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How Do Workplace Smoking Laws Work?

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Abstract

A large literature shows that state and local laws requiring smoke-free workplaces are associated with improved worker outcomes (lower secondhand smoke exposure and own smoking rates) in cross sectional analyses. In this study we provide new quasi-experimental evidence on the effects of workplace smoking laws by using the differential timing of adoption of over 100 local smoking by-laws in Ontario, Canada over the period 1997-2004. We employ restricted-use repeated cross section geocoded outcome data to estimate reduced form models that control for demographic characteristics, year fixed effects, and county fixed effects. Because we observe the respondent's report of the smoking policy at her worksite, we can directly test for compliance. Although results indicate that local by-laws increase workplace bans in the aggregate, we find that the effects are driven entirely by blue collar workers. Among blue collar workers, we find the largest effects on the extensive margin: local by-laws significantly reduced the fraction of worksites without any smoking restrictions (i.e. where smoking is allowed anywhere at work) by over half. These local policies also improved health outcomes: we find that adoption of a local by-law significantly reduced SHS exposure among blue collar workers by 25-30 percent, and we confirm that workplace smoking laws reduce smoking. For all outcomes we find plausibly smaller and insignificant estimates for white collar and sales/service workers – the vast majority of whom worked in workplaces with privately initiated smoking bans well before local by-laws were adopted. Overall our findings advance the literature by confirming that workplace smoking bans reduce smoking, documenting the underlying mechanisms through which local smoking by-laws improve health outcomes, and showing that the effects of these laws are strongly heterogeneous with respect to occupation.

Keywords: workplace smoking bans, SHS exposure, smoking, local policy adoption

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The state of California recently became the first state to declare that secondhand smoke (SHS) is 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 SHS. One of the most important ways this sentiment has been exhibited in the US is though the steady increase in restrictions on smoking at private worksites: by 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 US has examined the effects of these workplace restrictions: multiple reviews of the evidence find that workplace smoking bans are associated with reduced exposure to secondhand smoke (SHS) and lower rates of own smoking and per capita cigarette consumption (Fichtenberg and Glantz 2002, Levy and Friend 2003, Brownson et al. 2002, and others).

An increasingly common policy tool aimed at achieving these improved outcomes is the adoption of local laws requiring smoke-free workplaces. These policies are widespread in the US: as of 2005, 328 municipalities have adopted 100% smokefree workplace laws (Americans for Nonsmoker's Rights 2006), and if one includes less restrictive policies the number of smoke-free ordinances is well over 1000 (Brownson et al. 2002). Moreover, the trend in adoption of local ordinances has been steadily upward, a phenomenon that has also occurred across Canada. Although a handful of US states and Canadian provinces have recently also adopted tough 100% smoke-free workplace laws, it is hypothesized that adoption of local laws may engender stronger community support than larger scale efforts, thereby increasing compliance (Levy and Friend 2003).

Previous research on the effects of these local policies in economics and public health generally follows a cross-locality research design, in which outcomes (smoking, SHS exposure)

for residents of cities with smoking by-laws are compared to outcomes for residents in untreated areas. These studies typically find that individuals living in cities and towns with strong local ordinances have better outcomes than otherwise similar individuals living in areas without such policies or with weaker policies. These cross-locality research designs 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 the researcher in the datasets used in these studies is fairly limited, these biases can generally not be ruled out. In this paper we revisit the effects of local smoking policies by using a quasi-experimental research design applied to the case of Ontario, Canada – the most populated province in that country – over a period of rapid and widespread adoption of local smoking by-laws. In so doing, our goal is to provide new evidence on the effects of local smoking ordinances on reported ban presence, secondhand smoke exposure, and own smoking outcomes.

Our study makes several distinct contributions to the existing literature. First, we provide direct estimates of the extent to which local workplace smoking by-laws actually affect reported work ban presence. While this may seem obvious, a review of the literature suggests this is not the case: very little of the public health research and none of the economics studies on this topic examine actual compliance by worksites.[1] Since a necessary condition for these local ordinances to improve health outcomes is that they must be adopted by worksites, this is an independently worthwhile endeavor. Indeed, local by-laws might *not* translate into increased worksite policies because of lack of compliance or enforcement. Our data on worksite smoking policies – which we describe below – allow us to evaluate whether local by-laws mainly affect the extensive

[1] Notable exceptions Pierce et al. (1994) and Moskowitz et al. (2000). Both of these studies use a cross-sectional research design.

margin (by increasing the likelihood of reporting any workplace smoking restriction) or intensive margin (by increasing the intensity or strength of existing workplace smoking bans). Similarly, we are interested in whether local ordinances completely eliminate or simply reduce exposure to secondhand smoke among the targeted workers. Understanding whether there are qualitatively and/or quantitatively distinct effects on these margins is an important goal of our research.

Second, our analysis of local smoking ordinances explicitly accounts for time-invariant unobserved heterogeneity through the inclusion of county fixed effects. Most previous research either does not control at all for area effects or does so at a much broader level (e.g. state). In this respect, our use of a well-accepted two-way fixed effects methodology marks a notable improvement over much of the previous literature in public health and economics on local clean indoor air laws.

Third, this study is – to our knowledge – the first to consider the effects of smoking ordinances separately by white collar and blue collar occupations. Although it has been previously documented that blue collar workers have poorer SHS 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 almost entirely descriptive in nature. Our results uncover the first evaluative evidence that local smoking ordinances were especially effective at improving outcomes for blue collar workers and essentially ineffective at improving outcomes for other types of workers.[2]

[2] Our study also offers one of the first large scale analyses of the effects of smoking bans in Canada. Although a handful of Canadian studies have considered sector-specific (e.g. office buildings), single-site, or single-city evaluations in cities in Canada, it is not obvious that those results generalize more broadly (Broder et al. 1993, Hammond et al. 2004). The general paucity of Canadian studies is notable given that workplace smoking restrictions have proliferated in the country in the past decade and given the enormous growth of the US-focused literature.

Finally, our research design allows us to provide complementary evidence on the important question of whether workplace smoking bans reduce smoking. Although most data sources show a large negative cross-sectional relationship between the presence of a work area smoking ban and individual smoking participation, there are reasons to be skeptical that those single equation estimates reflect true causal effects (e.g. unobserved factors that may be correlated with both smoking decisions and workplace characteristics). Previous research has attempted to deal with this problem by using firm characteristics as instruments for workplace smoking bans and finds very little evidence of an omitted variables bias (Evans et al. 2000). Our use of the differential timing of adoption of local smoking by-laws provides an interesting complementary way to estimate whether plausibly exogenous variation in workplace smoking bans generated by these local by-laws is associated with predictable declines in smoking participation.

Our analyses use restricted-use repeated cross section data from the 1997-2004 CAMH Monitor (formerly the Ontario Adult Drug Monitor). These data provide information on individual outcomes both before and after the implementation of over 100 local smoking by-laws within Ontario. Importantly, these data also identify the respondent's postal code, which we use to provide a detailed match of the local smoking restrictions to the respondent's residence. 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 increased reported ban presence after accounting for individual demographics, year fixed effects, and county fixed effects. In the full sample, a local by-law is estimated to increase the likelihood of reporting a complete work area ban by about seven percent. We also find large estimated reductions in the

likelihood of reporting that smoking is allowed "anywhere" at work (i.e. no workplace restrictions). These aggregate effects, however, mask important differences associated with occupation. Specifically, the effects of by-laws on ban presence are driven entirely by blue collar workers. Among blue collar workers we find that adoption of a local by-law cuts the likelihood of reporting no workplace policy by about half and increases complete ban presence by about 25 percent. We also find a significant reduction in SHS exposure of about 25-30 percent and a large estimated reduction in own-smoking rates for workers in these occupations. Importantly, our estimates for outcomes of white collar and sales/service workers – whose worksites were much more likely to have privately initiated smoking bans in the pre-reform period – are plausibly smaller and statistically insignificant. Turning to unique data on attitudes regarding support for smoking restrictions in various venues (bars, restaurants, and workplaces), we find some support for the idea that health improvements experienced by blue collar workers were driven in part by the effects of the by-laws on social norms. That is, adoption of a local smoking by-law appears to have had small direct effects at increasing social disapproval of smoking in the relevant settings covered by the ordinances.

Taken together, our results confirm the beneficial effects of local clean indoor air laws – and by implication workplace smoking bans – on both the extensive and intensive margins but 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 and 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 goes into effect in Ontario in 2006. Since other states and provinces have similarly adopted wide ranging smoking

ordinances, it is particularly important to understand extent to which pre-existing local by-laws 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 very large body of economics and public health literature has considered the effects of workplace smoking bans. In fact, multiple reviews of the literature have been published in the past 5 years. These reviews generally focus on the effects of smoking bans on smoking behavior and SHS exposure (see, for example, Fichtenberg and Glantz 2002, Levy and Friend, 2003, and Brownson et al. 2002). An example is Evans et al. (1999) who use multiple US 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 preferences for risk or health that may bias the cross-sectional relationships, Evans et al. 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.

Most economics research on the effects of smoking bans, however, considers the effects of public policies that restrict smoking in various settings. Wasserman et al. (1991), for example, use several waves of the National Health Interview Survey from 1970-1985 and append state smoking regulations to the individual level data. They control for 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) also control for a local smoking regulations index in California that adjusted for exposure in a manner similar to the Wasserman et al. study; they also find some evidence that local ordinances reduced cigarette consumption. Chaloupka (1992) uses data from NHANES II and controls for the extent of workplace smoking restrictions at the state level. He finds that state clean indoor air laws are associated with lower cigarette consumption, with results driven mostly by males. Yurkeli and Zhang (2004) use state panel data on cigarette sales in the US from 1970-1995 and control for clean indoor air laws through the use of an index similar in spirit to those described above. Importantly, they include unrestricted state and year fixed effects and find that the clean indoor air law index is significantly and negatively related to cigarette consumption.[3]

Public health researchers have also generally followed a cross-locality research design to study the effects of local smoking restrictions. Moskowitz et al. (2000), for example, use a single cross section of the 1990 California Tobacco Survey and append independently coded data on the strength of local workplace ordinances. They find that, among individuals who were smokers or who had smoked in the 6 months prior to the survey, residents in areas with strong ordinances were significantly more likely to report the existence of a worksite smoking policy and to report quitting behavior compared to residents of areas with no local ordinance. Emont et al. (1992) 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

[3] It is worth noting, however, that the variation in the clean indoor air index in most of the economics literature cited above does *not* reflect the tough 100% completely smoke free policies we consider here. To see this, note that the organization Americans for Nonsmokers' Rights – which closely tracks state and local activity on these issues –

residents of states with more restrictive laws had lower cigarette consumption and higher quit rates than residents of states with less restrictive laws.

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 by-laws 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.[4] 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 recent smoke-free by-law played any role in helping smokers quit (Hammond et al. 2004). Using self-reported assessments of the importance of the smoke-free by-law, the researchers conclude that more stringent smoke-free policies "were associated with a greater impact upon motivations to quit."

To summarize, there is a large body of evidence on the question of whether smoking bans improve worker outcomes and on the effects of clean indoor air legislation. Our study differs from the previous literature in the following ways: 1) we are most interested in tough 100% completely smoke-free local by-laws as opposed to less restrictive policies pertaining to other public spaces; 2) unlike the majority of the economics studies, we consider only consider local laws, which allows us to abstract away from prices and policies that vary at the broader state or provincial level (e.g. cigarette taxes, antismoking media campaigns, etc.); 3) unlike the public health literature on local smoking laws, we explicitly account for time-invariant unobserved

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reported that no state had such a restrictive policy covering private worksites over the period of the economics studies cited above. These laws instead reflect restrictions in smoking government buildings, etc.

[4] That study, however, included any municipal by-law that restricted smoking. Indeed, their by-law data show that fully 76% of Ontario residents were covered by a municipal by-law that restricted smoking as of 1991. It is likely that these included much less extensive restrictions such as Toronto's 1970's law that banned smoking in elevators, escalators, and service line-ups. Our approach, in contrast, focuses only on by-laws with substantial worksite

heterogeneity through inclusion of county fixed effects; and 4) we provide the first estimates of the effects of local by-laws and smoking bans by broad occupation group.[5]

III. Data Description and Empirical Approach

Our outcome data come from restricted use versions of the 1997-2004 CAMH Monitor. The CAMH monitor is a telephone survey administered throughout the calendar year. These data are repeated cross sections of approximately 2300 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. We also observe information on the county and region of residence. The main topic coverage of the CAMH monitor is substance use; as such, these data provide rich information on own smoking behavior for all adults in the sample. We use these responses to create an indicator variable for being a Current Smoker. 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.[6]

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smoking restrictions. As indicated by independent data from the Ontario Tobacco Action Network, these did not proliferate across Ontario until the late 1990s.

[5] Again, we note that others have previously investigated the role of occupation, though not in an evaluative context. Wortley et al. (2002), for example, found heightened serum cotinine levels – a biological marker of SHS 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.

[6] 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).

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 asses whether local ordinances affect the extensive margin by increasing some worksites from no workplace policy to some nontrivial restriction. We then 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.[7] Finally, we use the range of responses described above to create a simple index called Ban Range that equals 1 if smoking is allowed anywhere at work, 2 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 secondhand 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 5 minutes in an area where someone is smoking." We create an outcome variable called No SHS equal to one if the respondent reports zero days of exposure to SHS at work and zero otherwise. We also create a variable called Daily SHS Exposure that equals one if

[7] Because the response options in the CAMH monitor are slightly different than associated options in US 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 US 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. If anything, 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.

the respondent reports exposure to other people's SHS on 5 or more days per 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 et al. 1989).[8]

Our local workplace smoking restriction data are publicly available and come from a publication entitled "Municipal Smoke-Free By Laws in Ontario" from the Ontario Campaign for Action on Tobacco (www.ocat.org). This organization tracks local bylaws in Ontario and codes these policies along several dimensions, including: whether workplaces, bars, and restaurants; whether the ordinance permits Designated Smoking Rooms (DSRs) or requires 100% compliance with smoke-free areas; and the implementation date of the ordinance. We match the localities covered by workplace smoking bans in the OCAT report with postal codes from the Canadian government, and we define a variable called Workplace By-Law that equals one if the individual's postal code is covered by any workplace smoking policy that requires workplaces to be smoke-free zero otherwise. In cases where the entire county or regional municipality is covered by a policy, we use the "county" variable in the CAMH monitor data to assign treatment status instead of the postal code.[9]

Our main empirical approach is two-way fixed effects, in which the change in outcomes (ban presence, days of SHS exposure, and own smoking behavior) of individuals living in ban-

[8] Importantly, the CAMH monitor question on SHS exposure provides a specific, objective outcome measure of SHS 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 SHS exposure. This marks a notable improvement of the CAMH monitor over other similar data sources in the US such as the Tobacco Supplements to the Current Population Survey, which only asks the questions about SHS exposure to those who report the presence of an official workplace smoking policy. Since it is possible that one might detect the effects of a ban on reported SHS 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.

adopting areas around the time of ban implementation are compared to the associated changes in outcomes for otherwise similar individuals living in other 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 fixed effects for postal code, county, or region.

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

(1) $Y = \alpha + \beta_1 X + \beta_2$ (Local Workplace By-Law) + Area + Survey Year + ε

where Y refers to the various outcomes described above. We use linear probability models (LPM) for the dichotomous outcomes for simplicity.[10] X is a vector of demographic information that includes: age, sex, marital status (3 categories), and education (4 categories). Area is a vector of dummies for either postal code, county, or region (depending on the model), while Survey Year is a vector of survey year dummies. Local Workplace By-Law is an indicator variable equal to one if the respondent's reported postal code is covered by a relevant workplace policy. 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 non-treated areas. All models use the sampling weights provided by the CAMH monitor, and standard errors are clustered by postal code.[11]

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[9] Unfortunately, the CAMH monitor data do not separately identify municipality or town within counties, so we pursue the postal code approach described above for the many sub county areas – small and large – that passed local smoking restrictions over this time period.

[10] Appendix Table 2 provides probit results that confirm our findings are not sensitive to estimation method.

[11] Note that the appropriate level of clustering is not obvious since the level at which the policies vary is not uniform. Despite this, the main results are not sensitive to clustering on county instead of postal code.

One limitation of the data in the context of evaluating the effects of local smoking by-laws 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 public health literature cited above – and there is little we can do to correct for this slippage. For our preferred county level analyses, however, this issue should be unlikely to bias our results. We provide a map of Ontario and its county borders in Figure 1.[12]

Another data issue worth noting is that that the questions about workplace bans and SHS exposure were only asked of a subset of respondents in each year. Because of this, we can only estimate the effect of local bans on reported ban presence and SHS 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.[13]

IV. Results

[12] 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. As in the US, there is substantial heterogeneity in the size of Ontario counties. To provide a sense of this, note that 90% of Ontario's land area but only about 7% 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 US neighbor to the west and south) is about 1,700 km^2. Although county identifiers are not available in the public use versions of the Canadian Census, US Census 2000 data show that 70% of Michigan residents live and work in the same county (US Census Bureau 2006). Nationally, this figure is about 73 percent.

We present descriptive statistics for the main sample and separately by the presence of a local workplace smoking by-law in Table 1.[14] The patterns suggest that localities adopting by-laws are somewhat different from non-adopters: individuals living in postal codes covered by a workplace by-law are more highly educated, less likely to be married, and less likely to be labourers. We also provide means for the relevant outcomes pertaining to workplace bans, SHS exposure, and own smoking. These patterns provide suggestive evidence that local by-laws may have improved outcomes: workers in areas with a by-law are more likely to report a work area ban on smoking and less likely to report the absence of any workplace smoking restrictions. Workers in areas with local by-laws are more likely to report zero days of SHS exposure and less likely to report daily SHS exposure. Finally, workers in areas with local by-laws have lower own-smoking rates than workers in areas without such by-laws. 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 by-laws 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 – then our research design would have little power to answer questions regarding the effects of these laws on other outcomes such as SHS exposure (distinct from reported ban presence) and own smoking behavior. We address this first question in Table 2, which shows the LPM

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

estimates of the effects of local smoking by-laws on reported ban presence over the period 1997-2004.

The format of Table 2 is as follows: we present OLS results for the likelihood of reporting a complete ban on smoking in one's work area in the top panel, results for an index of smoking restrictions called Ban Range in the middle panel, and results for the likelihood of reporting that smoking is allowed anywhere at work (i.e. no restriction) in the bottom panel. In Column 1 we report results from the model that includes region dummies (6), Column 2 presents results from models that include count/regional municipality dummies (60), and Column 3 presents results from models that include postal code dummies (440). Columns 4-6 perform the same exercise for males, while Columns 7-9 present results for females.

The estimates in Table 2 provide suggestive evidence that the local restrictions represent a true "program": local smoking by-laws are associated with a marginally significant increase in the likelihood an individual reports her workplace has a work-area smoking ban when we include either county or postal code dummies. Relative to a pre-reform mean of .70, our results suggest that local by-laws increase ban presence by about seven percent.[15] The middle panel presents results for the Ban Range outcome and again provides suggestive evidence of an increase in workplace bans associated with local by-laws: the estimate is marginally significant in the model with postal code fixed effects. Finally, in the bottom panel we find that local by-laws affect the extensive margin of ban presence. Specifically, we find that a local by-law reduces the likelihood that an individual reports that "smoking is allowed anywhere" at work by 30-50 percent, and this result is statistically significant at the five percent level in the model with postal

[14] The sample includes only those respondents with valid responses to the workplace smoking ban question (i.e. the sample excludes nonworkers), though sample characteristics for the full sample were very similar.

code fixed effects. We also confirm the result in previous research (Chaloupka 1992) that there are sex-differences in the responsiveness to clean indoor air laws. Stratifying the sample by sex shows that these increases in ban presence are driven mostly by males; estimates for females in the rightmost columns are generally small and always insignificant.

In Table 3 we further investigate the nature of relationship between local by-laws and workplace smoking policies. Specifically, we stratify the sample into two broad types of occupations: blue collar workers and non-blue collar workers (including professionals, administrative/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 SHS exposure (Gerlach et al. 1997 and others). While food service workers are also at high risk – indeed many areas in the US and Canada have smoking bans specifically directed at workers in bars and restaurants – there are simply too few of these types of workers to provide a meaningful analysis using the CAMH monitor data.[16] We present results from an estimation of equation (1) with either region, county, or postal code fixed effects for each of the three occupation groups.

What is first apparent from Table 3 is that mean outcomes in the pre-reform period were consistently worse for blue collar workers in comparison to their white collar and service worker counterparts: labourers were less likely to report a work area ban and more likely to report the absence of any worksite smoking restriction. The estimated effects of the local workplace by-laws in the first three columns of Table 3 return consistently large and generally statistically

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[15] 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, males are less likely to work at sites that ban smoking, as are more highly educated workers.

[16] There are only 139 workers who could arguably be classified as "food service" workers (e.g. bartenders, waiters/waitresses, cooks, kitchen helpers, etc.) in the CAMH monitor over this time period. Notably, a model predicting the presence of a workplace smoking ban returned a large positive coefficient on the local by-law

significant effects of the local by-laws on workplace ban presence. This is particularly true for the extensive margin: adoption of a local by-law in Ontario reduced the likelihood of reporting that smoking is allowed anywhere at work by 50-60 percent in the model with county or postal code fixed effects. This result is supported visually in Figure 2, which shows that workplace policies were actually worsening among these workers until the period of widespread by-law adoption starting around 2000. We also find a statistically significant increase in the likelihood of reporting a work area smoking ban on the order of 20-30 percent, and the results on the ban range index are similarly positive and significant.

For other workers – including white collar professionals and service workers – we find much less evidence that local by-laws increased ban presence. Specifically, we find very small and statistically insignificant effects for all outcome variables when we restrict attention to non-blue collar workers in the rightmost columns. The estimates for the likelihood of a work area ban indicate small increases associated with a local workplace by-law across all models, though none is statistically significant. Estimates on the ban range outcome – the measure with the most potential variation – return estimates that are consistently and substantively zero. The same is true for the "no work restrictions" outcome for these non-blue collar workers.

The results in Table 3 therefore demonstrate that local smoking by-laws 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. This is also 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 by-law adoption (2000-2004) was well over 30 percentage points but

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indicator indicating an increase of about 35 percentage points (a 75% increase relative to pre-reform levels), but the estimate was only statistically significant at the 10 percent level. These results are available upon request.

fell to just around ten percentage points by the end of the sample period during a time of widespread local by-law adoption.

In Table 4 we further investigate the robustness of the result that local by-laws in Ontario increased reported ban presence by labourers. The top panel reports results for the work area ban outcome, the middle panel reports results on the ban range outcome, and the bottom panel reports estimates for the "no work restrictions" indicator. We present results for our preferred specification with county fixed effects – which in our opinion best balances sample size considerations in the CAMH monitor with concerns about accounting for unobserved area-specific heterogeneity – though results with postal code dummies were very similar (and generally indicated even larger increases in ban presence).

In column 1 of Table 4 we present models that exclude residents from the areas surrounding Toronto, including: 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 may alleviate concerns about the work/home slippage described above. Doing so returns a large and significant effect of the local by-laws on all outcomes, providing some suggestive evidence that work/home slippage is unlikely to bias our estimates of the effect of workplace by-laws. In Column 2 we report results from a model where we only controlled for county level by-laws, ignoring the many small towns and even major cities (such as Windsor) at the sub-county level that adopted workplace by-laws. Although we have no reason to believe that the sub-county by-laws were not binding – indeed, this is a key reason we are interested in sub-provincial policies in the first place – doing so may permit a cleaner interpretation of the results with county dummies and alleviates the work/home postal code slippage. Overall, the estimates are little changed and remain statistically significant across the three outcomes.

In Column 3 we restrict attention to individuals in postal codes that adopted any local by-law 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 in Column 3 confirm this to be the case. Column 4 reports results from a model that restricts attention to the period of widespread by-law adoption (2000-2004) and returns larger estimates than the baseline that remain highly significant. Finally, in Column 5 we allow 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 may trend smoothly over time, such as anti-smoking sentiment (Friedberg 1998). Notably, all of our main results are similar in magnitude to the baseline estimates and retain statistical significance at the ten percent level or better.[17]

The results of Tables 3 and 4, then, confirm local workplace smoking by-laws are most (indeed, only) effective at improving worksite smoking policies among workers in labourer occupations. Workers in other occupations, in contrast, were much more likely to have worked at job sites that restricted smoking (e.g. offices) well before the period of local by-law 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 by-laws – and by implication workplace smoking bans – on outcomes such as SHS exposure and own smoking. Specifically, the estimates in Tables 3 and 4 suggest that any

[17] In results not reported, we also attempted to separately control for "gold" versus "silver" workplace by-laws. Although the estimate on the "gold" by-law is large, positive, and statistically significant, we could not reject that the estimates were the same. We also estimated models where we separately controlled for the presence of a local workplace by-law and the presence of a local bar or restaurant by-law. Although all three types of laws are highly correlated (particularly the bar and restaurant laws), we found that controlling both for the workplace by-law indicator and a bar/restaurant by-law indicator returned a large positive coefficient for the workplace by-law indicator and a smaller negative coefficient for the bar/restaurant by-law indicator, as expected. We could not, however, reject that the estimates were the same. Given our limited sample size, we present models that control only for the workplace by-laws unless otherwise noted.

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 5, which offers estimates on the four main outcome variables of interest separately by broad occupational grouping. The format of Table 5 follows the previous tables, in that each row presents a different outcome and each column is a different demographic sample. Specifically, we present results pertaining to SHS 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 non-workers.

The results in Column 1 of Table 5 confirm that local smoking by-laws – and by implication workplace smoking bans – are effective at improving SHS outcomes for blue collar workers. Specifically, we find in the top row that a local workplace by-law increased the likelihood of reporting zero days of SHS exposure at work by 12 percentage points for blue collar workers, and this result is statistically significant at the five percent level. Similarly, in the second row of Column 1 we find that a local workplace by-law reduced the likelihood of reporting 5 or more days of SHS exposure per week by 11 percentage points, again statistically significant at the five percent level. 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 SHS exposure outcomes for the other (non-blue collar) workers, neither of whom exhibited a substantive relationship between a local by-law and reported worksite smoking policy.

The bottom row of Table 5 present results for the likelihood of being a current smoker.[18] The estimates in Column 1 for blue collar workers provide support for the idea that workplace smoking by-laws (and by implication, workplace bans) affect own-smoking behavior. Specifically, we find that the probability that blue-collar workers report being a current smoker fell by an estimated 4 percentage points when a by-law was adopted, with very small and statistically insignificant reductions for other workers. The magnitude of the estimated reduction in own smoking is large but not implausible. Regression adjusted single equation estimates, for example, suggested that a workplace smoking ban is associated with an 11.3 percentage point reduced likelihood of being a current smoker among blue collar workers. Similarly, working at a site with no restrictions was associated with a 17.2 percentage point increased likelihood of being a current smoker. Evans et al. (2000) used a variety of techniques to show that the cross-sectional relationship between workplace bans and smoking participation was not subject to an omitted variables bias. Their 2SLS estimates (using instruments such as firm size) imply that workplace smoking bans reduce smoking participation by a quarter to a third (see their Table 6). Although our relatively small sample does not permit us to identify a statistically significant 2SLS estimate of the effect of smoking bans on own smoking, the implied IV estimate is similar to the single equation result and comparable to that found in Evans et al. (2000). This is further complementary evidence that workplace smoking bans do, indeed, reduce smoking.[19]

Finally, in Table 6 we present novel evidence on the relationship between adoption of a local by-law and self-reported attitudes regarding smoking in various environments. The

[18] We do not control for other tobacco policies such as cigarette excise taxes, which generally vary at the provincial (as opposed to region or county) level. We assume such policies affect all Ontario residents equally.

[19] In results not reported, we find that the estimated reduction in current smoking rates is largely driven by reductions in occasional (but not daily) smoking; since daily smokers are more likely to be addicted (and therefore less likely to be sensitive to policy interventions), this further suggests that the estimated reductions in own smoking rates for blue collar workers are "real".

relationship between local ordinances and attitudes has been regularly acknowledged as a possible important pathway through which laws improve outcomes, though previous research has had little to say on this question, mainly due to data limitations (i.e. most surveys do not ask questions about attitudes).[20] The CAMH monitor, however, includes a handful of relevant questions regarding the respondent's feelings about smoking in workplaces, bars, and restaurants. Response options range from the possibility that smoking should be allowed in all sections (of the bar, restaurant or workplace) to the possibility that smoking should not be allowed in any sections. We create an index where higher values represent more disapproval of smoking in the relevant arena, and we estimate reduced form models with county fixed effects where the explanatory variable of interest is an indicator for whether the respondent lives in an area covered by a relevant by-law (either bar, restaurant, or workplace).

We do so in Table 6, which follows the general format of Table 5. Each entry is from a separate regression with county fixed effects, and we only report the relevant coefficient on the appropriate by-law indicator.[21] The top row reports results for the model predicting feelings about workplace smoking, while the second row reports results for bar attitudes, and the third row reports results for restaurant attitudes. In the bottom row we provide estimates from a linear probability model where we predict the likelihood the respondent reports that – in her opinion – excise taxes on cigarettes should be increased. In this model we control for the presence of a workplace smoking by-law and an indicator for the presence of any bar or restaurant by-law, and we report the estimate on the workplace by-law indicator. Our goal here is to uncover whether

[20] An important exception is recent work by Tang et al. (2003) who used self-reported attitudes from telephone surveys done at 3 points in time after implementation of the state's smoke-free bar law to show that California bar patrons increasingly supported the law.

[21] An alternative approach would be to try to trace out the time path of attitudes relative to the policy changes within communities. Unfortunately, since many of our policies were passed in the latter part of the sample period (see Figures 2-4), we lack sufficient sample to pursue this approach.

the local policies change overall sentiments toward tobacco as opposed to venue-specific attitudes.

We find modest support for the idea that local by-laws change social norms regarding the appropriateness of smoking in workplaces, bars, and restaurants, again driven mostly by blue-collar workers. Specifically, we find that the estimates regarding attitudes about smoking in public spaces in Column 1 for blue collar workers are all sizable and negative, indicating that after these by-laws were passed blue collar workers were more likely to agree that smoking should be restricted in those areas. Although the restaurant smoking model is the only one that is statistically significant, all models return estimates that indicate more disapproval for smoking in that venue relative to the estimates for non-blue collar workers, some of which are wrong-signed (i.e. implying that by-laws *decrease* support for restrictions).[22] Finally, it is also suggestive that the model predicting opinions about increasing cigarette excise taxes fails to yield any substantive relationship between a workplace by-law and anti-tobacco sentiment in this distinct arena for blue collar workers. This is consistent with – but of course does not prove – the idea that local by-laws improve environmental health outcomes for blue-collar workers in part by changing venue-specific social norms related to smoking.

V. Conclusion

Understanding whether and how much state and local smoking ordinances affect SHS exposure and own smoking is crucial for a comprehensive evaluation of the costs and benefits of such

[22] It should be noted that the point estimates for the non-worker sample also consistently indicate more social disapproval of smoking in the various venues, which could itself reflect true effects of the local by-laws (that is, it is possible for bans to change everyone's attitudes, not just the targeted workers). More troubling is the possibility that attitudes are not changed by the bans but instead that the bans reflect changes in attitudes. To adequately address this issue would require estimating models of attitudes with leads and lags of policies to trace out the relevant time path; unfortunately, given the short sample period and the nature of the timing of adoption of local by-laws (most between 2000-2004), this approach is not feasible.

ordinances, particularly since public health benefits are routinely touted as the main motivation for these laws. Unfortunately, most previous research has relied on cross-sectional variation, which may reflect unobserved characteristics correlated both with the decision to adopt a ban and decisions regarding own smoking and type of workplace. We have revisited this question using detailed outcome data from Ontario, Canada and substantial variation in the timing of adoption of over 100 local by-laws over the period 1997-2004. Our data are particularly attractive because – in addition to SHS 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 all workers are "treated" when bans are adopted.

We confirm that local by-laws increase reported ban presence along several different dimensions. This positive relationship survives inclusion of detailed area fixed effects to account for time-invariant unobserved heterogeneity. Importantly, however, we demonstrate that the aggregate benefits of local by-law adoption with respect to reported ban presence are driven entirely by blue collar workers. White collar and sales/service workers – the vast majority of whom were already working at job sites with strict workplace smoking policies well before the period of local by-law adoption – experienced no marginal increase in ban presence when local by-laws were adopted. While plausibly 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 labourers were associated with significant improvements in health outcomes. Specifically, we estimate that a local workplace by-law reduced the likelihood of reporting daily SHS exposure by about 30 percent. Measured differently, we find that a local by-law increased the likelihood of reporting no SHS exposure at work by 25 percent among these workers. Estimates for own-smoking behavior confirm that workplace smoking bans reduce own smoking. Finally, we provide suggestive evidence that these improvements are brought about partly by a change in social attitudes. Overall, our results provide important new insight into the underlying mechanisms through which smoking bans improve outcomes. Future research evaluating the effects of state and province-wide bans must take care to account for these pre-existing improvements associated with local by-laws in order to produce accurate estimates.

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Table 1
Descriptive Statistics, CAMH Monitor 1997-2004

Variable	Full Sample	Local By-Law=0	Local By-Law=1
Age	39.2 (.176)	39.2 (.203)	39.1 (.362)
Male	.53 (.007)	.53 (.008)	.54 (.016)
Less than HS	.10 (.004)	.11 (.005)	.05 (.007)
HS degree	.25 (l.006)	.26 (.007)	.22 (.013)
Some college	.34 (.007)	.35 (.008)	.31 (.015)
University degree	.31 (.007)	.28(.008)	.40 (.016)
Married	.66 (.007)	.69 (.008)	.59 (.016)
Never married	.24 (.007)	.22 (.008)	.30 (.015)
Blue collar worker	.30 (007)	.31 (.008)	.27 (.014)
Other worker	.70 (.007)	.69 (.008)	.73 (.014)
Work area ban	.74 (.007)	.70 (.008)	.84 (.012)
Ban range	2.82 (.011)	2.77 (.013)	2.95 (.021)
No work restrictions	.07 (.004)	.08 (.005)	.04 (.006)
No SHS Exposure	.66 (.009)	.64 (.011)	.69 (.015)
Daily SHS Exposure	.20 (.008)	.21 (.009)	.17 (.012)
Smoker	.29 (.007)	.30 (.008)	.25 (.014)
N	5917	4620	1297

Weighted means, workers only.

Table 2:
Local By-laws Toughened Workplace Smoking Restrictions, Mostly among Males
Models Include Region, County, or Postal Code Fixed Effects
CAMH Monitor 1997-2004

	All			Males	Females
	Region Dummies	County Dummies	Postal Code Dummies	County Dummies	County Dummies
Work Area Ban					
Local workplace by-law	.034	.038*	.048*	.084**	-.022
	(.021)	(.022)	(.027)	(.033)	(.028)
R-Squared	.086	.107	.197	.122	.075
N	5826	5826	5826	2888	2938
Ban Range					
Local workplace by-law	.042	.062	.088*	.122**	-.022
	(.036)	(.039)	(.047)	(.056)	(.053)
R-Squared	.091	.105	.201	.107	.063
N	5826	5826	5826	2888	2938
No Work Restriction					
Local workplace by-law	-.012	-.023	-.035**	-.044*	.007
	(.013)	(.014)	(.016)	(.023)	(.013)
R-Squared	.044	.057	.160	.061	.032
N	5826	5826	5826	2888	2938

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. 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. No work restriction is an indicator that equals one if smoking is allowed anywhere at work and zero otherwise. Estimates are from OLS regressions that control for area fixed effects (as indicated by the column headings), survey year dummies, and demographic characteristics (age, sex, education – 4 categories, and marital status – 3 categories). Standard errors below in parentheses, clustered by postal code. Sample excludes individuals reporting they don't know or refused a response to the question about workplace smoking restrictions. *** Significant at 1%, ** Significant at 5%, * Significant at 10%.

Table 3:
The Effects are Entirely Driven by Blue-Collar Workers
CAMH Monitor 1997-2004

	Blue Collar Workers			Other Workers		
	Region	County	Postal Code	Region	County	Postal Code
Work Area Ban						
Pre-reform mean		.537			.774	
Local workplace by-law	.069	.102**	.152**	.019	.013	.015
	(.044)	(.047)	(.070)	(.022)	(.023)	(.030)
R-Squared	.084	.125	.343	.058	.078	.209
N	1724	1724	1724	4102	4102	4102
Ban Range						
Pre-reform mean		2.45			2.92	
Local workplace by-law	.140*	.217***	.319***	.001	-.004	-.00003
	(.076)	(.080)	(.112)	(.037)	(.042)	(.053)
R-Squared	.076	.114	.358	.055	.069	.199
N	1724	1724	1724	4102	4102	4102
No work restrictions						
Pre-reform mean		.172			.032	
Local workplace by-law	-.056*	-.087**	-.108**	.007	.003	-.005
	(.030)	(.034)	(.048)	(.008)	(.010)	(.012)
R-Squared	.054	.083	.334	.014	.028	.163
N	1724	1724	1724	4102	4102	4102

See notes to Table 2.

Table 4:
Robustness of By-Law Effect on Ban Presence, Blue-Collar Workers
All Models include County Fixed Effects
CAMH Monitor 1997-2004

	(1) Exclude Peel, York, and Durham residents (around Toronto)	(2) Control only for county-wide by-laws	(3) Only residents in localities adopting a by-law (changers)	(4) Only 2000 - 2004	(5) Baseline + county specific time trends
Work area ban					
Local workplace by-law	.112**	.093*	.111**	.141**	.098*
	(.051)	(.056)	(.050)	(.055)	(.053)
R-squared	.122	.124	.136	.169	.147
N	1522	1724	1418	913	1724
Ban Range					
Local workplace by-law	.224***	.242***	.250***	.291***	.209**
	(.087)	(.099)	(.083)	(.104)	(.092)
R-squared	.108	.113	.126	.158	.131
N	1522	1724	1418	913	1724
No Work Restrictions					
Local workplace by-law	-.078**	-.104**	-.095***	-.126***	-.081**
	(.037)	(.042)	(.037)	(.044)	(.041)
R-squared	.079	.083	.089	.115	.101
N	1522	1724	1418	913	1724

See notes to Table 2.

Table 5:
By-laws Reduced SHS Exposure for Blue Collar Workers
All Models Include County Fixed Effects
CAMH Monitor 1997-2004

	(1) Blue Collar Workers	(2) Other Workers	(3) Retired persons & Homemakers (nonworkers)
No SHS Exposure			
Pre-reform mean	.474	.719	
Local workplace by-law	.121**	-.001	--
	(.057)	(.030)	
R-squared	.103	.084	
N	1276	3160	
5 or more days SHS Exposure/Week			
Pre-reform mean	.348	.148	
Local workplace by-law	-.110**	.041*	--
	(.051)	(.024)	
R-squared	.102	.063	
N	1276	3160	
Current Smoker			
Pre-reform mean	.371	.259	.155
Local workplace by-law	-.041	-.003	-.002
	(.032)	(.017)	(.019)
R-squared	.091	.067	.059
N	3261	7948	4300

See notes to Table 2.

Table 6:
There is Modest Evidence that Local By-Laws Change Attitudes
Negative coefficients mean more disapproval
All Models Include County Fixed Effects
CAMH Monitor 1997-2004

	(1) Blue Collar Workers	(2) Other Workers	(3) Retired persons & Homemakers (nonworkers)
Feelings about workplace smoking			
Pre-reform mean	1.99	1.54	1.65
Local workplace by-law	-.052	.049	.044
	(.097)	(.040)	(.067)
R-squared	.115	.067	.091
N	1343	3320	1657
Feelings about bar smoking			
Pre-reform mean	2.77	2.46	2.20
Local bar by-law	-.145	-.075	.071
	(.107)	(.066)	(.091)
R-squared	.160	.121	.120
N	1494	3784	1841
Feelings about restaurant smoking			
Pre-reform mean	2.04	1.86	1.84
Local restaurant by-law	-.191**	-.017	-.001
	(.082)	(.052)	(.074)
R-squared	.083	.074	.114
N	1333	3297	1634
Thinks cigarette excise taxes should be increased (0/1)			
Pre-reform mean	.39	.46	.48
Local workplace by-law	-.004	-.014	-.164*
	(.110)	(.053)	(.089)
R-squared	.069	.049	.059
N	1546	3673	1732

See notes to Table 2. For all of the attitudes questions, the ranges are: 1=Smoking should not be allowed in any sections, 2=Smoking should be allowed in an enclosed section with ventilation, 3=Smoking should be allowed only in a smoking section, and 4=Smoking should be allowed in all sections. For the model in column 4, we control for both the workplace by-law and an indicator for any bar or restaurant by-law, but we present only the coefficient on the workplace by-law indicator.

Appendix Table 1
Expanded Set of Coefficient Estimates
Model is LPM of Work Area Ban with County Fixed Effects, All Adults
CAMH Monitor 1997-2004

Variable	Estimate
Local By-law	.038*
	(.022)
1998	.018
	(.025)
1999	.040*
	(.023)
2000	.061**
	(.024)
2001	.054**
	(.024)
2002	.074***
	(.025)
2003	.125***
	(.023)
2004	.181***
	(.026)
Age	.001
	(.001)
Male	-.110***
	(.012)
Previously married	.003
	(.020)
Never married	-.009
	(.019)
HS degree	.044*
	(.025)
Some college	.105***
	(.024)
University degree or more	.200***
	(.024)
R-Squared	.107
N	5826

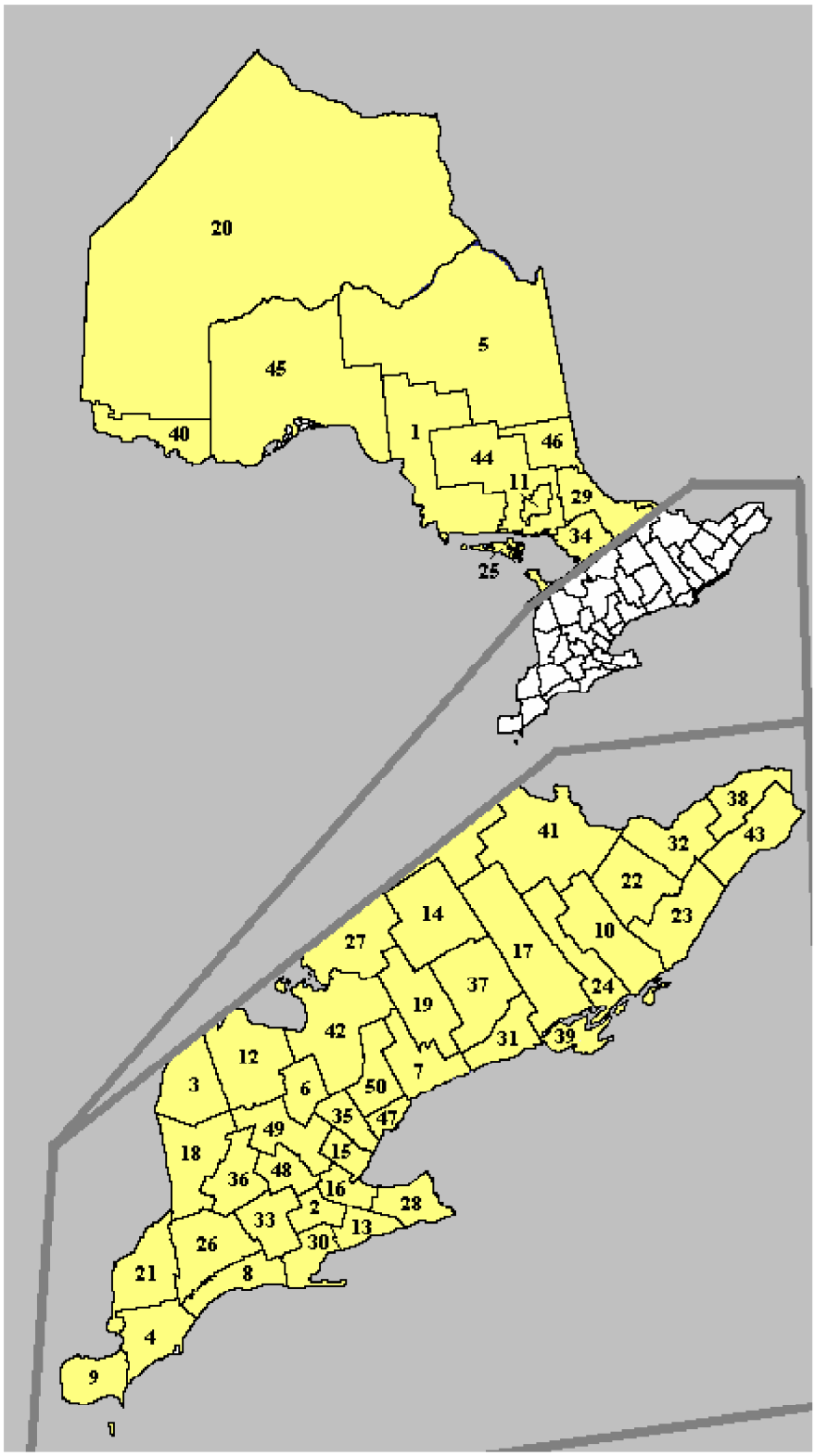
Coefficients on county dummies not shown. See notes to Table 2.

Appendix Table 2:
Robustness of Main By-Law Results to Probit Estimation
Models with County Fixed Effects
CAMH Monitor 1997-2004

	Full Sample	Blue Collar Workers
Work Area Ban		
Local By-law	.056**	.126**
	(.023)	(.051)
Pseudo R-squared	.099	.100
N	5826	1724
No work restrictions		
Local By-law	-.021*	-.087***
	(.010)	(.025)
Pseudo R-squared	.119	.102
N	5762	1678

Models are estimated using probit. See notes to Table 2 for control variables. Marginal effects (calculated at sample means) are presented.

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



*To provide a sense of size, note that Toronto (#47) covers apx. 700 square kilometers.

Figure 2.
Trends in Local By-laws, Work Smoking Policies, SHS Exposure, and Own-Smoking, Blue Collar

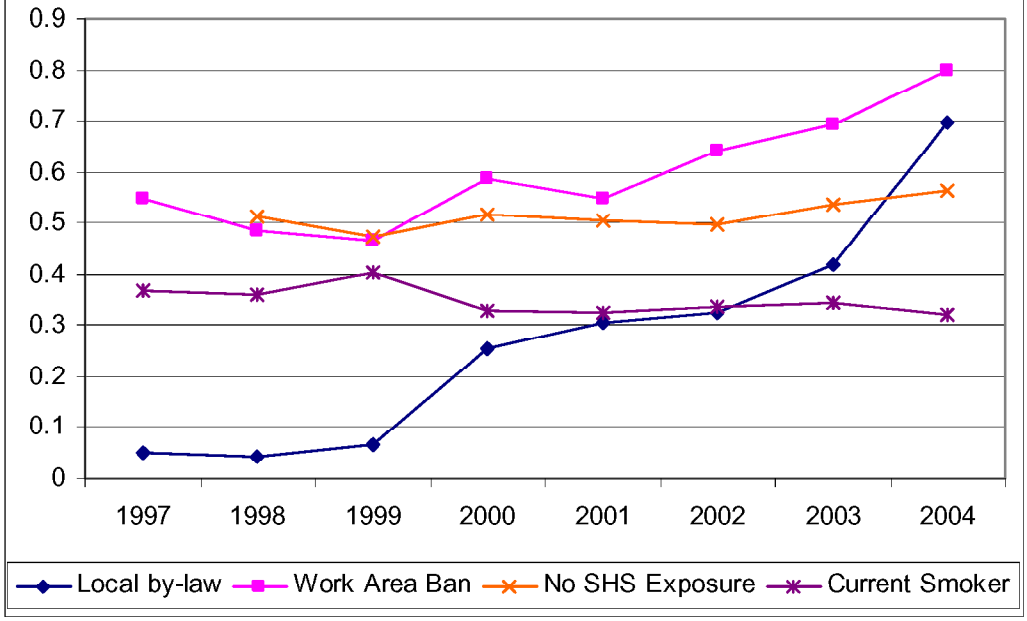


Figure 3.
Trends in Local By-laws, Work Smoking Policies, SHS Exposure, and Own-Smoking, Other Workers

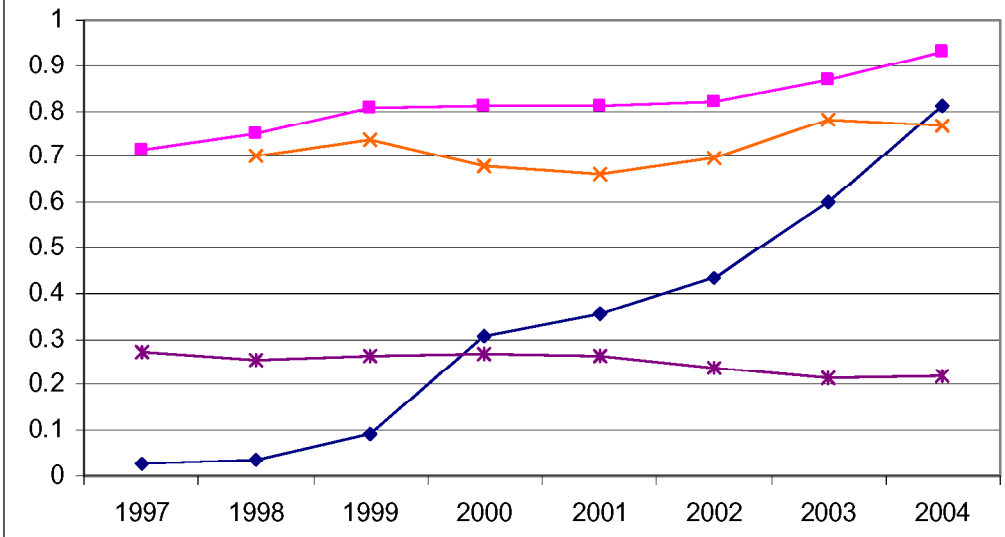


Figure 4.
Trends in Work Area Ban, by Occupation

