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Abstract

South Africa has since 1994 consistently and aggressively increased excise taxes on cigarettes in order to maintain a total tax burden of around 50% of the average retail selling price. The tax rises have translated into large increases in the inflation-adjusted price of cigarettes. For instance, the average real price per pack increased by 110% between 1994 and 2004. This paper uses a transparent and data-driven technique, the Synthetic Control method, to evaluate the impact on cigarette consumption of South Africa's large-scale tobacco tax increases. We find that per capita cigarette consumption would not have continued declining in the absence of the consistent tax rises that began in 1994. Specifically, we find that by 2004, per capita cigarette consumption was 36% lower than it would have been had the tax increases not occurred. Our treatment effect estimates survive a series of placebo and robustness tests.

1 Introduction

South Africa has since 1994 aggressively and consistently increased the excise tax on cigarettes so as to meet and maintain a total tax burden (including Value Added Tax) of 50% of the average retail selling price. The target was met in 1997 and revised upwards to 52% in 2004. The tax rises have translated into substantial increases in the inflation-adjusted retail selling prices of cigarettes. For instance, the average real price per pack of cigarettes increased by 110% between 1994 and 2004 and by 190% if one extends the period to 2012 (see Figure 1). The increase in prices has coincided with substantial declines in prevalence and consumption. Van Walbeek (2005) estimated that prevalence declined from 31% of the adult population in 1993 to 24% in 2003 while aggregate cigarette

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consumption and per capita consumption declined by 32% and 46% respectively over the same period.

Declines in prevalence and consumption were well underway by the time the tax increases began in 1994 (Van Walbeek, 2002; 2005). In the absence of a credible counterfactual (a what-if scenario), the impact of taxes on consumption and prevalence is likely to be overstated. The literature on evaluating the impact of South Africa's aggressive tobacco control efforts is not very extensive.

This paper uses a transparent data-driven technique, the Synthetic Control method developed by Abadie and Gardeazabal (2003) and extended in Abadie *et al.* (2010), to create a credible counterfactual of cigarette consumption in South Africa from 1994 to 2004. The counterfactual is constructed as a weighted average of the per capita cigarette consumption of countries similar to South Africa that did not initiate large-scale tobacco control measures over the period 1994 to 2004. Using this counterfactual, we are able to estimate a "treatment effect" of South Africa's tax increases on cigarette consumption. We find that per capita cigarette consumption would not have continued declining in the absence of the consistent tax and price rises that began in 1994. Specifically, we estimate a treatment effect of 36% by 2004. That is, per capita cigarette consumption in 2004 was 36% lower than it would have been had the government not consistently increased excise taxes in the preceding years.

The rest of this paper is structured as follows: Section 2 provides some background to South Africa's tobacco control measures. Section 3 reviews the literature evaluating tobacco control measures in South Africa and in other parts of the world. Section 4 describes the Synthetic Control method in some detail and Section 5 describes the data. Section 6 discusses the selection of the control countries (what we call the donor pool) while Section 7 presents the main results and conducts placebo tests. We present the results of the robustness tests in Section 8 while Section 9 discusses what implications, if any, illicit trade has for my estimates of the treatment effect. Section 10 concludes.

2 Tobacco Control in South Africa

Prior to 1994, South Africa did not consciously target the consumption of tobacco products on public health grounds. According to Van Walbeek (2005), the relegation of public health concerns in tobacco tax policy was likely due to the cordial relations that existed between the tobacco industry and the National Party, the party that ruled South Africa from 1948 to 1994. The end result was that the real tax on cigarettes, the main tobacco product in South Africa, declined by 70% between 1961 and 1990 (ibid.). Coincidentally, per capita cigarette consumption increased by 60% from 50 packs in 1961 to 80 packs in 1991 (ibid.).

In the 1980s and early 1990s, the medical research community (Yach, 1982) and the South African Medical Research Council (1988, 1992) published research showing that tobacco consumption imposed a net cost on the country. For instance, the 1992 study by the South African Medical Research Council

(SAMRC) estimated the costs of tobacco consumption at 1.82% of GDP against benefits of 0.49% of GDP (SAMRC, 1992). The publicity generated by these studies rallied the public health community and civil society behind the common goal of getting the South African government to take tobacco control seriously. The momentum that had built up during the 1980s and early 1990s, along with the impending change of government, culminated in the passing of the Tobacco Products Control Act of 1993 by Parliament.[1] The big turning point, however, came in 1994 when the new African National Congress-led government announced that the government would target a tax burden on cigarettes (including Value Added Tax) of 50% of the retail price to be phased in over a number of years (Republic of South Africa, 1994). As a result, 1994, 1995 and 1996 saw excise tax increments of respectively 25%, 25% and 18% (Republic of South Africa, 1994, 1995, 1996). In 1997, the Minister of Finance announced a large increase of 52% in the excise tax on cigarettes, a move that was expected to bring the total tax burden (including Value Added Tax) to 50% of the average retail selling price (Republic of South Africa, 1997). From 1997, the annual increases on excise taxes on cigarettes have, therefore, been predictable in order to maintain the stipulated tax burden.[2] In 2004, the total tax burden was revised upwards to 52% of the average retail selling price (Republic of South Africa, 2004).

South Africa's aggressive excise tax policy since 1994 has translated into substantial increases in the real price of cigarettes (see Figure 1). From 1994 to 2012, the average real price per pack of cigarettes increased by 190%. Between 1994 and 2004, which is the period we evaluate in this paper, the increase in the real price per pack was 110%. This is in stark contrast to the period before 1994 which saw considerable declines in the real excise tax on cigarettes and in the real price of cigarettes. It is this unprecedented increase in real cigarette prices, beginning in 1994, whose impact on consumption we seek to evaluate in this paper.

3 Literature review

The literature evaluating the impact of South Africa's tax increases since 1994 on prevalence and consumption is not very extensive. Van Walbeek (2002, 2005) investigated the impact of the tax increases on prevalence and consumption by fitting a linear trend to the All Media and Products Survey (AMPS), which is a commercially generated dataset. He estimated that smoking prevalence in South Africa declined from 31% of the adult population in 1993 to 24% in 2003. He also found that African and Coloured population groups experienced the biggest declines in prevalence over the same period. In terms of consump-

[1] Saloojee (1994), Malan and Leaver (2003) and Van Walbeek (2005) contain detailed accounts of the events and debates leading up to the adoption of the Tobacco Products Control Act of 1993.

[2] Because the industry responds by increasing retail prices, the tax burden is always slightly less than the government's target (see Van Walbeek, 2005, 2006).

tion, Van Walbeek (2005) found that aggregate consumption declined by 32% over the period 1993 to 2004 whereas per capita consumption declined by 46%. Boshoff (2008) estimated a vector autoregression (VAR) in an attempt to assess the relative importance of price changes, income changes and general health awareness in influencing cigarette demand over the period 1996 to 2006. He estimated demand elasticities for price, income and health awareness and found that all three factors were important in influencing cigarette demand over the period 1996 to 2006. Other work has instead focussed on estimating the impact of the tax increases on illicit trade (Blecher, 2010, 2011; Van Walbeek, 2014) and on the impact of the Tobacco Products Control Amendment Act of 1999 on restaurant revenues (Blecher, 2006; Van Walbeek *et al.*, 2007).

An implicit assumption in the South African literature that evaluates tobacco control measures is that pre-intervention trends in, say, prevalence and consumption would have continued in the absence of the intervention. Given this, impact can be assessed by comparing present day consumption or prevalence with the magnitudes of these variables before the onset of treatment. That pre-intervention trends would have continued without treatment is not directly evident for consumption and prevalence. The two variables were already declining by the time the government introduced its new tax policy on tobacco products (Van Walbeek, 2002, 2005). Any evaluation that does not attempt to create a counterfactual is likely to give a biased estimate of the impact of a particular tobacco control measure.[3]

Internationally, researchers in the United States (US) have made progress in evaluating tobacco control initiatives by using counterfactuals. Warner (1977) was one of the first US studies to do this. He estimated a regression of per capita cigarette consumption on cigarette price and other covariates using time series data from before the Surgeon-General's 1964 report and the ensuing anti-smoking campaigns. He then used the estimated coefficients from the pre-treatment period to predict what per capita cigarette consumption would have been like in the absence of the campaigns. The treatment effect was then calculated as the difference between predicted consumption and actual consumption in the treatment period. Warner's conclusion was that per capita cigarette consumption would have been 30% higher in 1975 had the anti-smoking measures not happened. Implicit in Warner's study was the assumption that the pre-intervention regression coefficients remained stable even after the intervention. That this assumption was unlikely to hold, in general settings, was pointed out by Lucas (1976) in his important critique of econometric models of policy evaluation.

More recent work in the US has focussed on developing methods of conducting policy evaluations that avoid some aspects of Lucas's critique. For instance, Fichtenberg and Glantz (2000) evaluated California's tobacco control programme by comparing rates of change in per capita cigarette consumption in California against rates of change in the rest of the US. They found that after the introduction of the programme in 1989, California's rate of decline in

[3] Koch and Tshiswaka-Kashalala (2008, p2) make a similar point.

per capita cigarette consumption exceeded that of the rest of the US by 2.72 packs per year. A critique of the method in Fichtenberg and Glantz (2000) is that treatment effects were underestimated since the rest of the US included states that, alongside California, had also implemented some tobacco control measures.[4] The method in Abadie *et al.* (2010), which we describe fully below, attempts to correct for this shortcoming by comparing California to only those states that did not implement large-scale tobacco control measures after 1989.

4 Method

This paper uses the method developed by Abadie and Gardeazabal (2003) and extended further in Abadie *et al.* (2010) to evaluate South Africa's tobacco control policies from 1994 to 2004. The method involves estimating South Africa's counterfactual cigarette consumption trend line following the consistent hikes in cigarette excise taxes that began in 1994. In other words, the method involves creating a synthetic South Africa, a country that looks like South Africa in all relevant respects except for the tax hikes. The observed outcome variable for the "real" South Africa is then compared to the outcome variable for the synthetic South Africa. In this section we discuss in some detail the formal aspects of the method.

4.1 Identification

Suppose we have $J+1$ regions and region 1 experiences a policy change and is therefore referred to as the "treated" region. The remaining J regions do not experience the policy change and since we use these regions to construct a counterfactual scenario for the treated country, we collectively refer to them as the "donor" pool. The policy change happens at time period T_0 where $1 \leq T_0 < T_0 + P$ with P being the number of time periods after treatment. In the case of South Africa, $P = 10$ and $T_0 = 1994$ (Below we motivate why we choose to end the evaluation 10 years after 1994). The outcome variable of interest is Y_{it} with $i = 1, 2, ..., J+1$ and $t = 1, ..., T_0 + P$. For any region i and time period t, we can define Y_{it}^I and Y_{it}^N .Y_{it}^I is the observed outcome variable and Y_{it}^N is the outcome variable in the absence of treatment (the superscripts I and N are chosen to represent respectively "intervention" and "no intervention"). That is, Y_{it}^N is unobserved after T_0 but is equal to Y_{it}^I before T_0. Given this, we can then define the treatment effect of the policy change, α_{it},as:

$$\alpha_{it} = Y_{it}^I - Y_{it}^N \tag{1}$$

for $t = T_0 + 1, \ldots, T_0 + P$. The complication is that Y_{it}^N is unobserved for all $t > T_0$.. In order to estimate the effect of the policy change, we need to estimate Y_{it}^N after treatment. Suppose Y_{it} evolves according to the equation

[4] For example, Alaska, Hawaii, Maryland, Michigan, New Jersey, New York and Washington had raised their state cigarette taxes by at least 50 US cents over the period 1989 to 2000 (Abadie *et al.*, 2010).

$$Y_{it} = \lambda_t + \boldsymbol{\theta}_t \mathbf{Z}_i + \boldsymbol{\delta}_t \boldsymbol{\mu}_i + \varepsilon_{it} \tag{2}$$

where λ_t is some factor common to all regions, $\mathbf{Z}_i$ is a vector of observed factors and $\boldsymbol{\mu}_i$ is a vector of unobserved factors that have an impact on Y_{it}. $\boldsymbol{\theta}_t$ and δ_t are the unknown time varying parameters associated with $\mathbf{Z}_i$ and $\boldsymbol{\mu}_i$ respectively[5]. ε_{it} is the unobserved error term with mean zero. Given a donor pool and a $J \times 1$ vector of weights $W = (w_2, \ldots, w_{j+1})'$ such that $w_j \geq 0$ and $w_2 + w_3 + \cdots + w_{j+1} = 1$, we can construct for any i

$$\sum_{j=2}^{J+1} w_j Y_{jt} = \lambda_t + \boldsymbol{\theta}_t \sum_{j=2}^{J+1} w_j \mathbf{Z}_j + \boldsymbol{\delta}_t \sum_{j=2}^{J+1} w_j \boldsymbol{\mu}_j + \sum_{j=2}^{J+1} w_j \varepsilon_{it} \tag{3}$$

That is, we can always express the outcome variable of a treated region as a weighted average of the regions in the donor pool. For $i = 1$ (i.e. the treated country), Abadie and Gardeazabal (2003) and Abadie *et al.* (2010) show that there exists a $J \times 1$ vector of weights $\mathbf{W}^* = (w_2^*, \ldots, w_{J+1}^*)'$ with $w_2^* + w_3^* + \ldots w_{J+1}^* = 1$ and $w_j^* \geq 0$ such that

$$\begin{aligned}
\sum_{j=2}^{J+1} w_j^* Y_{j1} &= Y_{11} \\
\sum_{j=2}^{J+1} w_j^* Y_{j2} &= Y_{12} \\
&\vdots \\
\sum_{j=2}^{J+1} w_j^* Y_{jT_0} &= Y_{1T_0} \textit{ and} \\
\sum_{j=2}^{J+1} w_j^* \mathbf{Z}_j &= \mathbf{Z}_1
\end{aligned} \tag{4}$$

That is, we can exactly recreate the pre-treatment characteristics of the treated region using only the donor pool and the weights in $\mathbf{W}^*$[6]. Since the factors in $\boldsymbol{\mu}_i$ are unobserved, we cannot create their empirical counterparts in equation (4). However, if the set of equations in (4) hold exactly, then

[5] Notice that $\mathbf{Z}_i$ and $\boldsymbol{\mu}_i$ do not have time subscripts. We can think of their values as fixed over short periods of time but still allow for their effects, via $\boldsymbol{\theta}_t$ and $\boldsymbol{\delta}_t$ respectively, to vary across time. The method also allows for more general specifications of $\mathbf{Z}_i$ and $\boldsymbol{\mu}$ with time subscripts.

[6] Appendix B of Abadie *et al.* (2010) contains the mathematical proofs related to this point.

$$\sum_{j=2}^{J+1} w_j^* \boldsymbol{\mu}_i = \boldsymbol{\mu}_1 \tag{5}$$

also holds (Abadie *et al.*, 2010, p495). Having recreated the pre-treatment characteristics of the treated country using the donor pool, we can then use the same linear combination of regions to trace out the time path of the outcome variable after treatment. This time path is the outcome variable we would have observed for the treated region in the absence of treatment (the counterfactual). The difference between the counterfactual trend line and the actual trend line is then an estimate of the treatment effect. Formally, given equations (4) and (5), the treatment effect estimator for $i = 1$ is

$$\hat{\phi}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt} \tag{6}$$

for $t > T_0$ and $j = 2, 3, \ldots, J$[7].

The treatment effects estimator in equation (6) is a generalized version of the standard difference-in-difference estimator (Angrist and Pischke, 2009, p227 – 243). Whereas the standard difference-in-difference estimator assumes that the effects of the unobserved factors are fixed and therefore can be "differenced" out, (6) allows for them to be time varying. This is an attractive property given that the impact of most factors is likely to change over time as opposed to remaining fixed. In addition, the treatment effects estimator in equation (6) is a dynamic estimator that gives us the treatment effect at each point in time after treatment. The standard difference-in-difference estimator only gives a static average treatment effect. Further, Abadie *et al.* (2015) show that the Synthetic Control estimator in (6) is related to the standard regression estimator in the sense that both apply the idea of weights that sum to one.[8] The only difference is that the Synthetic Control estimator restricts the weights to be non-negative, whereas the regression estimator places no such restriction on the weights. Not placing this restriction allows regression to perfectly fit a counterfactual even when the data does not allow for one. In more technical terms, regression allows extrapolation outside the support of the data whereas the synthetic control estimator can only perfectly fit a counterfactual if the data allows it to do so. Extrapolating from outside of the support of the data makes regression susceptible to the problem of "extreme counterfactuals" (King and Zeng, 2006).

The equations in (4) are unlikely to hold exactly in practise. It is, therefore, desirable to get as close approximations to these equations as possible. One of

[7] For $t < T_0, \hat{\phi}_{1t} = 0$.That is, before treatment, the treatment effect is zero.

[8] Recall that the standard regression estimator for the model $\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \mathbf{u}$, is $\hat{\boldsymbol{\beta}} = (\mathbf{X}^{'}\mathbf{X})^{-1}\mathbf{X}'\mathbf{y}$. The estimate of the treatment effect (or the predicted outcome) is then $\hat{\boldsymbol{y}} = \mathbf{X}\,\hat{\boldsymbol{\beta}}$. But since $\hat{\boldsymbol{\beta}} = (\mathbf{X}^{'}\mathbf{X})^{-1}\mathbf{X}'\mathbf{y}$, the treatment effect is obtained by applying the weight $\mathbf{X}(\mathbf{X}^{'}\mathbf{X})^{-1}\mathbf{X}'$ on the outcome vector $\mathbf{y}$. Further technical details related to this point are contained in Abadie *et al.* (2015).

the ways of assuring this is to have a donor pool of regions that share a "common support" with the treated region. In other words, the outcome variable for the regions in the donor pool should be influenced by the same factors as the outcome variable for the treated region. That is, the outcome variable for both types of regions should evolve according to equation (2). Secondly, the treated region should be contained within the set of all linear combinations of the donor pool. This is technically known as the "convex hull" requirement (King and Zeng, 2006). These two conditions essentially require the treated region to not be too extreme relative to the regions in the donor pool. In any case, the degree of pre-treatment discrepancy between the treated country and its synthetic counterpart can be assessed by calculating the Root Mean Square Error (RMSE) as:

$$RMSE = \left(\frac{1}{T_0} \sum_{t=1}^{T_0} (Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt})^2 \right)^{\frac{1}{2}} \tag{7}$$

A large RMSE would suggest a poor pre-treatment fit between the treated region and its synthetic counterpart. Using the Synthetic Control Method in this situation would not be advisable.

$\mathbf{W}^*$ (the vector of optimum weights) is chosen as the solution to the following constrained optimization problem:

$$\min_{\mathbf{w} \epsilon \mathbf{M}} \parallel \mathbf{X}_1 - \mathbf{X}_0 \mathbf{W} \parallel = \sqrt{(\mathbf{X}_1 - \mathbf{X}_0 \mathbf{W})' \mathbf{V} (\mathbf{X}_1 - \mathbf{X}_0 \mathbf{W})} \tag{8}$$

such that $w_j \geq 0$ and $w_2 + w_3 + \cdots + w_{j+1} = 1$

where $\mathbf{X}_1$ is a matrix of pre-intervention characteristics of the treated region (including Y_{1t} and Z_1) and $\mathbf{X}_0$ is a matrix of the same pre-intervention characteristics for the regions in the donor pool. $\mathbf{M}$ is the set of all vectors satisfying the requirement that their elements sum to one and are non-negative[9] and $\mathbf{V}$ is some diagonal matrix whose diagonal elements weight factors in $\mathbf{Z}_1$ according to how well they predict the outcome variable Y_{it}. The problem in (8) seeks to minimize, by selecting $\mathbf{W}^*$, a measure of distance between the treated region and the donor pool.[10] The minimization problem in (8) can be solved numerically in Stata using the *Synth* routine.[11]

4.2 Inference

In order to ensure that the treatment effect identified in equation (6) is not due to random chance, Abadie *et al.* (2010, 2015) suggest inferential techniques based on the idea of placebo tests. They suggest constructing synthetic counterparts for all the regions in the donor pool, one at a time, and for each region estimating a treatment effect according to equation (6). This exercise results in

[9] For instance, $\mathbf{M}$ might contain a vector with the following elements (1 0 0 ... 0) or another vector with elements (0.5 0 0 ... 0.5) and so on.

[10] Recall that $\parallel\parallel$ is the Euclidean norm or Euclidean metric, a distance function.

[11] Available from Jens Hainmueller's website at http://web.stanford.edu/∼jhain/synthpage.html

the construction of an empirical distribution of treatment effects similar to the student's t distribution. The identified effect for the treated region is statistically significant (i.e. not due to chance) if the probability of obtaining an effect as large as that of the treated region, in the empirical distribution of treatment effects, is small. In other words, the effect for the treated region is statistically significant if the number of donor regions that show a treatment effect, even after receiving a placebo, is small.[12]

4.3 Implementation

In terms of implementing the method for South Africa, we follow the approach in Abadie *et al.* (2010). Y_{it}, the outcome variable, is cigarette consumption per capita (in sticks). The vector $\mathbf{Z}_1$ comprises of the standard predictors of cigarette demand found in the literature (Chaloupka and Warner, 2000; IARC, 2011). The variables in $\mathbf{Z}_1$**,** include the following : the real price of a pack of cigarettes, real Gross Domestic Product per capita (real GDP per capita), alcohol consumption per capita (expressed in litres of pure alcohol) and the proportion of adults in the total population. $\mathbf{Z}_1$ also includes lagged values of per capita cigarette consumption to capture some aspect of habit formation (Warner, 1977; Chaloupka, 1991). The data sources for all these variables are discussed in detail in Section 5 below.

Our choice of conducting the evaluation over the period 1994 to 2004 is due to the World Health Organization's Framework Convention on Tobacco Control (FCTC) which came in to effect in 2005. The treaty encourages countries to implement a wide array of tobacco control measures. We, therefore, expect that most of the countries in our donor pool began, from 2005 onwards, to think seriously about tobacco control, a situation that might result in a downward bias in our treatment effect estimates. Further, Abadie *et al.* (2010, 2015) consider a ten year period to be a sufficient timespan to properly evaluate the effects of a policy change.[13]

The Synthetic Control method has gained prominence after being favourably reviewed by Imbens and Wooldridge (2009) in their extensive survey of the impact evaluation literature. It has been used to assess episodes of economic liberalization across the world (Billmeier and Nannicini, 2013), to quantify the economic costs of conflict in Spain (Abadie and Gardeazabal, 2003) and the economic effects of reunification in Germany (Abadie *et al.*, 2015). From a public health perspective, the method has been used to evaluate California's Tobacco Control Programme (Abadie *et al.*, 2010), to quantify the health benefits of the liberalization of the sex trade in the US state of Rhode Island (Cunningham and

[12] This idea is borrowed from medical trials, where patients receiving a placebo are not expected to show results that are similar to patients receiving the actual drug, if the drug is effective.

[13] In their 2010 paper on California's tobacco control initiative, Abadie *et al.* evaluate the initiative's effect for the period running from 1989 to 2000. In their 2015 paper on the economic effects of reunification on West Germany's economy, Abadie *et al.* conduct the evaluation over the period 1990 to 2000.

Shah, 2014) and to estimate the effect of bar closing times on traffic accidents in the United Kingdom (Green *et al.*, 2014).

5 Data

The data used in this paper come from a number of sources. Data on the outcome variable, cigarette consumption per capita (in sticks), come from the World Cigarette Report published by the ERC Group (ERC, 2010). The ERC Group is an independent research company that compiles market intelligence data on an annual basis on a number of products, including cigarettes. The country coverage of the World Cigarette Report is extensive and also contains complete time series on cigarette consumption from 1990 to 2009. Consumption data from the report has been used previously by Blecher (2011) to investigate the impact of advertising bans on cigarette consumption.[14]

Cigarette price data is from the Economist Intelligence Unit's (EIU's) Worldwide Cost of Living Survey. The survey has been collecting cigarette price data alongside the price of other goods and services for 140 cities since 1990.[15] For cigarettes, prices are collected semi-annually from supermarkets, medium-priced retailers and more expensive specialty stores for two brands: Marlboro (or the nearest international equivalent) and the cheapest local brand (or the cheapest brand in the absence of a local brand). We follow Blecher and Van Walbeek (2004, 2009) and Blecher (2008, 2011) and use the price of a pack of the cheapest brand. This is because the cheapest brand is usually the most popular brand in a country and consequently its price is the most representative. The price data is expressed in constant 2000 US dollars using the United States Consumer Price Index City Average for All Items (United States Department of Labour).[16] A drawback of using the EIU price data is that cigarette prices are only collected from a few cities (sometimes only a single city) within a country. This might reduce the representativeness of the price data.

GDP per capita and data on the proportion of adults (16 to 64 years) in the population come from the World Bank's World Development Indicators database.[17] GDP per capita is expressed in constant 2000 US dollars. Finally, data on alcohol consumption per capita (in litres of pure alcohol) comes from the World Health Organization's Global Information System on Alcohol and Health.[18]

[14] An alternative data source for consumption is the Tobacco Country Profiles available from the World Health Organization (WHO) at http://www.who.int/tobacco/surveillance/policy/country_profile/en/. Unfortunately, and as noted by Blecher (2011, p139), the Tobacco Country Profiles do not contain complete consumption series for the time periods that we are interested in.

[15] For more see: http://www.eiu.com/handlers/PublicDownload.ashx?fi=data-section/worldwide-cost-of-living.pdf&mode=m

[16] Available at www.bls.gov

[17] Available at http://data.worldbank.org/data-catalog/world-development-indicators

[18] Available at http://apps.who.int/gho/data/node.main.GISAH

6 Selection of the Donor Pool

The validity of the Synthetic Control method relies on the selection of a donor pool that meets the following set of criteria: (i) the common support requirement, (ii) the convex hull requirement and (iii) regions in the donor pool should not have experienced treatment during the relevant time period. In selecting an appropriate donor pool, we begin by addressing the third requirement and then work backwards to (i) and (ii).

In order to select a donor pool consisting of untreated countries, we rely on the work on cigarette affordability by Blecher and Van Walbeek (2004, 2009). Blecher and Van Walbeek propose a measure of cigarette affordability, the Relative Income Price (RIP), which is calculated as the ratio of the cost of 100 packs of cigarettes in a country to that country's real GDP per capita. A declining RIP means that cigarettes are becoming more affordable while a rising RIP signifies declining affordability. In their 2009 paper, Blecher and Van Walbeek were able to classify 77 countries according to whether they experienced increasing affordability or declining affordability over the period 1990 to 2006. These were countries for which the authors were able to obtain complete and comparable data on real cigarette prices and real GDP per capita over the period 1990 to 2006. The authors identified 37 countries where cigarettes became more affordable over the period 1990 to 2006.[19] For 20 out of the 37 countries, the increase in affordability occurred because of a decrease in the real price of cigarettes coupled with an increase in real GDP per capita. For the remaining 17 countries, the increase in affordability was due to real GDP per capita growing faster than the increase in real prices.

We opt to use the increase in affordability over the period 1990 to 2006 as a proxy for the absence of treatment. That is, we regard countries whose affordability increased on average over this period as not having enacted significant tobacco control measures. This is obviously the case for the 20 countries where affordability increased as a result of declining real cigarette prices. We contend, however, that even for the remaining 17 countries where affordability increased due to real incomes growing faster than real prices, a conclusion of the absence of treatment is a reasonable one to make. This is because effective tobacco control measures require (i) real tax/price increases and (ii) real tax/price increases that grow faster than the rate of growth in incomes (WHO, 2010; IARC, 2011). We also recognise that the Relative Income Price (RIP) might have some shortcomings in identifying whether a country has instituted tobacco control measures or not. For instance, a country may have adopted a wide set of tobacco control measures such as advertising bans and/or clean indoor air policies but neglected to significantly increase real cigarette prices. Our measure of treatment would consign this country to the pool of potential donor countries in spite of its tobacco control efforts. In as much as we recognise that tobacco control measures constitute more than just tax/price measures, the tobacco control literature recognises the primacy of tax/price policies in curbing

[19] See Figure 4 in Blecher and Van Walbeek (2009).

demand (Chaloupka and Warner, 2000; IARC, 2011). In any case, we would consider our estimates of the treatment effect to be lower bound estimates if the donor pool had some countries whose treatment status was misclassified in the manner suggested above.

An alternative approach would be to determine treatment status based on the Tobacco Country Profiles available from WHO.[20] Unfortunately, the country profiles are often not clear as to whether the listed tobacco control measures have been implemented effectively or not. Further, the country profiles often provide the analyst with lots of room for discretion in classifying treatment status. On the other hand, the Relative Income Price (RIP) measures outcomes and not the intent of treatment. Secondly, the RIP, in using a rigid decision rule, leaves the analyst with little room for discretion and in this way limits errors due to misclassification. Lastly, the procedure of assigning treatment based on the RIP is transparent, a hallmark of the Synthetic Control method.

Our criterion for identifying treatment correctly classifies many of the countries that are known for having instituted significant tobacco control measures over the period 1990 to 2006. For example, South Africa, the country of interest in this paper, is correctly classified as treated since its Relative Income Price (RIP) increased (i.e. affordability declined) on average over the period 1990 to 2006. Thailand, a country whose positive experience with tobacco control is often held up as a model for other developing and emerging countries (Levy *et al.*, 2008; Sangthong *et al.*, 2012), is also classified as having undergone treatment. Most of the developed countries, whose tobacco control efforts predate the 1990s, are also classified correctly as treated. On the other hand, the list of untreated countries consists mainly of developing and emerging countries, an expected outcome given these countries' slow progress in implementing effective tobacco control measures over the period 1990 to 2004 (Jha and Chaloupka, 2000). The full list of treated and untreated countries from Blecher and Van Walbeek's 2009 paper are contained in Table A1 in the appendix.

Having identified the potential donor pool, we need to ensure that the common support and convex hull requirements are met. The two requirements are readily satisfied by excluding from the potential donor pool in Table A1 countries that are dissimilar to South Africa in some fundamental way. One of the most transparent ways of ensuring this is to use the World Bank's Country Classification System based on per capita income.[21] We rely on Blecher and Van Walbeek's (2009) usage of the Classification System as it stood at the time of writing their paper and exclude from the donor pool all high income countries.[22] These countries are often perceived as being structurally different in many respects to Low- and Middle-Income countries such that including them in the donor pool would risk violation of the convex hull and common support requirements. Lastly, we drop from the potential donor pool countries without

[20] Available at http://www.who.int/tobacco/surveillance/policy/country_profile/en/

[21] Available at http://data.worldbank.org/news/2015-country-classifications

[22] This results in the exclusion of Kuwait, Bahrain, Czech Republic, Ireland, Denmark, Greece, Finland, Luxembourg and Norway.

a complete set of data for all variables over the period 1990 to 2004.[23] The final donor pool consists of 24 countries which are listed in Table 1.

The final donor pool consists of countries that are often thought of as South Africa's peers. The list contains Latin American, sub-Saharan African, North African and South-East Asian countries. The donor pool also contains three countries from the BRICS group (Brazil, India and China).[24] The BRICS countries are often thought of collectively as the vanguard of emerging economies.

7 Main results

This section presents the main results of implementing the Synthetic Control method for South Africa using the donor pool listed in Table 1.

7.1 Treatment effects

Table 2 presents the results of the solution to the minimization problem stated in equation (8). According to Table 2, synthetic South Africa is a linear combination of 27.6% of Argentina, 47.6% of Brazil, 14.6% of Chile, 0.7% of Romania and 9.4% of Tunisia. In other words, this combination of countries with their respective weights, produces the lowest pre-treatment root mean square error (RMSE) between the actual South Africa and its synthetic counterpart. The pre-treatment RMSE between the actual South Africa and its synthetic counterpart obtained by applying the weights in Table 2 is 0.144. That is, on average, the pre-treatment difference between South Africa and synthetic South Africa for the outcome variable is about one-tenth of a per capita cigarette. The optimal weights in Table 2 show that synthetic South Africa is mostly made up of Latin American countries (with a combined weight of 90%) with Brazil being the most important.

In Table 3, we compare the average pre-treatment characteristics, the predictors in $\mathbf{Z}_1$, for South Africa with its synthetic counterpart using the weights in Table 2. The table shows that synthetic South Africa resembles the actual South Africa in most of the pre-treatment characteristics. The only variable whose pre-treatment average differs between South Africa and its synthetic counterpart is alcohol consumption per capita: South Africa's average is somewhat higher than its synthetic counterpart. This is due to the fact that South Africa's alcohol consumption per capita is "extreme" relative to the countries in the donor pool. In other words, there is no linear combination of countries in the donor pool than can perfectly reproduce South Africa's alcohol consumption profile (i.e. in terms of alcohol consumption, South Africa is unlikely to be in the convex hull of the donor pool). Having one or two predictors that differ in magnitude between the treated country and its synthetic counterpart is typical of the Synthetic Control method as the treated country is likely to have some "extreme"

[23] This results in the exclusion of Bangladesh, Croatia, Iran and Serbia and Montenegro.

[24] BRICS stands for Brazil, Russia, India, China and South Africa. Russia is not in the donor pool as it was classified as treated according to criterion outlined in Section 6.

predictors.[25]

Having shown that synthetic South Africa largely matches actual South Africa in its pre-treatment characteristics (as evidenced in Table 3 and by the pre-treatment RMSE), we can now use synthetic South Africa to estimate the treatment effect of the policy change. Figure 2 plots cigarette consumption per capita for South Africa and synthetic South Africa over the period 1990 to 2004. The vertical distance between the two lines is the estimate of the treatment effect (see equation 6). As one would expect, there is hardly any treatment effect before 1994 as the two lines are indistinguishable from one another. The last point is another way of judging the success of the Synthetic Control method in reproducing South Africa's pre-treatment characteristics.

After the onset of treatment in 1994, the two lines in Figure 2 begin to diverge with South Africa's consumption line being everywhere lower than synthetic South Africa's consumption line. South Africa's per capita cigarette consumption declines throughout the treatment period whereas synthetic South Africa's trend line initially rises and eventually stabilises at around 800 cigarettes per capita from the year 2000.

One of the factors that might explain why per capita cigarette consumption stopped declining for synthetic South Africa after 1994 is the performance of the economy. The literature on the demand for cigarettes in South Africa tends to find a positive income elasticity of demand (Reekie, 1994; Van Walbeek, 1996; Economics of Tobacco Control Project, 1998; Van Walbeek, 2005; Boshoff, 2008). That is, on average and *ceteris paribus*, cigarette demand tends to rise with an increase in incomes and tends to fall with a decrease in incomes. Between 1980 and 1994, South Africa's real GDP per capita declined at the average rate of 1% per year.[26] On the other hand, between 1994 and 2004, real GDP per capita increased at the rate of 2% per year. Therefore, the decline in consumption that was already underway by 1994 would likely have stopped, in the absence of tax increases, simply because incomes began to rise. Per capita consumption for synthetic South Africa stabilized as opposed to increasing after 1994 likely because other factors such as increased health awareness were also at play (Boshoff, 2008).

Figure 3 presents another way of visualizing the treatment effect. The line in the figure measures the cigarette consumption gap between South Africa and its synthetic counterpart (Table A2 in the appendix provides actual estimates

[25] In their study assessing the economic costs of reunification on West Germany's economy, Abadie *et al.* (2015) were unable to find a linear combination of donor countries that reproduces West Germany's average pre-treatment inflation rate. This is because West Germany had a very low inflation rate in the pre-treatment period compared to the OECD countries which form the donor pool in their study. Similarly, Abadie and Gardeazabal (2003) in their study of the economic costs of conflict in Spain were unable to reproduce the Basque region's pre-treatment industrial share as a percentage of total production. This is because the Basque region, which is the treated region in their study, had a very high pre-treatment industrial share relative to the rest of Spain.

[26] Obtained from the World Bank's Development Indicators database available at: http://data.worldbank.org/data-catalog/world-development-indicators (accessed October 2015).

of the treatment effect). Between 1990 and 1993, the treatment effect is approximately zero. By 1995, the first year after treatment begins, South Africa's per capita cigarette consumption is 38 cigarettes less than its synthetic counterpart (or 4% below). The treatment effect increases with each additional year the authorities raise excise taxes on cigarettes so that by 2004, South Africa's per capita cigarette consumption is about 290 cigarettes less than its synthetic counterpart. That is, South Africa's per capita cigarette consumption is 36% lower than where it would have been had treatment not began in 1994.

7.2 Placebo tests

The treatment effects from Section 7.1 might have been produced by random chance in which case they would not be statistically significant. To confront this assertion, we use the inferential techniques suggested by Abadie *et al.* (2010, 2015) and described in Section 4.2. We place South Africa in the donor pool and subject each of the countries in Table 1 to the same synthetic control routine as we did for South Africa. This exercise results in a distribution of effects against which South Africa's treatment effects can be compared. South Africa's treatment effects would be statistically significant (i.e. not due to random chance) if the probability of obtaining a treatment effect as large as South Africa's, in the distribution of treatment effects, were small. These are called placebo tests because we do not expect many of the untreated countries in Table 1 to have treatment effects as large as those observed for the treated country. Figures 4 to 7 present the results of running the placebo tests. We also include in the figures South Africa's treatment effect from Figure 3.

Figure 4 presents the treatment effects for all 25 countries. In the figure, most of the countries have treatment effects that are greater than zero or equal to zero over the period 1995 to 2004 (recall from equation 6 that a successful treatment results in a negative difference between a country's cigarette consumption per capita and its synthetic counterpart in the treatment period). South Africa's treatment effect appears unusual in the figure although it is matched by Brazil's treatment effect (Brazil's treatment effect is the other line that is also everywhere less than zero). Brazil's pre-treatment fit, with a RMSE of 95, is however poor making it a bad comparison for South Africa which has a pre-treatment RMSE of 0.14. Looked at differently, Brazil's pre-treatment fit is about 600 times poorer than South Africa's pre-treatment fit. Consequently, in Figure 5 we do not present the treatment results of countries whose pre-treatment RMSEs are greater than 500 times South Africa's pre-treatment RMSE. This results in the exclusion of four countries.[27] South Africa's unusual treatment effect is now visible. By 2004, no other country has a treatment effect as large as South Africa's. The probability of obtaining a treatment effect as large as South Africa's is $1/21 = 4.76\%$, which is less than the 5% level used in standard tests of statistical significance. Figures 6 and 7 continue the exercise

[27] Brazil (RMSE = 95), China (RMSE = 281), Romania (RMSE = 139) and Tunisia (RMSE = 123).

of not presenting the treatment results of countries with poor pre-treatment fits. Figure 6 excludes countries with a pre-treatment RMSE that is 100 times greater than South Africa's.[28] Figure 7 excludes countries with a pre-treatment RMSE that is 50 times greater than South Africa's.[29] The unusual nature of South Africa's treatment effect is now more evident in figures 6 and 7. The probability of obtaining an effect as large as South Africa's in Figure 6 is 1/14 = 7% whereas in Figure 7 the probability is 1/10 = 10%. Both probabilities are small given the number of countries in Figures 6 and 7. Cunningham and Shah (2014) and Dube and Zipperer (2015) make the point that a 10% level is actually a stringent threshold for making inference under the Synthetic Control method given that donor pools usually contain a small number of countries.

Another way of presenting the results of the placebo tests is to divide each country's post-treatment RMSE by its pre-treatment RMSE and then to rank the ensuing ratios for all countries. This is attractive because it avoids the arbitrary RMSE cut-offs that we used in Figures 4 to 7 and at the same time penalises countries with large treatment effects but poor pre-treatment fits (like Brazil).[30] Figure 8 presents the results of this ranking exercise for all the 25 countries in Table 1. In the figure, the ratio for most countries is so small that it is not even visible in the figure (the actual ratios are reported in Table A3 in the appendix). On the other hand, at about 5, 000, the magnitude of South Africa's ratio is large and is only surpassed by Indonesia's ratio. The results from Indonesia's placebo test cannot, however, be regarded as a successful treatment. This is evident in Figure A1 in the appendix which plots Indonesia's treatment effect against South Africa's. Indonesia's treatment effect is mostly positive over the period 1995 to 2004 implying that its per capita cigarette consumption is mostly greater than synthetic Indonesia's consumption, a situation that can hardly be described as a successful treatment. Indonesia's unusually high ratio in Figure 8 is the result of a very low pre-treatment RMSE relative to South Africa and the fact that the calculation of the post-treatment RMSE does not distinguish between negative and positive treatment effects.[31] The Indonesian case notwithstanding, the probability of obtaining a ratio as large as South Africa's in Figure 8 is 2/25 = 8% which is small given the number of countries ("sample size").[32]

[28] In addition to the countries in footnote 27, the following countries are also excluded: Argentina (RMSE = 17), Colombia (RMSE = 32), Costa Rica (RMSE = 39), Egypt (RMSE = 23), India (RMSE = 23), Jordan (RMSE = 28) and Vietnam (RMSE = 26).

[29] In addition to the countries excluded in footnotes 27 and 28, Figure 7 excludes Chile (RMSE = 12), Pakistan (RMSE = 9), Panama (RMSE = 13) and Philippines (RMSE = 10).

[30] This ratio is similar to the t statistic used in standard inferential methods. A large t statistic is obtained whenever the identified effect is large relative to the standard error. The pre-treatment RMSE, in our case, plays the role of a standard error while the post-treatment RMSE plays the role of the identified effect.

[31] The RMSE formula squares and sums over the deviations (which are essentially the treatment effects). See the RMSE formula in equation (7).

[32] If we only consider successfully treated countries, then this probability reduces to 1/25 = 4%.

8 Robustness

This section tests the robustness of our treatment effect estimates from Section 7. Firstly, we check whether the treatment effects are sensitive to the composition of the donor pool. We do this by excluding, one at a time from the donor pool, the countries in Table 2 that have positive donor weights and re-estimating the treatment effect. This is done so as to guard against the possibility that our estimated effects are being driven by a single donor country with a positive weight. Secondly, we vary the timing of the onset of treatment to account for any delays in the implementation of the policy.

Figures 9 to 13 present the results of successively excluding from the donor pool countries which earlier had positive weights. The pattern of the trajectories of synthetic South Africa is similar across the five figures and, more importantly, similar to the pattern in Figure 2. By 2004, the five figures all show a counterfactual consumption level of around 800 cigarettes per capita which was what we found in Figure 2. Table 4 compares the actual treatment effect estimates of the robustness tests with the main results from Section 7. The treatment effects are presented as annual percentage deviations from their respective counterfactual trend lines. Column (2) shows the main results while columns (3) to (7) show the results from excluding, one at a time, donor countries with positive weights from the donor pool. The treatment effect estimates by 2004 are similar across columns (2) to (7). By 2004, all specifications report a treatment effect of at least 30%. Our treatment effects estimates are, therefore, not disproportionately influenced by the composition of the donor pool.

The final robustness check allows for the possibility that treatment did not begin in earnest in 1994. This is likely to have been the case if the initial tax increase was small relative to the ones in later years or if tobacco companies did not immediately pass-on, in full, the 1994 tax increase.[33] Figure 14 and the last column of Table 4 (column 8) show treatment effect estimates under the assumption that treatment implementation was delayed by at least a year (i.e. started in 1995). In Figure 14, the pattern of the counterfactual trend line is very similar to the one in Figure 2 and similar to the ones in Figures 9 to 13. In the figure, counterfactual cigarette consumption per capita is also around 800 cigarettes by 2004. The treatment effect by 2004 is also similar to the treatment effects obtained for the main result (column 2 of Table 4) and for the other donor pool specifications (columns 3 to 7).

9 The impact of illicit trade on the treatment effect

The argument is often made that an aggressive excise tax policy, such as the one that South Africa has been implementing since 1994, might translate into an

[33] Although the available evidence shows that tobacco companies immediately passed-on to consumers some of the tax rise (Van Walbeek, 2006), we nonetheless confront the possibility that full treatment was delayed.

increase in the market for illicit cigarettes. If this is the case, then the treatment effect estimates from Section 7 might be overestimated. Blecher (2010, 2011) has provided some estimates of the size of South Africa's illicit market over most of the period that we study in this chapter. Using several data sources, he obtained an estimate of the illicit market that was implied by smoking prevalence and legal consumption data. For 2004, which is the cut-off point in our evaluation, Blecher estimated an illicit market of between 5% and 12% of the total market.

Using legal consumption data for 2004 from Table A2 in the appendix (see column 2) and Blecher's estimates of the illicit market in 2004, we can obtain an estimate of the total market (legal and illegal cigarettes) for South Africa. Our estimates suggest that the total market for cigarettes in 2004 was somewhere between 548 and 592 cigarettes per capita.[34] Comparing these estimates to synthetic South Africa's estimate for per capita consumption in 2004 (column 3 in Table A2) results in a treatment effect of between 27% and 32%. That is, the treatment effect estimates, when one takes into account the size of the illicit market, are not very different from the main treatment effect estimate of 36% for 2004. In any case, the 27% estimate of the treatment effect, corresponding to Blecher's upper bound estimate of the illicit market share, can be taken to be a lower bound estimate of the treatment effect.

Subsequently, Van Walbeek (2014) has also attempted to measure the size of South Africa's illicit market for cigarettes. He uses a method that compares predicted percentage changes in total consumption with actual changes in legal consumption. If predicted changes in total consumption are greater than actual changes in legal consumption, then the share of the illicit market is growing and vice versa.

Between 1995 and 2004, Van Walbeek's estimates suggest that the share of the market that was due to illicit cigarettes remained virtually unchanged.[35] Unfortunately, his method does not allow for us to obtain a treatment effect that takes into account the illicit market. This is because he estimates percentage changes in the share as opposed to providing estimates of the actual share. However, given the consensus that the illicit market share was very low when the new tax policy started (Blecher, 2010, 2011), Van Walbeek's estimates suggest a small illicit market share over the period 1995 to 2004. This implies that our main treatment effect of 36% by 2004 is, therefore, not incredibly overestimated.

10 Summary and conclusion

South Africa has consistently increased the excise tax on cigarettes since 1994 largely on public health grounds. In increasing the tax, the government has

[34] In Table A2, the figure for legal cigarette consumption per capita in 2004 is 521 cigarettes. A 5% illicit market share implies that the legal market was 95% of the total market. Similarly, an estimate of 12% of the illicit market implies that the legal market was 88% of the total market.

[35] Van Walbeek's estimates of the change in the illicit market share range from an average decline of 2 percentage points to an average increase of 2 percentage points over the period 1995 to 2004.

sought to maintain a total tax burden of at least 52% of the average retail selling price (the target was initially set at 50%). This has resulted in substantial increases in the real price of cigarettes. For instance, between 1994 and 2004, the average real price per pack increased by 110%.

The main focus of this paper was to evaluate the impact on consumption of this unprecedented increase in the price of cigarettes. We argued in the paper that comparing current cigarette consumption to cigarette consumption before 1994 was likely to overstate the impact of the tax rises. This is because consumption had already started declining by the time the government's policy of raising taxes began.

The challenge in conducting impact evaluations is to create a credible counterfactual of what would have happened to cigarette consumption in the absence of the tax rises. This paper, therefore, used the Synthetic Control method to create such a counterfactual for South Africa. The counterfactual was created as a linear combination of the per capita cigarette consumption of countries similar to South Africa that did not engage in large-scale tobacco control initiatives over the period 1994 to 2004. Using this counterfactual, we found that South Africa's cigarette consumption per capita would not have continued declining in the absence of the tax rises. Specifically, we found that cigarette consumption would have stabilized at around 800 cigarettes per capita from the year 2000. Further, we found that by 2004, South Africa's per capita cigarette consumption was 36% lower than it would have been had the tax rises not happened.

South Africa's successful experience with tobacco control holds many lessons for countries, particularly those in Africa, that are trying to forestall an impending tobacco epidemic. South Africa's experience shows that significant public health dividends can be obtained by consistently increasing the real tax on cigarettes.-

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Table 1: Donor Pool

Donor Pool	
Argentina	Morocco
Brazil	Pakistan
Chile	Panama
China	Peru
Colombia	Philippines
Costa Rica	Romania
Cote d'Ivoire	Senegal
Ecuador	Sri Lanka
Egypt	Tunisia
India	Uruguay
Indonesia	Vietnam
Jordan	
Malaysia	

Notes: List of untreated countries from Table A1 that are not high income countries and have a complete set of data over the period 1990 to 2004.

Table 2: Synthetic weights

Country	Weight
Argentina	0.276
Brazil	0.476
Chile	0.146
China	0
Colombia	0
Costa Rica	0
Ecuador	0
Egypt	0
India	0
Indonesia	0
Cote d'Ivoire	0
Jordan	0
Malaysia	0
Morocco	0
Pakistan	0
Panama	0
Peru	0
Philippines	0
Romania	0.007
Senegal	0
Sri Lanka	0
Tunisia	0.094
Uruguay	0
Vietnam	0

Notes: The table shows the vector of optimal weights, $\boldsymbol{W}^*$, obtained as the solution to the problem in equation (8).

Table 3: Average pre-treatment characteristics for South Africa and Synthetic South Africa

	South Africa	Synthetic South Africa
Log of GDP per capita	8.44	8.28
Price of cigarette pack in USD	0.89	1.15
Pure alcohol consumption (in litres)	9.05	7.11
Proportion of adults in population	58.62	60.95
Consumption (1992)	947	945.58
Consumption (1990)	1010	1008.86

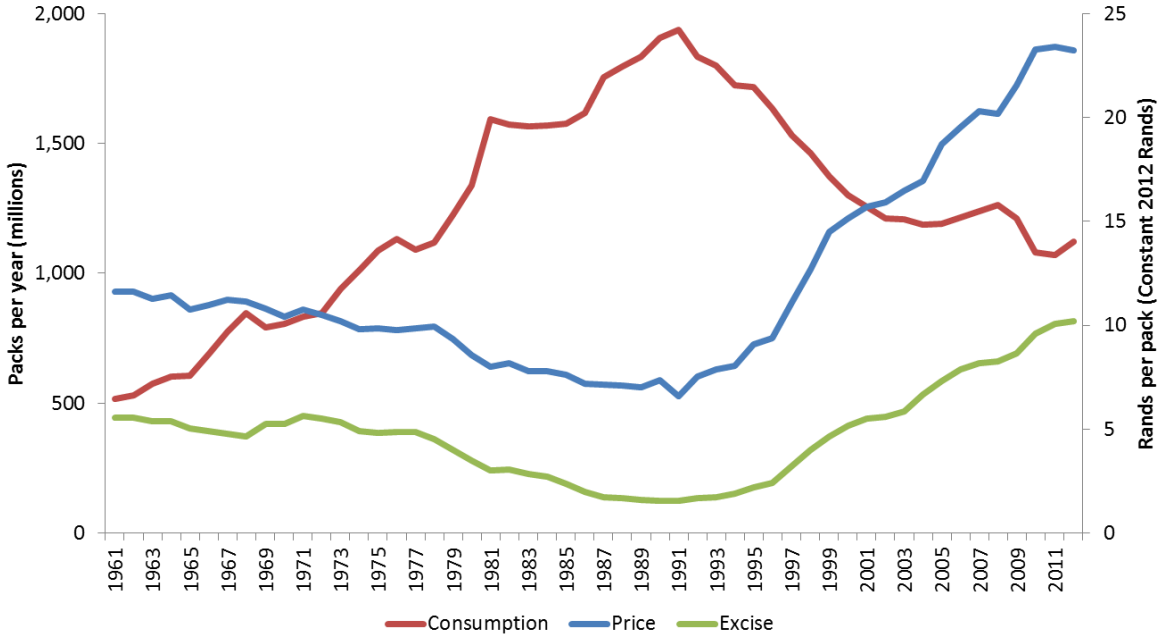
Notes: Average pre-treatment characteristics for South Africa and synthetic South Africa. Obtained by applying the weights in Table 2 to the pre-treatment characteristics of the donor pool. Alcohol consumption is in litres of pure alcohol per capita.

Table 4: Treatment effects (in %) associated with robustness tests

Year (1)	Main Results (2)	Excluding Argentina (3)	Excluding Brazil (4)	Excluding Chile (5)	Excluding Romania (6)	Excluding Tunisia (7)	Treatment from 1995 (8)
1990	0.11	0.00	-0.05	-0.03	-0.10	0.00	0.09
1991	0.11	-0.07	1.74	0.36	0.09	-0.03	0.33
1992	0.15	0.03	-0.06	0.85	0.01	2.04	0.11
1993	0.12	-0.14	-0.23	-0.22	0.05	-1.52	0.08
1994	1.97	4.02	-8.74	2.75	1.72	-0.44	0.10
1995	-4.31	2.18	-9.88	-2.56	-4.38	-6.63	-3.28
1996	-9.60	-9.80	-17.99	-7.30	-9.72	-11.32	-10.08
1997	-14.7	-16.02	-25.77	-11.64	-14.94	-15.26	-15.70
1998	-16.07	-19.32	-27.98	-12.32	-16.26	-17.14	-17.29
1999	-22.49	-24.60	-28.16	-19.95	-22.78	-23.48	-23.21
2000	-26.51	-27.63	-35.85	-23.27	-26.72	-25.75	-28.24
2001	-27.22	-29.40	-33.64	-23.96	-27.25	-25.61	-29.06
2002	-25.54	-22.69	-28.83	-22.50	-25.54	-23.86	-26.59
2003	-39.26	-32.71	-42.09	-36.48	-39.34	-38.51	-39.84
2004	-35.75	-30.78	-36.11	-32.11	-35.84	-33.73	-37.19

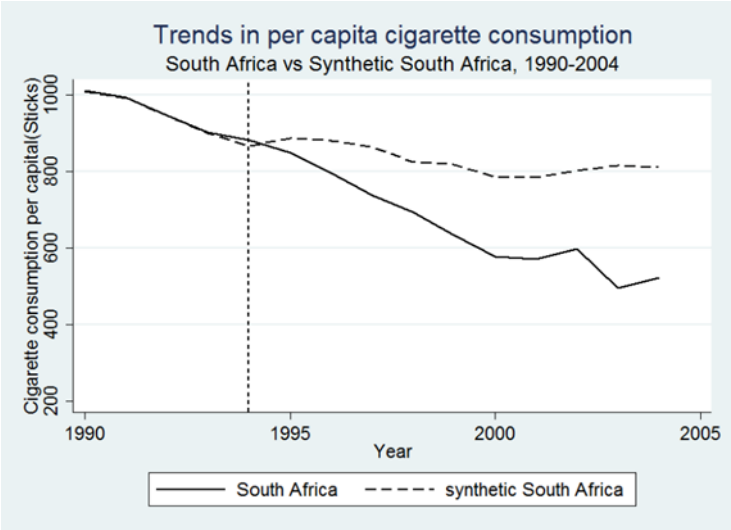
Notes: The numbers in columns (3) to (8) are treatment effects in percentages associated with the six tests for robustness. The numbers represent annual percentage deviations from their respective counterfactual trend lines. Column (2) reports the main results from Section 7. In column (3), Argentina is excluded from the donor pool, column (4) excludes Brazil, column (5) excludes Chile, column (6) excludes Romania and column (7) excludes Tunisia. Column (8) presents results for treatment beginning in 1995 as opposed to 1994.

Figure 1: Trends in the excise tax per pack of cigarettes, real price per pack of cigarettes and consumption of cigarettes in packs, South Africa 1960 to 2012



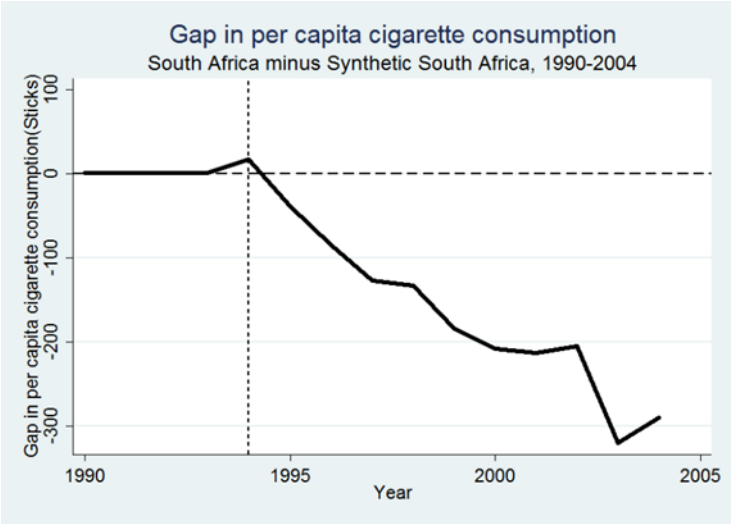
Notes: Based on data from the National Treasury of South Africa and Statistics South Africa.

Figure 2: Cigarette consumption per capita, South Africa vs Synthetic South Africa



Notes: The figure shows the trend lines in per capita consumption of cigarettes for South Africa and its synthetic counterpart. As is clear in the figure, the two lines are indistinguishable before the onset of treatment in 1994 but diverge after treatment.

Figure 3: Treatment effect



Notes: The figure shows the gap in per capita cigarette consumption between South Africa and synthetic South Africa over the period 1990 to 2004. The gap is calculated using equation (6). As can be seen from the figure, the treatment effect (the gap) is on average zero between 1990 and 1993. Thereafter, it is negative which means that synthetic South Africa has a higher cigarette consumption per capita than actual South Africa during the entire treatment period.

Figure 4: Placebo test 1

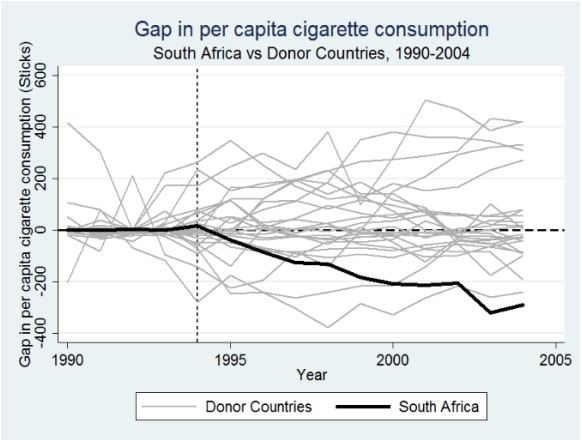


Figure 5: Placebo test 2

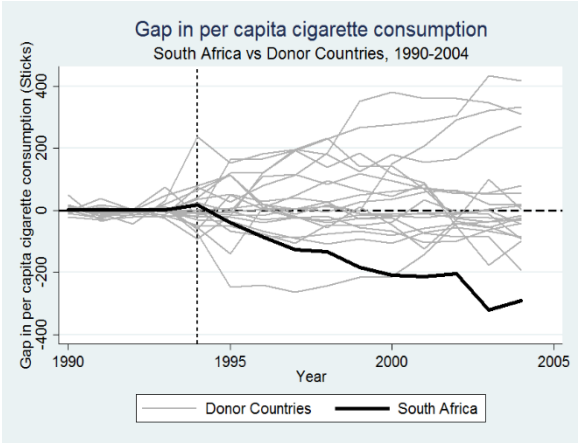


Figure 6: Placebo test 3

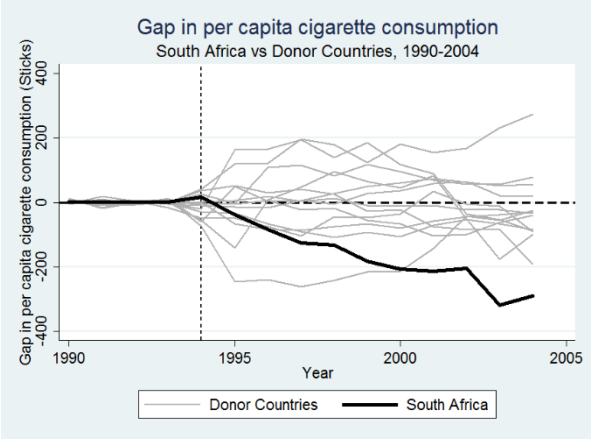


Figure 7: Placebo test 4

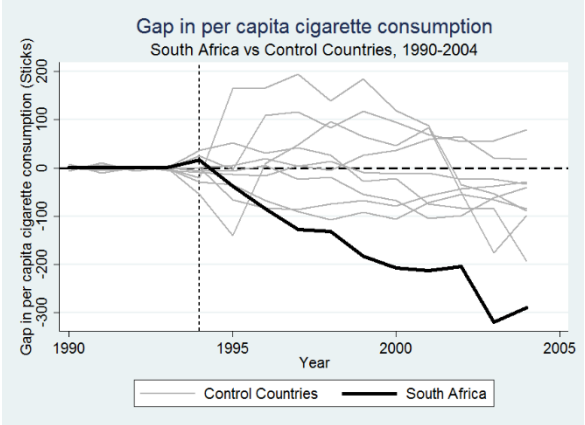
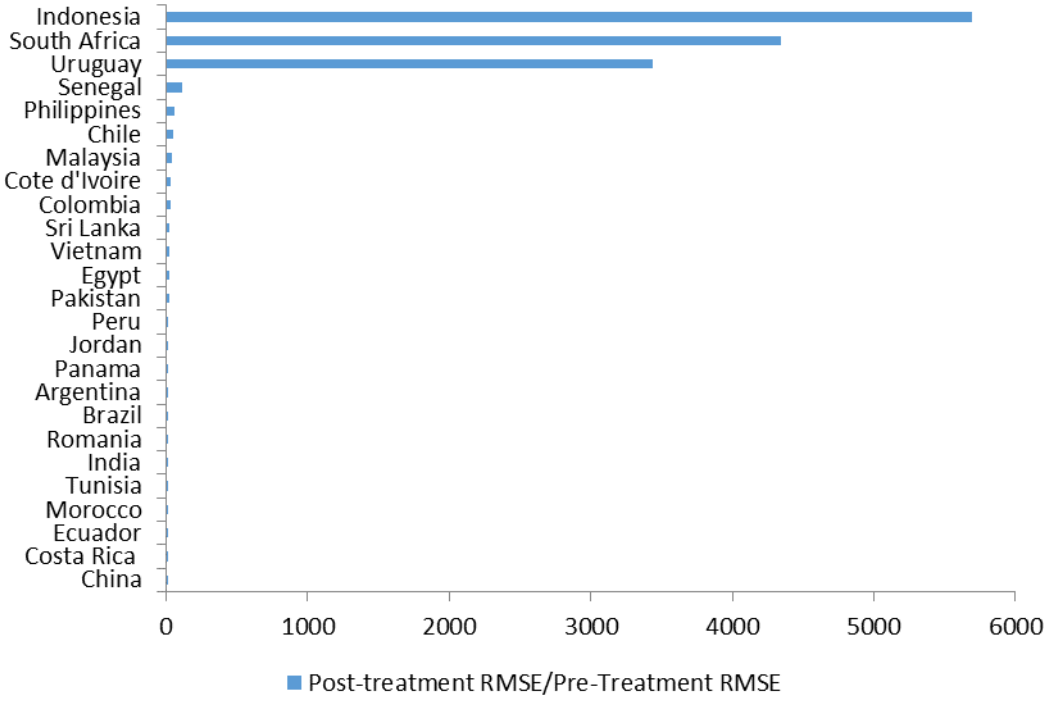


Figure 4: Ranking of treatment effects



Notes: The figure shows rankings of ratios of post-treatment root mean square errors (RMSE) to pre-treatment RMSEs for the countries in Table 1 plus South Africa.

Figure 9: Excluding Argentina

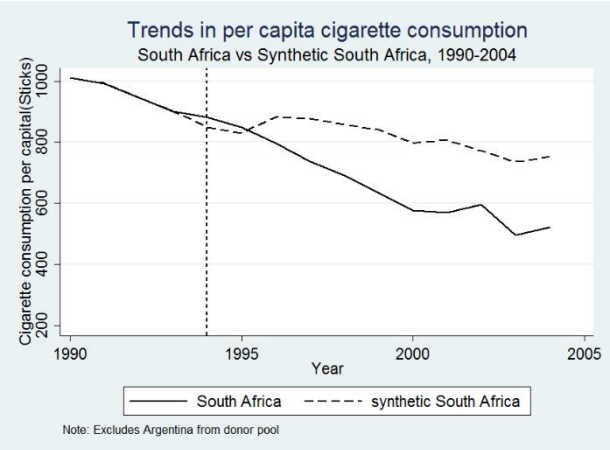


Figure 10: Excluding Brazil

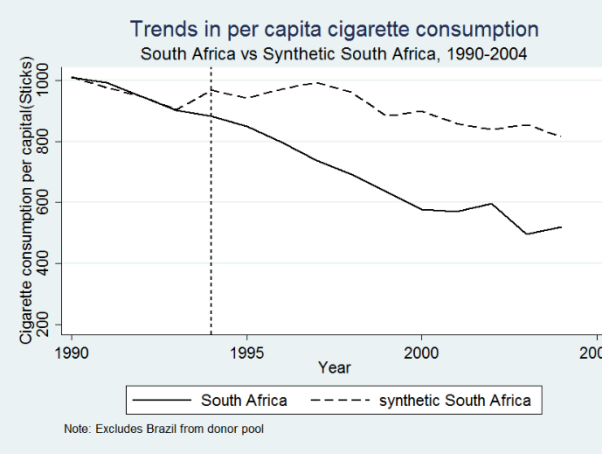


Figure 11: Excluding Chile

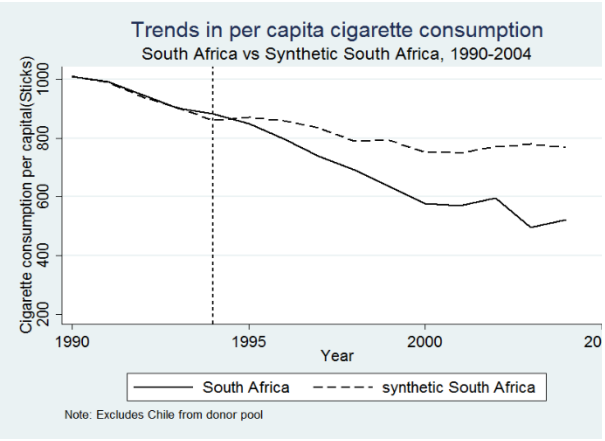


Figure 12: Excluding Romania

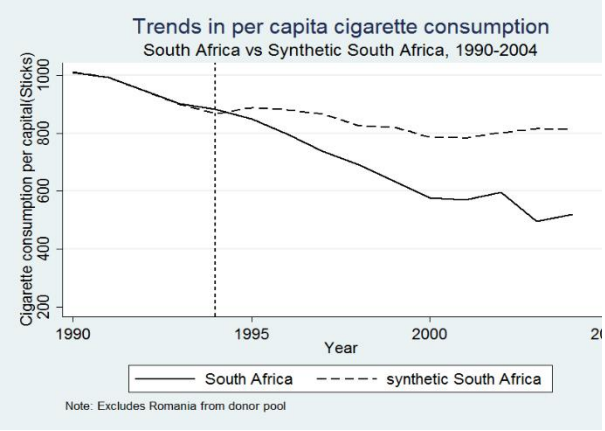


Figure 13: Excluding Tunisia

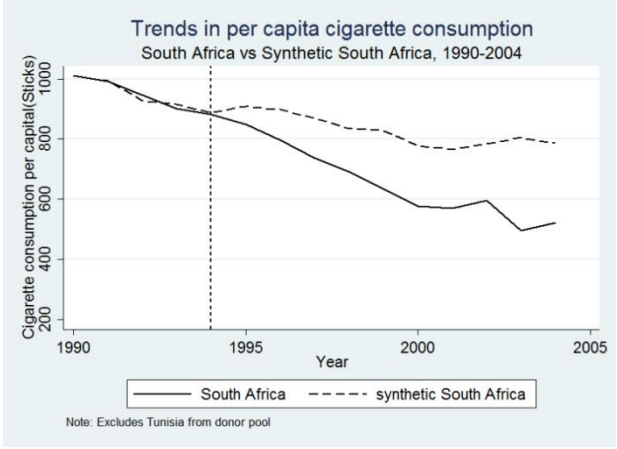
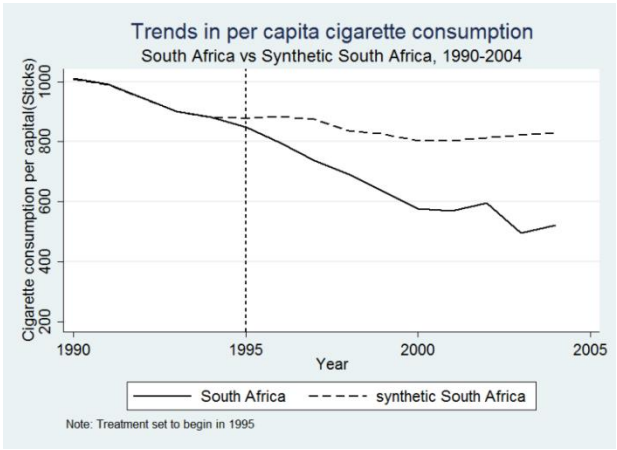


Figure 14: Treatment beginning in 1995



APPENDIX

Table A1: Treated and Untreated Countries

Treated		Untreated	
Australia	New Zealand	Argentina	Ireland
Austria	Nigeria	Bahrain	Jordan
Azerbaijan	Papua New Guinea	Bangladesh	Kuwait
Belgium	Paraguay	Brazil	Luxembourg
Cameroon	Poland	Chile	Malaysia
Canada	Portugal	China	Morocco
France	Russia	Colombia	Norway
Gabon	Saudi Arabia	Costa Rica	Pakistan
Germany	Singapore	Cote d'Ivoire	Panama
Guatemala	**South Africa**	Croatia	Peru
Hong Kong	Spain	Czech Rep	Philippines
Hungary	Sweden	Denmark	Romania
Iceland	Switzerland	Ecuador	Senegal
Israel	Thailand	Egypt	Serbia & Montenegro
Italy	Turkey	Finland	Sri Lanka
Japan	U.A.E	Greece	Tunisia
Kenya	United Kingdom	India	Uruguay
Korea, Rep.	United States	Indonesia	Vietnam
Mexico	Venezuela	Iran	
Netherlands	Zimbabwe		

Notes: Treated countries are those whose Relative Income Prices (RIPs) increased on average over the period 1990 to 2006 (i.e. where affordability declined). Untreated countries are those whose RIPs declined on average over the same period (i.e. where affordability increased). The information on RIPs is taken from Blecher and Van Walbeek (2009).

Table A2: Actual estimates of treatment effects

Year	South Africa (Consumption Sticks p.c.)	Synthetic South Africa (Consumption Sticks p.c.)	Treatment Effect (Sticks p.c.)	Treatment Effect (%)
1990	1010	1008.86	1.14	0.11%
1991	993	991.93	1.07	0.11%
1992	947	945.58	1.42	0.15%
1993	901	899.94	1.06	0.12%
1994	883	865.95	17.05	1.97%
1995	849	887.26	-38.26	-4.31%
1996	796	880.52	-84.52	-9.60%
1997	737	864.11	-127.11	-14.71%
1998	692	824.46	-132.46	-16.07%
1999	634	817.92	-183.92	-22.49%
2000	577	785.12	-208.12	-26.51%
2001	570	783.16	-213.16	-27.22%
2002	597	801.77	-204.77	-25.54%
2003	495	814.90	-319.90	-39.26%
2004	521	810.86	-289.86	-35.75%

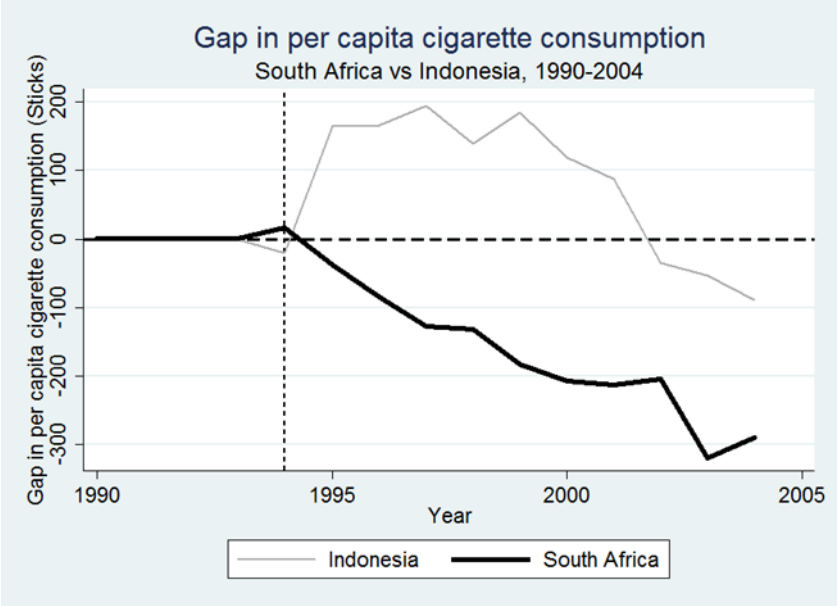
Notes: Treatment effects in the fourth column obtained by using equation (6). The last column presents treatment effects as a percentage difference. The consumption numbers for synthetic South Africa are obtained by applying the weights in Table 2 to the cigarette consumption numbers of the donor countries in Table 1. Cigarette consumption data is from ERC Group (2010).

Table A3: Ranking of ratios of post-treatment RMSE to pre-treatment RMSE

Rank	Country	Post-treatment RMSE / pre-treatment RMSE
1	Indonesia	5695
2	South Africa	4341
3	Uruguay	3436
4	Senegal	112
5	Philippines	58
6	Chile	49
7	Malaysia	37
8	Cote d'Ivoire	28
9	Colombia	27
10	Sri Lanka	26
11	Vietnam	23
12	Egypt	23
13	Pakistan	17
14	Peru	16
15	Jordan	16
16	Panama	15
17	Argentina	13
18	Brazil	9
19	Romania	8
20	India	3
21	Tunisia	3
22	Morocco	2
23	Ecuador	2
24	Costa Rica	2
25	China	2

Notes: The Table shows a ranking of ratios of post-treatment RMSE to pre-treatment RMSE for all 25 countries in Table 1 plus South Africa. The data in this table is used to create Figure 8.

Figure A1: Treatment effect, South Africa vs. Indonesia



Notes: Comparison of treatment effects between Indonesia and South Africa.