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The Economic Impact of Carbon Pricing

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Abstract

Putting a price on carbon has become a frequently used policy tool to curb carbon emissions on the way to net-zero. This paper investigates the economic effectiveness of such policies. Using data on carbon prices and GDP per capita for a panel of 127 countries between 1971 and 2016, it is analyzed how the introduction and level of a carbon price affect the economic growth of countries. First, a time-series approach is employed to investigate the effect of the carbon tax introduction in Finland in 1990 on the economy of the country. Second, the average change in economic growth associated with carbon pricing as well as the elasticity of economic growth with respect to the price level is studied by employing a synthetic control approach with staggered adoption. It is found that Finland's economy declined in the period after 1990 relative to countries on the same growth path as Finland before that period. Introducing a carbon price did not, on average, affect total economic growth when studying the panel of countries. The change in economic growth was also found to be inelastic to the level of the carbon price. This indicates that increased efforts to limit climate change by setting higher carbon prices are unlikely to hurt economic growth of countries.

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1 Introduction

Fourteen of the fifteen hottest years ever recorded have occurred since the beginning of the century. The consensus among scientists is large: These temperature abnormalities are driven by the high stock of carbon in the atmosphere, which accumulated due to humans burning fossil fuels. The recent Intergovernmental Panel on Climate Change's (IPCC) Sixth Assessment Report thus called for strong and sustained reductions in global carbon dioxide emissions (IPCC, 2021). According to the report, rapid and large-scale reductions in greenhouse gas emissions are necessary if global warming should be limited to well below 2°C in this century, an ambition that 120 world leaders reaffirmed last November (2021) during the 26th Conference of the Parties (COP26) in Glasgow.[1]

While the goal of achieving net-zero emissions within this century finds global support, the pathway to achieving this remains contested and unclear. Putting a price on carbon - through, for instance, carbon taxes or emission trading schemes - is considered to play an essential part in the reduction of carbon emissions. Since the first carbon tax was implemented in Finland in 1990, pricing carbon emissions has become a more frequently used policy instrument, covering 21.5% of global greenhouse gas (GHG) emissions in 2021 (World Bank, 2022a). However, these policies often find little public support due to the price burden that they place on producers and consumers, making some policy makers reluctant to implement carbon pricing policies. A common argument against carbon pricing strategies is that it will hurt the competitiveness and economic growth of a country. To gain more public support for the implementation and setting of carbon prices it is of importance to analyze the environmental and economic effectiveness of pricing carbon, drawing from evidence of already implemented policies.

This paper extends the growing literature assessing the impact of carbon pricing policies. So far, the focus of empirical carbon pricing research has been on its environmental effectiveness in reducing emissions (Andersson, 2019; Best et al., 2020; Rafatya et al., 2020). The effect of carbon pricing policies on the economy has been scarcely studied. Previous literature investigating these policies and the damage of climate change on the economy have been predominantly ex-ante projections based on general equilibrium or integrated assessment models (Nordhaus & Moffat, 2017; Wills et al., 2022). A few scholars have studied the topic empirically through ex-post evaluations using regression and event study techniques (Känzig, 2021; Metcalf, 2019). These studies, however, have been

[1] COP26 was the 26th Conference of the Parties, an annual UN climate change conference attended by countries that signed the United Nations Framework Convention on Climate Change of 1994. The conference was held in November 2021 in Glasgow. Originally it was planned to take place in 2020, however, had to be postponed to 2021 due to the Covid pandemic.

inconclusive, finding no effects of carbon pricing policies on the gross domestic product (GDP) in British Columbia (Metcalf, 2019), but temporary adverse effects on economic activity in the European Union (Känzig, 2021).

In this paper data on average carbon prices, adjusted for emissions coverage and made available by Dolphin et al. (2020), for a panel of 38 countries that implemented a carbon price during 1990-2016 (and 89 countries which did not) is employed. This data has been previously used by Rafatya et al. (2020) for a similar study on the environmental effectiveness of carbon pricing. I combine this data with country specific GDP per capita data from 1971-2016 obtained from the World Bank (2022b).

Using this data set, I wish to answer the following questions: First, what is the effect of carbon pricing on economic growth? Further, how does the level of the carbon price affect the economic growth of a country? Phrased differently, I wish to analyze the extensive, the average effect of carbon prices, and the intensive margin, the elasticity of GDP growth with respect to the carbon price.

To answer the above-mentioned questions, different methodological approaches are used. First, the effect of carbon pricing on the economy in Finland is studied by comparing the results of a synthetic control approach (Abadie et al., 2015) and fitting a time series model (Harvey & Thiele, 2021). The analysis of Finland is of particular interest since it was the first country to introduce a carbon tax in 1990. The country also has one of the highest prices on carbon worldwide with a price of 73.02 USD per tonne carbon emissions (2021). This early introduction of a substantial carbon price allows investigation of the short and long term effects carbon pricing has on GDP growth. Next to this country specific study, the data on all countries having implemented a carbon price between 1990 and 2016 is used to investigate the average treatment effect of introducing carbon pricing on GDP growth, irrespective of the level of the carbon price in the respective country. Since different countries implemented carbon pricing policies at distinct points in time, the synthetic control approach with staggered adoption (Xu, 2017) is employed as proposed by Rafatya et al. (2020). This method and the compiled data set, however, not only allow me to study the treatment effect but also the role that treatment intensity, that is the level of the carbon price, plays for the magnitude of the effect. Therefore, to answer the second question proposed, I investigate whether heterogeneity in the treatment effect across countries can be explained by heterogeneity in the carbon price implemented by different countries. Using the staggered synthetic control approach I estimate the elasticity of GDP growth with respect to the level of the carbon price. This allows me to disentangle the introduction and level effect of pricing carbon on economic growth.

The paper finds that Finland's GDP per capita decreased significantly in the period after 1990, the year the carbon price was introduced. This economic downturn, however, might not have been caused by the carbon price introduction, but could be linked to other factors such as the global economic recession of the early 90s. Analyzing the panel of all countries having implemented a carbon pricing policy shows that pricing carbon does not, on average, affect the economic growth of a country. Lastly, I find no evidence of a relationship between the level of the carbon price and the effect of the policy on economic growth, suggesting that economic growth is inelastic with respect to the amount of the carbon price.

Through employing different methodological approaches the paper contributes to the study of the economic effectiveness of carbon pricing in various ways. First, by applying a time series strategy to select controls in the analysis of Finland's carbon price implementation, the paper presents a methodology improving the study of country specific emission reduction pathways and their economic impacts. Second, the investigation of the average effect of carbon pricing schemes on economic growth in a pool of countries enhances the external validity of the study. To assess and improve the validity of the underlying assumptions of this analysis, this paper introduces a novel approach combining the donor selection strategy proposed by (Harvey & Thiele, 2021) and the generalized synthetic control approach. Lastly, to the best of my knowledge this paper is the first to analyze the economic impacts of carbon pricing policies through a methodological approach allowing to disentangle the extensive and intensive margin of the effect. Investigating the economic effectiveness of carbon pricing will inform policy makers what effect rising carbon prices have on the global economy in the future. Moreover, separating the introduction and price-level effect will address the concern of decision makers and the public if the introduction and the level of a carbon price affect the economy adversely.

The paper is structured as follows. In the subsequent Section 2 economic theory on carbon pricing is introduced, followed by an overview of the existing literature on carbon pricing and its effect on the economy in Section 3. Next, in Section 4, I elaborate on the development of carbon pricing schemes around the world. Section 5 outlines the methodology employed, while Section 6 describes the data used in the study. The results of the analysis are presented in Section 7. Finally, Section 8 concludes and limitations as well as suggestions for further research are discussed in Section 9. Acronyms used in the paper are defined in Appendix A.

2 Economic Theory on Carbon Pricing

Under perfect market conditions, first-best theory suggests that markets and redistribution yield an efficient (First Welfare Theorem) and social welfare maximizing (Second Welfare Theorem) equilibrium (Blaug, 2007). Relying on the absence of market failures, these theories fail in most market settings in the real world, requiring governments to correct existing market deficiencies.

The presence of externalities is a prominent example of a setting in which markets fail. An externality describes a cost or benefit incurred by an external party that is not considered by the agent producing the activity. Therefore, the presence of the negative or positive externality will lead to an over- or underprovision, respectively, of the activity compared to what would be socially optimal. In the case of negative externalities, the price of generating the activity does not capture the cost incurred on society. Hence government intervention is necessary to either limit the quantity of the activity, through cap and trade systems, or internalize the external costs to archive the socially desirable quantity of the activity. For this purpose, the English economist Pigou proposed levying a tax set equal to the marginal cost of the negative externality to correct for the market imperfection (Baumol, 1972).

Global warming has been referred to as one of the greatest market failures by the climate economist Nicholas Stern and the framework of externalities has been used to conceptualize it. Carbon emitted into the atmosphere generates substantial and lasting external costs incurred to society. These include direct health costs arising from pollution as well as future damages as a result of the accumulation of carbon in the atmosphere and the change in global climate. Failure to take these local and global external costs of carbon emissions into account led to the overproduction of carbon emissions over the last centuries. The accumulation of carbon stock in the atmosphere set in motion an irreversible shift in the global climate (IPCC, 2021). To correct this global market failure, governments have been urged to implement policies internalizing current and future costs of carbon emissions released into the atmosphere. These damages incurred to society are often referred to as the social cost of carbon (SCC). Putting a direct or indirect price on carbon emissions is thus in line with economic theory and builds the foundation of this empirical analysis.

3 Evidence from the Literature

This paper extends the growing literature on carbon pricing. As the main policy objective of carbon pricing policies is to curb carbon emissions, scholars so far have mostly focused on the environmental effectiveness of emission trading schemes and carbon taxes. Simulation (ex-ante) and empirical (ex-post) studies found that carbon pricing policies are effective in reducing carbon emissions (Andersson, 2019; Best et al., 2020; Rafatya et al., 2020). However, it remains unclear how those policies and reduced carbon emissions relate to economic growth.

Ex-ante studies on the relationship between the economy and emissions have concentrated on the study of economic damage from GHG emissions and the resulting global temperature increase. Using an integrated assessment model (IAM), Nordhaus and Moffat (2017) concluded that an increase of 3°C in global temperature would reduce global income by 2%. These results call for stringent environmental policies to reduce global carbon emissions and stabilize the climate. The evaluations provide evidence of the economic damages arising from climate change but give no indication of the economic effects of the policies needed to limit emissions.

Empirical studies on the (causal) relationship between carbon emissions and GDP found ambiguous results (Ghosh, 2010; Jahangir Alam et al., 2012; Jalil & Mahmud, 2009; Lu, 2017; Mikayilov et al., 2018). Ghosh (2010) suggested a bi-directional causality between the two variables in the short run, indicating that efforts to reduce carbon emissions could lead to economic decline. Others have found a relative decoupling of emissions and GDP (Mikayilov et al., 2018), meaning that emissions grow less rapidly than the economy, and no directional causality running from carbon emissions to GDP (Jalil & Mahmud, 2009; Lu, 2017).

While these studies investigate the overall relationship between carbon emissions and economic growth, it remains unclear how carbon pricing policies affect the economy. Such environmental policies could slow down economic growth and reductions in emissions achieved by those policies could be merely driven by economic decline. More precisely, pricing carbon could affect the economy through different channels: First, pricing carbon emissions could hurt the economic situation by increasing production costs and diminishing the competitiveness of a country. Increased prices could be passed on to the consumer and could lead to a decrease in consumption spending of households. On the other hand, pricing carbon could also foster technological innovation and economic progress by encouraging companies to pollute less and push unsustainable "dirty" industries out of the market. These two channels through which environmental regulations can affect the economy of a country resulted in two contending theories: the pollution haven hypothesis (Copeland & Taylor, 1994) and the Porter hypothesis (Porter & Van der Linde, 2017).

The pollution haven hypothesis argues that stringent environmental policies increase the costs for businesses, hurting their international competitiveness, and induce profit maximizing firms to leave the country (Copeland & Taylor, 1994). Those firms could relocate to jurisdictions in which the environmental regulatory regime is less strict and thus fewer pollution abatement costs are imposed. This would result in a flow of investments and trade out of the country and thus hurt the competitiveness and overall economic situation of the country imposing tight environmental regulations. Reviewing the literature, Copeland and Taylor (2004) concluded that empirical studies indeed support this hypothesis. They found that environmental regulation is among the determinants of the direction of trade and investment flows, however, could not be identified as the main driver. Studying pollution abatement costs in the US from 1977 to 1986, Levinson and Taylor (2008) showed that a tightening of environmental regulation in the country led to an increase in net imports from Mexico and Canada. Similar studies, however, concluded that the link between environmental regulations and trade flows is weak, and increased pollution abatement costs pose no significant threat to the competitiveness of a country (Jaffe et al., 1995).

On the converse, the Porter hypothesis postulates that stringent environmental policies have the potential to increase the productivity of firms and benefit the economic situation of a country by triggering innovation and technological progress (Porter & Van der Linde, 2017). Notable in the context of this study, Porter and Van der Linde (2017) highlight the potential of emission taxes and cap-and-trade emission allowances to induce innovation and offset the direct costs incurred by compliance with the policies. However, empirical evidence of the links between environmental regulation, innovation, and business performance is mixed. Scholars have found a positive relationship between pollution reduction costs and innovation proxies such as research and development (RD) spending and successful patents (Jaffe & Palmer, 1997; Johnstone et al., 2010; Lanoie et al., 2011). Studying data of more than four thousand firms, Lanoie et al. (2011) concluded, however, that the positive effect of tightening environmental regulations on firm's performance through innovation, does not outweigh the direct negative effect the stricter policy has on business performance. Still, in the long term stricter policies can lead to productivity gains (Lanoie et al., 2008).

Concerns about productivity losses, relocation of firms and a decline in international competitiveness deters policy makers from implementing stringent environmental policies (van Soest et al., 2006). The inconclusive findings from empirical studies on the two discussed contested hypotheses show that no scientific consensus on the relationship between environmental regulations and (firms') competitiveness has been established. These studies have investigated tightening of environmental and pollution control policies in general, often drawing upon proxies for the stringency of these policies. No conclusion can be drawn from these studies regarding the economic effectiveness of carbon pricing policies in particular.

The literature on carbon pricing and economic activity remains to date scarce. Studying the effect of the introduction of carbon pricing policies on emissions, GDP, and employment in British Columbia and in a sample of EU countries, Metcalf (2019) concluded that carbon pricing policies have no adverse effects on GDP, while inducing reductions in carbon emissions. These results, however, should be taken with caution as they rely on Difference-in-Difference and simple dummy regression designs, and thus cannot account for common trends and shocks in GDP between different countries. Similarly, using a general equilibrium model approach, Wills et al. (2022) concluded that pricing carbon is cost effective and can result in economic growth. Contrary to these findings, Känzig (2021) found that tighter carbon price regularities in the EU emission trading system (ETS) led to a temporary fall in economic output.

To sum up, previous literature on carbon pricing has been focused on its environmental effectiveness. While simulations show that anthropogenic climate change, caused by the accumulation of carbon in the atmosphere, will result in substantial damage to global economic activity, policies reducing carbon emissions have also been argued to potentially hurt the economy in the short run. A large part of literature has focused on the relationship between general environmental policy stringency and economic activity, however, few studies have investigated the effect of carbon pricing policies in particular. The empirical studies conducted on the topic are inconclusive and rely on methodologies with rigid model assumptions.

4 Carbon Pricing Worldwide

Pricing carbon, through taxes or trading schemes, has become an increasingly popular policy tool to decrease carbon emissions worldwide. Carbon taxes fix a price on carbon emissions. Emission trading schemes (ETS) allow countries and companies to buy and sell a limited number of emission certificates and therefore indirectly price carbon emissions through the market mechanism.

The first carbon pricing scheme was implemented in Finland in January 1990 through a tax of 1.41 USD per ton of carbon emitted, with exemptions for peat, natural gas, and the wood industry. The tax underwent various reforms in the following years, extending its coverage and increasing the carbon price to 73.02 USD per ton of carbon emissions (2021). Following the first carbon tax implementation in Finland, many countries have followed and implemented a direct or indirect price on carbon emissions. In 2021 prices on carbon ranged from less than 1 USD to 142 USD per ton of carbon emissions and covered 46 national jurisdictions (World Bank, 2022a). An overview of the national carbon pricing initiatives implemented until 2016 can be found in Table C7 (Appendix C).

Reviewing the literature, Wang et al. (2019) could not find a consensus in the literature on the level of effective carbon pricing to achieve net-zero and internalize the externalities of carbon emissions. However, many researchers propose significantly higher global carbon pricing strategies than currently implemented (Kikstra et al., 2021; Wang et al., 2019).

5 Methodology

Different methodological approaches are employed to analyze the impact of carbon pricing on economic growth. This section describes each one.[2]

5.1 The Country Specific Effect of Introducing a Carbon Price

To study the country specific effect of introducing a carbon price on GDP, the synthetic control method (Abadie et al., 2015) and a time series model approach (Harvey & Thiele, 2021) are used. Both methods construct a counterfactual (weighted average of selected countries) for the treated country (Finland) of interest to evaluate changes in the outcome variable (GDP) after treatment (implementation of carbon pricing policy). The time series approach has been argued to be preferable over a synthetic control approach for selecting the counterfactual by accounting for the dynamic properties of the data (Harvey & Thiele, 2021).

5.1.1 Synthetic Control Method

First, the synthetic control method promoted by Abadie et al. (2015) is followed. This makes use of a data driven approach to construct a synthetic unit used to evaluate changes in the outcome variable in the periods after the treatment. This synthetic unit, also called synthetic control (SC), is a linear combination of untreated countries (donors).

[2]The methodological approaches are implemented using the statistical software R. The packages employed to perform the analyses are described in Appendix G.

The weighting of the donors is determined using data from the pretreatment period. More precisely, the synthetic control (y_t^c) is constructed, by choosing weights for the different donors, such that it closely resembles the movement in the outcome variable and other variables, covariates predicting the outcome variable, of the treated country in the pretreatment period. This gives the synthetic control

$$y_t^c = \boldsymbol{w'y_t}, t = 1, ..., T \tag{1}$$

where $\boldsymbol{w}$ is a $Nx1$ vector of weights, $\boldsymbol{y_t}$ is a vector of all N donors and T denotes the entire observation period. Subtracting the synthetic control from the outcome variable of the treated country (y_{0t}) gives the treatment effect at time t: $y_{0t} - y_t^c, t = \tau, ..., T$, where τ is the period in which the treatment occurs.

The main underlying assumption of the synthetic control method is that, in the post-treatment period and in the case of no treatment, the target variable of the treated country would have behaved in the same way as the synthetic control. This key assumption implicitly assumes that the series $y_{0t} - y_t^c$ is stationary in the absence of treatment, meaning that no stochastic trend is present that renders the treatment effect inconsistent. More precisely, for the synthetic control to be valid, balanced growth must hold, meaning that control countries and the treated country must share a common trend. Harvey and Thiele (2021) argue that the balanced growth assumption does not hold in many applications and that the synthetic control model crucially neglects information on the dynamics of the time series.

5.1.2 Common Trend and Time Series Model

To improve upon the synthetic control model suggested by Abadie et al. (2015), this paper employs the approach by Harvey and Thiele (2021) to account for the time series properties of the data. The notation presented in this section thus follows Harvey and Thiele (2021). Employed in the context of the German reunification and a Californian smoking law, this model has been shown to produce a more reliable counterfactual and better results compared to the synthetic control method (Harvey & Thiele, 2021). Replications of these two analyses are presented in Appendix B. In particular, the application of the model to the study of the German reunification, which likewise employs GDP growth as the outcome variable, suggests that fitting a time series model in my setting could provide possible gains over the synthetic control approach presented in Section 5.1.1. The application of this approach is further motivated by the relatively short time series with respect to the large number of donor countries (89 countries) in my setting, which, as expressed in Harvey and Thiele, 2021, p. 73, "leads to a substantial risk of mistaking in-sample overfitting for common trends". The main contributions of the approach proposed by Harvey and Thiele (2021) are, that (i) it makes use of the dynamic properties between individual donors and the treated country in the donor selection strategy to ensure that the synthetic control is valid and (ii) it increases the efficiency of the estimates through a time series modeling approach.

In line with the assumption underlying the synthetic control approach previously outlined, Harvey and Thiele (2021) suggest only considering countries in the donor pool, used for the construction of the counterfactual, that satisfy the balanced growth assumption. In other words, only countries whose outcome variable is on the same growth path as that of the treated country before the treatment are considered for the analysis. The validity of individual countries as controls is assessed using the stationarity test proposed by Kwiatkowski et al. (1992) on the difference between it and the treated country, referred to as the contrast. The donors are then ordered based on the magnitude of the obtained test statistic, indicating how likely it is that each individual control is valid. More specifically, a control country is considered as a donor when the null hypothesis of stationarity is not rejected. The donor countries are selected based on the stationarity test and the variance of their contrast with the treated country.

With this donor selection strategy ensuring that all donor countries have a common trend with the target country, the model can be written as follows:

$$\boldsymbol{y_t} = \boldsymbol{i}\mu_t + \boldsymbol{\mu} + \boldsymbol{\epsilon_t}, t = 1, ..., T, \tag{2}$$

$$y_{0t} = \mu_t + \mu_0 + \lambda d_t + \sum_{j=1}^{m} \lambda_j d_{j,t}^{\;*} + \epsilon_{0t}, t = 1, ..., T, \tag{3}$$

where $\boldsymbol{y_t}$, $\boldsymbol{i}$, $\boldsymbol{\mu}$ and $\boldsymbol{\epsilon_t}$ are $Nx1$ vectors. The common stochastic trend μ_t is modeled by

$$\mu_t = \mu_{t-1} + \delta_{t-1} + \eta_t, \delta_t = \delta_{t-1} + \zeta_t, \tag{4}$$

with δ_t being the slope of the common trend and μ_t being 0 at $t = 0$. Further, λ denotes the permanent treatment effect that remains m periods after the treatment period τ, that is

$$d_t = \begin{cases} 0 & \text{if } t < \tau + m \\ 1 & \text{if } t \geq \tau + m \end{cases}, 1 < \tau + m \leq T, \tag{5}$$

where d_t is referred to as the step dummy, and the gradual adjustment effects over m periods after treatment are modeled through the pulse dummies

$$d^*_{j,t} = \begin{cases} 0 & \text{if } t \neq \tau + j - 1 \\ 1 & \text{if } t = \tau + j - 1 \end{cases}, j = 1, ..., m. \quad (6)$$

The donor countries are modeled through equation (2), where $\boldsymbol{i}$ is a vector of ones, $\boldsymbol{\mu}$ includes the donor specific constants and $\boldsymbol{\epsilon_t}$ consists of a vector of error terms that is multivariate normal and serially independent. It is important to note that through the specification of $\boldsymbol{i}$ all donor countries are modeled through the same stochastic trend. Further, the outcome variable of the treated country is modeled through equation (3). The dynamic specification allows for the modeling of a permanent, long-term treatment effect and the intermediate, short-term treatment effects over $m \geq 1$ time periods. This dynamic response model is appropriate in particular in the setting of this policy study, as gradual adjustments can take place due to lagged implementation or response. The error terms ϵ_{0t}, ζ_t, η_t are normally distributed with mean 0 and variances ω^2_ϵ, ω^2_ζ and ω^2_η as well as serially independent.

In the above model specification all donor countries and the target country share a common trend, namely μ_t, which ensures that all the series are on the same growth path. The selected donor countries, satisfying the balanced growth assumption by construction since only controls being cointegrated with the target were selected, are used to perform two counterfactual analyses: (i) a synthetic control analysis applied to the restricted set of cointegrated donor countries and (ii) a time series modeling approach.

To obtain a synthetic control, a restricted least squares (RLS) estimator for $\boldsymbol{w}$ is obtained. Therefore, the best fit between the target variable modeled in (1) and a weighing of the selected cointegrated donor countries in (2) is found with respect to $\boldsymbol{w}$. The restriction $\boldsymbol{w'i} = 1$ ensures that the difference $y_{0t} - y^c_t$ is stationary. Note that in contrast to the synthetic control method suggested by Abadie et al. (2015) this RLS approach allows for some weights to be negative and therefore better matching of the synthetic control to the target in the pretreatment period. The weighting matrix is then again used to obtain the synthetic control using equation (1). Finally, a stationarity test is performed on the difference between the synthetic control and the target to verify that it is stationary. Employing the selected set of cointegrated donor countries to construct the synthetic control ensures that the balanced growth assumption is satisfied. It, however, fails to make use of all available information on the dynamics in the series in the pretreatment period (Harvey & Thiele, 2021). To increase the efficiency of the estimates, Harvey and

Thiele (2021) propose estimating the treatment effects, the permanent and intermediate effects, by fitting the time series model.

To fit the time series model, I implement a regression approach. First, the common trend μ_t is estimated by fitting the system of equations in (2) for the selected donor countries and restricting the trend to be the same across the countries. For this estimation, all T observations from the donors, chosen through the before described donor selection strategy, are used. A key assumption when using the full sample for the donor countries is that the donors are unaffected by the treatment in the treated country. Then the contrast between the target country and one of the donor countries $y_{0t} - y_{it}$ is regressed on the common trend, the $N-1$ contrasts of the remaining donor countries with y_{it}, the step dummy and the pulse dummies. The resulting single equation estimates are equivalent to the estimates obtained by full maximum likelihood (ML) on equations (2) and (3), as shown in the replication of the German reunification study (Appendix A.2). Through this time series modeling approach that accounts for the dynamics in the common trend and makes use of all donor observations, more information contained in the data set is considered resulting in potential efficiency gains. Indeed, using a Monte Carlo simulation study, Harvey and Thiele (2021) demonstrate that the time series model performs better, by reducing the variance of the estimates, compared to the RLS synthetic control in most cases.

Note that the separate estimation of the common trend, through employing the described regression approach, presents an additional source of uncertainty and estimation error. Fitting the full time series model by full maximum likelihood (ML) could thus further increase the efficiency of the estimators and is recommended when suitable software is available.[3]

5.2 The Average Effect of Introducing a Carbon Price

Building on the country specific analysis presented in the previous section, the paper employs time series cross-sectional data of multiple treated countries with different treatment timings. To allow for this structure in the data, I follow recent advancements in counterfactual analysis under staggered adoption (Xu, 2017). This approach allows for the estimation of the average treatment effect and, therefore, improves upon the external validity of the analysis.

[3]The Structural Time Series Analyser, Modeller and Predictor (STAMP) software was used by Harvey and Thiele (2021) to fit the full model. This software was, however, unavailable for the analysis in this paper.

5.2.1 Synthetic Control with Staggered Adoption

Following the approach by Rafatya et al. (2020) I extend the analysis using a generalized synthetic control model. This model resembles the synthetic control approach by likewise using pretreatment observations to construct a weighting for control countries used to find valid counterfactuals for the treated countries in the posttreatment period. However, the generalized synthetic control (GSC) model proposed by Xu (2017) differs from the original synthetic control approach introduced by Abadie et al. (2015) by combining it with an interactive fixed effects (IFE) model (Bai, 2009). The GSC approach has several advantages. First, it allows for multiple treated countries with different treatment periods, also referred to as staggered adoption. This is, in particular, relevant for the application in this paper since different countries introduced carbon pricing policies at different times. Secondly, it has been shown to be more efficient than the original synthetic control procedure (Xu, 2017) as information from all control countries over the entire sample period is used. Lastly, variance estimates of the average treatment effects on the treated (ATTs) can be obtained from parametric bootstrapping.

To model the outcome variable $y_{i,t}$, consider the following two-way (country and time) IFE model:

$$\Delta y_{it} = \lambda_{it} D_{it} + \boldsymbol{x'_{it}} \boldsymbol{\beta} + \boldsymbol{\gamma'_i} \boldsymbol{F_t} + \alpha_i + \theta_t + \epsilon_{it}, \text{for } i \in 1, ..., N, \tag{7}$$

with treatment of country i at time τ_i and the country specific treatment dummy

$$D_{it} = \begin{cases} 0 & \text{if } t < \tau_i \\ 1 & \text{if } t \geq \tau_i \end{cases}, \tag{8}$$

where λ_{it} captures the treatment effect of a (treated) country i at time t, $\boldsymbol{x'_{it}}$ is a vector of time-varying observed covariates and α_i and θ_t denote the country and time fixed effect respectively. Further, the vector $\boldsymbol{F_t}$ captures time-varying unobserved trends and shocks, also referred to as latent factors. These latent factors affect the outcome variable through the country-specific coefficient vector $\boldsymbol{\gamma'_i}$, called the factor loadings. The outcome variable is transformed through first differencing as nonstationarity is plausible in my application and would lead to spurious results.

To produce consistent estimates, the model assumes strict exogeneity of the idiosyncratic error terms ϵ_{it}. This means that, given the observed covariates, latent factors, and factor loadings, the disturbances must be independent of the policy treatment. Further, the assumption of all disturbances being cross-sectionally independent has to hold.

The counterfactuals for the treated countries are obtained from this model through an out-of-sample prediction method following Xu (2017). First, the IFE model is estimated using only control group data, of the untreated countries (Bai, 2009). Using an iterative approach, a fixed optimal number of factors is selected, whose limited amount reduces the risk of overfitting the model. The estimated IFE model specification is then employed to estimate factor loadings for each treated country using pretreatment data on the outcome variable of the treated countries. Based on these estimated factor loadings and factors, the counterfactuals are obtained.

From this model, the average treatment effect over all treated countries in period t is

$$\widehat{ATT}_t = \frac{1}{n_t^{Tr}} \sum_{i \in Tr} \hat{\lambda}_{it}, \text{for } t = \tau^*, ..., T \tag{9}$$

where n_t^{Tr} is the number of treated countries in period t and τ^* denotes the period in which the first country is treated. The overall average treatment effect over all periods is obtained by computing the average of $\widehat{ATT}_t$ over all periods.

5.2.2 Cointegrated Donor Selection Strategy under Staggered Adoption

This paper presents a novel approach to improve upon the validity of the counterfactual analysis for a panel of treated countries by combining the donor selection strategy outlined in Section 5.1.2 with the generalized synthetic control method described in Section 5.2.1.

The generalized synthetic control approach does not allow to retrieve the weights used for the construction of the counterfactual of each treated country, as the weights are not uni-dimensional (Xu, 2017). Therefore, the compliance of each counterfactual, and the respective control countries used to construct it, with the balanced growth assumption cannot be verified. However, the risk of overfitting can be reduced by eliminating untreated countries validating the balanced growth assumption for all treated countries from the analysis. In other words, only control countries cointegrated with at least one of the treated countries are considered as donors. Control countries not satisfying the balanced growth assumption for any of the treated countries are excluded from the donor pool.

In line with the previously outlined methodology, the stationarity test introduced by Kwiatkowski et al. (1992) is performed on the differences between each untreated country $j \in nTr$ and every treated country $i \in Tr$, where nTr is the set of untreated countries and Tr is the set of treated countries in the sample period. Under staggered adoption the treatment timing can vary across treated countries resulting in different length of the pretreatment period. For comparability the stationary test is thus performed on the same T periods before treatment of each country $i \in Tr$. All controls for which the stationarity test, with the null of cointegration, is not rejected for at least one of the treated countries are included in the donor pool. This cointegrated donor pool is then employed to repeat the generalized synthetic control approach outlined before.

Note that this novel donor selection strategy for the synthetic control analysis under staggered adoption does not ensure that the balanced growth assumption is satisfied for the counterfactual of each treated country. The approach, however, increases the likelihood of the supposition to hold by excluding control countries for which the assumption surely is not fulfilled.

5.3 The Carbon Price Elasticity of Economic Growth

The setting of analyzing carbon price policies allows not only for the analysis of the (average) treatment effect, the extensive margin, but also the intensive margin of the effect. Specifically, we now turn to investigating if heterogeneity in the treatment effects (within and between countries) can be explained through heterogeneity in the intensity of the treatment effect. In other words, I want to answer the question whether a higher carbon price level results in a stronger effect of carbon pricing policies on GDP growth or if solely the introduction of a non-zero carbon price, irrespective of its level, affects the outcome variable.

To do so, I follow the approach proposed by Rafatya et al. (2020) to estimate elasticities based on the average treatment effect obtained from the counterfactual analysis with staggered adoption. The treatment effect is therefore considered to be a linear function of an introduction effect of the treatment (α), regardless of the price level, and a price effect (ρ): $\lambda_{it} = f(\alpha_i, \rho_i, p_{it}) = \alpha_i + \rho_i p_{it}$, for $i \in Tr$ and for $t = \tau^*, ..., T$, where p_{it} is the carbon price level in country i at time t. The underlying assumption for this specification, decomposing the introduction and price effect, is that the changes in the carbon price are strictly exogenous. This has been argued to be a reasonable assumption in the setting of carbon price studies (Rafatya et al., 2020). Further, simultaneity can be assumed to be no issue as it has been suggested that there does not exist a feedback effect of GDP on the carbon price and the decision for its introduction (Metcalf & Stock, 2021).

The estimates of the average treatment effect are employed to estimate the introduction and price effect. I use the within and between country variation in the treatment effect to differentiate between a country specific introduction and price effect. A variable coefficient fixed effects panel data model, a fixed effects panel data model and a pooled panel data model are used to estimate

$$\hat{\lambda}_{it} = \alpha_i + \rho_i p_{it}, \tag{10}$$

where ρ_i is the country specific carbon price elasticity of GDP growth or in other words the change in the GDP growth rate due to a 1$ increase in the carbon price. Lastly, an F-test on poolability is performed on both the introduction and price effects, to investigate if those effects are homogeneous across treated countries and to identify the appropriate panel data model specification.

6 Data

To analyze the effect of carbon pricing policies on the economy, data on GDP per capita in constant 2015 US$ is obtained from the World Bank (2022b) over a period of 46 years from 1971 to 2016.[4] This time period is chosen based on data availability and sufficiently long pre- and post-treatment periods. The real GDP per capita data is transformed using a natural logarithmic transformation, as proposed by Harvey and Thiele (2021), to reduce the skewness of the variable and therefore make the variable follow the normal distribution closer. Further, data on the implementation timing of carbon pricing policies in different countries is used, first presented in Dolphin et al. (2020) and updated by Rafatya et al. (2020). This data-set allows construction of a panel of 38 countries that implemented carbon pricing from 1990, the period of the first implementation, until 2016 (and 89 countries that did not implement a carbon pricing policy during that time). Data for the years that countries have phased out their carbon pricing schemes is excluded from the sample, concerning Australia, Ukraine (both 2015 and 2016) and Kazakhstan (2016).

Descriptive statistics of the data employed are presented in Table C8 (Appendix C), together with the variable codes used to retrieve data from the World Bank (2022b) (Table C10).[5]

6.1 Covariates

For the baseline specification in the models that include covariates, I employ a subset of the GDP predictors suggested in similar counterfactual analyses on economic growth

[4] Real GDP per capita, at constant prices, is used to account for inflation.

[5] Data from the World Bank (2022b) is retrieved in the statistical software R using the wbstats package (version 1.0.4).

(Abadie et al., 2015).[6] Therefore, country specific data on gross capital formation (as % of GDP) and trade openness measured as exports plus imports (as % of GDP) are obtained from the World Bank (2022b). Moreover, I make use of inflation data presented in Ha et al. (2021). Data on all variables is collected over the sample period from 1971 to 2016. Further covariates suggested by Abadie et al. (2015) measuring educational attainment and industry value added are excluded from the analysis due to the high number of missing observations in the sample period (Table C9, Appendix C).

6.2 Sample for Study of Finland

In the country specific study of Finland, the baseline analysis is conducted over the period from 1980 to 2004, with the carbon tax being introduced in Finland in 1990. The beginning of this time period is chosen due to data availability of the covariates and GDP per capita data, and provides 10 pre-treatment periods. 20 countries implemented a carbon pricing scheme in 2005, after the European Union emissions trading system (EU ETS) was launched in that year (Table C7, Appendix C). Thus, the sample period until 2004 allows for the inclusion of more control countries and the analysis of 14 post-treatment periods. This long observational time-period after the implementation creates an interesting case, as it allows study of short and long term effects of the policy. For this part of the analysis, countries having implemented a carbon pricing policy in the sample period are excluded from the sample (5 countries). Further, only countries with complete GDP per capita data and without missing data for the covariates in the sample period are considered, resulting in 78 countries in the control group.

6.3 Sample for Study of Panel of Treated Countries

For the analysis of the average effect of implementation across countries the period from 1971 to 2016 is analyzed in the baseline analysis. This allows for at least 19 pre-treatment years for every country. In accordance with Rafatya et al. (2020) the control group is restricted to those countries whose average per capita GDP over the sample period was at least as high as the lowest average of the variable among the treated countries. This leaves 36 treated countries and 15 untreated countries.[7] The robustness of the results and the estimated counterfactual is analyzed by varying the sample period, including a different set of countries and excluding covariates from the analysis.

[6] Covariates are included in the synthetic control approach in the study of Finland and the generalized synthetic control approach studying the average treatment effect.

[7] Sweden and Norway are excluded in the baseline analysis, due to the significantly higher carbon prices in these countries.

6.4 Emission Adjusted Carbon Prices

The setting of carbon pricing policies and their implementation constitutes a particularly interesting case, as the treatment intensity can be measured through the carbon price that each country sets. To explore this heterogeneity in the treatment intensity further, I obtain country specific carbon prices over the period of 1990 to 2016 from Rafatya et al. (2020). These data are based on the World Carbon Pricing Database provided by Dolphin et al. (2020), which contains information on the level of the carbon price for 198 countries, the mechanism through which the price is applied, the coverage of the different carbon pricing policies and is disaggregated at the sectoral level.[8] The country specific carbon prices provided by Rafatya et al. (2020) are adjusted for the coverage of the carbon pricing schemes to allow for the comparison of carbon prices between and within countries. This is of importance as different pricing schemes can apply to different shares of carbon emission in distinct countries and coverage within a country can vary over time.[9]

7 Results

7.1 The Effect of Introducing a Carbon Price

To investigate the effect of carbon pricing on economic growth, this section presents two counterfactual analyses. Firstly, the implementation of the carbon tax in Finland in 1990 is studied. This country specific study is then extended by looking at a panel of countries having implemented a carbon pricing policy between 1990 and 2016.

7.1.1 The Effect of Introducing a Carbon Price in Finland

This analysis addresses the question whether the implementation of the carbon tax of 1990 in Finland had an effect on the real per capita GDP of the country. To analyze this effect, the paper aims to construct a valid counterfactual drawing from data of 78 control countries over the period from 1980 to 2004.

First, weights for the synthetic control are obtained using data on real GDP per capita and the before presented predictors of economic growth over the pretreatment period from 1980 to 1991. Following the approach outlined in Section 5.1.1, Japan (weight of 0.377), the United States (0.211) and Switzerland (0.115) are identified as the main donors for

[8]For a full description of the data provided by Dolphin et al. (2020) and its sources, see https://github.com/g-dolphin/WorldCarbonPricingDatabase

[9]Please refer to Rafatya et al. (2020) and Dolphin et al. (2020) for more information on the computation of the emission-weighted carbon prices.

the construction of the synthetic Finland. Next to these three main donor countries, the other 75 countries in the donor pool receive small weights.

Next, it is investigated whether the balanced growth assumption holds for Japan, Switzerland and the United States. An inspection of the dynamic behavior of the control countries suggests that the assumption is not satisfied for Switzerland and Japan (Figure 7.1). The contrasts of both countries with Finland move towards 0 in the pretreatment period, from opposite directions. When combined in the synthetic control, these different trends could offset each other and result in an invalid SC through the inclusion of countries not on the same growth path as Finland before 1990. The United States seems to be on the same growth path as Finland as the gap remains approximately the same from 1980 to 1990.

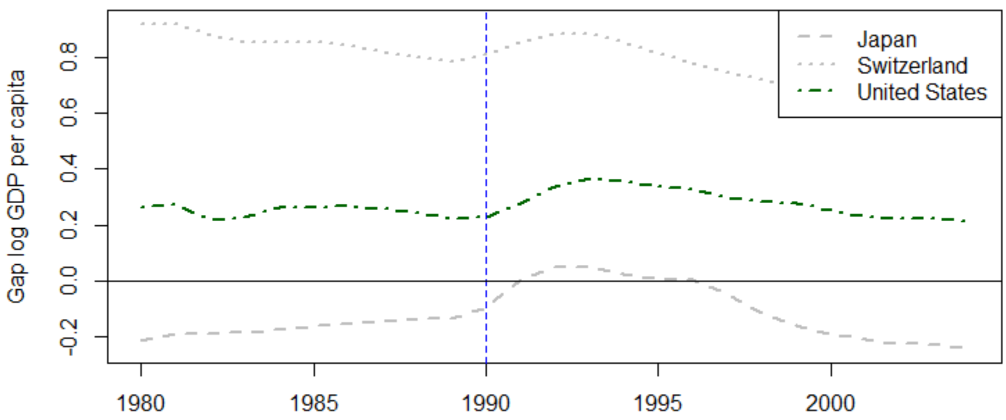

Figure 7.1. Contrasts of Japan, Switzerland and the United States with Finland

The balanced growth assumption of the donor countries is further investigated through a Kwiatkowski–Phillips–Schmidt–Shin (KPSS) balanced growth cointegration test (column 3 of Table 1). This test supports the preceding analysis that Japan and Switzerland are not on the same growth path as Finland before 1990, rejecting the null hypothesis of cointegration at the 10% significance level for both countries. This makes both countries unsuitable controls and suggests to replace these two donors through countries being cointegrated with Finland. For the US the balanced growth assumption seems to hold. Of the 78 control countries, 22 are found to be cointegrated with Finland from 1980 to 1990 and therefore considered as controls (Table D11, Appendix D). Next to the KPSS balanced growth cointegration test, the dynamic behavior of each country is investigated by means of two additional KPSS tests, testing for the presence of a stochastic trend (column 1 and 2 of Table D11, Appendix D) and a stochastic slope (column 3 and 4 of

Table D11, Appendix D) in the individual series. It is desirable that the target and each of the donors have the same trend in the pretreatment period. Thus, the donor countries should not only be cointegrated with Finland but also have the same order of integration. It is reassuring to see that both Finland and the United States seem to be best modeled as an I(1) series (Table 1). Further, on the basis of the KPSS results, India and the United Kingdom are included in the control group to replace Switzerland and Japan. Both countries are cointegrated with Finland, seem to be following an I(1) process and have the lowest variance among the cointegrated donor countries.

Table 1. *KPSS tests for log annual real per capita GDP from 1980 to 1990*

	I(0)	I(1)	Coint (level)	variance
Finland	0.1355*	0.2099	-	-
United States	0.1011	0.1491	0.1723	0.0004
India	0.1292*	0.2305	0.2743	0.0002
United Kingdom	0.1232*	0.1946	0.3124	0.0002
Switzerland	0.1418*	0.3324	0.4611*	0.0020
Japan	0.1437*	0.3667*	0.5050**	0.0011
RLS-SC	0.47475**	0.2418	0.33811	0.0001

Notes. [**]5% level of significance. [*]10% level of significance. All KPSS tests use a number of lags of 2, found to give the best balance between power and size of the test in this application.

A plot of the contrasts once more highlights the suitability of the UK, the US and India as donors, as the individual contrasts and the SC contrast move roughly parallel to the x-axis prior to 1990 (Figure 7.2). The RLS weights for the three countries over the period from 1980 to 1990 are 0.454 for India, 0.300 for the UK and 0.246 for the US. It is encouraging to observe that the constructed RLS-SC is also best modeled as an I(1) process and is cointegrated with the target Finland (Table 1). A closer look at the contrast between the RLS-SC and Finland indicates that the carbon pricing policy in 1990 might indeed have had an effect on the economy of the country (Figure 7.2).

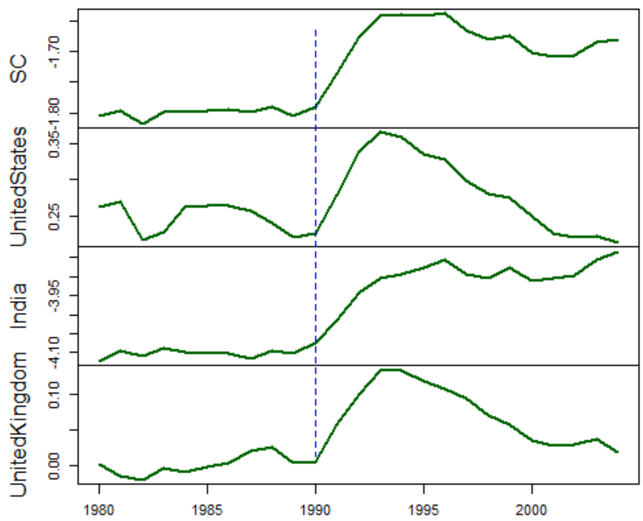

Figure 7.2. Contrasts of India, the UK, the US and the RLS-SC with Finland

The constructed donor pool consisting of the UK, the US and India was used to estimate the transitory and permanent effects of the carbon tax. To make a choice on the number of suitable pulse dummies, representing the transitory effect of the policy, a structural break test was performed, identifying 1996 and 1999 as structural breaks in the target during the post-treatment period.[10] It is important to note that these structural changes in the GDP per capita data of Finland are potentially caused by exogenous shocks other than the impact of the carbon pricing policy. Finland acceded into the European Union in 1995 and joined the eurozone in 1999. Both entries are likely to have affected the economic situation of the country. Since it is unclear if those or other events caused the structural break in the series and no specific event was identified for 1996, I chose to assume that the carbon pricing policy effect stabilized by 1996. Therefore, a single equation model with 5 pulse dummies and a step dummy was fitted.[11]

The estimated intervention effects for the full model, referred to in the table as univariate model, and the SC regression results using the computed RLS weights are compared in Table 2. Both models suggest that the GDP per capita in Finland declined relative to its constructed counterfactual in the posttreatment period after 1990. The full model estimates this decline to be larger. In 1991 Finland's per capita output was about 7.7% lower compared to the computed counterfactual, based on the estimates obtained from

[10]The number of 2 structural breakpoints was found to be optimal based on minimizing the BIC (Figure D1, Appendix D).

[11]A robustness analysis of the results for the breakpoint choice can be found in Table D12 (Appendix D).

the full time series model (Table 2).[12] This gap in real GDP per capita further widened in the subsequent years, leading to a total decline of Finland's economy of approximately 16% in 1995. The sharp increase of the gap between 1991 to 1993 shows that economic growth of Finland declined severely in that period. The closing of the GDP per capita gap after 1996, shown through the step dummy estimate in 1996, indicates that the countries economy slowly recovered after 1996.

Finland's economic decline after 1990 is found to be robust to the choice of the level break (Table D12, Appendix D). The interpretation that this decline is caused by the introduction of the carbon tax in 1990 relies on the assumption that, without the implementation of the carbon tax, Finland would have proceeded to have the same growth path as the counterfactual consisting of the US, the UK and India.

Table 2. *Estimated intervention effects for Finland; level break in 1996*

	Univariate model			SC (RLS)		
Year	Estimate	SE	Gap in GDP (pc) in percent	Estimate	SE	Gap in GDP (pc) in percent
1990	-0.0186	0.0179	-1.84	-0.0101	0.0170	-1.00
1991	-0.0730***	0.0180	-7.70***	-0.0639***	0.0170	-6.19***
1992	-0.1330****	0.0186	-12.45****	-0.1237****	0.0170	-11.64****
1993	-0.1690****	0.0190	-15.55****	-0.1574****	0.0170	-14.56****
1994	-0.1749****	0.0198	-16.05****	-0.1605****	0.0170	-14.83****
1995	-0.1776****	0.0222	-16.27****	-0.1577****	0.0170	-14.59****
1996	**-0.1478******	**0.0233**	**-13.74******	**-0.1178******	**0.0074**	**-11.11******

Notes. Abbreviations: SC, synthetic control; SE, standard error; RLS, restricted least squares; Donors: United States, United Kingdom, India; [****]0.1% level of significance. [***]1% level of significance. [**]5% level of significance. [*]10% level of significance.

7.1.2 The Average Effect of Introducing a Carbon Price

Next the average effect of a carbon price policy introduction is evaluated using the generalized synthetic control method.[13] Norway and Sweden are excluded from the analysis due to the significantly higher carbon prices in the two countries. The sample is restricted to countries with data for at least 5 pre-treatment periods as suggested in Rafatya et al. (2020), leading to an exclusion of Poland and Slovenia from the analysis. This leaves a panel of 34 treated countries, having implemented such a policy between 1990 and 2016,

[12] The gap in GDP (pc) in Table 2 is obtained by rearranging the formula of the intervention effects to $\frac{GDP(pc)_{Finland,t}}{GDP(pc)_{Counterfactual,t}} = \exp(estimate_t)$.

[13] The analysis is performed using the gsynth package in R with two-way fixed effects, the IFE estimator, the cross validation procedure to select the optimal number of factors from 0 to 5, 1000 bootstrap runs, parallel computing and a minimum number of 5 pretreatment periods.

and 15 untreated countries between 1971 and 2016 for the baseline analysis. The study is extended by restricting the donor pool to only untreated countries being cointegrated with at least one of the treated countries 10 years prior to the policy implementation in the respective country. This leads to the exclusion of 5 countries from the donor pool. For these countries the KPSS stationarity test on its contrast with each treated country is rejected for all treated countries (Table E13, Appendix E), meaning that the balanced growth assumption does not hold. It is reassuring to see that the remaining 10 donor countries are on the same growth path with at least half of the treated countries.

The estimation output for the average treatment effect, among the 34 countries having implemented such a policy between 1990 and 2016, for the original donor pool of 15 countries and for the cointegrated donor pool of 10 countries is shown in Table 3. The estimates and standard errors obtained from the GSC approach using the two different donor pools are close, indicating that the approaches perform similarly well. This suggests that it is reasonable to assume that the balanced growth assumption holds for the counterfactuals produced using the original donor pool.

Both analyses suggest that on average the introduction of a carbon pricing policy did not affect the economic growth of a country in the long run. This finding is visualized in Figure 7.3 showing the average effect of carbon pricing on economic growth using the baseline specification with the original set of donors. The average effect over all treated countries and periods (average ATT) is not significantly different from 0 and thus growth in GDP per capita in the post-treatment period does not differ significantly from the estimated counterfactual. In the short run a significant negative effect of carbon pricing on economic growth is found after 5 and 6 years, relative to the policy implementation, using the baseline model. This indicates that growth in per capita GDP is approximately 2.1 percentage points (SE = 0.9 points) and 2.2 percentage points (SE = 1.1 points) lower in these post-treatment years. A similar short run effect is found for the years 6 and 7 after carbon pricing implementation when employing the cointegrated donor pool. Estimates for the average treatment effect beyond 12 years of implementation should be interpreted with caution. These long-run effects were only observed in 2 countries (Finland and Denmark), due to the rather recent implementation of carbon pricing policies in most countries.

The zero average treatment effect across countries and time is found to be robust to the inclusion of Sweden and Norway in the analysis, but sensitive to the sample period and inclusion of covariates (Table F15, Appendix F). A significant negative effect of carbon pricing policies on GDP growth is found when studying the sample period from 1980 to

2016 and when excluding covariates from the baseline analysis. The negative short run effect has been found to be most expressed in the analysis involving the sample from 1980 to 2016 (Figure F5, Appendix F). The insignificance of the long-run effect (10 or more years after implementation) is found to be robust to the model specification (Figure F1, Figure F3 and Figure F5, Appendix F).

Table 3. *Generalized synthetic control analysis 1971-2016 with countries having implemented carbon pricing policy; Dependent Variable:* $\Delta log(GDPpc)_{i,t}$

	Original donors		Cointegrated donors		
Years relative to implementation	ATT	SE	ATT	SE	number of treated countries
1	-0.0043	0.0086	-0.0037	0.0079	34
2	-0.0017	0.0078	-0.0011	0.0086	33
3	-0.0027	0.0349	0.0001	0.0116	32
4	-0.0118	0.0097	-0.0109	0.0124	30
5	-0.0209**	0.0087	-0.0153	0.0146	28
6	-0.0219*	0.0114	-0.0264**	0.0105	27
7	-0.0143	0.0093	-0.0218*	0.0127	27
8	-0.0045	0.0064	-0.0150	0.0097	27
9	-0.0057	0.0101	-0.0151	0.0125	26
10	0.0079	0.0091	0.0001	0.0115	24
11	0.0138	0.0106	0.0007	0.0125	22
12	0.0071	0.0087	-0.0053	0.0106	22
13	0.0005	0.0301	-0.0046	0.0355	2
14	-0.0044	0.0332	-0.0117	0.0333	2
15	0.0024	0.0257	-0.0018	0.0296	2
16	-0.0186	0.0299	-0.0215	0.0355	2
17	0.0012	0.0291	-0.0198	0.0356	2
18	-0.0040	0.0260	0.0040	0.0428	2
19	-0.0094	0.0326	-0.0104	0.0354	2
20	-0.0363	0.0238	-0.0335	0.0396	2
21	-0.0089	0.0254	-0.0159	0.0294	2
22	-0.0059	0.0259	-0.0153	0.0353	2
23	-0.0062	0.0197	-0.0168	0.0291	2
24	-0.0025	0.0256	-0.0162	0.0309	2
25	0.0059	0.0211	-0.0062	0.0278	2
26	-0.0056	0.0353	-0.0215	0.0419	1
27	0.0138	0.0275	-0.0008	0.0358	1
average ATT:	**-0.0055 (0.0047)**		**-0.0096 (0.0066)**		

Notes. Abbreviations: ATT, average treatment effect over all countries having implemented a carbon pricing policy in the sample period; SE, bootstrap standard error; [**]5% level of significance. [*]10% level of significance; Poland and Slovenia are excluded from analysis due to missing GDP per capita data in the sample period.

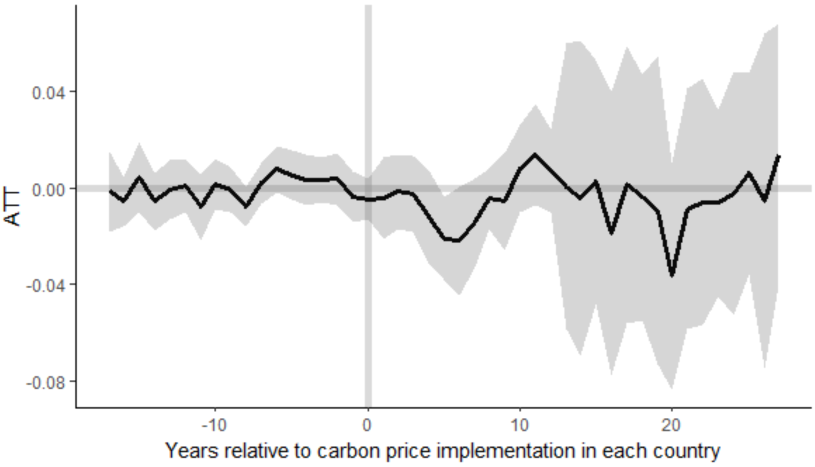

Figure 7.3. Average treatment effect of the introduction of carbon pricing

Notes. The figure shows the estimated treatment effect, difference between observed and counterfactual, and the 95% confidence interval (shaded), based on the bootstrap standard errors, using the baseline model with the full set of 15 donors.

Figure 7.4 shows the estimated average treatment effect for each country, together with the average carbon price level set in the respective country over the post-treatment period for the original donor pool. The bootstrap confidence intervals for the average treatment effects of the countries confirm that an implementation of a carbon pricing policy does not affect the growth of per capita GDP in most countries. Kazakhstan and Ukraine are the exception, however, both countries implemented a very low carbon price recently and abolished it in 2016 and 2015 respectively. Therefore, the validity of the estimates for these two countries is dubious. The carbon pricing policy effects do not differ strikingly between the countries and there does not seem to be a clear relationship between the level of the carbon price and the effect of the policy on economic growth of a country. This relationship is further investigated in the following section.

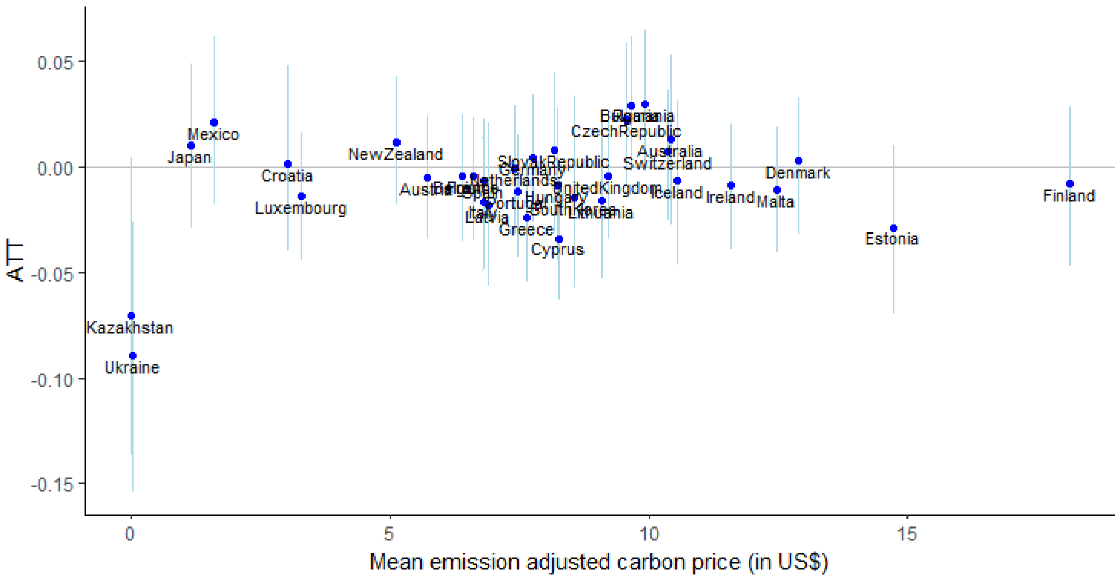

Figure 7.4. Average treatment effects of each country with 95% confidence interval

7.2 The Carbon Price Elasticity of Economic Growth

The previously presented findings indicate that the introduction of a carbon pricing policy does not affect the overall economic growth of a country. It remains unclear, however, what role the level of the carbon price plays for this relationship. The treatment effects of each country over the treatment period obtained from the baseline model and the corresponding implemented carbon prices are used to investigate this relationship further. Estimating a variable coefficient panel data model with country fixed effects, a panel data model with country fixed effects and pooled coefficients as well as a pooled panel data model suggests the poolability of the carbon price coefficients across countries (Table 4).[14] In other words, the introduction effect of the carbon pricing policy, irrespective of the price level, seems to differ between countries, while the price effect is the same across countries. The price effect, elasticity of economic growth with respect to the carbon price level, is found to be insignificant (Table 4), in line with the earlier visual inspection of the relationship. The small estimate and standard error indicate that the level of the carbon price does not affect the change in GDP per capita growth.[15]

These findings are robust to using the cointegrated donor pool, the inclusion of Norway and Sweden, countries with significantly higher prices on carbon, the exclusion of covariates and the alteration of the sample period (Table F16, Appendix F). Further, the country specific introduction effects are presented in Table E14 (Appendix E).

Table 4. *Estimation results country fixed effects panel data model with pooled price effect; Dependent Variable:* $\Delta log(GDPpc)_{i,t}$

Model	Estimate	p-value	F-test for poolability of carbon price coefficient	F-test for poolability of country fixed effects
Baseline (original donors)	-8.6881×10^{-5} (2.1384×10^{-4})	0.6848	p = 0.9968	p = 0.0000

Notes. The SE is reported in parenthesis. 343 treated observations are included in the analysis. Countries with less than 5 treatment periods are not included in the estimation. The null hypotheses of the F-tests is poolability.

[14]For this part of the analysis the sample of treated countries is restricted to those countries with at least 5 years of treatment to allow for the estimation of the variable coefficient panel data model with country fixed effects.

[15]Results can be insignificant due to uncertainty of the true result, so whether the effect is positive or negative, or due to a true null hypothesis, so the absence of an effect. The small standard error and small estimate indicates that the non-significance of the elasticity estimate is likely caused by the latter.

8 Conclusion

This paper examined the economic impacts of carbon pricing policies by inspecting the extensive and intensive margin of the effect. I investigated whether the introduction of a price on carbon affected the economic growth of countries in general and of Finland in particular. The study was extended by looking at the elasticity of GDP growth with respect to the level of the carbon price.

For the analysis this paper employed data on GDP per capita, predictors of economic growth and implemented carbon prices, adjusted for their coverage of emissions, for 127 countries between 1971 and 2016. Of these countries, 89 did not implement a carbon pricing policy in the sample period, allowing for the study of the effect of such policies through counterfactual analysis.

First, the effect of the carbon tax implemented in Finland in 1990, being the first pricing scheme on carbon worldwide, was investigated through a time series framework. Graphs of the series and stationarity tests were used to inspect the growth path of GDP per capita in the different countries before 1990. Accounting for the dynamic properties of the controls and target allowed for the selection of valid controls being on the same growth path as Finland in the pretreatment period. For the data at hand, the UK, the US and India were found to resemble the trend of per capita GDP in Finland the best. Using these selected countries satisfying the balanced growth assumption, economic impacts of the carbon tax in Finland until 2004 were estimated by means of a synthetic control and a time series modeling approach. The country specific study was extended by looking at the average introduction effect across all countries having implemented a carbon pricing policy, either a carbon tax or emission trading scheme, between 1990 and 2016. Employing a generalized synthetic control approach with staggered adoption allowed investigation of the treatment effect in different countries as well as improvement upon the external validity of the results by estimating the short and long run average treatment effects across counties. A novel approach for the donor selection strategy under staggered adoption was introduced to assess and improve upon the validity of the counterfactuals constructed through the generalized synthetic control approach. Excluding control countries not on the same growth path as the treated countries was found to perform similarly well compared with the results using the original unrestricted donor pool. At the same time the risk of overfitting was reduced. Further, heterogeneity in the carbon price level and estimated treatment effects, within and between countries, was analyzed through a panel data model, testing for the poolability of the introduction and price effect.

In the country specific study of Finland it was found that the countries' GDP per capita decreased significantly after 1990, the year the country implemented the carbon tax, compared to the constructed counterfactual. The decline was estimated to be stronger using the time series modeling approach compared to the RLS-SC estimate. The estimation output for the generalized synthetic control method analyzing the whole panel of countries, having implemented a carbon pricing scheme between 1990 and 2016, suggested that on average carbon pricing policies do not affect the economic growth of a country in the long run. The policies can, however, temporarily lead to a decline of economic growth in the short run. The magnitude of the negative short run effect was detected to be sensitive to the model specification. Further, it was found that merely the introduction of a carbon price, not its level, affected economic growth of a country. This result was found to be robust to the model specification. In other words, the results suggest that economic growth is not elastic with respect to the level of the carbon price.

Carbon pricing, therefore, was detected to affect the economy merely through the extensive margin in the short run, but not through the intensive margin. This indicates that policy makers can commit to higher prices on carbon emissions, which are essential to limit emissions and global warming.

9 Limitations and Further Research

Causal interpretation of both counterfactual analyses relies on the construction of a valid counterfactual and forms the main limitation of this research.

In the country specific study of Finland, this means that the results presented in this paper rely on the assumption that Finland's economic growth would have continued on the same path as its counterfactual in the case of no carbon tax implementation. However, this period saw a decline in the economy of many Western countries, due to the recession in the early 1990s caused among other factors by the 1990s oil price shock. Causal inference of my results thus relies on the unverified assumption that the recession affected Finland similar to its control countries, the United States, the United Kingdom and India. The validity of this assumption is doubtful since Nordic countries such as Finland were severely affected by this crisis. Therefore, the country specific results are likely to be overestimated and the study does not allow for the distinction of the economic effect caused by the carbon tax and by factors inducing the recession.

In the generalized synthetic control method, the validity of the counterfactuals and the estimated effects is difficult to assess, as the donor countries for each treated country cannot be identified. The novel donor selection strategy presented in this paper is a first step towards judging the validity of the counterfactuals, however, does only allow to assess the violation, not the verification, of the balanced growth assumption. An additional limitation stems from the limited set of countries having implemented a carbon pricing policy to date and the short period of implementation in most countries. In particular, the reported average long-term effect of the policies should be interpreted with caution, as only a few countries allow for the evaluation of the policies over more than 10 years. Moreover, the found absence of a relationship between carbon price level and economic impact of the policy is restricted to the carbon prices studied. More specifically, it could be that significantly higher carbon prices than currently implemented would affect the economic situation of a country. The robustness of the results to the inclusion of Sweden and Norway in the sample studied is, however, reassuring that higher carbon prices within current limits do not correspond with larger adverse effects on the economy of a country. So far mostly developed and high income countries have implemented prices on carbon emissions. The reported heterogeneity in which the implementation of a carbon price affects countries, even among predominantly developed countries, indicates that the results cannot be generalized. Therefore, the findings cannot be used to make reliable and accurate GDP forecasts when studying the economic impact of different global carbon pricing strategies proposed to mitigate climate change. The estimated effects can merely suggest a range for the response of global economic growth to future carbon policies.

Further research should be carried out with regards to identifying the economic impacts of country specific emission reduction pathways. The study of Nordic countries is hereby of particular interest due to the early implementation of comparably high carbon prices in those countries. The simultaneous occurrence of the recession in the early 1990s, coinciding with the period that countries such as Finland, Sweden or Norway implemented carbon taxes, poses a difficulty for the construction of valid counterfactuals and the evaluation of the policies. Measures accounting for the divergent severity in which countries were affected by the 1990s recession should thus be considered to identify the effect of the carbon pricing policies in Nordic countries. Studying countries having implemented carbon pricing policies more recently, in particular after 2004, largely restricts the pool of donor countries due to the EU ETS enacted in 2005. More recent data on carbon prices in different countries is needed to study the long-term effects in those countries.

Extending the country specific study by analyzing a panel of treated countries allowed for the study of the general effects of carbon pricing policies across countries. The presented cointegrated donor selection strategy for synthetic control methods under staggered adoption is a beginning for testing the assumptions underlying this method. Further research on ways to test and verify the assumptions of the generalized synthetic control approach, in particular the balanced growth assumption, is needed.

To draw a more conclusive picture on the economic effects of carbon pricing policies, its effect on different economic variables, such as unemployment, firms' international competitiveness, (sustainable) investments and technological progress, should be investigated using the methods proposed in this paper. Findings from this additional research would help to inform policy makers on the channels through which pricing carbon affects the economy. In particular the distributional effect of such policies on the socioeconomic inequality within and between countries is an important area of future research, as the impact of carbon pricing policies is likely to be a function of adaption capabilities of the system to which it is applied. The revenues from carbon pricing policies could be used to compensate groups or countries identified to be hurt the most by these policies.

Higher carbon prices implemented in more countries in the future could be informative with regards to the carbon price elasticity of economic impacts resulting from the policies. The study of more countries and carbon pricing policies will also enable more accurate forecasts on the economic impact of future carbon pricing strategies. These trajectories would contribute towards cost-benefit analyses of carbon pricing policies. Consequently, this would strengthen the consensus among scientists and decision makers on the level of effective carbon pricing to achieve net zero and alleviate the economic consequences of climate change and mitigating policies.

10 References

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A Appendix: Abbreviation List

Table A1. *Abbreviations used in this paper and their meaning.*

Abbreviation	Meaning
IPCC	Intergovernmental Panel on Climate Change
GHG	Greenhouse gas
CO2	Carbon dioxide
COP26	26th Conference of the Parties
IAM	Integrated Assessment Model
ETS	Emissions Trading System
GDP	Gross domestic product
SC	Synthetic control
GSC	Generalized synthetic control
IFE	Interactive fixed effects
RLS	Restricted least squares
KPSS	Kwiatkowski–Phillips–Schmidt–Shin

B Appendix: Replication

To show the validity of the methods employed in the study of carbon pricing in Finland, the paper replicated the results of Harvey and Thiele (2021). Investigating the growth path of the control units and the treated unit, they propose a novel approach to estimate dynamic treatment effects. This method, described in Section 5.1.2 of this paper, is applied to two different empirical studies by Harvey and Thiele (2021). Firstly, they employ the methodology to study the effect of the German reunification in 1990 on economic growth in West-Germany. Further, the effect of the smoking law in California in 1989 on cigarette consumption is analyzed. A replication of both applications is presented in the following. For more details on the methods and results, please refer to Harvey and Thiele (2021).

B.1 Replication: German Reunification

For the replication of the results for the German reunification study, data on annual real per capita GDP from 1971 to 2003 for 17 different countries is employed and a logarithmic transformation is applied. Using this data, I investigate which control countries were on the same growth path as West-Germany in the pre-treatment period by means of an KPSS cointegration test on the contrasts between West-Germany and the control countries. The results are presented in the third column of Table B2. The table shows that the results of Harvey and Thiele (2021) are replicable for all countries except Austria and the United Kingdom. For those two countries the cointegration test results are switched, indicating that the misreporting of the results led to incorrect conclusions in the paper. More specifically, my results show that the KPSS balanced growth cointegration test is rejected for Austria, making it an unsuitable donor for the proposed donor selection method by Harvey and Thiele (2021). Instead, Belgium should have been selected as a donor since the results in Table B2 show that the country is on the same growth path as Germany and its contrast with West-Germany has the lowest variance after France.

Table B2. *KPSS(2) tests for log annual real per capita GDP from 1971 to 1990*

	I(0)	I(1)	Coint (level)	Variance
West Germany	0.1917**	0.4318*	-	-
USA	0.1966**	0.5360**	0.1449	0.0004
UK	0.2048**	0.5962**	0.2034	0.0009
Austria	0.1960**	0.5155**	0.4793**	0.0005
Belgium	0.1932**	0.4598*	0.0736	0.0003
Denmark	0.1906**	0.5507**	0.5931**	0.0018
France	0.1915**	0.4609*	0.0780	0.0002
Italy	0.1932**	0.4462*	0.6587**	0.0007
Netherlands	0.1940**	0.4173*	0.6885**	0.0017
Norway	0.1949**	0.5083*	0.6636**	0.0058
Switzerland	0.1351*	0.1463	0.6099**	0.0053
Japan	0.1770**	0.3187	0.7234**	0.0028
Greece	0.1998**	0.5775**	0.6417**	0.0047
Portugal	0.1255*	0.1370	0.2233	0.0024
Spain	0.1774**	0.3970*	0.2881	0.0019
Australia	0.1924**	0.4676**	0.6393**	0.0021
New Zealand	0.1495**	0.3634*	0.6082**	0.0091

Notes. Abbreviations: ADH, Abadie, Diamond, and Hainmueller; GDP, gross domestic product; KPSS, Kwiatkowski, Phillips, Schmidt, and Shin. [**]5% level of significance. [*]10% level of significance.

I then proceeded to study the treatment effect by fitting a multivariate time series model and estimating the effect through the synthetic control approach applied to a restricted set of (cointegrated) donors.

For the synthetic control approach the weights for the donor countries are obtained by restricted least squares estimation (RLS). These weights are then used to construct the synthetic control. The estimates for the temporary and permanent effects are obtained by regressing the contrast between the SC and the target on the treatment dummies, the step dummy and the pulse dummies. When the panel is balanced, meaning that the number of observations is the same in each year, the standard error of each pulse dummy estimate will be the same.

The time series approach is implemented by first estimating the common trend that the control units and the target are following. Then the contrast between Germany and the US is regressed on the common trend, the $N-1$ contrasts of the remaining controls with the US and the treatment dummies. The resulting single equation estimates are equivalent to the estimates obtained by full maximum likelihood (ML) on the multivariate model (carried out in Harvey and Thiele (2021)). Note that the standard errors I obtain using the regression approach are higher compared to the ones presented by Harvey and Thiele (2021) using ML since the separate estimation of the common trend is an additional source of uncertainty and estimation error.

The synthetic control and time series approach are carried out on the donors pool employed by Harvey and Thiele (2021), including Austria, and a set of donors of which all have a common trend with the target (US, Belgium, France). This allows me to investigate how the erroneously inclusion of Austria in the donor pool, even though not being on the same growth path as the target in the pre-treatment period, affected the results in Harvey and Thiele (2021). A replication of the results of Harvey and Thiele (2021), using the US, Austria and France as donors, is presented in Table B3 and compared to the results drawing from a pool of donors all satisfying the balanced growth assumption (Table B4). For the replication of the analysis of Harvey and Thiele (2021), RLS weights matching the analysis in the paper were found for the US (0.3730), Austria (0.1532) and France (0.4738). For the corrected set of cointegrated donors, the RLS weights were 0.3587 for the US, 0.1172 for Belgium and 0.5241 for France. The estimation output suggests that Harvey and Thiele (2021) marginally underestimate the intervention effect due to the wrongly inclusion of Austria in the donor pool. Thus, the reunification in Germany had a slightly smaller adverse effect on economic growth after 1990 than shown in their analysis.

Table B3. *Estimated intervention effects for Germany; level break in 1999; Donors: US, Austria, France*

	Univariate		SC	
Year	Estimate	SE	Estimate	SE
1991	0.0269**	0.0103	0.0276***	0.0093
1992	0.0126	0.0103	0.0137	0.0093
1993	-0.0210*	0.0104	-0.0183*	0.0093
1994	-0.0421****	0.0107	-0.0387****	0.0093
1995	-0.0495****	0.0105	-0.0466****	0.0093
1996	-0.0595****	0.0111	-0.0556****	0.0093
1997	-0.0846****	0.0101	-0.0820****	0.0093
1998	-0.0934****	0.0098	-0.0915****	0.0093
1999	-0.1169****	0.0055	-0.1147****	0.0045

Notes. Abbreviations: SC, synthetic control; SE, standard error. [****]0.1% level of significance. [***]1% level of significance. [**]5% level of significance. [*]10% level of significance.

Table B4. *Estimated intervention effects for Germany; level break in 1999; Donors: US, Belgium, France*

	Univariate		SC	
Year	Estimate	SE	Estimate	SE
1991	0.0350***	0.0108	0.0306***	0.0103
1992	0.0207*	0.0106	0.0173	0.0103
1993	-0.0119	0.0107	-0.0138	0.0103
1994	-0.0312**	0.0114	-0.0347***	0.0103
1995	-0.0392***	0.0114	-0.0434****	0.0103
1996	-0.0482****	0.0108	-0.0490****	0.0103
1997	-0.0801****	0.0106	-0.0779****	0.0103
1998	-0.0918****	0.0108	-0.0874****	0.0103
1999	-0.1120****	0.0053	-0.1110****	0.0103

Notes. Abbreviations: SC, synthetic control; SE, standard error. [****]0.1% level of significance. [***]1% level of significance. [**]5% level of significance. [*]10% level of significance.

B.2 Replication: Californian Smoking Law

A similar analysis for a Californian smoking law is carried out using cigarette consumption data from 39 states over the period from 1970 to 2000. In accordance with Harvey and Thiele (2021) I found that Idaho, North Carolina, Colorado, Montana and Wyoming were on the same growth path as the target, California, over the period from 1970 to 1988 (Table B5). I then estimated the intervention effects using the same methods as in the German reunification application (Table B6). The key difference between the two applications is the (common) stochastic trend present in the cigarette consumption data of the target and the cointegrated control states. Therefore, the estimates from the univariate (single equation), bivariate and multivariate model are no longer the same. Estimating the stochastic trend μ_t using a system regression approach, in order to fit the bivariate and multivariate model, posed a difficulty as this required knowledge on parameters and initial conditions. Harvey and Thiele (2021) overcame this problem by fitting the time series models using the STAMP software, which was however unavailable for the analysis in this paper. Therefore, I was unable to replicate the estimation results of the bivariate and multivariate model. Table B6 shows that the estimation results of the single-equation time series (univariate) model and the synthetic control approach are in accordance with Harvey and Thiele (2021).

Table B5. *Ordered stationarity test statistics for contrasts with California from 1970 to 1988 (T = 19)*

	KPSS(2)	Rank	Variance	ADH weights
Idaho	0.2185	1	24.95	-
North Carolina	0.2491	2	364.53	-
Colorado	0.2857	3	20.30	0.164
Montana	0.3092	4	19.37	0.199
Wyoming	0.3343	5	129.92	-
Nevada	0.4220*	6	183.68	0.234
Kentucky	0.5060**	7	375.16	-
North Dakota	0.5222**	8	123.40	-
Delware	0.5307**	9	55.12	-
Indiana	0.5474**	10	67.42	-
Conneticut	0.5928**	11	95.28	0.069
Vermont	0.6065**	12	191.00	-
Oklahoma	0.6138**	13	154.02	-
New Hampshire	0.6239**	14	589.73	-
Utah	0.6247**	15	56.65	0.334

Notes. Abbreviations: ADH, Abadie, Diamond, and Hainmueller; GDP, gross domestic product; KPSS, Kwiatkowski, Phillips, Schmidt, and Shin. [**]5% level of significance. [*]10% level of significance.

Table B6. *Estimated intervention effects for California; level change in 1995 and m = 6 pulse dummies*

	Univariate		SC	
Year	Estimate	SE	Estimate	SE
1989	-1.162	4.277	-0.813	3.676
1990	-7.273	4.710	-7.751**	3.676
1991	-15.668****	4.078	-15.986****	3.676
1992	-17.539****	4.403	-17.580****	3.676
1993	-21.876****	4.390	-22.060****	3.676
1994	-29.154****	4.888	-29.586****	3.676
1995	-28.409****	4.139	-27.822****	1.678

Notes. Abbreviations: ADH, Abadie, Diamond, and Hainmueller; SC, synthetic control; SE, standard error. [****]0.1% level of significance. [***]1% level of significance. [**]5% level of significance. [*]10% level of significance.

C Appendix: Overview Data

Table C7. *Overview of implemented carbon pricing policies*

Country	implementation year	implementation price	scheme (first implemented)	carbon price 2016
Finland	1990	0.984	tax	46.372
Poland	1991	0.023	tax	3.640
Norway	1991	44.872	tax	37.238
Sweden	1991	26.163	tax	75.145
Denmark	1992	7.860	tax	13.800
Slovenia	1997	5.305	tax	12.546
Austria	2005	11.839	ets	2.382
Belgium	2005	13.203	ets	2.737
Cyprus	2005	17.473	ets	3.680
Czech Republic	2005	19.305	ets	4.178
Estonia	2005	35.992	ets	5.324
France	2005	8.411	ets	18.184
Germany	2005	15.456	ets	3.343
Greece	2005	16.041	ets	3.585
Hungary	2005	18.243	ets	3.131
Ireland	2005	12.419	ets	13.220
Italy	2005	14.033	ets	2.956
Latvia	2005	18.144	ets	2.614
Lithuania	2005	21.523	ets	3.583
Luxembourg	2005	7.298	ets	1.301
Malta	2005	26.213	ets	4.979
Netherlands	2005	15.163	ets	3.269
Portugal	2005	14.449	ets	6.768
Slovak Republic	2005	16.712	ets	3.685
Spain	2005	14.229	ets	3.157
United Kingdom	2005	15.550	ets	10.749
Bulgaria	2007	0.898	ets	4.804
Romania	2007	1.030	ets	4.508
Iceland	2008	12.496	ets	8.419
Switzerland	2008	2.853	ets	20.717
New Zealand	2009	0.694	ets	8.181
Ukraine	2011	0.024	tax	-
Australia	2012	10.384	tax	-
Japan	2012	0.647	tax	1.888
Croatia	2013	2.334	ets	2.730
Mexico	2014	1.617	tax	1.590
Kazakhstan	2015	0.006	ets	-
South Korea	2015	6.731	ets	10.394

Notes. Australia, Kazakhstan and Ukraine phased out carbon pricing scheme. Abbreviations: tax, carbon tax; ets, emission trading scheme. Carbon prices are adjusted for emission coverage.

Table C8. *Descriptive statistics for the full sample period (1971-2016) and restricted sample period (1980-2004)*

Sample	Variable	mean	std. dev.	min	max	missing observations
1971-2016 (5842 observations)	logGDP (per capita)	8.531	1.412	5.053	11.630	815
	ecp	13.863	17.009	0.003	93.721	0
	investment rate	24.189	8.299	-13.405	89.381	1067
	trade	76.014	49.110	0.021	437.327	997
	inflation	47.673	516.120	-72.729	23773.100	363
1980-2004 (3175 observations)	logGDP (per capita)	8.438	1.425	5.120	11.622	407
	ecp	20.369	21.745	0.003	78.982	0
	investment rate	23.563	8.297	-12.880	89.381	544
	trade	73.000	47.026	0.021	410.937	539
	inflation	79.332	697.759	-71.330	23773.100	193

Notes. Abbreviations: std. dev., standard deviation. Descriptive statistics for the variable ecp only consider the non-zero observations of the variable (treatment observations).

Table C9. *Descriptive statistics for excluded covariates*

Sample	Variable	mean	std. dev.	min	max	missing observations
1971-2016 (5842 observations)	industry	30.776	12.360	6.064	90.513	1365
	education	51.156	25.044	0.317	96.308	5020
1980-2004 (3175 observations)	industry	30.964	11.437	6.094	84.824	750
	education	38.060	24.206	0.645	89.210	3018

Notes. Abbreviations: std. dev., standard deviation.

Table C10. *Overview of variable names and codes used to retrieve the data*

variable	variable code World Bank	variable name used in analysis	source
(real) GDP per capita (constant 2015 US$)	NY.GDP.PCAP.KD	GDP (per capita)	World Bank (2022b)
Gross capital formation (% of GDP)	NE.GDI.TOTL.ZS	investment rate	World Bank (2022b)
Industry (including construction), value added (% of GDP)	NV.IND.TOTL.ZS	industry	World Bank (2022b)
Trade openness measured as exports plus imports (% of GDP)	NE.TRD.GNFS.ZS	trade	World Bank (2022b)
Educational attainment, at least completed upper secondary, population 25+, total (%) (cumulative)	SE.SEC.CUAT.UP.ZS	education	World Bank (2022b)
Headline consumer price inflation, annual	-	inflation	Ha et al. (2021)

Notes. The wbstats package (version 1.0.4) is used to retrieve data from the World Bank in R.

D Appendix: Analysis Finland

Table D11. *KPSS(2) tests for log annual real per capita GDP from 1989 to 1990*

	I(0)	I(0) p-val	I(1)	I(1) p-val	Coint(level)	Coint(level) p-val	variance
Morocco	0.1290	0.0815	0.2273	0.1000	0.1137	0.1000	0.0005
Portugal	0.1375	0.0658	0.3254	0.1000	0.1375	0.1000	0.0018
Ireland	0.1447	0.0523	0.3947	0.0795	0.1429	0.1000	0.0010
Pakistan	0.1375	0.0657	0.3219	0.1000	0.1496	0.1000	0.0002
Indonesia	0.1426	0.0564	0.3044	0.1000	0.1514	0.1000	0.0003
Cuba	0.1486	0.0478	0.4435	0.0584	0.1567	0.1000	0.0084
United States	0.1011	0.1000	0.1491	0.1000	0.1723	0.1000	0.0004
Malaysia	0.0940	0.1000	0.1410	0.1000	0.1750	0.1000	0.0015
Egypt	0.1392	0.0626	0.3245	0.1000	0.1873	0.1000	0.0014
Oman	0.1354	0.0697	0.3332	0.1000	0.1891	0.1000	0.0089
Spain	0.1450	0.0518	0.4061	0.0745	0.1949	0.1000	0.0005
Malta	0.1396	0.0618	0.3380	0.1000	0.1995	0.1000	0.0018
Turkey	0.1025	0.1000	0.1759	0.1000	0.2015	0.1000	0.0007
Sri Lanka	0.1260	0.0870	0.1789	0.1000	0.2110	0.1000	0.0010
Bulgaria	0.1025	0.1000	0.2059	0.1000	0.2188	0.1000	0.0016
Chile	0.1386	0.0637	0.2897	0.1000	0.2570	0.1000	0.0073
Republic of the Congo	0.1320	0.0759	0.3136	0.1000	0.2648	0.1000	0.0205
India	0.1292	0.0810	0.2305	0.1000	0.2743	0.1000	0.0002
Cameroon	0.1464	0.0496	0.4336	0.0627	0.2902	0.1000	0.0219
Iraq	0.1523	0.0447	0.3898	0.0816	0.3110	0.1000	0.0290
United Kingdom	0.1232	0.0922	0.1946	0.1000	0.3124	0.1000	0.0002
Mozambique	0.1331	0.0740	0.2540	0.1000	0.3223	0.1000	0.0338
Finland	0.1355	0.0695	0.2099	0.1000	-	-	-

Notes. Table only includes countries in sample which are cointegrated with the treated country Finland (Coint(level) not significant at 10% level). KPSS tests performed with optimal lag order of 2.

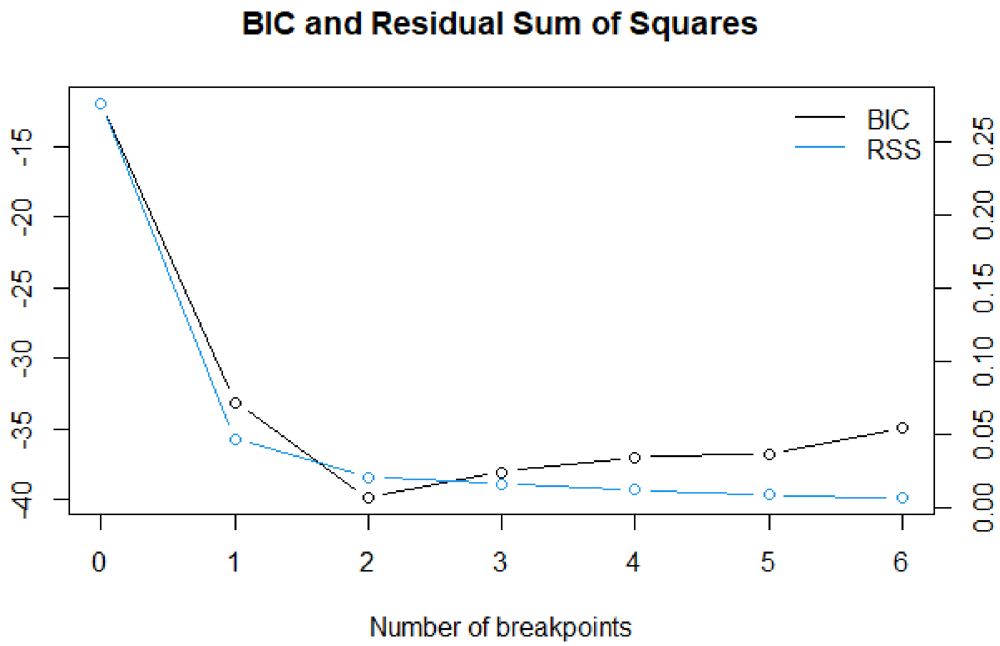

Figure D1. Breakpoint test: Selection of optimal number of breakpoints in the target

Notes. Abbreviations: RSS, Residual Sum of Squares; BIC, Bayesian Information Criterion.

Table D12. *Estimated intervention effects for Finland; level break in 1999*

	Univariate model		SC (RLS)	
Year	Estimate	SE	Estimate	SE
1990	-0.0086	0.0123	-0.0101	0.0111
1991	-0.0620****	0.0125	-0.0639****	0.0111
1992	-0.1168****	0.0134	-0.1237****	0.0111
1993	-0.1505****	0.0138	-0.1574****	0.0111
1994	-0.1529****	0.0147	-0.1605****	0.0111
1995	-0.1469****	0.0173	-0.1577****	0.0111
1996	-0.1473****	0.0199	-0.1621****	0.0111
1997	-0.1233****	0.0184	-0.1344****	0.0111
1998	-0.1062****	0.0193	-0.1192****	0.0111
1999	**-0.0880*****	**0.0231**	**-0.1074******	**0.0054**

Notes. Abbreviations: SC, synthetic control; SE, standard error; RLS, restricted least squares; Donors for Multivariate and SC (RLS) model: United States, United Kingdom, India; [****]0.1% level of significance. [***]1% level of significance. [**]5% level of significance. [*]10% level of significance.

E Appendix: Analysis Synthetic Control with Staggered Adoption

Table E13. *KPSS stationary test for control countries to select cointegrated donors for GSC approach*

Country	total times stationarity test not rejected (cointegrated)
Argentina	19
Bahrain	0
Brunei Darussalam	21
Canada	0
Gabon	20
Israel	0
Kuwait	0
Oman	17
Qatar	0
Saudi Arabia	18
Singapore	20
Trinidad and Tobago	17
United Arab Emirates	21
United States	20
Uruguay	18

Notes. Only control countries whose average GDP per capita is above the lowest average GDP per capita among all treated countries are included in the analysis. KPSS tests performed with optimal lag order of 2.

Table E14. *Country specific introduction effects; Dependent Variable:* $\Delta log(GDPpc)i, t$

Year	introduction effect
Austria	-0.0046
Belgium	-0.0042
Bulgaria	0.0301
Cyprus	-0.0333
Czech Republic	0.0232
Denmark	-0.0123
Estonia	-0.0277
Finland	-0.0065
France	-0.0039
Germany	0.0047
Greece	-0.0233
Hungary	-0.0082
Iceland	-0.0060
Ireland	-0.0080
Italy	-0.0159
Japan	0.0101
Latvia	-0.0177
Lithuania	-0.0155
Luxembourg	-0.0140
Malta	-0.0101
Netherlands	-0.0002
New Zealand	0.0120
Portugal	-0.0111
Romania	0.0304
Slovak Republic	0.0087
Spain	-0.0060
Switzerland	0.0077
United Kingdom	-0.0036

Notes. Variable intercept model used due to rejection that fixed effects are poolable. Countries with less than 5 treatment periods are not included in the estimation.

F Appendix: Robustness Analysis

Table F15. *Estimation results average treatment effect on the treated (average ATT) for different model specifications*

Model	(average) ATT Estimate	p-value
(1) Baseline	-0.0055 (0.0047)	0.2383
(2) Including SW and NW	-0.0046 (0.0046)	0.3215
(3) Excluding predictors	-0.0099 (0.0056)	0.0784
(4) 1980 to 2016	-0.0132 (0.0039)	0.0006
(5) Cointegrated donors	-0.0096 (0.0066)	0.1336

Notes. The bootstrap standard error is reported in parenthesis.

Table F16. *Estimation results country fixed effects panel data for different model specifications; Dependent Variable:* $\Delta log(GDPpc)i,t$

Model	Estimate	p-value	F-test for poolability of carbon price coefficient	F-test for poolability of country fixed effects
(1) Baseline	-8.6881 x 10^{-5} (2.1384 x 10^{-4})	0.6848	p = 0.9968	p = 0.0000
(2) Including SW and NW	2.0321 x 10^{-5} (1.3798 x 10^{-4})	0.8830	p = 0.0.9842	p = 0.0000
(3) Excluding predictors	1.0319 x 10^{-4} (2.3719 x 10^{-4})	0.6693	p = 0.9981	p = 0.0000
(4) 1980 to 2016	-1.1788 x 10^{-4} (2.1371 x 10^{-4})	0.5816	p = 0.9899	p = 0.0001
(5) Cointegrated donors	-9.6944 x 10^{-5} (2.0774 x 10^{-4})	0.6411	p = 0.7795	p = 0.0000

Notes. The SE is reported in parenthesis. 343 treated observations are included in the study of model specification (1), (3), (4) and (5). 395 treated observations are included in the analysis including Sweden and Norway.

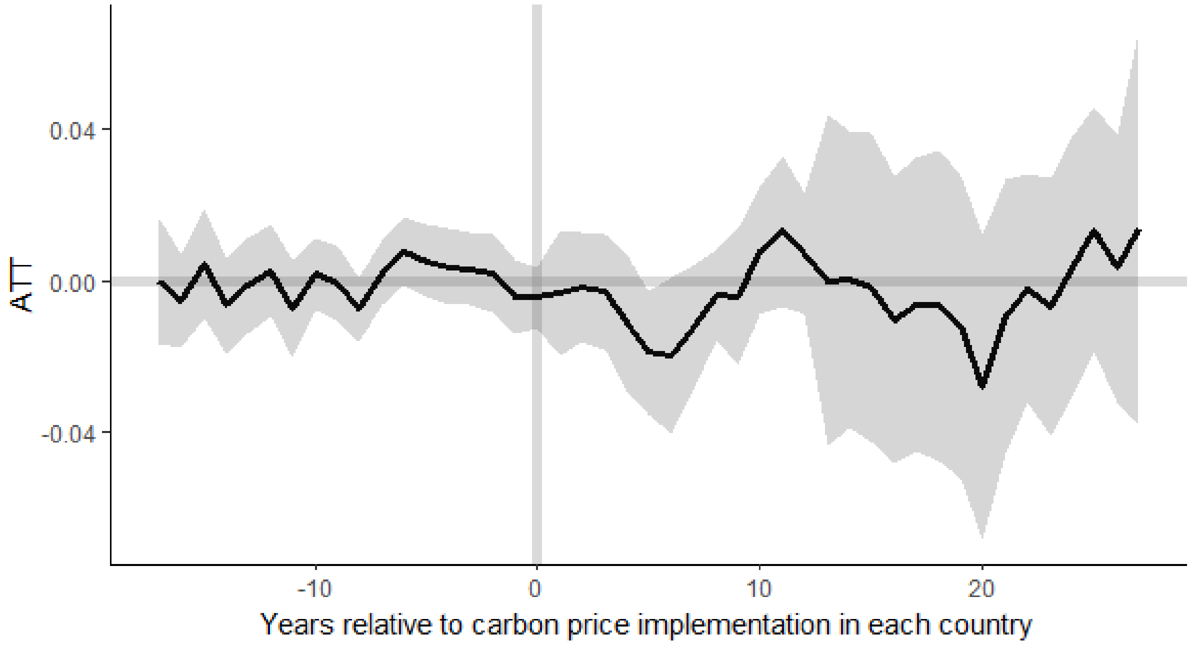

Figure F1. Average treatment effect; including Sweden and Norway

Notes. The figure shows the estimated treatment effect, difference between observed and counterfactual, and the 95% confidence interval (shaded), based on the bootstrap standard errors.

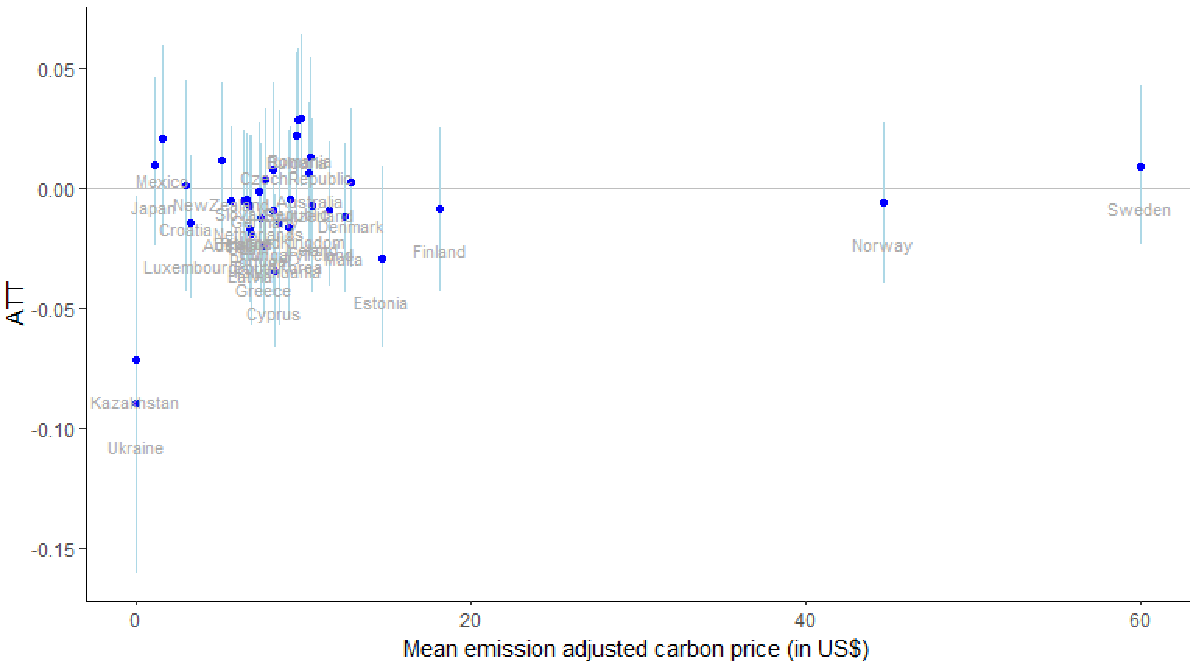

Figure F2. Average treatment effects of each country with 95% confidence interval; including Sweden and Norway

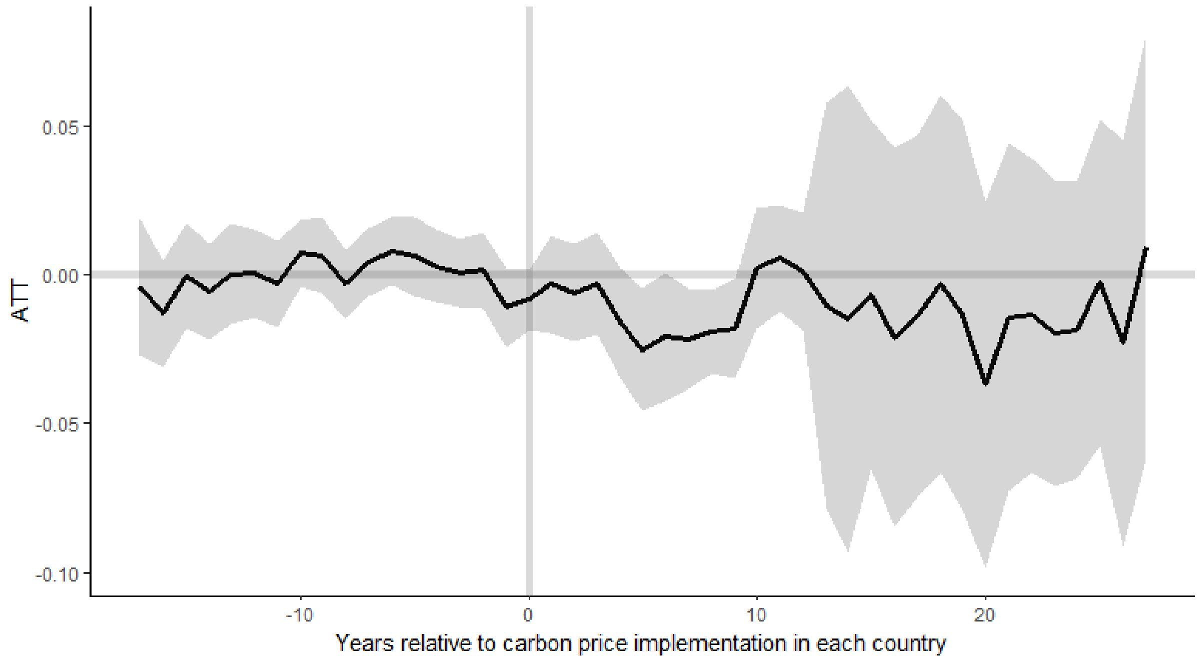

Figure F3. Average treatment effect; Analysis excluding GDP predictors

Notes. The figure shows the estimated treatment effect, difference between observed and counterfactual, and the 95% confidence interval (shaded), based on the bootstrap standard errors.

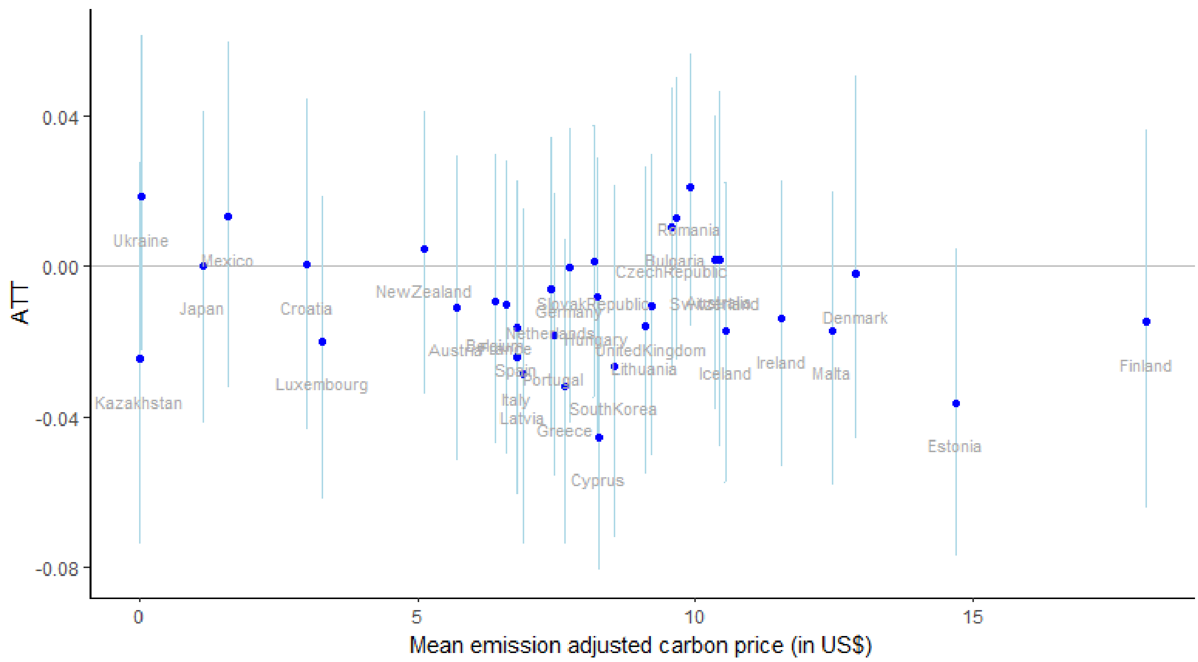

Figure F4. Average treatment effects of each country with 95% confidence interval; Analysis excluding GDP predictors

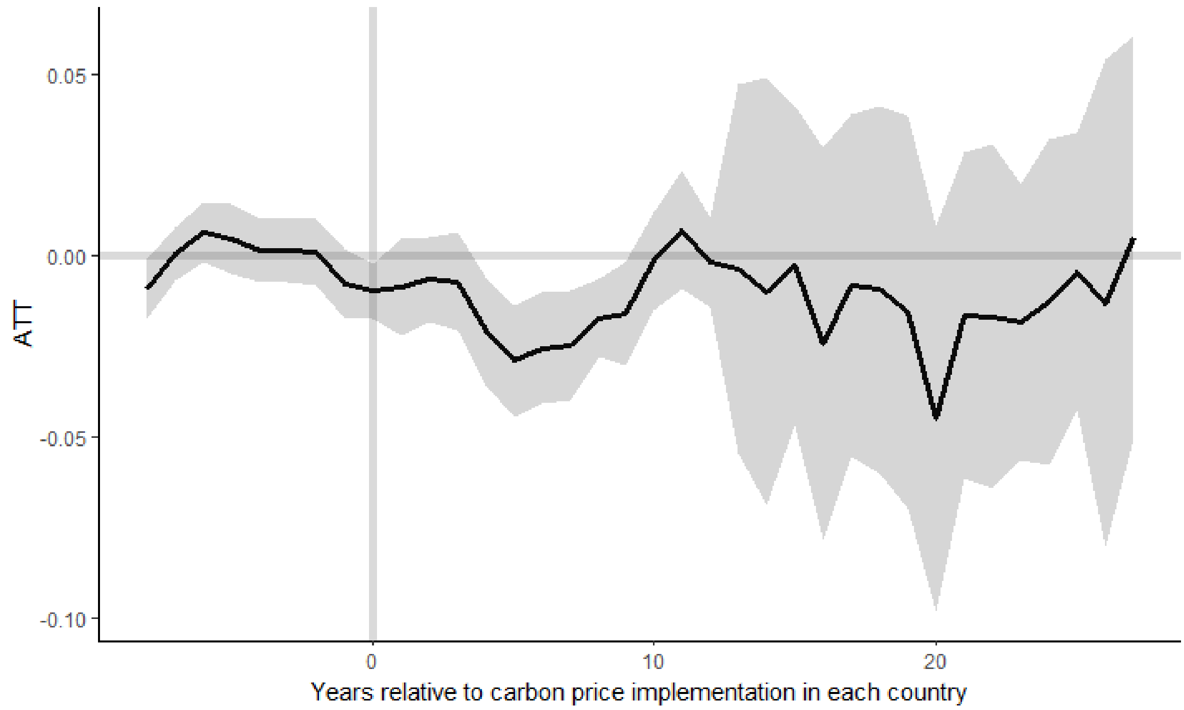

Figure F5. Average treatment effect; Analysis 1980 to 2016

Notes. The figure shows the estimated treatment effect, difference between observed and counterfactual, and the 95% confidence interval (shaded), based on the bootstrap standard errors.

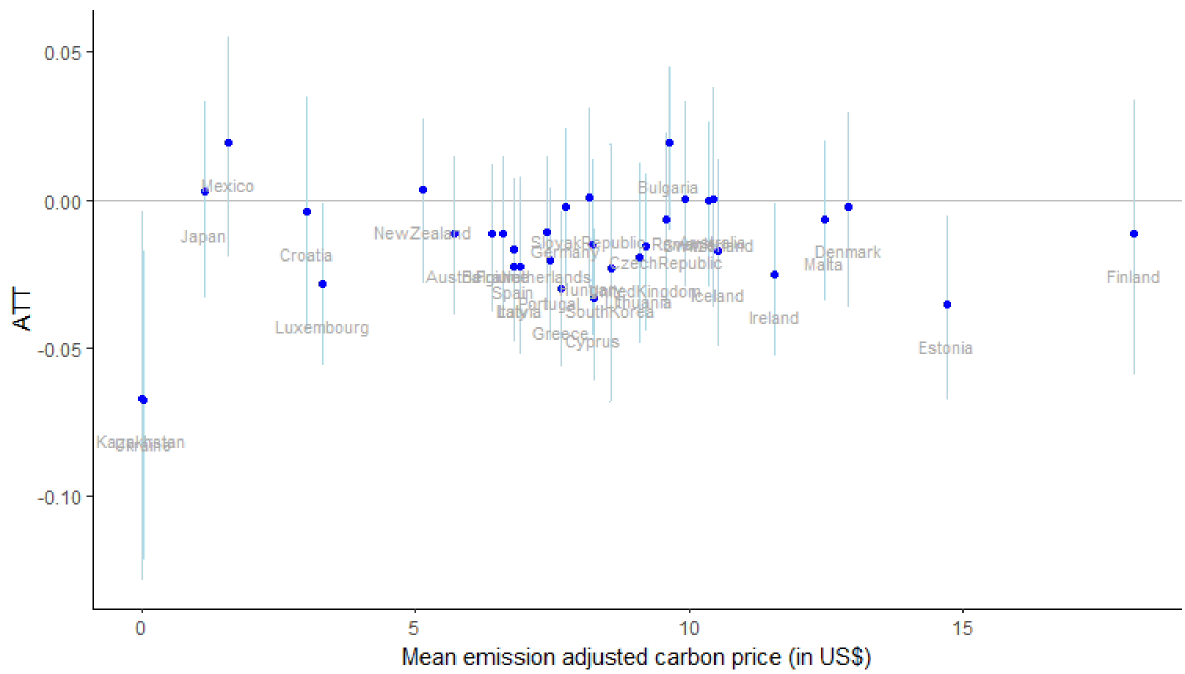

Figure F6. Average treatment effects of each country with 95% confidence interval; Analysis 1980 to 2016

G Appendix: Statistical Software Use

The analysis presented in this paper is performed using the statistical software R (version 4.0.5). Different packages are employed for the study. To retrieve the data from the World Bank the wbstats package (version 1.0.4) in R is used. For the synthetic control method described in Section 5.1.1 the Synth package (version 1.1-6) in R is employed. The KPSS statistics are computed using the tseries package (version 0.10-50) in R and the breakpoint test in the study of Finland is performed employing the package strucchange (version 1.5-2). The generalized synthetic control models are estimated using the R packages gsynth (version 1.2.1).

For a complete overview of the packages used for the study in this paper, please see the file *load_packages.R*. The file *Replication_main.R* has been used for the replication analysis presented in Appendix B. The country specific study of Finland has been performed using the R file *Finland_analysis.R*. To study the panel of treated countries using the generalized synthetic control approach, the file *Extension_panel.R* has been programmed. For the replication of the robustness analysis, the parameters specified in lines 24 to 33 need to be adjusted. More specifically, to obtain the output for model specification (3), excluding the predictors, the baseline specification of the formula *gsynth_form* (line 33) needs to be changed by deleting the covariates (*dinvestment_rate, dtrade, dInflation*) from the formula. To retrieve the results for the analysis of the reduced sample period from 1980 to 2016, model (4), the baseline specification needs to be changed by setting the parameter *period_start* (line 28) to 1980.