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TOBACCO CONTROL CAMPAIGN IN URUGUAY: IMPACT ON SMOKING CESSATION DURING PREGNANCY AND BIRTH WEIGHT

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ABSTRACT

Background. In 2005, Uruguay instituted a nationwide tobacco control campaign that has resulted in a substantial decline in nationwide smoking rates. We sought to determine the quantitative contributions of each of the major tobacco control measures adopted by the Uruguayan government. We focused sharply on smoking cessation by pregnant women and on the effect of quitting smoking during pregnancy on birth weight.

Data. We analyzed a nationwide registry of all pregnancies in Uruguay during 2007–2012, supplemented by data on cigarette prices and various governmental policies.

Methods. We estimated linear probability models of quitting smoking in the third trimester as well as linear models of the effect of quitting on birth weight. Our explanatory variables included maternal characteristics, provider-level and national-level policy interventions, and real price. In our models of quitting smoking, we used taxes as an instrument to address price endogeneity. In our models of birth weight, we used tobacco control policies as instruments to address the endogeneity of smoking cessation.

Results. During 2007–2012, the proportion of pregnant women who had quit smoking by their third trimester increased markedly from 15 to 42 percent. Each of the major non-price tobacco control measures – including programs to treat nicotine dependence at health centers, banning of advertising nationwide, rotating warnings with pictograms on each pack, restriction of brands to a single presentation, and an increase in the size of pictograms to 80% of the front and back of each pack – was separately associated with a significant increase in the rate of quitting. During 2007–2009, tobacco manufacturers responded to tax increases and non-price policies by moderating their pretax prices. Quitting smoking by the third trimester increased birth weight by an estimated 163 grams.

Conclusion. Uruguay's nationwide tobacco control campaign led to a substantial increase in the likelihood that a pregnant smoker would quit by her third trimester. Each of the major tobacco control measures adopted by the government had a measurable impact on the rate of quitting and thus on neonatal health.

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Abstract

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Key Words: economic evaluation, cigarette taxes, package warnings, advertising bans, tobacco control, pregnancy, birth weight

JEL: I18 (government policy, regulation, public health), I12 (health production), D12 (consumer economics, empirical analysis)

Introduction

The tobacco epidemic continues to represent a serious public health threat throughout the world. By one recent estimate, the worldwide annual mortality burden has already reached 5 million deaths from direct tobacco smoking and another 600,000 deaths attributable to the effects of environmental smoke (World Health Organization 2012). Within the next 20 years, annual deaths from tobacco are projected to continue to rise to 8 million, of which more than 80% will occur in low- and middle-income countries (Mathers, Boerma et al. 2008).

Beginning in 2005, Uruguay instituted a series of aggressive anti-smoking measures that placed this small South American country of 3.3 million inhabitants in the forefront of tobacco control policy worldwide. By 2012, the Uruguayan government had banned nearly all advertising and promotion of tobacco products, prohibited smoking in enclosed public spaces and workspaces, mandated that pictograms with warnings cover 80% of the front and back of every pack, outlawed multiple versions of the same brand such as Silver or Blue, banned misleading marketing terms such as “light” and “mild,” required healthcare providers to treat nicotine dependence, and raised tobacco taxes.

In a previous report, two of us (JH and PT) found that Uruguay’s comprehensive nationwide antismoking campaign was associated with a substantial, unprecedented decrease in tobacco use (Abascal, Esteves et al. 2012). During 2005–2011, per capita cigarette consumption decreased by 4.3% per year, while the 30-day prevalence of cigarette use among students aged 13–17 years and the overall population prevalence of current tobacco use declined at annual rates of 8.0% and 3.3%, respectively. The observed declines in each of these three indicators of tobacco use were significantly larger than those seen in Argentina, which had not conducted a comprehensive antismoking campaign.

These prior results provided estimates of the impact of Uruguay’s tobacco control program taken as a whole. They did not permit us to identify the contributions of each of the component anti-smoking measures. Identification of the impacts of individual components is important in assessing the generalizability of Uruguay’s experience to other jurisdictions that have instituted some but not all such tobacco control policies. Identification is further important because two of the country’s anti-smoking policies – the requirement that pictograms with warnings cover 80% of the front and back of each pack, and the “single presentation rule” that bars multiple versions of the same brand – have recently come under legal challenge (Philip

Morris International 2010). Moreover, our prior work focused solely on measures of tobacco use. Assessment of health outcomes is a critical component of the evaluation of tobacco control programs (Lien and Evans 2005, Kabir, Connolly et al. 2008, Adams, Markowitz et al. 2012).

In this article, we study the impact of Uruguay's tobacco control campaign on a critical target population: pregnant women. Cigarette smoking is one of the most important modifiable risk factors associated with adverse perinatal outcomes, including low birth weight and preterm birth (Sexton and Hebel 1984, Permutt and Hebel 1989, da Veiga and Wilder 2008, McCowan, Dekker et al. 2009). These adverse outcomes impose substantial private and social costs as a result of increased morbidity and mortality, low income and poor educational trajectories over a lifetime (Petrou, Sach et al. 2001, Adams, Miller et al. 2002, Boardman, Powers et al. 2002, Black, Devereux et al. 2007).

We used data from a continuous nationwide registry of pregnancies to assess the impact of Uruguay's tobacco control campaign on a pregnant smoker's decision to quit smoking, as well as the resulting impact on birth weight. In contrast to prior studies that have focused narrowly on the effects of cigarette prices (Evans and Ringel 1999, Ringel and Evans 2001, Colman, Grossman et al. 2003, Lien and Evans 2005, Levy and Meara 2006) or workplace restrictions on smoking (Adams, Markowitz et al. 2012, Bharadwaj, Johnsen et al. 2012), we sought to decompose the effects of a series of tobacco control measures on smoking cessation rates. Our identification strategy depended critically on two aspects of Uruguay's multifaceted anti-smoking campaign. First, different tobacco control measures came into effect – and, in some cases, went out of effect – at different times from 2005 onward. Second, certain policies – particularly those directed at improving healthcare providers' treatment of nicotine dependence during pregnancy – were implemented at different health centers providing prenatal care at different times.

We found that the proportion of pregnant women who quit smoking by their third trimester increased markedly from 15 percent in 2007 to 42 percent in 2012. Taking into account women's personal characteristics and incorporating fixed effects for the health centers where they received prenatal care, we found that the major policy interventions adopted by the Uruguayan government had significant, positive impacts on pregnant smokers' probability of quitting. The estimated price elasticity of quitting was on the order of 0.4 at the sample mean. The absolute individual impact of each non-price policy intervention on the probability of quitting ranged from 3 to 14 percentage points. Cigarette price played a relatively minor role in the overall increase in smoking cessation, as manufacturers moderated their pre-tax prices in response to tax increases and other non-price policies, particularly during 2007–2009. Each pregnant smoker who quit smoking in her third trimester in response to Uruguay's tobacco control policies experienced a mean increase in birth weight of 163 grams.

Health Policy in Uruguay

Nationwide Anti-Smoking Policies

In July 2004, Uruguay's legislature ratified the Framework Convention on Tobacco Control (FCTC), a treaty originally adopted in May 2003 at the 56th World Health Assembly of the World Health Organization (WHO) and now accepted by 177 countries (World Health Organization 2013). In 2005, the newly elected administration created a National Program for Tobacco Control within its Ministry of Public Health to serve as a focal point for implementing a series of measures codified under the FCTC. There followed a succession of progressively more stringent tobacco control policies (Abascal, Esteves et al. 2012). In May 2005, the Ministry established clearly delimited, ventilated smoking areas in private bars, restaurants and other entertainment venues. In July of that year, the Ministry further decreed that all public offices, including government, public service companies and public schools, be 100% free of tobacco smoke. By March 2006, all enclosed public spaces and all public and private workspaces were declared 100% smoke-free. In June 2008, another ministerial decree extended the scope of tobacco-free spaces to taxis, buses, airplanes and other public transport.

These curbs on smoking in certain public and private spaces were paralleled by restrictions on tobacco advertising. A May 2005 ministerial decree barred cigarette advertising on television during children's viewing hours (that is, before 21:30). The decree also prohibited advertising, promotion or sponsorship by tobacco companies of all sporting events. These restrictions were subsequently codified in March 2008, when comprehensive tobacco control legislation (Law 18.256) prohibited all advertising and promotion of tobacco products except at point of sale. In October 2008, logos, trademarks and other tobacco-related symbols were banned on non-tobacco products.

At the same time, the Uruguayan government promulgated warning requirements on cigarette packages and imposed restrictions on manufacturers' branding practices. A May 2005 ministerial decree banned all references to “light,” “ultra light,” “mild,” “low tar” and other descriptors that might misleadingly imply reduced harm. The decree also required that a series of rotating warnings with images was to cover 50% of the front and back of each cigarette pack. The deadline for compliance with the first round of these rotating warnings was April 2006. Subsequent rounds had respective deadlines of December 2007, February 2009, February 2010, January 2012, and April 2013. A “single presentation rule,” issued as a ministerial decree along with the third round of warnings, barred the marketing of multiple versions of the same brand, such as Silver or Blue. A 2009 decree mandated that the size of the warnings be increased to 80% of the front and back of each pack. This requirement was implemented with the fourth round of warnings and became effective by February 2010.[1]

Figure 1 shows a timeline summarizing the major nationwide non-price regulatory measures from 2005–2013. The blue text describes each of the six rounds of package warnings, while the red text describes regulatory measures other than the mandated warnings. The black lines point to the compliance deadlines for each regulatory measure.[2]

Figure 2 further describes the six rounds of rotating package warnings. In each round, we show only one of several mandated images. The first, third and fourth rounds, in particular, included warnings and accompanying images that directly addressed the consequences of smoking during pregnancy. The relative sizes of the images in the figure correspond to their relative sizes on each pack, with the last three rounds reflecting the required increase from 50% to 80% of the front and back surfaces.

Smoking Cessation Programs Directed at Healthcare Providers

In 2003, National Resource Fund (“Fondo Nacional de Recursos” or FNR), the governmental agency responsible for financing resource-intensive medical technologies, began to establish quit-smoking services at hospitals aimed initially at preventing a recurrent heart attack in patients who had undergone cardiac bypass. In 2005, the FNR expanded its smoking cessation program to cover the entire Uruguayan patient population. A key element of the

[1] This “80% rule” was promulgated 3 months before the issuance of the fourth round of images. However, we have no evidence of significant compliance with the 80% rule before the deadline for compliance with the fourth round of images.

[2] With the exception of the comprehensive tobacco control law, all measures provided for a 180-day compliance period. By specifying the end of the compliance period as the effective date of each measure, we assumed that tobacco manufacturers waited until each deadline to comply.

expanded nationwide program was a system of agreements ("convenios") between the FNR and individual clinics and hospitals, in which the FNR trained healthcare providers in the diagnosis and treatment of tobacco dependence and provided free nicotine patches and bupropion (a medicine widely used to blunt the symptoms of nicotine withdrawal) in return for setting up a smoking cessation program with little or no patient copayments (Esteves, Gambogi et al. 2011).

In 2008, the comprehensive tobacco control law (Law 18.256) mandated that every primary care provider, whether public or private, incorporate the diagnosis and treatment of tobacco dependence into its menu of basic services. Pursuant to this legislation, the FNR and the Ministry of Public Health in 2009 established national guidelines for primary care providers on the diagnosis and treatment of nicotine dependence. Healthcare institutions that had no agreements with the FNR were required to provide smoking cessation services in accordance with the guidelines, but they were permitted to charge nontrivial copayments to patients.

By 2010, the FNR had trained 657 health professionals, while a total of 21,527 patients in 45 healthcare institutions had been treated under the program. Among all patients enrolled in FNR's smoking cessation protocol, the reported one-year abstinence rate was 14 percent. Among those with at least 4 visits to the program, the reported one-year abstinence rate was 22 percent (Esteves, Gambogi et al. 2011).

Institutions caring for pregnant women likewise participated in the FNR's system of agreements. Among all sites providing prenatal care to pregnant women, the proportion with FNR agreements increased from 6 to 10 percent during 2005–2012. Concurrently, the proportion of all pregnant women receiving prenatal care at sites with FNR agreements increased from 24% in 2005 to 35% in 2007, but then declined to 32% by 2012.

Cigarette Tax Increases

In addition to the foregoing policy interventions, the Uruguayan government increased its indirect taxes on tobacco products. Imposed solely at the national level, these taxes consist of an excise tax ("impuesto específico interno" or IMESI) and a value added tax ("impuesto al valor agregado" or IVA). The IMESI, which was first applied to cigarettes in 1993, underwent a series of discrete increases in June 2002, May 2003, July 2007, June 2009, and February 2010. The IVA, by contrast, was first applied to cigarettes in July 2007 and since then has constituted 22% of the pre-tax price including the IMESI or, equivalently, 18% of the retail price. We describe the quantitative impact of these tax increases on the real price of cigarettes in the Data section below.

In Uruguay, an estimated 99% of tobacco users smoke manufactured cigarettes, hand-rolled cigarettes, or both (Abascal, Esteves et al. 2012). Manufactured cigarettes, in particular, make up more than 85% of taxable cigarette consumption (Dirección General Impositiva 2012). During 2004–2012, by one estimate, contraband cigarette sales constituted approximately 12% of total cigarette consumption on average (Curti 2013). With the possible exception of less densely populated provinces ("departamentos") along Uruguay's borders with Brazil and Argentina, where contraband tobacco use appears more prevalent, there has been little effective geographical variation in retail price.

Expanding Access to Prenatal Care

Through a series of reforms beginning in 2008, the Uruguayan government established an integrated national health system ("Sistema Nacional Integrado de Salud" or SNIS) aimed at universal coverage financed by premiums based on a graduated share of income (Ministerio de Salud Pública 2010). These reforms included a series of new incentives to reorient the healthcare system toward primary care, disease prevention and early diagnosis, particularly in the area of reproductive health (Ministerio de Salud Pública 2010). In July 2008, the Uruguayan government began to compensate healthcare institutions for adhering to specific quantitative goals ("metas") for a range of performance indicators. In the area of maternity care, one quantitative goal was based on the proportion of pregnancies with an initial visit in the first trimester and at least six prenatal visits prior to delivery. An additional goal addressed the completeness of individual patient data entered into the national perinatal registry, a database that we describe in detail below (Junta Nactional de Salud (JUNASA) 2009, González, Olesker et al. 2010). From 2007–2012, the percentage of pregnant women initiating prenatal care in the first trimester increased from 59 to 77 percent, while the percent initiating care in the third trimester dropped from 8 to 4 percent.

Literature Review

Smoking Cessation during Pregnancy and Birth Outcomes

Maternal smoking during pregnancy causes a wide range of adverse birth outcomes, including low birth weight, preterm birth, intrauterine growth retardation, spontaneous abortion, and mental retardation, among other conditions. These adverse effects have been documented in epidemiological studies (Andres and Day 2000, Lindley, Becker et al. 2000, Windham, Hopkins et al. 2000, Phung, Bauman et al. 2003, Ward, Lewis et al. 2007, da Veiga and Wilder 2008, McCowan, Dekker et al. 2009) and in clinical trials (Sexton and Hebel 1984, Permutt and Hebel 1989, Ershoff, Quinn et al. 1990, Shipp, Croughan-Minihane et al. 1992, Pollack 2001).

Smoking cessation early in pregnancy has been associated with a reduction in the risk of adverse outcomes (Vardavas, Chatzi et al. 2010). Even delayed cessation of smoking during the second or third trimester can reduce the adverse effects of smoking during pregnancy (Lieberman, Gremy et al. 1994, Raatikainen, Huurinainen et al. 2007, Batech, Tonstad et al. 2013, Yan and Groothuis 2013).

Some economists have suggested that the impact of smoking on birth outcomes may be smaller in magnitude than traditionally estimated. These authors have used either maternal fixed effects in longitudinal databases that included pregnancies among non-twin siblings (Abrevaya 2006, Abrevaya and Dahl 2008, Walker, Tekin et al. 2009, Wüst 2010, Juarez and Merlo 2013) or anti-tobacco policies as instrumental variables (Lien and Evans 2005).

Tobacco Control Policies and Smoking During Pregnancy

Another strand of the literature has evaluated the effects of tobacco control policies on smoking and birth outcomes. With few exceptions, these studies have focused on the impact of changes in cigarette prices. Evans and colleagues found that US pregnant women responded significantly to increases in cigarette excise taxes, and that smoking during pregnancy reduced birth weight by 182 grams and doubled the risk of low birth weight (Evans and Ringel 1999, Ringel and Evans 2001, Lien and Evans 2005). Other authors have also found that the price elasticity of smoking participation is higher among pregnant women than in the general population (Gruber and Koszegi 2001, Colman, Grossman et al. 2003).

More recent studies of the U.S., however, have found smaller reductions in smoking participation in response to tax hikes. Levy and Meara examined the effect of the large, unprecedented price increase following the 1998 Master Settlement Agreement (MSA) between various state attorneys general and the major tobacco manufacturers. Compared with the prevalence of prenatal smoking predicted by secular trends leading up to the MSA, prenatal smoking prevalence was found to have declined proportionally by less than 3 percent (Levy and

Meara 2006). Similarly, Adams and colleagues (Adams, Markowitz et al. 2012) found that increases in excise taxes and restrictions on indoor smoking were effective in improving birth outcomes, but the effects were small in magnitude and limited to infants born to mothers in certain age groups. Bharadwaj and colleagues found that the extension of smoking bans to bars and restaurants in Norway in 2004 decreased smoke exposure in utero for children of female workers in restaurants and bars, and concomitantly decreased rates of very low birth weight (Bharadwaj, Johnsen et al. 2012).

Warnings and Images on Cigarette Packs; Advertising Restrictions

Numerous studies point to the critical role of pack design in the marketing of cigarettes, particularly for creating an in-store presence at the point of purchase, for communicating brand image, and for reinforcing false perceptions about smoking risks (Mutti, Hammond et al. 2011). Experimental studies and surveys suggest that pack-based warnings decrease the perceived attractiveness of the package, produce higher levels of negative affect, and increase smokers' risk perception and intention to quit (Kees, Burton et al. 2006, Hammond 2011). Hammond, Fong and colleagues assessed the impact of graphic Canadian cigarette warning labels on self-reported smoking of 616 adult subjects before and after the enactment of new warnings. They found that smokers who had read, thought about, and discussed the new warnings at baseline were more likely to have quit, made a quit attempt, or reduced their smoking three months later (Hammond, Fong et al. 2003, Hammond, Fong et al. 2004). Marketing studies in experimental and naturalistic settings indicate that plain packaging – with no imagery, colors or symbols – reduced the appeal of tobacco products, increased the prominence and effectiveness of health warnings, and neutralized those package designs that tend to mislead consumers about the harmfulness of tobacco products (Hoek, Wong et al. 2011, Moodie, Hastings et al. 2011, Thrasher, Rousu et al. 2011). In a cross-sectional study conducted during the rollout phase of Australia's plain packaging law, Wakefield and colleagues found that smokers of plain packs perceived their cigarettes to be lower in quality and less satisfying, and gave a higher priority to quitting (Wakefield, Hayes et al. 2013).

Economists have studied the effects of restrictions and bans on cigarette advertising with mixed results. Saffer and Chaloupka, in a study of OECD countries, concluded that a comprehensive set of tobacco advertising bans could reduce tobacco consumption, but that a limited set of advertising bans would have little or no effect (Saffer and Chaloupka 2000). In a study of 30 developing countries, Blecher found even stronger evidence for the effectiveness of advertising bans in reducing tobacco consumption (Blecher 2008). Nelson, on the other hand, has argued that tobacco advertising elasticities are small and that advertising bans have had no effect on cigarette consumption (Nelson 2003, Nelson 2006). In a recent analysis of data from 19 developing countries, Kostova and Blecher maintained that individual advertising exposure is endogenous, and that the positive relation between advertising and consumption can be explained largely by the disproportionately higher propensity of smokers to observe advertising (Kostova and Blecher 2012).

Data

Perinatal Information System (SIP)

Our source of micro data on the smoking practices of pregnant women was the Perinatal Information System ("Sistema Informático Perinatal" or SIP), a mandatory nationwide electronic registry operating in all prenatal care clinics in Uruguay since 1990. Developed and overseen by the Latin American Center for Perinatology ("Centro Latinoamericano de Perinatología" or CLAP) of the Pan American Health Organization, the database contained information at the level of the individual pregnancy on maternal characteristics, self-reported smoking behavior, current and past obstetric history, the timing of prenatal care, the sites of prenatal care and delivery, and birth outcomes including birth weight (CLAP 2001). In 2012, the SIP covered an estimated 94% of all live births in Uruguay.

Prior to 2007, each individual record in the SIP database contained the pregnant woman's smoking status only during the trimester in which she initiated prenatal care. It did not show changes in smoking, if any, during the course of her pregnancy. Under a new data entry system beginning in 2007, the prenatal record noted the woman's smoking status separately in each trimester of her pregnancy. For example, if a woman initiated prenatal care in her second trimester, the healthcare provider recorded her smoking status in the first trimester, based on her recall, as well as in the current trimester. Her smoking status would subsequently be recorded in a follow-up prenatal visit during her third trimester.

Under the new data entry system, we had data for 251,260 pregnancies terminating in a live birth during 2007 through 2012. Elimination of pregnancy records of less than 25 weeks of gestation or unknown gestation, as well as multiple-birth records or records with unknown multiplicity, left 241,270 records (96%).

Smoking Cessation

Our analysis of quitting behavior was confined to the new SIP system, covering the period 2007–2012, when data on smoking habits during each trimester were available. We studied the subpopulation of pregnant smokers, which included any woman recorded as currently smoking during at least one trimester of her pregnancy. Among these smokers, our principal outcome variable was quitting smoking. We defined a woman as having quit smoking if she was specifically recorded as not currently smoking during the third trimester. Pregnant women who smoked in the first or second trimester but whose smoking status was recorded as unknown in the third trimester were excluded from our primary analyses. In secondary analyses of robustness, however, we included such individuals as smokers. As a further robustness check, we studied quitting in the second trimester as well. Eliminating non-smokers, records encoded under the old SIP data entry system by health centers that were still making the transition to the new system during 2007–2008, and records of smokers with unknown smoking status in the third trimester, gave us an analytic sample of 28,597 observations.

Figure 3 shows the trend in the proportion of pregnant smokers who quit smoking by the third trimester, based on our analytic sample. The horizontal axis shows the year corresponding to the midpoints of each woman's third trimester. Each plotted point represents the annual mean, while the capped vertical bars represent 95% confidence intervals. The quit rate increased from 15% in 2007 to 42% in 2012.

Maternal and Pregnancy Characteristics

Table A1 in Appendix A shows the individual-level material characteristics, derived from the SIP registry, that we used in our econometric analyses. These include variables referable to: the timing of the first prenatal visit (first-trimester prenatal care); the mother's age (<16, 17–19, 20–34, 35–39, and 40+ years), marital status (single, married, cohabiting, other), and educational attainment (primary, secondary, university); the number of prior deliveries (0, 1, 2, 3, 4+); number of prior abortions; a history of diabetes or hypertension; whether any complications of pregnancy were observed, in particular, the presence of preeclampsia or eclampsia; the mother's body mass index based on her self-reported height and weight prior to the pregnancy (underweight, normal weight, overweight, obese); the mother's use of alcohol or illicit drugs; the sites of prenatal care (not shown in Appendix Table A1); and the newborn's sex and birth weight. To avoid loss of observations, we included dummy variables equal to 1 when some maternal characteristics were missing.

Nationwide Tobacco Control Measures

We assigned a binary indicator variable to each of the five temporally distinct tobacco control measures that entered into effect during 2007–2012, as delineated in Figure 1. These tobacco control measures, along with the respective dates on which they entered into effect, were: (1) second round of package warnings (8-Dec-2007); (2) comprehensive tobacco control law (6-Mar-2008); (3) third round of package warnings and single presentation rule (14-Feb-2009); (4) fourth round of package warnings and 80% coverage requirement (28-Feb-2010); and (5) fifth round of package warnings (07-Jan-2012). We did not include binary variables for the comprehensive ban on smoking on enclosed public spaces and public and private workspaces or for the first and sixth round of warnings, as they occurred outside our interval of analysis (2007–2012).

Cigarette Prices and Taxes

We constructed a monthly time series of the real retail price of a pack of 20 cigarettes, as well as a corresponding monthly series of real total taxes per pack. Based on unpublished data made available from the Billion Prices Project (Cavallo 2013), we first computed the nominal average price of a pack of 20 Nevada cigarettes, the best-selling brand, for each month from October 2007 through May 2013. As detailed in Appendix B, we found that these average retail prices quite closely tracked the tobacco consumer price index maintained by Uruguay's National Statistical Institute (Instituto Nacional de Estadística 2013). We then used the predicted values from a linear regression of the average retail price of Nevada on the tobacco price index to construct a complete monthly series of nominal retail prices. Nominal prices were then converted to real prices based on the monthly all-goods consumer price index (Instituto Nacional de Estadística 2013). In our econometric analyses, our principal price variable was the logarithm of the calculated real price per pack.

As noted in our description of tobacco control policies, Uruguay's tobacco taxes consist of an excise tax ("impuesto específico interno" or IMESI) and a value added tax ("impuesto al valor agregado" or IVA). The IMESI has undergone a series of discrete increases since it was first applied to cigarettes in 1993. The IVA, by contrast, was first applied to cigarettes in July 2007 and since then has constituted 22% of the pre-tax price, including the IMESI. Total nominal taxes were likewise converted to real values based on the monthly all-goods consumer price index. In our econometric analyses, our principal tax variable was the logarithm of the calculated real tax per pack.

Figure 4 shows our calculations of real cigarette price and taxes on a monthly basis from 2001–2012. Although we use only the data from 2007 onward in our econometric analyses of smoking cessation, we show the entire series for completeness. Only 45% of the abrupt increase in cigarette taxes in July 2007 was passed on to consumers in the form of higher retail prices. By contrast, cigarette manufacturers raised their prices in December 2011 without any corresponding change in tax rates. Below, we consider the implications of these findings for the treatment of real price as an endogenous variable and real tax as an instrumental variable.

Interventions at the Level of the Healthcare Provider

For our econometric analyses, we constructed a binary indicator variable that equaled 1 if the clinic or hospital where a pregnant woman received prenatal care had an agreement ("convenio") with the FNR during the year in which she delivered her baby. We based our construction of this variable on a list of formal agreements provided by the FNR. In the public sector, however, these formal agreements often specified the governing national or municipal healthcare authority, but not the specific health center. While we attempted to map every such agreement into specific health centers, it is likely that we undercounted the number of publically sponsored health centers with such agreements.

Methods

Smoking Cessation: Model Specification

Let the binary dependent variable q_{ijt} denote the smoking status of woman i who received prenatal care at site j, and who was in her third trimester of pregnancy at calendar date t. We let $q_{ijt} = 1$ if the woman was a quitter at that time. If the woman had at least one recorded prenatal visit during her third trimester, we computed t as the average date among all such visits. If all dates of prenatal care in the third trimester were unknown, we defined t as 30 days prior to delivery, which was the sample mean for our population.

Let X_i denote a vector of personal characteristics of woman i that remained constant during her pregnancy, including her age, education, marital status, history of prior abortions, number of prior deliveries, body mass index before becoming pregnant, history of diabetes or hypertension, and whether she suffered from preeclampsia or eclampsia. This vector also included an indicator variable equal to 1 if the woman began her prenatal care in the first trimester. For consistency with our model of birth weight, to be described below, it also included the newborn's sex.

Let A_{jt} denote a binary variable equal to 1 if the site of care j had an agreement for the treatment of tobacco dependence with the FNR at calendar date t. Let $\log P_t$ denote the logarithm of the real price of a pack of cigarettes at date t. We explicitly considered the possibility that cigarette price is endogenous. That is, tobacco manufacturers might respond to tax increases and other anti-tobacco policies by reducing their pre-tax prices. As observed in Figure 4, for example, the increase in real cigarette taxes in mid-2007 was accompanied by a much smaller increase in real cigarette price.

Finally, for each $k = 1,\ldots,5$, let Z_{kt} denote a binary variable equal to 1 if national-level tobacco control measure k was in effect on calendar date t. In particular, for the first binary variable $(k = 1)$, which corresponded to the second round of package warnings (line C in Figure 4), we reverted $Z_{1t} = 0$ on the date that the third round of warnings went into effect (line E in Figure 4). For the second binary variable $(k = 2)$, which corresponded to the comprehensive tobacco control law (line D in Figure 4), Z_{2t} remained equal to 1 from the date that the law went into effect and did not revert to 0. For the third binary variable $(k = 3)$, which corresponded to the single presentation rule and the third round of warnings (line E in Figure 4), Z_{3t} also remained equal to 1 from the date that the rule went into effect. For the fourth binary variable $(k = 4)$, which corresponded to the 80-percent rule and the fourth round of warnings (line F in Figure 4), Z_{4t} likewise remained equal to 1 from the date that the rule went into effect. For the fifth binary variable $(k = 5)$, which corresponded to the fifth round of warnings (line G in Figure 4), Z_{5t} remained equal to 1 from the date that the warning went into effect through the end of

2012, when our period of analysis terminated. Although the third and fourth rounds of warnings were of limited duration, we could not revert the corresponding binary variables Z_{3t} and Z_{4t} to 0, as these measures were coupled with rules that remained permanently in effect.

Based on the foregoing notation, our econometric specification for the smoking cessation model was

$$(1) \qquad q_{ijt} = X_i\alpha + \gamma A_{jt} + \delta \log P_t + \sum_{k=1}^{5} \beta_k Z_{kt} + \nu_j + \varepsilon_{ijt}$$

where the vector α and the scalars γ, δ, and $\{\beta_1,\ldots,\beta_5\}$ are the unknown parameters of interest, ν_j is the fixed effect for site of care j, and ε_{ijt} is an error term.

While the parameter γ in equation (1) gauges the effect of tobacco control measures at the healthcare provider level, the parameters $\{\beta_k\}$ capture the incremental effects of non-price tobacco control measures at the national level. For example, if a woman was observed in the third trimester of her pregnancy on 1-July-2007, before any of the five specific measures went into effect, then the corresponding values of the variables $\{Z_{kt}\}$ would be $\{0,0,0,0,0\}$, and the combined effect of all non-price national-level measures would be $\sum_{k=1}^{5} \beta_k Z_{kt} = 0$. On the other hand, if a woman was observed in the third trimester of her pregnancy on 1-January-2011, then the corresponding values of the variables $\{Z_{kt}\}$ would be $\{0,1,1,1,0\}$, and the combined effect of all non-price national-level measures would be $\beta_2 + \beta_3 + \beta_4$. Thus, in the evaluation of the five specific policy measures, our reference case was the interval during 2007 when the prohibition of smoking in public spaces and enclosed public and private work spaces (line A in Figure 4) and the first round of warnings (line B in Figure 4) were in effect.

Smoking Cessation: Identification Strategy

In our specification in equation (1), we sought to identify three different types of tobacco control policies. The first represents policies operating at the level of the healthcare provider, specifically, the tobacco cessation clinics established through agreements with the FNR. Here, our identification strategy was based on both temporal and provider-specific variation in these agreements, so that equation (1) effectively corresponds to a difference-in-difference model.

The second set of policies refers to tax increases that operate through retail prices. Here, our identification strategy was based on temporal variation in cigarette prices at the national level. Entertaining the possibility that cigarette price was endogenous, we chose cigarette taxes as an instrument.

The third set of policies refers to non-price nationwide tobacco control measures, including rotating warnings on packages, restrictions of brands to a single presentation, increasing the size of package warnings, and a ban on nearly all tobacco advertising. Here, our identification strategy was based on differences in the timing of these measures. We specified a separate binary variable covering the time period when each measure remained in force, and we linked each measure to the date when each woman was in the midpoint of the third trimester of her pregnancy. Thus, we did not address the possibility that a recently enacted measure became effective only with a delay, or that an expiring measure (as in the case of the second round of warnings) may have had a persistent effect. By assuming an additive linear model for each binary policy variable, we effectively attempted to identify its average effect over time. By assuming that our binary policy variables are exogenous, we excluded the possibility of policy endogeneity, which might occur if the decision to implement a particular policy depended on the observed effects of a prior policy.

Smoking Cessation: Estimation Methods

We estimated equation (1) as a linear probability model, both by ordinary least squares (OLS) and via two-stage least squares (2SLS), in each case with robust standard errors. Our instrument for the endogenous price variable $\log P_t$ was the logarithm of real total taxes per pack. To assess the validity of $\log T_t$ as an instrument, we checked its significance in the first stage equation.

The linear probability model has the advantage that the estimates of the parameters $\alpha, \gamma, \delta,$ and $\{\beta_k\}$ will be consistent even if the estimates of the fixed effects $\{\nu_j\}$ are inconsistent due to small numbers of smokers in some sites of care. However, in our subsequent decomposition of the effects of policy measures, maternal and health center characteristics, we relied upon the estimated values of these fixed effects. Accordingly, we excluded all observations from any site of care in any given year that had fewer than 15 smokers in that year.

Smoking Cessation: Tests for Potential Measurement Bias

In Appendix C we consider two additional sources of bias in the measurement of quit rates. First, we studied whether the reporting of smoking cessation was subject to non-response bias. Second, we studied the possibility of selection bias resulting from earlier observation during the course of pregnancy, as women increasingly responded to policies urging them to seek timely prenatal care.

Birth Weight: Econometric Specification

Let the continuous dependent variable w_{ijt} denote the birth weight of the newborn delivered by woman i, who received prenatal care at site j, and who was in her third trimester of pregnancy at calendar date t. We assumed the following econometric specification:

$$(2) \qquad w_{ijt} = X_i\lambda + \theta q_{ijt} + \mu_j + \eta_{ijt}$$

where q_{ijt} and X_i are defined in (1), the vector λ and the scalar θ are the unknown parameters of interest, μ_j is a fixed effect for site of care j, and η_{ijt} is an error term.

As a consequence of unobserved heterogeneity, the error term η_{ijt} in (2) is likely to be correlated with the error term ε_{ijt} in (1), and thus the variable q_{ijt} in (2) is likely to be endogenous. In principle, the correlation between the two error terms ε_{ijt} and η_{ijt} could be positive or negative. A woman with a propensity to engage in risky behaviors will tend not to quit smoking and deliver a low-weight baby. In that case, the correlation will be positive, and failure to account for the unobserved heterogeneity will overestimate the parameter θ. Alternatively, a woman who runs into complications during her pregnancy will be under pressure to quit smoking and tend to deliver a low-weight baby. In that case, the correlation will be negative and the parameter θ will be underestimated.

Birth Weight: Identification and Estimation Methods

We used instrumental variables to identify the key parameter θ in equation (2). Our instruments included the full set of tobacco control policy variables $\left\{\log T_t, A_j, Z_1,\ldots,Z_5\right\}$ defined above. We estimated the parameters of equation (2) using 2SLS with robust standard errors. We used Hansen's J statistic to test for over-identifying restrictions. We used the Wald F based on the Kleibergen-Paap statistic and the Stock-Yogo critical values for weak instruments to check for instrument relevance and to reject weak instruments (Stock and Yogo 2005, Baum, Schaffer et al. 2007).

Robustness Checks

In addition to estimating equations (1) and (2), we considered several alternative specifications. In particular, we tested whether the effect of price was different for women who sought care at health centers located along the frontiers with Argentina and Brazil, where significant traffic contraband cigarettes was a possibility. We further tested interactions between the maternal characteristics, site-level tobacco control measures and national-level tobacco control measures including price. While the timing of prenatal care was not the focus of our investigation, we recognized that our variable corresponding to initiation of care in the first trimester could be endogenous due to unobserved heterogeneity. For example, pregnant women with heightened perceptions of risk and thus a higher likelihood of quitting might seek care earlier. To address this possibility, we ran our model solely on the subset of pregnant smokers who initiated care in the first trimester.

As additional robustness checks, we varied the sample used to test our model. In particular, we reran our model excluding women who received care at the Centro Hospitalario Pereira Rossell, the largest public maternity hospital in the country, which had not made the transition to the new SIP data entry system until 2009. We also reran our model excluding all observations in 2007, which had fewer observations than subsequent years (Figure 4). Finally, we varied the threshold for the minimum number of smokers required to include observations on a particular site of care in a given year.

Other robustness checks entailed changes in the definitions of our variables. In particular, we assumed that women with missing values of their third-trimester smoking status had continued to smoke. Alternatively, we redefined a quitter as a pregnant woman who did not smoke in both the second and third trimesters. We also varied the list of maternal characteristics X_i, adding the use of alcohol and other drugs during pregnancy. Finally, in our linear models, we estimated clustered standard errors based on year and site of prenatal care. We also computed 2SLS estimates of the birth weight model using alternative subsets of instruments.

Quitting, Relapse and Repeat Quitting between Pregnancies

In addition to our econometric analyses of smoking cessation, we performed a descriptive analysis of transitions between quitting, relapse and repeat quitting for the subset of women who had at least two pregnancies recorded in the new SIP data entry system during 2007–2012.

Results

Smoking Cessation

Table 1 displays the principal results of our regression models, with a focus on the impacts of tobacco control policies on smoking cessation. The complete listing of parameter estimates is shown in Table A2 of Appendix A. The first column (1) shows the results of ordinary least squares (OLS) estimation. The remaining two columns (2 and 3) show the results of two-stage least squares (2SLS) estimation, where real price was treated as endogenous and real cigarette tax was included as an exogenous instrument. In particular, column (2) shows the results of the first stage equation, in which the dependent variable was the logarithm of real price, while column (3) shows the estimated effects on quitting smoking.

With OLS estimation, the coefficient of the real price variable was not significant. However, our treatment of real price as endogenous (column 3) gave a significant coefficient with the theoretically expected positive sign. In particular, a unit increase in the log real price was found to increase the probability of quitting by 0.143. At the sample mean value of the dependent variable equal to 0.368 (see Table A1), our 2SLS model gave an estimated price elasticity of third-trimester quitting equal to $\varepsilon = \left(\frac{1}{q}\right)\left(\frac{dq}{d\ln P}\right) = \left(\frac{1}{0.368}\right)(0.143) = 0.39$.

The estimated effect of each non-price nationwide tobacco control policy was statistically significant with a positive sign. The earlier policies that took effect during 2007–2009 had larger quantitative impacts than subsequent measures entering into effect during 2010–2012. In particular, in column (3) (2SLS), the combination of the single presentation rule and the third round of package warnings, which entered into effect in February 2009, had the largest estimated impact, with an absolute increase of 14 percentage points in the quit rate. The presence of an agreement between the National Resource Fund (FNR) and the healthcare provider for the diagnosis and treatment of tobacco dependence during the year in which the mother gave birth had a statistically significant positive impact of 5 percentage points.

The first stage estimates of our 2SLS model (column 2) show a significant positive coefficient of the log real tax, supporting its validity as an instrument for log real price. The significant negative coefficients of the first three non-price tobacco control measures, enacted during 2007–2009, suggest that the tobacco industry responded to these measures by moderating its pre-tax price.

Figure 5 shows the contribution of different groups of explanatory variables to the overall trend in quit rates. All data are plotted according to the year corresponding to each woman's third trimester of pregnancy. The circles show the observed quit rates. Line (1) shows the predicted values from our 2SLS model, as given in Table 1. The other plotted lines, each annotated by a number in parenthesis, show the predicted quit rate as the following groups of explanatory values were successively held constant at their mean values for 2007: (2) all non-price policy measures; (3) real cigarette price; (4) the presence of an agreement for treating nicotine addiction, as well as all maternal characteristics other than the fixed effects for the site of prenatal care. Comparison of the four line segments permits us to observe the cumulative effects of different groups of explanatory variables over time, taking the midpoint of 2007 as the starting point and the midpoint of 2012 as the ending point.

It is evident from Figure 5 that the non-price nationwide tobacco control measures were responsible for the vast proportion of the observed rise in quit rates during 2007-2012. The observed quit rate increased from 15.4% in 2007 to 41.6% in 2012, while the corresponding predicted values were 15.3% in 2007 and 41.5% in 2012 (curve 1). Setting all non-price nationwide tobacco control measures to their 2007 mean values reduced the predicted quit rate in 2012 to 15.8% (curve 2). Setting both the non-price nationwide measures and real cigarette price to their 2007 mean values further reduced the predicted quit rate to 13.1% (curve 3). Eliminating the influence of all explanatory variables except the fixed effects for each site of care reduced the predicted quit rate to 12.5% in 2012 (curve 4).[3]

The minor contribution of price to the observed rise in quit rates was due in great part to the relatively small net increase in mean real price. Specifically, during 2007–2012, the mean

[3] The 2-percentage-point decline between 2008–2009 in curve 4 was attributable to a substantial drop in the proportion of pregnant smokers attending a particular health center. As a result of changes in the national health system, this health center had attracted a large number of nonsmoking adolescent patients who effectively crowded out many pregnant smokers.

logarithm of real price increased by 0.19 (equivalent to a 20-percent increase in real price). With an estimated price coefficient of $\hat{\delta} = 0.143$, the estimated net change in the quit rate due to price would be 2.7 percentage points. This is to be compared to an absolute predicted increase of $26.2 = 41.5 - 15.3$ percentage points during 2007-2012.[4]

Nor did the contractual agreements between the National Resource Fund and healthcare providers appear to have played a major role in the overall increase in smoking cessation during pregnancy, despite the significant coefficient ($\hat{\gamma} = 0.047$) observed in Model 2 (Table 1). Within the complete SIP registry, the estimated proportion of women who received care at health centers with FNR agreements went from 35% in 2007 to 32% in 2012. Within our sample of pregnant smokers, the corresponding participation rates were 27% in 2007 and 24% in 2012. Our inability to identify agreements between the FNR and some public health centers may explain part of the difference in penetration between smokers and the entire population.

In addition to the results displayed in Table 1 and Figure 5, we ran a wide variety of robustness checks, as described above in the Methods section. Our findings, which are detailed in Table D1 in Appendix D, were generally consistent with our principal results.

Birth Weight

Table 2 shows our principal regression results for birth weight, based on equation (2) above. The complete results are given in Table A2 of Appendix A. Column (1) shows the results of our OLS regression of birth weight (measured in grams) on the fact of quitting, along with the same maternal and site-specific covariates used in our regression model for quitting (Table 1). According to our OLS estimate, quitting smoking by the third trimester was associated with an increase of 122 grams in birth weight.

Columns (2) and (3) present the results of our 2SLS estimation procedure. Column (2), in particular, shows the first stage, in which the fact of quitting is a linear function of all maternal and site-specific covariates, as well as the following seven additional instruments: the five non-price nationwide policies, the log real cigarette tax, and the presence of a tobacco treatment agreement with the health center during the year of delivery. All instruments had significant

[4] This calculation, which is based on annual averages, does not take into account the increase in real price within the year 2007. However, the change in logarithm of real price from January 2007 through December 2012 was 0.23, which would yield an estimated increase in the quit rate of 3.3 percentage points. See Table 3 below for an alternative calculation of the mean population-attributable effects of each tobacco control policy.

positive coefficients. Moreover, all tests rejected the hypotheses of weak instruments or over-identification. The application of 2SLS, based on the foregoing instruments, increased the estimated impact of quitting smoking to 163 grams (column 3).

The results in Table 2 permit us to calculate the mean population-level impact on birth weight of each of the tobacco control measures used as instruments. These calculations are shown in Table 3. Column (1) reproduces the coefficients of the seven instruments from the first-stage regression in Table 2. Column (2) shows the mean change in each policy variable during the entire observation period 2007–2012, that is, the sample mean value less its pre-2007 base value. Column (3) computes the mean population-level impact on the probability of quitting as the product of columns (1) and (2). Column (4) computes the mean population-level impact on birth weight as the product of column (3) and the estimate of 163 grams derived from Table 2.

Quitting, Relapse, and Repeat Quitting, 2007–2012

For women with more than one pregnancy during 2007–2012, we could also assess rates of relapse and other transitions in smoking status from one pregnancy to another. Based on unique identifiers, we located 37,521 records among 18,325 women who had at least two pregnancies. Within this sample, we defined three mutually exclusive states: (1) did not smoke during pregnancy; (2) smoked during pregnancy but had quit by the third trimester; and (3) smoked during pregnancy but had not quit by the third trimester.

Table 4 shows the resulting transition matrix. An estimated 89.9 percent of women who did not smoke in a prior pregnancy remained nonsmokers in their subsequent pregnancy. Among those who smoked but did not quit in a prior pregnancy, 36.8 percent were either nonsmokers or quit smoking during the next pregnancy. Among women who had quit smoking in a prior pregnancy, 51.2 percent remained nonsmokers, while 48.8 percent were found to have relapsed in their subsequent pregnancy. However, within the relapsing group, the probability of quitting once again in the next pregnancy was $0.199/0.488 = 40.8$ percent.

Discussion

Since 2005, Uruguay has embarked on an unprecedented nationwide tobacco control campaign. In order to assess the impact of the campaign, we analyzed Uruguay's comprehensive nationwide registry of pregnancies ending in a live birth during 2007–2012. We focused sharply on smoking cessation among those women who reported smoking at any time during pregnancy, as well as the consequences of smoking cessation for birth weight. We observed a striking increase in the proportion of pregnant smokers who had quit by their third trimester, from 15% in 2007 to 42% in 2012 (Figure 3). In an econometric analysis of individual-level data on nearly 29,000 pregnant smokers, we found that the tobacco control campaign, taken as a whole, was in fact responsible for the marked increase in quit rates. We were able to identify a quantitative impact of price, with an estimated elasticity of quitting on the order of 0.4, similar to that observed in demand studies of the general population. However, we found that price increases explained only a small proportion of the overall rise in smoking cessation during pregnancy (Figure 5). This was due in great part to the reaction of tobacco manufacturers, who responded to tax increases and non-price policies by moderating their pretax prices. To the contrary, nearly all of the observed increase in quit rates from 2007–2012 could be explained by a series of non-price nationwide tobacco control measures, including rotating pictograms with warnings on cigarette packs, a ban on almost all tobacco advertising, and the restriction of all cigarette brands to a "single presentation" (Figures 1 and 2). The latter measure, in particular, appeared to have had the largest quantitative impact on quit rates (Table 1). Based upon our results, the "single presentation" rule in combination with the third round of rotating images had a quantitative impact equivalent to a 168-percent increase in real price.[5]

Numerous economic studies of the determinants of tobacco use have concentrated on the effects of two factors: price and restrictions on smoking in public spaces and private workspaces. Specialists in marketing and consumer psychology have conducted many experimental studies of the influence of cigarette pack design on smokers' perceptions. We are unaware, however, of any prior economic evaluations, based on individual-level data on actual smoking behavior, of the impacts of the non-price tobacco control measures studied in this paper.

In a study of women who had more than one pregnancy, we documented the presence of significant recidivism. During the 2007–2012, about half of the women who quit smoking in the third trimester were reported to be smoking again at the onset of prenatal care in a subsequent pregnancy. A significant proportion of those who relapsed subsequently quit during the next pregnancy (Table 3). We did not have sufficient data to test whether the rate of relapse after

[5] From Table 1, the estimated impact of tobacco control policy $k = 3$ on the probability of quitting was $\hat{\beta}_3 = 0.141$, while the marginal effect of a change in logarithm of real price $\hat{\delta} = 0.143$. The impact of policy $k = 3$ is thus equivalent to multiplying the real price by $\exp\left(\hat{\beta}_3 / \hat{\delta}\right) = 2.68$.

giving birth had changed over time. Still, our analysis of the impact on birth weight indicated that cessation in the third trimester, even if impermanent, had a substantial impact on neonatal health. Permanent cessation of smoking remains an important public health goal.

The deceleration in the upward trend of the quit rate after 2010 (Figure 3) is consistent with diminishing marginal returns to tobacco control policies. One interpretation is that during the 5-year period from 2007-2012, the subpopulation of women of reproductive age who were most susceptible to Uruguay's sequence of tobacco policies was gradually being exhausted. This explanation focuses on the extensive margin. Another interpretation is that each successive tobacco control measure had a smaller marginal impact for each woman. This explanation focuses on the intensive margin. By our estimates, those non-price nationwide measures entering into force during 2007–2009 had substantially larger effects on quitting than those enacted later (Table 1). What we cannot determine from this study are two critical counterfactuals – namely, what would have been the impact on quit rates if the order of these measures had been reversed or if the time intervals between them had increased.

In Table 1, we found that the combination of the single presentation rule and the third round of warnings increased the rate of quitting by 14.1 percentage points. This coefficient was significantly larger than those estimated for the second round (7.9 percentage points), the comprehensive law banning nearly all advertising (7.7 percentage points), the 80% rule combined with the fourth round (2.9 percentage points) and the fifth round (3.1 percentage points). The single presentation rule effectively went into effect at the same time as the third round of rotating images on cigarette packs, while the third round was immediately followed by the fourth, which was in turn coincident with the 80% rule (Figure 1). Unless we made overly strong assumptions about the common mean impact of the warnings, we could not distinguish the effect of the single presentation rule from the third round (nor the effect of the 80% rule from the fourth round). Nonetheless, there are reasons to believe that the single presentation rule was by itself a critically important measure. A manufacturer that previously marketed both red and gold varieties of the same brand had to choose between the two, thus alienating one or another group of previously loyal customers. The same would apply to the choice between plain and menthol varieties of the same brand.

We do not see the tobacco control policies adopted by the Uruguayan government as subject to significant policy endogeneity. Once Uruguay's legislature ratified the Framework

Convention on Tobacco Control, the Ministry of Public Health essentially followed a roadmap established by the FCTC. On the other hand, we uncovered strong evidence that the conduct of tobacco manufacturers was in fact an endogenous response to governmental policies. We found, in particular, that the industry reduced price in response to those non-price tobacco control measures enacted during 2007–2009 (Table 1). Endogenous responses of the tobacco industry to state and nationwide tax increases have been previously documented in the U.S. (Harris 1987, Harris, Connolly et al. 1996).

At the individual level, the smoking cessation programs established under agreement with the FNR had a significant effect on the probability of quitting (Table 1). The relatively low, declining penetration of these programs, however, resulted in a small attributable impact at the population level (Figure 5). Our findings suggest that enhanced targeting of healthcare providers, as well as increased recruitment of patients, could have a high payoff. We cannot address here the cost-effectiveness of programs targeted at healthcare providers relative to national level policies.

As noted above, some economists have suggested that the impact of smoking cessation during pregnancy on birth weight, as estimated from cross-sectional databases, is exaggerated by the presence of unobserved heterogeneity. That is, a woman who tends to engage in risky behaviors will continue to smoke during pregnancy and have lower weight babies. However, the presence of unobserved heterogeneity can also result in an understatement of the impact of smoking cessation. Thus, a woman who encounters complications in the third trimester, such as intrauterine growth retardation, will quit smoking and have a lower weight baby. Our OLS estimate of the effect of quitting on birth weight was 122 grams (95% CI, 108–136). When we used Uruguay's tobacco control policies as instruments for quitting smoking, our 2SLS estimate was 163 grams (95% CI, 40–285). While our 2SLS estimate reinforces the conclusions that smoking cessation during pregnancy, even in the third trimester, has a significant positive effect on birth weight, the confidence interval surrounding that estimate is too wide to draw definitive conclusions about the direction of bias, if any, in the OLS estimate.

Study Limitations

Our study has several limitations. We could not evaluate the impacts of tobacco control measures prior to 2007, as the old SIP registry did not report data on smoking separately by trimester. Only 15 percent of pregnant women quit smoking during their third trimester in 2007 (Figure 3). This finding might suggest that those measures enacted prior to 2007 – including the first round of package warnings and the nationwide regulations on smoke-free public and private spaces, both of which went into effect in 2006 (Figure 1) – had little or no effect on the quit rate. However, our analysis of women with multiple pregnancies (Appendix C) pointed to an extremely low rate of quitting prior to the initiation of the nationwide tobacco control campaign in 2005. Moreover, in a cohort of 716 pregnant women attending two maternity clinics in Montevideo, Uruguay, who were interviewed during January–May 2005, only 5.2 percent reported thus far having quit smoking during pregnancy (Althabe, Colomar et al. 2008).

We had data only on temporal variations in the nationwide real price of legally sold cigarettes and no reliable data on the prices of contraband cigarettes. Nonetheless, we constructed an indicator variable for those healthcare centers located along the international borders with Argentina and Brazil, where the possibility of increased contraband sales might result in effectively lower prices. This indicator variable, when interacted with price, had no significant effect on the quit rate (Appendix D, Table D1, column (4)).

The data on smoking in the SIP registry were self-reported. The use of such self-reported data can potentially lead to measurement errors. Nonetheless, a number of authors have found a strong correlation between self-reported cigarette consumption and objectively measured levels of nicotine metabolites (Castellanos, Munoz et al. 2000, Althabe, Colomar et al. 2008, Himes, Stroud et al. 2013). Uruguay's tobacco control campaign in all likelihood increased the social pressure for a pregnant woman to deny smoking when queried by her obstetrician. If a pregnant smoker falsely denied smoking at all, from her first prenatal visit onward, she would not enter into our analytic sample of smokers. If she admitted smoking initially, but then falsely stated that she had quit, inclusion of her case would tend to overstate the quit rate.

Our analysis did not explicitly take account of the substantial growth in real per capita income that took place in Uruguay during 2007–2012 (World Bank 2013). Nor did it consider possible changes in attitudes and perceptions about the risks of smoking independent of those brought about by Uruguay's anti-smoking campaign. To the extent that cigarettes are a normal good, an increase in incomes would tend to enhance demand and thus decrease quit rates. Argentina, a country with a common international border, language and culture, experienced a comparable rise in real incomes during this period, but did not launch a nationwide tobacco control campaign until 2011. As we have previously noted, consumption of cigarettes among adolescents and the general population declined substantially more in Uruguay than Argentina (Abascal, Esteves et al. 2012).

Conclusions

A series of nationwide tobacco control measures adopted in Uruguay was associated with a marked increase in the proportion of pregnant smokers who quit during their third trimester, from 15% in 2007 to 42% in 2012. The accumulated evidence strongly points to a causal relationship. In an econometric study of individual-level data derived from a comprehensive national registry of pregnancies during 2007–2012, we estimated the separate quantitative contribution of each of the country's major anti-smoking policies. These included the establishment of units to treat nicotine dependence at health centers, a comprehensive tobacco control law that banned nearly all advertising, changes in cigarette price through tax increases, a series of rotating pictograms with warnings on each pack, the banning of multiple presentations of the same brand, and an increase in the size of the required pictograms to 80% of the front and back of each pack. While each of these policies was found to have a significant impact on the rate of quitting during pregnancy, the non-price policies dominated.

Our findings have important implications for future research and for the future design of tobacco control policies. While we had sufficient data to identify the individual contributions of price and non-price tobacco control policies, we were unable to assess possible synergies between them. Nor were we able to address critical questions concerning the order and temporal spacing of tobacco control policies. These should be core areas for future investigation. At the same time, our findings strongly suggest that non-price policies can have an important impact in reducing tobacco use. Such policies include not only those directed to the consumer, but also to the healthcare provider.

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Conflicts of Interest

We have no conflicts of interest to declare.

Author Contributions

All three coauthors contributed to the conceptualization and design of this study, the analysis of the data, and the writing of this report.

Text Tables

Table 1. Smoking Cessation Model: Principal Results [a,b]

	OLS	*2SLS*	
		First Stage	*Second Stage*
Dependent variable	Quit smoking	Log real price	Quit smoking
	(1)	(2)	(3)
Log real price per pack	0.070		0.143**
	(0.052)		(0.061)
Second round of package warnings	0.081***	-0.088***	0.079***
	(0.023)	(0.004)	(0.024)
Comprehensive tobacco control law	0.073***	-0.027***	0.077***
	(0.019)	(0.001)	(0.019)
Third round of package warnings / single-presentation rule	0.152***	-0.036***	0.141***
	(0.027)	(0.005)	(0.028)
Fourth round of package warnings / 80% coverage rule	0.038***	0.030***	0.029***
	(0.010)	(0.002)	(0.011)
Fifth round of package warnings	0.028***	0.004***	0.031***
	(0.008)	(0.001)	(0.008)
Tobacco treatment agreement at site of prenatal care	0.047**	-0.009***	0.047**
	(0.021)	(0.002)	(0.020)
Log real tax per pack		0.501***	
		(0.007)	
Maternal and pregnancy characteristics	yes	yes	yes
Site of care fixed effects	yes	yes	yes
Constant	-0.314	2.316	-0.604**
	(0.212)	(0.028)***	(0.246)

a. Coefficients and standard errors in parentheses. *** = p < 0.01; ** = p < 0.05; * = p < 0.10
b. Based on a sample of 28,597 pregnant smokers during 2007–2012.

Table 2. Birth Weight: Principal Regression Results [a,b,c]

	OLS	*2SLS*	
		First Stage	*Second Stage*
Dependent variable	Birth weight	Quit Smoking	Birth weight
	(1)	(2)	(3)
Quit smoking	122.102***		162.735***
	(7.250)		(62.505)
Second round of package warnings		0.067***	
		(0.025)	
Comprehensive tobacco control law		0.073***	
		(0.019)	
Third round of package warnings / single-presentation rule		0.137***	
		(0.029)	
Fourth round of package warnings / 80% coverage rule		0.033***	
		(0.010)	
Fifth round of package warnings		0.031***	
		(0.008)	
Tobacco treatment agreement at site of prenatal care		0.048**	
		(0.021)	
Log real tax per pack		0.071**	
		(0.031)	
Maternal and pregnancy characteristics	yes	yes	yes
Site of care fixed effects	yes	yes	yes
Constant	2923.694***	-0.272**	2912.188***
	(40.256)	(0.108)	(44.052)

a. Coefficients and standard errors in parentheses. *** = p < 0.01; ** = p < 0.05; * = p < 0.10
b. Based on a sample of 28,563 pregnant smokers during 2007–2012.
c. For tests of weak instruments and over-identification of instruments, see notes to Table A3.

Table 3. Calculation of Mean Population-Level Impacts of Each Tobacco Control Policy

Policy Variable	1st Stage Coefficient (1)	Mean Change (2)	Population Effect: Quit (3)	Population Effect: Birth Weight (4)
Second round of package warnings	0.067	0.105	0.007	1.1
Comprehensive tobacco control law	0.073	0.964	0.070	11.5
Third round of package warnings / single presentation	0.137	0.877	0.120	19.6
Fourth round of package warnings / 80% coverage rule	0.033	0.654	0.022	3.5
Fifth round of package warnings	0.031	0.209	0.006	1.1
Tobacco treatment agreement at site of prenatal care	0.048	0.038	0.002	0.3
Log real tax per pack	0.071	0.866	0.061	10.0
Total Effect			0.289	47.1

(1) From Table 2, Column (2)
(2) Sample mean during 2007–2012 less baseline value at end of 2006
(3) Column (1) × Column (2)
(4) Column (3) × 163 grams (from Table 2, Column (3))

Table 4. Transition Probabilities during 2007–2012.[a]

	Did not smoke [b] (1)	*Smoked and quit* [c] (2)	*Smoked and did not quit* [d] (3)	*Number of transitions* [e] (4)
Did not smoke	0.899	0.037	0.064	15,134
Smoked and quit	0.512	0.199	0.289	1,024
Smoked and did not quit	0.252	0.116	0.632	3,038

a. Calculated from 37,521 records among 18,325 women with at least two pregnancies, representing a total of 19,196 transitions between pregnancies. The rows represent the woman's smoking state in the prior pregnancy. Columns (1) through (3) represent the same woman's smoking state in her subsequent pregnancy. Column (4) shows the number of transitions between pregnancies. In each row, the transition probabilities in columns (1) through (3) add to 1.0, subject to rounding error.
b. Recorded as not smoking in all three trimesters.
c. Recorded as smoking in at least one trimester, but not smoking in the third trimester.
d. Recorded as smoking in at least one trimester, but either smoking or unknown smoking status in third trimester.
e. The total number of transitions between pregnancies (19,196) exceeds the total number of women (18,325) because some women had more than two pregnancies during 2007–2012.

Text Figures

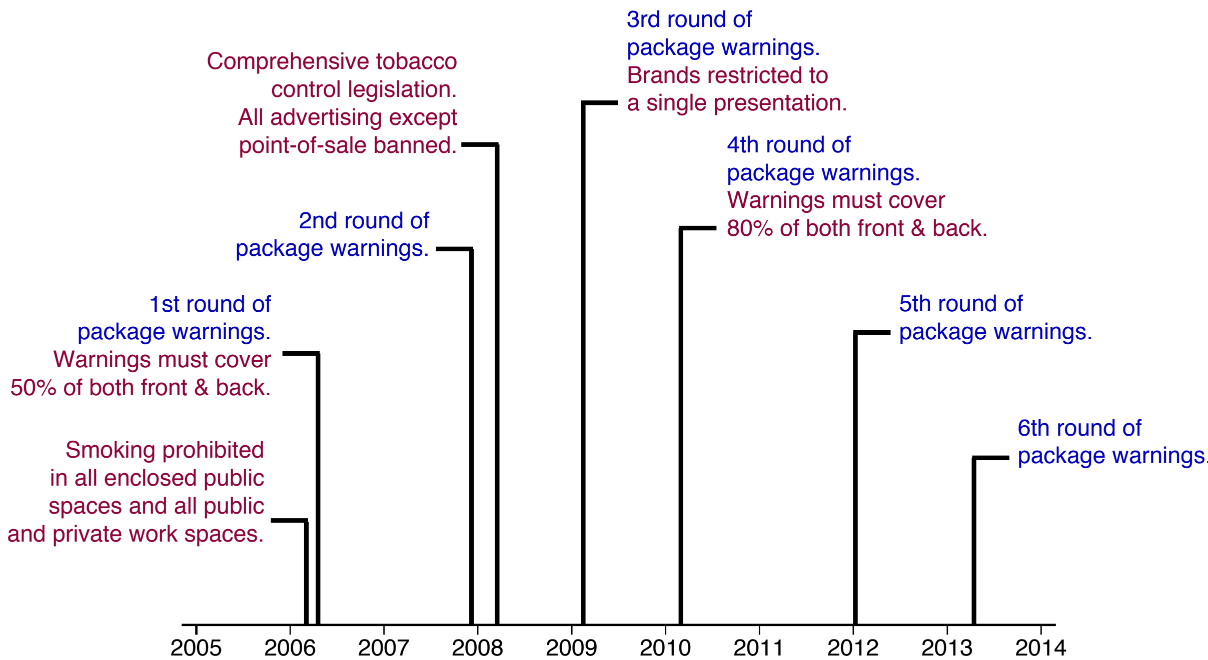

Figure 1. Timeline of Nationwide Non-Price Tobacco Control Measures. The blue text refers to the deadlines for each of the six rounds of rotating package warnings, while the red text refers to other tobacco control measures.

Figure 2. Timeline of Six Rounds of Rotating Package Warnings. Each round displays only one of several mandated images. The relative sizes of the images correspond to their relative sizes on each pack, with the last three rounds reflecting the required increase from 50% to 80% of the front and back surfaces.

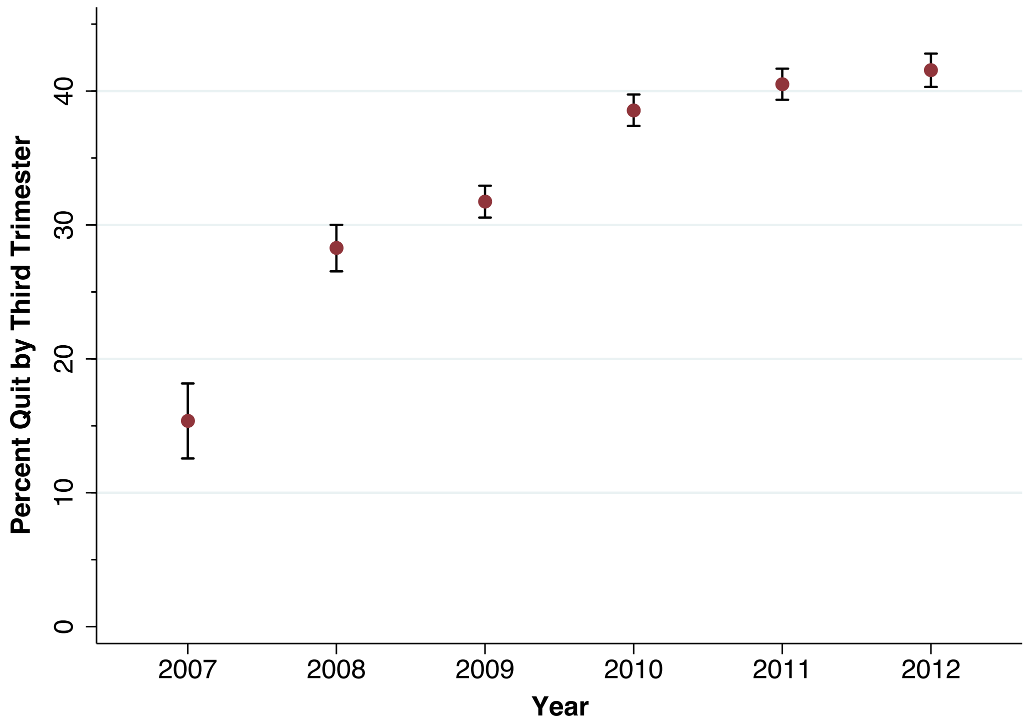

Figure 3. Annual Proportion of Current Smokers Who Had Quit Smoking by the Third Trimester. The data are plotted in relation to the year containing the midpoint of each woman's third trimester. The capped vertical bars represent 95% confidence intervals. The estimates were derived from 28,597 observations in the analytic sample for our regression models in Table 1.

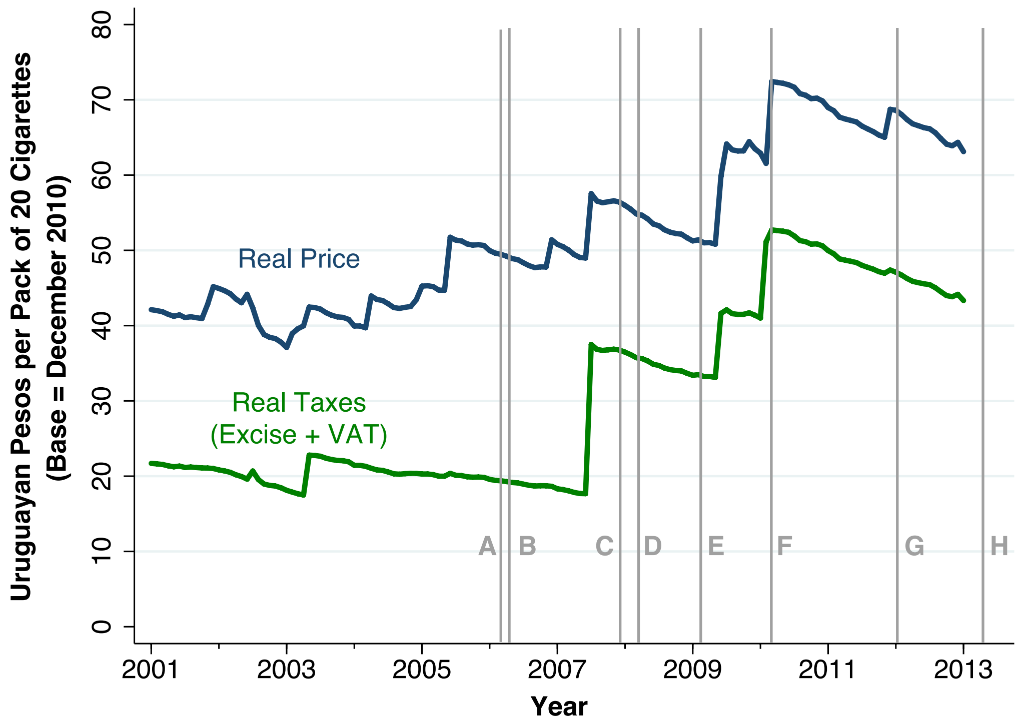

Figure 4. Real Price and Real Taxes per Pack of 20 Cigarettes, 2001–2012. The vertical lines show the timing of non-price nationwide policy measures, as described in Figure 1. A: Smoking prohibited in all enclosed public spaces and all public and private workspaces. B: 1st round of package warnings. C = 2nd round of package warnings. D: Comprehensive tobacco control legislation. E: 3rd round of package warnings; brands restricted to a single presentation. F: 4th round of package warnings; warnings must cover 80% of front and back. G: 5th round of package warnings. H: 6th round of package warnings.

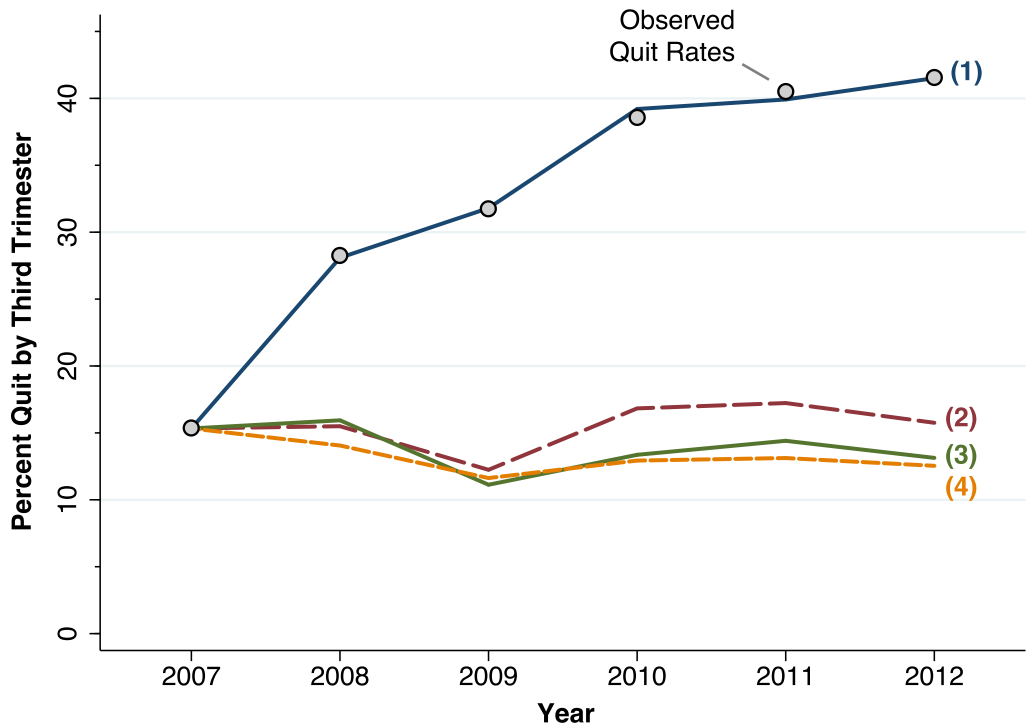

Figure 5. Decomposition of the Effects of Groups of Explanatory Variables on the Predicted Quit Rate, 2007–2012. All data are plotted according to the year corresponding to each woman's third trimester of pregnancy. The circles show the observed quit rates. Line (1) shows the predicted values from our 2SLS estimates, as given in column 3 of Table 1. The other plotted lines, each annotated by a number in parenthesis, show the predicted quit rate as the following groups of explanatory values were successively held constant at their mean values for 2007: (2) all non-price policy measures; (3) real cigarette price; (4) the presence of an agreement for treating nicotine addiction, as well as all maternal characteristics other than the fixed effects for the site of prenatal care.

Appendix A. Smoking Cessation: Descriptive Statistics and Regression Results

Table A1. Descriptive Statistics of Analytic Sample of Smokers, 2007–2012.[a]

Variable Name [b]	*Variable Definition* [c, d, e]	*Sample Mean*	*Sample S.D.*
Outcomes			
QUIT *	Not smoking in third trimester	0.368	0.482
BIRTH WEIGHT	Birth weight	3157	556
Price variables			
LOGPRICE	Logarithm of real price per pack (Uruguayan pesos, base = Dec-2010)	4.162	0.100
LOGTAX	Logarithm of real tax per pack (Uruguayan pesos, base = Dec-2010) †	3.792	0.154
Tobacco-Control Measures			
ROUND2 *	Second round of package warnings (8-Dec-2007)	0.105	0.307
LAW *	Comprehensive tobacco control law (6-Mar-2008)	0.964	0.187
ROUND3_SINGLE *	Third round of package warnings and single-presentation rule (14-Feb-2009)	0.877	0.329
ROUND4_80PCT *	Fourth round of package warnings and 80% coverage requirement (28-Feb-2010)	0.655	0.476
ROUND5 *	Fifth round of package warnings (07-Jan-2012)	0.209	0.407
Provider-level Variables			
AGREEMENT *	Prenatal care site had tobacco treatment agreement with the FNR in the year of delivery	0.254	0.435
Maternal characteristics			
TRIM1 *	Prenatal care initiated in 1st trimester	0.574	0.494
AGE<16 *	Age < 16 years	0.016	0.127
AGE17–19 *	Age 17–19 years	0.155	0.361
AGE20–34 *	Age 20–34 years §	0.726	0.446
AGE35–39 *	Age 35–39 years	0.079	0.270
AGE40+ *	Age 40 or more years	0.023	0.151
ABORT *	At least one prior abortion	0.206	0.405
ABORT_MISS *	Data on prior abortions missing	0.105	0.307
EDUC_PRIM *	Did not complete secondary school education §	0.357	0.479
EDUC_SEC *	Completed secondary school education	0.568	0.495
EDUC_UNIV *	Completed university education	0.052	0.222
EDUC_MISS *	Completed education missing	0.023	0.151
STAT_SINGLE *	Marital status single	0.244	0.429
STAT_MARRIED *	Marital status married §	0.165	0.371
STAT_COHABIT *	Marital status cohabitation	0.577	0.494
STAT_OTHER *	Marital status other	0.015	0.120
STAT_MISS *	Marital status missing	0.040	0.197

PREECLAMP *	Preeclampsia reported during pregnancy	0.020	0.141
PREECLAMP_MISS *	Preeclampsia report missing	0.191	0.393
ECLAMPSIA *	Eclampsia reported during pregnancy	0.001	0.029
ECLAMPSIA_MISS *	Eclampsia report missing	0.192	0.394
DIABETES *	Prior history of diabetes reported	0.026	0.159
DIABETES_MISS *	Diabetes report missing	0.193	0.395
HYPERT *	Prior history of hypertension reported	0.016	0.125
HYPERT_MISS *	Hypertension report missing	0.191	0.393
MALE *	Gender of newborn male	0.510	0.500
GENDER_MISS *	Gender of newborn missing	0.003	0.054
DELIV0 *	No prior deliveries §	0.369	0.482
DELIV1 *	One prior delivery	0.268	0.443
DELIV2 *	Two prior deliveries	0.160	0.367
DELIV3 *	Three prior deliveries	0.089	0.285
DELIV4+ *	Four or more prior deliveries	0.113	0.317
DELIV_MISS *	Number of prior deliveries missing	0.086	0.281
BMI<18.5 *	Body Mass Index (BMI) at onset of pregnancy < 18.5	0.087	0.282
BMI18.5–24.9 *	18.5 ≤ BMI < 25 at onset of pregnancy §	0.724	0.447
BMI25–29.9 *	25 ≤ BMI < 30 at onset of pregnancy	0.131	0.338
BMI30+ *	30 ≤ BMI at onset of pregnancy	0.058	0.234
BMI_MISS *	BMI at onset of pregnancy missing	0.211	0.408
ALCOHOL *	Alcohol use reported during pregnancy ‡	0.030	0.172
DRUGS *	Use of drugs reported during pregnancy ‡	0.041	0.198
CHPR *	Received prenatal care in the Centro Hospitalario Pereira Rossell, the largest public obstetric hospital in Uruguay ‡	0.117	0.322
FRONTIER *	Site of prenatal care located close to the international frontier with Brazil or Argentina ‡	0.069	0.253

a. Based on a sample of 28,597 pregnant smokers during 2007–2012, for whom all principal variables were observed.

b. Binary variables are marked with (*).

c. Omitted reference categories are marked with (§).

d. Variables used in alternative specifications are marked with (‡).

e. Instrumental variables are marked with (†).

Table A2. Regression Results: Probability of Quitting Smoking by the Third Trimester.[a]

	OLS	*2SLS*	
		First Stage	*Second Stage*
Dependent variable[b]	Quit smoking	Log real price	Quit smoking
	(1)	(2)	(3)
LOGPRICE	0.070		0.143**
	(0.052)		(0.061)
ROUND2	0.081***	-0.088***	0.079***
	(0.023)	(0.004)	(0.024)
LAW	0.073***	-0.027***	0.077***
	(0.019)	(0.001)	(0.019)
ROUND3_SINGLE	0.152***	-0.036***	0.141***
	(0.027)	(0.005)	(0.028)
ROUND4_80PCT	0.038***	0.030***	0.029***
	(0.010)	(0.002)	(0.011)
ROUND5	0.028***	0.004***	0.031***
	(0.008)	(0.001)	(0.008)
AGREEMENT	0.047**	-0.009***	0.047**
	(0.021)	(0.002)	(0.020)
AGE<16	-0.005	0.002	-0.005
	(0.023)	(0.002)	(0.023)
AGE17–19	-0.022***	0.000	-0.022***
	(0.008)	(0.001)	(0.008)
AGE35–39	0.006	-0.000	0.006
	(0.010)	(0.001)	(0.010)
AGE40+	-0.004	0.001	-0.004
	(0.018)	(0.001)	(0.018)
TRIM1	0.069***	-0.001	0.069***
	(0.006)	(0.000)	(0.006)
ABORT	-0.013*	-0.001	-0.013*
	(0.007)	(0.000)	(0.007)
ABORT_MISS	0.014	-0.001	0.014
	(0.011)	(0.001)	(0.011)
EDUC_SEC	0.063***	0.000	0.063***
	(0.006)	(0.000)	(0.006)
EDUC_UNIV	0.117***	0.001	0.117***
	(0.015)	(0.001)	(0.014)
EDUC_MISS	-0.028	-0.001	-0.028
	(0.018)	(0.002)	(0.018)

BMI<18.5	-0.050***	0.000	-0.049***
	(0.010)	(0.001)	(0.010)
BMI25–29.9	0.026***	-0.001	0.026***
	(0.008)	(0.001)	(0.008)
BMI30+	0.025**	-0.002**	0.025**
	(0.012)	(0.001)	(0.012)
BMI_MISS	-0.041***	0.000	-0.041***
	(0.007)	(0.001)	(0.007)
STAT_COHABIT	-0.026***	-0.000	-0.026***
	(0.009)	(0.001)	(0.009)
STAT_SINGLE	-0.047***	-0.000	-0.047***
	(0.010)	(0.001)	(0.010)
STAT_OTHER	-0.032	-0.001	-0.032
	(0.023)	(0.002)	(0.023)
STAT_MISS	-0.039**	-0.002*	-0.039**
	(0.017)	(0.001)	(0.016)
PREECLAMP	0.077***	0.001	0.077***
	(0.021)	(0.001)	(0.021)
PREECLAMP_MISS	0.001	0.001	0.002
	(0.060)	(0.004)	(0.060)
DELIV1	-0.111***	-0.000	-0.111***
	(0.008)	(0.001)	(0.008)
DELIV2	-0.154***	-0.001	-0.154***
	(0.009)	(0.001)	(0.009)
DELIV3	-0.192***	-0.000	-0.192***
	(0.011)	(0.001)	(0.011)
DELIV4+	-0.227***	-0.001	-0.226***
	(0.010)	(0.001)	(0.010)
DELIV_MISS	-0.029**	-0.000	-0.029**
	(0.015)	(0.001)	(0.015)
DIABETES	0.050***	-0.002**	0.050***
	(0.017)	(0.001)	(0.017)
DIABETES_MISS	-0.061**	-0.005	-0.062**
	(0.029)	(0.003)	(0.029)
HYPERT	0.004	0.003*	0.004
	(0.022)	(0.001)	(0.022)
HYPERT_MISS	-0.030	0.010**	-0.031
	(0.044)	(0.004)	(0.044)
ECLAMPSIA	0.035	-0.007	0.035
	(0.100)	(0.006)	(0.100)

ECLAMPSIA_MISS	0.100	-0.008	0.100
	(0.072)	(0.006)	(0.071)
MALE	-0.006	-0.000	-0.006
	(0.005)	(0.000)	(0.005)
GENDER_MISS	-0.026	0.001	-0.026
	(0.044)	(0.004)	(0.044)
LOGTAX		0.501***	
		(0.007)	
Site of care fixed effects	yes	yes	yes
Constant	-0.314	2.316	-0.604**
	(0.212)	(0.028)***	(0.246)

a. Coefficients and standard errors in parentheses. *** = P < 0.01; ** = P < 0.05; * = P < 0.10
b. For variable definitions, see Table A1. Sample size: 28,597 pregnant smokers during 2007–2012.

Table A3. Regression Results: Birth Weight [a]

	OLS	2SLS	
		First stage [c]	Second stage
Dependent variable [b]	Birth weight	Quit	Birth weight
	(1)	(2)	(3)
QUIT	122.102***		162.735***
	(7.250)		(62.505)
AGE<16	-54.738**	-0.004	-54.577**
	(24.772)	(0.023)	(24.756)
AGE17–19	-10.520	-0.022***	-9.629
	(9.594)	(0.008)	(9.693)
AGE35–39	-85.849***	0.007	-86.212***
	(13.556)	(0.010)	(13.532)
AGE40+	-113.309***	-0.004	-112.984***
	(23.690)	(0.018)	(23.623)
TRIM1	43.728***	0.069***	40.622***
	(7.011)	(0.006)	(8.480)
ABORT	-14.744*	-0.013**	-14.200*
	(8.400)	(0.007)	(8.412)
ABORT_MISS	-24.987*	0.013	-24.781*
	(13.645)	(0.011)	(13.632)
EDUC_SEC	37.730***	0.063***	35.050***
	(7.356)	(0.006)	(8.425)
EDUC_UNIV	24.941	0.117***	19.931
	(17.009)	(0.015)	(18.565)
EDUC_MISS	35.430	-0.029	36.633
	(23.374)	(0.018)	(23.372)
BMI<18.5	-152.623***	-0.049***	-150.569***
	(11.325)	(0.010)	(11.749)
BMI25–29.9	135.777***	0.026***	134.643***
	(9.763)	(0.008)	(9.871)
BMI30+	192.759***	0.025**	191.608***
	(15.787)	(0.012)	(15.842)
BMI_MISS	-19.138**	-0.041***	-17.154*
	(9.109)	(0.007)	(9.670)
STAT_COHABIT	-4.402	-0.026***	-3.600
	(10.533)	(0.009)	(10.575)
STAT_SINGLE	-32.608***	-0.047***	-30.977**
	(12.089)	(0.010)	(12.311)

STAT_OTHER	-10.047	-0.032	-8.984
	(29.022)	(0.023)	(28.969)
STAT_MISS	1.453	-0.039**	3.065
	(19.660)	(0.017)	(19.785)
PREECLAMP	-212.399***	0.077***	-215.436***
	(33.137)	(0.021)	(33.387)
PREECLAMP_MISS	-17.404	0.002	-17.059
	(94.348)	(0.060)	(93.851)
DELIV1	69.555***	-0.111***	74.099***
	(9.122)	(0.008)	(11.391)
DELIV2	53.491***	-0.154***	59.724***
	(11.037)	(0.009)	(14.566)
DELIV3	59.408***	-0.193***	67.208***
	(13.652)	(0.011)	(17.898)
DELIV4+	80.339***	-0.226***	89.481***
	(13.815)	(0.010)	(19.543)
DELIV_MISS	19.972	-0.030**	21.234
	(17.150)	(0.015)	(17.159)
DIABETES *	135.748***	0.049***	133.555***
	(23.531)	(0.017)	(23.684)
DIABETES_MISS	-40.161	-0.063**	-37.887
	(36.161)	(0.029)	(36.249)
HYPERT	-66.089**	0.004	-66.121**
	(32.810)	(0.022)	(32.726)
HYPERT_MISS	-99.849*	-0.029	-98.554*
	(58.169)	(0.044)	(58.373)
ECLAMPSIA	-193.199	0.034	-195.069
	(134.957)	(0.101)	(133.526)
ECLAMPSIA_MISS	176.203*	0.100	173.702
	(105.798)	(0.072)	(105.705)
MALE	100.402***	-0.006	100.665***
	(6.378)	(0.005)	(6.382)
GENDER_MISS	-81.472	-0.064	-78.052
	(73.056)	(0.047)	(72.969)
ROUND2		0.067***	
		(0.025)	
LAW		0.073***	
		(0.019)	
ROUND3_SINGLE		0.137***	
		(0.029)	

ROUND4_80PCT		0.033***	
		(0.010)	
ROUND5		0.031***	
		(0.008)	
AGREEMENT		0.048**	
		(0.021)	
LOGTAX		0.071**	
		(0.031)	
Site of care fixed effects	yes	yes	yes
Constant	2923.694***	-0.272**	2912.188***
	(40.256)	(0.108)	(44.052)

a. Coefficients and standard errors in parentheses. *** = P < 0.01; ** = P < 0.05; * = P < 0.10
b. For variable definitions, see Table A1. Sample size: 28,563 pregnant smokers during 2007–2012.
c. First-stage statistics:

Tests for weak instruments:	
Kleibergen-Paap Wald F(7, 28374) statistic	61.13
Stock-Yogo weak ID test critical values (Stock and Yogo 2005)	
5% maximal IV relative bias	19.86
10% maximal IV relative bias	11.29
20% maximal IV relative bias	6.73
30% maximal IV relative bias	5.07
10% maximal IV size	31.5
15% maximal IV size	17.38
20% maximal IV size	12.48
25% maximal IV size	9.93
Overidentification test of instruments:	
Hansen J statistic	4.799
$\chi^2(6)$ P =	0.570

Appendix B. Computation of Real Cigarette Prices

The filled circles in Figure B1 represent the monthly mean values of the nominal price of a pack of the best-selling Nevada cigarettes during October 2007 through May 2013, as derived from a major online merchandizing outlet in Uruguay (Cavallo 2013). The connected black line labeled “Nominal Retail Price” shows the fitted values of a regression of the monthly mean Nevada price on the monthly tobacco component of the consumer price index (“Índice de Precios del Consumo,” or IPC) extended out of sample to January 2001 (Instituto Nacional de Estadística 2013). Specifically, the fitted regression equation without constant term was: $NEVADA = 0.6985 \times IPC$ (standard error of slope = 0.00131, $R^2 = 0.91$).

The connected brown line labeled “Nominal Excise Tax” shows the monthly nominal excise tax per pack (“impuesto específico interno,” or IMESI), based on unpublished data from the Dirección General Impositiva. The light blue line labeled “Nominal Excise Tax + VAT” adds the value added tax (“impuesto al valor agregado,” or IVA) to the monthly nominal excise tax. The value added tax was computed as 22% of the price, inclusive of the excise tax.

Prior to July 2007, tobacco products were excluded from the value added tax. This exclusion was abruptly eliminated with the fiscal reforms (Law 18.083) that became effective in that month. During June-July 2007, nominal total taxes increased by approximately 16 Uruguayan pesos per pack, whereas the nominal retail price increased by approximately 7 pesos per pack. That is, suppliers bore approximately 55 percent of the total tax increase at that point.

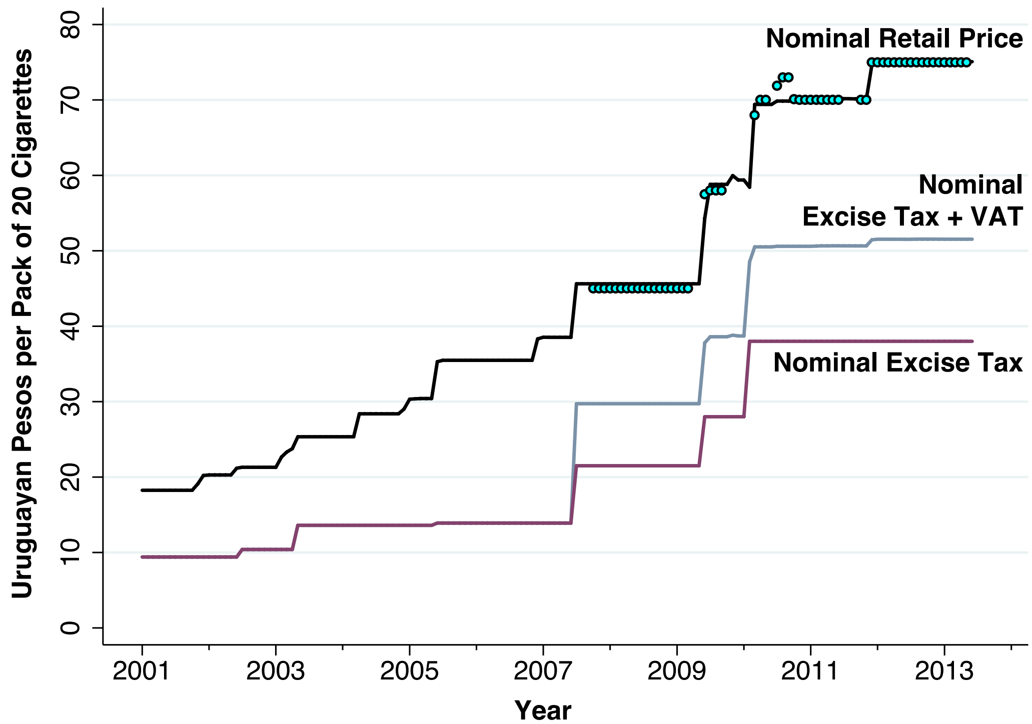

Figure B1. Estimated Nominal Retail Price, Nominal Excise Tax, and Total Taxes per Pack, Monthly, 2001–2012. The filled circles represent the nominal average retail price of a pack of 20 Nevada cigarettes during October 2007 through May 2013. The connected black line at the top shows the fitted values of a regression of mean Nevada price on the tobacco component of the consumer price index, extended to January 2001. The light blue and brown lines show nominal total taxes and nominal excise taxes, respectively.

Appendix C. Analysis of Potential Biases in the Measurement of Quit Rates

In this Appendix, we investigate two potential sources of bias in the measurement of quit rates: non-response bias, and selection bias due to changes in the timing of the first prenatal visit.

Non-response Bias

Figure C1 shows the annual trends during 2000–2012 in the proportion of pregnant women reporting that they currently smoke ("Prevalence," left axis) and the mean gestational age at initiation of prenatal care ("Weeks Gestation," right axis). These trends are plotted in relation to the calendar year in which the mother gave birth. The prevalence calculations are based on the mother's smoking status at the first prenatal visit, which was the only data on smoking reported under the old SIP system (2000–2006). After remaining stable at approximately 21% during 2000–2005, the prevalence of smoking at the onset of prenatal care declined to 19% in the year 2006, 17% in 2007, and 16% in 2008. Thereafter, smoking prevalence appears to have increased to approximately 18% during 2009–2012. Concurrently, the mean gestational age at initiation of prenatal care declined progressively from 16.3 weeks in 2000 to 15.2 weeks in 2005, to 11.5 weeks in 2012.

Figure C2 reexamines the trend in smoking prevalence during 2004–2012, but with a more refined timeline divided into calendar months. The dark blue-filled points ("Prevalence," left axis) show the proportion of pregnant women reporting that they currently smoke, as recorded at the time of the first prenatal visit. By contrast, the red-filled points ("Proportion Missing", right axis) show the proportion of pregnancy records with missing data on smoking status at the onset of care. Both time series are plotted in relation to the calendar month in which the mother gave birth.

The apparent increase in smoking prevalence during the full year 2009 (Figure C1) corresponded to a discrete jump in smoking prevalence in April 2009 (Figure C2). This abrupt increase in prevalence occurred nine months after the Uruguayan government initiated its system of financial incentives to meet specific performance goals ("metas"), including goals for the completion of perinatal records in the SIP registry.[6] Moreover, the abrupt increase in prevalence was nearly coincident with a marked decline in the proportion of missing values for smoking

[6] In statistical tests of both the individual level data and the aggregated data, we consistently rejected the hypothesis that this jump was the result of random variation. In particular, a linear discontinuity regression on quarterly prevalence data with an ARIMA error structure gave an estimated discrete increase in prevalence of 4.05 percentage points, with a 95% confidence interval of [3.39, 4.61].

status at the time of initiation of care. During 2000 through early 2009, the proportion of missing values had gradually increased from approximately 10 to 18% of pregnancy records. By mid 2009, the proportion of missing data was back down to approximately 10%, and by the year 2012, the proportion had fallen to under 5%.

The discontinuity in the monthly prevalence series, coinciding with new regulatory incentives to submit more complete registry data, pointed to a non-response bias in the ascertainment of smoking status at the onset of prenatal care. That is, women whose records contained missing data in fact had higher smoking rates than those women whose smoking status was reported. As a result, the abrupt inclusion of previously missing data raised the observed smoking prevalence in mid 2009 and likely retarded the observed decline in prevalence thereafter.

We performed two tests to determine whether the non-response bias observed in the prevalence data extended to the measurement of quit rates. First, we found no discontinuity around April 2009 in the corresponding monthly series of quit rates. Second, we reran our regression model under the strong assumption that all smokers with missing data on smoking status in the third trimester had not quit. The results, shown in column (5) of Appendix Table D1, showed somewhat smaller coefficients of the tobacco policy variables. However, within the precision of our model, these coefficients were indistinguishable from those of our core model, shown in column (1) of the same table.

While our tests excluded significant non-response bias in the measurement of quit rates, there remains a serious problem of non-response bias in the measurement of prevalence rates. We leave the quantitative analysis of this non-response bias, along with the construction of a corrected series of prevalence rates, to another report.

Selection Bias

The progressive decline in the mean gestational age at initiation of prenatal care, observed in Figure C1 (“Weeks Gestation,” red points, right axis), suggested another potential source of bias in the measurement of smoking prevalence as a result of earlier observation during the course of pregnancy. The underlying hypothesis was that pregnant women tend to quit smoking as soon as they find out that they are pregnant, which typically occurs within the first 15 weeks of gestation. Thus, a woman who arrived for her first prenatal visit at the end of her first trimester, after she had quit smoking on her own, would report that she was not currently smoking. But if she were urged to seek prenatal care early in her first trimester, before she quit smoking on her own, then she would instead report that she was currently smoking at her first prenatal care visit. What's more, when she returned for prenatal care in her second trimester, she would be recorded as having quit smoking.

We used longitudinal data on successive pregnancies of the same woman to assess the magnitude of this potential selection bias. Let y_{im} be a binary indicator of the smoking status of woman i at the time she initiated prenatal care in pregnancy m, where $y_{im} = 1$ if the woman was smoking. Let x_{im} denote the corresponding number of weeks of gestation at initiation of prenatal care. If changes in x_{im} during successive pregnancies were the result of exogenous policies to encourage early prenatal care, but national-level and provider-level measures to encourage smoking cessation during pregnancy had no effect on smoking cessation, then the relation between y_{im} and x_{im} would trace out the natural history of smoking cessation in pregnancy.

We identified 121,105 pregnancies among 55,026 women who had at least two pregnancies during 2001–2012 and whose date of initiation of prenatal care and smoking status upon initiation of care were known. Within this database, we estimated various linear probability models of the relation between y_{im} and x_{im}, all of which had the functional form:

(C1) $$y_{im} = f\left(x_{im}\right) + \varphi_i + \xi_{it}$$

where φ_i is a fixed effect for woman i and ξ_{it} is an error term. In one specification of the function $f\left(x\right)$, we assumed a fixed effect for each week of gestation from $x = 4$ to $x = 40$. The estimated fixed effects are plotted as individual points in Figure C3. In an alternative specification, we assumed a linear spline function with a node at 15 weeks gestation:

(C2) $$f\left(x\right) = \begin{cases} \theta_0 + \theta_1 x, & \text{if } x \le 15 \\ \theta_0 + 15\theta_1 + \theta_2\left(x - 15\right), & \text{if } x > 15 \end{cases}$$

The estimated parameters of specification (C2), with standard errors in parentheses, were $\hat{\theta}_0 = 0.281\left(0.005\right)$, $\hat{\theta}_1 = -0.0034\left(0.0005\right)$, and $\hat{\theta}_2 = -0.00019\left(0.00023\right)$. Effectively, the probability of smoking declines absolutely by 0.34 percentage points per week up to the fifteenth week of gestation. The estimated function $f(x)$ of equation (C2) is shown as the piecewise linear plot in Figure C3.[7]

To assess the magnitude of a possible selection bias, for each pregnant woman i, we first computed $x_{i,\max} = \max_m \{x_{im}\}$, that is, the week of her most delayed prenatal visit. We then calculated the expectation of $\Delta y_{im} = f(x_{i,\max}) - f(x_{im})$ across all women i and all pregnancies m. For the spline model (C2), the overall expectation during 2000–2012 was $E[\Delta y_{im}] = 0.0053$, that is, the potential selection bias was on the order of 0.53 percentage points. This expectation varied from 0.3% in 2001 through 0.8% in 2012. These quantities are small in comparison to the observed 5% absolute decline in smoking prevalence during 2005–2008, as well as the observed rebound in prevalence in 2009. Accordingly, the data are inconsistent with the hypothesis that the observed trends in smoking prevalence were simply the result of selection bias due to changes in the timing of the first prenatal visit.

Such a selection bias would likewise contribute little to the observed trend in the proportion of smokers who quit in the third trimester. Consider the extreme case of a woman who initiated prenatal care at 16.3 weeks gestation in 2000 (the mean for the entire SIP registry in that year) and, in a subsequent pregnancy, initiated care at 11.5 weeks in 2012 (the corresponding mean for that year). The model of equation (C2) implies an increase in the probability of smoking on the first visit from 23% in 2000 to 24% in 2012. Such a 1-percent increase in prevalence would correspond to a $1/24 \approx 4$ percentage-point increase in the proportion quitting, as compared to a 27 percentage-point increase in the quit rate actually observed during 2007–2012 (Figure 3). This calculation also implies a low baseline rate of quitting during pregnancy prior to the initiation of tobacco control campaign.

[7] When we ran the model (C2) only for women who had at least two pregnancies during 2001–2006, the estimated parameters were $\hat{\theta}_0 = -0.279\,(0.0134)$, $\hat{\theta}_1 = -0.0017\,(0.0011)$, and $\hat{\theta}_2 = -0.00006\,(0.00042)$. Thus, the use of data confined to 2001–2006 would result in a smaller estimate of potential selection bias, as well as a smaller estimate of the rate of smoking cessation before 2007.

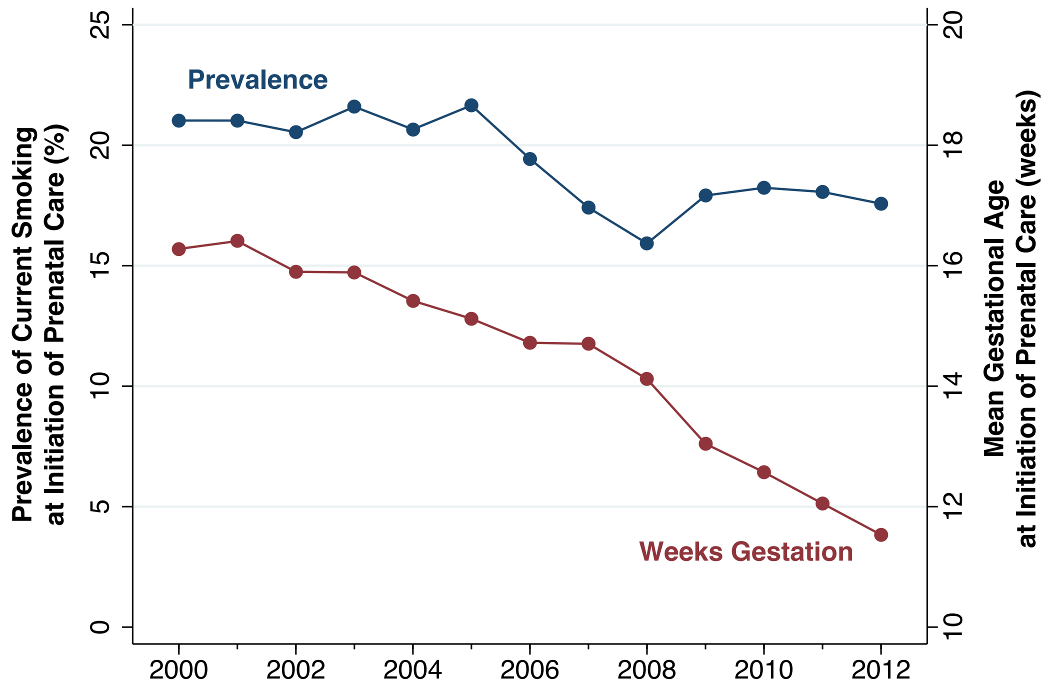

Figure C1. Annual Prevalence of Current Smoking at Initiation of Prenatal Care ("Prevalence," blue points, left axis) and Mean Gestational Age at Initiation of Prenatal Care, 2007–2012 ("Weeks Gestation," red points, right axis). Both data series are plotted in relation to the calendar year in which each woman gave birth. The calculations were based on 247,721 records in the old SIP system (2000–2006) and 251,260 records in the new SIP system (2007–2012).

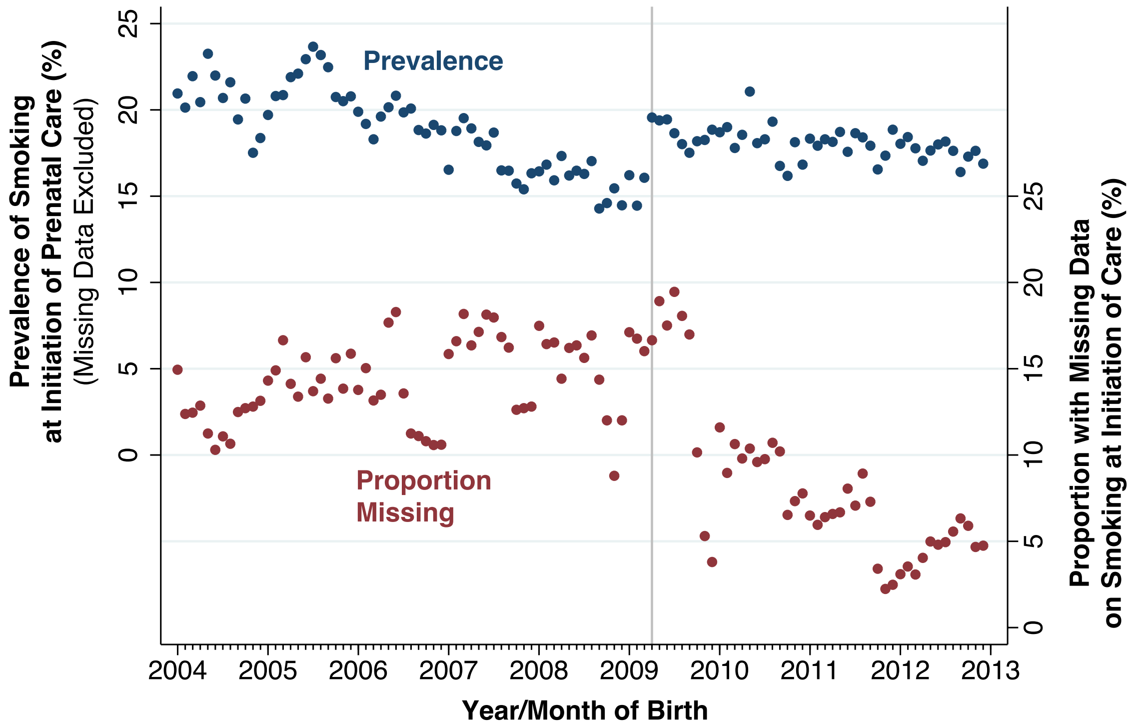

Figure C2. Monthly Trends in the Prevalence of Smoking ("Prevalence", blue points, left axis) and the Proportion of Observations with Missing Data on Smoking ("Proportion Missing," red points, right axis) at the Initiation of Prenatal Care, 2007–2012. The data are plotted in relation to the calendar month in which each woman gave birth. The vertical line corresponds to the second quarter of 2009.

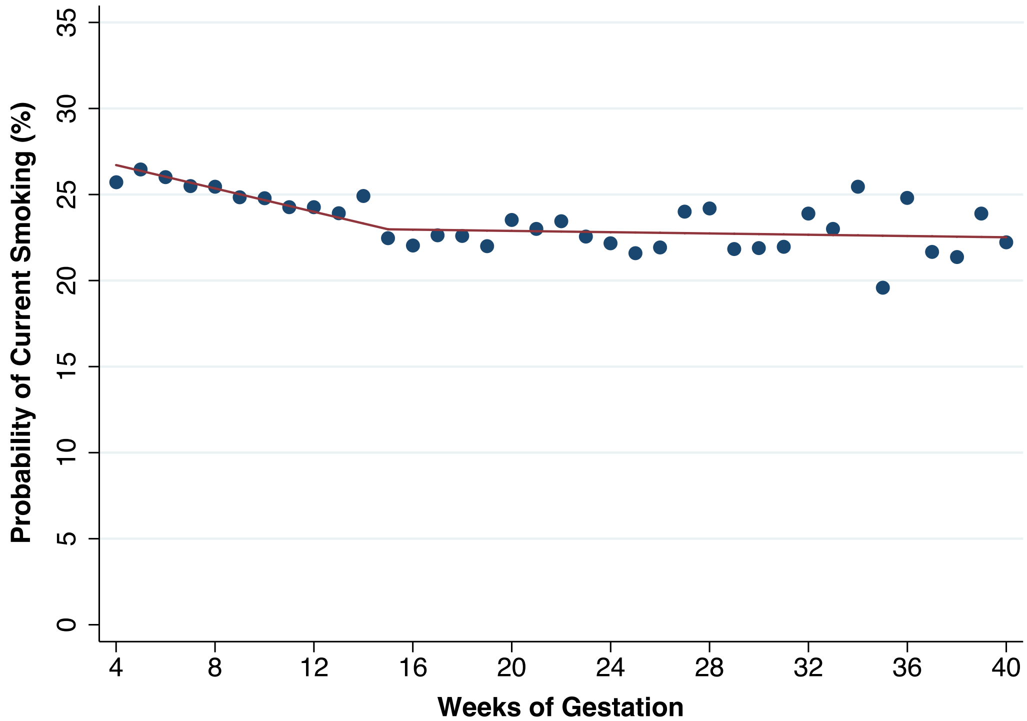

Figure C3. Estimated Natural History of Smoking Cessation during Pregnancy. The figure shows the results of two models estimated on a panel of 55,026 women who had at least two pregnancies during 2001-2012. In the first model, each week of gestation had a separate fixed effect, as represented by the blue points. In the second model, the decline in the probability of smoking was a linear spline function with a node at 15 weeks, as represented by the brown connected line segment.

Appendix D. Robustness Checks: Smoking Cessation

In addition to our principal regression results in Tables 1 and 2, we ran a wide variety of robustness checks. Each of these checks was performed on both our model of smoking cessation and our model of birth weight. Table D1 summarizes our most salient findings. Each column of the table represents an alternative regression model, to be described below. The upper panel shows the corresponding 2SLS estimates of the tobacco control policies in our model of smoking cessation ("Dependent Variable: Smoking Cessation in the Third Trimester"). The lower panel shows the corresponding estimated coefficient of quitting, again estimated by 2SLS, in our model of birth weight ("Dependent Variable: Birth Weight (grams)").

First, we tested additional covariates, including reported consumption of alcohol and drugs during pregnancy (column 2) and an indicator for the location of a healthcare center along the international border (column 3). In our model of quitting, drug but not alcohol consumption significantly reduced the probability of quitting. Proximity to international frontiers had no effect on quitting, nor did women who sought prenatal care in health centers near international borders have a differential response to price. (Coefficients not shown.) In our model of birth weight, drug use likewise was associated with lower birth weight, while proximity to international borders had no effect. (Coefficients not shown.) However, as indicated in columns 2 and 3, the inclusion of these covariates did not significantly alter the coefficients of the policy variables in our model of quitting or the coefficient of quitting in our model of birth weight.

Second, we studied alternative definitions of the dependent variable. In the model of column 4, we defined quitting as reporting non-smoking in *both* the second and third trimesters. With the exception of a statistically insignificant coefficient for the fourth round of warnings combined with the 80% rule, the results for our model of smoking cessation were indistinguishable from those of our core model (column 1). However, the change in the definition of quitting reduced its impact on birth weight from 163 grams (column 1) to 122 grams (column 4). Women who quit only in the third trimester were evidently more likely to have complications of pregnancy, including intrauterine growth retardation. When these women were included in our core model, the 2SLS estimates effectively corrected for this source of unobserved heterogeneity (Table 2). But with the exclusion of these women from the group of quitters, our 2SLS estimate of the impact of quitting did not differ from the OLS estimate (column 4).

In column 5, by contrast, we treated those pregnant smokers with unknown smoking status in the third trimester as continuing smokers. As noted in Appendix C above, this strong assumption resulted in somewhat smaller coefficients of the tobacco control variables in our model of quitting, but these coefficients remained statistically significant. On the other hand, this alternative definition significantly reduced the impact of quitting on birth weight. This finding suggests that many women with unknown smoking status in the third trimester were not, in fact, continuing smokers.

Third, we explored changes in our analytic sample. In the model of column 6, we excluded women who had prenatal care at the Centro Hospitalario Pereira Rossell, the largest public maternity hospital in the country. In alternative robustness checks, we excluded women whose third trimester occurred in 2007 (column 7), as well as women who did not initiate care in the first trimester (column 8).

Data on changes in smoking practices during pregnancy became available only with the introduction of a new prenatal questionnaire and coding system in 2007. However, during 2007–2008, many providers of prenatal care were still making the transition from the old to the new SIP system. Our removal of those providers still in transition resulted in smaller sample sizes for those years. (See the larger confidence intervals in Figure 3.) Under the old SIP system, in which a pregnant woman reported her smoking status only in the trimester of her first prenatal visit, a patient who reported smoking when she received prenatal care in the first trimester could be falsely coded as smoking in all three trimesters. Consequently, incorrect retention on our part of records coded under the old SIP system could result in understatement of the quit rate in the first two years.

Nonetheless, exclusion of women from the Pereira Rossell hospital, which did not make the transition to the new SIP system until 2009, had little discernable effect on the policy-related coefficients in our model of quitting or on the effect of quitting on our model of birth weight (column 6). Exclusion of women whose third trimester occurred in 2007 (column 7) had a marked effect on estimated price coefficient, as this exclusion effectively removed observations surrounding the tax increase in mid 2007 (Figure 4). It also reduced the estimated impact of the agreements for treating nicotine dependence, but otherwise had little effect on the estimates in both our quitting and birth weight models.

We recognized that our variable corresponding to initiation of care in the first trimester could be endogenous due to unobserved heterogeneity. To address this possibility, we ran our model solely on the subset of pregnant smokers who initiated care in the first trimester (column 8). With the exception of the fourth round of warnings combined with the 80% rule as well as a tobacco treatment agreement at the site of prenatal care, the exclusion of women with later initiation of care enhanced the policy coefficients in our quitting model and the coefficient of quitting in our model of birth weight.

We also ran a number of other robustness checks not highlighted in Table D1. In our core model, we excluded sites of care in those years when there were fewer than 15 pregnant smokers. Changing the threshold to 25 smokers had no significant effects on either the tobacco policy coefficients or the impact of quitting on birth weight. Attempts to identify interactions between maternal characteristics and tobacco control policies, or between different tobacco control policy variables yielded insignificant coefficients. We also estimated clustered standard errors based on year and site of prenatal care, but the coefficients of the tobacco control policy variables in the quitting equation and the quitting variable in the birth weight equation remained significant. Finally, in our 2SLS estimates of the birth weight equation, we tested different subsets of instruments, but the results remained essentially unchanged.

In our analysis of the probability of quitting during pregnancy, we relied on linear models (Table 1). A well-known limitation of the linear probability model is that the predicted values of the dependent variable can fall outside the unit interval. Figure D1 shows the distribution of the predicted probability of quitting smoking, calculated from our core model of Tables 1 and 2 (repeated in column 1 of Table D1). Only 1.8% of the predicted values of the dependent variable were situated outside the unit interval.

We recognize that equation (1) for quitting smoking could have been estimated by nonlinear methods where the dependent variable is binary but some regressors are endogenous (Newey 1987). Likewise, we could have used nonlinear methods to estimate equation (2) for birth weight, where the endogenous regressor is binary (Woolridge 2010). These methods generally require parametric assumptions about the underlying distribution of a latent variable that are unverifiable and, if they are incorrectly specified, may lead to inconsistent parameter estimates. We took advantage of the linear structure of our models to decompose the marked increase in the observed quit rate into policy-specific components (Figure 3 and Table 3), and to estimate the mean population-attributable effects of different tobacco control policies on birth weight (Table 3). Our approach is consistent with recent trends in the use of linear models for policy evaluations (Angrist and Krueger 2001).

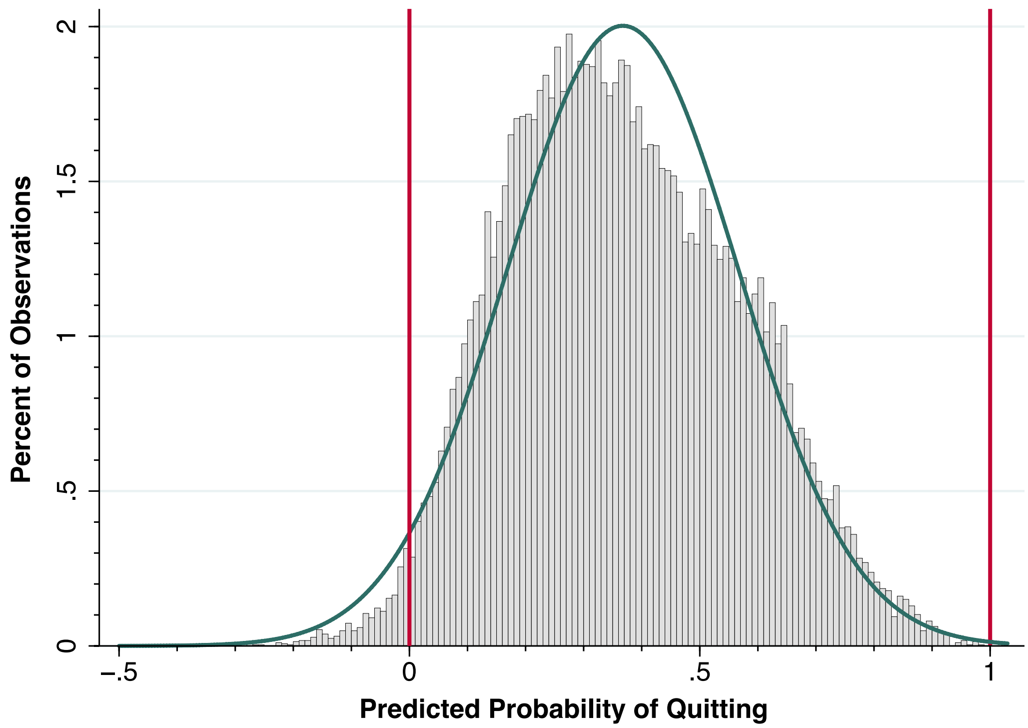

Figure D1. Distribution of Predicted Probabilities of Quitting Based on 2SLS Model. The histogram shows the percentage distribution of the predicted probability of quitting among 28,597 women who smoked during pregnancy and gave birth during 2007–2012. The proportion of predicted values outside the unit interval (whose boundary is marked by the vertical red lines) was 1.8%. The green curve shows the fitted normal density based on the mean and standard deviation of the predicted probabilities.

Table D1: Robustness Analyses of the Smoking Cessation Model

	Core Model	Additional Covariates		Change in Variable Definitions		Changes in Analytic Sample		
Dependent variable: Smoking cessation in the third trimester		Alcohol and drugs	International border and interaction of international border with prices	Quit smoking in second trimester	Unknown smoking status in 3rd trimester assumed to be smoker	Exclusion of Pereira Rossell Hospital	Exclusion of all women whose 3rd trimester occurred in 2007	Exclusion of women not initiating care in 1st trimester
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable: Smoking cessation in the third trimester								
Log real price per pack	0.143**	0.138**	0.136**	0.187***	0.120**	0.189***	0.026	0.215**
	(0.061)	(0.061)	(0.062)	(0.061)	(0.058)	(0.065)	(0.062)	(0.089)
Second round of package warnings	0.079***	0.081***	0.078***	0.070***	0.050**	0.078***	0.057	0.120***
	(0.024)	(0.024)	(0.024)	(0.021)	(0.021)	(0.024)	(0.054)	(0.036)
Comprehensive tobacco control law	0.077***	0.077***	0.077***	0.074***	0.072***	0.080***	0.071***	0.077***
	(0.019)	(0.019)	(0.019)	(0.020)	(0.017)	(0.019)	(0.019)	(0.029)
Third round of package warnings and single presentation	0.141***	0.143***	0.140***	0.107***	0.128***	0.133***	0.134**	0.195***
	(0.028)	(0.028)	(0.028)	(0.025)	(0.025)	(0.028)	(0.056)	(0.042)
Fourth round of package warnings & 80% coverage	0.029***	0.030***	0.029***	0.010	0.032***	0.026**	0.044***	0.021
	(0.011)	(0.011)	(0.011)	(0.011)	(0.010)	(0.012)	(0.011)	(0.016)
Fifth round of package warnings	0.031***	0.031***	0.031***	0.040***	0.030***	0.027***	0.025***	0.040***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.008)	(0.010)
Tobacco treatment agreement at site of prenatal care	0.047**	0.049**	0.047**	0.049**	0.060***	0.046**	0.026	0.033
	(0.020)	(0.020)	(0.020)	(0.020)	(0.019)	(0.020)	(0.022)	(0.029)
Maternal and pregnancy characteristics	yes	yes	yes	yes	yes	yes	yes	yes
Site of care fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
N	28597	28597	28597	28597	30728	25237	28130	16422
Dependent variable Birth Weight (grams)								
Quit smoking in third trimester	163.***	174.***	162.***	122.*	109.*	146.**	153.*	183.**
	(63.)	(62.)	(62.)	(70.)	(60.)	(63.).	(79.)	(73.)
Maternal and pregnancy characteristics	yes	yes	yes	yes	yes	yes	yes	yes
Site of care fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
N	28563	28563	28563	28515	30684	25205	27880	16400

Coefficients and standard errors in parentheses.

* = P < 0.10, ** = P < 0.05, *** = P < 0.01

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