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ABSTRACT

How Effective Are Pictorial Warnings on Tobacco Products? New Evidence on Smoking Behaviour Using Australian Panel Data*

Studies examining the introduction of pictorial warnings on cigarette packages provide inconclusive evidence due to small samples and methodological issues. We use individual-level panel data from Australia to examine the association between pictorial warnings and smoking behaviour – prevalence, quitting, initiating and relapsing. The pictorial warnings were accompanied by a reference to a smoking cessation helpline and supportive television commercials. Applying an event study framework, we show that the reform reduced smoking rates by around 4% within the first year of the policy. The effect decreases with age, is similar for men and women, and is slightly larger for low-educated compared to high-educated individuals. The reform permanently lowered smoking rates primarily due to increased quitting in the year of the reform. Thus, pictorial warnings combined with a reference to a smoking cessation helpline and supportive media campaigns are an important tobacco control measure to reduce the social costs of smoking.

JEL Classification: I12, I14, I18

Keywords: pictorial warnings, smoking, cessation, smoking initiation, smoking relapse

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1 Introduction

Tobacco smoking remains one of the main preventable causes of disease and mortality worldwide (Samet, 2013). Despite the well-documented detrimental health effects of smoking, the World Health Organisation (WHO) estimates that, even in 2017, around 20% of individuals aged 15 and above smoke regularly and that 5 million deaths worldwide are attributable to smoking each year (WHO, 2017). Aside from preventable deaths, the total costs of smoking on society include both the direct costs (e.g., smoking related health care expenditures) and indirect costs (e.g., including productivity losses from morbidity attributable to smoking). Goodchild et al. (2018) estimate that the direct and indirect costs of smoking amount to $US 1.4 trillion in 2012, corresponding to 5.7% of global health care expenditure and 1.8% of the world's gross domestic product. Moreover, tobacco usage not only harms smokers directly, but also imposes large negative fiscal and health externalities on non-smokers: for the case of second-hand smoking, Öberg et al. (2011) estimate that around 600,000 individuals die each year from the exposure to second-hand smoke.

To reduce these substantial costs to society, most countries have implemented different tobacco control policies to help individuals quit smoking and to prevent individuals from starting to smoke. These policies include the regulation of cigarette prices, advertising of tobacco products, legal minimum vending age, smoking bans, and warning labels on tobacco products (for an overview, see, e.g., De Beyer and Brigden, 2003). While text-only warnings on tobacco products were introduced in the mid-1960s, Canada was the first country to legally require full-colour pictorial warnings on tobacco products in 2001 (Hiilamo et al., 2014). Since then, many countries have followed and, in 2016, pictorial warnings had been implemented in 105 countries covering 58% of the world's population (Canadian Cancer Society, 2016).

The rationale behind pictorial warnings on tobacco products is to inform smokers and non-smokers about the documented health risks of smoking and to achieve behavioural changes through the shock-value of loss-framed graphical images (e.g., Hammond, 2011). Seen through the lens of economic theory, the shock-value of the images and the additional health information are intended to lower the marginal utility of consumption and to act as an implicit increase

in the price of current and future cigarettes through the provision of more information about the health care costs of tobacco consumption. Both the rational addiction model (Becker and Murphy, 1988) and behavioural models of irrational addiction (e.g., Gruber and Köszegi, 2001) predict that lower marginal utilities of consumption and future price increases will lead some smokers to quit smoking altogether and other smokers to decrease their current cigarette consumption. Thus, we would expect the policy to reduce smoking rates immediately and on a permanent level. However, the physical and psychological processes through which health information campaigns can permanently affect health behaviour is extremely complex. Thus, it is not surprising that the previous literature examining the effectiveness of health education has produced ambiguous results (see, e.g., Glanz et al., 2008; Tones et al., 2013).

While numerous studies have examined the short-term effectiveness of pictorial warnings on individuals' ability to recall the content of the warning messages, their attitudes and beliefs towards smoking, and their intentions to smoke (see Section 2.1), surprisingly few studies have examined the effects on actual smoking behaviour. In a comprehensive literature review, Monárrez-Espino et al. (2014, p. 30) are fairly critical of the quality of existing studies: "studies assessing the effect of pictorial warnings on cigarette packages on cessation, reduction, and attempt to quit smoking have been very limited in amount and quality and therefore have provided no clear evidence regarding the question of effectiveness of pictorial warning on smoking behavior". While most observational studies find decreases in smoking prevalence and reductions in the number of cigarettes smoked, many of these studies cannot isolate the effect of the policy due to the simultaneous introduction of other tobacco control policies such as changes in cigarette taxes and/or smoking bans, and by failing to account for the general downward trends in smoking over the past few decades (see reviews by Noar et al., 2016a,b). Thus, the evidence on the effectiveness of pictorial warnings on smoking behaviour is still inconclusive. This lack of rigorous empirical evidence is probably best reflected by the current legal battle in the US between the Food and Drug Administration (FDA), which intended to implement graphic warnings in the US in 2011, and the tobacco industry that keeps challenging the empirical evidence for the effectiveness of pictorial warnings in court (NYT, 2016).

The purpose of this paper is to provide new evidence on the impact of pictorial warnings on

tobacco products on smoking behaviour. It is important to recognise that the reform we analyse combines three elements: First, the major change relates to the introduction of pictorial warnings on the front and back of tobacco products' packages. Second, the reform required tobacco manufacturers to move the previously existing reference to the Australian Quitline number, which provides smoking cessation advice, from the side to the back of packages. Third, a series of mass media campaigns were initiated by the central government, several Australian states, and territory-based non-government health agencies to support and reinforce the messages of the new pictorial warnings. As we are not able to disentangle the relative contribution of each of these policies to the overall effect, the effect we estimate hence combines these three elements.

Our study makes the following contributions to the literature. *First*, in a new attempt to transparently estimate the impact of pictorial warnings on smoking outcomes, we examine the reform within an event study approach. Previous studies typically compare mean differences before and after the introduction of similar policies without accounting for secular time trends (see Monárrez-Espino et al., 2014; Noar et al., 2016a,b). This approach, however, is likely to overestimate the true effect given declining trends in smoking rates. The advantage of our approach is that we use data from 2002 to 2010 which allows us to control for secular trends in smoking prevalence prior to the reform to identify the short-run impact of the policy. Compared to previous studies, a further advantage of our setting is that most tobacco control policies, including changes in cigarette taxes, the supply of cigarettes, advertising, or the legal minimum vending age, were not changed simultaneously with the reform. So far, no study has examined the effect of the Australian reform on smoking prevalence at the national level.[2]

Second, many previous studies have relied on small samples where the sample size ranges from less than 100 observations to around 2000 observations per cohort and wave (Monárrez-Espino et al., 2014). We use nationally representative data from the Australian HILDA (Household, Income and Labour Dynamics in Australia) survey to avoid the issues of small and potentially selective samples: our estimation sample consists of 18,863 individuals resulting in 103,007 person-year observations. Moreover, we use longitudinal data which allow us to explore the previously neglected dynamics of individuals' smoking behaviour (Noar et al., 2016a), in particular whether individuals change their smoking status due to the policy. Whereas the previous literature has largely focused on intentions to quit (see Section 2.1), the HILDA data allow us to examine actual smoking behaviour, including quitting and initiating/relapsing between waves.

Third, the large sample size allows us to provide novel evidence on effect heterogeneities, paying particular attention to differences by age groups, gender, and socio-economics status. The previous literature suggests that younger individuals may be more susceptible to the new

[2]See Section 2 for other studies on the 2006 reform. Analysing a different reform, Zacher et al. (2014) document smoking rates in outdoor cafés before and after the simultaneous introduction of standardised packaging and increased pictorial warnings in October/November 2012 in Australia. They find a reduction in smoking rates by 1.7 percentage points (-20.5%) following the introduction of both measures without controlling for declining trends in smoking behaviour.

pictorial health warnings (e.g., Elton-Marshall et al., 2018) and that lower-educated individuals may react more strongly since they typically have less knowledge about the negative health effects of smoking and may find the pictorial warnings easier to process compared to text-only warnings (e.g., Durkin et al., 2009; Cantrell et al., 2013). While such heterogeneous responses have been documented for smoking bans (e.g., Kuehnle and Wunder, 2017) and may have potentially large consequences for socio-economic health disparities, the previous literature on pictorial warnings has largely neglected such heterogeneities.

We find that the introduction of pictorial warnings on tobacco products in Australia was associated with a significant decrease in smoking prevalence of about 0.9 percentage points (4%) on average, and for individuals aged 15 to 29 in particular (-9.3%). The smoking behaviour of individuals aged 50 and over was not affected by the policy. The effect materialises within the first year of the policy and permanently lowered smoking rates. Exploring potential mechanisms, we show that the policy significantly increased the probability to quit smoking only in the year of the reform. The effects are slightly stronger for low-educated compared to high-educated individuals. The results are robust to numerous sensitivity checks, such as controlling for cigarette prices, varying the observation window around the reform date, controlling for individual fixed-effects, and using a placebo reform that takes place one year earlier. Overall, we conclude that pictorial warnings on cigarette packages, supported by targeted commercials and a direct reference to a smoking cessation helpline, are an effective means to reduce smoking rates.

The paper proceeds as follows. First, Section 2 provides a brief review of the relevant literature and the institutional details of the reform. Section 3 introduces the methodological framework and discusses the empirical challenges. Section 4 describes the data and variables used in the analysis. Section 5 discusses the results and shows their robustness. We conclude our paper in Section 6.

2 Background

2.1 Review of effectiveness of pictorial warnings on mechanisms

A large number of studies have examined the effectiveness of pictorial warnings compared to text-only warnings on different outcomes, in particular: (1) general awareness and attention, and (2) health beliefs and attitudes towards tobacco consumption. We examine each of these in turn, as these are the potential transmission mechanisms through which pictorial warnings are expected to affect smoking behaviour (for more comprehensive surveys, see Hammond (2011); Noar et al. (2016a,b).

Many studies show that smokers and non-smokers are highly aware of health warnings, both text-only and pictorial, on cigarette packages (e.g., Borland and Hill, 1997; Hammond et al., 2003; Fathelrahman et al., 2009; Comission, 2009). Studies consistently find an increased level of awareness for health warnings on tobacco products following the introduction of pictorial warnings (see, e.g., Hammond et al., 2006, 2007; Borland et al., 2009a). A closer examination of results reveals that the size, position and image depicted matter for attention: for instance, Bansal-Travers et al. (2011) find that both smokers and non-smokers rate larger, pictorial and loss-framed warning labels as the most effective to attract attention. Gravely et al. (2016) show that increasing the size of the picture from 50% to 80% on the cigarette package in Uruguay led to more recognition of the pictures, a finding that was also reported for Canada (Études de Marché Créatec, 2008). Strasser et al. (2012) find that participants in an eye-tracking experiment looked at pictorial warnings significantly longer than at text-only messages, and that longer viewing times improve the recall of warning messages. Finally, Hammond et al. (2012) show that graphical images depicting real health consequences of smoking were rated more effective than "comic-book-style" images.

Compared to text-only warnings, numerous studies show that pictorial warnings are linked with stronger beliefs and increased knowledge about the health risks of smoking as well as increased motivation to quit (e.g., Borland and Hill, 1997; Hammond et al., 2006; O'Hegarty et al., 2006; Hammond et al., 2007; White et al., 2008; Borland et al., 2009a). In a meta-analysis of 32 longitudinal studies from 20 countries with 812,363 participants, Noar et al. (2016a) find

that stronger health warnings, in particular pictorial versus text-only, were associated with longitudinal increases in knowledge about the health risks of smoking. In another meta-review of 37 experimental studies, Noar et al. (2016b) find that pictorial warnings are more effective than text-only warnings on numerous outcomes, including attention, cognitive elaboration, affective reactions, intentions to quit and intentions to keep away from smoking. The effects on health attitudes and beliefs may be stronger for young adults as Magnan and Cameron (2015) show that young adults perceive pictorial health warnings as providing more information and easier to understand than text-only warning labels. Finally, Elton-Marshall et al. (2018) show that pictorial warnings also reduce knowledge disparities about the health risks of smoking among young adults.

2.2 Institutional background - Australia

Between 1995 and 2006, cigarette packages in Australia were required to include text-only warnings covering 25% of the front and 33% of the back of cigarette packages. In September 2004, the Australian government passed legislation (the "Trade Practices (Consumer Product Information Standards) (Tobacco) Regulations 2004") which required all tobacco products (including cigarettes, loose or pipe tobacco, and cigars) either produced in or imported to Australia to include a pictorial health warning from March 1st 2006 onwards. This new health warning consisted of a text-warning combined with a colourful image that together had to cover 30% of the front and 90% of the back. The warnings showed several tobacco-related health diseases including the risk of developing lung and mouth cancer, heart diseases, and infant health. Two sets of seven pictures were rotated on cigarette packages every 12 months to ensure that viewers did not get accustomed to the same pictures.[3] The size, position, explanatory messages and graphical images were regulated very strictly by law.[4]

In addition to the new graphic images, the 2006 reform also required tobacco products to

[3]For details on the pictures, see `http://www.tobaccolabels.ca/countries/australia/`, last accessed 10/04/2019.

[4]From December 1st 2012 onwards, Australia increased the size of the picture warnings and required the new warnings to cover 75% of the front and 90% of the back. At the same time, mandatory plain packaging was introduced as another tobacco control policy. Since the 2012 reform changed the size of the picture warnings and simultaneously introduced standardised packaging, the 2012 policy reform cannot be used to disentangle the impact each policy had on smoking behaviour. For a very comprehensive review of the literature on the effectiveness of standardised packaging, see McNeill et al. (2017).

include a stronger reference to the Australian Quitline number. Prior to the reform, packets were required to print the Quitline on the side of the pack in small text size. Under the new law, tobacco products had to include a 'stamped' reference to the Quitline number on the pictures on the back of all packs, as well as the message "You CAN quit smoking. Call the Quitline 131 848, talk to your doctor or pharmacist, or visit www.quitnow.info.au". The pictorial warnings were also supported by a number of mass media campaigns that were introduced at different points in 2006. The central government started screening an awareness raising campaign about the new health warnings in mid-February 2006. In addition to the campaign by the central government, several Australian state and territory-based non-government health agencies collaborated to develop supportive television commercials. These commercials depicted instances of amputation and mouth cancer and began screening in May and July 2006 and were intended to reinforce the messages of the new pictorial warnings (Miller et al., 2009b).

A few observational studies have examined the effect of the Australian policy on smoking behaviour for specific subgroups. Focusing on secondary school children aged 12-16, White et al. (2008) examine children's awareness of the warnings, their intentions to smoke, and the cigarette consumption for teenage smokers. Without accounting for time trends in outcomes, the study uses a pre-post comparison and finds a decrease in the proportion of smokers (-5pp, -10.8%) and a decrease in smokers' cigarette consumption (-23.5%). Borland et al. (2009b) use survey data from the International Tobacco Control Policy Research Survey (ITC) study to examine quitting behaviour. The paper compares smokers' quit attempts between 2005 and 2006 and finds a 1.2 percentage points increase in quit attempts (from 42.7% to 43.9%) and no effect on cessation. These high quitting figures are due to the data being a cohort study of smokers; thus, the study cannot investigate smoking prevalence at the population level or smoking initiation.

An important question for the empirical analysis is whether the reform disrupted the supply of cigarettes. For instance, tobacco companies may not have been ready to supply a sufficient amount of compliant products immediately following the policy implementation. If this were the case, we would identify the supply and not the demand channel. However, this is quite unlikely as the new legislation was passed in September 2004, i.e., 18 months prior to its im-

plementation date. Thus, it appears plausible that cigarette manufacturers had sufficient time to ensure that no disruptions in supply would occur.[5] We also examine the tobacco manufacturers' compliance with the reform: As tobacco companies may have an incentive to stockpile the old packages prior to the implementation date, it could take a while before the packages carrying the new warnings have penetrated the market fully. Using self-collected data from Melbourne and Adelaide, Miller et al. (2009b) show that by September 2006, i.e., 6 months after the implementation date, around 80% of the top-selling brands carried the new warnings. This finding is consistent with a study by White et al. (2008) who surveyed Australian secondary school children (age 15 on average) around 6 months after the implementation of the reform and found that 88% of students had noticed the new warning labels. Thus, the majority of the old cigarette stock appears to have been replaced by the end of 2006 – when the individuals in our sample were interviewed.

3 Empirical strategy

We use both non-parametric and parametric event study models to examine the effect of the pictorial warnings on different outcomes (for a similar application, see Dobkin et al., 2018).[6] The models lend themselves very intuitively to our analysis, particularly as the parametric models estimate the consequences of the policy by measuring the policy-induced deviation from a secular pre-reform time trend in smoking rates.

We start with a non-parametric specification which allows us to visually examine how the outcome variable develops over time (condition on control variables). Specifically, we estimate the following equation:

$$y_{ist} = \gamma + X'_{it}\beta + Xstate'_{st}\beta + \sum_{t=S}^{-2} \mu_t + \sum_{t=0}^{F} \mu_t + \alpha_i + \epsilon_{ist} \tag{1}$$

[5] Furthermore, no official statistics exist to check whether the enforcement of minimum age purchasing restrictions were changed following the reform. However, any changes in the enforcement of minimum age restrictions are unlikely to cause substantial demand shifts, as purchases by under-age individuals account for only 1.3% of the total tobacco consumption. This estimate is based on a study by White and Scollo (2003), who estimate that under-age smokers spent around 125$ million AUD on cigarettes in 2002, and National Accounts data from the Australian Bureau of Statistics which shows that the total consumption of tobacco products was 9.03$ billion AUD in 2002.

[6] As our method identifies deviations from an expected time trend, the model may also be referred to as an interrupted time series design, see Bernal et al. (2017).

where the dependent variable y_{ist} denotes the smoking behaviour of individual i living in federal state s at event time t. X_{it} denotes a vector of individual covariates (such as an individual's gender, age, the employment status and type, highest level of education, marital status, citizenship, union status, and federal state dummies). $Xstate_{st}$ denotes a vector of time-varying state-level covariates to account for potentially confounding factors such as state-level smoking bans, the state unemployment rate of 16-64 year olds and the real gross state income per capita.[7] For our main specification, we specify the observation window to range from 2002 to 2010 (corresponding to $S = -4$ and $F = 4$, respectively).[8] The coefficients of interest are the μ_ts which show how the outcome variable changes non-parametrically relative to the year prior to the reform (μ_{t-1}). Since we have panel data, we can include individual-specific random effects (α_i) in our regressions.[9] We cluster the standard errors at the individual level to account for serial-correlation and heteroskedasticity.

We also employ parametric event study models, which control for declining trends in smoking rates over time, to estimate the reform effects and to assess their statistical significance. Consistent with the pre-reform trends we observe in the non-parametric event-study (see Section 5), we use a linear time trend for our main specification. We now include linear event time t as a regressor instead of the indicator variables for the pre-reform periods and estimate the following equation:

$$y_{ist} = \gamma + X'_{it}\beta + Xstate'_{st}\beta + \delta t + \sum_{t=0}^{F} \mu_t + \alpha_i + \epsilon_{ist} \tag{2}$$

where our coefficients of interest, the μ_ts, identify the treatment effect as the relative deviation from the pre-existing linear time trend (δ) in smoking rates.

[7] These state level characteristics are available from the Australian Bureau of Statistics (`https://www.abs.gov.au/`), last accessed 10/04/2019. In a robustness check, we show that our main conclusions also hold when we do not account for these state level variables (see Table 6).

[8] In a robustness check, we also use a shorter observation window (2003-2008) which confirms our main results (see Table 6).

[9] In Section 5, we show that the introduction of the reform does not correlate with any of the covariates in the X vector. Hence, the choice between fixed and random effects depends on whether the null hypothesis that $cov(\alpha_i, \mu_{t=0}) = 0$ can be rejected. We perform the corresponding panel-robust Hausman-tests (Wooldridge, 2002) for all estimation samples and never reject the null-hypothesis of any correlation between the individual specific fixed-effect and the reform indicator variable (results available upon request). We therefore use the random effects estimator as our preferred estimator. We also report the results using the pooled OLS estimator and the fixed-effects estimator in Table 6 – the point estimates for μ_t are almost identical.

To consistently estimate the impact of the reform on smoking behaviour, the empirical approach relies on the two assumptions that i) the introduction of the reform does not correlate with any changes in observed or unobserved individual characteristics correlated with smoking, and ii) that no other policy changes affecting smoking behaviour were introduced simultaneously with the reform. To check the plausibility of these identifying assumptions, we first evaluate the assumptions regarding compositional changes. Similar to other balancing tests, we first adapt equation 2 and estimate the following equation:

$$X_{ist} = \alpha + \beta t + \sum_{t=0}^{F} \mu_t^1 + \nu_{ist} \tag{3}$$

which regresses each covariate X separately on a constant, the time trend t, and the indicator variables for the deviations in each post-reform year from the pre-reform time trend. This strategy checks whether observed socio-economic characteristics that may correlate with smoking behaviour change discontinuously with the introduction of the reform. If the observable characteristics do not deviate from their secular trend between 2005 and 2006, the coefficients for the reform year ($\mu_{t=0}^1$) should not be statistically different from zero. Our regressions (see Section 5) provide strong evidence that many relevant observable characteristics do not change with the introduction of the reform.

The approach also assumes that (ii) no other policy changes affecting smoking behaviour were introduced simultaneously with the pictorial warnings. This assumption is particularly important given the lack of an explicit control group. Thus, we need to ensure that our estimated treatment effect is not confounded by discontinuous changes in cigarette taxes (and consequently prices), the supply of cigarettes, smoking bans, advertising, or any other health campaigns in the reform year. Next, we address each of these issues that could potentially confound our estimates.

First, tobacco excise and customs duty on tobacco products did not change in Australia between 1999 and 2010, apart from adjustments for inflation. Figure 1 shows that real cigarette prices fluctuated only marginally between 48.5 cent and 51 cent per cigarette between 2002 and 2010 and, importantly, were not raised discontinuously between 2005/06. Cigarette prices

even dropped between 2005 and 2006, which would suggest a small positive effect on cigarette consumption given a negative price elasticity for cigarettes (Jawad et al., 2018). Whether to control for cigarette prices is unclear though, as cigarette prices may react endogenously to the reform if printing the warning labels changed the marginal cost of producing a cigarette pack, or if the warning labels shifted the demand curve. Controlling for cigarette prices may therefore result in over-controlling bias and would isolate just the shift in the demand curve, and not the overall effect due to changes in both the demand curve and in prices. As we are interested in the overall policy effect, we do not control for cigarette prices in the main specifications. In the robustness section, we additionally control for changes in real cigarette prices and show that the main conclusions are hardly affected. Thus, the policy seems to have mainly shifted the demand curve for cigarettes, with small indirect effects on prices.

Second, some federal states introduced partial smoking bans in 2006. For instance, in January 2006, Tasmania became the first Australian state to impose a total indoor smoking ban. On July 31 2006, Western Australia also introduced a smoking ban in all indoor areas of pubs, bars, and clubs. Queensland introduced a partial indoor smoking ban in 2002, which was amended in 2005 and gradually phased-in between 2005 and 2006.[10] The prior literature on the effects of smoking bans on smoking behaviour has generally produced ambiguous findings, with many studies showing no effect on smoking prevalence (for an overview, see Anger et al., 2011). To avoid that the effect we estimate for pictorial warnings is confounded by any smoking bans, we control for the adoption of smoking bans in our main models.

Third, we would overestimate the effect of the pictorial warnings if the stronger Quitline reference, which received twice as many calls in the reform year compared to the pre-reform year (Miller et al., 2009a)[11], had an independent effect on smoking rates. However, it is much more likely that the more prominent Quitline reference acts as a mediator of the effect, i.e., that individuals only call the Quitline because of the new pictorial warnings. This is supported by Miller et al. (2009a) who show that calls to the Quitline increased significantly and immediately with the introduction of the new cigarette package warnings. They argue that the size and

[10] For more details, see `http://www.tobaccoinaustralia.org.au/15-7-legislation`, last accessed on 10/04/2019.

[11] Willemsen et al. (2002) have also documented a increase in quit line calls in the Netherlands following the 2002 reform of the EU health warnings on cigarette packages.

timing of the increase in calls to the Quitline is "highly likely due to the introduction of the new graphic cigarette packet warnings that included the Quitline number" (Miller et al., 2009a, p. 237). Moreover, the reform does not appear to have changed the overall awareness about the Quitline services: Miller et al. (2011) show that, in 2005, 71% of respondents were aware of the Quitline; in 2006, the year of the reform, 75% of individuals reported being aware of the Quitline. Examining methods of quit attempts, the proportion of quitters who had called the Quitline was virtually unchanged as it increased from 7% to 8% between 2005 and 2006. Taken together, it appears unlikely that the new Quitline reference itself had an independent effect on smoking rates as the Quitline services were already widely known prior to the reform.

Fourth, the additional television commercials may have affected smoking outcomes. While prior research suggests that television advertising has the potential to increase smoking cessation (e.g., McVey and Stapleton, 2000), recent Cochrane review articles conclude that the quality of existing studies on the effectiveness of mass media campaigns on smoking prevalence are of very low quality and do not provide any firm conclusions, particularly for young people (Bala et al., 2017; Carson-Chahhoud et al., 2017). We therefore cannot provide a reliable benchmark estimate for the potential effect of the supportive television campaigns.

However, it is unlikely that the television commercials had a large and independent effect on smoking outcomes for two main reasons. First, similar to the Quitline calls, policy makers introduced the mass media campaigns to support and reinforce the messages of the pictorial warnings. Thus, the television commercials should not be viewed as having an independent effect, but rather as a moderator for the effect of the pictorial warnings. Second, the frequency of exposure to the television commercials is considerably smaller than the exposure from the new cigarette packs: pack-a-day smokers will be exposed to the new pictures around 140 times per week just through their own consumption. Social-smoking and peer-group effects additionally increase this exposure and may also lead smokers to discuss the contents of the pictorial warnings. In contrast, Miller et al. (2009a) show that, in the weeks of releasing the new commercials, individuals aged 18 and above were expected to view the new commercial at most around 20 times per week; in all other weeks, individuals were typically expected to view the commercials less than 10 times per week, from August to December 2006 even less than five

times per week.[12] Thus, the actual weekly exposure for pack-a-day smokers from the pictures on the cigarette packs is 7-28 times higher compared to the expected exposure from the television commercials. In addition, the proportion of individuals who reported noticing anti-tobacco advertising on Australian television increased only marginally from 89% to 93% between 2005 and 2006 (Miller et al., 2011). Thus, the moderating effect of the television commercials appears to be small, although we cannot reliably assess its relative magnitude.

4 Data

We use longitudinal data from the Australian Household, Income and Labour Dynamics in Australia (HILDA) survey. The HILDA has been conducted annually since 2001 and surveys all individuals in private households aged 15 and above. The survey is nationally representative and provides rich information on respondents' socio-demographic characteristics, their smoking behaviour, and their self-reported physical and mental health.

Our baseline model uses a nine-year window around the reform date (i.e., from 2002 to 2010) and includes all individuals who report non-missing values on their socio-economic characteristics and smoking behaviour. As socio-economic control variables we include an individual's age, gender, highest level of education, employment status, employment type, union status, marital status, ethnicity, and state of residence. For our heterogeneity analysis, we classify individuals with Year 12 or less as low-educated and individuals with at least an advanced diploma or any university degree as high-educated.

We construct our main dependent variable on smoking behaviours from a question in the self-completion questionnaire (SCQ). This is important for the validity of the smoking variables as the confidential mode of the SCQ reduces the potential for social-desirability bias compared to interviewer-administered questionnaires (Bowling, 2005; Kreuter et al., 2008; Krumpal, 2013). In all waves, individuals were asked "Do you smoke cigarettes or any other tobacco products?". The answers allow us to distinguish between never-smokers, quitters, and current smokers. We drop the first survey year, 2001, from our analysis as the question on

[12] Miller et al. (2009a) report in their Figure 1 the target audience rating points (TARPs) for the television commercials as a standard measure of television advertising weight. A TARP of 100 indicates that an individual from the target audience is expected to view the advertisement 1 time per week of the campaign.

smoking was changed between 2001 and 2002.[13] To alleviate concerns about social-desirability bias in reporting smoking behaviour, Online Appendix Figure B.2 shows that the HILDA survey provides very similar estimates of smoking prevalence compared to two other Australian health surveys.

The sample consists of 18,863 individuals resulting in 103,007 person-year observations. Appendix Table A.1 provides descriptive statistics for the entire sample, as well as for the periods before and after the introduction of the reform. The first row shows that the proportion of smokers in the sample has declined from 23.4% between 2002 and 2005 to 20.5% between 2006-2010. No other substantial changes in socio-economic characteristics stand out from the table.

5 Results

5.1 Effect on smoking behaviour

We begin our discussion of the effect of pictorial warnings on smoking behaviour with a series of graphs: Figure 2 plots the coefficients from the non-parametric event study models to show how the proportion of smokers changed relative to the year before the reform. We also superimpose the pre-reform time trend to predict the counterfactual scenario in the post-reform period. Panel A documents a constant decline in the proportion of smokers in the years leading up to the reform, followed by a sharp drop in the level of the trend in the year of the reform. The lower level persists four years after the reform. We observe the same pattern for women and men (Panels B and C) and for individuals below age 50 (Panels D and E), with the strongest response occurring among the youngest age group. Individuals above age 50 do not react to the reform (Panel F). The linear time trend fits the pre-reform patterns very well and implies that simple before-after comparisons that do not account for the secular decline in smoking rates overestimate the effectiveness of the pictorial warnings.

As discussed in Section 3, we first examine the covariate balancing, i.e. whether the socio-

[13] From 2002 onwards, the survey distinguishes between daily, weekly, and occasional smokers. In 2001, in contrast, individuals can only identify themselves as smokers, but not as daily, weekly, or occasional smokers. This changing definition underestimates the smoking rate in 2001 since non-daily and occasional smokers likely do not identify themselves as smokers in 2001, but do so in later waves.

economic characteristics of individuals deviate from their pre-existing trend in the reform year for our main estimation sample (2002-2010). Table 1 presents the coefficient for the μ_0s, i.e., the reform year, from a regression of the socio-economic characteristics as in equation 3. The table shows that the considered covariates do not systematically deviate from the pre-existing trend in the reform year (see Online Appendix Table B.3 for full results on other years). Although some coefficients are statistically significant (i.e. Year 11 or less) due to the large sample size, the economic significance (1.2 percentage points) is very small. Moreover, an F-test for the joint significance of these characteristics between the years 2005 and 2006 shows that these observables do not jointly differ between the immediate pre- and post-reform period (p=0.9582). We show in Online Appendix Tables B.1 and B.2 that the balancing also holds for men and women separately. As the observables are balanced across the pre- and post-period, this should lessen concerns about unobservables changing across the pre- and post-period as well. Thus, any treatment effect we estimate is not due to any unbalanced observable covariates (e.g., ageing effects) or compositional changes in the sample, but must be due to some event occurring between 2005 and 2006.

Next, we estimate the extent to which the reform reduced the probability of individuals to smoke in the first year after the reform. Table 2 presents the results from the parametric event study models for the different subgroups. The results for the whole sample (col. 1) are consistent with Panel A in Figure 2 and provide compelling evidence that the reform significantly lowered smoking rates by 0.9 percentage points.[14] This corresponds to a reduction of 4% based on the pre-reform mean (22.7%). We observe similar patterns for men and women in the year of the reform, although the relative effect size is slightly larger for women (-4.4%) than for men (-3.6%). Substantial differences emerge across the different age groups: individuals aged 15 to 29 show the largest response to the reform, reducing their smoking probability by around 2.6 percentage points (-9.3%). Individuals aged 30 to 49 reduce their smoking probability by 1.3 percentage points (-4.7%); we estimate a zero effect for individuals over the age of 50.[15]

[14]Consistent with the nonparametric estimates from the event-study models in Figure 2, we observe a 1.5 percentage points reduction in average smoking rates between 2005/06. Hence, we would overestimate the true effect (0.9 pp) by around 66% without accounting for the pre-treatment time trend.

[15]Online Appendix Figure B.1 shows that the total amount of consumed tobacco products (including both cigarettes and cigarette equivalents and accounting for contraband products) decreased in Australia between 2003

The results also provide some evidence that the reform had a persistent effect on smoking rates: the negative point estimates persist for up to 54 months after the reform for the entire sample. Consistent with the graphical analysis, we estimate negative point estimates for all subsamples, apart from the oldest group, up to 54 months after the reform. These suggests that the reform caused a persistent downward shift in the probability of smoking. However, the long-term results may be confounded by other changes, such as changes in economic conditions. For instance, the Australian economy slowed down between 2008 and 2009 due to the fallout of the Global Financial Crisis (Eslake, 2009). When we estimate the reform effect on socio-economic characteristics for up to four years after the reform, Online Appendix Table B.3 shows that the employment characteristics of the sample begin to change significantly from 2009 onwards (corresponding to $\mu_{t=3}$). For this reason, we emphasise that our empirical design can consistently estimate the short-run effect of the reform, but other changes, both observed and unobserved, may also affect smoking outcomes in subsequent years. We therefore proceed in the subsequent analyses by estimating the same model (see equation 2), but we present and interpret only the coefficients for the year of the reform, which we can attribute to the introduction of the pictorial warnings.[16]

Table 3 presents a further breakdown of the impact of the reform in its first year, splitting the sample by the highest level of education, gender, and age group. For ease of interpretation, we also report the pre-reform mean for each subgroup as well as the relative effect size. The results for the pooled sample in the first three columns show that the reform significantly reduced the smoking probabilities on average for all groups below the age of 50; only the negative point estimate (-0.010) for men aged 30-49 is insignificant. As before, neither men nor women aged 50 and above reacted to the reform. The strongest reaction emerges among men and

and 2007 and deviated somewhat more strongly from its pre-reform trend between in 2006. However, consumption fell by around 2.9% between 2005 and 2006, which is lower than the 3.9% we estimate. Note, however, that Figure B.1 also mixes changes at the extensive and intensive margins of smoking, whereas we analyse the extensive margin of smoking. How changes at the extensive margin translate into overall consumption changes depends on the question which type of smokers quit. Depending on the change in the number of cigarettes consumed, this would affect the overall consumption level and may lead to a deviation from the 3.9%. Nevertheless, the picture reveals a dip in consumption which we would expect from the policy.

[16]To assess whether the reform changed the trend in smoking rates, we ran an additional model regressing smoking behaviour on the event time variable, an indicator for the post-reform period, an interaction term for the two variables, and the remaining control variables. We then tested whether the interaction terms statistically differ from zero. We never reject the null hypothesis for any of the main subgroups. This suggests that the reform led to a level shift in smoking prevalence, and not to a change in the trend.

women aged 15-29 who both reduce their smoking prevalence by 2.8/2.3 percentage points, corresponding to a reduction of around 9%.

Splitting the sample by educational attainment, the results show that the reform significantly reduced the smoking probability for low-educated individuals below age 50, including men and women, but not for high-educated individuals. More specifically, the reform reduced the probability of smoking among low-educated men by around 1.8 percentage points and by 1 percentage point among low-education women. That we do not find any significant effects for highly educated women aged 15-29 (and even positive effects for men) likely relates to the small sample size for high-educated individuals in this age group, as they may still be in education. The point estimates for high-educated individuals aged 30-49 are insignificant, but show the expected sign.

An important question is what drives these heterogeneous responses – do individuals quit more often in response to the reform, or do they start to smoke less frequently? To address these two potential mechanisms, we next investigate how the reform affected the probability to quit and to start smoking, respectively. We begin our discussion with a graphical inspection for quitting smoking. Figure 3 again plots the non-parametric coefficients from equation 1 and superimposes the pre-reform trend line from the parametric model (equation 2). Figure 3 provides strong visual evidence that individuals quit at a much higher rate in the year of the reform compared to the pre-reform year, as well as compared to the subsequent years. Panel A shows that, on average, the probability of quitting increased discontinuously between 2005 and 2006 by around 4 percentage points, before almost returning to its pre-reform level again in 2007. Panels B and C show that we observe similar patterns for men and women. Panels D and E show that the effect is particularly pronounced for the youngest age groups and non-existent for the oldest group in our sample (Panel F).

We again estimate the probability to quit smoking within the parametric event study model, focusing on the effect in the first year of the reform. The results from the first three columns in Table 4 show that the reform increased the quitting probability by 3.4 percentage points on average in the year of the reform, with slightly stronger (though not statistically different) effects for women compared to men. Panel B and C show that the reform strongly increased the quitting

probability by around 7 percentage points for the youngest age group and by 3.6 percentage points for middle-aged individuals, respectively. Panel D shows that older individuals are not significantly affected.

Columns 4 to 6 of Table 4 presents the results for the probability to initiate smoking or to relapse. Overall, the reform reduced the probability to initiate/relapse by about 0.5 percentage points, though the effects are statistically not different from zero. We observe slightly stronger effects on average for men (-0.8pp) than for women (-0.2pp). The effects are largest in magnitude for the youngest age group, and Appendix Figure A.1 indeed reveals a substantial discontinuous decrease in initiation/relapsing rates for this age group. However, these differences are not statistically significantly different from zero due to the lack of statistical power.

We also investigate whether any differences emerge between low- and high-educated individuals. Table 5 shows that the main effect on quitting is largely driven by individuals with low-levels of education – high-educated individuals hardly change their quitting-behaviour following the reform. This finding is consistent with the hypothesis that the pictorial warnings can help to reduce knowledge gaps, particularly for low-educated individuals (Durkin et al., 2009). We find no statistically significant differences with respect to initiating/relapsing.

We also checked whether the probability to quit after the reform depends on the smoking intensity or the household composition. Due to the small cell sizes, we cannot sensibly perform this analysis by age groups. We start with smoking intensity, which we compute based on the number of cigarettes smoked per week in the year prior to the reform. We classify individuals as high (low) intensity smokers if they consume more (less) than the median of the distribution of smoked weekly cigarettes. Panel A of Appendix Table A.2 shows that the effect on quitting is similar in absolute terms (between 3.2pp and 3.6pp) for both low- and high-intensity smokers, although relatively larger for high-intensity smokers. We observe some gender differences across the two groups, although none of the coefficients are statistically significant from each other.

With respect to the household composition, we now distinguish between smokers who are the only smoker in a household and smokers who live with at least one other smoker in the household. We may expect that social norms and within-household bargaining between smokers

and non-smokers should increase the demands for only smokers to quit smoking, but less so for at least two smokers in the households. In accordance with these expectations, Panel B shows that the quitting effect (+5.7 pp on average) is more pronounced for the only smokers in the household compared to smokers who are not the only smoker in the household.

Taken together, our analysis shows that the reform decreased smoking rates by around 4% in the first year of the policy and led to a permanent decline in smoking rates. This decline primarily stems from increases in the quitting probability in the first year of the reform, particularly by low-educated individuals, combined with a relatively smaller (and statistically insignificant) short-term decrease in initiation/relapsing rates.

Finally, we can try to assess how much of the overall effect is mediated by the Quitline calls. Based on our main estimate, the size of the Australian population aged 15 and above in 2005 (16.6 million), and the smoking rate from the HILDA data in 2005 (22.7%), we estimate that around 150.000 individuals quit smoking because of the reform within its first year. Based on Miller et al. (2009b), the Quitline received around 165.000 calls in 2006, and 77% of callers were first-time callers. Thus, around 127.000 individuals called the Quitline in 2006. Based on earlier work by Miller et al. (2003) who use data from 1997/1998, around 30% of callers to the Quitline quit after 12 months. Hollis et al. (2007) are one of the few studies that use randomised trials to assess the effects of similar telephone counselling services. They report quit rates of around 11.7-13.8% after 12 months for counselling sessions without follow-up calls. Based on their estimates, we would expect that around 15.000-17.500 individuals quit after 12 months because of calls to the Quitline. Thus, we can estimate that between 10 to 25% of the overall effect can be explained by calls to Quitline. The Quitline therefore appears to be a relevant, though by no means dominant, mechanism.

5.2 Robustness

In this section, we examine the robustness of our results with respect to a number of potential concerns. We present the results in Table 6. Panel A reports our baseline specification for ease of comparison. First, Panel B show that the size and statistical significance of our main estimates do not depend on controlling for individual socio-economic characteristics and state-specific

control variables. We then drop all assumptions about individual-specific heterogeneity (α_i in the estimation equations) and estimate a simple pooled OLS model. The results in Panel C show that we reach the same conclusions although the estimates are, as to be expected, somewhat less precise (for the corresponding event-study graph, see Online Appendix Figure B.3). Next, we use the cross-sectional survey weights to ensure that the estimates are representative of the Australian population. The point estimates in Panel D again confirm our main findings (for the corresponding event-study graph, see Online Appendix Figure B.4). Next, instead of using the random-effects estimator, we control for individual-specific fixed-effects which difference out time-invariant unobserved heterogeneity. Importantly, the results presented in Panel E show that our main estimates do not change, which is re-assuring given that this estimator controls for time-invariant unobserved heterogeneity by only using variation within the same individual over time.

In Panel F, we control for the average real yearly cigarette price of the five top-selling brands showing that our results do not change. Thus, the policy seems to have mainly shifted the demand curve for cigarettes, with small indirect effects on prices. In Panel G, we investigate whether our results are mainly driven by occasional smokers who may react more strongly to the pictorial warnings. For this check, we exclude occasional smokers and keep only regular smokers who report to smoke daily. The coefficients reduce slightly, but the overall conclusions remain the same. This provides further evidence that the pictures do not just get occasional smokers to quit, but also appeal to regular smokers.[17]

The validity of our identification strategy rests on the correct specification of the pre-reform time trend, which depends on the size of the observation window and the functional form for the time trend. We therefore perform two robustness checks: First, we adjust the observation window around the reform date and use a shorter window ranging from 2003 to 2008 in Panel H. Our main conclusions do not change alleviating any concerns that the chosen time frame may affect our analysis. Second, we also use a quadratic specification for the time trend. Given that we can estimate the pre-reform time trend on only four data points, the quadratic specification is not our preferred specification due to the increased risk of overfitting the data at the boundaries.

[17]We also performed the main analysis splitting the sample by the smoking intensity at the previous wave. However, the sample size gets too small for a meaningful analysis.

Nevertheless, the point estimates presented in Panel I confirm our general conclusions, despite considerably noisier estimates.

We also estimate the effects of a placebo reform, pretending the reform was implemented one year prior to its actual implementation date. This helps to check if anything changed in the year before the reform, which may indicate some confounding trends. The results presented in Panel J show that one out of the twelve estimates is significant (for females aged 50 and above) which is likely due to statistical chance.

We also ran all robustness checks on the samples split by educational attainment. Online Appendix Tables B.4 and B.5 show that our main conclusions are robust for both subgroups.

6 Conclusion and discussion

This paper studies how the introduction of pictorial warnings on tobacco products in Australia was associated with smoking behaviour. Our paper adds three important contributions to the previous literature. First, we use a transparent research design which controls for secular time trends in smoking prevalence to estimate how smoking behaviour changed with the reform. We show that observable socio-economic characteristics do not change discontinuously with the introduction of the reform. During this period, other relevant tobacco control policies (cigarette taxes, advertising of tobacco products, legal minimum vending age) did not change and thus cannot confound our estimated treatment effect. Second, we use longitudinal and nationally representative household data; our estimation sample consists of 103,007 person-year observations. Third, this large sample size allows us to investigate potentially important heterogeneities by age, gender, and educational background. Our study combines a longitudinal cohort design with a natural experiment to offer a high degree of internal validity (Fong et al., 2006).

Our results show that the introduction of pictorial warnings in Australia was associated with a significant decrease in smoking prevalence of about 0.9 percentage points (around 4%) within the first year after the introduction of the policy. The reform effect i) decreases with age and is strongest for individuals below age 30, ii) is similar for women and men, and iii) is slightly larger for low-educated compared to high-educated individuals. Exploiting the longitudinal study design, we show that the policy led to a short-term increase in the probability to quit

smoking, and reduced the probability to relapse or initiate smoking in the year of the reform (though not significant). The results for the short-term effects on quitting are consistent with the previous literature that has documented a wearing-out effect (Miller et al., 2011). The results are robust to numerous sensitivity checks and a placebo reform that takes place one year earlier.

Our estimates also allow us to perform a simple back-of-the-envelope calculation to address the cost savings of the policy. One report for the Australian Department of Health and Ageing (Collins and Lapsley, 2008) estimated total costs of smoking of $AUD 31.5 billion in 2004–05, the year before the reform. They split the total societal costs into tangible costs (including productivity losses, additional health care expenses, fires caused by smoking), which amount to $AUD 12 billion per year. The non-tangible costs amount to $AUD 19.5 billion (62% of the total), but only include the costs of premature death excluding all other non-tangible societal costs. Assuming that these costs are evenly caused by all smokers, our estimates for smoking prevalence suggest a reduction in the total societal costs of about 4%, corresponding to cost-savings to society of at least 1.26$ AUD billion (around 0.18% of nominal GDP in 2005). Although these cost-savings are very rough, they likely underestimate the true cost-savings since many non-tangible positive externalities of the policy are not accounted for, including additional benefits for non-smokers from less second hand smoking and lower psychological costs for smokers due to fewer smoking-related illnesses. Finally, additional benefits may also materialise in future generations due to the intergenerational correlation of smoking status; thus, the policy may also indirectly lower the smoking prevalence among future generations.

One limitation of our study is that we cannot disentangle the separate contributions of the pictorial warnings, the more prominent Quitline reference, and the mass media campaigns. We estimate that between 10% to 25% of the overall effect is mediated by calls to the Quitline, but we cannot reliably estimate the relative contribution of the supportive television commercials. Overall, we therefore conclude that pictorial warnings on cigarette packages, supported by a direct reference to a smoking cessation hotline and targeted television commercials, are an effective means to reduce smoking rates.

Another important limitation of our paper is that we are unable to measure the long-run effect of the reform due to changes in macroeconomic conditions and other tobacco control

policies, such as smoking bans and plain packaging, in subsequent years. Our research design is therefore only able to identify the short-run impact of the pictorial warnings. Finally, despite our large sample, the low proportion of quitters and new smokers substantially reduces the power of our analysis for these outcomes. This finding highlights the point that future research examining quitting and initiating/relapsing behaviour should strive to use data from large data sources to increase the precision of the analysis.

Given the current legal battle in the US between the Food and Drug Administration (FDA), which intended to implement graphic warnings in the US in 2011, and the tobacco industry, which keeps challenging the evidence for the effectiveness of warning labels, the present study has made an important advance to examine how such warning labels affect smoking behaviour. Future research needs to continue this strand of research, collecting population-wide evidence on smoking outcomes, quitting and smoking initiating behaviour, to contribute more reliable evidence from different countries and contexts.

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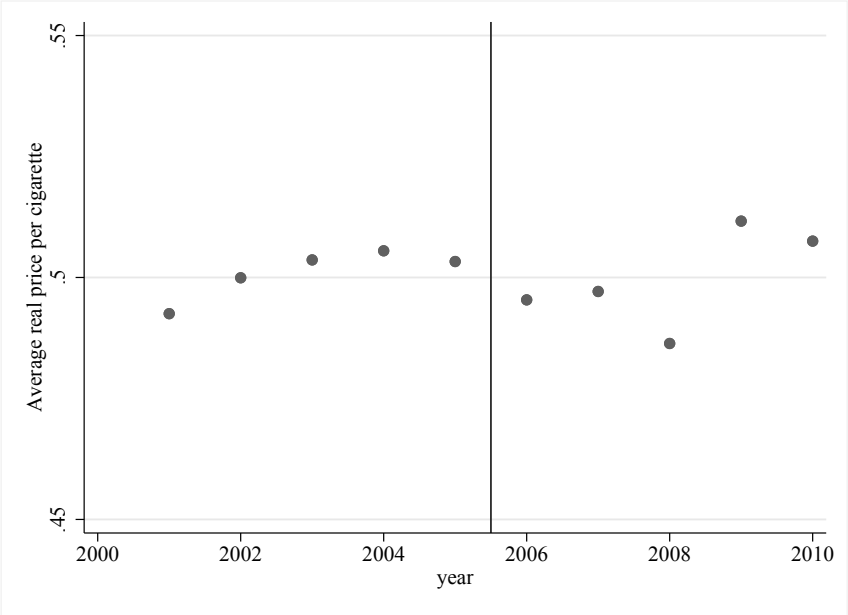
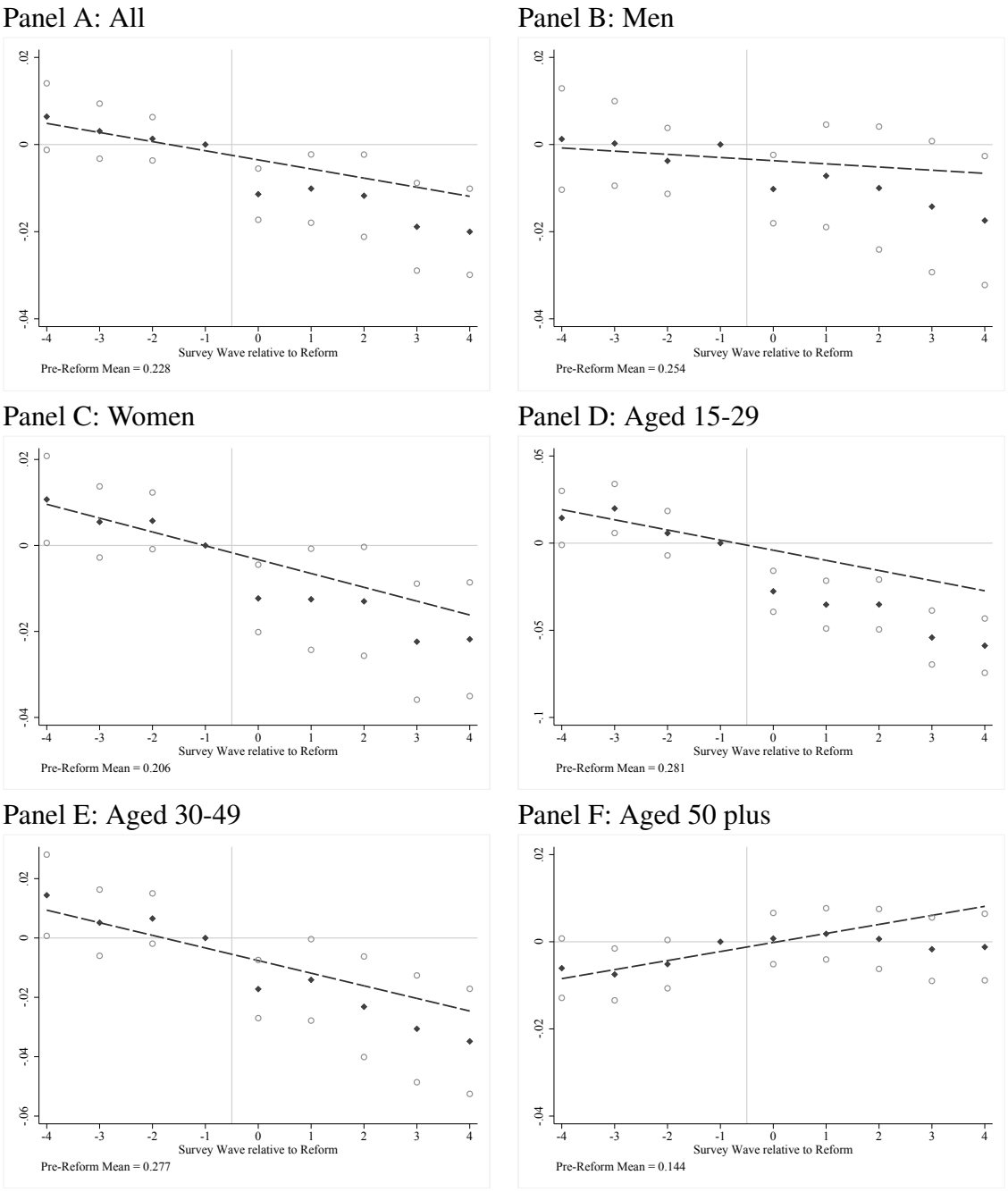
7 Tables and Figures

Figure 1: Average real retail price per cigarette, by year



Notes: This figure shows the average real retail price per cigarette based on the first five top-selling brands in Australia for the period of our analysis.
Source: Table 13.3.2 from `http://www.tobaccoinaustralia.org.au/13-3-the-price-of-tobacco-products-in-australia`, adjusted for inflation. Last accessed 19/10/2018.

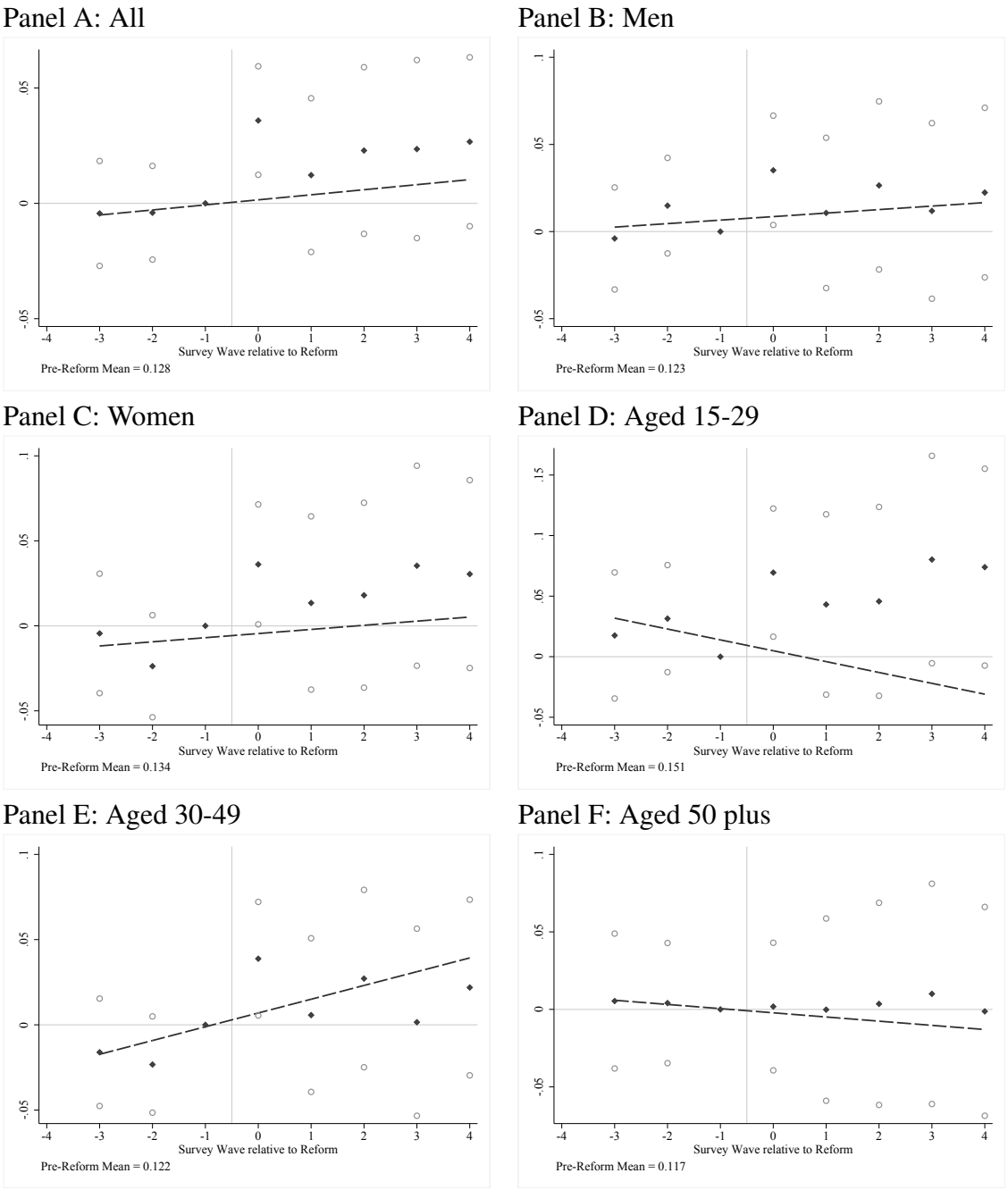
Figure 2: Impact of the pictorial warnings on smoking rates



Notes: The diamonds in each figure represent the estimated effects of event time (i.e., the μ_ts from the nonparametric event study in equation 1). The introduction of the reform occurs in year zero. The hollow circles present the 95 percent confidence intervals. The dashed line represents the estimated pre-reform linear relationship between smoking and event time from the parametric event study in equation 2 with the level normalised to match the nonparametric estimates. All estimates based on random effects estimation using all covariates listed in Table 1.

Source: Own calculations based on HILDA, V15, survey years 2002-2010.

Figure 3: Impact of the pictorial warnings on quitting



Notes: The diamonds in each figure represent the estimated effects of event time (i.e., the μ_ts from the nonparametric event study in equation 1). The introduction of the reform occurs in year zero. The hollow circles present the 95 percent confidence intervals. The dashed line represents the estimated pre-reform linear relationship between quitting and event time from the parametric event study in equation 2 with the level normalised to match the nonparametric estimates. All estimates based on OLS estimation using all covariates listed in Table 1. The estimation sample includes individuals interviewed in consecutive years between 2002 and 2010 (to construct the change in the outcome variable).

Source: Own calculations based on HILDA, V15, survey years 2002-2010.

Table 1: Covariate balancing.

	mean 2005	diff	se	
Panel A: Individual characteristics				
Age	44.301	0.060	(0.119)	
Female	0.535	-0.002	(0.003)	
Employed full-time	0.431	-0.003	(0.004)	
Employed part-time	0.218	-0.001	(0.004)	
Unemployed	0.029	0.004	(0.002)	
Not in labour force, marginally	0.059	0.002	(0.003)	
Not in labour force	0.263	-0.002	(0.004)	
Casual	0.143	0.006	(0.004)	
Permanent	0.394	-0.010	(0.004)	**
Type missing	0.463	0.005	(0.004)	
Married	0.509	0.008	(0.003)	**
De facto	0.118	-0.001	(0.003)	
Separated	0.028	-0.002	(0.002)	
Divorced	0.062	-0.001	(0.002)	
Widowed	0.051	-0.000	(0.001)	
Single	0.231	-0.004	(0.003)	
Non-Australian origin	0.210	-0.000	(0.002)	
Aboriginal/Torrest Strait Islander	0.018	0.002	(0.001)	*
New South Wales	0.299	-0.009	(0.003)	***
Victoria	0.242	0.005	(0.003)	*
Queensland	0.210	0.002	(0.003)	
South Australia	0.093	0.002	(0.002)	
Western Australia	0.097	0.004	(0.002)	**
Tasmania	0.034	-0.002	(0.001)	*
Northern Territory	0.006	-0.000	(0.001)	
Australian Capital Territory	0.020	-0.000	(0.001)	
Postgraduate degree	0.031	-0.001	(0.001)	
Graduate diploma	0.050	-0.001	(0.001)	
Bachelor degree	0.128	-0.003	(0.002)	
Diploma	0.088	-0.001	(0.002)	
Certificate level III and IV	0.189	-0.003	(0.003)	
Finished year 12	0.145	-0.004	(0.003)	
Finished year 11 or less	0.369	0.012	(0.003)	***
Union member	0.167	-0.004	(0.003)	
Panel B: Federal state characteristics				
Smoking ban in enclosed places	0.000	0.343	(0.219)	
Unemployment rate (0-100%)	4.995	0.095	(0.240)	
State real GDP per capita	52560.683	303.165	(826.622)	
N		103,007		
Joint F-test for 2005/06 (N=22,887)			p=0.9582	

Source: Own calculations in Panel A based on HILDA, V15, survey years 2002-2010. Panel B based on data from the Australian Bureau of Statistics.
Notes: Column (1) presents the pre-reform mean value for 2005, column (2) the coefficients for the μ_0s as in equation 3, i.e., the reform year, from a regression of the socio-economic characteristics on event time. For categorical variables we left out the missing category. Standard errors printed in parentheses and clustered at the individual level in Panel A and at the state level in Panel B. The joint F-test is based on a regression of the post-reform dummy on all covariates for the survey years 2005 and 2006. *** p<0.01, ** p<0.05, * p<0.1

Table 2: Impact of graphic warning labels on smoking.

	All	Men	Women	Aged 15-29	Aged 30-49	Aged 50 plus
	(1)	(2)	(3)	(4)	(5)	(6)
6-month effect	-0.009***	-0.009**	-0.009**	-0.026***	-0.013**	0.003
	(0.003)	(0.005)	(0.004)	(0.008)	(0.005)	(0.003)
18-month effect	-0.006	-0.004	-0.007	-0.031**	-0.006	0.008
	(0.005)	(0.007)	(0.006)	(0.013)	(0.008)	(0.005)
30-month effect	-0.005	-0.006	-0.004	-0.025	-0.011	0.006
	(0.006)	(0.008)	(0.007)	(0.015)	(0.010)	(0.007)
42-month effect	-0.010	-0.008	-0.012	-0.035*	-0.015	0.000
	(0.007)	(0.011)	(0.010)	(0.020)	(0.013)	(0.009)
54-month effect	-0.009	-0.011	-0.008	-0.037*	-0.014	0.000
	(0.008)	(0.012)	(0.011)	(0.022)	(0.014)	(0.010)
Pre-reform mean	0.227	0.252	0.206	0.281	0.276	0.143
N	103007	48194	54813	25554	38399	39054

Notes: This table reports the results from random effects regressions of pictorial warnings on tobacco products on smoking prevalence based on equation 2. Control variables as listed in Table 1. Standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1. Pre-reform mean calculated for survey year 2005.
Source: Own calculations based on HILDA, V15, survey years 2002-2010.

Table 3: Heterogeneities - effect of graphic warning labels on smoking.

	All			Low educated			High educated		
	All	Males	Females	All	Males	Females	All	Males	Females
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: All	-0.009***	-0.009**	-0.009**	-0.013***	-0.018**	-0.010*	-0.006	-0.009	-0.005
	(0.003)	(0.005)	(0.004)	(0.004)	(0.008)	(0.005)	(0.005)	(0.007)	(0.006)
N	103007	48194	54813	52948	21719	31229	30429	13767	16662
Pre-reform mean	0.227	0.252	0.206	0.255	0.292	0.229	0.142	0.159	0.128
% effect size	-0.040	-0.036	-0.044	-0.051	-0.062	-0.044	-0.042	-0.057	-0.039
Panel B: Aged 15-29	-0.026***	-0.028**	-0.023**	-0.028***	-0.034**	-0.023*	-0.006	0.009	-0.017
	(0.008)	(0.013)	(0.010)	(0.010)	(0.016)	(0.014)	(0.015)	(0.025)	(0.019)
N	25554	12136	13418	17055	8358	8697	4868	1870	2998
Pre-reform mean	0.281	0.294	0.269	0.295	0.299	0.291	0.164	0.162	0.165
% effect size	-0.093	-0.095	-0.086	-0.095	-0.114	-0.079	-0.037	0.056	-0.103
Panel C: Aged 30-49	-0.013**	-0.010	-0.016**	-0.034***	-0.045***	-0.028**	-0.008	-0.013	-0.005
	(0.005)	(0.008)	(0.007)	(0.009)	(0.015)	(0.012)	(0.007)	(0.011)	(0.008)
N	38399	17813	20586	14788	5756	9032	15036	6409	8627
Pre-reform mean	0.276	0.314	0.243	0.362	0.426	0.323	0.160	0.197	0.132
% effect size	-0.047	-0.032	-0.066	-0.094	-0.106	-0.087	-0.050	-0.066	-0.038
Panel D: Aged 50 plus	0.003	0.003	0.003	0.008*	0.010	0.007	-0.003	-0.005	0.000
	(0.003)	(0.005)	(0.005)	(0.005)	(0.008)	(0.006)	(0.006)	(0.008)	(0.009)
N	39054	18245	20809	21105	7605	13500	10525	5488	5037
Pre-reform mean	0.143	0.162	0.126	0.146	0.180	0.127	0.104	0.109	0.098
% effect size	0.021	0.019	0.024	0.055	0.056	0.055	-0.029	-0.046	0.000

Notes: This table reports the results for the short-term effect (i.e., the μ_0s) from the random effects regressions of pictorial warnings on tobacco products on smoking prevalence based on equation 2. Control variables as listed in Table 1. Standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1. Pre-reform means calculated for survey year 2005.

Source: Own calculations based on HILDA, V15, survey years 2002-2010.

Table 4: Mechanisms - effect of graphic warning labels on quitting and initiating/relapsing.

	Quitting			Initiating/Relapsing		
	All	Males	Females	All	Males	Females
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All	0.034**	0.027	0.041*	-0.005	-0.008	-0.002
	(0.014)	(0.019)	(0.021)	(0.004)	(0.006)	(0.005)
N	16395	8549	7846	62435	28056	34379
Pre-reform mean	0.124	0.110	0.139	0.034	0.041	0.028
% effect size	0.274	0.245	0.295	-0.147	-0.195	-0.071
Panel B: Aged 15-29	0.070**	0.095**	0.046	-0.008	0.004	-0.014
	(0.032)	(0.046)	(0.044)	(0.012)	(0.019)	(0.015)
N	4111	2059	2052	12686	5769	6917
Pre-reform mean	0.125	0.122	0.127	0.073	0.087	0.061
% effect size	0.560	0.779	0.362	-0.110	0.046	-0.230
Panel C: Aged 30-49	0.036*	0.036	0.038	0.000	-0.012	0.009
	(0.020)	(0.026)	(0.032)	(0.006)	(0.010)	(0.008)
N	7844	4055	3789	22035	9659	12376
Pre-reform mean	0.130	0.101	0.159	0.032	0.041	0.024
% effect size	0.277	0.356	0.239	0.000	-0.293	0.375
Panel D: Aged 50 plus	0.004	-0.036	0.052	-0.007*	-0.009	-0.006
	(0.025)	(0.033)	(0.039)	(0.004)	(0.006)	(0.005)
N	4440	2435	2005	27714	12628	15086
Pre-reform mean	0.113	0.116	0.110	0.018	0.020	0.017
% effect size	0.035	-0.310	0.473	-0.389	-0.450	-0.353

Notes: This table reports the results for the short-term effect (i.e., the μ_0s) from pooled OLS regressions of pictorial warnings on tobacco products on quitting smoking initiation between waves based on equation 2 (without the individual-specific heterogeneity, α_i). Control variables as listed in Table 1. The estimation sample includes individuals interviewed in consecutive years between 2002 and 2010 (to construct the change in the outcome variable). Standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

[a] Pre-reform mean calculated for survey year 2005.

Source: Own calculations based on HILDA, V15, survey years 2002-2010.

Table 5: Quitting and initiating/relapsing, by educational attainment.

	Quitting			Initiating/Relapsing		
	All	Males	Females	All	Males	Females
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: SES - low education	0.047**	0.034	0.060**	-0.003	-0.010	0.001
	(0.019)	(0.026)	(0.026)	(0.006)	(0.011)	(0.006)
N	9156	4335	4821	29949	11380	18569
Pre-reform mean	0.112	0.109	0.114	0.040	0.057	0.029
% effect size	0.420	0.312	0.526	-0.075	-0.175	0.034
Panel B: SES - high education	0.005	0.003	-0.011	-0.004	-0.006	-0.003
	(0.036)	(0.047)	(0.052)	(0.005)	(0.008)	(0.007)
N	3199	1582	1617	21243	9474	11769
Pre-reform mean	0.183	0.136	0.225	0.022	0.023	0.021
% effect size	0.027	0.022	-0.049	-0.182	-0.261	-0.143

Notes: This table reports the results for the short-term effect (i.e., the μ_0s) from pooled OLS regressions of pictorial warnings on tobacco products on quitting smoking initiation between waves based on equation 2 (without the individual-specific heterogeneity, α_i). Control variables as listed in Table 1. The estimation sample includes individuals interviewed in consecutive years between 2002 and 2010 (to construct the change in the outcome variable). Standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

[a] Pre-reform mean calculated for survey year 2005.

Source: Own calculations based on HILDA, V15, survey years 2002-2010.

Table 6: Robustness checks.

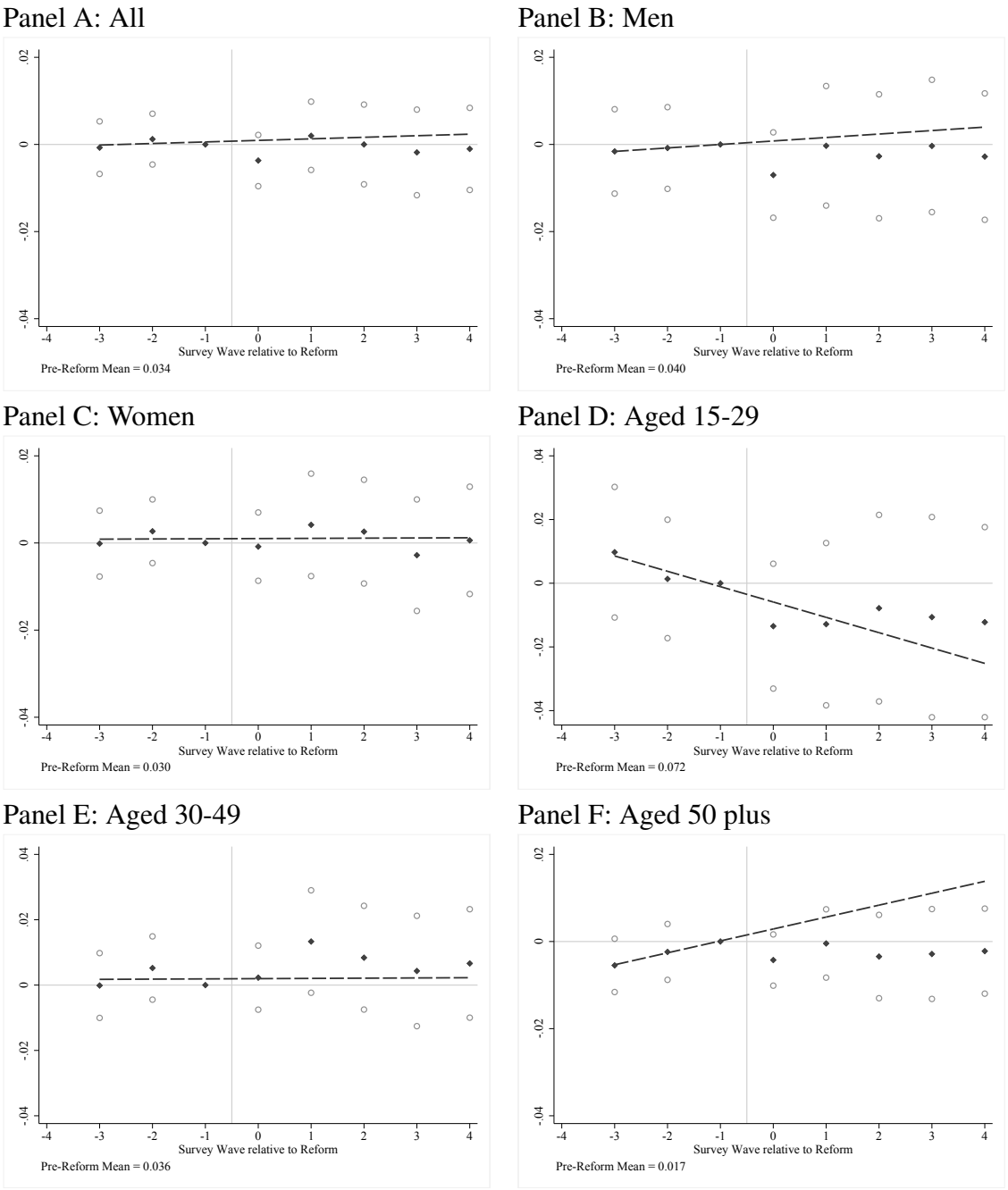
	All				Males				Females			
	Age group				Age group				Age group			
	All	15-29	30-49	50+	All	15-29	30-49	50+	All	15-29	30-49	50+
Panel A: Baseline model												
6-month effect	-0.009***	-0.026***	-0.013**	0.003	-0.009**	-0.028**	-0.010	0.003	-0.009**	-0.023**	-0.016**	0.003
	(0.003)	(0.008)	(0.005)	(0.003)	(0.005)	(0.013)	(0.008)	(0.005)	(0.004)	(0.010)	(0.007)	(0.005)
N	103007	25554	38399	39054	48194	12136	17813	18245	54813	13418	20586	20809
Panel B: Baseline model, no control variables												
6-month effect	-0.010***	-0.019**	-0.014***	-0.000	-0.010**	-0.020*	-0.012*	0.001	-0.010***	-0.018*	-0.015**	-0.001
	(0.003)	(0.007)	(0.005)	(0.003)	(0.004)	(0.012)	(0.007)	(0.005)	(0.004)	(0.009)	(0.006)	(0.004)
N	103007	25554	38399	39054	48194	12136	17813	18245	54813	13418	20586	20809
Panel C: Baseline model, pooled OLS												
6-month effect	-0.011***	-0.020**	-0.014**	-0.001	-0.010*	-0.016	-0.018*	0.001	-0.011**	-0.025**	-0.010	-0.004
	(0.004)	(0.010)	(0.006)	(0.004)	(0.005)	(0.015)	(0.009)	(0.007)	(0.005)	(0.012)	(0.008)	(0.006)
N	103007	25554	38399	39054	48194	12136	17813	18245	54813	13418	20586	20809
Panel D: Baseline model, pooled OLS using cross-sectional survey weights												
6-month effect	-0.018***	-0.035***	-0.025***	-0.003	-0.021**	-0.023	-0.027*	-0.011	-0.015**	-0.046***	-0.022*	0.003
	(0.005)	(0.013)	(0.010)	(0.007)	(0.009)	(0.021)	(0.015)	(0.011)	(0.006)	(0.016)	(0.012)	(0.008)
N	102432	25390	38196	38846	47919	12036	17720	18163	54513	13354	20476	20683
Panel E: Baseline model, controlling for individual fixed-effects												
6-month effect	-0.008***	-0.022***	-0.013**	0.004	-0.009*	-0.027**	-0.010	0.004	-0.008**	-0.018*	-0.015**	0.003
	(0.003)	(0.008)	(0.005)	(0.003)	(0.005)	(0.013)	(0.008)	(0.005)	(0.004)	(0.010)	(0.007)	(0.005)
N	103007	25554	38399	39054	48194	12136	17813	18245	54813	13418	20586	20809
Panel F: Baseline model, including controls for cigarette prices												
6-month effect	-0.010***	-0.033***	-0.014**	0.005	-0.010**	-0.033**	-0.009	0.002	-0.011**	-0.033***	-0.019**	0.007
	(0.003)	(0.009)	(0.006)	(0.004)	(0.005)	(0.014)	(0.008)	(0.006)	(0.004)	(0.011)	(0.008)	(0.005)
N	103007	25554	38399	39054	48194	12136	17813	18245	54813	13418	20586	20809
Panel G: Baseline model, excluding occasional smokers												
6-month effect	-0.006**	-0.019***	-0.008*	0.004	-0.003	-0.019*	-0.003	0.005	-0.008**	-0.019**	-0.012**	0.002
	(0.002)	(0.007)	(0.004)	(0.003)	(0.004)	(0.010)	(0.006)	(0.005)	(0.003)	(0.009)	(0.006)	(0.004)
N	99054	23875	36801	38378	46164	11282	16960	17922	52890	12593	19841	20456
Panel H: Baseline model, shorter observation window (2003 - 2008)												
6-month effect	-0.009***	-0.019**	-0.016***	0.001	-0.008	-0.018	-0.017**	0.003	-0.010**	-0.020*	-0.016**	-0.001
	(0.003)	(0.009)	(0.006)	(0.004)	(0.005)	(0.014)	(0.008)	(0.005)	(0.004)	(0.011)	(0.008)	(0.005)
N	67990	16697	25697	25596	31760	7913	11906	11941	36230	8784	13791	13655
Panel I: Baseline model, controlling for quadratic time trend												
6-month effect	-0.011**	-0.013	-0.017**	-0.004	-0.014*	-0.014	-0.025**	-0.000	-0.009	-0.012	-0.010	-0.007
	(0.005)	(0.014)	(0.008)	(0.006)	(0.007)	(0.021)	(0.012)	(0.008)	(0.007)	(0.017)	(0.011)	(0.007)
N	103007	25554	38399	39054	48194	12136	17813	18245	54813	13418	20586	20809
Panel J: Baseline model, placebo reform one year earlier												
6-month effect	0.003	0.006	0.003	0.003	0.003	0.012	0.006	-0.004	0.003	-0.002	0.000	0.009**
	(0.003)	(0.007)	(0.004)	(0.003)	(0.004)	(0.011)	(0.007)	(0.005)	(0.004)	(0.009)	(0.006)	(0.004)
N	103007	25554	38399	39054	48194	12136	17813	18245	54813	13418	20586	20809

Notes: This table reports the results for the short-term effect (i.e., the μ_0s) from random effects regressions of pictorial warnings on tobacco products on smoking prevalence based on equation 2 (i.e., the μ_0s). Control variables as listed in Table 1. The sample size is always the same as in the baseline specification (Panel A), unless otherwise shown. Control variables as listed in Table 1. Standard errors clustered at the individual level.

*** p<0.01, ** p<0.05, * p<0.1 *Source:* Own calculations based on HILDA, V15.

Appendix A: Supplementary material

Figure A.1: Impact of the pictorial warnings on smoking initiation/relapsing



Notes: The diamonds in each figure represent the estimated effects of event time (i.e., the μ_ts from the nonparametric event study in equation 1). The introduction of the reform occurs in year zero. The hollow circles present the 95 percent confidence intervals. The dashed line represents the estimated pre-reform linear relationship between smoking initiation (between waves) and event time from the parametric event study in equation 2 with the level normalised to match the nonparametric estimates. All estimates based on OLS estimation using all covariates listed in Table 1. The estimation sample includes individuals interviewed in consecutive years between 2002 and 2010 (to construct the change in the outcome variable).

Source: Own calculations based on HILDA, V15, survey years 2002-2010.

Table A.1: Descriptive statistics.

	Sample					
	All		2002-2005		2006-2010	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Panel A: Individual characteristics						
Current smoker	.218	.413	.234	.423	.205	.404
Age	44.393	18.199	44.141	17.809	44.595	18.504
Female	.532	.499	.53	.499	.533	.499
Employment status						
Employed full-time	.43	.495	.425	.494	.434	.496
Employed part-time	.212	.409	.21	.407	.214	.41
Unemployed	.033	.179	.034	.18	.033	.178
Not in labour force, marginally	.06	.238	.066	.248	.056	.23
Not in labour force	.264	.441	.265	.441	.263	.44
Working type						
Casual	.141	.348	.141	.348	.141	.348
Permanent	.395	.489	.381	.486	.407	.491
Missing	.463	.499	.478	.5	.452	.498
Marital status						
Married	.505	.5	.521	.5	.493	.5
De facto	.124	.33	.11	.313	.136	.343
Separated	.028	.164	.031	.173	.025	.157
Divorced	.061	.24	.061	.239	.061	.24
Widowed	.05	.219	.05	.217	.051	.22
Single	.231	.422	.228	.419	.234	.423
Non-Australian origin	.207	.405	.217	.412	.199	.4
Aboriginal/Torrest Strait Islander	.019	.138	.017	.131	.021	.143
State						
New South Wales	.295	.456	.299	.458	.291	.454
Victoria	.247	.431	.248	.432	.246	.43
Queensland	.209	.406	.202	.401	.214	.41
South Australia	.094	.292	.095	.293	.093	.291
Western Australia	.097	.296	.098	.298	.096	.295
Tasmania	.032	.177	.033	.177	.032	.176
Northern Territory	.007	.081	.006	.076	.007	.085
Australian Capital Territory	.02	.14	.019	.137	.021	.143
Highest level of education						
Postgraduate degree	.033	.179	.029	.168	.036	.187
Graduate diploma	.05	.218	.047	.212	.053	.223
Bachelor degree	.126	.332	.122	.327	.129	.335
Diploma	.086	.281	.085	.279	.088	.283
Certificate level III and IV	.191	.393	.184	.388	.196	.397
Finished year 12	.149	.356	.145	.352	.152	.359
Finished year 11 or less	.365	.481	.388	.487	.347	.476
Union member	.161	.368	.165	.371	.158	.365
Panel B: Federal state characteristics						
State smoking ban (in enclosed places)	.467	.499	0	0	.841	.365
State unemployment rate (0-100%)	5.167	.904	5.636	.792	4.79	.807
State Real GDP per capita	53871.62	7258.674	49809.09	4726.621	57134.46	7291.155
N	103007		45881		57126	

Source: Own calculations in Panel A based on HILDA, V15, survey years 2002-2010. Panel B based on data from the Australian Bureau of Statistics.

Table A.2: Mechanisms - effect of graphic warning labels on quitting by smoking intensity.

Panel A:	Low-intensity smokers			High-intensity smokers		
	All	Males	Females	All	Males	Females
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All	0.036	0.047	0.021	0.032**	0.016	0.051**
	(0.023)	(0.033)	(0.033)	(0.016)	(0.021)	(0.026)
N	7764	3735	4029	8163	4572	3591
Pre-reform mean	0.176	0.155	0.194	0.069	0.067	0.072
% effect size	0.205	0.303	0.108	0.464	0.239	0.708

Panel B:	Only smoker in household			At least two smokers in household		
	All	Males	Females	All	Males	Females
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All	0.057**	0.051*	0.062*	0.033	0.015	0.050
	(0.023)	(0.031)	(0.034)	(0.023)	(0.031)	(0.033)
N	6785	3526	3259	6424	3217	3207
Pre-reform mean	0.133	0.121	0.145	0.121	0.110	0.132
% effect size	0.429	0.421	0.428	0.273	0.136	0.379

Notes:This table reports the results for the short-term effect (i.e., the μ_0s) from pooled OLS regressions of pictorial warnings on tobacco products on quitting smoking based on equation 2 (without the individual-specific heterogeneity, α_i). Smoking intensity and and smoking status of household members are both defined based on variables from the year before the reform (see Section 5). Control variables as listed in Table 1. The estimation sample includes individuals interviewed in consecutive years between 2002 and 2010 (to construct the change in the outcome variable). Standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

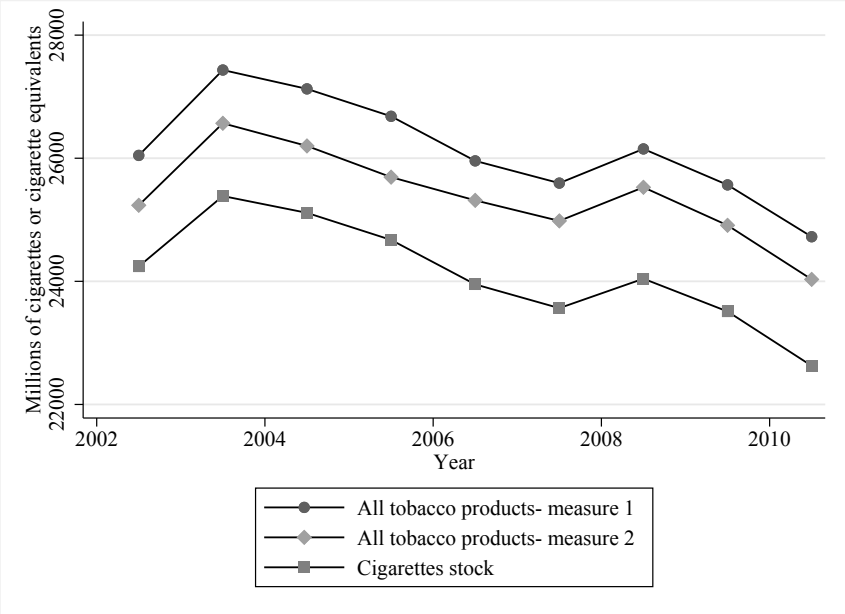
[a] Pre-reform mean calculated for survey year 2005.

Source: Own calculations based on HILDA, V15, survey years 2002-2010.

Online Appendix: Supplementary material

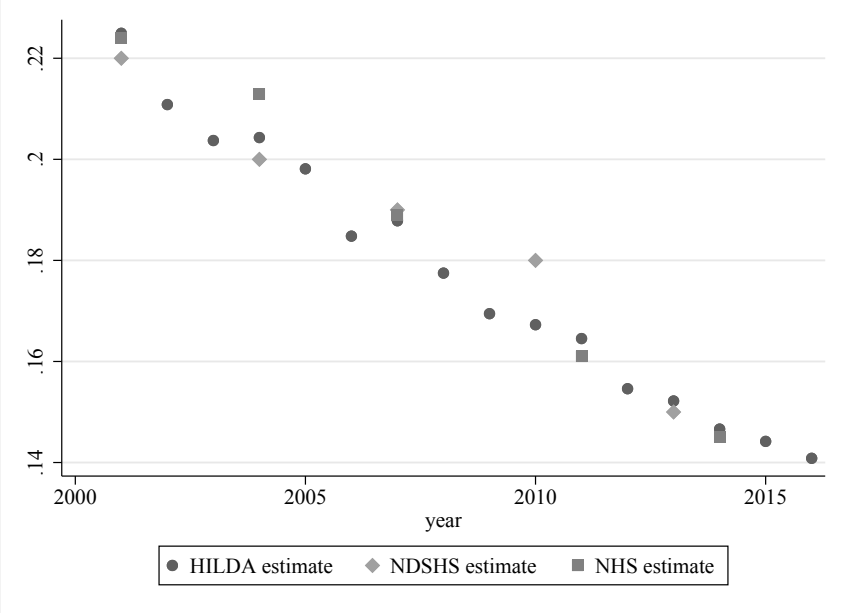
Supplementary material for Kuehnle, D. (2019): "How effective are pictorial warnings on tobacco products? New evidence on smoking behaviour using Australian panel data."

Figure B.1: Development of tobacco consumption over observation period



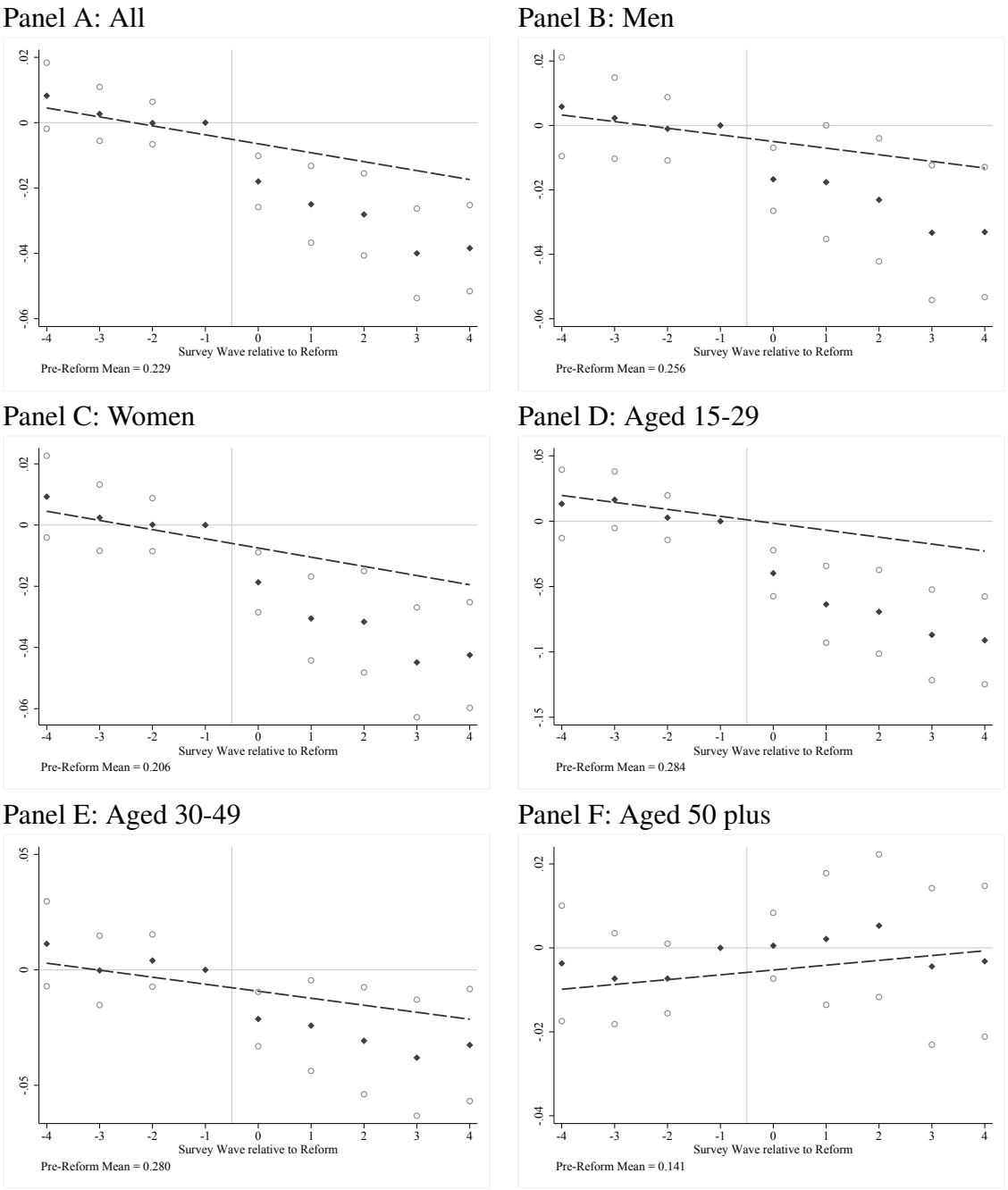
Notes: This figure shows different estimates for tobacco consumption in Australia. All estimates take into account both official tax receipts and the best available estimates of counterfeit and smuggled cigarettes in Australia. Measure 1 uses the Euromonitor estimate of contraband cigarettes, measure 2 uses a different measure developed by the Cancer Council Victoria.
Source: Data taken from `https://www.tobaccoinaustralia.org.au/chapter-2-consumption/2-9-best-estimate-of-recent-tobacco-consumption-in`, last accessed 19/10/2018.

Figure B.2: Comparing different estimates for smoking prevalence in Australia



Notes: This figure shows estimates for smoking prevalence in Australia for three different surveys: the HILDA (Household, Income and Labour Dynamics in Australia) survey; the National Drug Strategy Household Survey (NDSHS); and the National Health Survey (NHS).
Source: NDSHS and NHS data points taken from `https://http://www.tobaccoinaustralia.org.au/chapter-1-prevalence/1-3-prevalence-of-smoking-adults`, last accessed 19/10/2018.

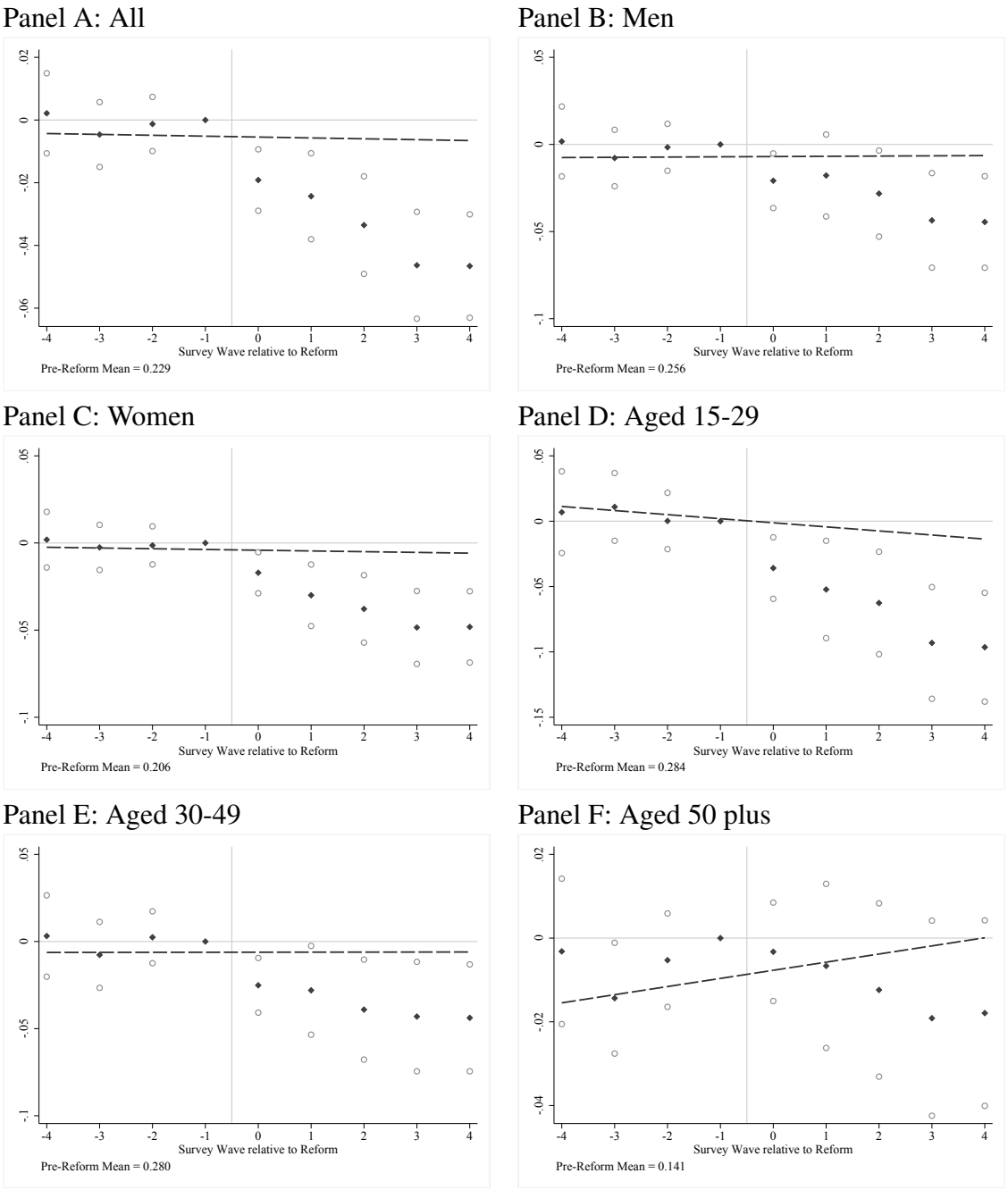
Figure B.3: Impact of the pictorial warnings on smoking (OLS)



Notes: The diamonds in each figure represent the estimated effects of event time (i.e., the μ_ts from the nonparametric event study in equation 1). The introduction of the reform occurs in year zero. The hollow circles present the 95 percent confidence intervals. The dashed line represents the estimated pre-reform linear relationship between smoking and event time from the parametric event study in equation 2 with the level normalised to match the nonparametric estimates. All estimates based on OLS estimation using all covariates listed in Table 1. The estimation sample includes individuals interviewed in consecutive years between 2002 and 2010 (to construct the change in the outcome variable).

Source: Own calculations based on HILDA, V15, survey years 2002-2010.

Figure B.4: Impact of the pictorial warnings on smoking (weighted OLS)



Notes: The diamonds in each figure represent the estimated effects of event time (i.e., the μ_ts from the nonparametric event study in equation 1). The introduction of the reform occurs in year zero. The hollow circles present the 95 percent confidence intervals. The dashed line represents the estimated pre-reform linear relationship between smoking and event time from the parametric event study in equation 2 with the level normalised to match the nonparametric estimates. All estimates based on OLS estimation using the cross-sectional survey weights and all covariates listed in Table 1. The estimation sample includes individuals interviewed in consecutive years between 2002 and 2010 (to construct the change in the outcome variable).
Source: Own calculations based on HILDA, V15, survey years 2002-2010.

Table B.1: Descriptives: covariate balancing, 2002-2010. Women only.

	mean 2005	diff	se	
Panel A: Individual characteristics				
Age	44.393	0.182	(0.164)	
Employed full-time	0.289	0.005	(0.006)	
Employed part-time	0.301	-0.009	(0.007)	
Unemployed	0.027	0.004	(0.003)	
Not in labour force, marginally	0.070	-0.001	(0.005)	
Not in labour force	0.312	0.001	(0.006)	
Casual	0.161	0.005	(0.006)	
Permanent	0.355	-0.007	(0.006)	
Type missing	0.485	0.003	(0.006)	
Married	0.490	0.008	(0.005)	*
De facto	0.115	0.001	(0.004)	
Separated	0.030	-0.002	(0.002)	
Divorced	0.075	-0.003	(0.002)	
Widowed	0.077	0.002	(0.002)	
Single	0.213	-0.005	(0.005)	
Non-Australian origin	0.207	0.002	(0.003)	
Aboriginal/Torrest Strait Islander	0.020	0.001	(0.002)	
New South Wales	0.304	-0.010	(0.004)	***
Victoria	0.240	0.006	(0.004)	
Queensland	0.210	0.002	(0.003)	
South Australia	0.094	0.000	(0.002)	
Western Australia	0.093	0.003	(0.002)	
Tasmania	0.034	-0.001	(0.001)	
Northern Territory	0.007	-0.001	(0.001)	
Australian Capital Territory	0.020	0.000	(0.001)	
Postgraduate degree	0.026	-0.000	(0.001)	
Graduate diploma	0.056	-0.001	(0.002)	
Bachelor degree	0.135	-0.000	(0.003)	
Diploma	0.086	-0.001	(0.002)	
Certificate level III and IV	0.124	-0.004	(0.003)	
Finished year 12	0.152	-0.003	(0.004)	
Finished year 11 or less	0.422	0.009	(0.004)	**
Union member	0.149	0.000	(0.004)	
Panel B: Federal state characteristics				
Smoking ban in enclosed places	0.000	0.340	(0.219)	
Unemployment rate (0-100%)	4.999	0.096	(0.240)	
State real GDP per capita	52532.312	278.875	(815.851)	
N	54,813			
Joint F-test for 2005/06 (N=12,219)			p=0.9803	

Source: Own calculations in Panel A based on HILDA, V15, survey years 2002-2010. Panel B based on data from the Australian Bureau of Statistics.
Notes: Column (1) presents the pre-reform mean value for 2005, column (2) the coefficients for the μ_0s as in equation 3, i.e., the reform year, from a regression of the socio-economic characteristics on event time. For categorical variables we left out the missing category. Standard errors printed in parentheses and clustered at the individual level in Panel A and at the state level in Panel B. The joint F-test is based on a regression of the post-reform dummy on all covariates for the survey years 2005 and 2006. *** p<0.01, ** p<0.05, * p<0.1

Table B.2: Descriptives: covariate balancing, 2002-2010. Men only.

	mean 2005	diff	se	
Panel A: Individual characteristics				
Age	44.196	-0.078	(0.172)	
Employed full-time	0.594	-0.015	(0.006)	**
Employed part-time	0.122	0.009	(0.005)	
Unemployed	0.032	0.004	(0.004)	
Not in labour force, marginally	0.046	0.006	(0.004)	
Not in labour force	0.206	-0.004	(0.005)	
Casual	0.123	0.007	(0.006)	
Permanent	0.439	-0.014	(0.006)	**
Type missing	0.438	0.007	(0.006)	
Married	0.532	0.009	(0.005)	*
De facto	0.122	-0.004	(0.005)	
Separated	0.026	-0.001	(0.002)	
Divorced	0.046	0.001	(0.002)	
Widowed	0.021	-0.003	(0.001)	**
Single	0.252	-0.002	(0.005)	
Non-Australian origin	0.214	-0.003	(0.004)	
Aboriginal/Torrest Strait Islander	0.015	0.003	(0.001)	**
New South Wales	0.293	-0.007	(0.004)	
Victoria	0.244	0.003	(0.004)	
Queensland	0.210	0.001	(0.004)	
South Australia	0.091	0.003	(0.003)	
Western Australia	0.101	0.004	(0.003)	
Tasmania	0.035	-0.003	(0.001)	**
Northern Territory	0.006	0.000	(0.001)	
Australian Capital Territory	0.021	-0.001	(0.002)	
Postgraduate degree	0.038	-0.001	(0.002)	
Graduate diploma	0.043	0.000	(0.002)	
Bachelor degree	0.120	-0.005	(0.003)	*
Diploma	0.090	-0.002	(0.002)	
Certificate level III and IV	0.264	-0.002	(0.004)	
Finished year 12	0.137	-0.005	(0.004)	
Finished year 11 or less	0.309	0.016	(0.005)	***
Union member	0.188	-0.008	(0.005)	
Panel B: Federal state characteristics				
Smoking ban in enclosed places	0.000	0.346	(0.219)	
Unemployment rate (0-100%)	4.990	0.095	(0.240)	
State real GDP per capita	52593.300	330.996	(840.092)	
N	48,194			
Joint F-test for 2005/06 (N=10,668)			p=0.9986	

Source: Own calculations in Panel A based on HILDA, V15, survey years 2002-2010. Panel B based on data from the Australian Bureau of Statistics.
Notes: Column (1) presents the pre-reform mean value for 2005, column (2) the coefficients for the μ_0s as in equation 3, i.e., the reform year, from a regression of the socio-economic characteristics on event time. For categorical variables we left out the missing category. Standard errors printed in parentheses and clustered at the individual level in Panel A and at the state level in Panel B. The joint F-test is based on a regression of the post-reform dummy on all covariates for the survey years 2005 and 2006. *** p<0.01, ** p<0.05, * p<0.1

Table B.3: Effect of reform on covariates.

	b($\mu_{r=0}$)	se($\mu_{r=0}$)		b($\mu_{r=1}$)	se($\mu_{r=1}$)		b($\mu_{r=2}$)	se($\mu_{r=2}$)		b($\mu_{r=3}$)	se($\mu_{r=3}$)		b($\mu_{r=4}$)	se($\mu_{r=4}$)	
Age	0.060	(0.119)		0.256	(0.164)		0.244	(0.207)		0.071	(0.248)		-0.008	(0.289)	
Age squared	8.630	(10.792)		24.903	(14.819)	*	21.053	(18.636)		-0.267	(22.354)		-10.447	(26.078)	
Employed full-time	-0.003	(0.004)		-0.006	(0.006)		-0.002	(0.007)		-0.021	(0.009)	**	-0.025	(0.010)	**
Employed part-time	-0.001	(0.004)		-0.005	(0.006)		-0.012	(0.007)		-0.012	(0.009)		-0.015	(0.010)	
Unemployed	0.004	(0.002)		0.005	(0.003)		0.006	(0.004)	*	0.014	(0.004)	***	0.017	(0.005)	***
Not in labour force, marginally	0.002	(0.003)		-0.000	(0.004)		0.003	(0.005)		0.015	(0.006)	***	0.022	(0.007)	***
Casual	0.006	(0.004)		0.005	(0.005)		0.000	(0.007)		-0.002	(0.008)		-0.001	(0.009)	
Permanent	-0.010	(0.004)	**	-0.011	(0.006)	*	-0.006	(0.007)		-0.025	(0.009)	***	-0.033	(0.010)	***
Married	0.008	(0.003)	**	0.011	(0.005)	**	0.023	(0.006)	***	0.024	(0.007)	***	0.029	(0.009)	***
De facto	-0.001	(0.003)		0.004	(0.004)		-0.003	(0.005)		-0.005	(0.006)		-0.004	(0.007)	
Separated	-0.002	(0.002)		0.000	(0.002)		0.000	(0.003)		0.000	(0.003)		0.003	(0.004)	
Divorced	-0.001	(0.002)		-0.004	(0.002)		-0.006	(0.003)	**	-0.006	(0.004)	*	-0.006	(0.004)	
Widowed	-0.000	(0.001)		0.001	(0.002)		-0.003	(0.002)		-0.006	(0.003)	*	-0.008	(0.004)	**
Non-Australian origin	-0.000	(0.002)		0.005	(0.003)		0.002	(0.004)		0.010	(0.005)	**	0.015	(0.006)	***
Aboriginal/Torrest Strait Islander	0.002	(0.001)	*	0.001	(0.001)		0.002	(0.002)		0.002	(0.002)		0.002	(0.002)	
Victoria	0.005	(0.003)	*	0.009	(0.004)	**	0.009	(0.005)	**	0.013	(0.005)	**	0.015	(0.006)	**
Queensland	0.002	(0.003)		-0.003	(0.004)		0.003	(0.004)		-0.002	(0.005)		-0.005	(0.006)	
South Australia	0.002	(0.002)		0.001	(0.002)		0.003	(0.003)		0.002	(0.004)		0.004	(0.004)	
Western Australia	0.004	(0.002)	**	0.001	(0.002)		0.001	(0.003)		0.003	(0.004)		0.005	(0.004)	
Tasmania	-0.002	(0.001)	*	-0.003	(0.001)	**	-0.006	(0.002)	***	-0.008	(0.002)	***	-0.009	(0.003)	***
Northern Territory	-0.000	(0.001)		0.001	(0.001)		-0.000	(0.001)		-0.001	(0.001)		-0.000	(0.001)	
Australian Capital Territory	-0.000	(0.001)		-0.001	(0.001)		-0.001	(0.002)		0.001	(0.002)		0.000	(0.002)	
Postgraduate degree	-0.001	(0.001)		-0.001	(0.001)		-0.001	(0.002)		-0.002	(0.002)		-0.002	(0.002)	
Graduate diploma	-0.001	(0.001)		-0.001	(0.002)		0.000	(0.002)		-0.001	(0.002)		-0.003	(0.003)	
Bachelor degree	-0.003	(0.002)		-0.007	(0.003)	**	-0.009	(0.004)	**	-0.010	(0.004)	**	-0.014	(0.005)	***
Diploma	-0.001	(0.002)		-0.002	(0.002)		-0.002	(0.003)		-0.003	(0.003)		-0.006	(0.004)	*
Certificate level III and IV	-0.003	(0.003)		-0.003	(0.004)		-0.005	(0.004)		-0.005	(0.005)		-0.002	(0.006)	
Finished year 12	-0.004	(0.003)		0.001	(0.004)		0.001	(0.005)		0.007	(0.006)		0.007	(0.007)	
Union member	-0.004	(0.003)		-0.017	(0.004)	***	-0.019	(0.006)	***	-0.008	(0.007)		-0.010	(0.008)	
Smoking ban in enclosed place	0.343	(0.219)		0.898	(0.091)	***	0.991	(0.009)	***	0.992	(0.008)	***	0.998	(0.002)	***
State unemployment rate	0.095	(0.240)		0.214	(0.385)		0.658	(0.509)		2.460	(0.880)	**	2.394	(0.990)	**
State real GDP per capita	303.165	(826.622)		519.650	(1140.068)		454.264	(1088.189)		-811.452	(1725.068)		-2846.886	(1381.729)	*

Source: Own calculations based on HILDA, V15, survey years 2002-2010.
Notes: The table presents the coefficients associated p-values for the event time dummies (the μ_ts) from a regression of the individual socio-economic characteristics on event time as in equation 3. For categorical variables we left out the missing category. Standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1

Table B.4: Robustness checks: Smoking, low-educated individuals.

	All				Males				Females			
	Age group				Age group				Age group			
	All	15-29	30-49	50+	All	15-29	30-49	50+	All	15-29	30-49	50+
Panel A: Baseline model												
6-month effect	-0.013***	-0.028***	-0.034***	0.008*	-0.018**	-0.034**	-0.045***	0.010	-0.010*	-0.023*	-0.028**	0.007
	(0.004)	(0.010)	(0.009)	(0.005)	(0.008)	(0.016)	(0.015)	(0.008)	(0.005)	(0.014)	(0.012)	(0.006)
N	52948	17055	14788	21105	21719	8358	5756	7605	31229	8697	9032	13500
Panel B: Baseline model, no control variables												
6-month effect	-0.013***	-0.022**	-0.026***	0.001	-0.017**	-0.024*	-0.036***	0.002	-0.011**	-0.019	-0.020**	0.001
	(0.004)	(0.009)	(0.008)	(0.004)	(0.007)	(0.014)	(0.013)	(0.007)	(0.005)	(0.013)	(0.010)	(0.005)
N	52948	17055	14788	21105	21719	8358	5756	7605	31229	8697	9032	13500
Panel C: Baseline model, pooled OLS												
6-month effect	-0.015***	-0.026**	-0.021*	-0.003	-0.019**	-0.024	-0.032*	-0.001	-0.013**	-0.028*	-0.014	-0.004
	(0.005)	(0.012)	(0.011)	(0.006)	(0.009)	(0.018)	(0.018)	(0.011)	(0.007)	(0.016)	(0.014)	(0.007)
N	52948	17055	14788	21105	21719	8358	5756	7605	31229	8697	9032	13500
Panel D: Baseline model, pooled OLS using cross-sectional survey weights												
6-month effect	-0.022***	-0.026	-0.053***	0.001	-0.029**	-0.008	-0.077***	-0.008	-0.016*	-0.044**	-0.031*	0.004
	(0.008)	(0.016)	(0.016)	(0.008)	(0.014)	(0.024)	(0.030)	(0.016)	(0.009)	(0.021)	(0.018)	(0.009)
N	52573	16922	14693	20958	21537	8275	5704	7558	31036	8647	8989	13400
Panel E: Baseline model, controlling for individual fixed-effects												
6-month effect	-0.012***	-0.023**	-0.033***	0.008*	-0.016**	-0.028*	-0.046***	0.011	-0.009*	-0.019	-0.026**	0.007
	(0.004)	(0.011)	(0.009)	(0.005)	(0.008)	(0.016)	(0.015)	(0.008)	(0.005)	(0.014)	(0.012)	(0.006)
N	52948	17055	14788	21105	21719	8358	5756	7605	31229	8697	9032	13500
Panel F: Baseline model, including controls for cigarette prices												
6-month effect	-0.013***	-0.033***	-0.037***	0.011**	-0.017**	-0.033*	-0.048***	0.009	-0.010*	-0.033**	-0.030**	0.012*
	(0.005)	(0.011)	(0.010)	(0.005)	(0.008)	(0.017)	(0.016)	(0.009)	(0.006)	(0.015)	(0.013)	(0.006)
N	52948	17055	14788	21105	21719	8358	5756	7605	31229	8697	9032	13500
Panel G: Baseline model, excluding occasional smokers												
6-month effect	-0.009**	-0.025***	-0.025***	0.010**	-0.004	-0.022*	-0.022	0.019**	-0.012**	-0.028**	-0.027***	0.005
	(0.004)	(0.009)	(0.008)	(0.004)	(0.006)	(0.013)	(0.014)	(0.008)	(0.005)	(0.012)	(0.010)	(0.005)
N	50931	15933	14214	20784	20779	7782	5501	7496	30152	8151	8713	13288
Panel H: Baseline model, shorter observation window (2003 - 2008)												
6-month effect	-0.011**	-0.020*	-0.032***	0.005	-0.012	-0.020	-0.047***	0.013	-0.011*	-0.019	-0.023*	-0.000
	(0.005)	(0.011)	(0.010)	(0.005)	(0.008)	(0.017)	(0.016)	(0.009)	(0.006)	(0.015)	(0.012)	(0.006)
N	35135	11120	9964	14051	14349	5448	3859	5042	20786	5672	6105	9009
Panel I: Baseline model, controlling for quadratic time trend												
6-month effect	-0.011	-0.012	-0.026*	0.004	-0.008	-0.008	-0.046**	0.025*	-0.012	-0.012	-0.012	-0.007
	(0.007)	(0.017)	(0.014)	(0.007)	(0.012)	(0.026)	(0.023)	(0.013)	(0.009)	(0.023)	(0.018)	(0.009)
N	52948	17055	14788	21105	21719	8358	5756	7605	31229	8697	9032	13500
Panel J: Baseline model, placebo reform one year earlier												
6-month effect	0.006	0.007	0.011	0.001	0.003	0.014	0.016	-0.017**	0.009*	-0.002	0.007	0.011*
	(0.004)	(0.009)	(0.008)	(0.004)	(0.007)	(0.014)	(0.012)	(0.008)	(0.005)	(0.011)	(0.010)	(0.005)
N	52948	17055	14788	21105	21719	8358	5756	7605	31229	8697	9032	13500

Notes: Sample: only individuals with low levels of education. This table reports the results for the short-term effect (i.e., the μ_0s) from random effects regressions of pictorial warnings on tobacco products on smoking prevalence based on equation 2 (i.e., the μ_0s). Control variables as listed in Table 1. The sample size is always the same as in the baseline specification (Panel A), unless otherwise shown. Control variables as listed in Table 1. Standard errors clustered at the individual level.
*** p<0.01, ** p<0.05, * p<0.1 *Source:* Own calculations based on HILDA, V15.

Table B.5: Robustness checks: Smoking, high-educated individuals.

	All				Males				Females			
	Age group				Age group				Age group			
	All	15-29	30-49	50+	All	15-29	30-49	50+	All	15-29	30-49	50+
Panel A: Baseline model												
6-month effect	-0.006	-0.006	-0.008	-0.003	-0.009	0.009	-0.013	-0.005	-0.005	-0.017	-0.005	0.000
	(0.005)	(0.015)	(0.007)	(0.006)	(0.007)	(0.025)	(0.011)	(0.008)	(0.006)	(0.019)	(0.008)	(0.009)
N	30429	4868	15036	10525	13767	1870	6409	5488	16662	2998	8627	5037
Panel B: Baseline model, no control variables												
6-month effect	-0.006	0.000	-0.010	-0.003	-0.006	0.014	-0.010	-0.004	-0.007	-0.009	-0.009	-0.002
	(0.004)	(0.014)	(0.006)	(0.006)	(0.007)	(0.024)	(0.011)	(0.007)	(0.006)	(0.017)	(0.008)	(0.009)
N	30429	4868	15036	10525	13767	1870	6409	5488	16662	2998	8627	5037
Panel C: Baseline model, pooled OLS												
6-month effect	-0.004	0.001	-0.009	0.000	-0.003	0.032	-0.013	-0.004	-0.005	-0.019	-0.006	0.005
	(0.005)	(0.019)	(0.008)	(0.008)	(0.008)	(0.032)	(0.013)	(0.011)	(0.007)	(0.023)	(0.010)	(0.012)
N	30429	4868	15036	10525	13767	1870	6409	5488	16662	2998	8627	5037
Panel D: Baseline model, pooled OLS using cross-sectional survey weights												
6-month effect	-0.011	-0.016	-0.015	-0.004	-0.015	0.022	-0.031	-0.011	-0.008	-0.041	-0.004	0.005
	(0.008)	(0.024)	(0.013)	(0.011)	(0.013)	(0.044)	(0.020)	(0.018)	(0.010)	(0.026)	(0.016)	(0.011)
N	30320	4855	14987	10478	13726	1865	6397	5464	16594	2990	8590	5014
Panel E: Baseline model, controlling for individual fixed-effects												
6-month effect	-0.006	-0.002	-0.007	-0.003	-0.008	0.002	-0.012	-0.005	-0.005	-0.007	-0.004	-0.000
	(0.005)	(0.015)	(0.007)	(0.006)	(0.007)	(0.025)	(0.011)	(0.008)	(0.006)	(0.019)	(0.009)	(0.009)
N	30429	4868	15036	10525	13767	1870	6409	5488	16662	2998	8627	5037
Panel F: Baseline model, including controls for cigarette prices												
6-month effect	-0.011**	-0.023	-0.009	-0.004	-0.013	-0.014	-0.008	-0.009	-0.011	-0.029	-0.011	0.001
	(0.005)	(0.016)	(0.008)	(0.007)	(0.008)	(0.027)	(0.013)	(0.010)	(0.007)	(0.020)	(0.009)	(0.009)
N	30429	4868	15036	10525	13767	1870	6409	5488	16662	2998	8627	5037
Panel G: Baseline model, excluding occasional smokers												
6-month effect	-0.002	-0.000	0.001	-0.003	-0.002	0.006	0.001	-0.009	-0.001	-0.004	0.000	0.003
	(0.004)	(0.011)	(0.005)	(0.005)	(0.005)	(0.018)	(0.009)	(0.007)	(0.005)	(0.014)	(0.006)	(0.007)
N	29302	4562	14431	10309	13181	1719	6085	5377	16121	2843	8346	4932
Panel H: Baseline model, shorter observation window (2003 - 2008)												
6-month effect	-0.008	0.003	-0.014*	-0.001	-0.008	0.030	-0.020*	-0.004	-0.008	-0.017	-0.010	-0.000
	(0.005)	(0.017)	(0.008)	(0.006)	(0.007)	(0.028)	(0.012)	(0.008)	(0.007)	(0.021)	(0.010)	(0.009)
N	19986	3214	10049	6723	9043	1247	4284	3512	10943	1967	5765	3211
Panel I: Baseline model, controlling for quadratic time trend												
6-month effect	-0.011	0.012	-0.019	-0.008	-0.019*	0.036	-0.038**	-0.012	-0.004	-0.007	-0.005	-0.003
	(0.008)	(0.025)	(0.012)	(0.010)	(0.011)	(0.038)	(0.018)	(0.012)	(0.011)	(0.034)	(0.017)	(0.016)
N	30429	4868	15036	10525	13767	1870	6409	5488	16662	2998	8627	5037
Panel J: Baseline model, placebo reform one year earlier												
6-month effect	0.002	-0.011	0.006	0.006	0.009	0.001	0.018*	0.003	-0.004	-0.017	-0.004	0.009
	(0.004)	(0.012)	(0.007)	(0.006)	(0.006)	(0.017)	(0.011)	(0.007)	(0.006)	(0.017)	(0.008)	(0.009)
N	30429	4868	15036	10525	13767	1870	6409	5488	16662	2998	8627	5037

Notes: Sample: only individuals with high levels of education. This table reports the results for the short-term effect (i.e., the μ_0s) from random effects regressions of pictorial warnings on tobacco products on smoking prevalence based on equation 2 (i.e., the μ_0s). Control variables as listed in Table 1. The sample size is always the same as in the baseline specification (Panel A), unless otherwise shown. Control variables as listed in Table 1. Standard errors clustered at the individual level.

*** p<0.01, ** p<0.05, * p<0.1 *Source:* Own calculations based on HILDA, V15.