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Can carbon taxes stimulate clean innovation? Evidence from the Swedish experience

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Abstract: This thesis investigates the impact of national carbon taxes on clean innovation. We employ the novel synthetic control method to estimate the effect of the Swedish carbon tax, introduced in 1991, on climate change mitigating technology patents. We estimate that the Swedish carbon tax increased clean technology patents by 14.1 percent in an average year in the post-treatment period, 1991-2005, compared to synthetic Sweden. Aggregating over the population, this corresponds to an increase of 249.88 clean patents in Sweden, compared to counterfactual case, had Sweden not implemented a carbon tax. These results are economically significant and prove robust to a number of placebo tests. Furthermore, we also conduct panel data regressions using cross-country data from 17 countries that implemented a carbon tax during 1990-2016. With this method, however, we find no detectable effect on innovation. We discuss potential explanations to these different outcomes and conclude that the discrepancies in our results might have to do with limited data on carbon taxes and differences in carbon tax levels of most countries compared with the uniquely high carbon tax level of Sweden. In terms of policy implications, our findings suggest that carbon taxes consistent with the targets of the 2015 Paris agreement stimulate clean innovation.

Keywords: Climate policy, National Carbon Tax, Clean Innovation, Synthetic Control Method

JEL: O30, H23, Q01, Q55, Q58

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Contents

1 Introduction

Climate change, fuelled by greenhouse gas (GHG) emissions from human activities and in particular carbon dioxide (CO_2), is one of the most pressing challenges facing the world today. There is widespread consensus, not least since the adoption of the Paris agreement in 2015, that ambitious climate policies are critical to limiting emissions and accelerating the transition to low-carbon societies. Economic research increasingly recognizes technological innovation as one of the most important determinants of abatement costs of fossil fuels in the long run, and a key factor in the efforts to limit climate change[1]. Consequently, it is critical to understand how climate policy impacts not only emissions but also the direction and speed of technological change (Popp et al. 2010; Acemoglu et al., 2016; Calel & Dechezlepretre, 2016). To this end, this thesis sets out to address the question of whether or not climate policy can stimulate "clean" technological innovation[2] by empirically estimating the effect of national carbon taxes on innovation in climate change mitigation technologies, measured by patent counts.

In this study we focus on the Swedish experience, evaluating the Swedish carbon tax introduced in 1991, using the rather novel synthetic control method developed in a series of papers by Abadie et al. (2003, 2010, 2015). Furthermore, we complement our case study analysis by also estimating the impact of carbon taxes on clean innovation using a classic regression model with fixed effects and cross-country panel data from 17 countries that implemented a national carbon tax over the years 1990-2016.

First off, we should provide some brief motivation to why we focus on national carbon taxes, as opposed to other potentially relevant climate policies. Firstly, a carbon tax is a carbon pricing[3] type of climate policy tool, which is increasingly adopted in jurisdictions across the world[4]. Secondly, apart from being the most favored type of climate policy of leading climate economists such as Nordhaus

[1] It is straightforward by classic economic theory that carbon pricing should reduce consumption of carbon fuelled activities and production and thereby effectively lower emissions of GHG. However, we should also expect that the increased relative cost of emissions would spur fuel-shifting and technological innovation to economize production. Concretely, apart from directly lowering carbon consumption, carbon pricing could potentially also spur technological direction change in favor of climate change mitigating technologies, as producers seek technologies that require less (or no) CO_2 emissions per unit of output.

[2] We will use the terms "climate change mitigating", "emissions-reducing" and "clean" innovation/technologies interchangeably throughout this thesis as we refer to technologies developed to lower GHG emissions and mitigate the carbon footprint of the product in question.

[3] This essentially means "putting a price on carbon", forcing emitters to internalize the externality cost of polluting.

[4] In 2019 alone 29 jurisdictions adopted or pledged to adopt a carbon tax (World Bank Group, 2019). This increased popularity of carbon taxes in itself calls for an assessment of its effectiveness.

(2008, 2013), there has recently been substantial steps forward in the theoretical literature, with the development of new models showing how carbon taxes can stimulate directed technological change and spur clean innovation (see Acemoglu et al., 2012). Furthermore, empirical studies comparing the efficiency of different climate policies have also suggested that tax measures is the most widely influential policy instrument for stimulating innovation (Johnstone et al., 2010). Altogether, there is ample motivation for looking deeper into the impact of carbon taxes on innovative activity.

The Swedish carbon tax ought to be a particularly appropriate subject of study using the synthetic control method, for a number of reasons. Firstly, since Sweden was an early adopter of a carbon tax (in 1991) it provides a long time-series of post-treatment observations. Moreover, the Swedish carbon tax has, since introduction, been relatively high (currently the highest in the world). In fact, the Swedish carbon tax is one of very few carbon pricing initiatives in the world that is consistent with the target carbon pricing level of the 2015 Paris Agreement[5] to keep emission "well below 2C", which implies carbon pricing at 40-80 USD/ton CO_2 by 2020 and 50-100 USD/ton CO_2 by 2030 (Stern and Stieglitz, 2017; World Bank Group, 2019). Lastly, the fact that carbon taxes were for many years implemented only by a few early adopters gives us a relatively large donor pool for our synthetic Sweden. Altogether, these contextual conditions and subject properties should make the Swedish carbon tax a subject highly relevant and appropriate for case study evaluation.

Notably, in a recent paper by Andersson (2019), the synthetic control method is in fact used to evaluate the impact of the Swedish carbon tax on carbon dioxide emissions. The author found, in contrast to earlier studies, that the carbon tax had been successful in significantly reducing Sweden's CO_2 emissions.[6] To our knowledge, however, there has been no previous study evaluating the impact of the Swedish carbon tax on technological innovation.

Furthermore, we should also make a few comments on the level of the data. While we recognize that there are some limitations with country level data compared to firm level data, as it provides less granular basis for analysis, it captures impact on a macro-level which is important for policy-makers when assessing carbon taxation on a grand

[5]Actually, as of 2019 still only 5% of the world's GHG emissions are priced at a level consistent with achieving the target of the 2015 Paris Agreement (World Bank, 2019).

[6]Andersson found that the introduction of the carbon tax (coupled with the VAT) contributed to a decline of almost 11 percent in an average year in CO_2 emissions from transport, and 6 percent from the carbon tax alone.

scale.[7] Furthermore, while there is a growing theoretical literature investigating the impact of carbon taxes on a macro-level, providing insightful simulations and ex-ante estimations of effects, there is a gap in literature on ex-post, empirical studies on the macro-level[8]. We hope that our research, using country-level data and an empirical strategy, can contribute to filling this gap and provide economy-wide insight on the impact of carbon taxes on innovation.

Our key finding in this thesis is that the Swedish carbon tax had a positive impact on clean innovation. By employing the synthetic control method, we estimate an increase in climate change mitigating patents per capita million by 1.88 yearly compared to the counterfactual, synthetic Sweden. In relative terms, this corresponds to an increase of 14.1% in an average year between 1991-2005, and 18.5% on average in the first 10 years of the carbon tax being in effect. We calculate that the cumulative effect amounts to an increase of 28.25 clean patents per capita million in total over the post-treatment period, 1991-2005. Aggregating over the population, this suggests that Sweden produced 249.88 more patents in this period that it would have done in absence of the carbon tax. Considering that the total Swedish production of clean patents in an average year between 1991-2005 was 114.95, this is arguably an economically substantial result. We run several types of placebo tests that indicate a robustness of our results. For example, when reassigning the treatment at random to our donor pool countries, we find that the probability of obtaining a post-treatment gap as large as that for Sweden is only 0.0625.

However, the second part of our empirical investigation, in which we use cross-country panel data to estimate the impact of carbon taxes on innovation, does not produce any statistically or economically significant results. Yet, given the limitations of carbon tax data, both in terms of availability[9] and comparability, this is not completely unexpected. On the other hand, previous research has shown that due to path-dependency effects it is likely that only relatively high carbon taxes would have an effect on shifting incentives toward clean innovation at any substantial scale. As we discuss more in depth in section 6, this might also be a potential explanation to

[7] Firm-level data has a number of benefits, but might fail to capture spillover, dynamic and general equilibrium effects by carbon pricing policies on the market, hence only providing a partial picture.

[8] Likely due to limited data on real carbon taxes.

[9] Because of the small sample of countries that implemented a national carbon tax before 2016 we only have 226 observations. When using the data-richer fuel-tax as a proxy for a carbon tax (fuel taxes are far more common across the world and this variable provides 1 135 observations from 37 countries), however, we find some indications of a positive direction of the effect on innovation, which is in line with our case study results.

why we find relatively strong effect in the case study of Sweden but no detectable effect from our cross-country sample, which generally consists of carbon taxes at levels substantially lower than consistent with the target of the Paris 2015 agreement.[10]

In sum, while our cross-country estimates provide little ground for analysis, our results from the Swedish experience suggest that carbon taxes of a magnitude in line with the 2015 Paris agreement indeed contributes to stimulate clean innovation at scale. The policy-implication of our findings is thus that carbon taxes of a sufficiently high level contribute to not only decrease emissions but also to accelerate a transition to low-carbon, sustainable societies by fostering technological advancements. Nevertheless, there is ample opportunity and need for more empirical research in this field. For example, future studies could focus on investigating and quantifying the economic value of clean technologies developments linked to carbon taxes, the overall net-impact on the economy, as well as on different sectors.

The rest of this thesis is organized as follows. In section 2 we give an overview on the existing theoretical and empirical literature related to our research question. In section 3 we describe the key data employed and discuss some of its benefits and limitations. Section 4 continues with a detailed description of our empirical strategy, in particular the synthetic control method and how we adapt it to the Swedish context, and in section 5 we present our results. In section 6 we thoroughly discuss our findings, including the validity of our results, the limitations of this study as well as potential policy implications. In section 7 we present our main conclusions and takeaways from our research.

[10]The Swedish carbon tax is on average more than 5 times higher than the average carbon tax on the rest of our sample.

2 Literature Review

Traditionally, economists and climate scientists alike have focused largely on the direct impact of various policies on GHG emissions and on general equilibrium models with exogenous technology[11], more or less neglecting the fact that changes in the relative price of energy inputs should have a meaningful impact on the direction of technologies developed (Acemoglu et al., 2012). In recent decades, however, there has been a substantive increase in the literature linking environmental policy and directed technological change, particularly in the context of climate change (see Goulder and Schneider, 1999; Newell et al., 1999; van der Zwaan et al., 2002; Popp, 2004; Lanoie et al., 2007 and Acemoglu et al., 2012).

On the theoretical side, the induced innovation hypothesis[12] (Hicks, 1932) has been a central building block for this strand of literature, and fundamental for influential later works by e.g. Porter (1991), Popp (2004), Gerlagh (2008) and notably Acemoglu et al. (2012). While specific features and assumptions vary somewhat across models in this strand of literature, naturally, the fundamental theoretical prediction remains the same. Essentially, theory suggests that as regulated (taxed) firms face a higher price on emissions in relation to production costs, they will be incentivised to make operational or technological changes to make production more efficient, i.e. less intense in emissions. In a context of climate policy, this implies that carbon pricing should, theoretically, increase firms incentives to invest in emissions-reducing technological change.

In this thesis, however, we mainly draw on the environmentally constrained growth model with endogenous and directed technological change introduced by Acemoglu et al. (2012). This comprehensive model provides a theoretical framework in which the economy has two sectors, relying on "dirty" and "clean" (fossil and non-fossil) inputs respectively, which are highly substitutable. In the model the sector using dirty inputs has an initial productivity advantage that, in absence of intervention and together with a market size effect[13], directs innovation to that sector, continuing to contribute to pollution and environmental degradation. Policy measures, in the form of (temporary) carbon taxes and/or research subsidies can redirect technological innovation from "dirty" to "clean" production. While the authors argue that the optimal policy consists of both a carbon tax and research subsidies, even a sufficiently large carbon tax would by itself encourage a change of direction of innovation towards clean

[11] I.e. the exogenous development of new and more "environmentally friendly" energy sources.

[12] The theory states that a change in the relative factor prices of production would induce technological innovation directed to economise the use of the relatively expensive factor.

[13] The and advantage of having a bigger share of the market.

technologies.

In the field of empirics, a growing body of research indicates that changes in the relative price of energy inputs have a quantifiable impact on the direction of technology development (Newell et al., 1999; Popp, 2002). One strand of empirical literature has used aggregate data on sectoral or national levels to investigate this relationship. Popp (2002) used US patent data from 1970–94 to estimate how energy price changes impacted energy-efficient innovations in the USA, and found that both energy prices as well as the prior knowledge stock had a significant impact on the direction of innovation. Brunnermeier and Cohen (2003) investigated the effects of changes in pollution abatement costs and regulatory enforcement of environmental innovation (measured by successful environmental patents applications) in US manufacturing industries between 1983-1992. Results indicated that increases in pollution abatement expenditures had a positive impact on environmental innovation, while stronger monitoring and enforcement mechanisms had no additional effects.[14] A more recent study by Dechezleprêtre et al. (2011), using patent data from 1978 to analyze the geographic distribution and diffusion of climate-mitigation patents, shows that climate policy has contributed to accelerating the pace of clean innovation[15].

Many recent empirical studies have used a microeconomic approach, employing firm-level data, to estimate environmental or climate policy on innovation. Notably, Aghion et al. (2016) use firm-level patent data from the international auto industry, exploiting variation in firms' exposure to different markets and fuel prices, to explore whether firms innovate more in clean technologies when facing higher tax-inclusive fuel prices. Results show that firms tend to respond to higher tax-inclusive fuel prices by innovating more in clean technologies. The authors also find strong evidence for path dependency in type of innovation ("clean" vs. "dirty"), which has important implications. Given that the stock of dirty innovation[16] is greater than the stock of clean innovation, this implies that the path dependency effect will lock economies into high carbon emissions even in a scenario of a modest carbon tax or Research and Development (R&D) investments in clean technologies. This implies that it is highly motivated to implement relatively high carbon taxes at an early stage, in order to change incentives for climate change migitating innovation in an efficient manner and

[14] Furthermore, empirical evidence suggested that internationally competitive industries are more likely to engage in clean innovation.

[15] This study measure innovation on a global level, and while the authors estimate that pre-1990 energy prices were the driving force of innovation, increasing enactment of environmental and climate policies also made such policies relatively more important in driving innovation in more recent decades.

[16] Innovation in technologies that rely on fossil fuels.

counter the force of path dependency (in dirty technologies).

Calel and Dechezleprêtre (2016) use firm-level data from the EU to investigate the impact of the European Emissions Trading System (EU ETS) on directed technological change. Similarly to Aghion et al. (2016) the authors use patents as a proxy for innovation, specifically patents registered at the European Patent Office (EPO) that fall under the low-carbon patent classification. Employing a matched difference-in-differences method, distinguishing between EU ETS-regulated and non-regulated firms, the authors find that the emission trading scheme can explain approximately a 1% increase in low-carbon innovation in the EU compared to a counterfactual scenario.

As evident from the literature review presented here, much of the empirical work in this field have used changes in (tax-inclusive) fuel prices or energy prices as proxies of carbon taxes, rather than investigated the impact of real carbon taxes (e.g. Aghion et al., 2016). Typically, this is a strategy used because of the relatively limited data on carbon taxes, compared to fuel price data. Furthermore, various studies have investigated the effects of other types of direct of indirect climate policies, such as investment incentives, R&D subsidies, emissions trading systems etc.[17] Moreover, as previously noted, the empirical literature is relatively dominated by micro-level studies on firms and industries while most macro-level studies are theoretical and estimate effects ex-ante.

Hence, there is a surprisingly big gap in literature estimating effects of carbon taxes on a macro-level using real carbon tax data, particularly on the subject of impact on innovation. Filling this gap in literature should be highly relevant for both researchers and policy-makers given the urgency of cost-effectively accelerating our economies transition to low-carbon societies. To this end, we hope that this study can contribute to this literature and we believe that the particular properties of the Swedish tax should make it particularly relevant to this objective.

[17]Notably, a large number of previous studies have also used R&D expenditure, rather than patents count, as a proxy measure for technological innovation (see Popp et al. 2010 for overview).

3 Data

In this section we present our data before we turn to the empirical strategy. The section is divided into two parts. First we describe our key variables of interest in depth and provide some context and motivation on the selection criteria of these variables. Secondly, we provide a technical overview of the details of the data and summary statistics.

3.1 Description of main variables

3.1.1 Carbon taxes

A carbon tax is a type of carbon pricing[18], a policy instrument with the purpose of shifting the externality costs, i.e. the societal costs paid for by the public, of GHG emissions back to the emitters. The first carbon taxes were implemented by Finland and Poland[19] respectively in 1990, followed by Sweden and Norway in 1991. Since then there has been a steady increase in carbon pricing initiatives. In 2019 total implemented and scheduled carbon pricing initiatives reached 57, comprising 28 ETSs and 29 carbon taxes[20]. These initiatives together cover about 20% of the world's total GHG emissions (World Bank Group, 2019). Nevertheless, still less than five percent of global emissions are currently priced at a level consistent with the targets of the Paris Agreement and public support is often lacking (World Bank Group, 2019)[21]. In this context, empirically evaluating the effectiveness of carbon pricing initiatives is highly motivated not only to inform the public but also to ensure that future mitigation policies are efficient, economically and environmentally, and based on lessons learnt from existing national climate policies.

There are two key limitations of the data on carbon taxes that we need to recognize. Firstly, the data is limited in terms of comparability. Carbon tax policies differ across countries in terms of the number of sectors covered, allocation and compensation methods applied and specific exemptions. For example, some of the EU countries

[18]Carbon pricing initiatives include not only carbon taxes, but also fuel taxes, coal taxes or directed energy taxes (indirectly taxing carbon), as well as cap-and-trade programs, such as the European Emissions Trading Program (ETS).

[19]Contrary to other early adopters, however, the Polish carbon tax has been kept stably at a very low rate over time, at a price around 0.1-0.05 real USD/ ton CO_2 emissions.

[20]Most of these are implemented on a national level, but examples of regional carbon taxes exist, e.g. in various provinces of Canada.

[21]In fact 96 out of the 185 countries that have submitted their Nationally Determined Contributions to the 2015 Paris Agreement have proclaimed to plan or consider adopting carbon pricing initiatives.

in our sample exempt operators covered by the EU ETS from the national carbon tax, while other countries do so only partially or not at all. Nevertheless, in all fundamental aspects carbon tax systems are mostly very similar. Typically, and with few exceptions, the carbon taxes cover all fossil fuels, and apply either universally to all sectors or are targeting important ones such as transports, power, industry and construction. Sectorial or process related exemptions vary from country to country, but generally all countries exempt commercial aviation, certain energy-intensive production and power production (the World Bank, 2020, see country profiles).

Secondly, due to the fact that few countries adopted carbon taxes pre-2005, data is limited in terms of availability. Furthermore, several of the national carbon taxes that have been enacted since 1990 have been kept at relatively low levels (below 5 USD/tons CO_2 emissions) throughout the time period. Hence, data is limited and quite unbalanced, the Nordic countries account for almost half of the observations (117/226) and almost all the longer time series.

We should note, however, that the concerns about comparability of national carbon taxes across countries is a potential problem only for our panel data regressions, and not for our Swedish case study in which we evaluate the carbon tax via the synthetic control method.

3.1.2 The Swedish carbon tax

In 1991, Sweden, one of the first countries in the world to do so, introduced a carbon tax that is still considered one of the keystones of Sweden's climate policy. At the inception the rate was set to SEK 250 (USD 40) per ton of CO_2 and has since then been gradually increased, with a steep incline in the early 2000s. Today at a rate of SEK 1 190 (USD 130), it is the highest carbon tax in the world. All fossil fuels are covered by the tax, but the final tax rate is applied based on the proportion of carbon content in different types of fuels (Swedish Government, 2020).

While the full carbon tax is levied on heating fuels (for households) and transport fuels[22], industry and agriculture has historically been granted exemptions to various degrees due to concerns over carbon leakages and international competitiveness (Johansson, 2000; Hammar and Åkerfeldt 2011). Between 1991-2005 industry paid from 21-50 percent of the tax rate[23] (in the last few years, however, there has been a steep increase and currently industries pay 80% of the general rate). Furthermore,

[22]It should be noted that the introduction of the carbon tax coincided with the addition of a VAT of 25% on the retail price of gasoline and diesel.

[23]Furthermore, at the introduction of the carbon tax the existing energy tax was simultaneously lowered.

due to Sweden's particular composition of energy sources[24] Swedish industry relies on fossil fuels only to a limited extent[25] (Johansson, 2000; Andersson, 2019).

Given these energy conditions, industry exemptions and the fact that technological innovation typically takes place within firms and industries, it is not clear from the outlook if the carbon tax would have an effect on innovation (Johansson, 2000). On the other hand, the Swedish carbon tax has been relatively high and even after exemptions, it is a non-negligible cost that should impact firms and their behavior. Furthermore, as households face the full tax rate this should theoretically give rise to an increase in consumer demand for cleaner technologies (e.g. an increase in the carbon tax would impact the costs of driving for households, which would likely spur an increase in demand for cars and engines that require less or no fossil fuels per mile), by which the carbon tax indirectly would give incentives to firms to shift towards clean innovation.

In addition, the Swedish Government states that the carbon tax, which is an application of the "polluter pays" principle[26], will ensure not only a cost-effective reduction of emissions but also the stimulation of new, clean technologies (Swedish Government, 2020). To this end, with our following empirical investigation of the Swedish carbon tax we hope to give some insight to whether or not the carbon tax has in fact had an impact on clean innovation.

3.1.3 Patent data

In this paper we use patent data as our measure of innovation, which is a commonly used practice in the literature.[27] Patent data has a number of benefits compared to alternative measures, which we will cover briefly. Firstly, patent data are

[24]The energy sources are dominated by nuclear, hydro power and biofuels,

[25]Less than 30% at the introduction of the tax in 1991.

[26]The cost of emissions should be borne by the polluter.

[27]Notably, a large number of previous studies have also used R&D expenditure as a proxy measure for technological change when investigating the impact of environmental or climate policies (e.g. Goulder and Schneider (1999), Gerlagh (2008), or see Popp et al. 2010 for literature overview). However, there are several drawbacks with using R&D as a measure for innovation, notably that data is typically only available on aggregate level without possibility to break it down by technology group. Data is also incomplete in terms of private sector R&D spending. The most notable issue with R&D expenditure as a proxy, however, is that it measures input in a context where we are interested in technological development in an outcome, environmental efficiency. R&D expenditure is typically allocated not by outcome but by sectoral allocation. More concretely, it has been found that environmental technologies draw on a broad range of scientific knowledge, so even if research that is not a priori "environmental", for example in chemistry and material sciences, it might contribute to building knowledge valuable for environmental innovation. Hence, R&D as a measure of environmental innovation is likely to fail to include transformative innovation results and spill-over effects from sectors that are not a priori classified as "environmental" (Haščič & Migotto, 2015).

commensurable, as patentable inventions generally need to fulfill some concrete well-defined criteria and objective standard. Secondly, patents measure innovation in terms of intermediate outputs, contrary to for example data on R&D expenditures, which focus on inputs. Furthermore, patent data is quantitative by nature and as such highly appropriate for statistical analysis. Patent data is widely available, it can be easily disaggregated into distinct technological fields, which is highly advantageous for the purposes of this study as it allows us to narrow down our analysis to a specific type of innovation, such as climate change mitigation technologies (Haščič & Migotto, 2015).

Patents can also be distinguished by size of the international patent family (i.e the number of patent authorities to which patent applications, protecting the same invention, have been filed). The most liberal measure is to count *all* inventions (family size equal to or larger than one), which includes patents applied to a single patent office[28]. These statistics do not exclude any data, but includes the entire world-wide stock of patents.[29] Yet, it is oftentimes argued that it is more adequate to use patent statistics on "claimed priorities" (i.e. patents with a family size of two or greater) for the purpose of international comparisons, mainly because this typically restricts the sample to "high-value" (HV) patents[30] (Haščič & Migotto, 2015). In this paper, we use HV patent statistics to investigate the impact of carbon pricing for to two reasons. Firstly, HV patent data is available for a longer time period (from the 1960's in the OECD Patent indicators data set) and secondly, these patents are much more likely to be of economic value than the more liberal count, which is a desirable feature since measuring the impact of low-value patents would be rather meaningless from an economic efficiency point of view.

Nevertheless, we should note that patents are not an exhaustive measure of innovation. The most important drawbacks are quite obvious. Firstly, not all innovations are patentable. Since patent criteria are developed only to protect technological innovations, non-technological or organizational innovations will not be included. This should not be a worrying concern in this case, since we are interested in the concrete technological features, in regard to CO_2 emissions, of inventions. Secondly, as there are other intellectual property rights regimes to protect various kinds of innovations, including trademarks and industrial designs, not all innovations

[28]Typically in the "home" office of the inventor or applicant.

[29]Notably the majority of inventions in the world are 1 family patents.

[30]Typically assumed as patenting is costly and a firm would rationally only seek to protect an invention in more than one jurisdiction if the prospect commercial value justifies the patenting costs.

that are patentable *will be* patented. Lastly, there is great variation on the quality of patented inventions. The economic value of patents vary greatly and not all patented inventions end up being commercialized and adopted (Haščič & Migotto, 2015)[31], however, we are not particularly concerned about this, although it is something to keep in mind for the analysis.

3.2 Technical details of data and summary statistics

Data on carbon taxes is retrieved from the World Bank (2020), and we need to make a few points on the methodology and comparability before analyzing this data. Carbon taxes are measured in USD nominal prices per tonnes of CO_2 emissions by year, and we use the US CPI index (U.S. Bureau of Labor Statistics, 2019) to adjust the data for inflation. However, given that purchasing power varies across countries, we should still be a little cautious in comparing these prices strictly at face value.

We use patent statistics from a dataset on selected environmental technologies (ENVTECH) and patent based-indicators, constructed jointly by The OECD Environment Directorate and Directorate for Science, Technology and Innovation (2020). The dataset pools original patents data from multiple national and international patents offices.[32] The data covers a wide-range of environment-related technologies, categorized into different sub-categories covering about 80 technological fields, from waste-management and water-related adaptation to climate change mitigation. In this paper, we further limit our sample to data on patents in the "climate change-mitigation"-technologies category, which aggregates all subcategories of patents in different usage fields with the common feature of mitigating GHG emissions.[33] In particular, we use patents indicator data on climate change mitigation inventions expressed in per capita million[34] (i.e. per million residents), which is particularly appropriate as a comparable measure on countries' innovative

[31] Another particular feature of patent data is that patent applications are usually disclosed 18 months after filing date, which leads to a "publication lag" in the raw data. This is adjusted for in the employed dataset, however, since patents are classified by "priority date" (the filing date of the first application), which is commonly considered a fairly good proxy of the invention date.

[32] The dataset on ENV-TECH patents is developed by the OECD by using algorithms applied to data extracted from the Worldwide Patent Statistical Database (PATSTAT), managed by the European Patent Office (EPO).

[33] This is an encompassing measure that includes climate change mitigation technologies related to production, transport, wastewater management, information and communication technologies (ICT), GHG capturing technologies and technologies related to construction and buildings.

[34] We construct the variable on climate change mitigation technology patents per million capita, by the simple method of summing together the contributions (percentages) of each of the climate change mitigation technologies to the selected environmental technologies per capita million indicator.

performance. Indicator data from ENVTECH is available in a balanced panel of 2 175 observations on 37 OECD countries from 1960 to 2016.

Finally, throughout our empirical investigation we also employ data on the following variables: GDP per capita (Feenstra et al., 2015), GDP growth per capita (World Bank, 2015), real fuel prices (IEA, 2020), real fuel taxes (IEA, 2020), patents per capita million (by residents) (World Bank, 2015), urban population share (World Bank, 2015) and government spending on environmental-related R&D (OECD, 2020). We present and motivate the use of each of these variables as we employ them in the next section: Empirical strategy (4). Below, we give an overview with a table of summary statistics of all variables that we use, including for which years we have data available.

Table 1: Summary statistics: Data from 37 OECD countries

Variable	min	mean	max	std	n	years
CC patents per capita mil	0.00	5.21	85.00	9.19	2052	1960-2016
Real carbon tax	0.01	18.27	105.63	22.05	226	1990-2016
GDP per capita	1983.95	26483.32	97717.02	13546.94	1512	1970-2016
GDP per capita growth	-14.27	2.21	23.99	3.18	1278	1978-2016
Government R&D	0.00	154.64	1505.30	230.99	1023	1981-2016
Patents app. per capita mil	2.56	249.06	3028.95	439.00	1161	1980-2016
Real fuel price	0.24	0.91	2.47	0.36	1255	1978-2016
Real fuel tax	0.00	0.48	1.40	0.23	1203	1978-2016
Urban population share	37.00	73.46	98.00	12.26	1666	1970-2016

Note: For real carbon tax data we only have data from the 17 countries that implemented a carbon tax between 1990-2016. See figure 14 in Appendix for overview.

4 Empirical Strategy

Our empirical strategy consists of two parts. In the first and main part of our empirical investigation, we do a case study of the Swedish carbon tax, estimating the effects of the tax on clean technological innovation in Sweden by a synthetic control method (Abadie and Gardeazabal 2003; Abadie et al. 2010, 2015). In the second part of the empirical strategy we conduct a panel data regression analysis using cross-country data on carbon taxes from 17 OECD countries.

The synthetic control method has a number of merits. Firstly, it allows us to evaluate the effects of carbon taxation on technological innovation using *ex post* empirical data as opposed to commonly used *ex ante* simulations. Secondly, the synthetic control method is specifically developed for cases when treatment is enacted at an aggregate level on one unit, with data available at its level for a large number of periods and there are multiple units that are untreated (Abadie and Gardeazabal 2003; Abadie et al. 2010, 2015). This should make it a suitable method in the particular context of the Swedish carbon tax.

In addition, compared to the differences-in-differences (DiD) estimator, commonly employed for policy evaluations, the synthetic control method has several advantages. Firstly, it relaxes the parallel trends assumption, central to the DiD, by allowing for variation over time in the impact of unobserved confounders (Abadie et al. 2010). Secondly, the synthetic controls method allows us to include covariates as predictors of low-carbon technological innovation in our model. In contrast, in a DiD empirical estimation we would have to leave such covariates out of the model as they, by their likelihood to also be affected by the carbon tax, would be considered "bad controls" (Angrist & Pischke, 2009). Thirdly, the synthetic control method allows us to choose comparison units by a data-driven approach, avoiding the risk of ambiguity often associated with the DiD method. Lastly, the transparency of the relative weights of each unit in the donor group to the synthetic control provides additional interpretability to the comparison between synthetic control and treated unit.

By this method, we construct a counterfactual "synthetic Sweden" from a weighted combination of carefully selected OECD countries that did not implement a carbon tax during nor prior to the treatment period. The details of this strategy will follow in the remaining part of this section. In the second and complementary part of the empirical investigation we use panel data on 17 countries over the years 1990-2016 to broadly estimate the effects of carbon pricing on technological innovation in our cross-country sample. This is interesting for investigating whether or not the potential effect in the Swedish case is also detectable in a classic panel data regression analysis with an international sample. Apart from our main explanatory variable, the carbon tax, we also use data on fuel taxes and fuel prices for robustness checks. While fuel taxes are different in nature to a carbon tax, and less comprehensive in its coverage, these can function as a proxy for a carbon tax, see e.g. Aghion et al., (2016) [35].

4.1 The synthetic control method

4.1.1 The donor pool

In order to empirically estimate the impact of the 1991 Swedish carbon tax on clean innovation we use annual panel data on climate change-mitigation technologies per million population for the years 1960-2005 for 37 OECD countries.

Although data is available up to 2016 and for several variables tends to be richer in later periods, we choose to use 2005 as our end date for several reasons. Firstly, 2005 marks the start of the European Emissions Trading System (EU ETS), which affects the majority of the countries in our sample. Secondly, part of our sample also enacted climate change policies such as national carbon taxes or substantial changes to fuel taxation rates in the years post-2005 to 2016. Furthermore, technically we would also be hesitant to use a longer treatment period in the synthetic control method (particularly given our limited pre-treatment period), as projecting longer periods of time in the post-treatment period is difficult since there is greater risk for encountering a structural break in the underlying model.

We use the year 1978 as our start date for the pre-treatment period. The reason for this is that for several of the key predictor variables that we use to construct our synthetic Sweden, there is no data available prior to this point. Hence, our pre-treatment period is slightly shorter than we would have preferred, but it still gives us 13 years of pre-treatment observations which is sufficient to carry out our analysis.

From the initial sample of 37 countries we exclude those that also implemented carbon taxes during the sample period, in this case Finland, Norway and Denmark. Furthermore, we exclude a number of countries[36] due to lack of data on our predictor variables during the pre-treatment period. This leaves us with a donor pool consisting

[35]Furthermore, it provides a much lengthier panel dataset compared to carbon taxes data (as there is simply much more data given that almost all countries have some sort of fuel tax on transport fuels, however, the variation in this data is generally smaller compared to the carbon tax.

[36]Chile, Czech Republic. Estonia, Hungary, Iceland, Israel, Korea, Latvia, Liechtenstein, Lithuania, Luxembourg, Mexico, New Zealand, Poland (actually Poland has a carbon tax but it is very small), Portugal, Slovak Republic, Slovenia, Turkey.

of the 15 following countries: *Australia, Austria, Belgium, Canada, France, Germany, Greece, Ireland, Italy, Japan, Netherlands, Spain, Switzerland, United Kingdom, United States.*

Hence, the sample period gives us 13 years of pre-intervention observations and 15 years of post-intervention observations. Although a longer pre-treatment period would have been preferred, had data been available, our sample arguably still provides us sufficient data to both construct a viable counterfactual and to evaluate the effects of the policy change. In figure 1, see below, we present graphs on our dependent variable, climate change mitigation technology patents (CC technology patents) per capita million for each donor country and Sweden, between 1978-2005.

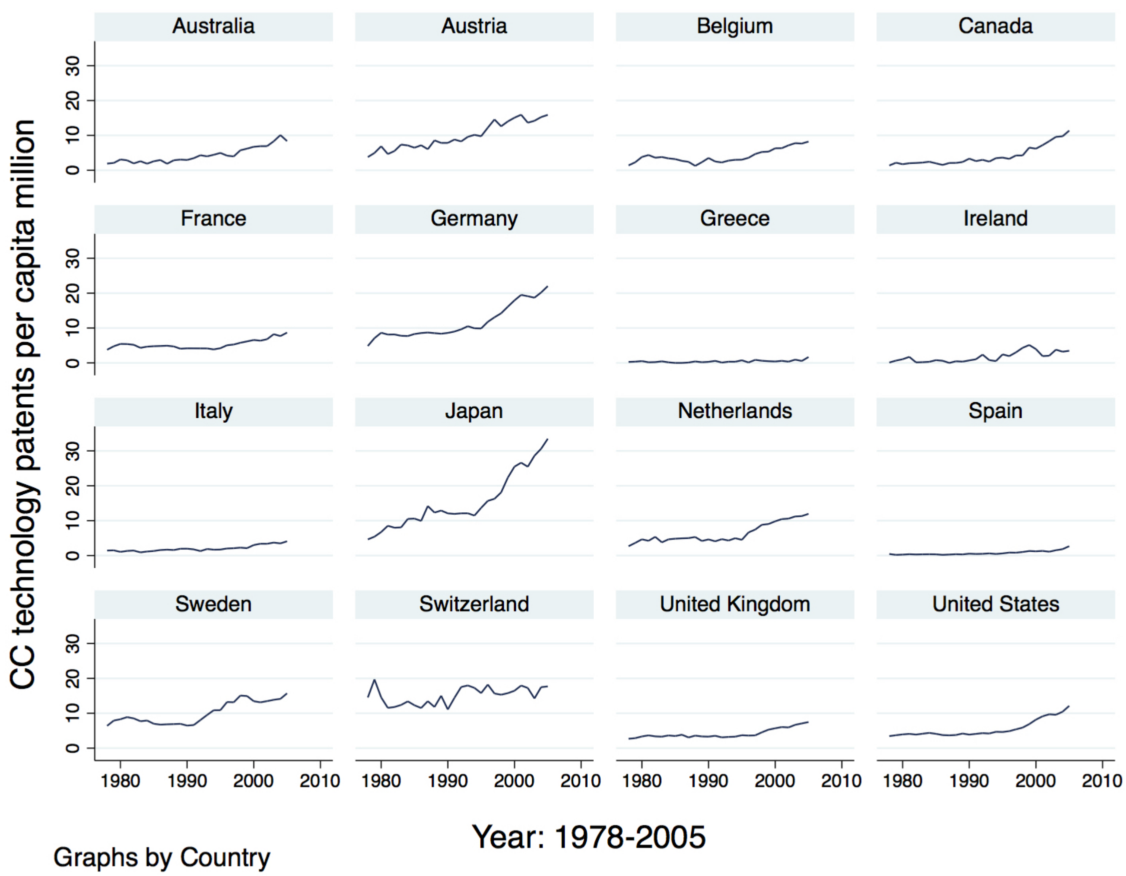

Figure 1: CC technology patents per capita million: Donor pool countries (15) and Sweden: 1978-2005

4.1.2 Predictor variables

We use the following predictor variables of clean innovation to construct our synthetic Sweden. Firstly, GDP per capita, adjusted for purchasing power parity (PPP) and measured in constant US dollars (Feenstra et al., 2015). GDP per capita is a standard measure for the wealth of a country and is typically positively associated with innovation (Ulku, 2004). Secondly, we use the tax-exclusive-fuel-price (IEA, 2020), also PPP-adjusted and in constant US dollars, which we obtain by removing the fuel price tax share from the tax-inclusive average fuel price. We use this variable as a predictor as it is quite straightforward to assume that a rise in tax-exclusive fuel prices, making fossil fuel consumption relatively more expensive, should give incentive to increase investment in clean innovation to economize production. The impact, however, will of course depend on how reliant households and industries are on fossil fuels. Furthermore, we use patent applications per capita million as an overall measure of innovation in a country. This data is obtained from the World Bank Indicators (2015) measure of total patent applications by residents. Moreover, we use the percentage of the urban population as a proxy for urbanization, as urbanization and innovation are typically positively associated. Finally, we add three lagged years of the outcome variable[37], climate change mitigation technologies patents per capita million, to our list of key predictors.

4.1.3 Methodology

As touched upon in the introduction of this chapter, the synthetic control method has a number of advantages compared to the DiD estimator, commonly employed in policy evaluation and comparative studies. The most important is that the synthetic control method relaxes *the parallel trends assumption*[38] that is fundamental for the DiD estimator, and which, apart from being difficult to verify, is violated as soon as the treated unit and the control unit do not follow a common trend. The synthetic control method, on the other hand, constructs the counterfactual through a data-driven process that forms a synthetic control unit from a weighted average of the available control units. The idea is that the weighted combination of available control units typically provides a more appropriate comparison to the treatment unit, than would a single control unit alone. Furthermore, the synthetic control methods make explicit

[37] As it is important that the countries also match on the outcome variable to some extent.

[38] The "parallel trends assumption" really consists of two assumptions i) conditional on common trends in pre-treatment unobserved covariates, effects on the outcome variable are constant over time, and ii) any shocks or other time effects are common to the treated unit and the control unit.

the contribution of each available control unit and the degree of similarity in the pre-treatment period between treated and control units, which grants an attractive feature of transparency compared to traditional regression methods. Restricting weights to be positive and sum to one, the method also provides a safeguard against extrapolation (Abadie et al., 2010).

In this thesis, we follow the method outlined in Abadie, Diamond and Hainmueller (2015) where the synthetic control is picked utilizing a *cross-validation* approach on part of the pre-treatment sample. The main advantage of this approach is that we use the validation set as a way to avoid bias due to overfitting by optimizing our weights on unseen data.[39] Essentially, this simply means that we divide the pre-treatment timespan into two periods, a training period followed by a validation period. In the training period donor weights $W^*(V)$ are optimized given predictor weights V. In the validation period, we use the estimated $W^*(V)$ from the training period to find the optimal predictor weights $V^* \in V$, and finally the weights W are re-optimized on V^* to obtain the final weights $W^*(V^*)$.

Formally, we denote the variable of interest as Y, climate change invention per million population, and the predictor variables as matrix X. Let the countries or units in our sample be denoted by $j = 1, ..., J+1$ for time periods $t = 1, ..., T$ with $j = 1$ being Sweden, the only unit that is exposed to the treatment, in our case the carbon tax. The carbon tax is introduced at time $T_0 + 1$ such that periods $1, 2, 3, ..., T_0$ are pre-intervention and periods $T_0 + 1, T_0 + 2, ..., T$ are post carbon tax.

We define two potential outcomes: Y_{it}^N refers to the outcome for country i at time t if country i is not exposed to the introduction of a carbon tax, and Y_{it}^I refers to the outcome for country i at time t if country i is exposed to the treatment or intervention. Our goal is to estimate the effect of the carbon tax on the treated unit (Sweden) in the post-treatment period. More formally we are interest in measuring the quantity:

$$\alpha_{1t} = Y_{1t}^I - Y_{1t}^N \quad \forall\, t\ \in T_0 + 1, ..., T \tag{1}$$

To construct our synthetic Sweden, we define a $(J \times 1)$ vector of weights $W = (w_2, ..., w_{j+1})'$ such that $w_j \geq 0\ \forall j = 2, ..., J+1$ and $\sum_{j=2}^{J+1} w_j = 1$. Each W represents a weighted average of our control units and is a potential candidate as a synthetic control. We then choose the weights W^* in such a way, that the synthetic control

[39] Note that we also test the synthetic control method without cross-validation, i.e. without dividing the pre-treatment period in a training and validation set but simply just minimizing the mean squared prediction error (MSPE) over the whole pre-treatment period instead. These results can be found in the Appendix.

approximates Sweden in both the outcome predictors, and linear combinations of pre-treatment outcomes. As mentioned previously, our estimation procedure follows two main steps in order to offer a robust counterfactual. A first phase where country weights W^* are determined minimizing the error on a training set, and a second phase where each predictor variable is assigned a weight that indicates the relative importance of it. We denote weights for the predictor variables as V, and the ideal components of this vector are such that the synthetic control will more or less follow the path of the original country as close as possible, minimizing the mean squared percentage error (MSPE) on unseen data in the validation set. In our case the training set contains years 1978 to 1985, and the validation set contains years 1986-1990.[40] On the training set we estimate country weights such that the total distance between synthetic Sweden and Sweden are similar[41].

Denote with $X_1^{(train)}$ the $k \times 1$ matrix of predictors for Sweden in the training period and with $X_0^{(train)}$ the $k \times J$ matrix of predictors for the donor pool, and the components of $V = (v_1, v_2, ..., v_k)$. We choose optimal country weights such that the squared sum of distances between predictors in Sweden and synthetic Sweden is minimized, more formally the optimal vector of weights $W^*(V)$ is chosen to minimize the following expression for each V:

$$\min_W ||V^{\frac{1}{2}}(X_1^{(train)} - X_0^{(train)}W)||^2 = \min_W \sum_{m=1}^{k} v_m(X_{1m}^{(train)} - X_{0m}^{(train)}W)^2$$

$$s.t.\ W \geq 0,\ 1'W = 1 \quad (2)$$

where V is a $(k \times k)$ symmetric and semi-definite matrix so that the matrix multiplication inside the square root operator above is positive or zero; $X_{1m}^{(train)}$ and $X_{0m}^{(train)}$ denote the m-th component and row of matrices $X_1^{(train)}$ and $X_0^{(train)}$. The V matrix is introduced to assign weights to the variables in X_0 and X_1 depending on their predictive power on the outcome.

[40] There is no exact rule for how to divide the training and validation periods. Nevertheless, typically the periods are divided either into equal lenghts (e.g. Abadie et al., 2015) or the training period is relatively longer, up to 70% of the pre-treatment period. In our case, in which we have an odd number of pre-treatment periods and a relatively short pre-treatment period overall, we set the cutoff to 1985, which gives a training set containing 2/3 of the total pre-treatment observations, since we judge, given our limited pre-treatment period, it is more important to have a sufficient training period, especially given the volatile nature of our outcome variable and the fact the the early 1980's might be a particular time period in some ways related to clean innovation (see results section 5), whilst still have enough observations for validation.

[41] The solution to this problem is a the set of W that minimize some distance across predictors for each V.

An optimal choice of V^* in our model assigns weights that minimize the mean squared prediction error (MSPE) of the synthetic control estimator on the validation set. Hence, V^* is the solution to the following minimization problem where the weights have been normalized to sum to 1:

$$\min_V ||Y_1^{(valid)} - Y_0^{(valid)} W^*(V)_{(train)}||^2 \quad s.t.\ V \geq 0,\ \ 1'V = 1 \tag{3}$$

Once we have calculated the optimal predictor weights, we can find the main country weights that are going to be used to construct the synthetic control by finding the optimal weights W^* that minimize the following problem on the validation set:

$$\min_W \sum_{m=1}^{k} v_m^* (X_{1m}^{(valid)} - X_{0m}^{(valid)} W)^2 \ \ s.t.\ W \geq 0,\ 1'W = 1 \tag{4}$$

Where $X_1^{(valid)}$ and $X_0^{(valid)}$ are similar to equation 2, but applied to the validation set and v_m^* are the components of the optimal vector V^* we found in equation 3 . It follows that $X_{1m}^{(valid)}$ and $X_{0m}^{(valid)}$ denote the m-th component and row of matrices $X_1^{(valid)}$ and $X_0^{(valid)}$.

Computations and routines for finding the optimal weight vectors W^* and V^* can be carried out by utilizing the R package Synth written by the authors of the method themselves, Abadie, Diamond and Hainmueller (2011).

4.2 Panel data regression

In this part of the empirical strategy, we employ a classic panel data regression approach to test our research question more broadly by using all data available on real national carbon taxes. For the panel data regression analysis we exploit data from 17 countries that implemented a carbon tax during the years 1990-2016, which gives us an unbalanced panel of 226 observations. Our main dependent variable is high value climate change mitigation technology patents per capita million[42]. We also test an alternative specification of the model using fuel taxes as a proxy of carbon taxes, since this offers us a substantially larger data material, 1 135 observations over the years 1978-2016.

Furthermore, we add several control variables to account for potential omitted variable bias (OVB), which we cover briefly below. We control for GDP growth per

[42] We use this measure for higher comparability across countries and to the synthetic control method.

capita (the World Bank, 2015) in most of our specifications. The motivation behind this is that it might be positively associated with both innovation and carbon taxes. For example in times of higher growth, economies tend to innovate more, and it might also be easier in such an environment for policy-makers to introduce or increase a carbon tax, and vice versa in times of lower/negative growth.[43] Furthermore, we include governmental research and development budget with an environment-related socio-economic objective (OECD, 2020) as a control variable. The motivation behind this is quite straight-forward, a government that introduces or increases a carbon tax is arguably also more likely to increase the government spending on environment-related R&D, which should also impact clean innovation. Lastly, we also control for the average tax-exclusive fuel price, which we simply calculate by taking the average tax-inclusive fuel price minus the average fuel tax in each period (IEA, 2020).[44] By including this variable, we control for the effect on clean innovation that is explained by the change in relative cost of consuming fossil fuels.

Table 2 below, shows summary statistics for the variables used in the panel regression from years 1990 to 2016.

Table 2: Variables summary statistics (over sample years: 1990-2016)

Variable	n	min	mean	max	sd
CC pat. per capita million	972	0.00	8.99	85.00	11.80
Real carbon tax	226	0.01	18.27	105.63	22.05
GDP per capita growth	942	-14.27	2.11	23.99	3.30
Real fuel price	869	0.24	0.92	2.47	0.37
Real fuel tax	847	0.06	0.52	1.40	0.23
Government R&D	792	0.00	160.23	1422.33	231.88

Finally, we could imagine some other potential sources of omitted variable

[43] On the other hand, there is a risk that GDP growth per capita may also be considered a "bad control", if we suspect that it is rather a channel of how carbon taxes affect innovation, e.g. if a higher carbon tax leads to lower growth which in turn lowers innovative activity. There is little support in the literature for this suspicion however, on the contrary, findings in recent research suggests that the economic impact would be minimal or even positive (see e.g. overview by Fawcett et al., 2018).

[44] From the IEA (2020) we have data on tax-inclusive fuel prices and fuel taxes, in 2005 constant USD and PPP-adjusted, from 37 countries from 1978-2016. We construct the country-specific variable on average tax-inclusive fuel price and average fuel taxes by simply averaging the prices and taxes on gasoline and diesel for each year, following the same method employed by Aghion et al. (2016).

bias, including e.g. exposure to media coverage of the climate crisis[45], however, comprehensive data on such variables have proved very difficult to find for the particular time period at hand. Since we do suspect that our controls might not fully capture all sources of potential OVB we also run several specifications that include a country-specific linear trend term as a way to partially control for such factors.

Baseline model: We build a linear model using country and time fixed effects. Below is the specification of the model:

$$CC\_patents_{c,t} = \beta \times Carbon\_Tax_{c,t-1} + \delta t_t + \Gamma C_c + K_{c,t-1} + \lambda growth_{c,t} + \alpha + \epsilon_t \quad (5)$$

$CC\_patents_{c,t}$ is the the number of "climate change mitigation technologies" patented in a specific country c in a given time period t_t. We also introduce t, a dummy for the time period, in this case a given year, and C_c, a dummy for countries, and δ and Γ are the relative vectors of coefficients. $Carbon\_Tax_{c,t-1}$ is the country and time specific carbon tax at time $t-1$, β is the coefficient regarding the carbon tax that we are interested in. We follow the example of Aghion et al. (2016) and lag the value (with one year) of the real CO_2 tax, as well as Government spending on environmental-related R&D and the tax-exclusive fuel price (FP), contained in the matrix of controls K. Further, we control for GDP growth. The motivation behind using lagged values is that we might expect that innovation responds to changes in carbon taxes (and fuel prices), as well as R&D investments, not immediately but with some reaction time.[46]

Alternative specification: Apart from the carbon tax we also estimate the effects of fuel taxes on clean innovation, using a model analogous to the one above. While fuel taxes are not exactly substitutes to carbon taxes[47], they are comparable in nature and hence a change in fuel taxes should impact innovation by similar logic. Data on fuel taxes is far more extensive than data available on national carbon taxes[48], which is the

[45]By the logic: increased media coverage would likely both increase opportunity room for policy-makers to introduce or increase a carbon tax, as well as spur climate change mitigating innovation directly.

[46]Also, it should mitigate any contemporaneous feedback effects (Aghion et al., 2016).

[47]Notably, carbon taxes levied on (theoretically) all activities that emits GHGs are more encompassing than fuel taxes, which are more or less a sales tax on gasoline and diesel and could be seen as a more narrow "sectoral carbon tax" (Sterner, 2015). Furthermore, in contrast to fuel taxes that are commonly considered regressive, a carbon tax, often designed with a "polluters pay" objective, should be simpler to levy "upstream" on producers that pollute.

[48]Fuel tax data provides a balanced panel of 1 135 observations from 37 OECD countries between 1978-2016, compared to 226 observation from 17 countries, 1990-2016 for carbon taxes.

main benefit of testing our model on this variable as a robustness check.[49] We retrieve data on fuel taxes, in constant 2005 USD and PPP-adjusted over, between 1978-2016, from the International Energy Agency (IEA, 2020). In line with Aghion et al. (2016), we use the average of diesel and gasoline taxes to construct a time-varying fuel tax for each country in our sample.[50]

[49]Furthermore, fuel taxes are also PPP-adjusted, whilst our data on carbon taxes are not, which is another drawback of the carbon tax data in terms of comparability.

[50]We construct a country-level average tax-inclusive fuel price by the same method. By subtracting the fuel tax from the fuel price, we get the tax-exclusive fuel price that we use both as a control in the panel data regression and as a predictor in our synthetic control method.

5 Results

5.1 Synthetic control results

In this section we analyze the results from our synthetic control strategy. First, we start out by comparing the pre-treatment fit between Sweden and the simple average of our donor countries, in terms of the outcome variable, for descriptive purposes. Next, we compare the fit between our synthetic Sweden and Sweden in the pre-treatment period to get a sense of how well our counterfactual resembles real Sweden. A similar pre-treatment path between Sweden and the counterfactual is central to the identifying assumption of the synthetic control method. Secondly, we turn to analyzing the divergence in trajectory between Sweden and our synthetic control in the post-treatment period and estimate the effects. Lastly, we conduct several placebo tests to check the robustness of our results.

5.1.1 Sweden versus synthetic Sweden

The synthetic control method relies on the identifying assumption that the synthetic control unit provides the path of climate change mitigating patents in Sweden in the post-treatment years 1991-2005, in absence of treatment i.e. had the carbon tax not been introduced. For this assumption to be credible, the synthetic Sweden should be able to satisfyingly track both the trajectory of climate change mitigating innovation, measured in patents per million capita, as well as values on key predictors in the pre-treatment period, 1978-1990.

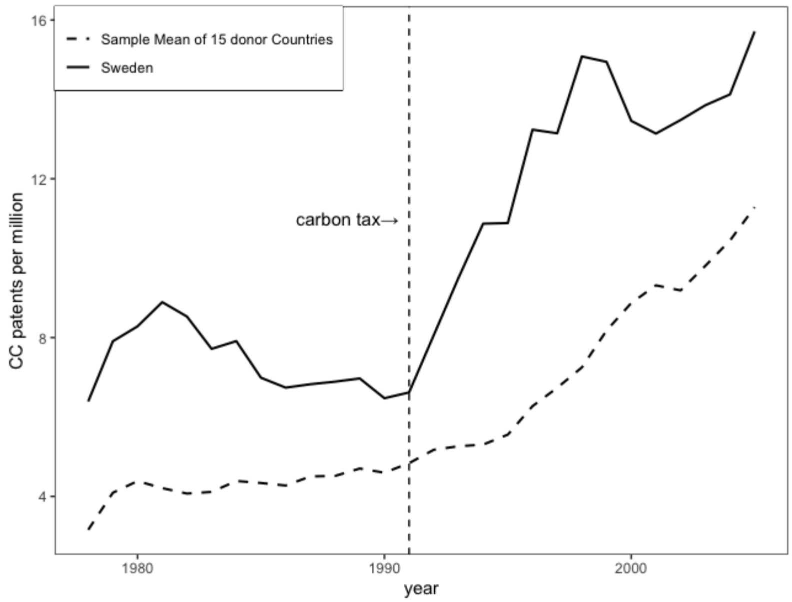

Figure 2: Path of CC technology patents per capita million between Sweden and 15 donor countries mean

To start off, we turn to figure 2, which shows the trajectory of clean innovation, as measured by climate change mitigation patents per capita million, in Sweden versus the simple averge of the 15 OECD-countries in our donor pool between the years 1978-2005. As can be depicted from the graph, the donor pool simple average gives a poor fit to the path of Sweden, both in terms of the pre-treatment trends but also (notably) in terms of levels. The donor pool average path of clean innovation is substantially lower compared to Sweden's levels, throughout the whole sample period. This result basically shows that the parallel trends assumption, underlying the DiD method, would clearly have been violated and our results would have been biased, had we used such a framework using the averaged donor pool as the control unit. We move on to our synthetic Sweden, composed of a weighted average of the donor countries, instead to compare how well it tracks Sweden in the pre-treatment years.

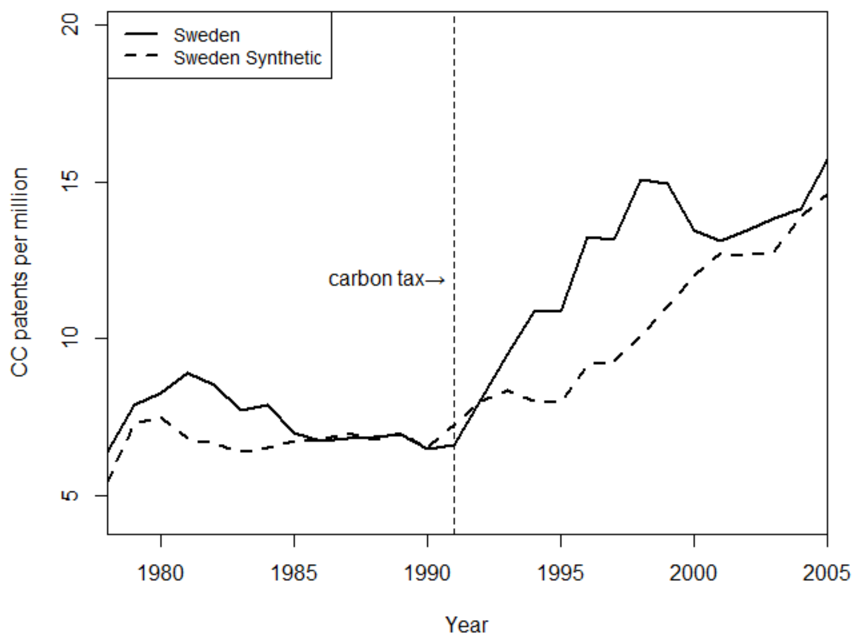

Figure 3: Path of CC technology patents per capita million between Sweden and synthetic Sweden

Figure 2 shows that prior to the treatment year, 1991, the climate change mitigation innovation paths of synthetic Sweden and Sweden follows similar trends. We note that the paths do not match perfectly, but given the volatility of our measurement unit as well as some particular economic and political circumstances marking the early 1980's[51], this is something not completely unexpected. We do, however, observe that Sweden and its synthetic counterpart track each other very closely after 1985, which is reassuring. Although, we should also note that the fact that the paths of Sweden and synthetic Sweden are better aligned post-1985 might also have to do with the cross-validation method, since this optimizes predictor weights between 1985-1990 in the validation period, given the optimized country weights obtained in the training

[51]Notably, there is a slight divergence also in the early 1980's in which the path of climate change innovation patents in Sweden lie strikingly above the synthetic Sweden, possibly, this could be related to a strong environmental movement in Sweden during these years, with some notable events such as the founding of the Swedish Green party in 1981 and a referendum about the future of nuclear power in 1980. Furthermore, we should also note that the 1979 oil crisis, prompted by the Iranian Revolution, shocked the global economy by causing a surge in oil prices (which did not return to pre-1979 levels until the mid 1980's) and an international recession. It is plausible that the oil shock would have caused heterogeneous effects in terms of innovative activity depending on underlying structures of the economy (here we note that Sweden has no own production of crude oil but is completely dependent on international imports), which might also possibly contribute to explaining some of the gap between Sweden and synthetic Sweden in the early 1980's.

period (see methodology section for more details). For comparison, see figure 15 in Appendix, where we show the results of the synthetic control method without cross-validation, in which we minimize the mean squared prediction error (MSPE) over the whole pre-treatment period. By this method, as expected, the trajectory of Sweden and synthetic Sweden are somewhat better aligned over the full period, but relatively worse matched in the years 1985-1990 compared to cross-validation. As we will come back to later, however, this does not affect our results. The cross-validation procedure, on the other hand, tends to produce less biased estimates (Abadie et al., 2015).

In table 3 we compare the average values of key predictors over the pre-intervention year 1978-1990, where the synthetic control unit represents a weighted average of the donor pool countries characteristics, and the sample mean just gives the mean over the whole sample. It is encouraging that the average predictor values of the synthetic control more closely resembles the values of Sweden for all variables compared to the sample means.

Table 3: Key predictor means pre-intervention

	Treated	synthetic	sample mean
GDP per capita	21722.486	22541.623	20759.293
Patents app. per capita million	463.422	410.492	323.454
Urban population share	83.068	75.241	73.462
Tax exclusive real fuel price	0.364	0.401	0.481
CC patents per capita million 1978	6.391	5.443	3.158
CC patents per capita million 1984	7.911	6.589	4.388
CC patents per capita million 1990	6.472	6.516	4.597

Table 4: Predictor weights V^*.

Variable	V^*
GDP per capita	0.336
Patent app. per capita million	0.034
Urban population share	0.016
Tax-exclusive real fuel price	0.275
CC patents per capita million (1978)	0.131
CC patents per capita million (1984)	0.023
CC patents per capita million (1990)	0.185

Table 4 shows the optimal predictor weights V^* that we found: 0.336 for GDP per capita, 0.034 for patent applications per million, 0.016 for urban population share, 0.275 for real fuel price exclusive of tax, and 0.131, 0.023 and 0.185, for climate change mitigation technology patents per million population respectively for the years 1978, 1984 and 1990[52].

GDP per capita and the tax-exclusive real fuel price are the most important predictors for our synthetic Sweden in this model and this is consistent with the relatively better match of these variables compared to the predictors with less weight assigned to them, such as urban population share. Furthermore, we note that our general measure of innovation, patent applications per million residents[53], takes a relatively small weight as a predictor in our synthetic Sweden. On the other hand, the combined weight of the three lagged values on climate change technology patents, with the final year before treatment (1990) being most dominant, amounts to 0.339, i.e. about a third of the total weights. This might suggest that a pre-treatment trend of innovation in clean technologies might be relatively more important to predict the

[52] We tried different combinations of the lagged values of climate change mitigation patents, such as e.g. 1980,1985,1990, and 1982,1986,1990, but none gave a better pre-treatment fit or had any substantial impact on the estimated effect.

[53] We should also note here that this measures only patent applications by residents in the country in question, and not total patent applications, i.e. it excludes patent applications in a country by non-residents. This is important to keep in mind since in some countries non-residents may account for a large part of innovations. However, for the purposes of this study, we think that patent applications by residents might better capture innovative activity in a country that is more stable in that country, even as policies change. Patent-applications by non-residents, on the other hand, is likely a measure more skewed to some countries with big high-technology sectors of industry or research, or home to some of the large patent application offices. Also, non-residential patent-applications as a measure of innovation might reflect more strategic geographic location decisions by major multinational companies and less actual domestic innovation.

future of innovation in clean technologies, than the pre-treatment trend of innovation in general.

Table 5: Country Weights W^*

Weight	Country	Country Number
0.350	Germany	6
0.344	France	5
0.161	Australia	1
0.145	Switzerland	14
0.000	Austria	2
0.000	Belgium	3
0.000	Canada	4
0.000	Greece	7
0.000	Ireland	8
0.000	Italy	9
0.000	Japan	10
0.000	Netherlands	11
0.000	Spain	12
0.000	United Kingdom	15
0.000	United States	16

Table 5 shows that the synthetic Sweden, in our model, is best reproduced by a combination of Germany, France, Australia and Switzerland.[54] The remaining countries in the donor pool gets either a weight of zero or a weight so close to zero that it is negligible.[55] We note without surprise, that the donor countries to synthetic Sweden are composed by other west-European countries, with the exception of Australia, as the combination of these countries most closely resemble Sweden in our key predictors such as GDP per capita, GDP growth rate and number of patent applications per year.

[54]Given the small sample size the country weights are not completely stable. While changing specifications leads to slightly different country weights, we still estimate the same gap in climate change mitigation patents per million population in the post-treatment period with a having a worse fit in the pre-treatment period.

[55]We report the weights up to the third decimal place in Table 5.

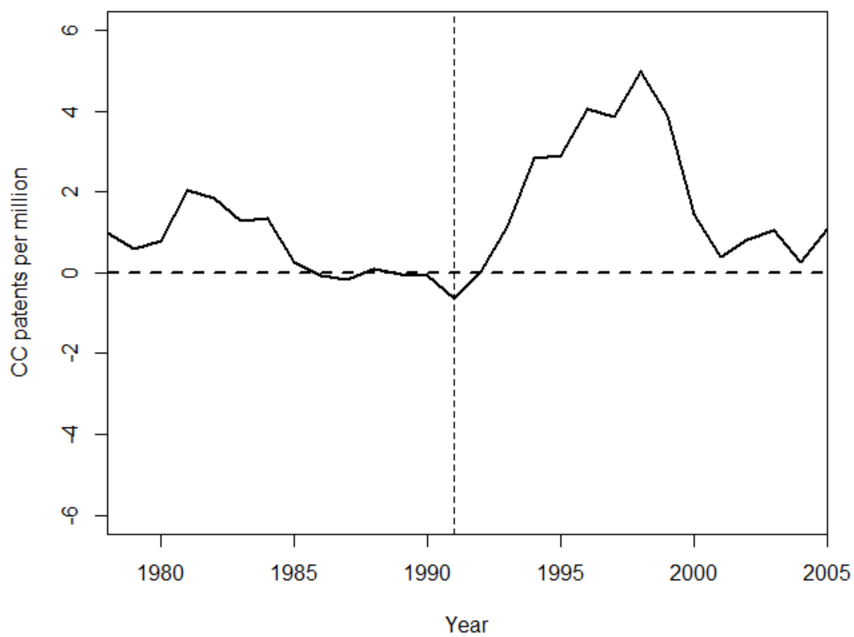

Figure 4: Gap in CC technology patents per capita million between Sweden and synthetic Sweden

5.1.2 Estimated effects

The divergence in trajectory of the post-treatment gap in climate change mitigating innovation, depicted in figure 3 is better visualized in the above gap plot, figure 4. From this plot we can clearly see the divergence of Sweden from the pre-treatment trend. Shortly after the introduction of the carbon tax in 1991 there is seemingly a strong positive effect on clean innovation. We estimate an increase, relative to the synthetic Sweden, of on average 1.88 climate change mitigating technology patents per capita million yearly, or 14.1%, in an average year in the post-treatment period 1991-2005. In the first 10 years of the carbon tax being in effect, we estimate a positive effect of 18.5% in an average year.[56] We also estimate the cumulative effect of the carbon tax on clean innovation in the post-treatment period (1991-2005) to 28.25 climate change mitigation technology patents per million in total. Aggregating over the total Swedish population, this suggests that Sweden produced in total 249.88 more patents in the post-treatment period than it would have done in absence of the carbon tax. To put

[56]We also note that Sweden suffered a financial crisis in the early 1990's, which might explain why in the first couple of years after the introduction of the carbon tax, 1991-1992, we observe a very small effect on clean innovation, whereas for the year 1993-2000, when Sweden's economy has largely recovered, we estimate a postive effect of wholly 24.35% in an average year.

this into context, consider that Sweden produced on average a total of 114.95 clean patents yearly between 1991-2005.[57]

Interestingly, we observe that although the carbon tax had a positive effect on innovation over the whole post-treatment period, there is a notable drop back in magnitude in the early 2000's. As can be depicted in figure 3, climate change innovation in Sweden, after seeing a substantial increase in the 1990's, returns to levels relatively much closer to the values predicted by synthetic Sweden after about 10 years in effect. In fact, in the year 1999 we estimate that the carbon tax had an average positive effect of 26.2% compared to the synthetic Sweden, which falls to 10.8% in the year 2000 and 3.1% in the subsequent year 2001. We estimate the average yearly effect between 2000-2005 to 6.2%, which is substantially lower than the average yearly effect of 18.5% throughout the 1990's. There could be several explanations to this, one being that the energy tax rate was reduced in the years 2001-2005, almost cancelling out the concurring increase in the carbon tax. The net effect on the combined real tax rate (change in carbon and energy tax) during these years was almost zero.[58] Unfortunately, an empirical investigation of this drop back is outside the scope of this thesis, but we present some potential causes in the discussion section. Even with this drop-back, however, the estimated total effect of 249.88 more clean patents over the first 15 years of the carbon tax being implemented, is arguably economically substantial.[59]

5.1.3 Placebo tests

To test the validity of our results and to obtain more insights regarding the uncertainty of our estimates we perform a number of placebo tests, including *in-time* and *in-space* tests, as well as a *leave-one-out* test, in line with Abadie et al. (2010, 2015).

The in-time test exploits the longitudinal dimension to test robustness. This means that we run the synthetic control model on our subject of treatment, i.e. Sweden, but let the dates of the intervention be set at random (Bertrand et al., 2004, Abadie et al., 2015). If the placebo interventions produce a divergence in trajectory between Sweden synthetic Sweden, it would cast doubts both over the existence of causal relationship between the predictors and the dependent variable as well as about the existence of

[57]The average total production of clean patents yearly between 1991-2016 is 172.53, which reflects the continued increase post 2005, which is the end date for our synthetic control evalution.

[58]It should also be noted that on the retail price of gasoline a VAT of 25% is applied to the combined value of the tax-exclusive price, the carbon tax and the energy tax, hence acting like a multiplier to any changes in the price components.

[59]As preciously noted, we also test the synthetic control method without cross-validation. A gap plot of the estimated results using that method can be found in the Appendix, see figure 16. Reassuringly, this does not change our result.

any true effect of the Swedish carbon tax on clean innovation. We carry out this test by assigning treatment to years 1988 and 1986 as is shown in the figure below[60].

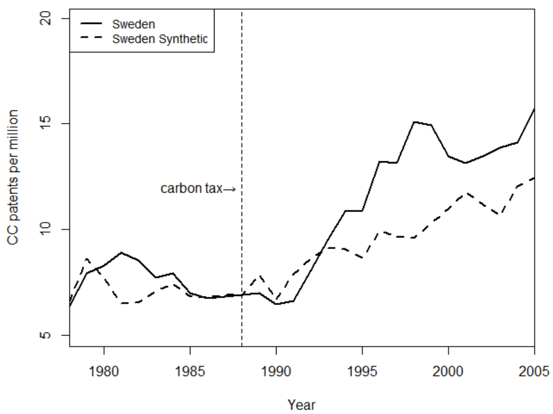

Figure 5: Path of Sweden vs. synthetic Sweden: placebo treatment year 1988

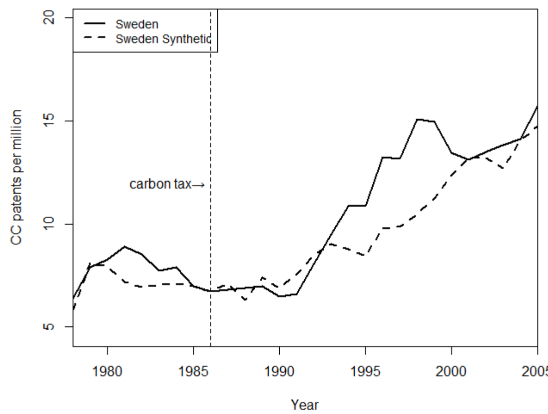

Figure 6: Path of Sweden vs. synthetic Sweden: placebo treatment year 1986

We see from figure 5 and 6 respectively that the gap between Sweden and the synthetic control persists even when we assign the treatment date at random. This is reassuring as it suggests that our result is robust to such manipulations, and that indeed our estimated effect of the introduction of the real carbon tax in 1991 could not be reproduced by an in-time arbitrary treatment.

A placebo test in space, on the other hand, means iteratively applying our synthetic control model to estimate the effect of the Swedish 1991 carbon tax, to each country in our donor pool. In other words, we reassign treatment to every country in our donor pool (as if they had implemented a carbon tax in 1991 instead of Sweden) and compute the estimated effect of each of these placebo runs. Here the premise is that this provides us with a distribution of the estimated placebo effects in all countries that were never directly exposed to the treatment. It is a way of quantifying the uncertainty around the estimated gap for Sweden. If the placebo tests register gaps of a magnitude similar to the one for Sweden, then we would have a hard time interpreting our results as evidence of any significant effect on the direction of technological innovation for Sweden. On the other hand, if the placebo test shows that the gap estimated for Sweden is abnormally large relative to the gaps in the placebo runs, this can be interpreted as evidence of a significant effect (Abadie et al., 2010).

[60]We split the training and validation sets keeping the same ratio as in the original specification.

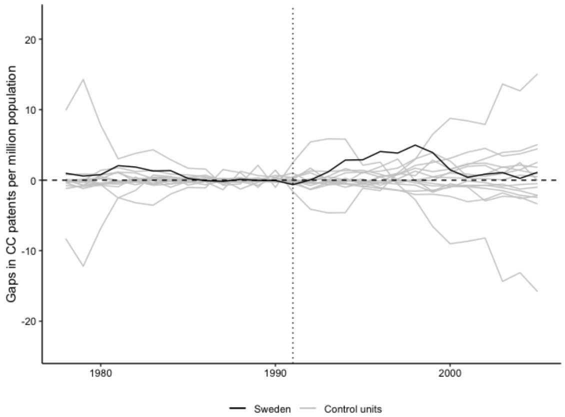

Figure 7: Permutation test: CC patents gap of Sweden vs. synthetic Sweden and placebo gaps in all 17 donor countries

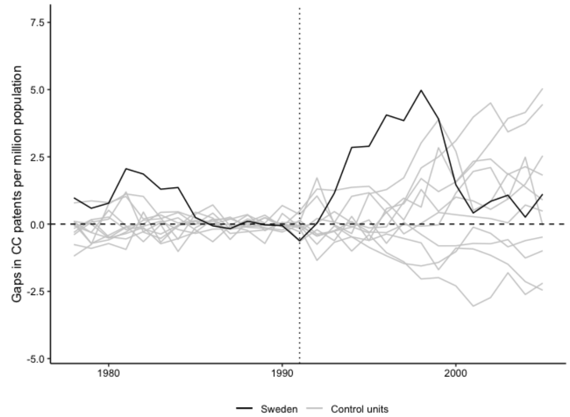

Figure 8: Permutation test: CC patents gap of Sweden vs. synthetic Sweden and placebo gaps in countries with a good pre-treatment fit

In the above figures, the grey lines represent the gap between each country in our donor pool and its respective synthetic counterfactual. The black line represents the gap between Sweden and synthetic Sweden.

Figure 7 shows the result of the in-space placebo test when we include all countries in the donor pool, even the cases where the synthetic control algorithm fails to find a convex combination of countries that can replicate the trajectory of climate change patents per million in the pre-treatment period. The figure indicates that the estimated gap for Sweden post-treatment is unusually large compared to the distribution of placebo gaps of the donor pool countries in the years 1991-2000, which is further evidence that the estimated gap is not due to variance in the data. However, as including donor countries with poor fit prior to treatment will plausibly not provide any interpretative post-treatment information about probabilities of finding a large estimated gap[61], Abadie et al. (2010) also suggests to run the same placebo test excluding units with a poor pre-treatment fit.

In figure 8, we exclude countries with a poor fit on the pre-treatment period, i.e. countries whose mean squared prediction error (MSPE) is 20 times larger, on the validation set, than Sweden's MSPE. This excludes 4 countries, namely: Austria, Belgium, Japan and United Kingdom. This give us a more adequate group of placebo gaps against which to measure whether our estimated gap for Sweden is indeed real and unusually large. Our results suggest that if we had assigned treatment at random we would have had roughly a 1/12 chance of Sweden being the one with the largest

[61] Including countries with a poor pre-treatment fit in the placebo test may lead to over-rejection of an effect.

post-treatment gap between years 1990-2000. In this case the p-value would be 1/12 = 0.083. Figure 8 shows that Sweden still had the highest post-treatment gap after intervention after after excluding the countries with a poor fit.

In addition, similarly to Abadie et al. (2010) and Andersson (2019), to avoid relying only on the MSPE cut-off threshold that is somewhat arbitrary (referring to the MSPE of 20 times higher than to that of Sweden), we also show the ratio of post-treatment MSPE to pre-treatment MSPE as an indication of a true causal effect. By looking at the ratio we do not need to arbitrarily exclude countries on our cut-off rule, this is useful in our case where the donor pool is not big.

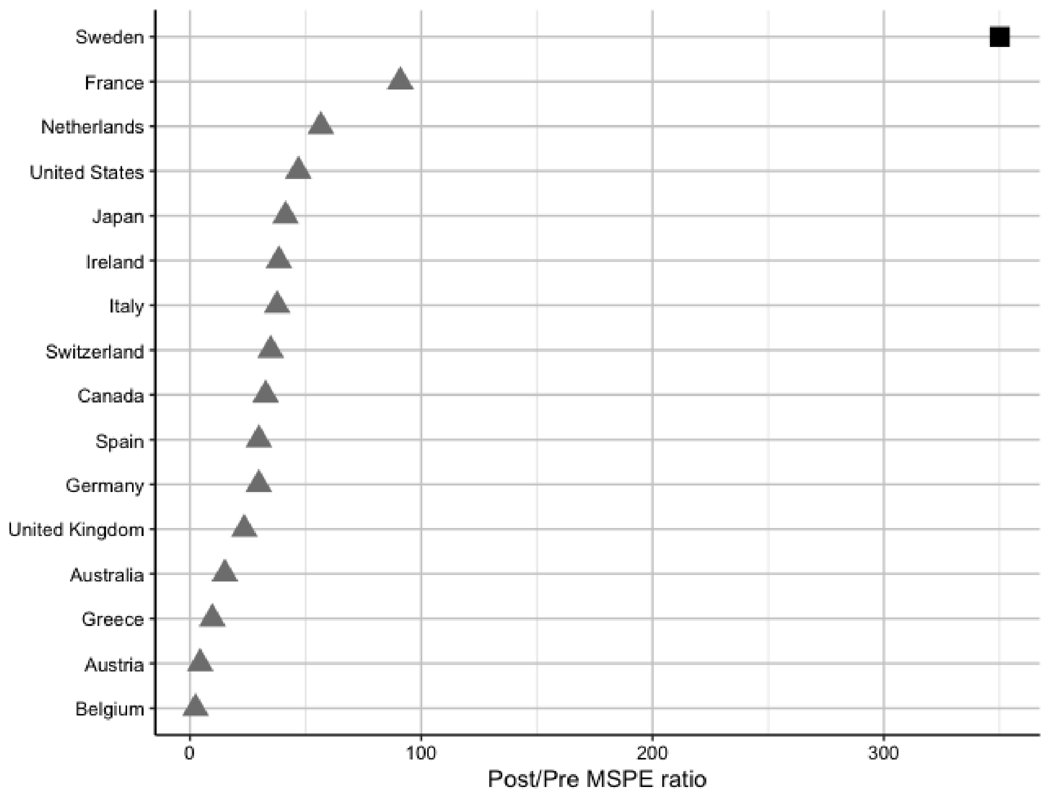

Figure 9: Ratio test: Ratios of post-treatment MSPE to pre-treatment MSPE: Sweden and 15 donor countries

Figure 9 shows that Sweden has by far the largest post-treatment to pre-treatment MSPE ratio of our 16 countries. If we have to assign the treatment at random, the probability of finding a ratio as large as Sweden's is 1/16 or 0.0625, the smallest possible p-value with our sample size.

5.1.4 Leave-one-out test

Lastly, we perform a leave-one-out test in which we try to estimate the uncertainty regarding our synthetic control results, by repeating the same procedure but iteratively removing one country from the donor pool.

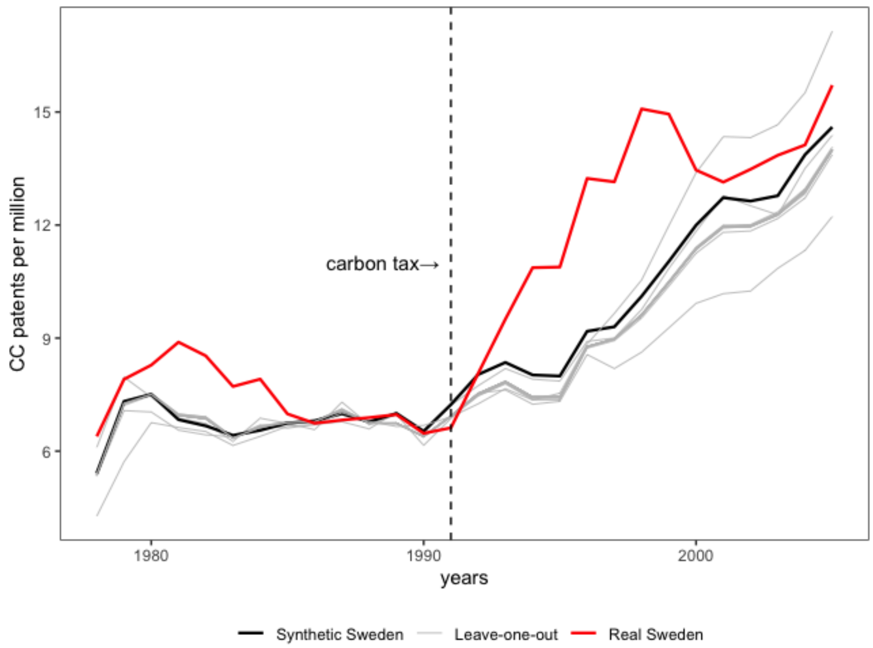

Figure 10: Leave-one-out: Distribution of synthetic controls for Sweden

In figure 10 the black line is the optimal synthetic control estimated using cross-validation and the entire pool of donor countries, the grey lines are the synthetic control estimated by each of the leave one out iterations. We also add real Sweden (the red line) as a comparison. From the plot we see that in all specification the grey lines are below the red one from 1991 to 2000. There is only one line that is above real Sweden, but only for a limited period of time at the end[62]. Overall the majority of the results tend to be largely unchanged in terms of estimated gaps.

5.2 Panel data regression results

In table 6 we present our results from the cross-country panel data regressions with the fixed effects estimator. In specification 1-4 we add our control variables one by one, but neither seems to have much impact on our the estimate of the independent variable,

[62]This only happens after year 2000.

the real CO_2 tax. In specification 5-7 we add a country-specific linear time-trend in order to capture any trend effects in various characteristics that might be a source of potential OVB in our baseline specifications[63].

Table 6: CC technology patents per capita million, real CO_2 tax as independent variable.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	FE	FE	FE	FE	FE	FE	FE
Real CO_2 tax (lag)	-0.0124	-0.0157	-0.139	-0.232	0.0644	0.0440	0.0136
	(0.141)	(0.145)	(0.158)	(0.185)	(0.089)	(0.106)	(0.124)
GDP growth per capita		0.324	0.388	0.199	0.0809	0.0964	0.0166
		(0.311)	(0.283)	(0.170)	(0.166)	(0.151)	(0.123)
GOV R&D (lag)			-0.000830	-0.0115		0.0217	0.0236
			(0.013)	(0.021)		(0.020)	(0.021)
Tax exclusive FP (lag)				34.34			25.42
				(41.105)			(24.790)
Trend					YES	YES	YES
Constant	4.657	6.999	1.156	-11.35	-986.1***	-1188.8***	-1383.7***
	(4.532)	(4.855)	(1.173)	(14.429)	(267.917)	(364.819)	(440.529)
Observations	203	203	179	161	203	179	161
R^2	0.428	0.434	0.530	0.626	0.797	0.810	0.832

Note: Standard errors in parentheses, clustered at country level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As evident from the table 6, we find no detectable effect of a carbon tax on clean innovation as measured by climate change technology patents per capita million in our cross-country sample. In fact, the estimate of the carbon tax on innovation is negative in all specifications, apart from specifications 5-7 when adding the country-specific trends. However, as the estimate in every specification is very close to zero and insignificant, we cannot draw any conclusions about these results, not even in terms of direction of a potential effect.

As discussed in the methodology section, one of the main drawbacks with the panel

[63]For example factors such as medial coverage of the climate crisis which is likely to have increased over time but to various extent in different countries, and which we would have liked to control for specifically had data been available.

data regression model is the limited number of observations in cross-country data. In our sample we have 17 countries that implemented a carbon tax sometime during the time period 1990-2016. However, due to the fact that few countries adopted carbon taxes pre-2005, only eight of these countries provides more than eight observations. Furthermore, five of the national carbon taxes that were enacted during this period have been kept at a very low level throughout (below 5 USD/tons CO2 emissions in 2016 nominal prices) and with relatively small changes to the rate (World Bank, 2020). Due to the limited data, we also conduct fixed effects panel data regressions on clean innovation using changes in fuel tax, as a proxy for carbon tax. These results can be found in table 7.

Table 7: CC technology patents per capita million, log of real fuel tax as independent variable.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	FE	FE	FE	FE	FE	FE	FE
Ln real fuel tax (lag)	-0.205	0.645	3.261*	3.584*	2.942*	2.715	2.867
	(1.577)	(1.589)	(1.789)	(1.776)	(1.567)	(2.014)	(1.992)
GDP growth per capita		-0.0538	0.124	0.0836	0.0777	0.166**	0.148*
		(0.090)	(0.110)	(0.115)	(0.067)	(0.074)	(0.076)
GOV R&D (lag)			0.00153	0.00105		-0.00701***	-0.00687***
			(0.009)	(0.010)		(0.002)	(0.002)
Ln tax exclusive FP (lag)				5.693**			2.483*
				(2.371)			(1.319)
Trend					YES	YES	YES
Constant	1.650	2.846	5.607**	9.098***	-553.7***	-846.5***	-887.7***
	(2.593)	(2.481)	(2.503)	(2.359)	(85.724)	(106.037)	(137.302)
Observations	1099	1047	845	845	1047	845	845
R^2	0.441	0.448	0.501	0.507	0.827	0.851	0.852

Note: Standard errors in parentheses, clustered at country level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In these specifications, we use the log of the fuel tax to estimate the effect on innovation. The reason for this is that it simplifies interpretation, the data on fuel tax is measured in USD per litre (compared to the carbon tax which is measured in USD per ton) and generally ranges between 0-1. Hence, in this case it makes sense to log the variable to be able to make meaningful interpretations of any changes in the tax.

Interestingly, the effect of the fuel tax is positive in all specifications, apart from the first one, which we should not worry too much about since it likely suffers from bias given the exclusion of controls. In specification 1-2, as in the regressions using the carbon tax, estimated effects are both statistically and economically insignificant (close to zero). When we add controls for government spending on environment-related R&D and the tax-exclusive fuel price, however, in specifications 3-4, the estimated effects of the fuel tax on climate change mitigation technologies jumps to a substantially higher value of 3.261 and 3.584 respectively. Furthermore, these results are significant at the 10% level. In specification 5-7 we add a country-specific linear time-trend to to our model to account for other trend effects that might potentially bias our estimates. In specification 5, the estimate is significant on the 10% level but as we add more controls in specification 6-7 estimates turn insignificant again. Interestingly, however, the estimated effect of the fuel tax on climate change inventions seems more stable generally in the specifications with a linear time-trend included[64], and varies between 2.715 and 2.942.

Altogether, regressions in table 7 provide some tentative indications on a positive direction of the effect of fuel taxes on technological innovation, which is theoretically and economically interesting. Nevertheless, the estimates are too statistically uncertain to draw any conclusions about the magnitude of the effect at all.

[64]This could also have to do with the time-trend partialling out much of the variation however.

6 Discussion

In this section we will discuss the internal and external validity of our results, as well as the merits and limitations of our empirical strategy. We will analyse our findings in relation to previous literature as well as the policy-implications of our results. Finally, we will suggest some potential opportunities for future research in the light of our findings.

To start off, we focus the discussion on the main results of this thesis, the evaluation of the Swedish carbon tax on clean innovation using the synthetic control method, which we subsequently connect to the results of our cross-country panel data regressions. Briefly summarizing the results we find, in line with theory and previous literature, that the Swedish carbon tax had a positive impact fostering clean innovation. We estimate that the Swedish carbon tax lead to an increase of average 1.88 clean patents per capita million on average yearly in the post-treatment years 1991-2005, than in the counterfactual case in absence of the carbon tax. This corresponds to an estimated increase of 14.1% in an average year. In cumulative terms, this suggests that Sweden produced 24.35 more clean patents per capita million, or 249.88 in aggregated total (population-adjusted), over the 15 years following the introduction of carbon the tax than it would have done without the tax. To put this in perspective, in an average year between 1991-2005, the total production of clean patents in Sweden amounted to 114.95. Hence, our results suggest that the Swedish carbon tax contributed to stimulate clean innovation over the full post-treatment period, of a magnitude corresponding to more than two years of average total yearly clean innovation. Arguably, this implies that the effect is economically meaningful, and quite substantial.

6.1 Internal validity

In order to be able to draw any conclusions from our results, however, we first need to have a thorough discussion about the internal validity of our study. Starting with the identifying assumption, one critique that could emerge to our strategy, is that the relatively short pre-intervention period might not provide sufficient pre-intervention observations for our identifying assumption to be fully credible. We do not believe this is a fundamental issue, however, for several reasons. First of all, as there is no technical rule defining how many periods are sufficient for a "long enough" pre-treatment time span, this is essentially a matter of judgement in each unique case. In our case, we use predictors that should, by economic theory and previous literature, be well-suited for creating our synthetic Sweden. Although for some predictors, longer pre-treatment time-spans of the data is available, it is from 1978 that we have a fully balanced data set on all predictors. In our judgement, a better balanced data set that still provides 13 years of observations formed a sounder basis to our counterfactual than would a longer but asymmetric and unbalanced pre-intervention data set.[65] Secondly, even from a relatively limited pre-treatment time-span, our predictors produce a satisfactory counterfactual in terms of matching the trajectory of Sweden.[66]. Taken together, these factors should lend credibility to the identifying assumption.

Furthermore, turning to our counterfactual we find that our synthetic Sweden is constructed primarily by weights from other west-European countries, which is not surprising given that these countries are similar to Sweden in many fundamental economic and social dimensions. That the weights are theoretically sensible adds another level of confidence that our method provides a credible counterfactual Sweden. Lastly, we perform multiple placebo tests of our results, as well as present (in the Appendix) an additional specification without cross-validation where we optimize over all available pre-treatment data. Our results prove robust to tests and changes in specification, which suggests that our estimated effect of the Swedish carbon tax is unlikely to be due to randomness. Hence, altogether, we are quite confident regarding the internal validity of our study.

6.2 External validity

Next, we turn to discuss the external validity, i.e. the generalization, of our results. External validity, however, is difficult to judge since results might be contingent on the particular context and features that apply to the Swedish carbon tax. As noted in the data section (3) the design of national carbon taxes vary across jurisdictions, which is a limitation when it comes to comparability. On the other hand, typically all fundamental aspects of carbon tax systems are mostly very similar, and the source of greater variation is instead the tax level, i.e. the price put on carbon. Hence, we expect that for countries with comparable economic and political structures to Sweden, perhaps most importantly with similarities in educational attainment levels

[65]Furthermore, if we compare to for example Abadie et al. 2015, in which the authors use data on some predictors - *schooling* - reported in five-year increments, this means that they have in total only 6 observations on this variable over the whole pre-treatment period (in their case 30 years), in a sense, our pre-treatment data with 13 observations on each variable gives, in absolute terms, more data to draw from.

[66]Additionally, the methodology applied is crafted in order to address most potential issues of the properties of our data: Low number of treated units, limited pre-treatment period, impossibility to verify the parallel trend assumption

and policies pertaining to innovation, a carbon tax with similar design and level should yield comparable results in terms of innovation in terms of clean technologies patented. However, for countries with fundamentally different economic and political structures to Sweden and/or with a carbon tax design relatively dissimilar to Sweden, the findings of this study may not apply.

6.3 Limitations

Having discussed the validity of our results, there are some limitations of the method and ambiguity of results that we still need to address. Firstly, as noted in the results section, the effect of the Swedish carbon tax on clean innovation, as estimated through the synthetic control method, does not seem completely stable over time. While our model estimates a substantial and increasing gap in clean innovation between Sweden and synthetic Sweden post-treatment (1991) throughout the 1990's, this falls back down again quite noticeably in the early 2000's. One explanatory factor for this could be the fact that climate change mitigating technologies in patents per million capita is a relatively volatile measure, as previously noted. This volatility around a trend is evident throughout the whole time series of data, which can be observed figure 11 in the Appendix.

Nevertheless, the drop back eliminates a substantial part of the gap between Sweden and synthetic Sweden, suggesting it cannot be explained only by the volatility in the measure. Hence, we need to consider what real factor could explain this. There are several potential explanations: that the effect of the carbon tax on clean innovation in Sweden is only temporary by nature; that other factors contributed to a pick-up in climate change mitigation innovation among donor countries around this time period or that Sweden experienced some kind of shock, that affected the key predictor values of Sweden comparatively more than the donor countries.

As mentioned in the results section, we note that the drop back in the early 2000's coincide with a reduction in the energy tax rate in the years 2001-2005, almost cancelling out the concurring increase in the carbon tax. In effect, the net impact on the combined real tax rate on fossil fuels during these years was almost zero. Furthermore, looking at the real carbon tax, see figures 12 and 13 in Appendix, we notice that the carbon tax actually declines in value in the years 1999 and 2000, from a rather stable level of fluctuating around real (1990) USD 40 to real (1990) USD 33.5 in 2000.[67] These real price factors are some likely explanations to the observed fallback, especially in

[67]The real carbon tax rates increases again 2002 to real USD 45 and the continues a steep increase in the following years.

the light of Sweden returning to a positive trend in clean innovation again in the years following the drop-back.

This seems especially plausible given the nature of patents as a measure of innovation, since additional patents are something that can only increase the current technological level to some extent. This goes by the following logic: if a carbon tax is implemented at a certain technological level, then this will spur clean innovation that economizes production, such that it offsets the increase in relative costs for using a dirty technology (affected by the rise in carbon tax), up the level of the tax. Theoretically, this would just give rise to one new patent[68] in one time period in which (or technically the period after) the tax was implemented, and not necessarily result in a stably higher level over all in clean innovation (because you really only need one new patent to get to the new equilibrium.) Hence, we would not necessarily expect that a real carbon tax that remains at a stable level, or even falls, would actually continue to stimulate innovation at a new equilibrium level (again, since patents are additive to the technology level). At least not until the knowledge stock and market size has become large enough in clean innovation to counter the path dependency effect in dirty technologies and shift incentives towards clean innovation (as suggested by Acemoglu et al., 2012). The fact that we cannot empirically investigate the explanations to or impact of this drop back however, is arguably a key limitation of this study.

Another limitation, highly related to the aforementioned, is the fact that although the synthetic control results are arguably robust enough that we can confidently state that the Swedish carbon tax indeed have had a positive impact on clean innovation in Sweden, which we estimate to an increase of 1.88 patents in the average year given the volatility of the measure, the exact magnitude of the effect is a bit uncertain and we would be hesitant to accept these estimates at face value. To this end, another important limitation is that estimating innovation through patent count does not capture all clean technology innovation. We focus only on high value patents in this study, which should suggest that our estimates are economically meaningful, but it is still important to keep in mind that this measure does not necessarily reflect the actual economic value of these innovations.

6.4 Case study evaluation vs. cross-country estimations

In this subsection we discuss the results of our complementary panel data regressions, in relation to the findings in our Swedish case study.

[68]Since typically you only apply to a patent once, which then gives you the right to the technology for the next 30 years or so.

First off, we employed a traditional panel data regressions method with a fixed effects estimator on the existing data available on national carbon taxes, in an attempt to see whether we could broadly estimate an effect on clean innovation from a cross-country sample. As described in the results section, the regressions using national carbon taxes as the independent variables results are statistically and economically insignificant throughout. The alternative specifications in which we use fuel taxes as a proxy of carbon taxes provide some indications of the direction of an effect of carbon taxation on climate change mitigating innovation. Nevertheless, the results are not conclusive and statistically weak throughout the majority of tested specifications. Hence, the panel data regressions conducted do not provide many interpretative results at all.

Yet, in the light of our relatively strong evidence of an effect in our case study of Sweden, these results might still provide an interesting ground for analysis. *How come that the results of our panel-data regression produce practically no support to our findings by the synthetic control method?*

There are two potential explanations to this question. Firstly, there might exist a true effect on clean innovation, which we fail to detect due to limitations of the data and empirical method. Secondly, there might not exist any true effect of carbon taxes on innovation in our sample, which is the simple explanation to why we find no effect.

If we start by focusing on the first case. As previously noticed, panel data on national carbon taxes is highly limited both in terms of availability and variation, which makes it empirically challenging to detect an effect even if there is one using a traditional panel regression method - simply because the sample size and variation is too small. Given this limitation, the synthetic control method is a more appropriate method just given the fact that there is relatively much more data available on countries that did in fact *not* implement a carbon tax (i.e. that can function as donor pool countries), and the fact that national policies, with unique national features, are typically better evaluated through a case study. Practical and technical limitations may well explain the lack of detectable effects by this method.

On the other hand, if we assume the latter case to be true, that our panel data regression model is adequate and our sample sufficient, what might then explain the fact that we find a strong and significant effect in our case study of Sweden but no effect in the panel data regressions? There is one potential explanation to this that seems quite plausible: that this result is due to the differences in the level of carbon taxes. The real Swedish carbon tax is on average more than 5 times higher than the combined average of all other countries in our sample[69], and as previously mentioned, the only carbon tax on par with the carbon pricing recommendations in line with the 2015 Paris Agreement. See figure 14 in Appendix for overview of cross-country real carbon taxes.

From previous literature, see literature review section (2) and specifically Acemoglu et al. (2012) and Aghion et al. (2016), the prevalence of path-dependency effects suggests that the level of a carbon tax is highly important in order to create a change in direction of innovation, since a moderate carbon tax may not be sufficient to shift innovation from "dirty" to "clean". If this is effect is substantial, as predicted by theory, it might plausibly both explain and support our results (and vice versa, our results would support this theory), which suggests that a relatively high carbon tax, such as in the case of Sweden, indeed contributes to foster clean innovation, while more moderate carbon taxes, as are dominant in our panel data sample (see figure 14, Appendix), produce no substantial effect at all, since a small carbon tax cannot counter the path-dependency effect that locks en economy with a relatively higher knowledge stock and market size in "dirty technologies". By this theory, our results in this thesis would imply that only carbon taxes of sufficiently high level of pricing actually contributes to spurring clean innovation, whilst moderate carbon taxation does not have a detectable effect on innovation.

6.5 Policy implications

We also want to spend a few sentences discussing the potential policy implications of this thesis. As noted in the introduction, the question of whether carbon taxes have an impact on clean technology innovation should be particularly relevant to policy-makers and especially in light of the carbon pricing targets set out in the 2015 Paris Agreement. To this end, the Swedish carbon tax is one of few climate policy initiatives that provides a real world example of a carbon pricing level that actually consistent with achieving the target temperature of the Paris agreement.

To the interest of policy-makers, our findings suggests the following: Firstly, a carbon tax of sufficiently high level can be an efficient policy-tool to stimulate clean innovation and accelerate the transition to a low-carbon economy. Secondly, a higher carbon tax likely pays off better in terms of stimulating clean innovation than does

[69] The simple average of the real Swedish carbon tax in our panel data is 65.08 in constant (1990) USD, while the simple average of all other countries' real carbon taxes together, over the whole time period in our sample, is 12.39 in constant USD. This makes the average real Swedish carbon tax 5.26 times higher than the average real carbon tax of all other countries together.

a carbon tax at a moderate level, which might even have no effect at all. This is consistent with predictions by Acemoglu et al. (2012) and findings by Aghion et al. (2016). Thirdly, our findings suggest that it is more efficient in efforts of shifting incentives for direction of innovation to act more strongly, early on in terms of climate policy.

Due to the limitations of this study, since we do not quantify the value of innovation measured, however, we cannot state anything certain about the economic magnitude of carbon taxes on clean innovation. Nevertheless, we might anticipate that the economic impact is significant, given the substantial increase in high value patents compared to the counterfactual case. This should also be an important take-away for policy-makers.

6.6 Opportunities for future research

Finally, we recognize that this study, to our knowledge, is one of the first to employ the synthetic control method to evaluate the impact of a carbon tax on clean technology innovation on a macro-level. As previously noted, there is a growing strand of literature that investigates the impact of carbon pricing policies from a theoretical standpoint or that estimates effects on a microeconomic level (of which many have investigated fuel prices as a proxy for carbon taxes rather than looking at real carbon taxes), however, there is a gap in literature that estimates the effect of existing carbon taxes on clean innovation using a general equilibrium approach. One reason the the relatively limited literature investigating the impact of real world carbon taxes on clean innovation possibly due limited availability of data. However, novel methods, such as the synthetic control method employed in this thesis, provides new and enhanced opportunities to investigate the impact from the data at hand. Furthermore, as carbon taxes are on the rise and many countries have recently implemented carbon taxes, the availability of data on carbon taxes is growing and in the next few years there should already be much broader data set available for empirical analysis.

In this study we have discussed that the existence of path-dependency effects might impact carbon taxes in such a way that levels matter, and only relatively high carbon taxes will actually shift incentives for innovation. This discussion provides two potential avenues of interesting further research. Firstly, with growing data material on real carbon taxes, there would be great merit in efforts to empirically investigate how differences *in levels* of carbon taxes impact the effect on clean innovation. Secondly, it should also be highly meaningful to empirically analyze path-dependency effects using real carbon tax data, potentially by looking at how carbon taxes impacts the substitution rates between dirty and clean technologies patented, within and economy.

Moreover, we believe that efforts to quantify the economic magnitude of clean innovation induced from carbon tax policies, would be a highly relevant subject of study for future research. Lastly, in our estimations we find that the effect of carbon taxes on clean innovation patents might not be stable over time. While this might to some extent be explained by the volatility of our measure used, patents per million capita, this is something that researchers could, with merit, study in greater detail in the future.

7 Conclusions

This thesis analyzed the impact of national carbon taxes on the development of clean innovation, as measured by climate change mitigating patents. We conduct a case study of the Swedish carbon tax, implemented in 1991, using the relatively novel synthetic control method. Through a data-driven approach, this method uses pre-treatment observations on a set of key predictor variables of clean innovation to construct a counterfactual by a weighted average of donor countries, which best matches the pre-treatment path of Sweden in terms of the outcome variable. To our knowledge this is the first study that uses this method to estimate the impact of a carbon tax on technological innovation on a macro-level.

Our results show that the Swedish carbon tax contributed to stimulating clean innovation in the 15 years after the introduction. We estimate that the Swedish carbon tax increased clean innovation by on average 1.88 patents per capita million, or 14.1% in an average year, over the post-treatment period in our sample, 1991-2005, compared to the counterfactual case. Aggregating over the population, this suggests that Sweden produced in total 249.88 more clean technology patents relative to the synthetic Sweden counterfactual in the first 15 years of the carbon tax being in effect. Reassuringly, these results are robust to a series of placebo tests.

We also employ a traditional panel data regression method to estimate the impact of carbon taxes on innovation on a cross-country sample of the 17 countries that implemented a national carbon tax during the time period 1990-2016. By this method, however, we find no significant effect of carbon taxes.[70] We discuss to potential reasons to why this might be, given that we find rather strong results of an effect from our case study of Sweden. Firstly, this might be due to the limitations of cross-country data on carbon taxes, since only few countries have implemented such taxes, the sample is small and the panel highly unbalanced. This makes it difficult to detect an effect using panel data regression methods even if there is one. The second potential explanation is that we only detect an effect in the case of Sweden because Sweden has a uniquely high tax, driving the effect. In fact, the Swedish carbon tax is on average more than 5 times higher than the average of all other sample countries' carbon taxes together. According to theory, see Acemoglu et al. (2012), and previous empirical findings, see Aghion et al. (2016), the prevalence of path-dependency effects (in dirty innovation) has a substantial

[70] We also test the same model using fuel taxes, which gives a much larger sample of 1 135 observations from 37 OECD countries in a balanced panel 1978-2016, as a proxy for carbon taxes. These results in fact give some tentative indications on a positive effect on clean innovation, but results are not significant or robust enough to take it as evidence of a true effect.

impact on the direction of innovation, and suggest, that only relatively high carbon taxes would create incentives to shift innovation, while a relatively moderate tax would make little difference.

In sum, while cross-country data shows no significant effects, our results from the Swedish experience suggest that carbon taxes that are on par with the goals of the Paris 2015 agreement indeed contribute to fostering clean innovation on a substantial scale. Arguably, our findings have meaningful policy-implications. Firstly, a sufficiently high carbon tax has a positive effect on stimulating clean innovation. Secondly, the level of the tax matters, according to our results only a carbon tax consistent with the pricing targets of the Paris 2015 agreement has a detectable effect. Altogether, our findings imply that (relatively high) carbon taxes can be an efficient policy tool to contribute in the efforts of accelerating the shift toward clean technologies and the transition to a low-carbon economy. The fact that we only detect an effect of a higher carbon tax tentatively indicates, in line with previous research, that there might be path-dependency effects, which could also explain why moderate taxes have little or no effects. This suggest that it is more efficient to act strongly in terms of policy to efficiently impact the incentives of innovation.

With this study we have tried to fill some of the knowledge gap in the field of carbon taxes' effects on innovation, however, there are still much ground to be covered in this area of research. We believe that for future studies it would be meaningful to investigate and quantify the economic magnitude of clean technologies that can be linked to carbon taxes, and the overall economic impact on the economy. Furthermore, in our study we note some time-incongruities in the effect of carbon taxes on clean innovation patents, while this might to some extent be explained by the volatility of our measure used, it might also be that the effect is not stable over time. This is something that future research could investigate further with merit. Lastly, as carbon taxation initiatives are increasing across the world, the availability of data on real carbon taxes is increasing rapidly, this should provide many opportunities for empirical studies ahead. In particular, in this study we have discussed that the effect of carbon taxes, because of path-dependency effect in the economy, might not be stable but differences in level might matter substantially. There are exciting future research opportunities exploring the use of growing data on carbon taxes to investigate the effects of differences in levels on clean innovation.

8 References

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9 Appendix

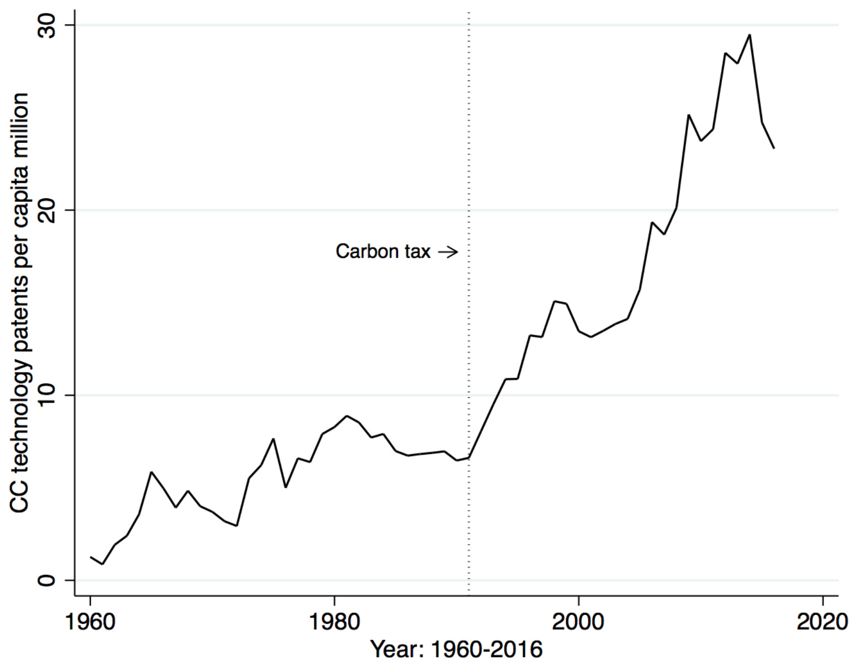

Figure 11: Sweden: CC technology patents per capita million
over full sample period 1960-2016

In figure 11 above we show the path of climate change mitigation technology patents per capita million for Sweden over 1960-2016, the full period of data available. Source: OECD Patents Indicator data (2020)

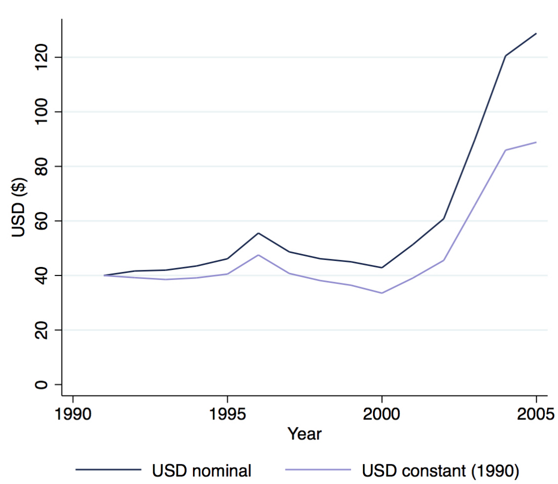

Figure 12: Sweden:
Carbon tax (USD)
1990-2005

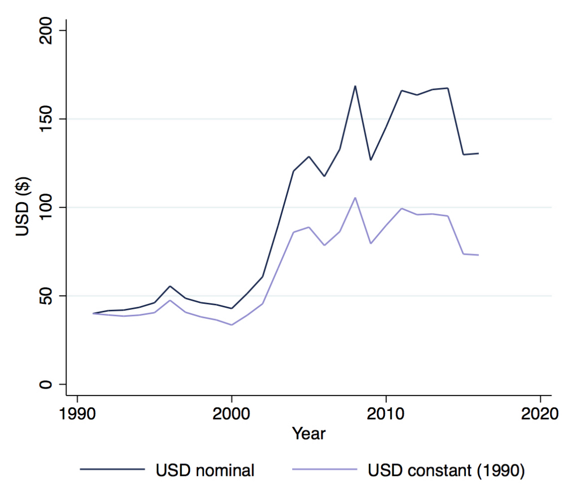

Figure 13: Sweden:
Carbon tax (USD)
1990-2016

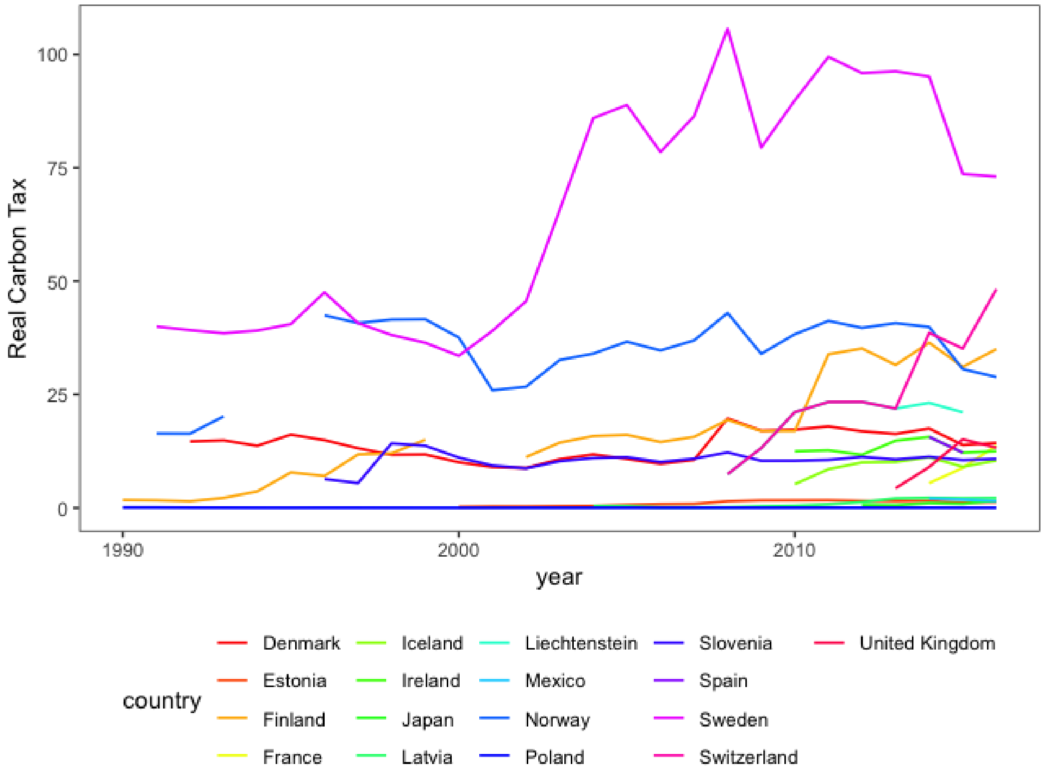

Figure 14: Real carbon tax in 17 OECD countries between 1990-2016

9.1 Synthetic control without-cross validation

We minimize the error on the whole pre-treatment period in this case. As we can see from the graph, this method produce a synthetic counterfactual to Sweden very similar to the synthetic Sweden using cross-validation. Summary statistics on predictor averages are shown below.

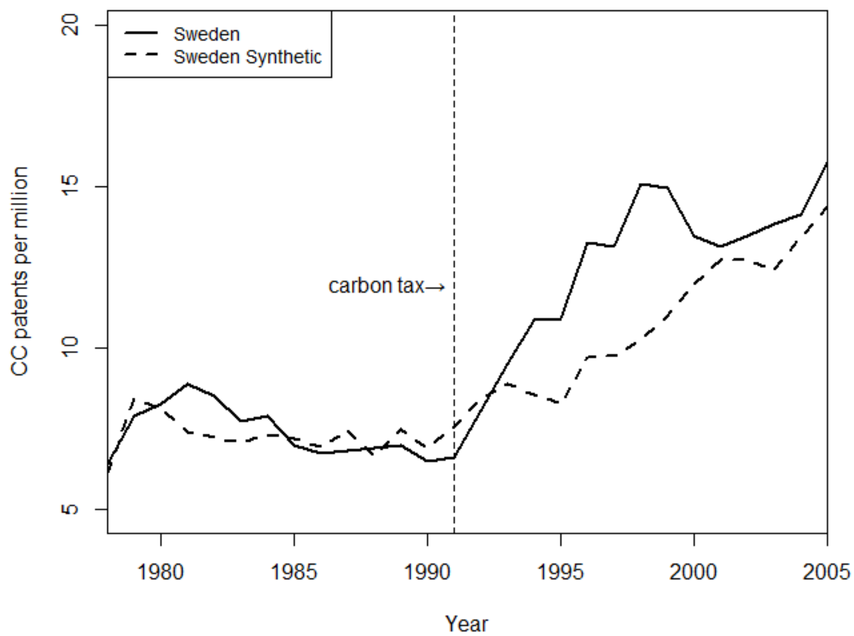

Figure 15: Path of CC technology patents per capita million between Sweden and synthetic Sweden, no cross-validation

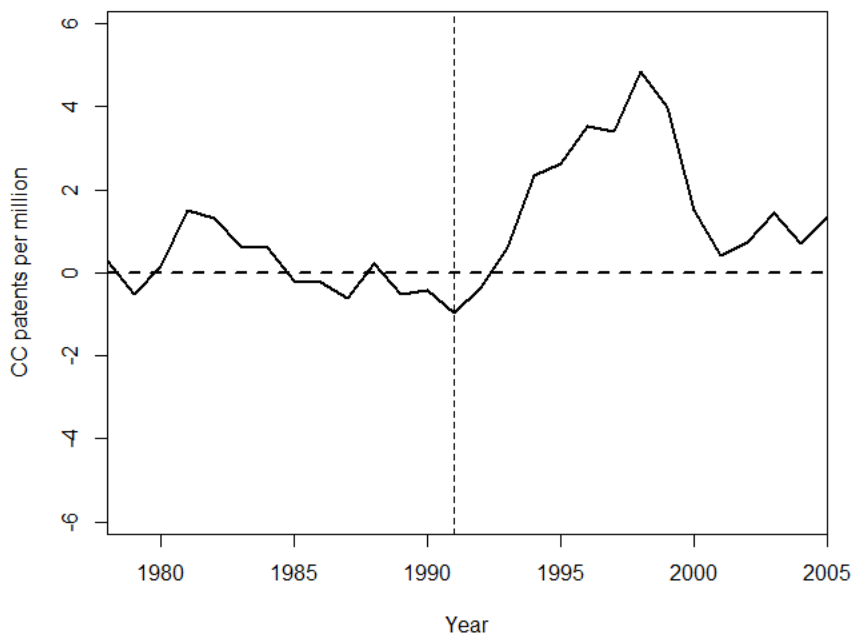

Figure 16: Gap in CC technology patents per capita million between Sweden and synthetic Sweden, no cross-validation

This graph is corresponds to figure 4, and shows the estimated gap in climate change mitigation technology patents between Sweden and synthetic Sweden using the synthetic control method without cross-validation.

Table 8: Key predictor means pre-intervention, no cross-validation

	Treated	synthetic	sample mean
GDP per capita	23650.146	24789.948	22242.449
Patents app. per capita million	439.898	381.816	344.149
Urban population share	83.089	78.717	73.716
Tax exclusive real fuel price	0.321	0.338	0.391
CC patents per capita million 1978	6.391	6.117	3.158
CC patents per capita million 1984	7.911	7.312	4.388
CC patents per capita million 1990	6.472	6.894	4.597

Above table shows average values on key predictors for Sweden, synthetic Sweden (optimized over the full pre-treatment period, i.e. no cross-validation) and the donor pool sample mean. Even with this method, we find a closer match on predictor values between Sweden and synthetic Sweden as compared to the donor country sample mean. Particularly, we note that the donor country sample mean is substantial lower on our three lagged values of the outcome variable compared to the synthetic control.

Table 9: Predictor weights V^*, no cross-validation

Variable	V^*
GDP per capita	0.207
Patent app. per capita million	0.000
Urban population share	0.051
Tax-exclusive real fuel price	0.215
CC patents per capita million (1978)	0.157
CC patents per capita million (1984)	0.262
CC patents per capita million (1990)	0.108

Table 10: Country Weights W^*, no cross-validation

Weight	Country	Country Number
0.270	Germany	6
0.250	France	5
0.242	Switzerland	14
0.231	Belgium	3
0.000	Australia	1
0.000	Austria	2
0.000	Canada	4
0.000	Greece	7
0.000	Ireland	8
0.000	Italy	9
0.000	Japan	10
0.000	Netherlands	11
0.000	Spain	12
0.000	United Kingdom	15
0.000	United States	16

In this table we see the country-weights produced by the synthetic control method without the cross-validation feature. While Germany, France and Switzerland still dominates the weights, similarly to the cross-validation approach, we note that Australia gets no weight in this specification whilst Belgium takes a rather large weight. This is arguably not worrying since it fundamentally does not change any results and also, it is not completely unexpected that we get a slightly different result when we optimize over a different period.