasi-experimental setting with county and year fixed effects to account for time invariant characteristics of localities that may determine both bylaw adoption and outcomes. Third, most economics research on smoking laws focuses on smoking outcomes but ignore environmental tobacco smoke (ETS) exposure, which is the stated target of the laws. We measure ETS outcomes directly using the respondents' self reports.

Another important gap in the literature is that nearly all research on workplace smoking laws has failed to test for effects on workplace ban presence.[5] Of course,

4. There is much less research on the effects of smoking laws in Canada. Stephens et al. (1997) used a cross-sectional data set of Canadian residents in different provinces and compared residents in areas with "extensive" and "weak" coverage of smoking bylaws in 1990/91; they found that individuals in extensive coverage areas had 21 percent lower odds of being a current smoker compared to individuals in areas with weak coverage. That study, however, included *any* municipal bylaw that restricted smoking. Indeed, their bylaw data show that fully 76 percent of Ontario residents were covered by a municipal bylaw that restricted smoking as of 1991. It is likely that these included much less extensive restrictions such as Toronto's 1970s law that banned smoking in elevators, escalators, and service lineups. Our approach, in contrast, focuses only on bylaws with substantial worksite smoking restrictions. As indicated by independent data from the Ontario Tobacco Action Network (described below), these did not proliferate across Ontario until the late 1990s. Another recent Canadian study used a telephone survey of former smokers in Waterloo, Ontario, and information on the timing of when they quit smoking to assess whether the city's smoke-free bylaw played a role (Hammond et al. 2004). Using self-reported assessments of the importance of the smoke-free bylaw, the researchers conclude that more stringent smoke-free policies "were associated with a greater impact upon motivations to quit."

5. A notable exception is Pierce et al. (1994) who test directly for ban presence. Their design, however, uses across location variation for a single cross-section.

it is intuitive that public policies mandating worksite smoking bans should have increased workplace smoking restrictions. This is particularly likely to be true for the strong local laws we consider here, since as the Canadian Non-Smokers' Rights Association notes, "'[B]ottom-up' action at the municipal level can be skill-building, brings a sense of ownership over local issues, and can sometimes create a greater awareness of the law, hopefully leading to satisfactory compliance" (2005).[6] Indeed, newspaper reports indicate that these laws were actively enforced by local public health units in Ontario, and the penalties for noncompliance—though they varied across locality—were sizable (for example, Toronto's 1999 ordinance provided for a fine to employers of $5,000 per violation).[7] At the same time, however, there are some reasons why a workplace smoking law might not directly affect actual worksite smoking restrictions. Some employers may choose to actively defy the smoking ordinance, and there is ample anecdotal evidence of this in the United States, Canada, and elsewhere—particularly among owners of eating and drinking establishments. Another possibility is that workplace smoking laws in the United States and Canada simply codified what was already in practice at private worksites (that is, if places would have gone smoke-free without the push of government intervention).[8] Our detailed data on the smoking policies at the individual's worksite allow us to provide new evidence on this question.

Finally, research has ignored the possibility that smoking bans may have different effects by occupation group. Although it has been previously documented that blue collar workers have poorer ETS and own smoking outcomes compared to white collar workers (see, for example, Wortley et al. 2002, and Gerlach et al. 1997), the existing literature on this topic is descriptive in nature.[9] Our research is the first to evaluate whether local smoking ordinances have different effects by class of worker.

6. Levy and Friend (2003) also hypothesize that local laws may engender stronger community support than larger-scale efforts, thereby also increasing compliance.
7. Many local ordinances (and the 2006 provincial law) require employers to: make employees aware of the new policies, remove ashtrays from the workplace, ensure that no one smokes in the workplace, ensure that noncompliers do not remain in the workplace, and post "No Smoking" signs at all entrances, exists, washrooms, and "other appropriate" locations. The city of London, Ontario's 2003 ordinance, for example, provided for a $100 fine for each sign infraction (failure to post a "No Smoking" sign).
8. This reflects the experience of hotel and restaurant chains over the past two decades, for example. Below, we show that this was also the experience of many white collar workers in Ontario: Well before the period of widespread local bylaw adoption, the vast majority of these workers were employed at worksites that banned smoking (that is, their employers restricted smoking without government intervention). Understanding the relationship between laws and actual workplace restrictions also affects the interpretation of the many studies that relate the laws to smoking behavior, since it is not possible to attribute smoking effects to workplace bans if the laws do not systematically affect workplace restrictions. In fact, some studies have found that smoking laws specifically directed at workplaces are not significantly related to smoking behaviors (see, for example, Tauras 2005); one possible reason is that the laws might not affect actual worksite smoking restrictions, even if worksite restrictions do have real causal effects on smoking.
9. Wortley et al. (2002) found heightened serum cotinine levels—a biological marker of ETS exposure—among operators, fabricators, and laborers. Gerlach et al. (1997) find that construction trades workers, fabricators, machine operators, and mechanics had some of the lowest smoke-free workplace coverage in the 1992/93 Tobacco Use Supplements to the Current Population Surveys.

III. Data Description and Empirical Approach

Our outcome data come from restricted-use versions of the 1997–2004 Centre for Addiction and Mental Health (CAMH) Monitor. The CAMH monitor is a telephone survey administered throughout the calendar year. These data are repeated cross-sections of approximately 2,300 adults in Ontario each year and contain detailed geographic information on the respondent's residence as measured by the first three characters of the individual's self-reported postal code. The CAMH monitor also includes standard demographic characteristics for all respondents, such as age, sex, marital status, and education. We control for these demographic characteristics in the regression models below.

In each year a subset of the core sample was asked a set of questions about tobacco policy, including smoking restrictions at their workplace. Individuals are first asked whether they work outside the home, and if so what the smoking restrictions are at their place of work.[10] Specifically, respondents are asked, "Which of the following statements describes the policy on smoking where you work? One: Smoking is allowed anywhere. Two: There are smoking areas indoors. Three: Smoking is only allowed outside. Or, Four: Smoking is not allowed at all. Not allowed at all means no smoking on company property, both indoors and outdoors." We first define an outcome called No Workplace Policy equal to one if the individual reports that smoking is allowed anywhere and zero otherwise. This outcome is meant to assess whether local ordinances affect the extensive margin by inducing some worksites with no workplace policy to adopt some nontrivial restriction. We next create an indicator called Ban Presence equal to one if the individual reports that smoking is completely banned on company property or if smoking is only allowed outside and zero otherwise.[11] Finally, we use the range of responses described above to create a simple index called Ban Range that equals one if smoking is allowed anywhere at work, two if there are smoking areas indoors, and so forth. Throughout, we assume that respondents report their current workplace smoking policy. This seems warranted given the wording of the question.

Individuals are also asked about exposure to environmental tobacco smoke at their place of work. Specifically, individuals are asked, "In the last week, how many days were you exposed to *other people's* [emphasis added] tobacco smoke while you were at work? By exposed, I mean spending at least five minutes in an area where someone is smoking." We create an outcome variable called No ETS equal to one if the

10. Later in the survey adults are also asked about their detailed labor force status, including: full-time or part-time job, sick leave, unemployed, retired, homemaker, student, or self-employed. We restrict attention to full-time workers, part-time workers, and those who say they have a job but are currently away from it (for example, because of sick leave or vacation).

11. Because the response options in the CAMH monitor are slightly different than associated options in U.S. data (such as the National Health Interview Survey), our "Ban Presence" outcome should most closely be thought of as a complete "work area smoking ban," such as those used by Evans et al. (1999). Note also that most U.S. data sources only ask the workplace smoking restriction question to those who work indoors; there is no such sample restriction used in the CAMH monitor. This is a limitation of the CAMH monitor data, since one would ideally remove outdoor workers from the sample since "clean indoor air laws" are fairly meaningless for this group. This should bias us against finding effects, since individuals who should plausibly not be affected by the local ordinances are unfortunately included in the sample. Our results, however, are robust to excluding the small number (about 1-2 percent) of workers in occupations related to farming, fishing, forestry, and mining (those least likely to be working indoors).

respondent reports zero days of exposure to ETS at work and zero otherwise. We also create a variable called Daily ETS Exposure that equals one if the respondent reports exposure to other people's ETS on five or more days in the last week. Our use of self-reported days of exposure is supported by previous research which demonstrates that these self-reported outcomes are strongly correlated with other biological markers of environmental tobacco smoke such as ambient nicotine measurements (Coghlin, Hammond, and Gann 1989).[12] Finally, we use information on the respondent's own smoking behavior to create a dummy variable indicating the respondent is a Current Smoker.[13]

Our local workplace smoking restriction data are publicly available and come from a publication entitled "Municipal Smoke-Free Bylaws in Ontario" from the Ontario Campaign for Action on Tobacco (www.ocat.org). This organization tracks the implementation dates of the local bylaws in Ontario and also indicates whether the ordinance permits Designated Smoking Rooms (DSRs) or requires 100 percent compliance with smoke-free areas. We match the localities covered by workplace smoking bans in the OCAT according to the geographic residence information provided in the CAMH monitor, and we create a variable called Workplace Bylaw that equals one if the individual's residence is covered by a local ordinance that requires workplaces to be 100 percent smoke-free and zero otherwise.

Our main empirical approach is two-way fixed effects, in which the change in outcomes (ban presence, days of ETS exposure, and own smoking behavior) of individuals living in ban-adopting areas around the time of ban implementation are compared to the associated changes in outcomes for otherwise similar individuals living in areas in Ontario that did not adopt a ban at the same time. The key identifying assumption in this model is that there were no other shocks at the same time of the implementation of the ban that differentially affected outcomes. This difference-in-differences approach addresses unobserved area-specific heterogeneity through the inclusion of county fixed effects.

We implement the basic model by estimating the following reduced-form regression on the sample of adults who report working outside the home and have no missing data on the demographic characteristics or outcome variables:

$$(1) \quad Y_{ict} = \alpha + \beta_1 X_{ict} + \beta_2 (Local\ Workplace\ Bylaw)_{ct} + C_c + T_t + \epsilon_{ict}$$

where Y_{ict} refers to the various outcomes described above for individual i in county c in survey year t.[14] X_{ict} is a vector of demographic information that includes age, sex,

12. Importantly, the CAMH monitor question on ETS exposure provides a specific, objective outcome measure of ETS exposure that does not require knowledge about the official smoking policy at the worksite. That is, even individuals responding that they did not know the policy on smoking at their worksite gave valid responses to questions about ETS exposure. This marks a notable improvement of the CAMH monitor over other similar data sources in the United Sates, such as the Tobacco Use Supplements to the Current Population Survey, which until 2003 only asked the questions about ETS exposure to those who reported the presence of an official workplace smoking policy. Since it is possible that one might detect the effects of a ban on reported ETS exposure but not ban presence, the CAMH monitor questions are likely preferred. In practice, however, the number of "don't know" responses to the question about workplace smoking policy is too small to have substantive effects on our estimates.

13. Smoking intensity is only observed for daily smokers, so we do not analyze it here.

14. In the tables and text we report and refer to probit marginal effects estimated at the sample means for the dichotomous outcomes (reported ban presence, no workplace restrictions, current smoker). For the continuous outcome (ban range), we estimate the models using OLS.

marital status (three categories), and education (four categories). C_c is a vector of county dummies, while T_t is a vector of survey year dummies.[15] *Local Workplace Bylaw*$_{ct}$ is an indicator variable equal to one if the respondent lives in a place covered by a relevant local smoking law. The coefficient of interest, β_2, captures the effect of the local bans as measured by the change in outcomes for individuals living in the treated areas relative to the associated change in outcomes for individuals living in nontreated areas. All models use the sampling weights provided by the CAMH monitor, and we cluster standard errors by county (Bertrand, Duflo, and Mullainathan 2004).[16] (See map, Figure 1.)

One important limitation of the data in the context of evaluating the effects of local smoking bylaws is that the CAMH monitor identifies the location of the respondent's residence, not the location of work. This is a common problem in evaluations such as ours—including all the related literature cited above—and there is little we can do to correct for this slippage. If home/work travel patterns in Ontario are similar to those for its neighbors in the United States, however, this is unlikely to seriously bias our estimates; almost three-quarters of workers aged 16 and older in the United States live and work in the same county (U. S. Census Bureau 2006).[17] Below, we use a variety of approaches in our robustness analyses to assess the degree to which our main estimates are likely to be affected by any work/home slippage.[18]

15. Note that our baseline model includes county fixed effects as opposed to, say, postal code or region. Although such models produced similar results, our focus on county derives from the fact that it is the most common subprovincial level at which health policies such as workplace smoking bylaws are set in Ontario. Although some small towns adopted 100 percent smoke-free ordinances, the large share (70 percent) of our Workplace Bylaw indicator is composed of ordinances set at the county level. Of course, we still make use of the postal code information to account for the handful of towns and large cities at the subcounty level that also adopted smoke-free ordinances (for example, Windsor) but that would not be appropriately captured by a county-specific algorithm. Unfortunately, "cities" and "towns"—unlike counties—are not identified in the CAMH monitor. Although we could aggregate postal codes to create such measures, this would be incomplete and subjective. Finally, information on postal code is missing for approximately 2 percent of the sample; in contrast, we observe the county for each respondent. Technically, Ontario's "counties" are a combination of counties, districts, regional municipalities, and other governmental designations. Throughout, however, we use the "county" variable in the CAMH monitor data. There are 60 county units in our analysis.

16. Note that for the model predicting smoking status we do not control for other tobacco policies such as cigarette excise taxes. These policies generally vary at the provincial (as opposed to county) level. We assume such policies affect all Ontario residents equally.

17. We do not know the associated figure for Ontario because county identifiers are not available in the public use versions of the Canadian Census. As in the United States, there is substantial heterogeneity in the size of Ontario counties. We provide a map of Ontario and its county borders in Figure 1. To provide a sense of this, note that 90 percent of Ontario's land area but only about 7 percent of its population is represented in the Northern part of the province. In Southern Ontario, counties are smaller and more densely populated. Toronto, Ontario's most populated city, covers about 700 square kilometers; its surrounding municipalities are York (1760 km^2), Peel (1240 km^2), and Durham (2500 km^2). As a point of comparison, the average land area of the 83 counties in Michigan (Ontario's U.S. neighbor to the west and south) is about 1,700 km^2.

18. For example, we use knowledge about the heaviest commuting patterns (from residents outside Toronto into Toronto for work) to estimate models that exclude observations where work/home slippage is likely to be the most severe. We also investigate sensitivity to considering only county-wide bylaws, since the work/home slippage is likely to be more problematic for the ordinances adopted by smaller individual towns. Also, note that any mismatch between location of work and location of residence will result in mismeasurement of the bylaw variable, which is likely to bias the bylaw estimate toward zero.

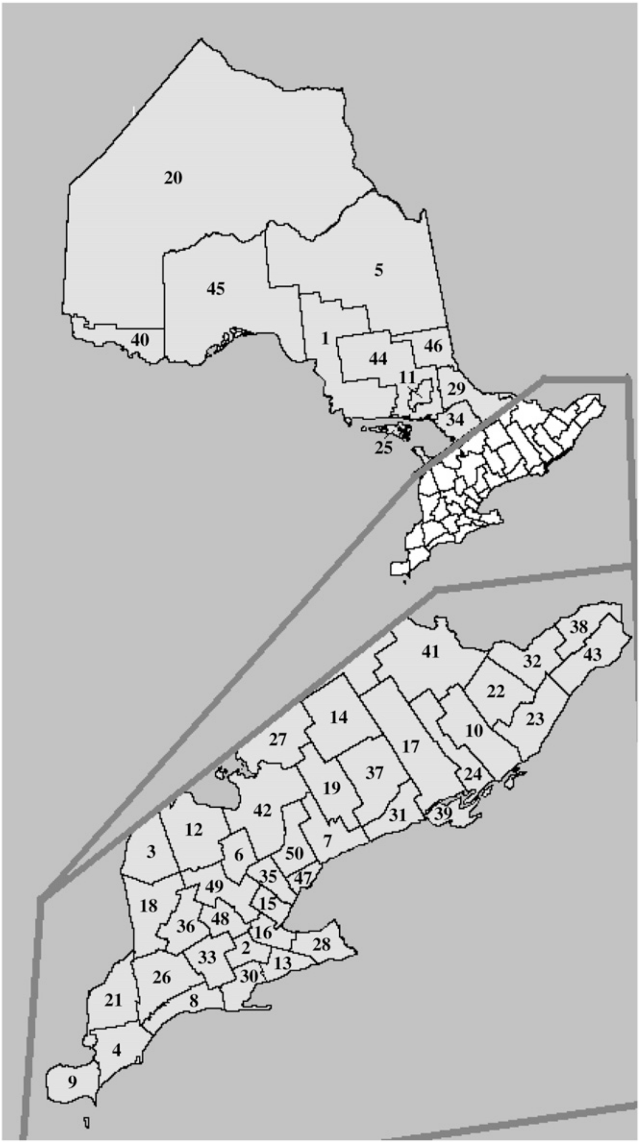

Figure 1
Map of Ontario County/Regional Municipality/District Boundaries

Note: To provide a sense of size, note that Toronto (#47) covers approximately 700 square kilometers.

Another data issue worth noting is that that the questions about workplace bans and ETS exposure were only asked of a random subset of respondents in each year. Because of this, we can only estimate the effect of local bans on reported ban presence and ETS exposure for a subset of the CAMH respondents. In contrast, the questions about own smoking behavior were asked of all adults. If we are willing to assume that the effect of the local bans on reported ban presence is the same for the respondents who were not asked the questions about workplace smoking restrictions—as seems reasonable—then we can still estimate the reduced-form effect of local policy adoption on own smoking outcomes for the entire sample and benefit from the increased precision afforded by having essentially twice as many observations as for the analyses of ban presence. We pursue this approach below.[19]

IV. Results

We present descriptive statistics for the main sample and separately by the presence of a local workplace smoking bylaw in Table 1.[20] The patterns suggest that localities adopting bylaws are somewhat different from nonadopters: Individuals living in areas covered by a workplace bylaw are more highly educated, less likely to be married, and less likely to be laborers. We also provide means for the relevant outcomes pertaining to workplace bans, ETS exposure, and own smoking. These patterns provide suggestive evidence that local bylaws may have improved outcomes: Workers in areas with a bylaw are more likely to report a work area ban on smoking and are less likely to report the absence of any workplace smoking restrictions. Workers in areas with local bylaws are also more likely to report zero days of ETS exposure and less likely to report daily ETS exposure. Finally, workers in areas with local bylaws have lower own-smoking rates than workers in areas without such bylaws. We investigate whether these mean differences in outcomes survive regression adjustment and controls for unobserved area heterogeneity in our regression models below.

An important first step in evaluating the effectiveness of local smoking bylaws in Ontario is showing that they affected respondents' reports of smoking restrictions at their place of work. Indeed, a key goal of our research is to provide evidence on the underlying mechanisms through which local ordinances affect actual adoption of workplace bans. If the local laws did not affect worker reports of ban presence—due, for example, to noncompliance or a high rate of privately initiated worksite smoking restrictions prior to bylaw adoption—then our research design would have little power to answer questions regarding the effects of these laws on other outcomes such as ETS exposure (distinct from reported ban presence) and own smoking behavior.

19. Moreover, it is not always the case that the same respondents were asked about both workplace bans and ETS exposure. As such, we also estimate the ETS exposure models on the sample of all individuals who were asked the ETS exposure questions, again assuming that the effect of local bylaws on ban presence is constant.

20. The sample includes only those respondents with valid responses to the workplace smoking ban question (that is, the sample excludes nonworkers), though sample characteristics for the full sample were very similar.

Table 1
Descriptive Statistics, CAMH Monitor 1997–2004

Variable	Full Sample	Local Bylaw = 0	Local Bylaw = 1
Age	39.2 (0.176)	39.2 (0.203)	39.1 (0.362)
Male	0.53 (0.007)	0.53 (0.008)	0.54 (0.016)
Less than high school	0.10 (0.004)	0.11 (0.005)	0.05 (0.007)
High school degree	0.25 (0.006)	0.26 (0.007)	0.22 (0.013)
Some college	0.34 (0.007)	0.35 (0.008)	0.31 (0.015)
University degree	0.31 (0.007)	0.28 (0.008)	0.40 (0.016)
Married	0.66 (0.007)	0.69 (0.008)	0.59 (0.016)
Never married	0.24 (0.007)	0.22 (0.008)	0.30 (0.015)
Blue collar worker	0.30 (0.007)	0.31 (0.008)	0.27 (0.014)
Other worker	0.70 (0.007)	0.69 (0.008)	0.73 (0.014)
Work area ban	0.74 (0.007)	0.70 (0.008)	0.84 (0.012)
Ban range	2.82 (0.011)	2.77 (0.013)	2.95 (0.021)
No work restrictions	0.07 (0.004)	0.08 (0.005)	0.04 (0.006)
No ETS Exposure	0.66 (0.009)	0.64 (0.011)	0.69 (0.015)
Daily ETS Exposure	0.20 (0.008)	0.21 (0.009)	0.17 (0.012)
Smoker	0.29 (0.007)	0.30 (0.008)	0.25 (0.014)
N	5,917	4,620	1,297

Note: Weighted means, workers only. Standard errors in parentheses.

We address this first question in Table 2, which shows the estimates of the effects of local smoking bylaws on reported ban presence over the period 1997–2004.

The format of Table 2 is as follows: We present results for the likelihood of reporting a complete ban on smoking in one's work area in the top panel, results for the likelihood of reporting that smoking is allowed anywhere at work (that is, no restrictions) in the bottom panel, and results for an index of smoking restrictions called Ban Range in the middle panel. The top two rows with dichotomous outcomes are estimated using probit, and we report the associated marginal effects. The bottom row is estimated using OLS. In Column 1 we report results from the model that includes demographic controls, year fixed effects, and county fixed effects for the full sample. Columns 2 and 3 perform the same exercise for the subsamples of males and females, respectively. The estimates in the Column 1 of Table 2 provide evidence that the local restrictions represent a true "program." Local smoking bylaws are associated with a statistically significant increase in the likelihood an individual reports a work-area smoking ban in the full sample. Relative to a prereform mean of 0.70, our results suggest that local bylaws increase ban presence in the aggregate by about 8 percent.[21] In the middle panel we find that a local bylaw significantly reduces the

21. We present an expanded set of coefficient estimates for this model in Appendix Table 1. The control variables entered as predicted: There is an increasing trend in ban presence over time, and highly educated workers and females are more likely to work at sites that ban smoking.

Table 2
Local Bylaws Toughened Workplace Smoking Restrictions

	(1) All	(2) Males	(3) Females	(4) Blue Collar Workers	(5) Other Workers
Work area ban					
Prereform mean	0.700	0.635	0.773	0.537	0.774
Local workplace bylaw	0.056**	0.113***	−0.010	0.135***	0.022
	(0.025)	(0.026)	(0.033)	(0.041)	(0.025)
Pseudo R-squared	0.099	0.104	0.078	0.097	0.080
N	5,917	2,930	2,979	1,755	4,143
No work restriction					
Prereform mean	0.076	0.116	0.031	0.173	0.032
Local workplace bylaw	−0.022***	−0.049***	0.005	−0.089***	0.001
	(0.007)	(0.009)	(0.014)	(0.017)	(0.009)
Pseudo R-squared	0.120	0.097	0.098	0.104	0.094
N	5,852	2,899	2,521	1,715	3,748
Ban range					
Prereform mean	2.77	2.63	2.93	2.45	2.92
Local workplace bylaw	0.065**	0.126***	−0.017	0.235***	−0.006
	(0.029)	(0.040)	(0.067)	(0.063)	(0.036)
Pseudo R-squared	0.105	0.105	0.065	0.111	0.069
N	5,917	2,930	2,987	1,755	4,162

Note: Data come from restricted-use versions of the 1997–2004 CAMH monitor. Work area ban is an indicator equal to one if smoking is allowed only outside or not at all on company property and zero otherwise. No work restriction is an indicator that equals one if smoking is allowed anywhere at work and zero otherwise. Ban Range is an index where 1 indicates that smoking is allowed anywhere and 4 indicates that smoking is not allowed anywhere on company property. Estimates in the top two rows for the dichotomous outcomes are implied marginal effects of a one unit change in the bylaw dummy (that is, from 0→1) derived from probit models. Sample sizes in these rows sometimes differ from those in the bottom row due to the fact that some small counties have no variation in the dichotomous outcomes, and observations in those counties are dropped from the probit estimation. Estimates in the bottom row are from OLS regressions on the continuous ban range outcome. All models control for county and year fixed effects and demographic characteristics (age, sex, education—four categories; and marital status—three categories) evaluated at sample means. Standard errors are printed below in parentheses, clustered by county. In all cases, the sample excludes individuals reporting they don't know or refused a response to the question about workplace smoking restrictions. *** Significant at 1 percent, ** Significant at 5 percent, * Significant at 10 percent.

likelihood that an individual reports that "smoking is allowed anywhere" at work by about 2.2 percentage points, and the bottom panel also shows evidence of a significant increase in workplace bans associated with local bylaws. In Columns 2 and 3 we confirm the pattern in previous research (Chaloupka 1992) that smoking laws are more effective in the male sample; all of the bylaw estimates for males are larger than the full sample estimate and highly significant. In contrast, estimates for females in Column 3 are much smaller, wrong-signed, and always statistically insignificant.

The difference in the apparent effectiveness of local bylaws at affecting workplace restrictions in the male versus female samples is surprising, because it is unlikely that sex per se is responsible for the large differences. Put differently, why would men be systematically more responsive to the local laws than women? Given this, we further investigate these relationships in Columns 4 and 5 of Table 2 by examining heterogeneity in the effects of local bylaws by occupation group. Specifically, we stratify the sample into two types of occupations: blue collar workers in Column 4 (including occupations related to processing, machining, product assembly, transport, and material handling) and other workers in Column 5 (such as professionals, administrative workers, clerical workers, and sales workers), which we defined using the occupation code available in the CAMH monitor data. Blue collar workers have been previously identified in public health research as being at especially high risk for ETS exposure (Gerlach et al. 1997 and others).[22] Moreover, our data show stark differences in the presence of workplace smoking bans associated with occupation. Table 1 showed that blue collar workers are far less likely than other workers to report that smoking is prohibited at their workplace.

The results in Column 4 of Table 2 show that the estimated effect of the local workplace bylaws is consistently large and statistically significant with respect to all three workplace ban outcomes for blue collar workers. This is particularly true for the extensive margin in the middle row; adoption of a local bylaw in Ontario reduced the likelihood of reporting that smoking is allowed anywhere at work by more than 50 percent. We also find a statistically significant increase in the likelihood of reporting a work area smoking ban on the order of 25 percent (top panel), and the results on the ban range index (bottom panel) are similarly positive and statistically significant. These results are also supported visually in Figures 2 and 3, which shows that workplace policies were actually worsening among these workers until the period of widespread bylaw adoption starting around 2000. For other (nonblue collar) workers in Column 5 we find no evidence that local bylaws changed any of the measures of ban presence. All of the estimates for these other workers are much smaller and statistically insignificant.

The results in Table 2, therefore, demonstrate that local smoking bylaws largely leveled the playing field between white collar and blue collar workers by inducing worksites such as factories and warehouses to adopt a workplace smoking policy.[23] In contrast, white collar workers were overwhelmingly employed at establishments

22. While food service workers have also been shown to have heightened risk, there are simply too few of these types of workers to provide a meaningful analysis using the CAMH monitor data. There are only 139 workers who could arguably be classified as "food service" workers (for example, bartenders, waiters/waitresses, cooks, kitchen helpers, etc.) in the CAMH monitor over this entire time period.

23. Note that we do not observe actual employers in the CAMH monitor data, so there is no way to identify the degree to which blue collar workers and other workers overlap within a firm. This information would be useful, since if there were equal representation of blue collar workers and other workers at any given worksite, it would be difficult to rationalize the differential effects of local bylaws on workplace smoking restrictions. In reality, there is some distribution of blue collar/white collar worker mix across firms, and the results suggest that establishments with predominantly blue collar workers are driving the main effects. An alternative explanation is that even at the same firm actual or perceived smoking restrictions vary across worker types (for example, if smoking were permitted on the factory floor but prohibited in the management offices at the same site), and local smoking bylaws may have reduced the blue collar/white collar disparity in workplace smoking restrictions even at the same establishment.

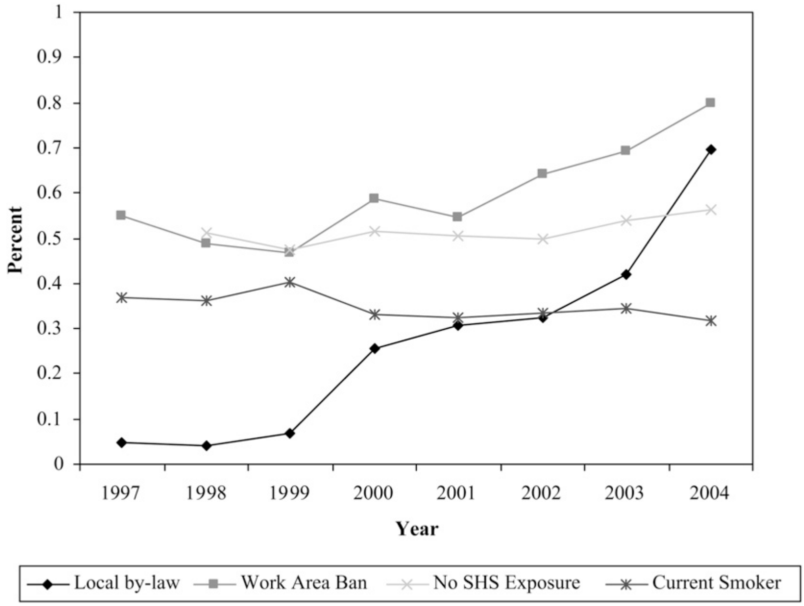

Figure 2
Trends in Local By-Laws, Work Smoking Policies, SHS Exposure, and Own-Smoking, Blue Collar Workers, CAMH Monitor 1997–2004

that were already smoke-free before local bylaws were adopted. These factors are visually apparent in the raw trends in outcomes presented in Figure 4, which shows that the gap in work area bans just before the main period of local bylaw adoption (2000–2004) was well over 30 percentage points but fell to just around ten percentage points by the end of the sample period during a time of widespread local bylaw adoption. Moreover, these occupational patterns can largely account for the apparent male/female differences in Columns 2 and 3 of Table 2, because men constitute more than three-quarters of the blue collar sample but only 43 percent of "other worker" sample.

We performed several robustness tests to evaluate the main result that local bylaws in Ontario increased reported worksite bans for laborers. We first estimated models that excluded residents from the regional municipalities (aka counties) surrounding Toronto (York, Peel, and Durham). These workers may be differentially likely to commute into Toronto for work; as such, excluding residents of the areas surrounding Toronto should alleviate concerns about the work/home slippage described above. Doing so returned estimates that were very similar to the baseline (all bylaw coefficients for blue collar workers are large and significant), providing suggestive evidence that work/home slippage is unlikely to bias our estimates of the effect of workplace bylaws. We also estimated models in which we only controlled for county level bylaws, ignoring the many small towns and even major cities (such as Windsor)

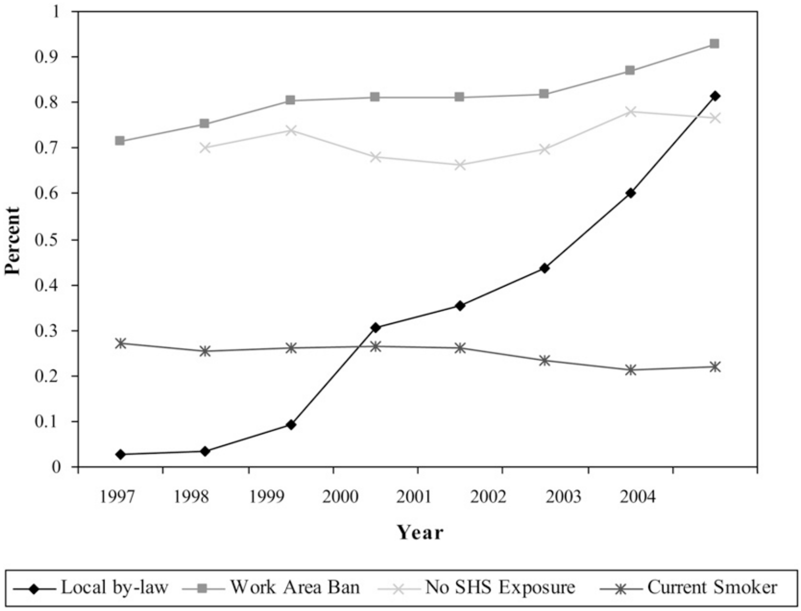

Figure 3
Trends in Local By-Laws, Work Smoking Policies, SHS Exposure, and Own-Smoking, Other Workers, CAMH Monitor 1997–2004

at the subcounty level that adopted workplace bylaws. Although we have no reason to believe that the subcounty bylaws were not binding—indeed, this is a key reason we are interested in subprovincial policies in the first place—doing so may permit a cleaner interpretation of our results with county dummies and alleviates the work/home postal code slippage which is likely to be more salient for bylaws adopted by very small towns. Overall, the estimates were little changed and remained statistically significant across the three workplace ban outcomes.

We also estimated models that restricted attention to individuals whose county or postal code of residence indicated that they live in a place that ever adopted a local bylaw over the sample period. Ensuring that the treatment effect estimate is driven by individuals in these "changer" communities is a standard robustness exercise, and the estimates from this exercise confirmed that the main results were not driven by worse outcomes in "control" areas when bylaws were adopted. In other models we restricted attention to the period of widespread bylaw adoption (2000–2004), and these models returned even larger estimates than the baseline that remained highly significant. We also estimated models that allowed for county-specific linear time trends in addition to county fixed effects. This is a common way to account for unobserved area-specific characteristics that are likely to trend smoothly over time, such as antismoking sentiment (Friedberg 1998). Again, all of our main results were similar in magnitude to the baseline estimates and retained statistical significance at standard confidence levels.

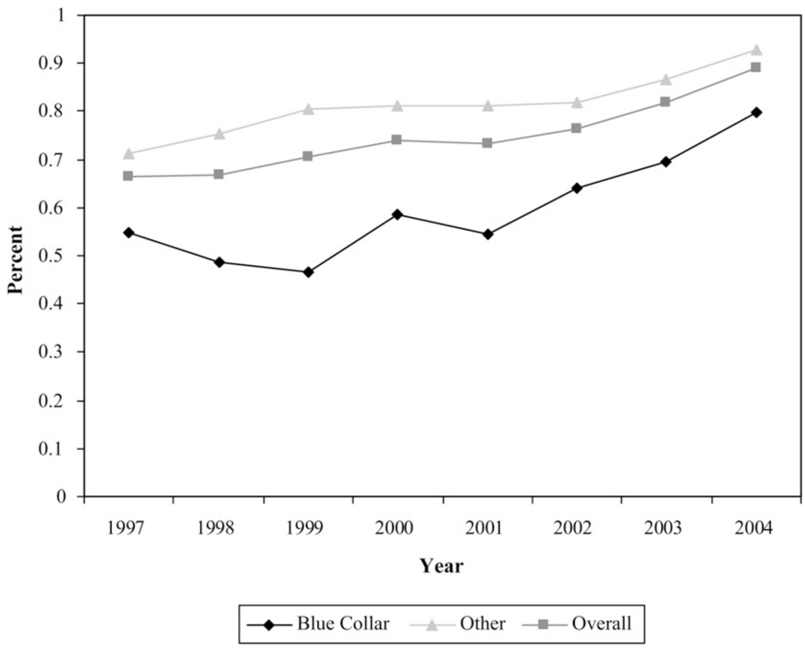

Figure 4
Trends in Work Area Bans, by Occupation, CAMH Monitor 1997–2004

Finally, we addressed the possibility of policy endogeneity, whereby localities might adopt smoking bylaws in response to changing outcomes for some group (for example, changes in white collar outcomes could lead to a local bylaw, which would invalidate the interpretation we have given to the pre/post comparisons). Indeed, our baseline model assumes that the timing of local bylaw adoption is exogenous to the unobserved determinants of ban presence. Although we are not aware of evidence regarding why certain localities adopted bylaws when they did, we assessed the empirical relevance of such a concern by explicitly tracing out the time path of the effects of the bylaws with year-long leads and lags of the main bylaw variable (excluding the year immediately prior to bylaw adoption in each locality). This is a common approach for evaluating potential policy endogeneity and allows us to consider short-run dynamics in the effects of the policies. The standard intuition with such an approach is that if the variation in the timing of bylaw adoption were truly exogenous, we would expect the leads of the bylaw indicators to be zero and the lags to be positive and significant (in the case of ban presence). Indeed, this is exactly what we found: Among blue collar workers, the policy leads in the two, three, and four or more years before policy adoption were all small and statistically insignificant. In contrast, the policy lags for one, two, and three or more years after policy adoption were each similar in size to the baseline estimate, and each was statistically significant. These patterns suggest that policy endogeneity is not empirically relevant in our context. Equally important, there was no strong pattern in the lead/lag specification predicting workplace bans for other workers, which is inconsistent with the idea that private establishments with white collar workers were *systematically* adopting strong restrictions leading up adoption of local laws. Put differently, although our results do indicate that the bylaws narrowed the blue collar/white collar gap in ban presence, they do not indicate that the bylaw adoptions were correlated with patterns of white collar outcomes in the years just before adoption.

Our results thus far confirm that local workplace smoking bylaws in Ontario were most (indeed, only) effective at improving worksite smoking policies among workers in blue collar occupations. Workers in other occupations, in contrast, were much more likely to have worked at job sites that restricted smoking (for example, offices) well before the period of local bylaw adoption; the local ordinances in Ontario appear not to have been binding for these individuals. This finding provides important information on the plausibility of estimates regarding the effects of local bylaws—and by implication workplace smoking bans—on outcomes such as ETS exposure and own smoking. Specifically, the estimates in Table 2 suggest that any improvement in outcomes for workers in Ontario over this time period should be primarily observed among blue collar workers if they are to be attributed to causal effects of smoking bans.

We present the reduced-form evidence on these questions in Table 3, which offers estimates on the main ETS exposure and smoking outcome variables of interest separately by broad occupational grouping. The format of Table 3 follows the previous tables, in that each row presents a different outcome and each column is a different occupation-specific sample. Specifically, we present results pertaining to ETS exposure in the top and second rows, while the bottom row presents results for the likelihood of being a current smoker. Column 1 presents results for blue collar workers, Column 2 presents results for other workers, and Column 3 presents results for retired persons and homemakers (whose smoking behavior should not have been directly affected by the local smoking ordinances).

The results in Column 1 of Table 3 confirm that local smoking bylaws—and by implication workplace smoking bans—were effective at improving ETS outcomes for blue collar workers. Specifically, we find in the top row that a local workplace bylaw significantly increased the likelihood of reporting zero days of ETS exposure at work by 13.2 percentage points for blue collar workers. Similarly, in the second row of Column 1 we find that a local workplace bylaw reduced the likelihood of reporting five or more days of ETS exposure per week by 11.5 percentage points, again statistically significant at the 5 percent level.[24] That these observed improvements are causal effects of the bans is further supported by the wrong-signed, smaller, and/or statistically insignificant estimates on those same ETS exposure outcomes for the other (nonblue collar) workers—the group that did not exhibit a substantive relationship between a local bylaw and reported worksite smoking policy.

24. Note that these large improvements in environmental tobacco smoke exposure also indirectly confirm the results on ban presence. A concern is that local bylaws may simply increase awareness of a smoking restriction by individuals who were previously unaware of the actual policy, even in the presence of no changes in actual restrictions. Because the environmental tobacco smoke exposure question is very objective, we can be more confident that the observed improvements in ETS exposure are real. Because these improvements are systematically associated with the local bylaws, the most straightforward explanation is that the local laws did, in fact, change actual worksite smoking policies for blue collar workers.

Table 3
Local Bylaws Reduced ETS Exposure for Blue Collar Workers

	(1) Blue Collar Workers	(2) Other Workers	(3) Retired persons & Homemakers (nonworkers)
No ETS Exposure			
Prereform mean	0.474	0.719	
Local workplace bylaw	0.132***	−0.0001	–
	(0.047)	(0.023)	
Pseudo R-squared	0.077	0.072	
N	1,273	3,159	
Five or more days ETS Exposure/Week			
Prereform mean	0.348	0.148	
Local workplace bylaw	−0.115**	0.038*	–
	(0.045)	(0.023)	
Pseudo R-squared	0.085	0.075	
N	1,265	3,151	
Current smoker			
Prereform mean	0.371	0.259	0.155
Local workplace bylaw	−0.047	−0.005	−0.002
	(0.032)	(0.019)	(0.019)
Pseudo R-squared	0.073	0.060	0.068
N	3,297	8,013	4,351

See notes to Table 2. Note that all outcomes in this table are dichotomous, so in each case we report the implied marginal effects from a probit regression.

The bottom row of Table 3 presents results for the likelihood of being a current smoker. The estimate in Column 1 for blue collar workers provides some support for the idea that workplace smoking bylaws (and by implication, workplace bans) affected own-smoking behavior. Specifically, we find that the probability that blue collar workers report being a current smoker fell by an estimated 4.7 percentage points when a bylaw was adopted (about 12.7 percent relative to the prereform mean), while the relevant estimates for other workers and nonworkers are much smaller. The own-smoking point estimate for blue collar workers—although statistically indistinguishable from zero—is very large.[25]

25. Our small samples do not permit us to identify statistically significant and plausibly sized effects of workplace bans on smoking. Note that we estimate a local law increases the probability of a complete work area ban by 13.5 percentage points for blue collar workers, and we estimate the local law reduces smoking in this group by 4.7 percentage points. The resulting implied IV estimate for the effect of bans on smoking is (implausibly) large (-0.047 / 0.135 = -0.35). Future research with larger samples should revisit this issue.

We also subjected the blue collar worker results on ETS exposure to the same battery of robustness checks described above for the workplace smoking ban outcomes. The patterns of estimates from these additional tests confirmed that the local smoking bylaws significantly improved ETS exposure outcomes for blue collar workers. As with the ban outcomes, we found that the main ETS results for blue collar workers were qualitatively and quantitatively robust to excluding the areas around Toronto, allowing for county-specific linear time trends, controlling for countywide laws, restricting attention the more recent time period since 2000, and restricting attention to individuals in ‘‘changer’’ communities. With respect to current smoking behavior, we found that the robustness exercises continued to return sizable estimated reductions in current smoking probability, ranging from 3.4 to 6.1 percentage points, though as with the baseline most were statistically insignificant.

Finally, we probed the plausibility of the estimated reductions in ETS and own-smoking for blue collar workers by estimating similarly specified models of Equation 1 on a set of outcomes that should plausibly be unrelated to the local smoking ordinances—and by implication workplace smoking bans. Specifically, we examined the probability the individual reports driving after having consumed at least two drinks in the past 30 days, the probability an individual reports fair or poor self-rated general health, and the probability the individual reports that she felt more unhappy than usual in the past few weeks.[26] Although one could argue that these outcomes could be plausibly affected through effects on own smoking behavior, such effects would be indirect and should arguably be smaller than the estimated smoking reductions. If we observed sizable improvements in these variables associated with local workplace bylaws, this would call into question our estimated improvements on ETS exposure and own-smoking outcomes, perhaps suggesting specification error or other unobserved coincident public health campaigns.

The overall patterns from this exercise strongly indicated that our estimated improvements in ETS and own-smoking outcomes for blue collar workers are not spurious. Specifically, across-all the ‘‘control’’ outcomes we considered, the vast majority of estimates on the local bylaw indicator were substantively zero and never statistically significant (for either the baseline estimation of Equation 1 or the additional robustness tests described above). Moreover, the signs of the estimates for the various outcomes were not consistent across specifications. These overall patterns of null findings for outcomes that should plausibly be further removed from the changes in smoking bans induced by the local smoking ordinances

26. Evans et al. (1999) also take a similar approach in evaluating the relationship between workplace smoking bans and health behaviors such as seatbelt use. An important consideration in choosing these ‘‘control’’ or ‘‘placebo’’ outcomes was the time frame of the question. Specifically, we restricted attention to outcomes pertaining to current or very recent conditions. This is why we chose, for example, not to consider overall alcohol use—the drinking question was asked using a time window of ‘‘past year.’’ Also, there is some evidence on the structural relationships between smoking and drinking: Picone, Sloan, and Trogdon (2004), for example, find that smoking bans reduce alcohol consumption among older individuals, and a large body of work shows drinking and smoking to be strongly complementary behaviors.

provide strong support for our interpretation that the local bylaws improved outcomes for blue collar workers.

V. Conclusion

Understanding whether and how much workplace smoking ordinances affect ETS exposure and own smoking among workers is crucial for a comprehensive evaluation of the costs and benefits of such ordinances, particularly because public health benefits such as reduced ETS exposure are explicitly stated as the main motivation for these laws. Unfortunately, most previous research suffers from one or more serious limitations. We have revisited this question using detailed outcome data from Ontario, Canada, and substantial variation in the timing of adoption of more than 100 local bylaws over the period 1997–2004. Our data are particularly attractive because—in addition to ETS exposure and own smoking outcomes—we observe the respondent's description of her worksite's smoking policies. This allows us to directly estimate the underlying "first stage" relationship between local policy adoption and subsequent worksite smoking policies. Previous research has implicitly assumed that these ordinances are binding; that is, that worksites for individuals who were previously "untreated" became "treated" when laws are adopted.

We confirm that local bylaws increase reported ban presence among workers along several different dimensions. This positive relationship survives inclusion of unrestricted county dummies to account for time-invariant unobserved heterogeneity. Importantly, however, we demonstrate that the aggregate benefits of local bylaw adoption with respect to reported ban presence are driven entirely by the blue collar sample. Other workers—the vast majority of whom were already working at job sites with strict workplace smoking policies well before the period of local bylaw adoption—experienced no marginal increase in ban presence when local bylaws were adopted. While intuitive, this is a new finding in the literature and suggests that recent movements toward occupation-specific bans in hospitality occupations (including bars and restaurants) may have overlooked an important group of workers who would benefit from increased protection.

We also demonstrate that the large increases in reported ban presence experienced by laborers were associated with significant improvements in health outcomes. Specifically, we estimate that local workplace bylaws—and by implication workplace smoking bans—reduced the likelihood of reporting daily ETS exposure by about 33 percent. Measured differently, we find that a local bylaw increased the likelihood of reporting no ETS exposure at work by 28 percent among these workers. Patterns of estimates for own-smoking behavior are consistent with the idea that workplace smoking bans reduce own smoking, though the baseline estimate is not statistically significant. Overall, our results provide important new insight into the underlying mechanisms through which smoking bans improve outcomes. Future research evaluating the effects of increasingly common state and province-wide bans must take care to account for these preexisting improvements associated with local bylaws in order to produce accurate estimates.

Appendix Table A1
Expanded Set of Coefficient Estimates Outcome is Work Area Ban (0/1)

Variable	Estimate
Local bylaw	0.056** (0.025)
1998	0.007 (0.025)
1999	0.036* (0.017)
2000	0.050*** (0.014)
2001	0.042* (0.022)
2002	0.062** (0.024)
2003	0.110*** (0.016)
2004	0.169*** (0.013)
Age	0.001* (0.0005)
Male	−0.112*** (0.012)
Previously married	0.001 (0.029)
Never married	−0.010 (0.015)
High school degree	0.032* (0.018)
Some college	0.085*** (0.021)
University degree or more	0.183*** (0.032)
Pseudo *R*-squared	0.099
N	5,917

Coefficients on county dummies not shown. See notes to Table 2.

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