How do pictorial warnings affect smoking behavior?

A quasi-experimental approach using the synthetic control method

Henrik Frimanson

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Swedish University of Agricultural Sciences, SLU
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Henrik Frimanson

Supervisor:	**Jens Rommel, Swedish University of Agricultural Sciences, Department of Economics**
Examiner:	Shon Ferguson, Swedish University of Agricultural Sciences, Department of Economics

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Swedish University of Agricultural Sciences
Faculty of Natural Resources and Agricultural Sciences
Department of Economics

Abstract

While the tobacco epidemic is a worldwide problem continuing to kill millions of people each year, evidence on the behavioral impact of pictorial health warnings remains limited. This study uses the synthetic control method to evaluate the effect Australia's 2006 implementation of pictorial health warnings had on smoking prevalence. A synthetic Australia is constructed, which is a weighted combination of 15 OECD countries that did not implement pictorial health warnings during the period of the study. To assess the impact of the policy, smoking rates in Australia are then compared with smoking rates in its synthetic control, over the time period 1980-2012. The results points at a five percent decrease in smoking prevalence over a seven year long post-intervention period, although placebo tests show limited robustness of this finding. Even though the independent policy implications of my findings are limited, they align with previous work evaluating the same policy. Hence, when considered alongside earlier research, this study provides suggestive evidence that the Australian policy did cause smoking prevalence to decrease.

Keywords: Australia, Behavioral public policy, Tobacco-control, Policy evaluation

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1. Introduction

Causing more than 8 million deaths per year, tobacco use is one of the greatest health threats humans have ever faced (WHO 2023). The vast majority is a result from cigarette smoking, which is the main form of tobacco use in most countries (Francis et al. 2019). Besides causing terrible loss and suffering among families world wide, smoking also induces considerable costs to society, both regarding health care expenditures, as well as decreased human capital coming from illness and mortality (WHO 2023). For example, Goodchild et al. (2017) estimate that in 2012, the total cost of smoking amounted to $1,436 billion, that is 1.8% of the global GDP.

Different tobacco control policies have been implemented in countries around the world to address the severe consequences and costs of tobacco use. An example is the adoption of health warnings on tobacco packages, which is seen as both a cheap and effective way of communicating the health risks connected to smoking (WHO n.d.a). They are especially powerful in reducing consumption when graphical and fear-arousing pictures are included, which has made pictorial health warnings (PHWs) a globally embraced tobacco control policy (Canadian Cancer Society 2023).

The objective of this study is to estimate the impact pictorial health warnings had on smoking prevalence in Australia, which implemented a law making them mandatory in 2006.

Following the wide adoption of pictorial health warnings, a rigorous body of research investigating their effects has also been established. Mainly, studies have looked at how PHWs affect different psychological outcomes. For example, Fong, Hammond & Hitchman (2009) summarize the research and conclude that PHWs are more effective in being noticed, eliciting thoughts about health risks and motivating smokers to quit, compared to text-only warnings. Furthermore, reviews by Francis et al. (2019) and Hammond (2011) suggest that pictorials have a desirable effect on multiple tobacco related outcomes, such as reactions, attitudes, awareness and intentions.

Besides the effects on psychological aspects, it is also relevant to consider how PHWs affect actual smoking behavior. Kuehnle (2019) investigates how prevalence, quitting, initiation and relapsing are affected by pictorial warnings, by using the Australian implementation in 2006 as an event study. The introduction of PHWs was related to a significant drop in smoking prevalence by an average 4%, where young people were affected to a higher extent, and people 50 years or older were not affected at all. The policy reduced smoking rates permanently, which mainly was due to a relatively instant increase in quitting behavior within a year after implementation. Ibarra-Salazar, Romero Rojas & Ayala-Gaytán (2021) study the effects on cigarette demand in Mexico, which introduced pictorial health warnings in 2010. They find that PHWs reduce cigarette consumption. However, the effect decreases over time. This corresponds to what Kuehnle (2019) found, and suggests that consumers may get used to images as they are exposed to them. Furthermore, Miller et al. (2011) also study the Australian implementation and find additional evidence on the role of wearing-out effects: their results suggest that the ability to recall the warnings decreased over time. Huang, Chaloupka & Fong (2014) use the USA as a control group to estimate the impact the Canadian introduction of PHWs had on smoking rates. They find that, in comparison to the U.S., PHWs reduced smoking rates significantly, specifically with 2.87-4.68 percentage points, i.e. a relative decrease between 12.1-19.6%. In a similar fashion, Fathelrahman et al. (2013) compare Thailand, where cigarette packages feature big graphical tobacco warnings, with Malaysia, with small text warnings only, to examine any differences in quitting attempts. They conclude that the larger and graphical warnings in Thailand were associated with higher rates of quitting attempts, illustrating their greater potency compared to text warnings. Other papers looking at smoking behavior also find pictorial health warnings to have a negative effect, e.g. Azagba & Sharaf (2013) and Yong et al. (2013). In contrast, Gospodinov & Irvine (2004) use two waves of monitoring surveys bordering the 2001 introduction of PHWs in Canada, and find the warnings to have no apparent impact on smoking prevalence.

Studies looking at outcomes related to actual smoking behavior are a clear minority among the previous research on the effects of PHWs. Mostly, aspects such as those covered in the reviews by Francis et al. (2019) and Hammond (2011) are examined, which is something that Monárrez-Espino et al. (2014), Kuehnle (2019) and Azagba & Sharaf (2012) bring up as a limitation of the existing literature. Hence, it is here this study intends to make its first and main contribution. By looking at smoking prevalence as the outcome, e.g. as in Kuehnle (2019) and Gospodinov & Irvine (2004), this paper investigates whether pictorial health warnings are associated with people actually reducing their cigarette consumption, which ultimately is the goal of the policy maker. Taking a story from Huang, Chaloupka & Fong (2014), one of the reasons for the US Court of Appeal to strike down the first ever regulation on PHWs in the USA, was the failure of the FDA to present clear evidence that pictorial warnings would "reduce the number of Americans who smoke". This illustrates the importance to build on the documentation of the behavioral effects of PHWs. Second, looking at how the 2006 implementation in Australia affected smoking prevalence at the national level is rare, as this is something Kuehnle (2019) reports to be the first one to do. Third, though the synthetic control method (SCM) has been used to evaluate the effects of aggregate tobacco-control policies (see Eoun Jung 2024), the SCM has not yet been used to estimate the individual effect of pictorial health warnings. Furthermore, Bouttell (2018) states that the synthetic control method has been underused in public health, a research area in which it is said to have great potential. In this study, by applying the SCM to estimate the effects of pictorial health warnings, I aim to contribute to filling this gap.

The study also contributes to the broader behavioral public policy literature in economics, i.e., studies investigating the impact of "soft policies" not changing monetary incentives or implementing product bans through regulation. For example, Shangguan et al. (2018) review 60 articles to assess the effects of food labeling (e.g. nutrition facts panel or menu calorie labels) on consumer food choices. They find that labelling decreased the intake of total fat by 10.6 percent, other unhealthy dietary options by 13 percent, while the consumption of vegetables increased by 13.5 percent. Correspondingly, Campos, Doxey & Hammond (2011) review 120 papers and show a consistent link between the use of labels and healthier diets. Also, individual studies find evidence suggesting a positive effect of nutrition labels on different health outcomes (Aranda, Darden & Rose 2021; Variyam 2008; Restrepo 2016). Ecolabels are another example of labelling, used to inform consumers about which products that are environmentally preferable. When reviewing 56 studies, Potter et al. (2021) assess that ecolabeling was related to individuals selecting and purchasing more sustainable food products. In contrast, Tiboni-Oschilewski et al. (2024) review 58 records to assess the strengths and weaknesses of ecolabelling on food products. They state that ambiguous results were observed as to whether the labels influence consumers' choices or not, with some papers arguing for and some against. Also, the authors underline that there is strong evidence of nutrition labels being effective in altering food choice, but that the effectiveness of eco-labels in driving behavioral change is disputed. However, using data from a large-scale experiment, Lohmann et al. (2022) investigate whether carbon footprint labels promote more environmentally friendly food choices. Their findings imply that the labels significantly decreased the probability of choosing a high-carbon footprint meal by roughly 2.7 percent.

Taken together, there is evidence on the behavioral effectiveness of food labels informing about nutritional values, while it is less clear whether ecolabels are effective in altering food choices. One aspect to consider is that the former is providing consumers with information that directly have an effect on their own health, in contrast to ecolabels. In this respect, nutrition labels function much like pictorial health warnings.

The remainder of the paper is structured as follows: Section 2 addresses the contextual background of Australia's implementation of PHWs, as well as relevant theory. Section 3 introduces the synthetic control method, explains how it was applied, and describes the used data. Section 4 presents the main findings, as well as the results of the applied robustness checks. Section 5 discusses the implications and limitations of the results, and Section 6 concludes the paper.

2. Backgound

2.1 The implementation

In January 2001, Canada became the first country in the world to use pictorial health warnings on cigarette packages (Hiilamo et al. 2014). Since then, many have followed, and PHWs became particularly popular after the adoption of the WHO Framework Convention on Tobacco Control (FCTC) in 2005. The treaty, which now 183 countries have ratified (FCTC 2023), includes a wide range of actions aiming at limiting both the supply and demand of tobacco products, where regulations on packaging and labelling are covered in Article 11 (FCTC 2003). Here, it is specified that cigarette packages have to include health warnings and messages covering at least 30% of the back and front, however, members are encouraged to take further measures as well (FCTC 2013). For example, it is stated that the warnings 'may be in the form of or include pictures or pictograms', a measure that, even though it isn't explicitly required, has become widely embraced all around the world. Today, at least 138 countries require PHWs of some sort (Canadian Cancer Society 2023). This broad adoption may partly be explained by PHWs being a low-cost and effective way of spreading information about the health risks of smoking. In addition, they are effective in reaching out to the consumers, as smokers are exposed to them every time they reach for a new cigarette. Hammond (2011) calculates that a pack-a-day smoker approximately is exposed to the warnings 7000 times a year.

In Australia, between 1995 and 2006, cigarette packages had to include text warnings only, covering 25% of the front and 33% of the back of the packages (Kuehnle 2019). However, after ratifying the FCTC treaty on October 27th 2005 (United Nations 2006), tobacco products either produced in or imported to Australia had to also include pictures, starting from March 1st 2006 (Miller et al. 2009b). These had to be colorful images covering 30% of the front and 90% of the back of the packages, many illustrating different tobacco related diseases. The pictures were accompanied with messages such as 'Smoking causes mouth and throat cancer', 'Smoking - a leading cause of death' or 'Do not let children breathe your smoke'. See Figure 1 for some examples of the new health warnings that were adopted. The requirement applied to all types of tobacco products, including cigarettes, cigars and loose or pipe tobacco. To prevent consumers from adapting to the images, two sets of seven pictures were rotated every 12 months (Miller et al. 2009b). Also, the manufacturers complied with the law quite quickly. In Miller et al. (2009b), it is shown that six months after the time of implementation, 80% of the best selling brands had the new warnings on their packages. Additionally, White, Webster & Wakefield (2008), who look at how

PHWs affect adolescents in Australia, suggest that around the same time, 88% of the youths had noticed the new labels. Thus, severe duration between the implementation date and adoption of the new warnings seems not to have been a problem.

Figure 1. Two examples of pictorial warnings implemented in Australia in 2006. Pictures on the left represent the back of the packages, and pictures on the right represent the front. (WHO n.d.b).

2.2 Entangling effects

Important to address are other aspects that changed at the time of the policy that risk being entangled with the effects of the PHWs. First, the law itself made the reference to the Australian Quitline number more prominent on the packages (Kuehnle 2019). Prior to 2006, the number was displayed in a small text size on the side of the packages, whereas after the reform, it was "stamped" on the pictures on the back of the pack. Also, a motivational message encouraging cessation was added to the back, see Figure 1 for illustration. Second, the Australian government implemented different mass media campaigns raising awareness of the labels at different periods of 2006 (Miller et al. 2009). Additionaly, multiple non-government health agencies went together and made supportive commercials portraying instances of amputation and mouth cancer, which were televised to emphasize the moral of the PHWs.

Because of these entangling effects, Kuehnle (2019), also studying the Australian implementation, is cautious when concluding about the impact of pictorial health warnings. Specifically, he states that these three aspects together: PHWs, notable references to a quitting line, and televised commercials, are an effective way of reducing smoking prevalence. Even though these effects cannot be methodically disentangled, he carefully discusses the relative importance each aspect may have. First, the more prominent reference to the Quitline number had a significant effect on the number calls, which doubled in the year of the new law compared to the year before (Kuehnle 2019). However, it is unlikely that this had any individual effect on smoking prevalence, since the reference should be seen as a mediator of the effect of PHWs, i.e. that consumers call the Quitline because of the new warnings. This is backed up by the findings of Miller et al. (2009a), who argue that the introduction of PHWs is most likely to be the reason for the rapid increase in the number of calls. Moreover, Kuehnle argues that the share of quitters that called the Quitline number only increased marginally between 2005 and 2006 (from 7% to 8%), which further indicates that the more notable reference didn't have any independent effect on smoking prevalence. Second, as for the campaigns and televised commercials, Kuehnle assesses that because previous papers studying these subjects hold too low quality, it is not possible to come up with a reliable estimate of the impact they had on smoking prevalence. Despite this uncertainty, he argues that they probably did not have any vital separate effect, based on two reasons. First, like the Quitline reference, the campaigns should be viewed as mediators of the effect, as the purpose of these was to strengthen the morale of the PHWs. Second, smokers would have been significantly more exposed to the warnings themselves compared to the campaigns and commercials. Someone who smokes one pack per day would see the pictorial warnings roughly 140 times per week. As for the commercials, however, Miller et al. (2009a) estimate that adults saw these at maximum 20 times per week, directly after they were introduced. As the commercials had been screened for a period of time though, this number decreased all the way down to less than 5 times per week.

2.3 Linking pictorial health warnings to smoking behavior

How are pictorial warnings supposed to alter consumer behavior and make individuals reduce their consumption? According to Kuehnle (2019), there are two main ways for this to happen: first, through increases in attention and awareness of the health warnings, and second, through changes in health beliefs and attitudes towards smoking. As was stated before, there have been multiple studies looking at how psychological outcomes like these are affected by PHWs, some of which I will further discuss here. Regarding the first point, prior research suggests that pictorial health warnings have a positive effect on consumers' awareness and attention. For example, in a meta-analysis of experimental studies, Noar et al. (2015) find that PHWs attract and hold attention in a more effective way than text-only warnings, which is in line with the findings of Fong, Hammond & Hitchman (2009) who conclude that PHWs are more likely to be noticed, and Borland et al. (2009) who show that the implementation in Australia led to substantial higher awareness. As for the second issue, also here the evidence is pretty clear. In a review, Francis et al. (2019) show that PHWs induce both cognitive reactions (i.e. consideration of the hazards of smoking), as well as negative affective reactions. Furthermore, they conclude that PHWs lead to more negative attitudes towards smoking, compared to text-only warnings. In another review, Hammond (2011) finds that PHWs increase the knowledge about tobacco related diseases, which is backed up by Mannocci et al. (2019) who come to the same conclusion when looking at the implementation of PHWs in Italy.

3. Methods

3.1 A primer on the synthetic control method

Ever since its first application in Abadie & Gardezabal (2003), the synthetic control method (SCM) has become widely popular in applied economics research, as well as other disciplines such as engineering and biomedicine (Abadie 2021). Susan Athey and Guido Imbens call it “arguably the most important innovation in the policy evaluation literature in the last 15 years” (Athey & Imbens 2017), which illustrates its great impact on empirical research. For example, it has been used to investigate the economic costs of terrorism in the Basque Country (Abadie & Gardezabal 2003), the effects of a large-scale tobacco control program in California (Abadie, Diamond & Hainmueller 2010), and the impact of carbon taxes on CO2 emissions in Sweden (Andersson 2019). In analogy to these applications, the method is especially suited to estimate the effect of aggregate interventions or events. That is, when a small number of large units, e.g. countries or regions, are affected by some infrequently occurring treatment, and the outcome is measured at an aggregate level (Abadie 2021). The wide popularity of the synthetic control method may be explained by different reasons. For example, it addresses the ambiguous way comparison groups normally are chosen in other quantitative comparative case study designs, such as the differences-in-differences (DiD) approach (Abadie 2021; Andersson 2019). This issue is called out as one of the main limitations of comparative case studies (Abadie, Diamond & Hainmueller 2010), and is key since using inappropriate comparison groups can lead to biased results (Abadie, Diamond & Hainmueller 2015). By letting a computer driven process construct a suitable comparison group, this selection is formalized, which reduces discretion (Abadie, Diamond & Hainmueller 2010). Furthermore, the synthetic control method deals with a problem often faced when the analysis is of aggregate nature, as often is the case in social science research: that it may be difficult to identify a single unit that constitutes a proper comparison group (Abadie 2021; Abadie, Diamond & Hainmueller 2010). This is tackled by the main idea of the SCM, which is that a combination of untreated units generally produces a better comparison than a single unit alone (Abadie 2021). A final example of an advantage of the SCM is that it relaxes the parallel trends assumption by letting unobserved confounders to vary over time (Andersson 2019; Abadie, Diamond & Hainmueller 2010). This assumption states that in absence of any treatment, the difference between the treated and untreated group would have remained the same in the post-treatment period as in the pre-treatment period (Huntington-Klein 2022). The parallel trends assumption is required to hold in order to obtain unbiased results when using the DiD, one of the most widely applied causal inference methods in the social sciences.

3.2 Steps in setting up the estimation model

The synthetic control method seeks to estimate a counterfactual outcome, that is, what would have happened to the treated unit in the post-treatment period if no treatment had occurred (Abadie 2021). This, often thought of as the great challenge of policy evaluation, is done by constructing a synthetic control group, which is a weighted combination of different untreated donor units. It is crucial that the donor pool consists of units that are similar to the treated group, however, there is currently no formal approach to judge this similarity (Bouttell et al. 2018). The weights are chosen based on predictors of the outcome in a data driven process, so that a larger weight is assigned to a given donor unit if the unit is similar to the treated group in a predictor of importance. In the end, the SCM chooses the combination of donor weights that minimizes the difference in each predictor, so that the trajectory of the synthetic control group matches the pre-treatment outcome of the treated group as closely as possible (Abadie 2021; Abadie & Gardezabal 2003; Abadie, Diamond & Hainmueller 2010). If the synthetic control group is able to do so for a longer period of time before treatment, it suggests low levels of bias (Abadie 2021), which lends confidence to the identifying assumption that the synthetic control group represents the counterfactual outcome in the post-treatment period (Andersson 2019). In this case, any divergence succeeding the treatment may be interpreted as a treatment effect (Bouttell et al. 2018).

3.3 Empirical strategy and data

In this study, the synthetic control method is applied to evaluate the 2006 implementation of pictorial health warnings on smoking prevalence in Australia. This approach was chosen because of the aggregate nature of the context, with few and large entities, infrequently occurring treatment, and an aggregate outcome variable. This made it difficult to find a single country to constitute a suitable comparison in order to perform a DiD, which was considered initially. Furthermore, in contrast to conventional methods of evaluating public health interventions (e.g. interrupted time series analysis or panel data regression), the synthetic control method includes a counterfactual, which improves the causal inference of the analysis (Bouttell et al. 2018).

A “synthetic Australia” is constructed using a donor pool consisting of a group of OECD member countries that did not implement PHWs during the period of the study. Countries of the OECD are chosen since most of them are developed, high-income economies, like Australia (World Population Review 2025). Other studies using the SCM to examine the effect of some intervention or event on an

OECD country use other OECD countries as donor units (see Abadie, Diamond & Hainmueller 2015 and Andersson 2019). A number of countries, some of which were desirable, had to be excluded from the donor pool as they implemented PHWs under the period of analysis, and hence could not be be regarded as untreated. These were Belgium, Canada, Chile, Colombia, France, Latvia, Mexico, New Zealand, Norway, Spain, Switzerland, Turkey and the United Kingdom. In addition, Costa Rica, Czechia, Estonia, Hungary, Israel, Lithuania, Poland, Slovakia and Slovenia had to be removed because of insufficient data. Table 1 lists all of the 15 countries that at last were used as donor units.

Table 1. List of donor countries.

Donor countries	**Donor countries**
Austria	Japan
Denmark	Korea
Finland	Luxembourg
Germany	Netherlands
Greece	Portugal
Iceland	Sweden
Ireland	United States
Italy	

Data on smoking prevalence in Australia and each donor country is gathered from Our World in Data (2018). It is measured as the percentual share of the population that smokes daily, and is observed yearly between 1980 and 2012, for both men and women of all ages. Important to note is that this data are estimates rather than direct measurements, retrieved from the Institute for Health Metrics and Evaluation (IHME). Specifically, they originate from the study by Ng et al. (2014), and are then processed by Our World in Data, which e.g. implies converting units or standardizing country names. The fact that the outcome is based on estimated values must be taken into account as it might give rise to some measurement error. Furthermore, data on GDP per capita is collected from the OECD Data explorer (2025) for the same countries and periods as for the smoking prevalence data. This is also observed yearly, and the unit of measure is U.S. dollars per person, purchasing power parity (PPP) converted.

As the data spans between 1980-2012, the pre-treatment period of the study is defined as 1980-2005, time of treatment is 2006, and the post-treatment period is 2006-2012. 2012 is chosen as the end year based on the trade-off between obtaining a sufficient number of possible donor countries, and enough post-intervention time periods. First, 2012 enables me to use countries such as Denmark and Ireland as donor units, implementing PHWs in 2012 and 2013, respectively. Furthermore, it allows me to include all the EU countries that introduced PHWs in 2016 following the adoption of the 2014/40/EU directive, e.g. Sweden, Finland and Germany. Second, I assess that 2012 provides a sufficient number of post-treatment periods given the rapid way pictorial health warnings are expected to affect cigarette consumption (see Kuehnle 2019 and Ibarra-Salazar, Romero Rojas & Ayala-Gaytán 2021). So, if my analysis finds any treatment effect, it should be noticed in the seven year long post-treatment period. Also, in 2012 plain packaging became required in Australia (Canadian Cancer Society 2023), so expanding the post-intervention period would induce further issues with entangling effects.

The variables I choose as predictors are averages of the outcome for three pre-intervention time intervals, as well as averages of GDP per capita for the same time intervals. Specifically, I average these two variables for the time periods 1980-1989, 1990-1999 and 2000-2005. I choose to predict on pre-intervention values of the outcome as it helps to control for unobserved confounders when there are many pre-treatment time periods (Abadie, Diamond & Hainmueller 2015), and because it is a common practice in SCM applications (Huntington-Klein 2022; Cunningham 2021). I also choose GDP per capita as a predictor since there is clear evidence of a strong relationship between income levels and smoking prevalence. For example, in their review of more than 13 500 articles, Casetta et al. (2017) conclude that higher smoking rates are associated with lower income levels worldwide and across subgroups. Furthermore, GDP per capita is often used as a predictor in SCM applications, even when the outcome isn't some income related variable (e.g. see Abadie, Diamond & Hainmueller 2010, Yao, Bolen & Williamson 2021 and Wang et al. 2020).

4. Results

4.1 Estimation results

To get an overview of how the synthetic control group is constructed, see Table 2. It lists all the donor units along with their assigned weights. USA, Ireland, Sweden and Portugal make up the majority of the synthetic control group, specifically 97.8 percent. The rest of the donor units only get negligible weights, ranging between 0.005 and 0.001, and Greece get a weight of zero. As was stated before, these weights are based on the matching predictors, whose relative importance are listed in Table 3. Pre-intervention values of the outcome get the biggest weights in the matching process, while GDP per capita only has a small influence. Both the average GDP per capita in 1990-1999 and 2000-2005 get a weight of zero.

Table 2. Donor countries and their assigned weights. Note: Greece get a weight of zero.

Donor country	**Weight**	**Donor country**	**Weight**
United States	0.5220	Netherlands	0.0020
Ireland	0.2130	Iceland	0.0020
Sweden	0.1290	Germany	0.0020
Portugal	0.1140	Luxembourg	0.0010
Korea	0.0050	Japan	0.0010
Italy	0.0040	Denmark	0.0010
Finland	0.0030	Austria	0.0010

Table 3. Matching predictors with respective weights.

Matching predictor	**Weight**
Avg. smoking prevalence 1980-1989	0.4942
Avg. smoking prevalence 1990-1999	0.4194
Avg. smoking prevalence 2000-2005	0.0817
Avg. GDP per capita 1980-1989	0.0047
Avg. GDP per capita 1990-1999	0.0000
Avg. GDP per capita 2000-2005	0.0000

The pre-treatment fit between Australia and the synthetic control group also needs to be evaluated, which is key for the credibility of the results. This can first be done by visual inspection of Figure 2, which plots the time trends in yearly smoking prevalence in real and synthetic Australia. Starting from the early 1990s, up to the time of the intervention in 2006, the two groups match relatively well. After the intervention they diverge with lower smoking rates in the treated group, which suggests that the policy may have had some effect. However, prior to the early 1990s, Australia and the synthetic control group differ to some extent, resulting in a less satisfactory fit. This becomes especially prominent in Figure 3, which plots the gap in smoking prevalence between the two groups over the years. Ultimately, a stable and small difference would be seen up to the point of treatment, however, this isn't the case here. Between 1980 and 1989, the gap in smoking prevalence dropped by roughly two percentage points, a greater change than what is observed following the intervention. The poor and volatile fit during the first 10 years clearly harms the credibility of the study.

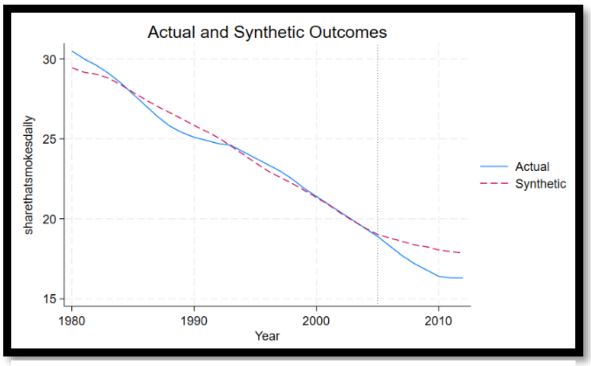

Figure 3. Time trends in smoking prevalence in real and synthetic Australia. Time of treatment is represented by the vertical line in the year 2006.

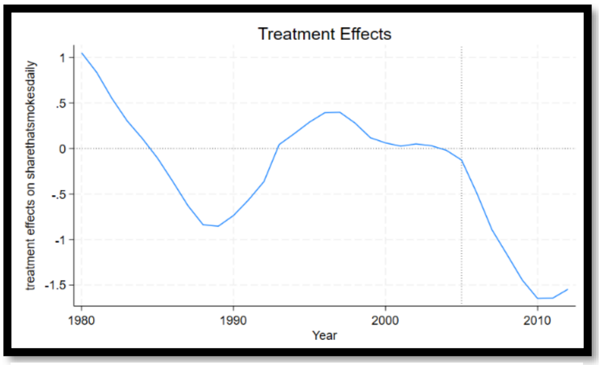

Figure 2. Gap in smoking prevalence across all years of the study. Time of treatment is represented by the vertical line in the year 2006.

Besides visual inspection, one can also judge the fit by examining the Root Mean Squared Prediction Error (RMSPE). This is the square root of the MSPE, which measures the average squared divergence between the treated group and the synthetic control. The RMSPE was calculated to 0.469, which indicates that during the pre-treatment period, smoking prevalence in Australia deviated from its synthetic control by an average 0.469 percentage points per year.

The average treatment effect is estimated to be -1.2631. This is equal to the mean difference in smoking prevalence between Australia and the synthetic control group in the post-intervention period (see Table 4). If the identifying assumption holds, i.e. if the trajectory of synthetic Australia represents the counterfactual outcome of real Australia, and that no entangling effects are influencing the results, this is interpreted as a causal effect. This would suggest that, over the seven years following their introduction in Australia, pictorial health warnings caused smoking prevalence to decrease by an average 1.2631 percentage points. Compared to the average level over the whole pre-intervention period (24.58 percent), this is equal to a relative decrease of 5.14 percent.

Furthermore, Table 4 suggests that the policy had a quite rapid effect on smoking prevalence, with a decrease of 0.49 percentage points (about 1.99 percent) within the same year as the implementation. For each post-intervention period this effect increases, up to the year of 2012, where the effect is smaller compared to 2011. This is also the only time smoking prevalence does not decrease from one year to the next, which otherwise is the case for the whole period of the study. Even though more post-intervention time periods are needed to make a confident claim, these findings suggest that the policy had a rapid but diminishing effect on smoking prevalence.

Table 4. Outcomes in smoking prevalence for Australia and the synthetic control group, along with treatment effects for each year in the post-intervention period.

Time	Actual Outcome	Synthetic Outcome	Treatment effect
2006	18.3000	18.7900	-0.4900
2007	17.7000	18.5935	-0.8935
2008	17.2000	18.3687	-1.1687
2009	16.8000	18.2480	-1.4480
2010	16.4000	18.0490	-1.6490
2011	16.3000	17.9449	-1.6449
2012	16.3000	17.8476	-1.5476
Mean	**17.0000**	**18.2631**	**-1.2631**

4.2 Robustness

To test whether the results are robust, in-space placebo tests were first performed. The effect of the intervention is then estimated for each country in the donor pool, as if they were the ones that got treated. The derived placebo effects are then compared to the actual treatment effect of Australia, in accordance to Figure 4. The bold line is the treatment effect of Australia, i.e. the same as the line in Figure 3, and the subtle lines are the corresponding placebo effects of each donor country. It is clear that the outcome for Australia does not stand out from the rest, which weakens the robustness of my findings and implies that the observed effect may arise by random chance.

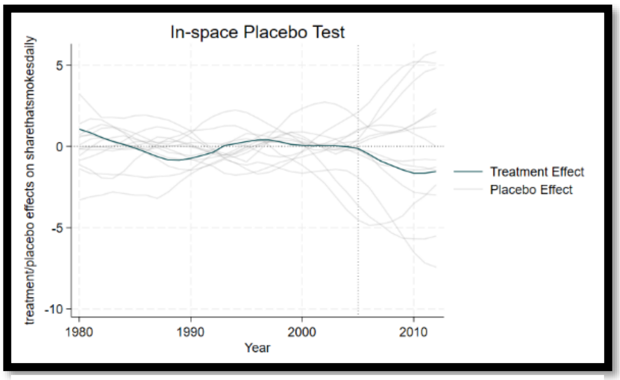

Figure 4. Treatment vs placebo effects

This way of repeatedly reassigning fake treatment and estimating placebo effects also provides methods for inference when using the synthetic control approach. First, the ratio of post- to pre-intervention MSPE is calculated for Australia and each donor unit. A large ratio implies a greater deviation between the treated group and its synthetic control in the post-treatment period relative to the pre-treatment period, pointing at a treatment effect. The ratio of Australia is then compared to the ones of the donor units, as in Figure 5. Australia's ratio ends up in the middle of the distribution, i.e. half of the donor units had greater post-to pre-intervention MSPE ratios than Australia. This yields a general p-value for the post-intervention period of 0.5, which suggests that the treatment effect is not statistically significant. Furthermore, yearly placebo effects for the donor units can be compared to the actual treatment effects for Australia to obtain a p-value for each year in the post-intervention period. These are displayed in Figure 6, and should be interpreted as the probability that the placebo effects are of larger magnitude (more negative) than the estimated treatment effect of Australia. Over the post-intervention period, these range between 0.5 and 0.3 approximately, further suggesting that the results are insignificant.

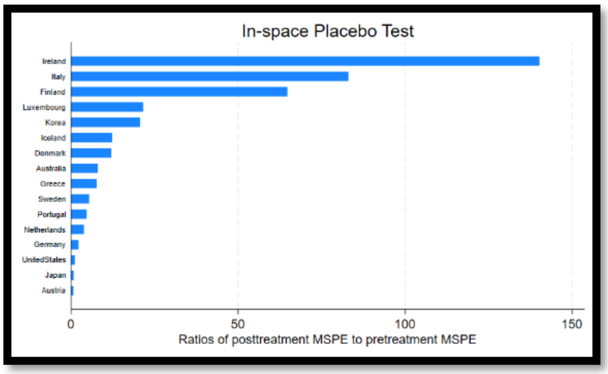

Figure 6. Distribution of post-to pre-intervention MSPE ratios for Australia and all donor units.

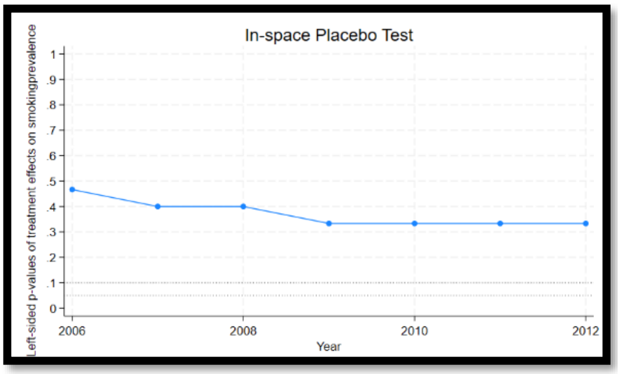

Figure 5. Left-sided p-values for each post-intervention time period.

As an additional robustness check, in-time placebo tests were also implemented. Here, fake treatments were assigned in three different pre-intervention years when no intervention actually occurred. These years were 2000, 1995 and 1990. Because the pre-treatment period is redefined for each of these years, the matching predictors had to be changed as well. For convenience, I averaged the same variables as in the main estimation but over four time periods instead of three: 1980-1984, 1985-1989, 1990-1994 and 1995-1999. For the fake treatment in 2000, all four intervals were used as predictors, for fake treatment in 1995, the first three were used, and for fake treatment in 1990 the first two were used. The estimated fake treatment effects are listed in Table 5, along with their general p-values. Also, the yearly left-sided p-values for each test are plotted in Figures 7 through 9.

Both the general and yearly p-values suggest that neither of the in-time placebo tests were statistically significant. Even though insignificant effects are reassuring, the estimates are of noticeable magnitude, where the effect in 2000 even exceeds the treatment effect of the main estimation. Moreover, the other two effects are relatively big, and although they are closer to zero they constitute between 47.8 and 68.8 percent respectively of Australia’s treatment effect. The relative magnitude of these fake effects compared to the real effect in 2006 further limits the robustness of my findings. It weakens the identifying assumption, and makes it even more difficult to attribute the decrease in smoking prevalence between 2006-2012 to the actual policy implementation.

Table 5. Estimated fake treatment effects, and their respective p-values.

Year of fake treatment	Estimated effect	P-value
2000	-1.2756	0.6875
1995	-0.6036	0.8750
1990	-0.8685	0.8125

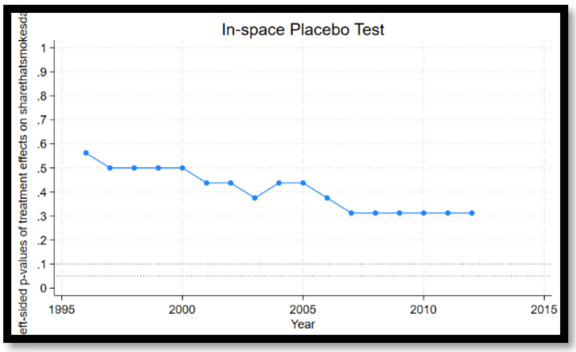

Figure 7. Yearly left-sided p-values for fake treatment in 2000.

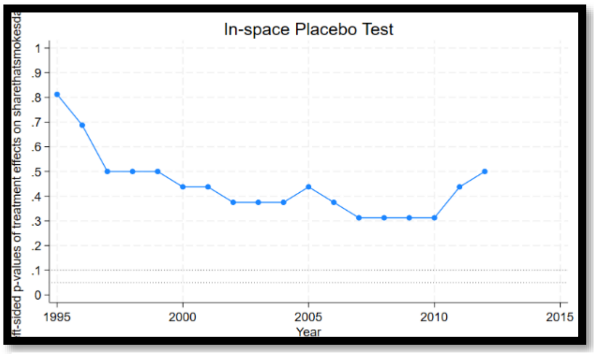

Figure 8. Yearly left-sided p-values for fake treatment in 1995.

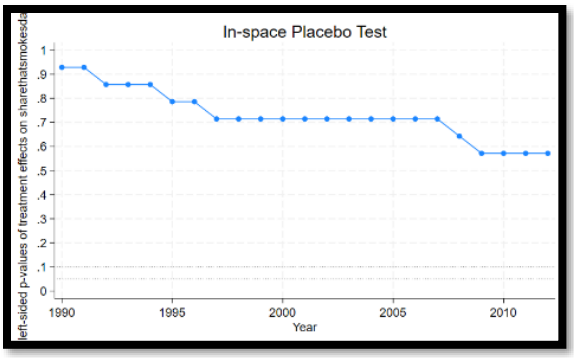

Figure 9. Yearly left-sided p-values for fake treatment in 1990.

5. Discussion

5.1 Interpretation of results

This paper examines the effect the Australian introduction of pictorial health warnings had on smoking prevalence, using the synthetic control method. The results point at a roughly five percent reduction in smoking prevalence over the seven year long post-intervention period, however, the findings are not robust to placebo tests, weakening the credibility of the study.

According to Kuehnle (2019), pictorial health warnings may affect smoking behavior through two different mechanisms: (1) increased attention and awareness and (2) changed beliefs and attitudes towards smoking. The observed reduction in smoking prevalence is in line with these expectations. While prior research suggests that PHWs lead to higher awareness, more negative attitudes, and greater knowledge about health risks (Fong, Hammond & Hitchman 2009, Francis et al. 2019 and Hammond 2011), my findings indicate that these mechanisms may translate to individuals actually reducing their cigarette consumption.

The results of the in-space and in-time placebo tests limit the robustness of my study and suggest that the findings are not statistically significant. Hence, the practical implications of my estimated effect should be interpreted with caution. Even though I cannot make a confident claim that the Australian policy had a desirable effect on smoking prevalence, my findings neither rule out the opposite, that the policy in reality didn't have any effect at all. To navigate in this ambiguity, consideration of what other studies have found constitutes helpful guidance, where the work by Kuehnle (2019) is especially relevant due to contextual similarities. First, his results, which are robust to multiple sensitivity checks, suggest that the policy had a negative overall effect on smoking prevalence. Specifically, he found smoking prevalence to be an average 4 percent lower over a 54 month long post-intervention period, compared to the pre-intervention mean. A a row of other studies also find PHWs to have a negative effect on smoking behavior (see Ibarra-Salazar, Romero Rojas & Ayala-Gaytán 2021, Huang, Chaloupka & Fong 2014, Fathelrahman et al. 2013, Azagba & Sharaf 2013 and Yong et al. 2013). Among these, only Huang, Chaloupka & Fong (2014) look explicitly at smoking prevalence as the outcome, and find a decrease between 12.1-19.6 percent over a nine year long post-intervention period. Taken together, the negative treatment effect estimated in my study corresponds to what prior research has found. Furthermore, based on the comparison to Kuehnle (2019), my estimated effect of five percent suggests PHWs to have a moderate impact on smoking prevalence. Even though Huang, Chaloupka & Fong (2014) found a considerably larger effect, their study examines a different context than this paper and Kuehnle (2019). Thus, their findings may reflect differences in policy design or country characteristics between the Australian and Canadian introduction of PHWs.

Second, the findings of Kuehnle (2019) suggest that the impact of the policy was realized quickly after the implementation, where a four percent decrease was observed within the year of the policy. This estimate is the average for both men and women above the age of 15 years, i.e. approximately the same group covered in my dataset. Even though I found an effect about half as large in 2006, the rapid decrease in smoking prevalence following the implementation is a similarity between my work and the paper by Kuehnle. Also, the rapid decrease aligns with the evidence of efficient compliance by cigarette manufacturers, which both Miller et al. (2009b) and White, Webster & Wakefield (2008) show.

Third, the role of wearing-out effects is an aspect that the literature on pictorial health warnings has highlighted. Miller et al. (2011) found a decreased ability among respondents to recall the warnings some years after implementation, while Kuehnle (2019) and Ibarra-Salazar, Romero Rojas & Ayala-Gaytán (2021) found a diminishing effect on quitting behavior and cigarette consumption, respectively. Although more post-intervention periods would be necessary to confirm a stagnation of the treatment effect in my case, my results bring suggestive evidence for it, which provides a final similarity to previous research.

Taken together, the findings of this study should not be interpreted as having any independent policy implication, due to low robustness and statistical significance. However, when considered alongside previous studies that correspond to my findings in different ways, they provide suggestive evidence that the Australian implementation might have contributed to a decline in smoking prevalence. Moreover, the findings suggest that the magnitude of the effect was modest, that it was realized quickly after implementation, and that it decreased over time.

It should be recognized that the overall documentation on the effects of PHWs on smoking behavior is still ambiguous, as Kuehnle (2019) and Monarrez-Espino (2014) underline. Hence, even though my findings correspond to some prior studies, it does not provide conclusive evidence. Nonetheless, pictorial health warnings are a relatively cheap tobacco-control measure, where all costs related to implementation are borne by the manufacturers, and thus does not induce any costs to governments or taxpayers (WHO 2014). As such, even if the Australian implementation of PHWs did not have the desired effect, the economic downside risk is not as large compared to other policies. This makes up a strong argument for the adoption of pictorial health warnings, and might explain why they have become so popular.

Also, research on non-tobacco labelling was addressed, specifically nutrition- and ecolabels. It was assessed that there is strong evidence on the behavioral effectiveness of nutrition labels (Shangguan et al. 2018, Campos, Doxey & Hammond 2011, Aranda, Darden & Rose 2021, Variyam 2008 and Restrepo 2016). However, it is less clear whether ecolabels effectively alter consumer food choices (Tiboni-Oschilewski et al. 2024). This difference may be because the latter don't have direct consequences on consumers' personal health, compared to nutrition labels. In this regard, nutrition labels and PHWs function in a similar way, which corresponds with my findings and the strong evidence on nutrition labelling.

5.2 Limitations

This study has limitations that should be considered when interpreting the results. First, changes in other tobacco-control related aspects imply entangling effects that may have influenced my estimate. Even though Kuehnle (2019) presents arguments suggesting that the more prominent Quitline reference and the mass media campaigns didn't have noteworthy independent effects on smoking prevalence, these aspects cannot be formally disentangled from the effect of pictorial health warnings. Hence, as Kuehnle also notes, the estimated treatment effect of this study should be interpreted as the joint impact of PHWs, prominent references to a cessation line and mass media campaigns.

Second, the quality of pre-treatment fit between Australia and the synthetic control group is somewhat disputed. The credibility of the synthetic control method depends on a good pre-treatment match, as this supports the identifying assumption that the post-intervention trajectory of the synthetic control represents the counterfactual outcome of the treated group. On the one hand, the gap in smoking prevalence between real and synthetic Australia changed to a greater extent over the first ten years of the study than what was seen following the intervention, which makes the identifying assumption less plausible to hold. On the other hand, a close pre-treatment fit was observed from the early 1990s up to 2006, which rather supports the credibility. This becomes particularly compelling given the clear divergence between the two groups immediately following the implementation in 2006. However, as highlighted in the literature on the synthetic control method, a close match over an extended period is needed for the pre-intervention fit to be regarded as satisfactory. Taken together, I thus conclude that the pre-treatment fit in my study weakens the overall credibility of the findings.

Finally, this study uses estimated values of smoking prevalence as the outcome variable, rather than direct measurements. This should be taken into account as estimated values may not accurately reflect the actual outcomes, and hence may induce some measurement error. Furthermore, a longer post-intervention time period would be desirable to enable examination of long-term effects. For example, it would provide clearer documentation on the role of wearing-out effects of pictorial health warnings, an aspect that my findings only can bring suggestive evidence for. However, because of both data and contextual limitations, including more post-treatment years in the analysis was not feasible. First, outcome data only extended to 2012, and second, including more years would demand me to exclude several countries from the donor pool, which potentially would harm the estimation. Finally, in 2012 plain packaging became mandatory in Australia, which would have interfered with the results.

5.3 Future research

Moving forward, studies should focus on the behavioral effects of pictorial health warnings, as this is a shortcoming of the existing literature. The synthetic control method holds great potential in research of public health interventions. Hence, if appropriate circumstances can be identified, in which the contextual requirements are fulfilled, the SCM may prove effective in estimating the causal effect of PHWs. Also, with additional time and resources, the analysis can be improved by testing multiple specifications with different donor units and matching predictors. With that being said, it should be noted that entangling effects so far have made it difficult for observational studies to isolate the individual effect of PHWs, an issue that also would restrict the applicability of the synthetic control method. This may also explain the wide use of experimental study designs, which are especially popular when the outcomes are related to psychological aspects. Even though it might not be feasible to use real-world experiments to examine behavioral outcomes such as smoking prevalence at the country-level, future research could perhaps apply them to other contexts. One approach is to use RCTs by looking at people who already smoke. For example, smokers could be randomly assigned to either smoke cigarettes from packages with text-only warnings or PHWs. After being provided with the respective type of packages for some period of time, outcomes such as the number of cigarettes smoked per day could be compared between the two groups.

6. Conclusions

This study asked how the 2006 implementation of pictorial health warnings affected smoking prevalence in Australia. PHWs have become widely embraced around the world, however, the evidence on their behavioral effects is restricted. It is here the study aimed to make its main contribution, as reducing consumption ultimately is the goal of policy makers.

Using the synthetic control method, a counterfactual was constructed by taking the weighted average of 15 OECD countries that did not implement pictorial health warnings under the period of the study. The goal of this group was to represent the outcome in smoking prevalence in Australia if no policy had been implemented, and thus, to provide a causal interpretation of the average treatment effect. This effect was estimated to be -1.2631, suggesting that over the seven year long post-intervention period, smoking prevalence decreased by 1.2631 percentage points, which compared to average pre-intervention levels implied a relative decrease of approximately five percent.

The observed reduction aligns with theoretical frameworks pointing at the psychological mechanisms through which PHWs are expected to affect consumption. Furthermore, the results correspond to the findings of previous papers also looking at the effects of PHWs on smoking behavior. However, the independent practical implications of the estimate are limited, due to low robustness and credibility. Both the in-space and in-time placebo tests, as well as the overall quality of the pre-intervention fit between Australia and its synthetic control, make the identifying assumption less plausible to hold. Also, the Australian introduction of pictorial health warnings came with other tobacco-control measures, specifically, a more prominent Quitline reference and mass media campaigns. These effects could not be methodically disentangled from the effect of PHWs, and thus may have influenced my results. Taken together, I conclude that a causal interpretation of my estimated effect is not possible.

With that being said, the synthetic control method may be an efficient tool for evaluating the effect of tobacco-control policies on national smoking levels, provided it is conducted in the right way. This is due to the aggregate nature of the context, with few and large entities, infrequently occurring treatment, and outcomes measured at an aggregate level. If future research can identify situations in which desirable donor units are available, and where entangling effects are minimal, this approach may even be able to estimate the causal effect of pictorial health warnings themselves. In this way, the SCM could provide clear evidence on the actual effects of PHWs, information that is vital for policy makers in their future attempts to tackle the tobacco epidemic.

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