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Renewable Portfolio Standards: A Double-Edged Sword?

Impact on State-Level GDP in the United States

Iraa Dahiya (42448) & Arjun Vasant Kumar (42439)

Abstract: Renewable Portfolio Standards (RPSs) are major state-level policies present in the US. We examine the impact of RPSs on state-level Gross Domestic Product (GDP) per capita, focusing on the complexities introduced by their staggered and heterogeneous implementation. Due to the endogenous nature of RPS adoption influenced by unique state characteristics, there are significant challenges in using traditional econometric methods. We, thus, employ the novel Synthetic Difference-in-Differences (SDID) approach,to address the non-random adoption of these policies. Overall, our results signify no loss of economic value by adopting RPSs. But we do find weak evidence confirming that, on average, these policies increase GDP per capita by USD 3092 over the period of our analysis, 1990-2022. This research contributes to the understanding of the efficacy of RPS programs and informs policy-making in the realm of state-level renewable energy initiatives.

Keywords: Renewable Portfolio Standards (RPS), State-level GDP, Synthetic Difference-in-Differences, Environmental Policy Impact, Energy Economics, Sustainable Development, Panel Data Analysis, Economic Activity, Greenhouse Gas Emissions, Electricity Prices.
JEL Classification: Q48, Q54, C23, R11

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Contents

1 Introduction

The long-standing debate on sustainable economic growth is becoming more and more relevant with an increased focus on achieving carbon neutrality and net-zero emissions. In a bid to reach these goals, a slew of environmental policies have been adopted over the past sixty years, e.g., the Clean Air Act, and the National Environmental Policy Act. RPSs take centre stage in supporting the federal government's endeavours, as they play a large part in driving the growth of renewables in the US (Natural Resources Defense Council (2013)). In 1991, RPSs were introduced as state-level environmental policies to de-carbonise the electricity sector. Since the 1990s, an increasing number of states have been adopting and investing in RPSs, with the standards now being present in over 30 states.

One of the key goals of the RPS policies is to promote economic development (NCSL (2021)). However, as with other environmental policies, over the years, the critics of RPS, including politicians and interest groups have raised concerns that the introduction of green policies such as the RPS hinders economic growth. Commonly cited reasons by these opponents include increased costs for businesses, reductions in the competitiveness of local firms, job loss, and misallocation of resources. The increasing significance of the RPS on the road towards a green electricity sector[1] and demands of national level standards, make it essential to understand its effects on economic growth, amongst the other goals of the policy.

So, the key question of our analysis is whether RPSs deliver on one of their key promises of promoting economic growth. There exists a significant gap in prior literature since relatively few studies estimate the impacts of the policy on economic activity. Some have focused on the effects of RPS implementation on growth and employment only in specific sectors, e.g., the manufacturing industry while others focus on the effects on the green industry (Bowen et al. (2013)) and not the overall effects. Therefore, our paper significantly adds to this gap and contributes to the growing literature supporting the employment of environmental policies. This paper will explore the effects of RPS on state-level GDP and additionally, will focus on analysing whether these standards hurt the economy, as suggested by the opponents of such policies.

RPS can affect state GDP through multiple direct and indirect channels such as effects on employment, electricity prices, and emissions. It is vital to understand these channels as it is of pertinence to policymakers in the field of environmental economics. An in-depth expansion, along with the existing literature on RPS and other relevant environmental policies is presented in the following section.

The other goals of this policy are to diversify the energy mix and reduce greenhouse (GHG) emissions from the power sector, especially the carbon emissions. There are multiple studies

[1] According to the Environmental Protection Agency, the electricity sector is among the top contributors to man-made greenhouse gas emissions in the US.

about the causal impacts of RPS programs on these other goals - share of renewables, renewable capacity investments, and carbon emissions.

The inherent design of RPS programs makes it difficult to definitively perform a causal inference study. The RPS policies are not randomly adopted by states, and multiple drivers of RPS adoption have been identified by prior literature (Lyon and Yin (2010), Upton and Snyder (2015)). The underlying structure of the industrial sector, political beliefs and renewable energy potential have been identified as major reasons for RPS implementation. For instance, Thombs and Jorgenson (2020) find that only moderate and liberal governments adopt RPSs in states that depend heavily on non-renewable industries. Secondly, there is significant variation in state-level RPS policy design including differences in the scope, as well as the timing at which states adopt these policies. Therefore, it was essential to account for this staggered adoption and the heterogeneity in treatment while designing our empirical strategy.

While standard fixed effects models are the most widely used empirical methods to analyse RPS, recent econometric literature has shown that such estimates can be biased in the presence of heterogeneous treatments(Goodman-Bacon (2021), Sun and Abraham (2021), de Chaisemartin and D'Haultfœuille (2020)). To account for these state-specific characteristics, and staggered adoption, we use the Callaway and Sant'Anna (2021) estimator. However, given multiple unobservables that can drive RPS adoption, very few papers account for this endogeneity in treatment. Upton and Snyder (2017) use the Synthetic Control Method (SCM) to assess the impacts of RPSs on other goals, such as emissions, and electricity prices.

This study addresses these inherent complexities of RPSs through the novel method, Synthetic Difference-in-Differences (SDID) as our main econometric model, which allows for greater flexibility than the SCM. A newly developed method, the Synthetic Difference-in-Differences approach has been used for comparative policy analysis in fewer instances, e.g.,.Zhang et al. (2023), and Bernhardt et al. (2023), relative to the series of works including DID, SCM methods, and their augmentations. Specifically, even fewer works use the Staggered Adoption Design developed by Arkhangelsky et al. (2021). We contribute methodologically by showing the advantages of the technique (discussed in section 4) and the precision of its estimates. To the best of our knowledge, this paper is the first to apply the novel SDID method to examine the effects of the RPS policy in the US.

Our results indicate a positive effect that is statistically significant at the 10% level of significance. However, we err on the side of caution and do not interpret them causally as a positive effect on GDP. We conclude that there is no overall average effect of RPS on the state-level GDP per capita, and therefore, no loss of economic value (in GDP terms) takes place due to the adoption of this key environmental policy by various states in the US.

The rest of the paper is organized as follows. In Section 2, we provide key details on the RPS programs. Section 3 provides a review of prior literature on the effects of RPS and the potential channels through which environmental policies such as the RPS affect GDP. Section 4 provides details about the data used for our empirical analysis and the sources of these data. Section 5 outlines our empirical strategy. Section 6 presents our results and Section 7 includes robustness checks for our analysis. Section 8 further explains these results by discussing the effects of RPS on our outcome variable through other indirect channels. Finally, Section 9 follows with the conclusion.

2 Details on Renewable Portfolio Standards

RPSs are state-level policies that mandate in-state electric utilities to achieve a minimum percentage target of total retail electricity sales through eligible renewable energy sources. 31 states and the District of Columbia (DC) have adopted it so far, and two states have repealed the program. Figure 1 and Table 1 show the states having RPS targets, and the timeline of RPS adoption, respectively. Among the twenty-nine states and DC that have an RPS in place, sixteen states have RPS targets of at least 50% of retail sales, and 17 states have a 100% Clean Energy Standards (CESs) or RPS target to be achieved by 2050 (Barbose (2023)). The structure of these programs varies significantly across the states. Out of the 29 states that have RPSs, except for two states, the rest of them have percentage targets or goals for electricity generation through renewables to be achieved within specific timelines.

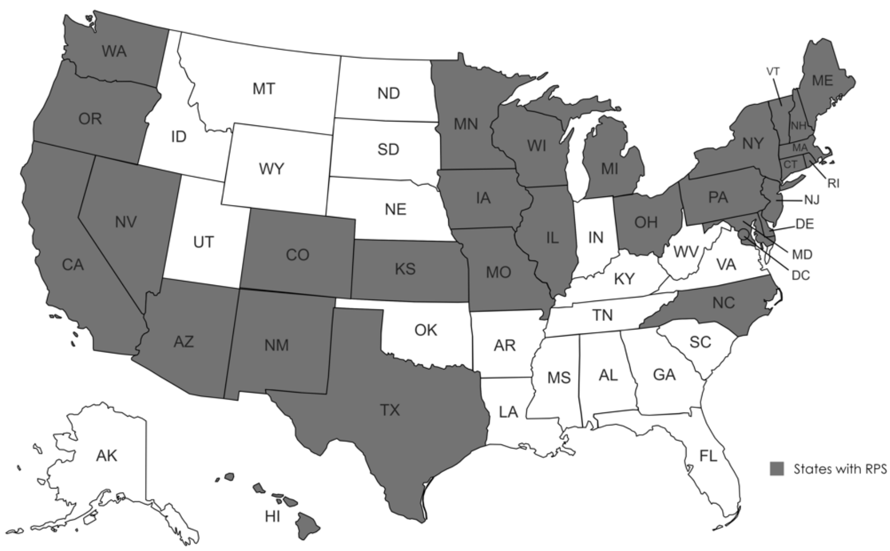


Figure 1: States with RPS Targets

Two features of these programs that make this an interesting choice of policy to study. First, there has been a staggered adoption across the states, with Iowa adopting it in 1983 (mandated in 1991) and Vermont introducing these standards in 2015. Second, the intensity, more formally defined as the stringency, of RPSs, measured by the magnitude of the percentage targets, varies across all states accounting for the heterogeneous impacts of the policy.

As environmental policymaking has progressed over the years, states have now started to implement CESs. CESs have a broader scope and can encompass all the clean energy sources which do not come under the definition of renewables, such as nuclear power and other Carbon Capture and Storage (CCS) technologies. In the context of RPSs, if states also have a CES policy, it will include the RPS goals as part of the requirement.

It should be noted that for a specific state, utilities generate, as well as buy power from other power generators, which can be situated in or out-of-state. Additionally, each megawatt-hour (MWh) of electricity generated from renewable sources results in the issuance of a Renewable Energy Credit (REC). These RECs can be purchased by utilities without the underlying electrical power associated with them, adding another layer of complexity to these policies. This interstate trading of RECs can introduce a measurement error in terms of measuring the actual in-state renewable electricity generation and the state-level emissions. Due to the added complexities introduced by interstate REC trading, it is out of the scope of this study. Feldman and Levinson (2023) and Hollingsworth and Rudik (2019) focus on studying the effects of RPS on neighbouring states and explain this issue, reporting the effects of RPSs on renewables' growth and emissions.

Table 1: Timeline of RPS Adoption

State	Year of Adoption	State	Year of Adoption
Iowa	1983	Maryland	2004
Massachusetts	1997	Pennsylvania	2004
Nevada	1997	Rhode Island	2004
Minnesota	1997	Delaware	2004
Connecticut	1999	Illinois	2005
Maine	1999	Montana	2005
Texas	1999	Washington	2006
Wisconsin	1999	Oregon	2007
Arizona	2001	North Carolina	2007
New Jersey	2001	New Hampshire	2007
California	2002	Michigan	2008
New Mexico	2002	Ohio	2008
Colorado	2004	Missouri	2008
Hawaii	2004	Kansas	2009
New York	2004	West Virginia	2009
		Vermont	2015

Notes: DC adopted RPS in 2005. Iowa's standards became mandatory only in 1991. West Virginia and Montana repealed their RPS policies in 2015 and 2021 respectively.

3 Literature Review

While adopting renewable portfolio standards, public authorities have aimed to achieve the following key goals: to increase renewable energy capacity, reduce GHG emissions, and promote economic development. There have been numerous studies examining the efficacy of RPSs on a multitude of these outcome variables including renewable energy capacity, emissions, and electricity prices. Very few studies have looked into the impacts of these policies on economic growth and whether or not, they succeed in achieving the overarching goals of all environmental policies,i.e., sustainable development. We aim to bridge this gap in the literature and study how adopting RPSs affects the GDP of a state. In addition, in the following subsection, we also review the effects of other market-based environmental policies on varied economic outcomes to gauge the potential channels through which RPSs affect state GDP.

3.1 Review of the Effects of RPS

Barbose et al. (2015) summarise earlier research which has mostly relied on standard Ordinary Least Squares (OLS) and Difference-in-Differences (DiD) methods. They also examine the overall costs of RPS and are one of the first to quantify the benefits of RPS adoption in the US. However, subsequent studies highlight the need for more sophisticated econometric approaches due to the staggered nature of RPS adoption and the variability in how states implement these policies. Recognizing these methodological challenges is crucial, as they influence our understanding of RPS's broad implications. Below, we shortly outline the four main dimensions of the RPS policies, i.e., their effects on renewable energy generation, electricity prices, GHG emissions, and most relevant for us, economic activity.

One of the most pertinent questions that have been asked of these policies is whether they have any effect on the share of renewable energy generation in the US. The literature provides mixed results on this. Menz and Vachon (2006) find that the presence of RPS policies in states strongly supports higher wind generation capacity. While Carley (2009) uses a variant of the standard fixed effects model and finds that it is not just RPS adoption but that states which have had the policy implemented for longer periods, see a statistically significant increase in green energy capacities. Maguire and Munasib (2016) account for policy heterogeneity and perform an analysis on renewable capacity development in early adopter states. They report a significant positive increase in capacity in the state of Texas. Yin and Powers (2010) include the relevant stringency of RPS states in their fixed effects analysis and thus, accommodate incremental heterogeneous treatments in their analysis. They find that states with more ambitious RPS targets have higher renewable generation capacity.

However, recently, more sophisticated empirical methods used by researchers find relatively smaller or no impacts of the policy on renewable generation capacity. Feldman and Levinson

(2023) expand on Yin and Powers (2010) and Greenstone and Nath (2020), taking into account the effects of interstate REC trading and endogenous nonadditionality - misattributing increase in renewable capacity (that was in the pipeline to be developed anyway), to RPS compliance. Their analysis uses an instrumental variable and they find marginal increases in renewable energy generation due to RPS compliance.

Deschenes et al. (2023) study the role of heterogeneity of RPS using the staggered difference-in-differences estimator developed by Callaway and Sant'Anna (2021). In addition, they highlight compositional differences between RPS and non-RPS states and use not yet treated RPS states as their control group. Contrary to Feldman and Levinson (2023), they find significant increases in wind energy generation and no such effects on solar energy capacity. Upton and Snyder (2017) focus on these compositional differences as drivers of RPS adoption and take into account the likely endogenous adoption of RPS by states by using the synthetic control method. They estimate no significant evidence of higher renewable generation in RPS states as compared to non-RPS states, as has been found in prior research by Shrimali et al. (2015).

Many academicians have also focused on the impacts of RPSs on the electricity markets of the US. Palmer and Burtraw (2005) estimated that more ambitious RPS targets in the future, requiring 20% or more renewable generation can increase electricity prices by 8%. While Barbose et al. (2015) find that RPS compliance costs were lesser than 2% of the average retail electricity rates, Greenstone and Nath (2020) exploit the variation in the timing of RPS adoption and find that due to mechanisms such as indirect grid integration costs, electricity prices increase by 11% on average. Similarly, the Upton and Snyder (2017) findings suggest that RPS states experience an increase of around 11% in electricity prices and a decrease of 7.5% in electricity demand compared to their synthetic non-RPS states. Others find smaller size effects, for instance, Heeter et al. (2014) estimate that electricity prices increase by 2-4% following RPS adoption, in their report sponsored by the National Renewable Energy Laboratory (NREL) of the US.

There exists sizeable literature on other variables of interest such as carbon emissions. Sekar and Sohngen (2014) find that RPS states have 30% less carbon emissions per unit of state GDP on average. More recently, Feldman and Levinson (2023) estimate a significant effect on the reduction of carbon emissions. Greenstone and Nath (2020), using their DiD design, estimate the impacts of RPS on electricity prices and carbon emissions. They conclude that RPSs reduce carbon emissions by up to 25%. Their paper contributes to the growing literature which generally finds that policies that fail to directly target carbon emissions tend to be expensive on a cost per ton basis. Upton and Snyder (2017), too find similar decreases in carbon emissions in RPS states and interestingly, attribute this to decreases in electricity demand and not renewable generation.

While there has been an extensive use of more modern methods assessing the impacts of RPSs on the outcome variables mentioned above, the literature closest to us, assessing the impacts of

RPS on economic activities, remains largely unexplored. Some however, do examine the topic at hand. Barbose et al. (2015) highlight that individual state bodies have quantified the impacts of RPS on various economic outcomes using varied methods including case-study approaches, input-output models, computable general equilibrium models and even some empirical methods. However, these studies often focus on gross impacts (such as on employment) and not the more detail-oriented net impacts, the studies may also quantify relationships between sectors at a point in time but do not incorporate time variant covariates, such as electricity prices. These studies find mixed results, where in some cases, millions of dollars of annual economic benefits accrue over a project's lifetime while in others, state GDP has been found to decrease by 1% due to rising electricity prices. Barbose et al. (2016) use the NREL's JEDI suite of models and found that RPS compliance in 2013 contributed to a generation of approximately 200,000 gross jobs in the construction and operations sectors.

Bowen et al. (2013) use a time series specification to examine the effects on green economic growth and the results are mixed. They find no discernible effect of RPSs on green jobs but that the persistence of RPSs through time causes an increase in the number of green businesses. Wolverton et al. (2022) use plant level data to assess how state level RPS policies affect the manufacturing sector. They find that a typical RPS policy reduces industrial electricity use by 1.2% - 1.5% while plant-specific output, employment and working hours decline by a smaller magnitude at roughly 0.15% - 0.2%. Greenstone and Nath (2020) find similar negative effects on employment in the manufacturing industry due to RPS implementation, however, their results are statistically insignificant.

Since, there have been mixed results on the effects of RPSs on economic outcomes and there has been limited use of modern empirical methods, our analysis contributes significantly by being one of the few to assess the direct impacts of RPS on state GDP. More importantly, we extend the use of modern econometric methods to study such effects by using the novel Synthetic difference in differences approach. Therefore, our analysis, combines the critical question on the impacts of RPS on state GDP with advanced empirical methods which accommodate the layered complexities of the policy and enable us to provide a novel insight.

3.2 Review of Environmental Policies

More generally, our thesis is also related to the literature addressing the effects of other environmental policies on economic activity. This body of literature provides a framework for understanding how market-based environmental interventions can affect economic variables like state GDP. While some argue that policies like taxes and subsidies disrupt market equilibrium, environmental economists suggest that these tools are essential for internalizing environmental externalities, thereby helping markets to achieve a more 'true' equilibrium that reflects environmental costs and benefits.

Very few countries have adopted carbon taxes and particularly in the context of US, there is intense scrutiny in terms of its economic and distributional effects. Conefrey et al. (2013) analyze the medium-term effects of a carbon tax on economic growth and emissions in Ireland. They find that most of the effects on the economy is due to changes in the competitiveness of the manufacturing and market services sectors. Martin et al. (2014) evaluated effects of carbon tax in the UK on employment, revenue and plant exits and found no adverse effects. Yamazaki (2017) perform an analysis on the revenue neutral carbon tax in British Columbia, Canada and they associate this tax with a small increase in employment.

There has been an intense debate on the environmental costs of regulation and widespread studies have estimated that manufacturers contend with exorbitant expenditures to comply with environmental regulations. Greenstone (2002) estimates the impacts of the Clean Air Act on industrial activity, particularly manufacturing. He finds that in the first fifteen years when Clean Air Act was in force, polluting industries lost jobs, capital stock, and output. Another study (Walker (2011)) examined the 1990 amendments of the Clean Air Act and the findings show that it caused regulated sectors to shrink by 15% over ten years.

In the jobs versus environment debate, Berman and Bui (2001) study air quality regulation in the US and they find that it significantly affects the productivity of oil refineries and there is a positive effect.

Although, there is significantly increasing evidence of well-designed policies aiding in economic growth in terms of green jobs and innovation. Overall, the literature presents a mixed record of results on the relationship between the environment and the economy. This is because environmental policies such as the RPS, affect a state's GDP through multiple direct and indirect channels. These channels are essential to understand, since they are directly relevant for state administrators who are considering expanding or reducing their respective RPS targets. Moreover, these opposing channels also affect our results through multiple ways.

Some of the reasons why RPSs have mixed impacts on economic outcomes is because although these policies are designed to increase renewable energy capacity, they often lead to rising electricity prices alongside, as discussed earlier. RPSs stimulate innovations in the green industry and the growth of renewable sectors drives investments into this sector and increases employment in the construction, operation and management of such industries. This transition also comes at high initial set-up costs and potential job losses due to reduction in traditional fossil-fuel based industries. Renewable industries are known to have higher costs in comparison to traditional industries, in general.

Rising electricity prices can in addition, lead to lower electricity demand as seen by Upton and Snyder (2017), which can lead to declining productivity. High electricity prices increase the costs of running businesses, although such costs can be eventually passed onto consumers, and potentially affect their disposable incomes. These effects on the electricity markets, however, depend on local energy markets and how easily newer technologies can be integrated into local electricity grids of a particular region (Barbose et al. (2015)). On the other hand, RPSs have been proven to reduce GHG emissions significantly and can thus, lead to varied health benefits in terms of increased mortality and improved morbidity rates. This means higher working hours, lower individual costs of environmental damage and, thus improved social welfare. Please refer to section 8 for a detailed discussion of these opposing channels and how they can potentially affect a state's GDP.

Finally, our contribution to the existing vast body of literature on RPS is primarily two-fold. First, we aim to estimate the direct economic impacts of RPS by doing a state level assessment, a largely unexplored area in the current literature on RPS. Second, we contribute in the research methodology being used for the analysis of this policy. Most of the earlier literature has used standard fixed effects or difference-in-differences methods for this estimation, which ignores integral features of the RPS policy such as the staggered adoption of the policy as well as the relative heterogeneity in RPS targets. We show our results using the standard fixed effects approach in the later sections as well as the more advanced staggered DiD estimator, designed by Callaway and Sant'Anna (2021). However, the synthetic difference-in-differences method allows us to additionally factor in the endogenous adoption of RPS policies by state and thus, seems to be a more appropriate method for studying this policy.

4 Data

In order to estimate the impact of the adoption of RPS on state-level GDP per capita (alternatively also referred to as the per capita Gross State Product or GSP), we compile a state-level panel data set covering the period 1990-2022. Since first being adopted by Iowa in 1991, 30 U.S. states and the District of Columbia adopted the policy in various years (as depicted by Figure 2), forming our 'treated' units with the remaining states being our 'control' units.[2] We exclude West Virginia and Montana from our analysis since these states repealed their Renewable Portfolio Standards' policies in 2015 and 2021 respectively.[3]

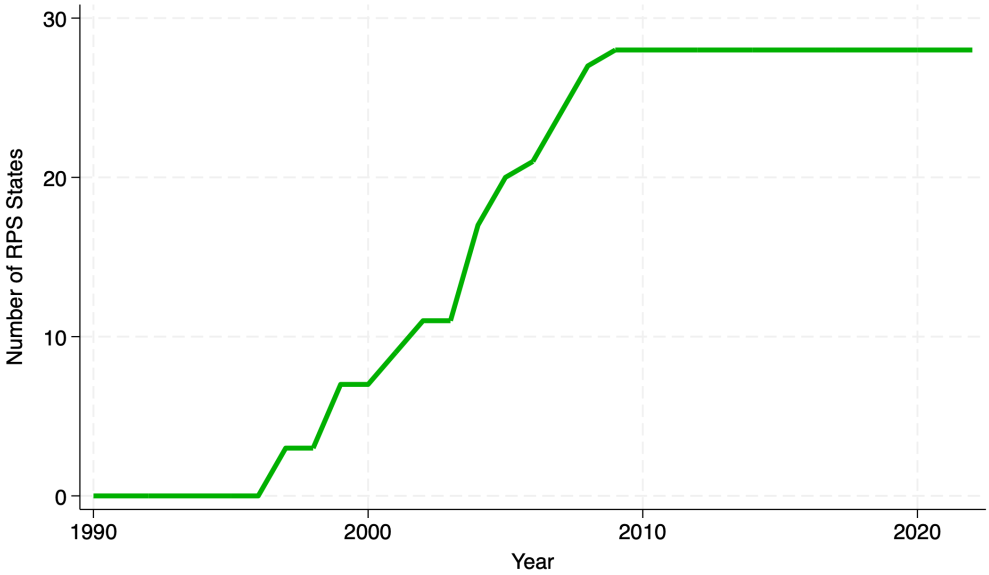
The dataset includes annual GDP per capita for each state as our outcome variable. Our dataset also includes state-level indicators for 'Natural Resource Endowments', 'Industrial Structure' and 'Political Indicators' as covariates as well as policy variables detailing the adoption and stringency of RPS. 'Natural Resource Endowments' includes data for Solar and Wind Energy Potential for each state, Natural Gas deposits per state and the production of Crude Oil per state. Data on Share of Manufacturing in a state's GDP proxies its industrial structure. LCV scores[4] at the House and Senate level have been categorized as political indicators.

[2]For the rest of our paper, we denote 'treated' units as RPS States and 'never treated' units as Non RPS states.

[3]We also exclude Alaska from our dataset since the geographical composition and other relevant factors prevalent in Alaska differ significantly from the rest of the states included in our study.

[4]The LCV (League of Conservation Voters) is an average score based on the environmental voting records

Figure 2: Number of states adopting RPS by Year



Note: The graph only includes states which remain treated once they adopt RPS between 1990-2022 and are included in our 'treated' units.

We now, briefly discuss the construction and accumulation of our dataset and provide descriptive figures and statistics. A detailed table including the comprehensive list of data sources has been included in the Appendix.

Outcome Variable

For our dependent variable State level GDP per capita, we collected State GDP data from the publicly available Regional Economic Accounts of the U.S. Bureau of Economic Analysis (BEA) and state level population data from the United States Census Bureau. The state GDP has been compiled using three datasets from the U.S. BEA's data archives, with the first including State GDP values (chained in USD 2000 values) covering periods 1990-1996. The second dataset covers the periods 1997-2016 and is chained in USD 2012 values. While, the third dataset covers the rest of our study period and is chained in USD 2017 values.[5] We adjust and make our GDP data comparable through the length of our analysis by chaining all state level GDP values in USD 2017 values. For these adjustments, we use annualised national Urban CPI data collected from the U.S. Bureau of Labor Statistics and national level inflation rates from the World Bank.

Furthermore, due to unavailability of annual state level population data going back to 1990, we use the Decennial Census on Population and Housing and annualised this until 2022 by the

for each congress member of a state, both at the senate and house levels. A higher score indicates that a state's senate and house are more supportive of legislation aimed at protecting and preserving the environment.

[5] The BEA's data on State GDP is based on the North American Industry Classification System (NAICS) beginning from 1998, while estimates prior to 1998 are based on the Standard Industrial Classification (SIC) System. The NAICS improves on SIC by more consistently classifying establishments and recognising newer, upcoming industries. We note that this difference in estimation might lead to some discrepancies in our State GDP data.

process of linear interpolation. Finally, we constructed our outcome variable, State GDP per capita (in USD thousands) normalised per 1000 residents.

Independent Variables

Renewable Portfolio Standards: We measure the presence of RPS in a state by a dummy variable which equals one if a state had adopted the policy in a given year. We then measure the duration of RPS in a state, computed as the difference between the first year of adoption and the last period of our analysis, 2022. While adopting the RPS, each state decides on the mandated share of electricity sales to be covered by its standard while setting its RPS target. We measure the stringency or ambition of a state's RPS policy using "yearly fractional goals of RPS" collected from the Lawrence Berkeley National Laboratory data repository, supplemented by state level documentation as needed. [6] The Stringency variable captures the heterogeneity in each state's target standards while adopting the RPS and helps us identify whether having more ambitious RPS targets has a larger impact, if any on the state's GDP. Figure 10 in the appendix depicts average RPS stringency increasing by year for treated states.

Natural Resource Endowments: In order to account for the variation in the naturally occurring availability and therefore, renewable electricity expansion potential for each state, we use data on solar and wind potential from the U.S. Department of Energy's National Renewable Energy Laboratory (NREL). The solar energy potential is measured as solar irradiation in $kWh/m^2{\cdot}day$ and the wind energy potential is measured in terms of the wind speed in m/s. We also employ natural gas deposits and state-specific crude oil production as proxies to assess a state's dependence on non-renewable energy sources for electricity generation. Data sources include the yearly annual figures from the U.S. Energy Information Administration and measurement units for crude oil reserves and natural gas reserves are crude oil production in thousand barrels per day and proved reserves of dry natural gas in billion cubic feet, respectively.

Industrial Structure: The industrial structure of a state, especially the presence of a variety of industries and the states dependence on them affects the state's capital base and thereby, influences economic growth (Bauer et al. (2006)). Therefore, we include the share of manufacturing (in percent) industries of total Gross State Product for each state. This data has been collected from the U.S. BEA.

Political Indicators: To address variations in state legislative actions on environmental policies like RPS, we utilise the LCV score index, which gauges the propensity of a state's government to support environmental legislation in both the House and Senate. Data for LCV scores are sourced from the League of Conservation Voters National Environmental Scorecard and are measured on a scale of 1-100.

Table 2 presents descriptive and inferential statistics for the year 1990 comparing pre-treatment characteristics for RPS versus Non RPS states. Evidently, RPS states differ from Non RPS

[6] For periods prior to 2000, we supplement this data using state reports as well documentation published by the Database of State Incentives for Renewables and Efficiency (DSIRE) on state level RPS policies.

states on a multitude of these indicators, with the average GDP per capita in 1990 being higher by almost ten thousand dollars in RPS states.

Table 2: Inferential Statistics by RPS Adoption

	RPS		Non RPS	
	Mean	Std. Dev.	Mean	Std. Dev.
A. Outcome Variable				
GDP per Capita	45.96	16.07	36.86	5.91
Share of Renewables in Electricity Generation	0.17	0.22	0.15	0.23
B. Natural Resource Endowments				
Solar Energy Potential	4.94	0.96	5.08	0.43
Wind Energy Potential	6.62	1.96	5.77	1.44
Crude Oil Production	110.4	368.29	77.11	123.18
Natural Gas Reserves	4580.93	15059.76	3323.78	6579.93
C. Industrial Structure				
Share of Manufacturing	0.18	0.07	0.18	0.07
D. Political Indicators				
House LCV Score	61.33	21.18	41.28	17.46
Senate LCV Score	61.37	24.41	33.06	22.61

Note: All statistics are from the year 1990. 'RPS' includes states which have adopted the RPS policy between 1990-2022 and 'Non RPS' includes states which have not adopted any RPS regulation. 'Crude Oil Production' and Natural Gas Reserves' are not present in all U.S. states and therefore, have larger standard deviations.

As anticipated, RPS adopting states have significantly higher LCV scores at both house and senate levels, reflecting the state governments' higher willingness to adopt policies such as RPS. As mentioned in previous sections, prior literature (Lyon and Yin (2010), Upton and Snyder (2015), Barbose et al. (2016)) establishes multiple economic, political and industrial factors as drivers of the adoption and expansion of RPS and therefore, it is considered that the decision of adopting the RPS policy by a state to is non-random. Our empirical strategy, as discussed in the next section, accommodates this non-random adoption of RPS by using the novel synthetic difference-in-differences method.

In terms of similarities in characteristics, both groups have a similar share of renewables in their electricity generation mix and are also similar solar and wind energy potential, prior to treatment. Interestingly, RPS adopting states on average, have higher crude oil and natural gas reserves than non-RPS states, albeit with a wide range of variation within the two groups due to the resources' natural sporadic presence.

5 Empirical Strategy

The RPS policy has been adopted by states at different times between 1990-2015 and the target goals of each RPS state (measures by stringency) also differ per state as described in prior sections. In addition, the adoption of the policy by a state is endogenous to a multitude of factors. Therefore, to assess the impacts of RPS on state GDP per capita, we need to account for this non-random staggered adoption and heterogeneous treatments in our research design. In this section, we set out our empirical strategy which accommodates the specific challenges posed by the RPS policy step-by-step. We use upcoming staggered adoption and heterogeneous treatment models developed by Callaway and Sant'Anna (2021) as well as the novel Synthetic Difference-in-Differences approach, developed by Arkhangelsky et al. (2021) to account for non-random adoption.

5.1 Two Way Fixed Effects Approach

Prior literature has usually depended on a difference in differences (DiD) approach using state and year fixed effects (Shrimali et al. (2015), Greenstone and Nath (2020), Lyon and Yin (2010)) to estimate the impact of RPS policies on various outcome variables. The canonical regression equation for such Two-Way Fixed Effects (TWFE) models are:

$$y_{it} = \alpha + \beta RPS_{it} + \gamma X_{it} + \delta_t + \lambda_i + \epsilon_{it} \tag{1}$$

where, in our case, y_{it} represents the outcome of interest, GDP per capita in state i at time t, RPS_{it} is a binary variable indicating the adoption of RPS policy, X_{it} represents state-specific control variables as described in the previous section and δ_t and λ_i are time and state fixed effects respectively. The average treatment effect on the treated (ATT), captured by the coefficient β shows the impact of the RPS policy on the outcome variable, GDP per capita, given all necessary assumptions hold. We also consider a dynamic extension of the static two-way fixed effects model since the impacts of the policy are likely to appear gradually, given the time needed to construct and operate renewable electricity generation facilities. We report our results using this standard two-way fixed effects approach in the following section in Table 3.

We also estimate a baseline specification using the TWFE approach described by the following equation. This estimation includes an interaction between the dummy variable "RPS" and the parameter "$Duration_{it}$" following from Upton and Snyder (2017) and extends their analysis by including an interaction term $Stringency_{it}$ with the dummy variable.

$$Y_{it} = \alpha + \beta(RPS \times Duration_{it}) + \gamma(RPS \times Stringency_{it}) + \delta_1 D_i + \delta_2 D_t + \epsilon_{it} \quad (2)$$

Here, D_i and D_t are state and time fixed effects. The baseline specification simply investigates whether the length of exposure to treatment and stringency of treatment have an impact on state GDP and do not account for the complexities of the RPS policy described above. Thus, this specification is simply a descriptive exercise and reflects the effects of the intensity of treatment, please refer to Table 10.1.2 in the appendix for the results.

Recently, a vast body of econometric literature has gained popularity pointing out flaws in the standard two-way fixed effects when treatments are assigned in a staggered manner and/or are non-uniform across treated units. In our case, this corresponds to the staggered adoption of RPS policies as well as state-by-state differences in the overall goals of the adopted policies. For example, in 2018, California's RPS target was set to 100% by 2045 whereas, Delaware's RPS target was at 50% by 2040. The standard TWFE approach estimates a weighted average of group-time treatment effects, which might not correspond to the overall ATT (de Chaisemartin and D'Haultfœuille (2020), Sun and Abraham (2021), Callaway and Sant'Anna (2021), Goodman-Bacon (2021)).

Goodman-Bacon (2021) proves this by decomposing the standard TWFE coefficient $\hat{\beta}$ as a weighted average of $\hat{\beta}$ coefficients estimated using multiple 2x2 comparisons present in a staggered setting. This is because, the traditional TWFE estimator makes "forbidden comparisons" between late treated and early treated groups and therefore, produces a biased estimate. We use this decomposition to check for the comparisons made by our TWFE estimates presented in Table 3 following Goodman-Bacon (2021) where we get estimated $\hat{\beta}$ decomposed by treatment timing, i.e., we get a $\hat{\beta}$ coefficient, each for the comparisons between non-RPS states and RPS states and between RPS states treated at different times.[7] The results (included in appendix B in Table 9 and Figure 11) indicate that the TWFE estimator in our case puts more than 50% weight on 2x2 comparisons between Non RPS and RPS states.

However, as evidenced by the differences in pre-treatment characteristics between RPS and Non RPS states, depicted by Table 2 and Figure 3, the two groups differ significantly on many of these characteristics. This is further substantiated by Upton and Snyder (2015), Fowler and Breen (2013) and Lyon and Yin (2010), who identify and prove the presence of differing state- specific socioeconomic characteristics and natural resource endowments as drivers of RPS adoption. Therefore, Non RPS states might not be a good comparison group for our analysis and so we move to the estimator proposed by Callaway and Sant'Anna (2021) and Sant'Anna and Zhao (2020).

[7] Covariates have been collapsed to averages at the timing-group by year level as suggested by Goodman-Bacon (2021) for this analysis.

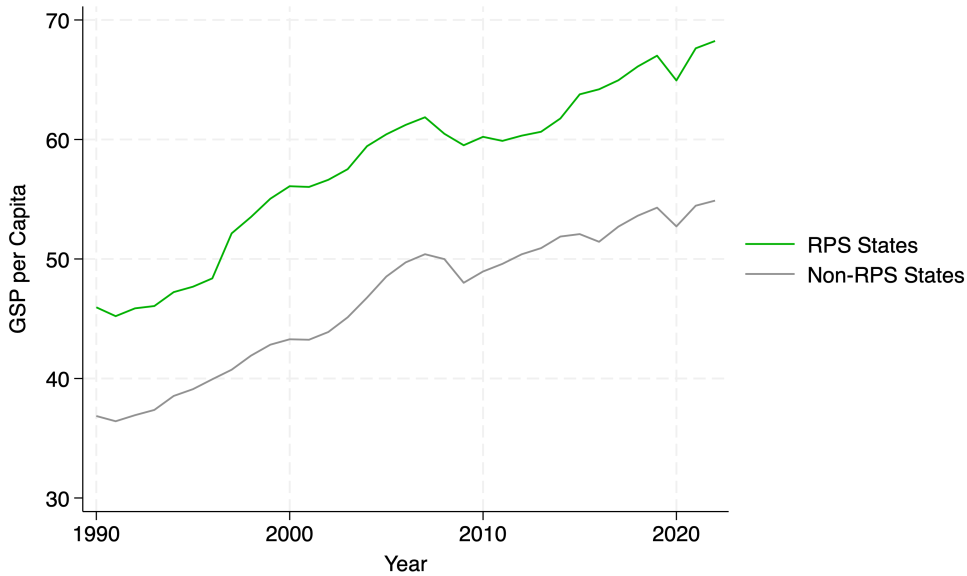


Figure 3: Average state GDP per capita by RPS Adoption Status

5.2 Staggered Adoption with Heterogeneous Treatment

Several recent econometric studies have suggested alternative solutions (see Borusyak et al. (2024), Sun and Abraham (2021), de Chaisemartin and D'Haultfœuille (2020), Imai and Kim (2021), Callaway and Sant'Anna (2021)) to isolate causal effects while studying policy instances such as the RPS and are part of the so-called "Difference-in-Differences Revolution." Roth et al. (2023) summarise multiple such estimators which are robust to treatment effect heterogeneity and/or staggered policy adoption. Of these, the estimator proposed by Callaway and Sant'Anna (2021) seems to be particularly well suited for our analysis. Their method effectively manages staggered adoption scenarios using a binary treatment indicator and calculates precise treatment effects for each group and period while avoiding invalid comparisons. This is achieved by only comparing treated units with those that are either never treated or not yet treated. In addition, the estimator offers significant flexibility, allowing for the aggregation of these detailed effects by duration of treatment exposure or across different cohorts, which sheds light on variations in treatment impacts.

The recent study by Deschenes et al. (2023) is one of the "first source-specific panel data evidence on the efficacy of RPS policies" and the authors use this methodology to assess the impacts of RPS adoption on renewable energy capacity. The authors find an average increase of 44% in wind generation capacity in instances where RPS has been adopted. Unlike traditional DiD settings, Callaway and Sant'Anna (2021)'s method incorporates differences in observed pre-treatment characteristics by allowing for conditional parallel trends. This ensures that comparisons between RPS and Non RPS states are fair and account for state-specific characteristics which might bias our results.

For our analysis, we prefer to use 'Not Yet Treated' RPS states as our choice of control group to further ensure fair comparisons and improve our estimation of the treatment effects given the variation in state-specific characteristics as discussed previously in Table 2.[8] Our estimator calculates the average treatment effect for RPS adopters grouped by their adoption year, t, comparing changes from the previous year, $t-1$. For instance, since Arizona and New Jersey both implemented their policies in 2001, they are categorized as 'Group 2001.' The estimator then assesses the same pre/post difference for a control group—those not treated by year t (e.g., 2001)—akin to a traditional DiD analysis.

In our application, it was not possible to use the default inverse probability weighting and doubly robust estimands due to the heavy data requirements of these methods. Thus, we followed Callaway and Sant'Anna (2021)'s suggestions and Deschenes et al. (2023), and use the outcome regression method, which is also more appropriate for our analysis given limited covariate overlap between Non RPS and RPS states as described previously.[9]

We control for 'Natural Resource Endowments', 'Political Indicators' and 'Industrial Indicators' across multiple model specifications in order to achieve conditional parallel trends and satisfy the parallel trends conditions. Average values of the covariates in the pre-treatment periods between 1990-1996 have been used in order to avoid bias in our final estimates. Further, we aggregate our individual group-time treatment estimates based on the author's suggestions by group, calendar time and in an event study style. These have been reported in Table 4.

Similar to the standard DiD estimates, Callaway and Sant'Anna (2021)'s method relies on the assumptions that once treated, a unit always remains treated and that the data structure must be a panel or a repeated cross-section. In addition, the treatment should follow a staggered adoption design for this process. Finally, two critical assumptions for this method are: first, there must be an overlap in pre-treatment covariates, which we address by using the outcome regression method for estimation as suggested by the authors. Second, the parallel trends assumption must hold between comparison groups, conditional on covariates. Although we cannot formally test this assumption due to numerous unobserved factors influencing our outcome variable, GDP per capita, and because it involves unobserved counterfactuals, our event study results (see Figures 5 and 4) offer visual confirmation that our pre-treatment effects are negligible.

Despite our initial expectations, the results from our analysis (Table 4) show minimal differences between Never Treated units (non-RPS states) and Not Yet Treated states (late RPS adopters). This indicates, that in fact, our choice of control group might not be the underlying issue. Instead, it is likely that state-specific observable and importantly, unobservable characteristics, which influence a state's decision to adopt RPS policies, play a significant role.

[8] We also produce results using 'Never Treated' or Non RPS states in the following section.

[9] Using doubly robust estimators and inverse probability weighting in cases where covariates differ significantly between comparison groups can lead to imprecise inference procedures (Khan and Tamer (2010)).

These characteristics/drivers make the adoption decision endogenous, a factor not fully accounted for by Callaway and Sant'Anna (2021)'s estimator. Their method presumes that units have an equal probability of treatment under given conditional parallel trends, an assumption which may not hold for RPS adoption. As mentioned earlier, prior literature has established the decision to adopt RPS by a state to be non-random. Therefore, we believe that using a synthetic method, which accounts for varied pre-treatment characteristics and constructs a synthetic control group might capture more precise effects for our analysis.

5.3 Synthetic Difference-in-Differences

One of the most prominent alternatives to DiD style designs is the synthetic control method (SCM) developed and introduced by Abadie et al. (2010). The SCM constructs a synthetic control unit which closely matches the treated unit pre-treatment. It relaxes the parallel trends assumption of the DiD methods by allowing the effects of unobserved confounders to vary over time. However, the novel Synthetic difference-in-differences (SDID) approach combines insights from both the difference-in-differences and synthetic control method and has been found by the authors to be more robust and precise in certain cases and equally robust in most cases. Therefore, we use the synthetic difference-in-differences approach to continue our analysis.

The SDID method essentially creates a synthetic control for each treated unit and then applies a DiD estimator to post-intervention outcomes of the treated units to their respective synthetic controls. The SDID method uses both unit and time weights which helps remove bias and improve precision of the SDID estimator by eliminating units or periods that are very different from post-treatment periods.

Comparing with Difference-in-differences methods.
Using both unit and time weights makes the two-way fixed effect regression "local" since it puts greater emphasis on units that are similar in terms of their past to the treated units and it also puts more emphasis on time periods that are on average similar or closer to the treated periods. Abadie et al. (2010) suggest that using similar units and similar periods make the SDID estimator more robust relative to the standard DiD estimator which utilises equal weights for all units and time periods. This combination of weights help solve the problem of meeting the parallel trends assumption commonly faced by traditional DiD methods.

This is particularly relevant in the context of RPS, since a more "local" two-way fixed effects regression allows us to account for latent state factors that make the adoption of the policy endogenous. These state latent factors are unobservables such as a state's culture, geography and political beliefs which contribute to heterogeneity and can bias our results, if ignored. This means using the SDID approach we are able to account for differing unobservable state-specific characteristics which not only drive the adoption of the RPS by a state but might also affect our outcome variable, GDP per capita.

The constructed synthetic unit allows us to closely match each state's pre-treatment trends more effectively, which might not be possible using traditional or modern staggered DiD methods such as Callaway and Sant'Anna (2021). This is because we are now able to construct a more appropriate control group than either of the alternatives presented by Callaway and Sant'Anna (2021) estimator, i.e., Never Treated or Not Yet Treated units. Essentially, because individual states vary significantly from not just Non RPS states but also amongst each other as can be seen from the standard deviations of covariates presented in Tab 2, it is difficult to find a fair, comparable control group.

Therefore, we are motivated to construct such 'comparable' control groups in our analysis using the SDID method and we thereby, handle the two key issues of endogeneity and non-uniform random adoption highlighted in the previous sections.

Comparing with Synthetic Control Methods.
In prior literature on RPS, Upton and Snyder (2017) have used the SCM to estimate the effects of RPS on renewable energy generation, electricity prices, electricity generation related CO2 emissions and electricity demand. The authors construct 30 synthetic controls for each RPS state and then, conduct multiple falsification and robustness checks since they find that the SCM accounts for observable factors impacting RPS adoption, however it may not account for unobservable factors simultaneously affecting RPS adoption and their outcome variables. The SDID method again, allows us to factor in such unobservables since it offers greater flexibility than the synthetic control method.

Since the SDID is designed for settings with multiple treated and untreated units as compared to the SCM which is typically applied in cases with a single treated unit, we believe that the SDID is more appropriate for our state-level analysis of the RPS policies. The SDID estimator also includes unit fixed effects unlike the SCM methods, which again, makes the SDID estimator more flexible and precise. In addition, the SDID method uses the intercept term, which implies that it is sufficient to make the trends parallel as opposed to the complete overlapping expected by the SCM. Put differently, like the DiD, the SDID allows treated and control units to follow only similar trends and they may differ on levels prior to the treatment (Clarke et al. (2023)).

Formally, the model as developed by Arkhangelsky et al. (2021) requires a balanced panel of N units observed over T time periods. The outcome variable, GDP per capita for each state i in each year t is denoted by Y_{it}. A binary variable, RPS_{it} (or $\mathbf{W}$) equals 1 if a state is treated for state i by year t, otherwise it equals 0. This model is for a single adoption period for the treated units. It is also assumed that once treated, states continue to remain treated forever and no states can be 'always treated' during the time period considered. Like the SCM, the authors align pre-exposure trends in the outcome of the Non RPS states with those of the RPS states using unit weights, $\hat{\omega}^{sdid}$. Time weights, $\hat{\lambda}_t^{sdid}$ balance pre and post intervention time periods. Then, the basic two-way fixed effects regression is used to estimate the average causal effect of the treatment where τ is the coefficient of interest, specified by the following equation:

$$(\hat{\lambda}_{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \arg\min_{\tau_{\mu,\alpha,\beta}} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \mu - \alpha_i - \beta_t - RPS_{it}\tau)^2 \hat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid} \right\} \tag{3}$$

The inclusion of unit fixed effects α_i allows for time-invariant unit-specific factors and time fixed effects β_t allow for shared temporal aggregate factors (Clarke et al. (2023)). This increased flexibility and combination of weights offered by the SDID method might help us to more effectively control for observed and unobserved pre-treatment differences between RPS and non RPS states and therefore approaches the endogenous adoption of RPS policies based on pre-treatment characteristics of states more successfully than the methods discussed previously.

Staggered Adoption Design

In the context of RPSs, SDID specifically aids us in incorporating the staggered adoption of the policy by states and thereby, also accounts for policy heterogeneity. The standard SDID estimator has been extended to the staggered adoption design by (Athey and Imbens (2022) and Clarke et al. (2023)). This makes the method particularly useful in our analysis given the varied adoption timings for RPS. Here, too, the assumption is that treated units remain treated forever after they first adopt the treatment.

Abadie et al. (2010) divide the single treatment assignment matrix from before, $\mathbf{W}$, into multiple treatment assignment matrices, for example, $\mathbf{W}^1$, $\mathbf{W}^2$, ..., $\mathbf{W}^A$ based on the number of distinct adoption periods, A. The SDID estimator is then applied to each of these samples separately, and a weighted average of these estimators corresponds to the overall ATT. The ATT for this staggered adoption is calculated by:

$$\widehat{ATT} = \sum_{for a \in A} \frac{T_{post}^{a}}{T_{post}} \times \hat{\tau}_a^{sdid} \tag{4}$$

Therefore, similar to the approach by Callaway and Sant'Anna (2021) all states which adopt the RPS in a given year form a 'Cohort' and the SDID estimator closely matches and constructs a corresponding synthetic twin for each of these cohorts. We use multiple specifications of our defined covariates for our estimates and the results have been presented in Table 5. .

6 Results

In the following subsections, we present the results of our analysis as described in the previous section. First, in section 5.1 we present the results from the standard two-way fixed effects approach across multiple covariate specifications. Although we do not find any statistical significance, the point estimates are in the positive direction. Then, in section 5.2, we discuss our results following Callaway and Sant'Anna (2021)'s staggered adoption estimator where we present four different aggregations of the average treatment effects. We again, do not find any statistical significance, but, the aggregated ATT's point towards the negative direction. Finally, in section 5.3, we factor in the endogenous adoption of the RPS policy using the novel synthetic difference-in-differences method and find a positive effect of the policy's adoption, statistically significant at the 10% level.

6.1 Two-Way Fixed Effects Approach

We present the results from estimating equation (1) in Table 3 where Y_{it} is the outcome variable, being GDP per capita.[10] All our specifications include time and state fixed effects to avoid bias from time varying trends in state GDP and national or economic shocks, such as the financial crisis in 2008 which are expected to affect our outcome variable across states in a similar manner. State fixed effects ensure that our estimate is not biased from state-specific characteristics.

Column (1) includes no additional covariates and checks whether the adoption of RPS on a state has any impact on the state's GDP, where the point estimate does point in the positive direction, however, statistically insignificant. Column (2) adds 'Share of Manufacturing' as a percentage of a state's total GDP as a control and specification (3) includes political indicators, that is, LCV scores at the house and senate level in a state as well. Across both specifications, the share of manufacturing has a significant (5%), positive effect on a state's GDP per capita captured by the coefficients, which are 0.714 and 0.718 USD thousands. This seems to be a reasonable estimate and is in line with prior research and confirms Bauer et al. (2006)'s results on the positive impacts of a state's capital base on its GDP. In addition to other covariates, conditioning on natural gas reserves and crude oil production in column (4) also does not yield any significant results on the impacts of RPS adoption on state GDP, even though the coefficient on 'RPS Dummy' seems to be growing marginally.

As discussed in the prior section, specification (5) further confirms our doubts regarding the efficacy of the standard two-way fixed effects model's applicability in our analysis, given the observable and unobservable differences between RPS states and Non RPS states. The stringency parameter has a large positive coefficient, in line with our results from our descriptive exercise with the baseline analysis (see Table 10.1.2), however, both are insignificant.

At this stage, it becomes important to note, that we fail to see any effects of the RPS policy on a state's GDP contrary to the common beliefs of the negative consequences of such policies on economic well-being. Overall, we believe that given the bad comparisons made by our estimator

[10] GDP per capita has been measured in USD thousands and has been normalised to per 1000 people for all our specifications.

Table 3: Estimated Impact of RPS

	Two Way Fixed Effects				
	(1)	(2)	(3)	(4)	(5)
RPS Dummy	1.377	1.873	2.239	2.337	1.809
	(1.812)	(1.802)	(1.594)	(1.617)	(1.546)
Share of Manufacturing		0.718**	0.714**	0.732**	0.727*
		(0.351)	(0.354)	(0.364)	(0.364)
House LCV Score			-0.006	-0.004	-0.009
			(0.048)	(0.045)	(0.044)
Senate LCV Score			-0.019	-0.019	-0.019
			(0.018)	(0.019)	(0.019)
Crude Oil Production				0.003	0.003
				(0.003)	(0.003)
Natural Gas Reserves				-0.000	-0.000
				(0.000)	(0.000)
Stringency					6.768
					(6.074)
Constant	53.147***	43.266***	44.387***	43.910***	44.121***
	(0.654)	(5.235)	(6.133)	(6.072)	(6.033)
Observations	1551	1551	1551	1551	1551

Note: Standard Errors have been clustered at state level. State and Time fixed effects included in all regressions.

(evident from our results of the Goodman-Bacon (2021) decomposition of the TWFE estimator, see Table 9 and Figure 11) as well as the inherent differences in the composition between RPS adopters and never treated states[11] , biases our estimates. Therefore, it is essential to look further and we move to our results from Callaway and Sant'Anna (2021)'s methods which aim to resolve some of the issues faced by the standard fixed effects estimators in staggered settings.

6.2 Staggered Adoption with Heterogeneous Treatments

Not only does Callaway and Sant'Anna (2021)'s take on the DiD estimator handle staggered adoption with heterogeneous adoption, more importantly, it also allows us to improve our control group by only comparing early treated RPS states with not yet treated RPS states. We believe that this would enable us to account for the observed differences between treated and untreated states and make more fair comparisons. We present our results from comparing RPS

[11] As seen in Table 2, the LCV scores and mean state GDP per capita between RPS states and Non RPS states differ significantly.

states with both 'Not Yet Treated' Groups and the 'Never Treated' states in Table 4. For our estimations, standard errors have been clustered at the state level.

For our estimations, we provide unconditional as well as conditional estimates on the pre-treatment means of covariates including solar and wind energy potential, crude oil and natural gas reserves and our political and industrial indicators. We present four aggregates of the estimated impacts of RPS. The average effects of RPS across all adopting states has been indicated by the 'Overall ATT (Group)'. This helps us understand the heterogeneous effects of adopting the RPS policy by state. The 'Overall ATT (Year) parameter aggregates the treatment effect by calendar time, reflecting the cumulative effect of adopting the RPS policy upto 2022. Finally, the parameters 'Post 1-5 Years' and 'Post 6-12 years' aim to capture the dynamics over short and long time periods of the treatment effect, i.e., how the evolution of the effects of adopting RPS changes after treatment.[12]

Panel A from Table 4 reports results with the control group being 'Never Treated' or Non RPS states and Panel B compares with 'Not Yet Treated' RPS states, our preferred specification. Strikingly, the results from both panels are almost identical. Initially, when no additional covariates were included, as specified in column (1), our analysis hinted at a potential positive direction for the Average Treatment Effect on the Treated (ATT) by group and calendar years. However, when further adjusted for covariates in columns (2) and (3), the overall ATT at the group and calendar year levels showed a trend towards negative values, though these findings were not statistically significant.

The next two parameters, 'Post 1-5 Years' and 'Post 6-12 Years' aggregate the treatment effects in an event study style to investigate into the short and long-term impacts of RPS adoption on state GDP, respectively. These too, revealed larger but still statistically insignificant negative impacts over the longer term, for both panels. Figures 4 and 5 plot the conditional dynamic treatment effects from column (3) of the table above for both never treated and not yet treated control groups respectively. In these event study style figures, each point presents the pre/post treatment aggregated ATT computed by averaging individual group-time specific estimates following Callaway and Sant'Anna (2021). Given that the average treatment effect pre-treatment is close to zero in both model specifications, we have visual evidence supporting the existence of parallel trends for our analysis. 10 periods post treatment have been represented and the average effects seem to be falling post period 5 in both figures. Over time, the standard errors appear to be increasing in both figures, this is likely due to the number of pure control units decreasing by time, as Callaway and Sant'Anna (2021)'s estimator attempts to make more fair comparisons and does not include comparisons between late treated and early treated units.

Our results contradict our hypothesis, since despite obvious observed differences between RPS and non-RPS states, the results from choosing never treated units as our control group are

[12] We choose to drop Iowa and Vermont for this analysis to balance our panel of states to prevent the estimated ATT to be driven by changes in the composition of policy adopters over a long time period, Callaway and Sant'Anna (2021)

Table 4: Estimated ATT of RPSs impact on State Level GDP

	(1)	(2)	(3)
Panel A: Never Treated			
Overall ATT (Group)	0.100	-3.764	-4.002
	(-1.40)	(-4.48)	(-3.79)
Overall ATT (Year)	0.564	-3.287	-3.170
	(-1.18)	(-4.27)	(-3.34)
Post 1-5 Years	-0.029	-0.726	0.156
	(-0.85)	(-1.67)	(-1.05)
Post 6-12 Years	-0.035	-5.152	-4.086
	(-1.61)	(-6.5)	(-5.16)
Panel B: Not Yet Treated			
Overall ATT (Group)	0.070	-3.612	-3.887
	(-1.37)	(-4.52)	(-3.82)
Overall ATT (Year)	0.250	-3.143	-3.165
	(-1.14)	(-4.29)	(-3.37)
Post 1-5 Years	-0.211	-0.160	0.735
	(-0.72)	(-1.87)	(-1.25)
Post 6-12 Years	-0.023	-5.116	-4.141
	(-1.58)	(-6.53)	(-5.17)
Controls			
Natural Resource Endowments		Yes	Yes
Industrial and Political Variables			Yes
Observations	1518	1518	1518

Notes: 'The Overall ATT(Group)' parameters indicate the average effect of RPS adoption by all treated States. The 'Overall ATT(Year)' parameters indicate the average effects of RPS adoption across all periods for all States. 'Post 1-5 Years' and 'Post 6-12 Years' parameters have been estimated separately for years post implementation indicating cumulative treatment effects. Robust standard errors (in parentheses) have been clustered at state level. Column 1 reports unconditional estimates. Column 2 includes Natural Resource Endowment Controls as defined previously. Column 3 includes Industrial and Political variables as controls. (*** p<0.01, ** p<0.05, * p<0.1)

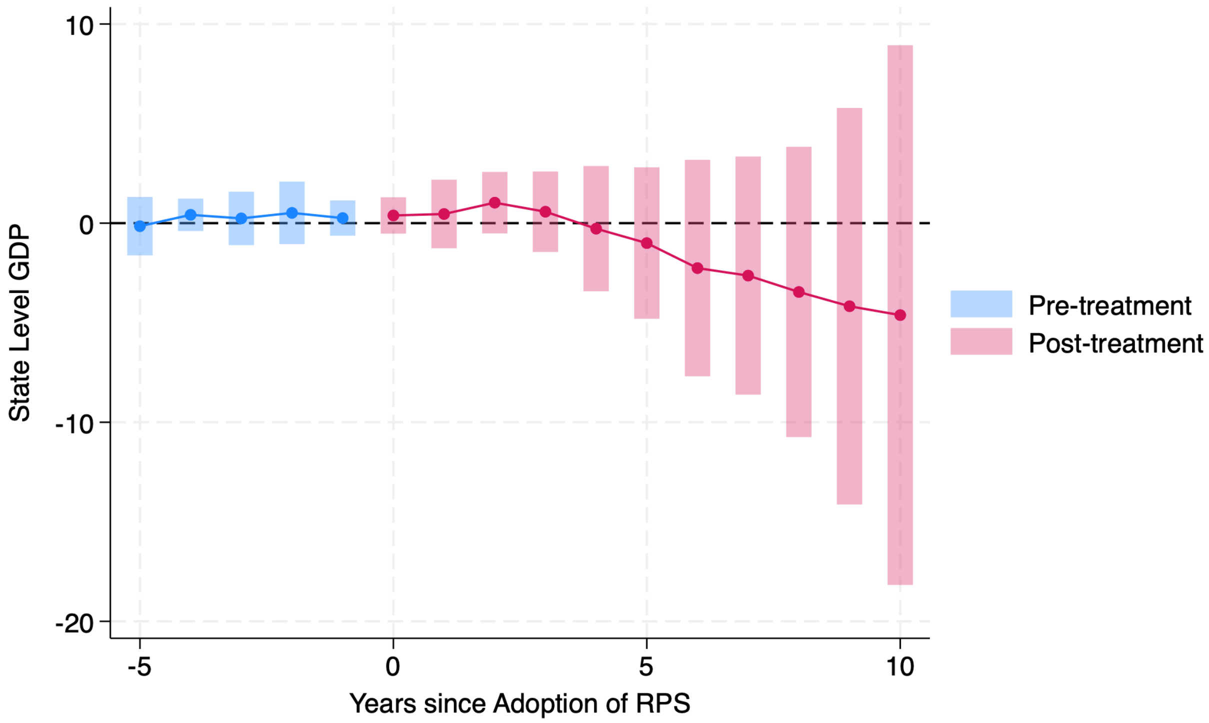


Figure 4: Never Treated: Dynamic impacts of RPSs on GSP

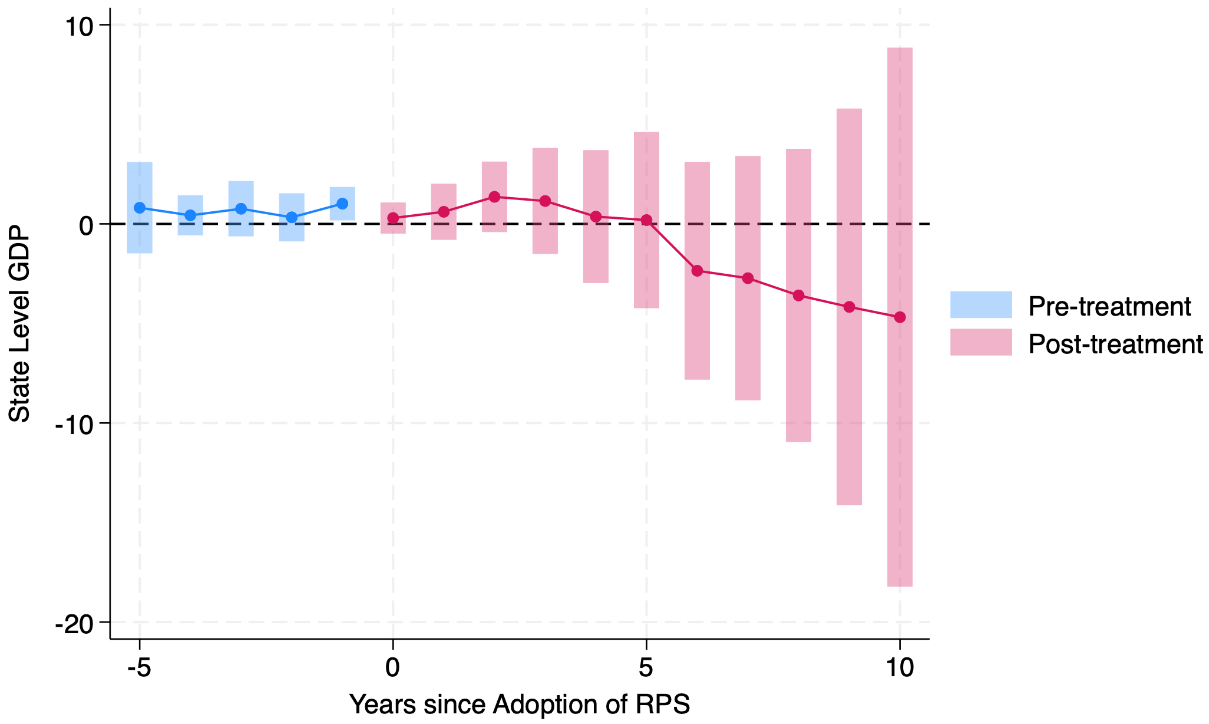
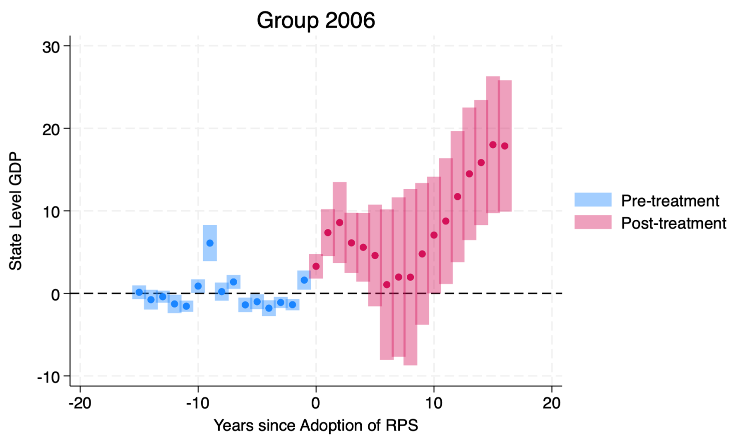
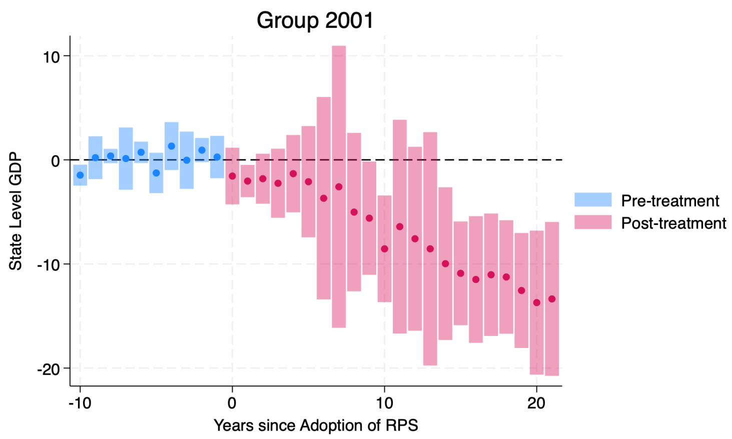


Figure 5: Not Yet Treated: Dynamic impacts of RPSs on GSP

strikingly similar to comparisons with never treated units. This highlights the presence of unobserved characteristics which might be affecting our outcome variable as well as a state's decision to adopt RPS. Even if the presence of parallel trends is accurate, our results from the two-way fixed effects approach and the Callaway and Sant'Anna (2021) estimator are statistically insignificant and make any conclusions dubious. The endogeneity in adopting RPS and thereby, it's impacts on GDP per state could be the underlying issue.



(a) Group 2006: Dynamic impacts of RPSs on GSP



(b) Group 2001: Dynamic impacts of RPSs on GSP

Figure 6: Group Wise Decomposition of ATT

In addition, we also analyse the aggregated treatment effects by RPS adopting groups with Non RPS states as the control group and present the results in Table 10. Here too, as presented by Figure 6, we find mixed results, with Group 2001 (Arizona and New Jersey) having large significant negative results at the 1% level while Group 2006 (Washington) has larger significant (1%) effects. Other groups, although marginally significant at 10% have either positive negative or no effects (see Figures 12, 13 and table 10 for full summary). Similarly, prior literature on the RPS policy presents mixed results regarding the efficacy of the policy, with Lyon and Yin (2010), Deschenes et al. (2023), Shrimali et al. (2015), find small positive increases in renewable electricity generation post adoption and Greenstone and Nath (2020) and Feldman and Levinson (2023) additionally find decreases in GHG emissions. While, Upton and Snyder (2017), Shrimali et al. (2015) find declining or no increase in renewable capacity post adoption.

For our analysis, we conclude that any one specific state or group might not be a sufficiently good comparison group. Therefore, we believe that using the synthetic difference in differences approach and constructing synthetic controls to closely match pre-treatment trends of RPS states based on both observed and unobserved states can handle the issue of endogeneity more effectively and help us identify the impacts of the RPS policy on state level GDP more clearly.

6.3 Synthetic Difference in Differences

Here, we present our results using the 'Staggered Adoption Design' of the synthetic difference in differences method as described by Arkhangelsky et al. (2021) and Clarke et al. (2023). the average treatment effect in the staggered adoption design is calculated by individually applying the SDID estimator to each RPS adopting cohort and then computing a weighted average of these individual estimates.[13] We present the unconditional coefficient of the average effect of RPS adoption on state GDP in column (1) of Table 5 and columns (2) to (4) add covariates as defined earlier.[14]

Conditional on natural resource endowments, industrial indicators and political indicators, in column (4) we find a significant (at 10%) positive coefficient on RPS adoption. This means that on average, RPS states' GDP per capita increased by USD 3092 between 1990-2022 following policy adoption. This estimate when compared to our results from the same specification of the standard fixed effects approach (Column (4) in Table 3), is slightly larger but notably, in the same direction. Figures 7 and 8 show the comparative average GDP per capita between the treated cohorts 1999 and 2006 and their synthetic control units respectively.[15] In Figure 7, cohort 1999 (including Connecticut, Maine, Texas and Wisconsin) and its synthetic control closely follow the same trend pre-treatment, which provides evidence of the presence of the parallel trends assumption. Post treatment, it can be seen that there is a small increase in the relative GDP per capita of the treated cohort as compared to it's synthetic control unit, providing visual evidence of our estimates. Time weights have been indicated by the green triangles on the x axis, these ensure that pre treatment periods most similar to post treatment periods in terms of GDP per capita and the included covariates are assigned more weight.

Table 5: SDID: Impacts of RPS Adoption on GSP

	(1)	(2)	(3)	(4)
RPS Dummy	1.611	0.733	1.328	3.092*
	(2.060)	(1.827)	(1.602)	(1.595)
Controls				
Natural Resource Endowments		Yes	Yes	Yes
Industrial Indicators			Yes	Yes
Political Indicators				Yes
Observations	1,551	1,551	1,551	1,551

Notes: The bootstrap method has been used for inferences (Please refer to Arkhangelsky et al. (2021) for further details). Column 1 presents unconditional estimation. Column 2 adds 'Natural Resource Endowments' as controls. Column 3 adds 'Industrial Indicators' and Column 4 includes 'Natural Resource Endowments', 'Industrial Indicators' and 'Political Indicators' as well. (*** p<0.01, ** p<0.05, * p<0.1)

[13] The weights are defined based on the number of treated units per cohort and time periods for these cohorts.
[14] Standard errors are clustered at the state level by default.
[15] Here, the filled green triangles on the x axis represent the assigned time weights

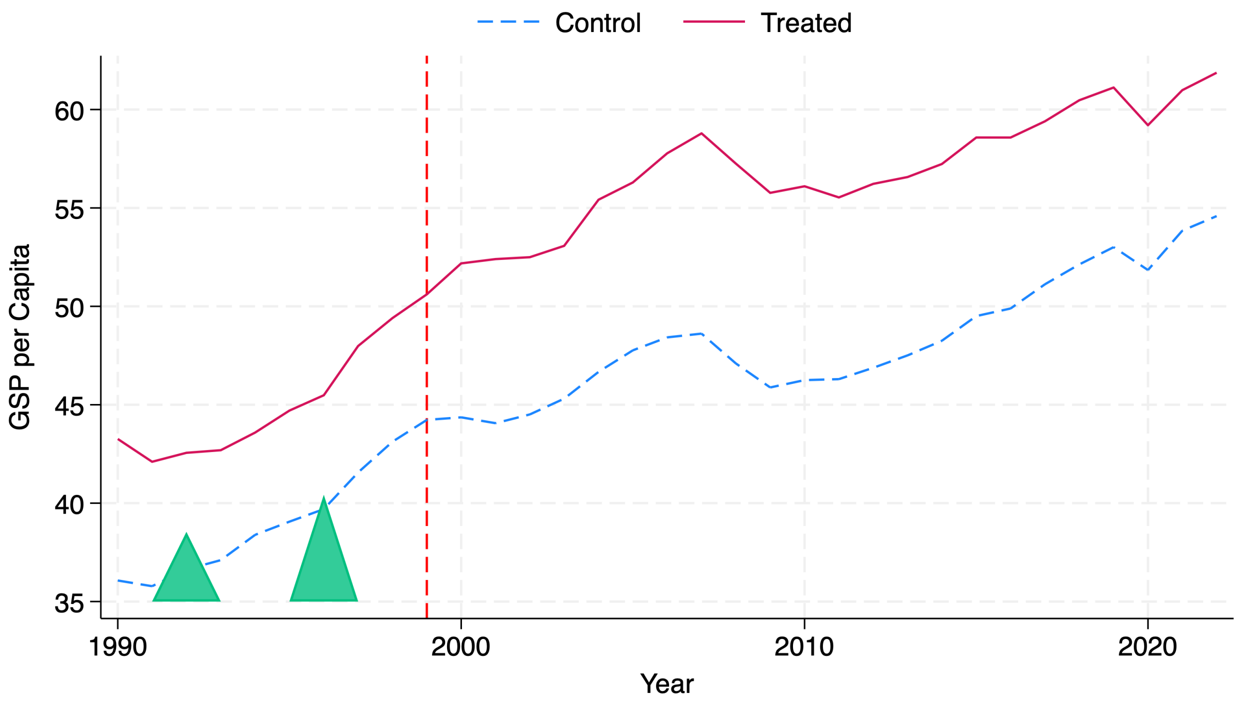


Figure 7: Cohort 1999: Impact of RPS Adoption on GSP

Similarly, Figure 8 represents another example, showing the comparison between the treated cohort 2006 (Washington) and its synthetic counterpart. Here too, trends pre-treatment match each other and a significant gap between the treated and control units can be seen post treatment, increasing with time. This implies, that not only did the RPS policy have an overall positive effect on state level GDP, the impact also varied by time and by adoption cohort, as has been previously discussed in section 6.2. Multiple factors within a state's RPSs such as the relative stringency of targets, or the implementation of cost caps on electricity price pass throughs differ by state and could lead to these differential impacts. Please refer to Appendix B for a full summary of graphs produced for all RPS cohorts.

Table 6 and 7 also present the unit weights assigned to Non RPS states for the formation of each cohort's respective synthetic twins. For instance, cohort 2006's synthetic control group consists of Georgia, Virginia, Utah and Tennessee. Whereas cohort 1999's synthetic control is composed by a larger number of Non RPS states in addition to Georgia, Virginia, Utah and Tennessee. This variation can be explained by the composition of the cohorts themselves, since cohort 1999 includes four RPS adopting states, the unit weights have been allocated to closely capture their average. Please refer to Table 11 in the appendix for a full summary of all units weights for each RPS adopting cohort part of our analysis.

The synthetic difference in differences specification therefore, indicates an overall positive impact of RPS adoption on state level GDP. We believe that the SDID estimator has enabled us to capture state latent factors which biased estimates in previous methods due to unobserved

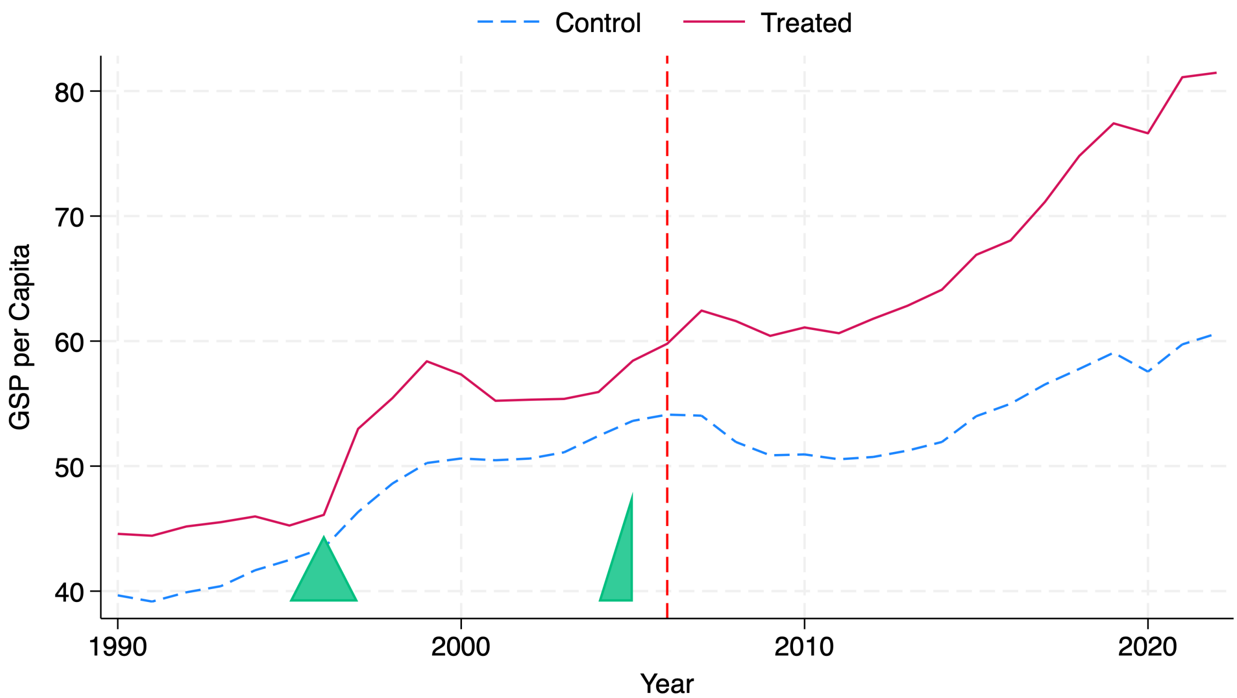


Figure 8: Cohort 2006: Impact of RPS Adoption on GSP

Table 6: Unit weights for Cohort 1999 by State

State	AR	FL	GA	ID	IN	KY	MS
Weights	0.064	0.055	0.139	0.043	0.062	0.038	0.076
State	ND	OK	SC	TN	UT	VA	NE
Weights	0.023	0.024	0.073	0.124	0.099	0.085	0.036

Note: States with a weight of 0.000 are not presented in this table. Cohort 1999 includes Connecticut, Maine, Texas, and Wisconsin.

Table 7: Unit Weights for Cohort 2006 (Washington) by State

State	Florida	Georgia	Indiana	Tennessee	Utah	Virginia
Weights	0.002	0.394	0.058	0.128	0.172	0.245

Note: States with a weight of 0.000 or lesser are not presented in this table. Please refer to Table 11 for the full summary of Unit Weights.

heterogeneity in states pre-treatment. This heterogeneity arises from intrinsic attributes of each RPS state, such as culture, geography, demographic composition, political beliefs and economic climates. All such variables combined with the drivers of RPS adoption as well as heterogeneity in policy implementation can lead to variation in the efficacy of RPS and also it's impacts on the state's GDP. While the SDID estimator is more successful in handling endogenous adoption of the RPS policy of a state, we see a significant positive effect in only one specification and believe that overall, the RPS policy has minimal to at best, slightly positive effects on GDP due to multiple opposing channels acting simultaneously (see Section 8).

7 Robustness Tests

7.1 Placebo Inference Estimation

The authors, Arkhangelsky et al. (2021) propose three distinct inference methods to estimate the variance and confidence intervals of the estimated treatment effect under the SDID method. To further solidify our results from the synthetic difference in differences method, we employ the placebo inference tests proposed by Arkhangelsky et al. (2021). The placebo inference process conserves just the control units and then randomly assigns the treatment, here RPS adoption, to these control units. This placebo treatment is repeated multiple times, assigning placebo treatments to different control units and re-estimates treatment effects for these placebo units. The variance of these multiple placebo treatment effects are used to construct confidence intervals for the true average treatment effect.

The authors suggest that the placebo inference option is particularly useful when the number of treated units is small and can only be applied when the number of pure control states exceeds the treated states. So, for our analysis, we break up our sample into two and use the placebo inference method for estimating the ATT, first for states which adopted the RPS before 2002 and second, for adopting cohorts after 2002.

Table 8: SDID (Placebo Inference): Impacts of RPS Adoption on GSP

	(1)	(2)	(3)	(4)
Pre 2002	1.569	0.388	0.959	2.296
	(2.595)	(1.972)	(1.984)	(2.034)
Post 2002	1.701	1.310	1.726	1.742
	(5.028)	(5.157)	(6.815)	(6.299)
Observations	957; 1,155	957; 1,155	957; 1,155	957; 1,155

Notes: Standard Errors have been clustered at state level. Column (1) presents unconditional results, columns (2)-(4) add covariates as defined in Section 5.3. Iowa and Vermont have been excluded from this analysis to maintain sufficient balance between treated and control units for placebo inference.

As presented in Table 8, from both samples we find no significance in any of our specifications. This robustness analysis indicate the positive significant effect found by the SDID estimation in our main specification is not a generalizable effect of the treatment itself. Therefore, we believe that overall, the adoption of the RPS policy seems to have no effect or no loss of GDP for a state. This is in line with our expectations since environmental policies such as the RPS have been known to have mixed effects and multiple channels through which an adopting state's GDP might be affected. These potentially opposing channels can cancel out each other overarchingly. No loss of GDP by the adoption of RPS is significant since the policy is not only one of the largest and most widely adopted policies in the US but also, that it targets emissions from one of the largest emitting industries, that is, power production. Our results support the expansion of the RPS policy as is being currently considered by multiple RPS states in the US.

7.2 Is there a GDP threshold for adopting RPS?

Some prior literature (Upton and Snyder (2017), Deschenes et al. (2023)) have suggested that a state's initial levels of GDP could be one of the drivers for RPS adoption. This can also be seen from our inferential statistics (see Table 2 and 3) where on average RPS adopting states have higher GDP than Non RPS states. This raises the concern whether there is a threshold GDP per capita which eases the adoption of the RPS policy for a state. Further, this concern is substantiated since it has been common that the developed world adopts and enacts environmental policies quicker and in higher magnitude in comparison with the developing nations. However, in Figure 9, we plot each RPS state's GDP per capita at that year of adoption and find no such evidence of state GDP driving RPS adoption. There also seems to be no GDP threshold for a state to enact the policy. As can be seen, most RPS states lie around the 50,000 USD mark around their time of adoption, with the District of Colombia being an outlier. It can also be seen that California and New York despite being two of the largest states in the US in terms of GDP per capita, adopt RPS relatively later than other smaller or larger states such as Nevada, Wisconsin or Texas. Furthermore, non-RPS states around the period of 2002-2010 when majority RPS policies were initially adopted, also lie in the same zone in terms of average GDP per capita in our analysis.

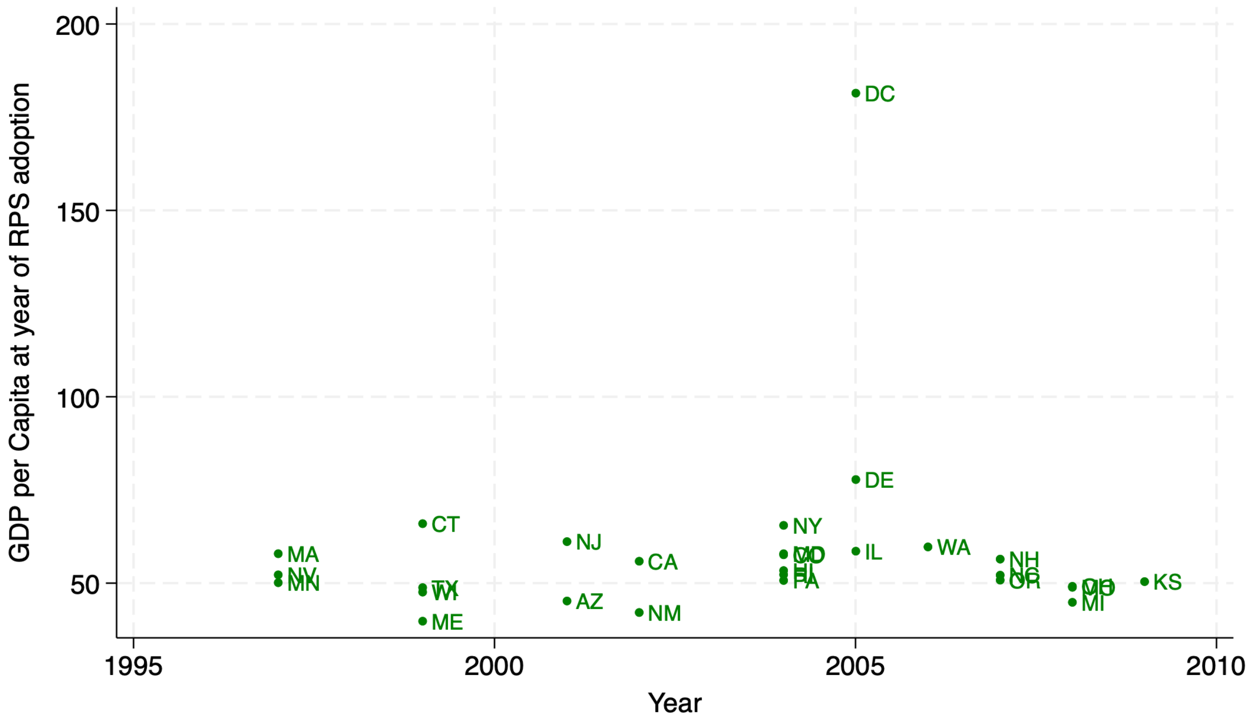


Figure 9: State level GDP per capita at year of adoption

8 Discussion

Renewable portfolio standards are one of the longest running and most prominent policies in the United States for it's transition towards a sustainable electricity sector. However, even though they have been widely adopted and have been in place since 1983 in various forms, their efficacy has always been questioned. Prior literature has found mixed effects in terms of RPSs abilities to actually increase renewable energy. In general, the impacts of environmental policies on the economy have also been widely debated and also present, mixed results. This assessment of the impacts of environmental policies on the economy is of particular relevance, given the focus on the negative consequences of such policies and significant pushbacks by opponents across public and private forums.

Our analysis aims to carefully bridge this gap in current research and critically examine the effects of RPS adoption on a state's economic well being. We use empirical methodology which is suited for studying the staggered adoption and heterogeneity in RPS policies as well as the endogeneity driving adoption decisions. This careful consideration is necessary since it enables us to possibly isolate the effects of such standards on the economy, given the inherent complexities of RPSs by design.

Overall, our results lead us to believe that there is no loss of GDP as we establish that no negative effects of RPS are present in RPS states. There is at best, a small positive impact which arises through RPS adoption (see Table 5) over a long time period, as seen by our implementation of the novel SDID estimator. The results from the standard two way fixed effects specification point towards a positive effect, albeit, insignificant. Surprisingly, as we run our model through Callaway and Sant'Anna's methods, we receive a negative effect which is also not statistically significant. At first glance, these results might seem conflicting but they are in line with our expectations since prior literature has presented a mixed record of the impacts of environmental policies on the economy. It is also essential to note, that policies such as the RPS have been found to affect GDP through multiple positive and negative channels (discussed below) and this reinforces our interpretation, that there is no overall effect of the RPS on state GDP.

We also believe that the new SDID estimator enables us to incorporate multiple unobservables that drive such impacts, thereby making our analysis more robust. While our empirical methodology highlights multiple intricacies of the RPS policies and handles them step by step. RPS policies also include multiple other layers which vary by state and can affect our analysis. RPS policies differ significantly per state not just through different targets or stringency but also include other aspects which are not uniform across RPS states. Some RPS states adopt cost caps to avoid electricity price increases for customers owing to their adoption of RPS, while several states also impose carve outs to ensure that their standards focus on some renewable energy sources more than others, etc. These aspects of the RPS policies have not been addressed given the scope of our analysis.

One important aspect of the RPS that we do not capture, is the ability to trade renewable electricity amongst states in order to achieve state set standards. This means that states need not produce or invest in renewable electricity themselves to the full capacity suggested by their individual RPS targets but can instead borrow renewable electricity via 'Renewable Electricity Certificates' (REC) to meet their state targets. Inter-state trading and intra-state trading can significantly affect the impacts of RPS on a state's GDP not only for the adopting states but also it's neighbouring states. Several recent studies have focused on the inclusion of REC's and it's effects on varied outcome variables (Feldman and Levinson (2023), Deschenes et al. (2023)). The inclusion of such aspects of the RPS policies will add more detail to our analysis and can alter our results.

8.1 Channels Affecting GDP

As previously discussed in section 2.2, RPSs can affect a state's GDP through various positive and negative channels, which are often opposing and can cancel out each other, leading to no overall effects of the standards, as has been established by our results. These channels can be quite relevant for policymakers and help them understand how RPS programs can affect a state's GDP. Some key areas through which RPS policies affect GDP have been discussed below.

Share of Renewables

One of the most important goals of the RPS policies is to increase renewable energy generation. It has been established by various papers (Deschenes et al. (2023), Yin and Powers (2010), Carley (2009)), that RPS implementation leads to increases in renewable electricity capacity. These policies lead to surges in investments into the renewable sector and also simultaneously create employment opportunities in such green sectors. States with high renewable standards or high renewable potential can also attract international investors, given the increased focus on sustainable finance globally. Moreover, introduction of renewable sources diversifies a state's energy mix, providing increased stability to industries heavily reliant on the power sector.

However, this transition to green electricity system also has significant set-up costs. Renewable energies are also presently known to have higher costs when compared to fossil-fuel based electricity production and therefore, increase costs for locally run businesses. These costs may also result in higher utility bills for consumers and thus, inhibit their spending habits to a degree. In addition, stricter environmental regulations may hinder the international competitiveness of local businesses, resulting in loss of exports and thereby, state GDP. Several authors have found that RPS policies may even lead to declining renewable generation capacities (Shrimali et al. (2015), Upton and Snyder (2017)). The relationship between RPS adoption and economic growth via renewable energy growth is therefore, not straightforward and can lead to mixed overall effects.

Employment

Potentially, environmental regulations such as the RPS can lead to increases in innovation within the renewable and electricity sectors. This can foster the growth of newer industries such as, renewable energy, environmental consulting, and pollution control technologies and simultaneously increase employment opportunities. Bowen et al. (2013) find that with RPS adoption, there is no effect initially on job growth. However, the persistence of RPS through time does cause a growth in the number of green jobs and green businesses. This channel leads to a positive effect on GDP. If we shift our focus to gross jobs in other sectors such as Construction and O&M, past evidence shows that RPS compliance led to an increase in the number of gross jobs thus potentially leading to an increase in the GDP (Barbose et al. (2015), Wolverton et al. (2022)).

However, such increases in green jobs can simply come from the transition towards renewable industries and the resulting loss of employment within more traditional, fossil-fuel reliant industries. Increase operational and compliance costs for businesses and the need to reinvest capital towards newer technologies commonly lead to job losses in traditional sectors, as has been evidenced by the coal-mining industry. Therefore, this is another area through which the RPS policy leads to mixed effects on overall economic outcome.

Electricity Prices

RPSs significantly change the electricity sector at the state level by diversifying its energy mix. It has been seen that states that have adopted RPS have seen an uptick in electricity prices (Upton and Snyder (2017), Palmer and Burtraw (2005), Barbose et al. (2016)) . We believe this is one of the main channels affecting GDP, at least in the short term. The direction of the effect is negative since higher electricity prices lead to higher costs for businesses. Furthermore, these costs are often passed over to consumers and can limit overall consumption, further reducing state GDP. This possibly explains some of the negative effects that we see while running some specifications.

However, Barbose et al. (2015) suggest that these rate impacts are mostly modest. Furthermore, over time, RPS costs will be affected by other market and policy changes, such as new federal environmental regulations and decreasing costs of renewable energy which will dampen these costs. Barbose et al. (2015) also suggest that the costs faced by each state's respective electricity sector varies based on their ability to integrate newer technologies as well as their transmission capabilities. Moreover, an increasing number of states have introduced cost-caps as discussed previously which leads to lesser increases in electricity prices faced by end-consumers. Therefore, there is no guarantee of the persistence of increased electricity prices and these too have varied effects on a state's GDP.

Emissions

Past research has corroborated the fact that RPS in fact does decrease power sector GHG emissions, particularly carbon emissions. Barbose et al. (2015) also estimate the health benefits of this and perform an analysis on six states and find positive health outcomes in the long run due to improved air quality. We believe this plays a role in yielding positive economic impacts over a period of time with workforce productivity improving as health outcomes improve.

The discussion of all the channels above strengthen our conviction in our results as there is a complex relationship between RPS and GDP and it can be affected through multiple channels - positive and negative. We also acknowledge that there are some limitations to our study that can potentially alter some results. Firstly, a lot of other unobservable factors that can affect GDP are not accounted for in our analysis. Secondly, we choose to not include some aspects of RPS such due to its inherent complexities and the fact that these policies vary significantly from one state to another.

9 Conclusion

RPSs have played an instrumental role in the environmental policy landscape in the US. The key objectives of these policies are to diversify the energy mix, to reduce emissions, and to promote economic development. The literature review presents numerous studies that examine the impact of RPSs on the energy mix and emissions. There are relatively fewer studies on its effects on economic development. In our research, we estimate the causal effects of RPS on economic growth, with GDP per capita being our key outcome variable of interest.

The staggered adoption by different states and policy heterogeneity introduces some challenges in analyzing the causal effects. To address these challenges, we use Callaway Sant'Anna's staggered DiD estimator and report insignificant results. Our main specification takes into account, the endogenous adoption of this policy and we proceeded to use SDID, one of the most recent methodologies developed in the econometrics toolkit.

In our specification, we find that following an RPS adoption by the states, on average, the GDP per capita increases by USD 3092 between 1990-2022. Due to lower statistical significance, we do not interpret these results as a strictly positive effect and, contrary to the opinion of opponents of RPS, these environmental policies do not negatively affect the economy. Additionally, we discuss the theoretical channels that can affect our empirical results. We infer that RPS affects the GDP possibly through electricity prices, emissions, and green jobs.

While we have focused on how widely prevalent these policies are, it is important to note that there are still 20 states that have not yet adopted this policy. Our results prove to be a starting point in the argument towards implementing these state-level standards. Moreover, it is also crucial to understand that interstate trading of RECs, solar and wind carve-outs, and the cost caps cause distortionary effects in the estimation of the true impact of these RPS programs. Future research can include these effects to gain a more comprehensive understanding of the long-term economic effects of RPS.

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List of Figures

List of Tables

10 Appendix

10.1 Appendix A: Descriptive statistics and analysis

10.1.1 RPS Stringency by Year

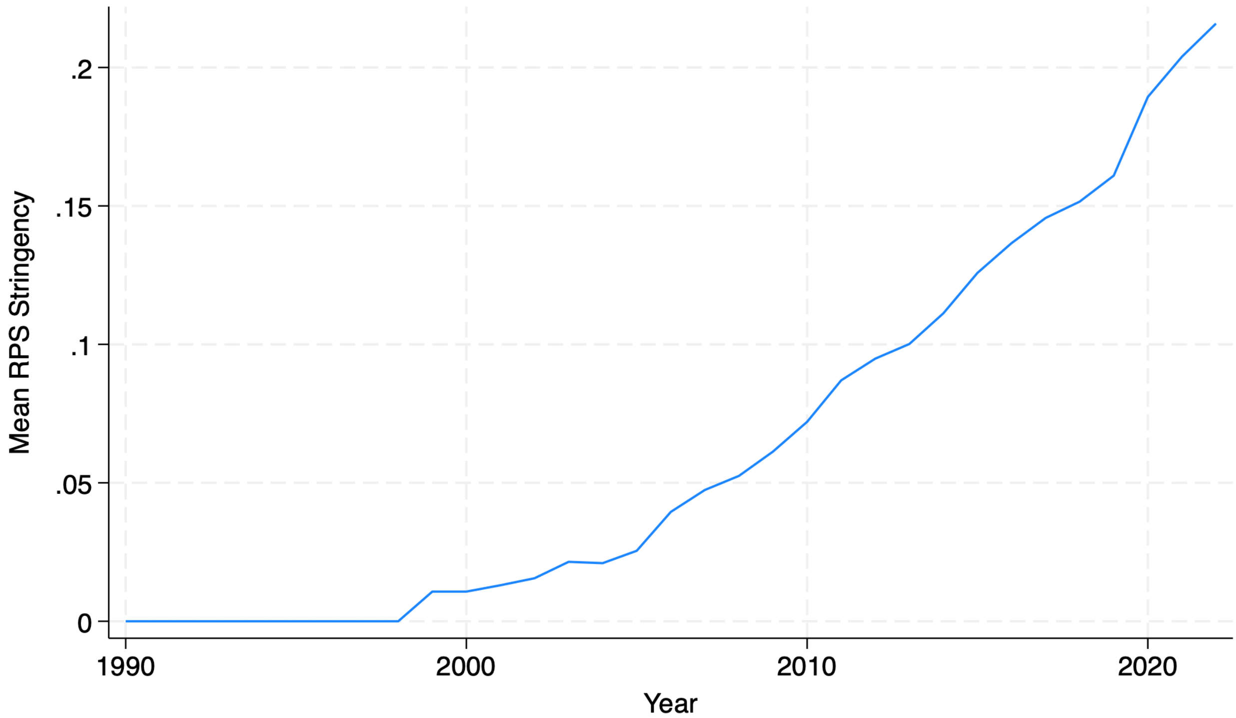

Figure 10: Average RPS Stringency by Year

10.1.2 Baseline TWFE Specification

Here, we present the results of the baseline TWFE specification as described in section 4. Please note, these results are simply a descriptive exercise measuring the change in GDP per capita due to the length of exposure to treatment and stringency of treatement.

	Baseline Specification
RPS Dummy x Stringency	6.564
	(7.395)
RPS Dummy x Duration	0.0267
	(0.145)
Constant	53.29***
	(0.527)
Observations	1551

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

10.2 Appendix B: Estimates

10.2.1 Goodman-Bacon Decomposition

Table 9: Bacon Decomposition of Treatment Effects

Component	Beta	Total Weight
Timing Groups	0.77	0.44
Never vs Timing	1.29	0.56
Within	4.48	0.00

Notes: 'Timing Groups' includes all 2x2 DD comparisons between treated states. 'Never vs Timing' includes all 2x2 DD comparisons between treated and never treated states and 'Within' represents the residual. 'Beta' is the estimated coefficient of each component and 'Weight' implies the weight given to each component while aggregating the TWFE ATT.

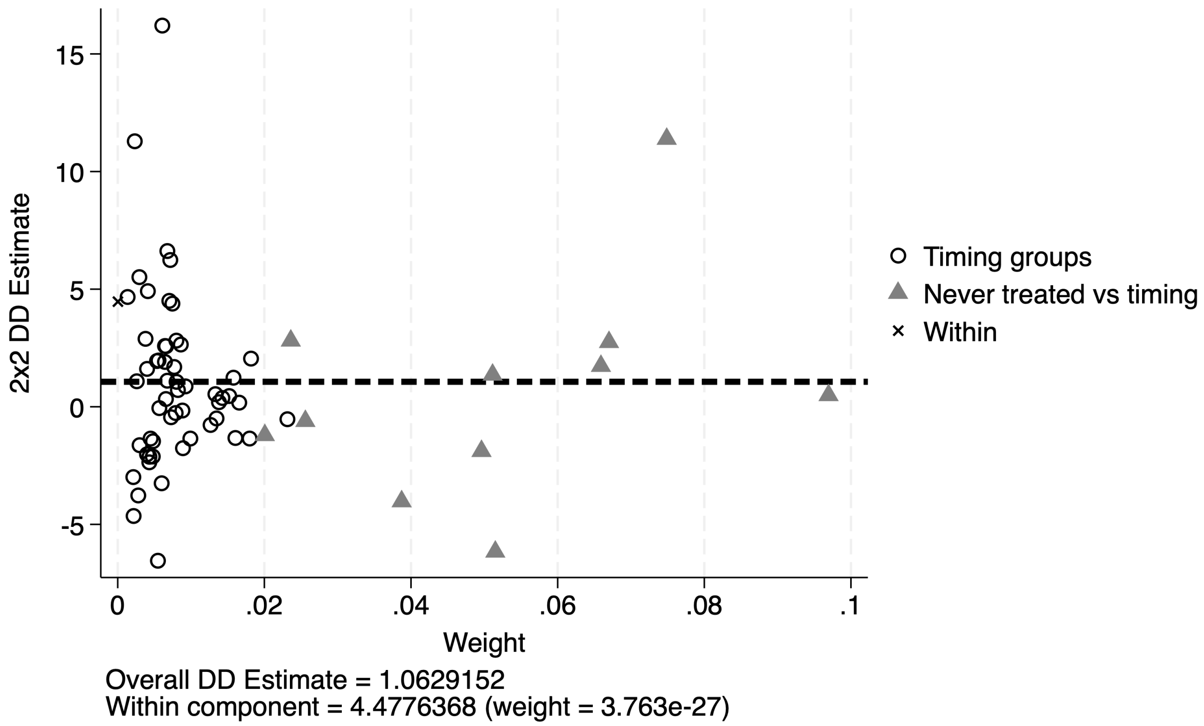

Figure 11: Bacon's DD Decomposition

10.2.2 Group Wise Treatment Effects using Callaway and Sant'Anna (2021)'s estimator

Table 10: Group-wise ATTs of the impacts of RPS on GSP

Variable	Coefficient	Std. Err.
Group Average	-4.0025	(3.7892)
Group 1997	-0.8182	(3.3060)
Group 1999	-13.3975*	(11.9942)
Group 2001	-6.9698***	(1.5744)
Group 2002	-19.3027	(41.3723)
Group 2004	-0.3579	(3.2498)
Group 2005	1.7401	(9.1876)
Group 2006	8.1784**	(3.0933)
Group 2007	-1.9670	(1.3325)
Group 2008	-4.8484	(3.2227)
Group 2009	5.7149*	(3.2353)

Notes: Standard errors in parentheses. The covariates included are Natural Resource Endowments, Industrial and Political indicators and Stringency.
*** p<0.01, ** p<0.05, * p<0.1

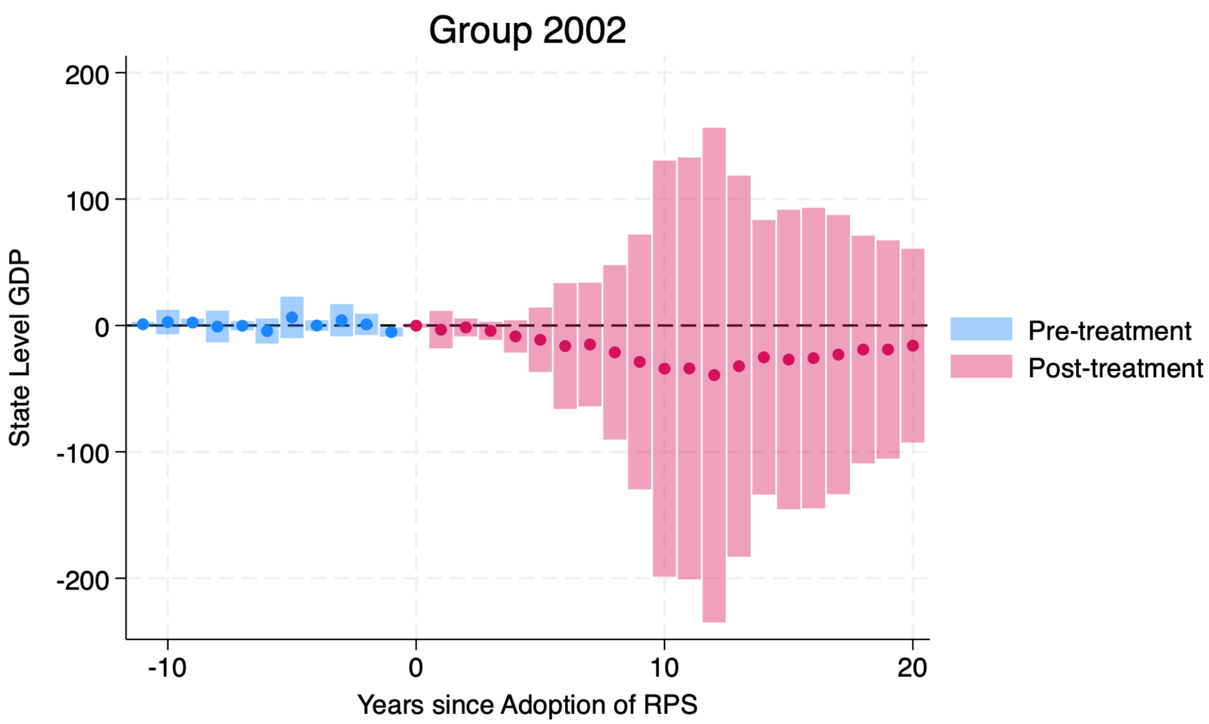

Figure 12: Group 2002: Dynamic impacts of RPSs on GSP

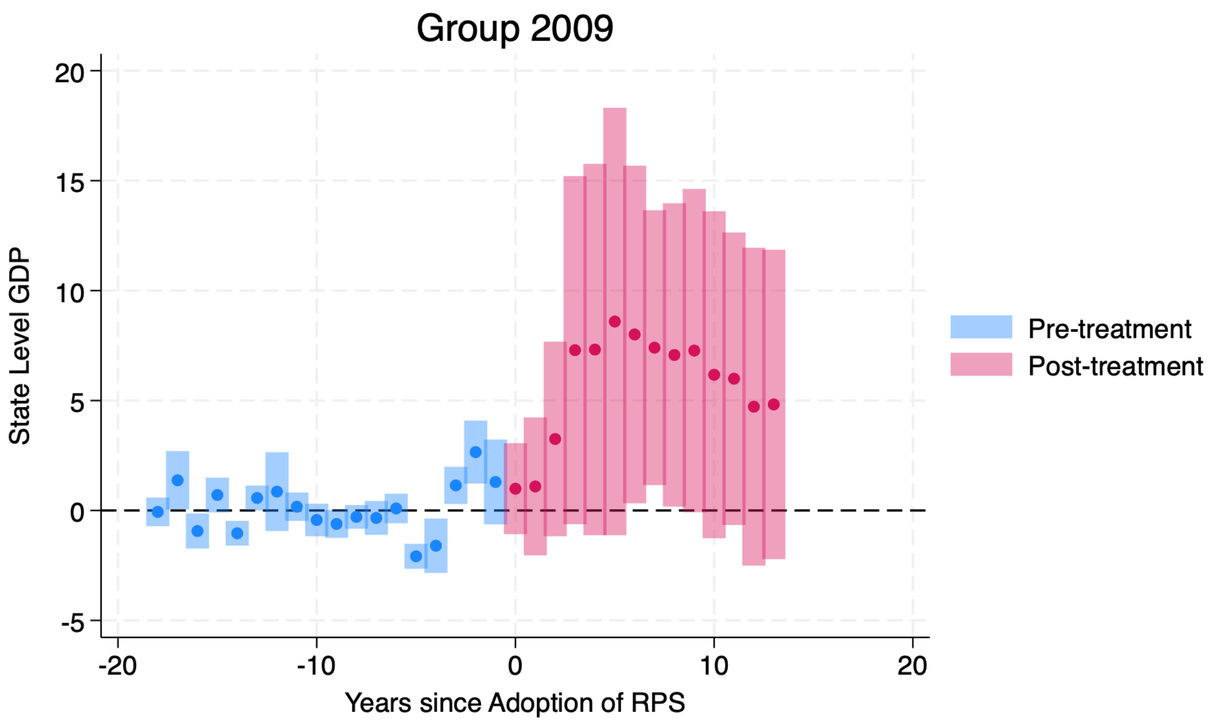

Figure 13: Group 2009: Dynamic impacts of RPSs on GSP

10.2.3 SDID

Table 11: Unit Weights By Non RPS States and Adopting Cohort(Year) for all RPS States

State	1997	1999	2001	2002	2004	2005	2006	2007	2008	2009	2015
Alabama	0.044	0.059	0.049	0.029	0.049	0.000	0.000	0.000	0.021	0.061	0.068
Arkansas	0.057	0.064	0.056	0.032	0.077	0.000	0.000	0.078	0.042	0.029	0.052
Florida	0.058	0.055	0.065	0.101	0.067	0.000	0.002	0.110	0.069	0.013	0.077
Georgia	0.057	0.139	0.134	0.125	0.121	0.169	0.394	0.192	0.188	0.041	0.033
Idaho	0.006	0.043	0.000	0.007	0.008	0.000	0.000	0.000	0.031	0.085	0.044
Indiana	0.053	0.062	0.063	0.077	0.035	0.000	0.058	0.071	0.110	0.136	0.047
Kentucky	0.041	0.038	0.049	0.032	0.062	0.000	0.000	0.003	0.060	0.068	0.046
Louisiana	0.084	0.000	0.041	0.075	0.017	0.000	0.000	0.023	0.000	0.002	0.097
Mississippi	0.062	0.076	0.084	0.016	0.058	0.000	0.000	0.000	0.060	0.075	0.061
Nebraska	0.055	0.036	0.019	0.000	0.019	0.048	0.000	0.000	0.000	0.060	0.051
North Dakota	0.130	0.023	0.039	0.007	0.017	0.000	0.000	0.046	0.025	0.032	0.030
Oklahoma	0.025	0.024	0.031	0.071	0.066	0.000	0.000	0.014	0.000	0.089	0.066
South Carolina	0.057	0.073	0.081	0.093	0.073	0.000	0.000	0.019	0.094	0.102	0.079
South Dakota	0.045	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.002	0.087
Tennessee	0.072	0.124	0.105	0.050	0.105	0.252	0.128	0.103	0.086	0.062	0.050
Utah	0.049	0.099	0.077	0.067	0.079	0.000	0.172	0.155	0.110	0.055	0.001
Virginia	0.048	0.085	0.104	0.140	0.145	0.531	0.245	0.183	0.102	0.002	0.109
Wyoming	0.055	0.000	0.000	0.078	0.000	0.000	0.000	0.001	0.000	0.085	0.001

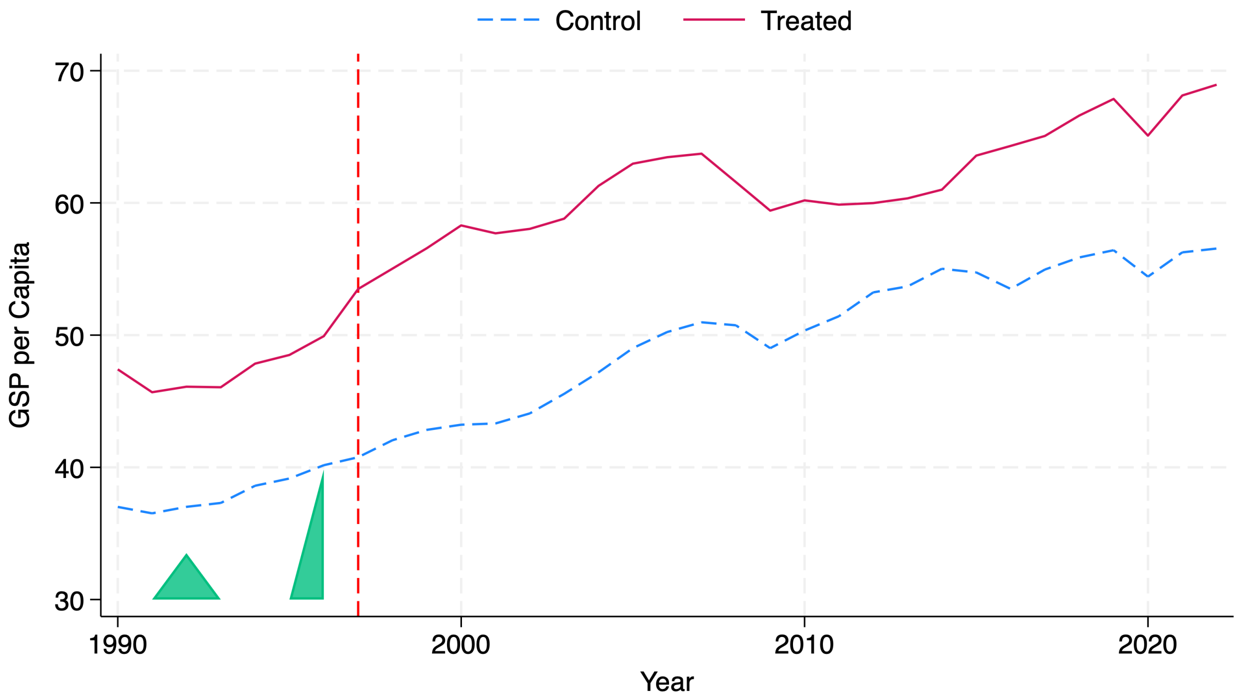

Figure 14: Cohort 1997: Impact of RPS Adoption on GSP

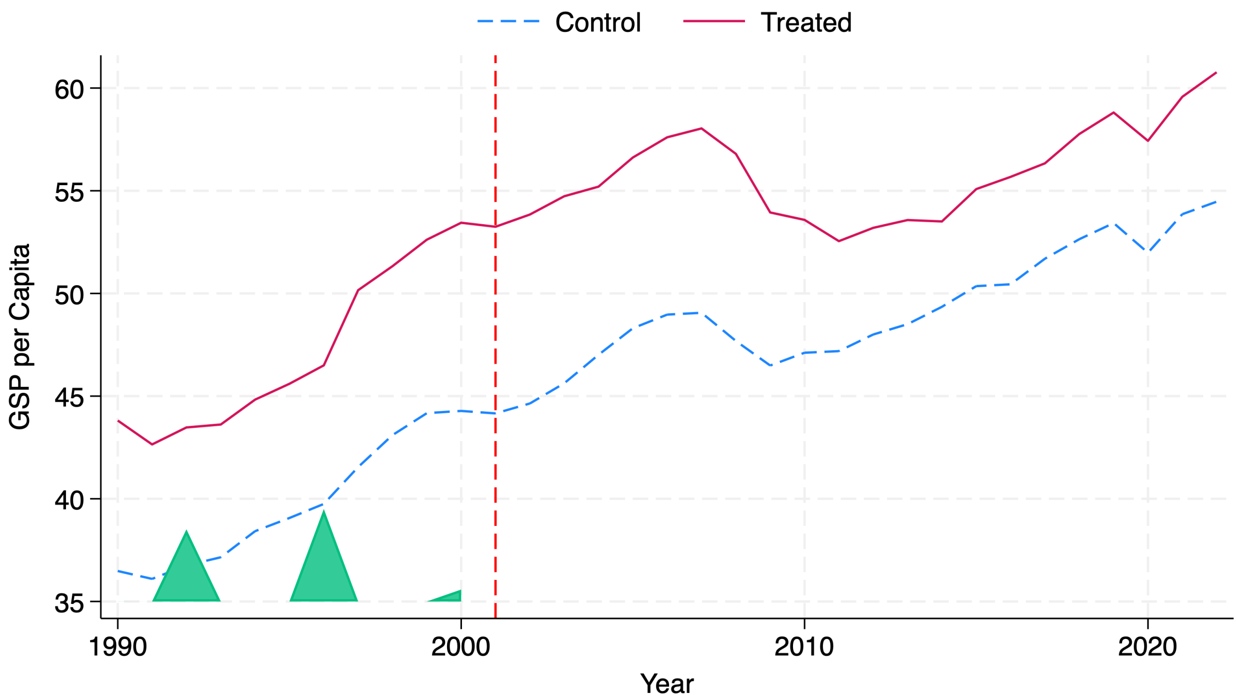

Figure 15: Cohort 2001: Impact of RPS Adoption on GSP

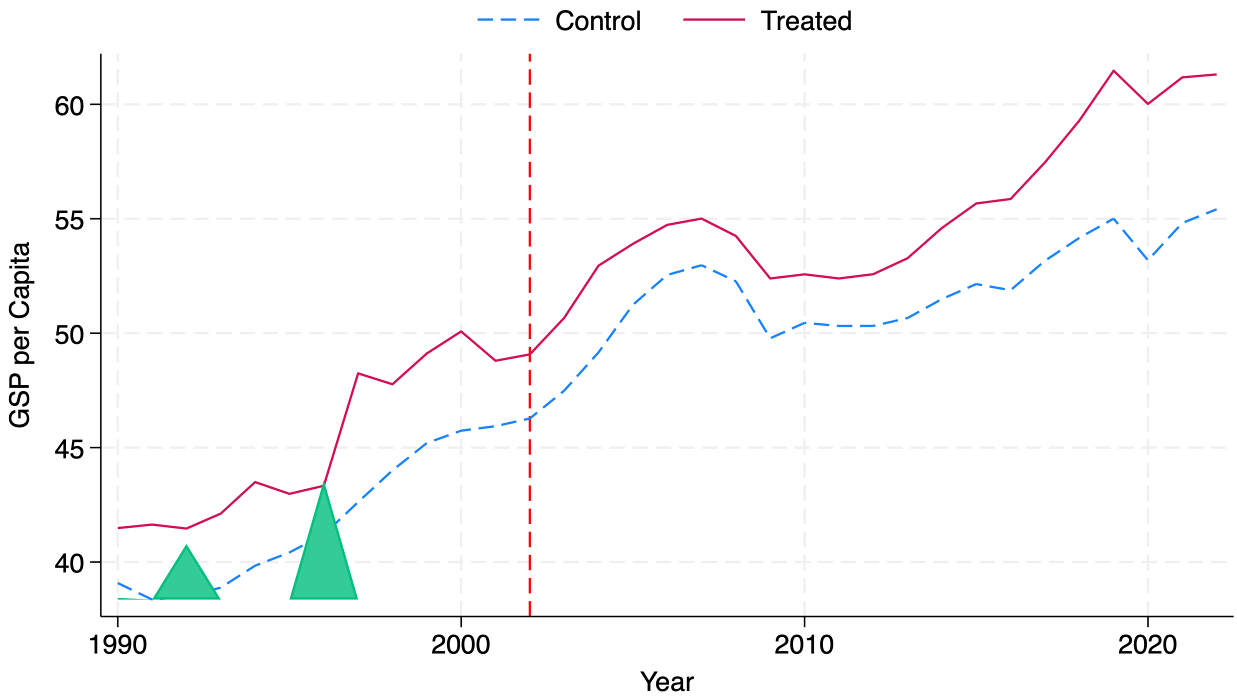

Figure 16: Cohort 2002: Impact of RPS Adoption on GSP

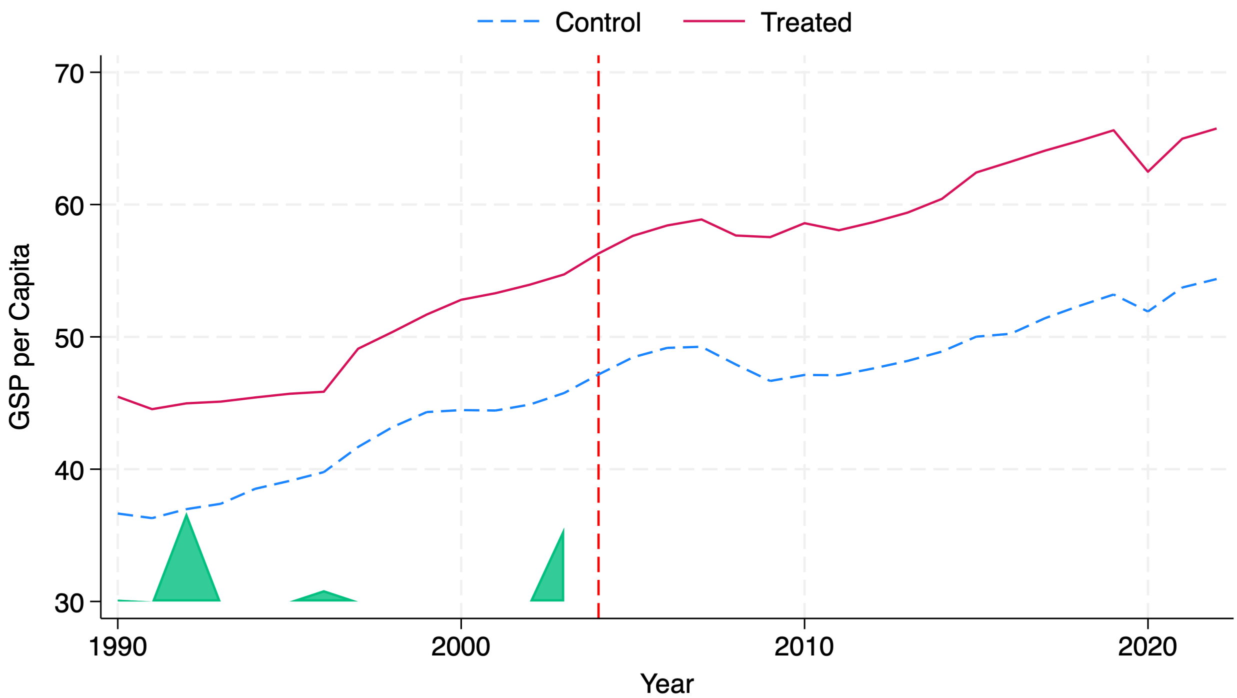

Figure 17: Cohort 2004: Impact of RPS Adoption on GSP

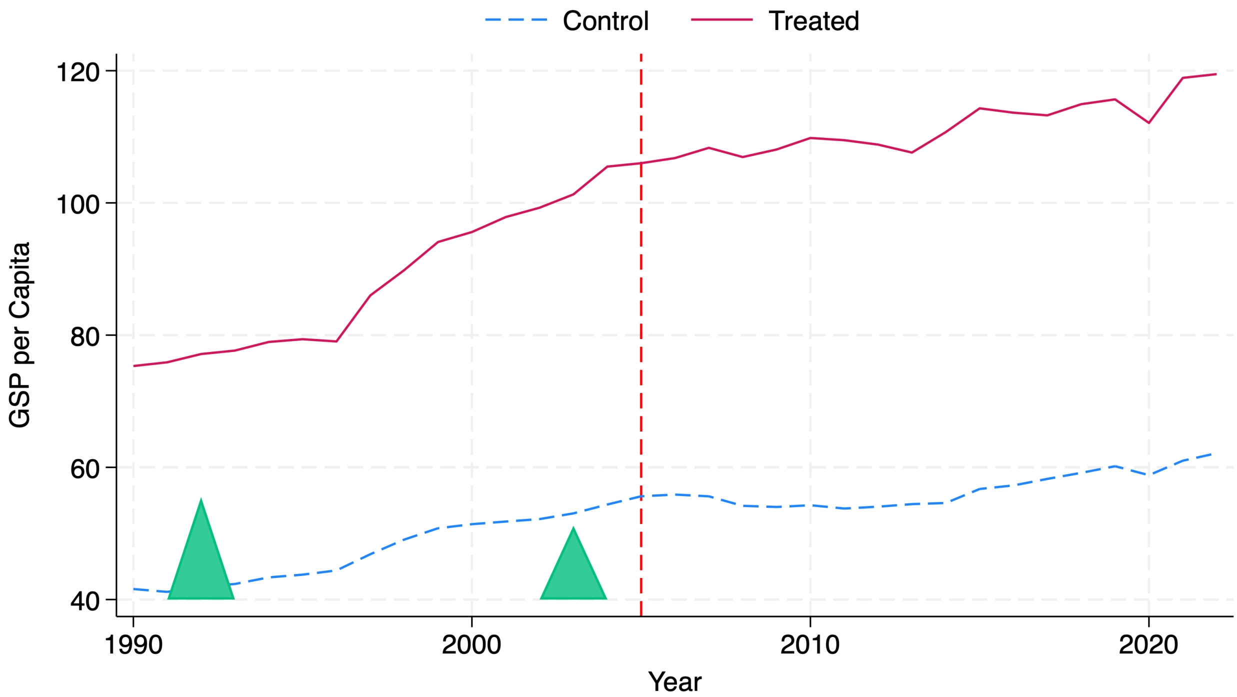

Figure 18: Cohort 2005: Impact of RPS Adoption on GSP

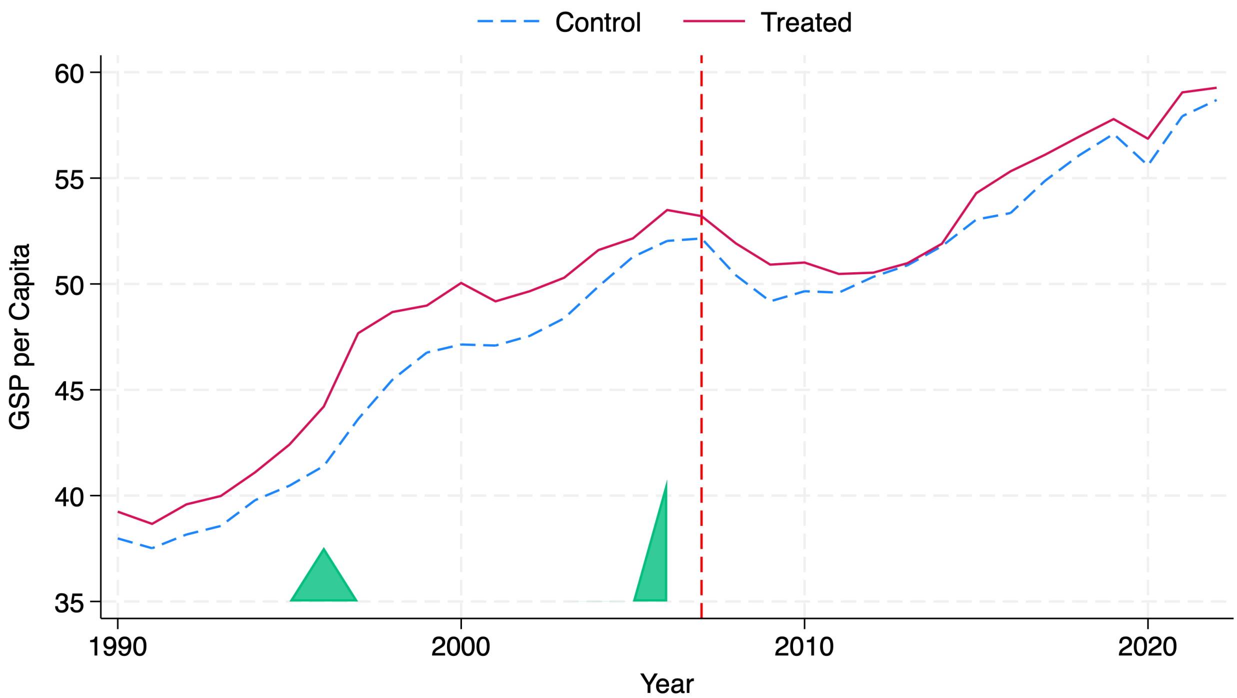

Figure 19: Cohort 2007: Impact of RPS Adoption on GSP

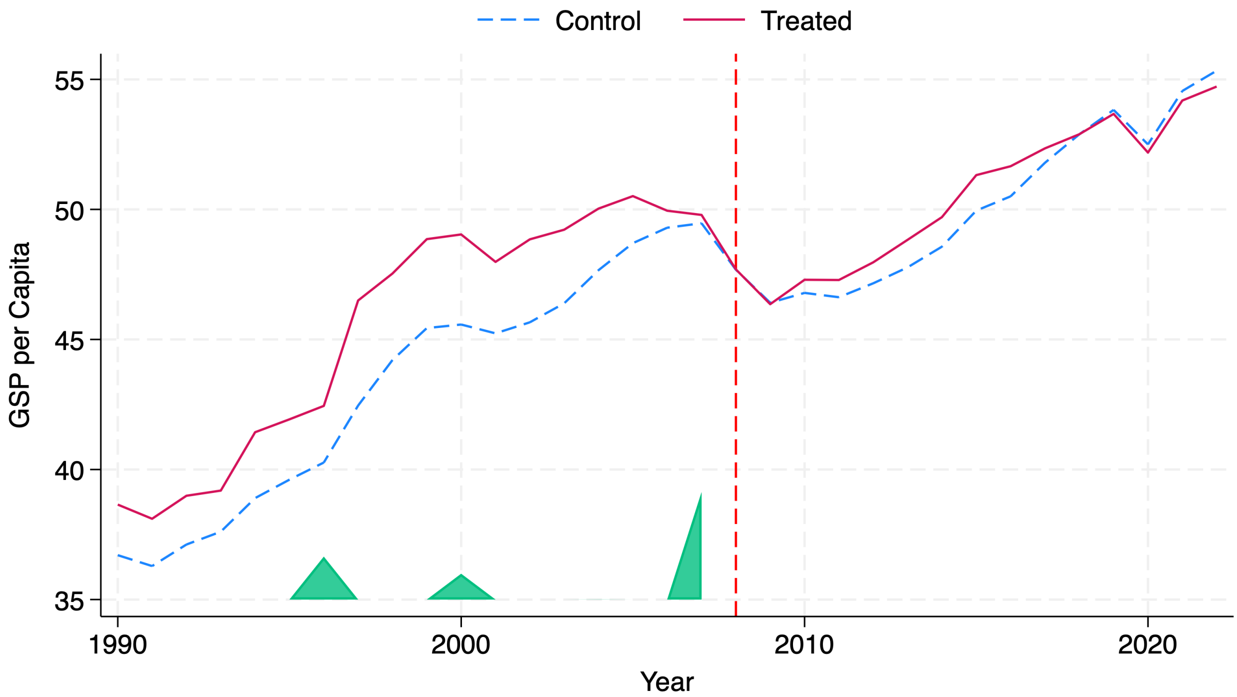

Figure 20: Cohort 2008: Impact of RPS Adoption on GSP

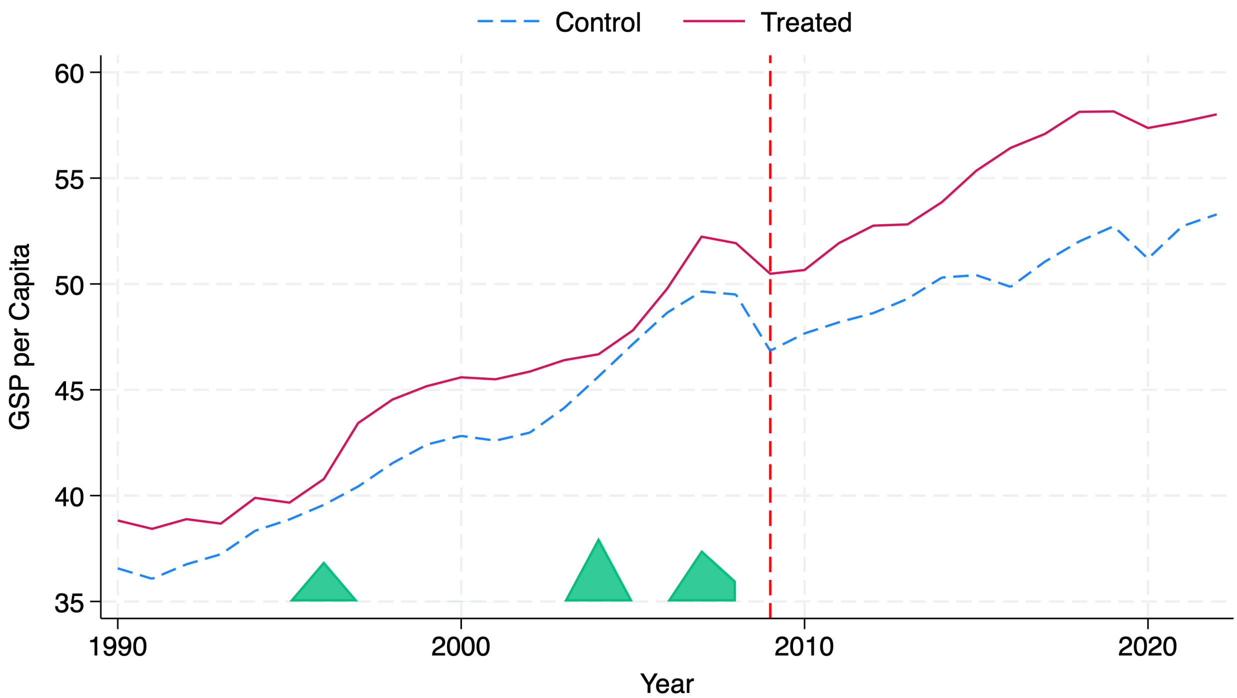

Figure 21: Cohort 2009: Impact of RPS Adoption on GSP

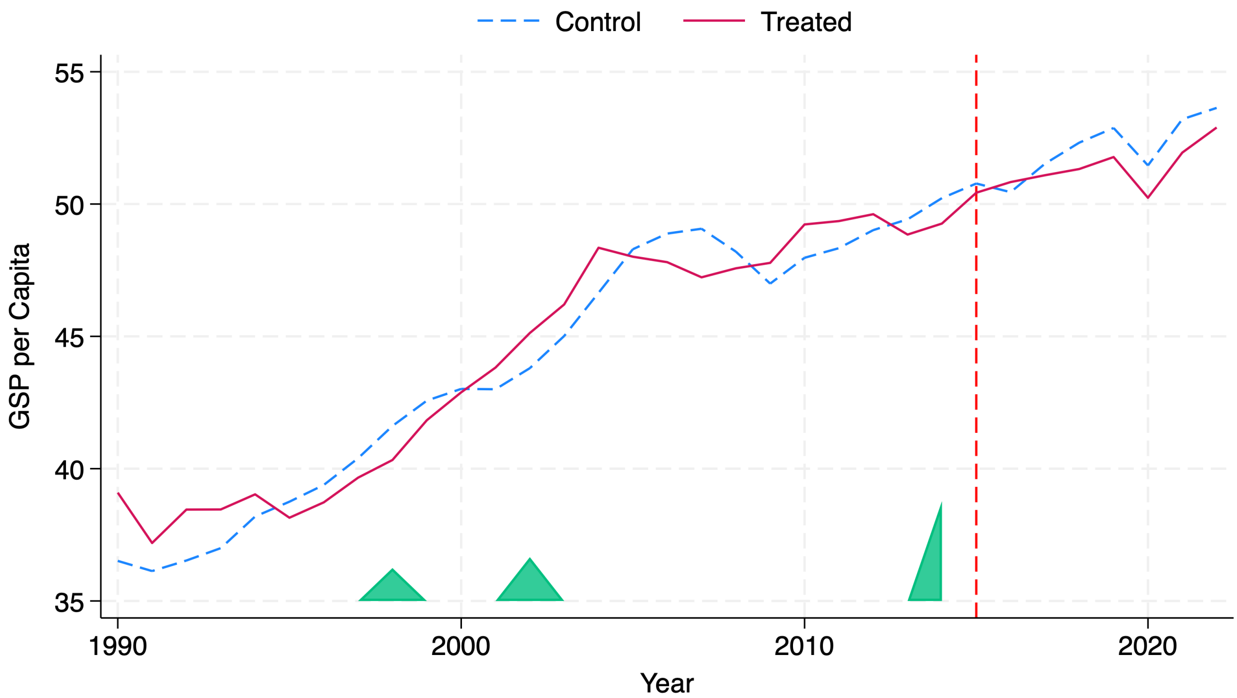

Figure 22: Cohort 2015: Impact of RPS Adoption on GSP