The Effects of Water Abstraction Fees on Non-public Water Abstractions: Evidence from German County Data

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Abstract

Water abstraction fees (WAF) have gained importance for policymakers in efforts to conserve water. This work investigates whether the German fee was effective in bringing about water savings by analyzing county-level data from non-public actors from 2007-2019. Using a frequentist Difference-in-Differences and Bayesian Regression design, I find that implementing a fee in two German states, along with rate changes in existing legislation, did not significantly impact abstractions. However, sizeable estimates might hint that a WAF implementation in two German states possibly reduced abstractions in the magnitude of a double-digit percentage range. The negative estimates remain fairly stable over time. Moreover increases in the WAF Rate were generally not significant in effect and ambiguous in nature. On the one hand, positive frequentist and Bayesian estimates seem to point toward increased abstraction levels. Also for higher rates there do not materialize significant savings. On the other hand, an event study for the rate change gives rise to a potential short-term savings effect. Further, the study provides a generic inelastic price elasticity estimate for the German context that lies lower than more elaborate designs have yielded.

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1 Introduction

Declining Groundwater levels have called for the widespread attention of policymakers. The case of Saxony-Anhalt which adopted strict abstraction limits for summer months in 2019 illustrates how pressing the issue of water scarcity and the sustainable handling of the resources has become in Germany. With 15 of 20 billion m^3 total abstractions, non-public abstractions account for almost 75 % of all German abstractions [Bundesanstalt für Geowissenschaften und Rohstoffe, 2019]. Therefore they are of particular importance with regard to possible conservation incentives. Governments and Institutions have increased efforts and developed strategic frameworks for respective areas such as with the European Water Framework Directive (WFD) in the European Union or the national water strategy in Germany. Its implementation however is the responsibility of the Member States and it is not uncommon that this is delegated to the state level as is the case for Germany. This leads observers to characterize systems as somewhat slow and ineffective, to display significant complexity, and to differ considerably by country or even region [Berbel et al., 2019,p. 822].

One example of an instrument deemed suitable to do so is the water abstraction fee (hereinafter WAF). In the German context, such a fee is the *Wasserentnahmeentgelt* (WEE) a fee legislated on the state level that is levied on the abstraction of groundwater and surface water. The state's authority in its legislation has led to observable differentials in the degree of implementation. Current fee heights vary widely from 0.25 Cents per m^3 for fishing operations in Bremen and Saxony-Anhalt m^3 to 31 Cents per m^3 Berlin in 2022 *(Grundwasser Regelsatz)*.

Politicians regularly link the fee to effectiveness in promoting water saving which has led me to the main question of whether the WAF is empirically affecting water savings. Variation in the WAF design is ubiquitous, but the base rate legislation can be broken down into two key general insights in the implementation and regarding its variation over time: three states newly adopted a rate in the time of 2007-2019 and a total of eight states adjusted a preexisting rate upwards. These two observations have motivated me to structure the analysis according to five research questions to illuminate the effectiveness of the WAF: First, I want to analyze how the implantation of a WAF by a state affected water abstractions. Linking to this, I investigate in the second question whether a potential implementation effect varies over time. Then I seek to answer the third question of whether a change in rates was able to affect water abstractions and fourthly whether this is potentially more effective for a change towards a higher rate level as one might assume. Furthermore, I would also like to investigate whether this effect of a changed rate varies over time which will be my fifth question to elaborate.

Academic literature such as Olmstead [2014, p. 502] has pointed out, that the responsiveness of water demand to price changes is more pronounced for economical activities compared to Private water demand. This is because household activities are generally assumed to cover more rigid basic needs while commercial agents have more potential to choose less water-intensive production technologies (or crops in the case of Agriculture) or substitute for cheaper ways of supply in some cases. This is the main reason why I concentrate on the non-public area as I hope that if any effect can be identified it should be more visible in this setting. The focus on non-public abstraction is thus taken as the most directly attributable variable to firm-level water usage. I use a dataset of German non-public county-level groundwater abstractions over the period 2007-2019 provided by the German regional statistical offices.

In line with approaches taken in relevant literature such as Bruno and Jessoe [2021] first a basic price elasticity to the rate is calculated in a Fixed effect model controlling for observables. Trying to condense a clear picture of the effect, I then limit my approach to the identification around the border context of two states that adopted a WAF, Rhineland-Palatinate, and Saxony-Anhalt, where I analyze the impact a WAF adoption has had on abstractions. I do this with a traditional frequentist Difference-in-Difference design but expand on this with the development of a Bayesian linear regression which promises a more nuanced view of the uncertainty around our estimate by effectively simulating multiple draws of the data that we observe and lets us update on our knowledge via Bayes-rule.

The main contribution of this work lies in its empirical nature to investigate the effect in the German context. Up until now most studies of the German WAF have been qualitatively and lacked a appropriate research design to establish a reliable estimate for the effect [Umweltbundesamt, 2011, Möller-Gulland et al., 2015, Gawel and Bretschneider, 2016]. Further, it adds to the existing literature investigating the impacts of a WAF and thus pricing measures in the field of industrial water consumption by giving insights on the German perspective but also providing results under a different estimation method that captures uncertainty better.

To approach these posed questions I will give a short description of the background of WAF in the general European framework, the effectiveness of other pricing policies in comparable settings, and the German context illuminating its design and the underlying water resources abstracted in Section 2. In Section 3, I continue on the data background as well as the methodological approach to my empirical analysis. Then Section 4 presents the results found and evaluates its robustness to differing model specifications. Subsequently, these are debated and reflected in Section 5 before I conclude on the questions posed in Section 6.

2 Background Literature Review

2.1 Background of Water Abstraction Fees

Even though private demand is often accordingly priced, Water is in many commercial contexts treated as a common good. This has led to observations that are typical in such contexts as namely water being inefficiently overused and excessively polluted. A whole strand of literature has analyzed how market failures occur in settings of common resources and negative externalities [Provencher and Burt, 1993]. The concept commonly known as a Pigouvian tax has been widely acknowledged by economists as a tool to internalize the occurring costs of over-consumption efficiently [Pigou, 1920]. This is achieved by assigning a cost for each unit of consumption or pollution. A prominent and timely example is the pricing of carbon emissions, which has become crucial in the fight against climate change. While the theoretical validity of such a tax tool has been long approved, its practical effectiveness in promoting sustainable handling of scarce resources has recently become more apparent. For instance Gugler et al. [2021] illustrates how a such tax in the sphere of UK power generation has effectively reduced carbon emissions. Although water abstraction fees are usually also charged to cover service costs incurred, this shows how such a tool could also contribute to more efficient utilization at an environmental-economic level.

Pricing policies for water abstraction follow differing implementations via a fee or tax scheme. Despite a clear conceptual difference between fee and tax instruments, we subsume comparable measures that address firm-level groundwater water abstraction here by the concept of taxes or fees identically to allow study effectiveness across the country and study settings. This shall enable us to provide a broader comparison of German WAF to policies conducted elsewhere.

It can be noted that WAF policy frameworks can be described and evaluated according to their underlying goals and influencing factors. Water abstraction taxes have been associated with a set of objectives that can be categorized according to two perspectives. First, they aim at recovering all occurring costs of water services while ensuring universal access and second promote the efficient usage of water resources. The orientation of this work is mainly towards the latter aspect of environmental conservation [Montginoul et al., 2015, p. 142]. Moreover, the taxes differ according to factors influencing prevalent water resources such as climatic conditions, precipitation, and surface and groundwater resources. Commonly, commercial Water abstraction taxes are higher for groundwater than for surface water and higher in scarcer regions. Furthermore, a common theme to the system is a general non-applicability to agriculture. Even though France, Italy, and Portugal countries have systems in place that charge water abstraction by agriculture its taxation is very low and often heavily subsidized. Alongside Germany, many countries/regions display substantial volumetric exemptions to agriculture which oftentimes effectively free the sector from any burden [Berbel et al., 2019].

For a large part of this analysis, I abstract from an exact consideration of the existing exemptions as these cannot be made comparable at all or only with substantially increased complexity due to their partly individual design.

2.2 Effectiveness of WAF policies in differing contexts

Studies have investigated the effectiveness of price mechanisms to bring about water savings in different country set-ups. One interesting case is the experience of the national groundwater tax (GWT) in the Netherlands around the turn of the millennium which was a national charge (last rate of 0.20 €/m^3 in 2011) on groundwater abstractions (also containing varying exemptions). Simultaneously there was a lower tax (avg. 0.014 €/m^3) addressing abstractions on a provincial level. The national tax was adjusted throughout the years but was finally abolished in 2011. The evaluation regarding its effectiveness in promoting water savings was considered limited. Schuerhoff et al. [2013] note that even though abstractions sunk to some extent it was incapable of bringing about noticeable savings and were rather useful in its funding function. The effect on abstractions was concluded to be small and rather attributed to shifting from ground to surface water resources. However, the authors admit that this was also obscured due to a lack of before-after observations and the inability to isolate the effect in the presence of other taxes [Schuerhoff et al., 2013, pp. 1,2,9] [Schuerhoff and Hellegers, 2015, p. 251].

As a direct measure reflecting the responsiveness of water demand to such a tax scientific analysis has often estimated differing price elasticities. Overall literature seems to have rallied around the insight that water demand is to be considered rather inelastic however dependent on the industry sector and varying to the context. The general nature of these inelastic estimates is explained by the fundamental necessity and the absence of any clear substitute for water in human activities. Their values seem to range in the sphere from -0.1 to -0.98. The significant heterogeneity is driven by sector-specific differences. Values at the higher absolute end are defined by especially water-dependent sectors such as car washes and chemical services while lower values stem from sectors as less dependent such as rubber and plastics [Olmstead, 2014, p. 502] [Renzetti, 1992, Reynaud, 2003]. Some of these results that seem most applicable to the setting of the WAF in Germany shall be presented in more detail hereinafter.

For a sample of French firms Reynaud [2003] shows that overall industrial water demand is inelastic and that a firm's water demand can be divided by production process. The study finds that in particular, the part covered by own abstractions is not responsive to price changes. In their work analyzing annual industrial firm data from 1994-1996, the author takes up the fact that water is entering the production process in different ways. Therefore, the demand is split up into parts that are covered through the utility provider (network), own water abstractions (autonomous), and water treatment before use. In their methodical approach firms minimize the occurring costs from these inputs. In this context, Water abstraction fees are a strong determinant of water costs. The authors show that especially the part of the network water demand is sensitive to price changes but not so much the autonomous withdrawals the measure most closely resembled by our dataset. For water covered by utilities, an increase in price by 10 % is associated with a decrease in water consumption of 3 % (-0.29). According to the author abstraction water demand is elastic to production output but not so water price changes. This is linked to the explanation that own abstractions are used for a limited number of particular important functions in the production process and are thus not easily substituted.

These results indicate doubts about the effectiveness of a tax (fee) change on autonomous non-public abstractions that are of high relevance in the German industrial context and the effectiveness of water abstraction charges in general as an efficient measure to incentivize water savings.

However, some recent results deducted in agricultural backgrounds under research designs that exploited natural experiment variation might indicate that the effectiveness of a tax is higher than previously thought.

Adding to the numerous estimations of price elasticities the work by Bruno and Jessoe [2021] investigates the effect of an introduction of a volumetric abstraction charge in the California farming context. This approach taken somewhat closely resembles ours as the study similarly uses a DiD design of a before-after implementation design. It investigates the impact of an extraction-based charge of 36.50$ per acre-foot (approx. $0.030\$/m^3$; 1 acre-foot = 1233.48 m^3) in the east region of *Coachella Valley Water District* that was adopted in 2004. They find that based on monthly data the introduction of a fee was reducing Groundwater abstractions by 24-26 % and an elasticity of -0.46 when using annual data [Bruno and Jessoe, 2021, p. 10]. It is observable that when analyzing annual elasticities the value is supposedly larger which is in line with the intuition that that an effect is somewhat different over time.

Smith et al. [2017] and Burlig et al. [2021] support this reevaluation of WAF potential as they find indications for a more elastic demand and substantial effectiveness of policy adoption in the farming contexts in Colorado and California. Smith et al. [2017] used a Difference-in-Diffrences design to investigate the impact of a groundwater tax 45$ per acre-foot (approx $0.036\$/m^3$) that was adopted in only one sub-district of the *San Luis Valley* in 2011 and find that controlling for relevant covariates the tax reduced withdrawals significantly by 32 %. Further, they derived in -0.77 a more elastic demand than calculated in other studies [Smith et al., 2017, pp. 1006].

Recent results by Burlig et al. [2021] support this impression. In their work, the authors use variation in electricity costs of pumps as an Instrumental Variable to estimate the causal effect of groundwater water price on demand for 2011-2017 in California. This exploits the fact that the main cost determinant of groundwater extractions is the cost to operate the pumps. They estimate an elasticity of -1.12 which is equally far more elastic than estimates of earlier studies. Simulating the outcomes of an imaginary groundwater tax of 10$ per acre-foot ($0.008\$/m^3$) the work shows that groundwater extractions could be reduced by about 27 % indicating a bigger potential of such a modest tax to promote water conservation in agriculture.

Results from a recent tax reform in China may suspect that it contributed to increasing groundwater efficiency for industrial water users where savings seem to be driven by a substitution mechanism. Ouyang et al. [2022] investigate in a case study the effectiveness of a recent reform that replaced the existing fee-based mandatory tax-based charging system. Based on data from 10 provinces where the reform was rolled out they quantify a short-term 9 % reduction in groundwater use in descriptive numbers. This effect is bound to regions that were over-exploiting existing resources and thus received higher tax rates. Due to insufficient available data and a less robust research design, this estimate is limited in any causal interpretation and if any qualitative. More important are the two theoretical mechanisms of how a tax can facilitate savings that the authors identify. A higher tax directly reduces total demand for water by lowering agents' purchasing power, referred to as the scale effect, or it fosters substitution, consumers switching to cheaper sources of water. Both effects should negatively impact groundwater abstraction. Based on fieldwork the authors derive that Industrial agents are among the most impacted by the reform and substitution effects are in fact pronounced while scale effects seem of minor importance.

The evaluations of the WAF effectiveness in Germany have been mostly local, equally descriptive, and obscured by interaction with other factors, e.g. adoption of water-saving technologies in energy production. Generally, its potential to incentivize water savings has been acknowledged. Hence, various papers have indicated that substitution effects are expected to be significant for German industrial water use. However, Literature fails to provide empirical estimations of such effects and establish clear causal relationships [Möller-Gulland et al., 2015, p. 61] [Umweltbundesamt, 2011, p. 120] . This empirical evaluation gap is to be addressed by this work. This can help domestic policymakers as well as interested private parties to evaluate the impact of the WEE on environmental savings that is often cited. Furthermore, this may expand the existing water management literature by adding to the limited number of works done outside the context of agriculture and providing evidence from the German policy perspective.

3 Data & Methods

3.1 Data

To answer the posed questions the regional database of the German Statistical Offices provides rich datasets on annual non-public water abstractions which will serve as the basis of the analysis. This contains county-level observations by source (surface and groundwater) and the associated number of companies registered for such water abstraction. Germany is structured on this level in 403 officially registered county units which consist of 296 counties (*Landkreise*), 106 independent cities (*kreisfreie Städte*), and three special municipal units (*Kommunalverbände besonderer Art*). These are collected every three years starting for non-public abstractions in 2007 up until 2019. For this reason, the analysis is to be limited to this period.

Dependent Variable - Non-public Groundwater consumption

To measure the impact of a WAF policy we decided to focus on firm water usage as it seemed more elastic than private water consumption which is in most parts covering people's basic needs [Olmstead, 2014]. In Germany it is common practice for companies of a certain size to operate their own extraction plants and thus cover their water needs themselves. Therefore, water consumption by companies is mainly covered under the non-public water supply. The main extractors of water for economic usage are electricity producers, mining companies, and the manufacturing sector. Overall non-public water abstractions have been decreasing steadily over the last 30 years. Especially the adoption of water-saving technologies to produce energy and increased costs of water service and disposal have been the main drivers of this reduction [Möller-Gulland et al., 2015, p. 61] [Umweltbundesamt, 2011, p. 121].Whereas Groundwater is the main source of water abstraction in agriculture (76%), industry and manufacturing only derived a limited part of its water demand from groundwater. In 2016, 11% of water used came from groundwater, while the predominant source was surface water, accounting for 86% of usage. This reflects the common practice of utilizing rivers and lakes for cooling in production processes or for generating energy. Nevertheless, I lay the focus of this work on groundwater for two reasons: first, there is not sufficient data available that collects surface water abstractions in frequency and granularity needed to allow for robust statistical analysis. Second, the WAF in Germany is most commonly defined for groundwater. Fee implementations on surface water are far less frequent and its application is far less consistent than for groundwater at all and thus lack comparability. My basic dependent variable is based on the groundwater abstraction quantity in German non-public water extraction. The variable is measured in 1000 cubic meters. It is adjusted for the number of firms that engaged in such abstractions outside the public water supply system, so-called, *Eigengewinnung*. Due to right-skewness in the abstraction data, the dependent variable is log-transformed (See Figure 1). Thus my main variable of interest is logged per-firm abstractions.

Independent Variable - WAF binary fee & rate

The German *WEE* is a water abstraction charge implemented in 13 out of 16 states associated to provide incentives for water savings and recoup costs. The design, amount, and exemption regulations differ in each state, often considerably. While Surface water abstraction is free of charge in Berlin, Hamburg, and the state of Saarland in the other states values range from 0.1-5 C/m^3 Groundwater charges vary a lot and are generally higher, oftentimes threefold or more. The highest value can be found in Berlin where ever since its adoption it is charged at 31 C/m^3. The lowest current value is 0.25 C/m^3 for fishing operations in Bremen and Saxony-Anhalt. High values are usually considered to be above 10 C/m^3 [BUND, 2019]. Over the last 20 years fees were abandoned such as in Hesse, adopted such as in Rhineland-Palatinate or Saxony-Anhalt, or changed in rate as examples from Hamburg or North Rhine-Westphalia illustrate. The analysis builds on parts of this variation in the treatment data and will describe these changes in more detail. From inspecting the current (2019) set of legislation we observe that Different economic uses of water are priced

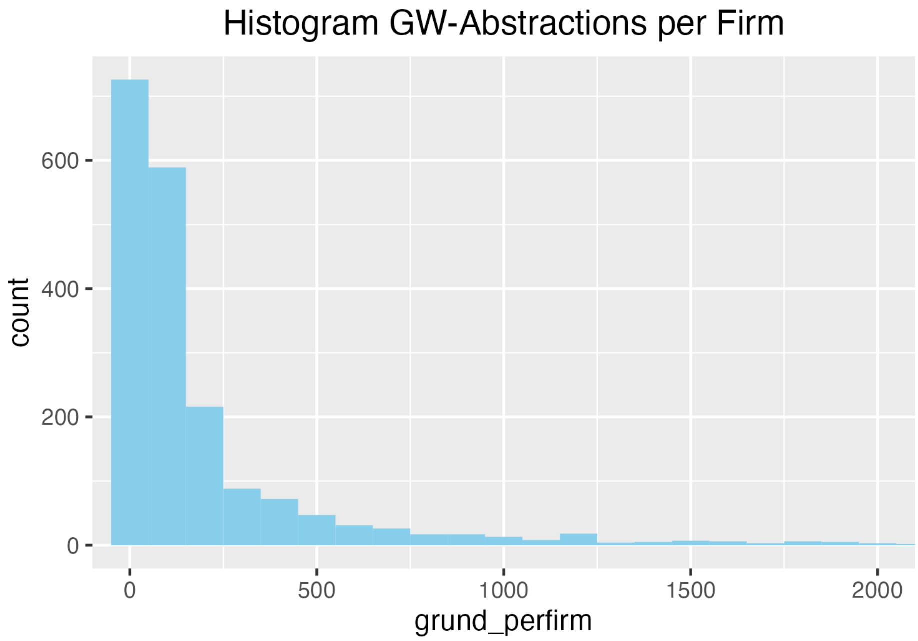

Figure 1: Distribution of Groundwater Abstractions per Firm

differently when aggregated over states. Agriculture is usually charged lowest with a mean rate of 8.25 C/m^3 followed by cooling production facilities at 8.87 C/m^3, companies abstracting drinking water of 10.32 c/m^3 as Water companies or industrial Food producers and Mining is 12.26 C/ m^3 [BUND, 2019, pp. 5-8].

Exemptions for differing economic sectors are widespread but also vastly differ between states. Agriculture, Mining, and Hydropower creation are often in large parts exempted. The example of Rhineland-Palatinate is striking because it currently exempts large parts of industrial production. A tough stance is taken in the City-states Berlin and Hamburg that refrain from substantial exemptions. Furthermore, many state legislation allow exemptions for ecological measures such as groundwater remediation. Discounts are used by some states as an incentive mechanism to implement water management systems, up-to-date technology or facilitate circular water use[BUND, 2019, p. 11]. A more detailed consideration of these and also the use of funds that are often bound to ecological uses will be foregone at this point.

Each of the three-year abstraction data points had thus to be linked to information on the existence and the height of a WAF on groundwater at the county level in the respective year these serve as the main independent variables in the form of a binary treatment and a continuous WAF rate that is logged for the elasticity estimation. As regulations and rates range vastly depending on the state, we drew on the groundwater general rate (*Regelsatz*) as my comparable benchmark. The information concerning the existence and details of the WAF for the years 2007-2019 were collected from overview articles such as by the BUND [2019] the Umweltbundesamt [2005, 2011, 2022], IHK Pfalz [2013] and IHK Brandenburg [2016]. For values for which uncertainties remained, the existence and the level of the WAF were supplemented by relevant legislative documents. Detailed information can be found in the Appendix A2.

Covariates

The main outcome variable water abstractions is assumed to be determined by relevant hydrological, economic, and urban circumstances in the underlying counties. This choice of controls is similar to approaches taken in literature such as Bruno and Jessoe [2021, p. 9] For the estimation of a price elasticity measure additionally it is adjusted for political conditions as a potential confounder. One can make the assumption that water pricing policies implemented and thus the adoption of a fee is especially determined by the political conditions [Conca and Weinthal, 2018][Merrett, 1999]. The data and the intuition behind the inclusion of these variables shall be illustrated.

To derive a price elasticity in a two-way fixed effects model selecting on observables we must adjust for potential confounders. These include the political conditions of a county, measured by the percentage of green votes in the last state election. In addition, we suspect that the implementation of a fee is highly politically driven as an example from NRW shows: amid the 2000s while led by a conservative-government plans to abolish the fee were advanced but then stopped with a change in the political landscape. To model this influence we use the percentage of votes of the Green Party in the last election as we assume a higher result to increase the likelihood of the establishment or upward adjustment of the fee. The political situations might also shape water abstractions by way of executing economic or environmental policies that influence water demand.

Data describing the economic and political situation, the degree of urbanization and the prevalent surface water area are drawn from the regional statistical offices in Germany which collect these at the county level and over the relevant period. We assume that higher economic activity is affecting water abstractions positively as productive activities be agricultural, energy, or industrial are water intensive. Among the GDP data available, I use the indicator derived by the calculation of origin (*Entstehungsrechnung*). To adjust for area and population differences in counties we thus adjust for population density. The reasoning for how it could affect the dependent variable is diverse. Abstraction quantities in more densely populated areas could arguably expected to be higher e.g. because of a higher number of production facilities. Conversely, it could be argued that water-intensive industries such as Agricultural operations, Mining firms, and Energy producers are operated in less densely populated areas which would mean higher abstractions in those units.

Researchers such as Bruno and Jessoe [2021, p. 9] have pointed out Groundwater extractions are also determined by the availability and the extent to which surface water is used as a substitute. The Surface water abstraction data in my dataset is not sufficiently available for my study period. Therefore, we use the surface water area data, which is also provided by the regional statistical offices, as a proxy for availability. It has to be noted that the data point for 2007 is not available and the respective value for 2008 was chosen. Furthermore, as firms decide according to Reynaud [2003, p.220] to either extract water themselves or use other sources e.g. the public supplier we adjust for the quantity that the firms draw these captured by the *Fremdbezug* variable in the initial dataset.

To control for county-level differences in the precipitation we use data from the *DWD*. Precipitation can somewhat proxy the ability to refill existent groundwater bodies due to seepage and also periods of low precipitation might indicate a higher need for abstractions from groundwater[Bruno and Jessoe, 2021][Smith et al., 2017]. However, precipitation is not collected on the county level but merely by its set of individual stations. Therefore it was linked to counties on the basis of regular expression matching which was linked to a register of communities per county. This allowed us to extract precipitation data on the county level for about 350 observations for each of the five periods.

Distribution of Variables and Handling of Missing values

To establish whether transformations of variables are necessary for the application of later methods that impose assumptions, most to be normally distributed, we inspected the distribution of the variables in the data set (see Figure A2). As noted before Abstractions for ground and surface water are right-skewed. The same can be concluded for rate height, 'Green votes', GDP, population density, and precipitation. For all right-skewed variables, logarithmic transformation is conducted. Thus I obtain an unimputed dataset of 1730 observations for 403 counties over the five time periods from 2007-2019 and data on respective covariates. Missing data points on all relevant covariates were imputed with statewide means for the first part that involved frequentist statistical analysis. In the section where we turn to the development of a Bayesian Regression model these are masked and sampled from the observed dataset.

3.2 Methodology

In the first part, we derive a price elasticity measure with an FE model controlling for observables in line with approaches taken in literature [Bruno and Jessoe, 2021]. To answer the research questions posed a Difference-in-Diffrence research design with varying specifications is the workhorse modelling approach. Building on this main body we then extend towards an ML estimation technique and run a Bayesian regression model selecting on observables.

3.2.1 Price Elasticity of Water Demand

For the estimation of the elasticity we specify the model according to

$$ln(Y_{it}) = \alpha_0 + \alpha_1 ln(rate) + \alpha_2 ln(green\_votes) + \delta_s + \gamma_t + X_i + \epsilon_{it} \tag{1}$$

where Y is the logged firm-level extractions for county i in year t. These are modeled by the logged rate for state s and year t. Here δ_t denotes year and γ_s state fixed effect. Relevant covariates, which include precipitation, population density, economic production, surface water area, and water drawn from other sources (e.g. public water supply) all on a logarithmic scale, are captured in X_i

per county. It is clear that we might lack data on other existing unobserved confounders which bias the relationship. Therefore the estimation of the elasticity is rather to provide a general indicator for the German context and allow for a very broad comparison to measures derived in other studies as its estimation is custom to any investigation of this type.

3.2.2 Effect of WAF Implementation

Basic Model Specification The main body of the work consists of the analysis of the effect of WAF adoption in the German context. To give insight into the first question regarding the implementation of a fee the possible control group consists of observations for states that do not have such a policy as Bavaria, Hesse, and Thuringia. For states with an implementation of a fee Rhineland-Palatinate (RPF), Saxony-Anhalt (SA), and the Saarland are suitable candidates in the time period 2007-2019. Due to the fact that there are no abstraction data points for the Saarland for 2007 and 2010 these disqualify for a before-after comparison. As the implementation time of SA is 2011 but the first post-observation is 2013 as is RPF we must treat 2013 as the first period after the implementation. This way we assume here that the effect is equally strong even though the first year of an effect would be 2012 for SA which is not collected in the data. This is a somewhat debatable assumption that has to be considered in the limitations of the insights drawn from this.

The identification strategy I choose is oriented on the one done in the well-known paper by Card and Krueger [1993]. As one can observe each RPF and SA share a common border with a state where no Tax is adopted, namely Hesse (HE) and Thuringia (TH). Thus I suspect that identification of the effect of a fee should be visible around the border of the states. To fine-tune the identification strategy I focus on the basic set-up of counties that are within a hypothetical benchmark of 50km to both sides of the RPF-HE and the SA- TH border. (*The counties considered are marked accordingly in Figure A1*). This wider definition of the border context is done in order to ensure a sufficient number of observations. We pool these observations under the assumption that both should be similar in overall characteristics. Thus we end up with a set of 45 counties over a total of 5 periods which provides us with a set of 225 county-by-year observations in the basic set-up.

In my initial approach to answer the first question regarding the effect of having a fee, I use especially the variation in the policy implementation of RPF and SA. I use a canonical 2x2 DiD approach with time and state fixed effects comparing before to after the implementation in 2013.

The Basic DiD model specification is described by the following term:

$$ln(Y_{it}) = \alpha_0 + \alpha_1 T_i Post_t + \delta_s + \gamma_t + X_i + \epsilon_{it} \tag{2}$$

Here my dependent variable are log-ground water abstraction quantities per firm for different counties at different times, T_i denotes the treatment dummy for counties having a fee, $Post_t$ is the treatment timing dummy that turns and stays one with the implementation period in 2013 for all following periods. δ_s and γ_t capture state and time-fixed effects and X_i denoting relevant covariates on county-level. We account for state and time-fixed effects in order to avoid state-level unobservables and yearly shocks to confound the relationship. Further, we analyze the impact to the results to the inclusion of relevant covariates to the dependent variable.

To test the robustness of the results we run the model under a reduced and an expanded control group set. Namely in a second specification, we compare only the counties directly at the border in the RPF-HE and TH-SA context. Expanding the number of considered observations we compare RPF counties with all HE counties. Here we do not include the SA-TH context because parallel trends seem implausible.

Time-varying effect

Further, we investigate the dynamics of the treatment effect by running multiple treatment-control comparisons as the control group remains stable in this setting.

$$ln(Y_{it}) = \alpha_0 + \sum_{i=1}^{5} \alpha_t T_i T_t + \delta_s + \gamma_t + \epsilon_{it} \tag{3}$$

where T_t denotes a time dummy that is one in the respective period but zero in all others and T_i denotes further whether treatment occurs in that particular period. Thus we effectively compare the difference between treated and control at every period. This way individual estimates for before treatment and after treatment onset are obtained. This allows us to infer about the pattern before the treatment administration as well as determine how the treatment effect develops the first, second, and third time period after the implementation of the fee in 2013.

Assumptions

The DiD relies on two crucial assumptions to derive causal statements. First Control and Treatment Observations must follow a common pre-treatment trend. This is because the control should be a good model for how the treated if it would not have been treated. Therefore trends should not be significantly different before the fee was implemented. This is tested graphically as well as with Prior trends and Placebo Tests for each specification. While prior trends test whether trends are statistically different the placebo tests simulate a setting in which the policy would have been conducted at another point, in this case in 2010, and estimate respective effect sizes. Secondly, we must assume that there are no other policies conducted around the implementation of the WAF affecting the control group meaning there were no other treatments conducted that affected the relationship around the point of policy adaption in 2013 [Huntington-Klein, 2023, Ch. 18]. This requires research into whether other policies were conducted in the states which is done in the relevant section.

3.2.3 Effect of WAF Rate Change

In a second approach to provide insight into the first question, we model the rate changes in itself as a categorical treatment as the independent treatment variable against the abstractions. The specification of the model is analogous to the one under the gee implementation just that T_i indicates whether a state changed its rate between 2007 and 2019. To assess the question of treatment heterogeneity, whether a higher fee can increase water savings we use a dichotomous interaction by cutoff to classify a high rate. In this case, We investigate the impact of a changed rate, so the treatment is no longer having the fee or not, but having conducted a rate change. Thus the dataset consists of observations for all states that have implemented a fee which is consisting data on 13 states, dividing it into a total of 9 states that changed its rate (treated) and 4 that did not. However, I finetune the approach by focussing on states that had a fee throughout the entire period thus excluding the RPF, Saarland, and SA, and those that only did one change in 2007-2019 as multiple subsequent adjustments e.g. by Mecklenburg-Vorpommern and Hamburg make it difficult to define the event and thus attribute the corresponding effect. Namely, this leaves me with the states of Brandenburg, Schleswig-Holstein, Lower Saxony, and North Rhine-Westphalia where fees were increased once and Baden-Wuerttemberg, Saxony, Bremen, and Berlin where there was no change in the base rate.

3.2.4 Rate Change: Continuous Interaction

To assess the question of treatment heterogeneity I interact the treatment indicator of a rate change with the continuous rate itself. This interacted model is described by the specification

$$ln(Y_{it}) = \alpha_0 + \alpha_1 T_i + \alpha_2 T_i rate_i + \alpha_3 rate_i + \delta_s + \gamma_t + X_i + \epsilon_{it} \tag{4}$$

where $rate_i$ is the denoting the rate for each observation. In this setup, α_2 is the test for the interaction effect. Differentiating this term with regard to T_i will give us the marginal effects while the average treatment effect will have to be calculated under the usage of the mean (log) rate observed.

3.2.5 Rate Change: Event-Study

Going beyond the average effect of a rate change we use a staggered event-time design to investigate the time-varying nature of the effect. Here we use the fact that for the 4 states where we observe a rate change, this is implemented at different points from 2007 through 2019. To analyze this we use an event-study design that can be described by the model:

$$ln(Y_{it}) = \alpha_0 + \sum_{\tau=-T}^{-1} \alpha_\tau W_{i,\tau} + \sum_{\tau=1}^{T} \alpha_\tau W_{i,\tau} + \delta_s + \gamma_t + X_i + \epsilon_{it} \tag{5}$$

$W_{i,\tau}$ being here the event-time dummy for when the first rate change occurred. e.g. $W_{i,-5}$ being one in event time =-5 and zero in all others. We thus, center each group (state) to its period when the first rate change occurred. To avoid forbidden comparisons we use the robust Callaway and Sant'Anna DiD-estimator from the 'did'-package which allows use for the deduction of group-time treatment effects [Callaway and Sant'Anna, 2021]. We compare those in the basic specification and specify the control group as the not yet treated counties. The package requires us to use county-fixed effects in this analysis. Further, it is not controlled for the rate in this specification as uptake of the rate led to failure in the estimation due to multicollinearity.

3.2.6 Bayesian Regression Model

Up until now, we have used frequentist techniques to analyze the problem posed. However, a different approach to statistical inference is the Bayesian Analysis. To illustrate this approach it is important to clarify the difference between the so-called frequentist concept and the Bayesian one. Under the frequentist approach, the observed data is random and varies every time we observe it. However the parameters, the effect f.e. are fixed and they can be described by one number, which is the point estimate we get. The problem can thus be described by a function. Thus we explain the data y given the parameters β in the form

$$f(y|\beta) \tag{6}$$

Under the Bayesian approach is not the parameter that is fixed but the data. The intuition is that the moment we observe it, it is not changing anymore. The parameters however are inherently uncertain and handled as random variables. The foundation of Bayesian Statistics is inference about the distribution of a parameter, in this case, the effect size, given the data that we observe. Bayesian statistical analysis makes entirely probabilistic assertions thus giving us an inherently better picture of the uncertainty in the estimate. It is described conditional on the data what is the parameter:

$$p(\beta|y) \tag{7}$$

While in frequentist statistics the method to estimate the parameter can vary, e.g. MLE, in Bayesian this is only done according to Bayes-rule:

$$p(\beta|y) = \frac{P(y|\beta)P(\beta)}{P(y)} \tag{8}$$

P(β) is called the prior, which captures the preexisting knowledge about the effect. This is one of the main advantages of Bayesian statistics is that it is able to reflect preexisting knowledge or belief of an effect. P($y|\beta$) is the so-called sampling distribution or likelihood of the observations which is the extent to which we update the existing belief [Gelman et al., 2013, p. 7] (Notebook). $P(y)$ is the probability of the data, which is integral over all β values :

$$\int P(y|\beta)P(\beta)d\beta. \tag{9}$$

The design of Bayesian regression models is done in the three steps described by Gelman et al. [2013, p. 3]: The specification, the calculation of the posterior and the model check against the observed sample. In the specification of priors, parameters, data, covariates and missings are assigned a probability distribution, Generelly my methodological implementation of Bayesian Diffrence-in-Diffrence draws a lot of insight from the approach taken in Liu et al. [2022, p. 11] that apply a comparable approach in the setting of automotive software testing. The specification of the model follows the form from the frequentist DiD:

$$ln(Y_{it}) = \alpha_0 + \alpha_1 T_i Post_t + \delta_s + \gamma_t + \sum \alpha_{X_i} X_i + \epsilon_{it} \tag{10}$$

Therefore prior values for the intercept, covariate coefficients and the error term are drawn from the Normal distribution. We choose a weakly-informative prior for intercept and covariates, meaning that they are drawn from a normal distribution [Liu et al., 2022, p. 3]. By the choice of $\sigma_t = 1$ the prior is chosen very broadly as we allow for the fact that we do not know much about the effect of the implementation.

$$\begin{align}
\sigma_{\text{prior}} &= 1 \tag{11}\\
\alpha_0 &\sim \mathcal{N}(0, 1) \tag{12}\\
\alpha_1 &\sim \mathcal{N}(0, \sigma_{\text{prior}}) \tag{13}\\
\alpha_X &\sim \mathcal{N}(0, 0.1) \quad \text{for each covariate in } X \tag{14}\\
\epsilon_{it} &\sim \mathcal{N}(\epsilon | 0, \sigma^2) \tag{15}
\end{align}$$

I assume Normal distribution for all standardized variables except the standard deviation after we inspect the distribution of the relevant variables and impose the necessary transformations. The standard deviation is Half-normally distributed as it can only take positive values.

$$\begin{align}
\sigma &\sim \text{HalfNormal}(1) \tag{16}\\
ln(Y_{it}) &\sim N(ln(Y_{it}) | \sigma) \tag{17}
\end{align}$$

Then the posterior distribution of the effect parameter is calculated after observing the data. Lastly, the model is checked for its fit against the data we observed.
Overall this approach does give us a distribution of plausible values of the effect size and reflects the inherent uncertainty. It is generally better suited to derive credible results under smaller sample sizes. Furthermore, the Bayesian Framework allows us to impute missing observations with samples from a distribution [Vincent, 2022]. To do so values on confounders were standardized and masked beforehand.

Markov Chain Monte Carlo Sampling

Solving the Bayesian Probability analytically is often impossible which is the reason why numerical solutions become the favorable solution. With recent improvements in computation technology, it has become possible to calculate the probability of the data $P(y)$. This is done with approximation by MCMC Sampling algorithms which has fuelled developments for an increased use of Bayesian regression models. MCMC sampling combines fundamental principles of Monte Carlo Sampling and Markov chains stationary. Here I will display a general explanation of the process that vastly oversimplifies the inherent complexity to illustrate the general idea of this methodology to readers. On the one hand, it consists of Monte Carlo sampling which infers about a parameter by drawing random samples from a distribution and learns about from the draw just previously obtained. Markov chains on the other hand are processes that describe a chain of states which transition from one state to another with connected transition probabilities. Among the properties of such a chain is that the next state is dependent on the state just before that. Given this it can be shown we end up in a situation where such a chain goes on for extended amounts of state we approximate the real underlying probabilities that each state occurs. We reach a stationary distribution.

Translated to the distribution this means we draw samples of the effect estimate which are the states of the chain. Given the property, we should arrive at one particular (probability) distribution. This sampled distribution should then have the exact properties of the underlying 'real' probability distribution. Because of the Markov property, every sampling step afterwards then should result in the same probability distribution, which we converge to and we sample from this over and over to simulate as if we were collecting many different observations on the imposed relationship. This illustrates in highly superficial terms the goal of this method. We use in the approximation a No-U-turn Sampler that allows for efficient and fast sampling.

All data was extracted in CSV files from the Website of the respective provider. DiD models were conducted in R (version 4.2.2) with mostly fixiest functionalities. For the Bayesian regression, the implementation was executed in Python (version 3.9.7) with functionalities from the pymc-package. Code implementation was oriented on the work by Vincent [2022]. For the Bayesian Estimation of the Price Elasticity and the Rate change, I decided to perform less computationally intensive processes. The elasticity was calculated using 500 samples executed on 1 core while the rate change was calculated by 500 draws using 3 cores. For the main fee adaption, a more resource-extensive estimation consisting of the default 1000 draws leveraging on 4 cores was done. All analysis files as well as relevant data files are accessible under the following Link to my Github Repository.

4 Results

4.1 Descriptive Statistics

Reviewing the entire context of 2015 possible county-by-year data points we observe 1730 German counties per year abstractions. On average a county abstracted about 292.410.00 m^3 per year. The data is heavily skewed to the right (see **??**). This indicates the plausibility for a log transformation that was confirmed by the graphical inspection of the overall distribution for all variables this deemed

Table 1: Descriptive Statistics Germany (Per-county)

Variable	Obs	Mean	SD	Min	Max
Groundwater Abstractions	1730	292.41	1370.74	0.75	33306.88
log-GW Abstractions	1730	4.27	1.39	-0.29	10.41
log-GW Abstraction (T=0)	630	4.20	1.23	0.10	7.93
log-GW Abstraction (T=1)	1100	4.32	1.48	-0.29	10.41
GDP	2005	7296525.16	11398580.28	944789.00	157130744.00
Precipitation	1759	834.42	214.21	351.10	2142.50
Surface Water Area	1998	1962.16	3133.38	24.00	52531.00
Population density	1997	537.48	696.05	35.60	4777.00
External water supply	1911	272.10	1271.38	1.38	27308.17

Note: Groundwater abstractions and External water supply are per firm and measured in 1000 m^3, GDP in 1000€, Precipitation as a annual sum at station in mm, Surface water area in m^2,Population density in people/km^2; External Water supply consists of public supply and water acquired from other sources such as Companies or organisations. T denotes whether counties had a fee (1) or did not (0)

relevant. Comparing logged abstractions by the existence of a WAF, we note that Abstractions seem higher in counties with a WAF. I confirm that 1100 county-year observations of states that have a fee show a mean of 4.32 compared to 4.20 in all 630 county-year data points without a fee. Also, the distribution of the fee rate is highly skewed to the right due to the high rates in Berlin and Hamburg. From 2007-2019, fees ranged from 1.8 Cents in Mecklenburg-Western Pomerania (2010) to 31 Cents/m^3 in Berlin. The mean rate lies at 6.5 Cents while the median rate is at 0.06 Cents/m^3. Investigating the evolution of mean rates in each state over time it can be noted that rates throughout Germany have increased from 7.0 Cents/m^3 in 2007 to 10.2 Cents/m^3 in 2019 (See Appendix A3).

Our dependent variable is positively correlated with GDP (0.13), Population density (0.2), and external water supply, our measure that incorporates public water demand (0.39). Marginal negative correlation I find with precipitation (-0.04) (See A6). For correlations in the border comparison, we find a weak positive relation between our binary treatment and logged abstractions per firm (0.17). Furthermore, our dependent variable is positively correlated with external water supply (0.40), population density (0.35), log-GDP(0.28), and rather weakly with available surface water (0.07). The correlation with precipitation is again negative but very weak (-0.04) (See Appendix A5).

4.2 Price Elasticity

For the elasticity calculation, the dataset's dependent variable was imputed with state-wide means which led us to a data frame containing a total of 1997 observations. The results of our fixed effect model of the price elasticity are according to Table 2. If we ignore the geographic structure of our dataset and only include year-fixed effects it seems that the WAF is associated with higher abstractions (1). This is what we have seen above in the descriptive measures. However, incorporating these state-fixed effects my results suggest a 5.37% decrease in groundwater abstractions given a 1% increase in the WAF rate (3). Also, a decrease of 11.09 % or an increase of 0.52 % is compatible with my data given the assumptions.

Table 2: Price Elasticity of Water Demand

Dependent Variable:	log-GW Abstractions				
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
log_rate	0.0273	-0.0524*	-0.0537*	-0.0598*	-0.0558
	(0.0187)	(0.0243)	(0.0218)	(0.0256)	(0.0331)
Fixed-effects					
year	Yes	Yes	Yes	Yes	Yes
state		Yes	Yes		Yes
id_c				Yes	
Observations	1,997	1,997	1,997	1,997	1,277
R^2	0.01238	0.18112	0.28482	0.73175	0.32275

*Clustered (year) standard-errors in parentheses; Signif. Codes: ***: 0.01, **: 0.05, *: 0.1 ; Note: The full output table can be found in the Appendix.*

The size of the estimate seems to hold constant for the exclusion of covariates and the political confounder (2), county fixed effects (4) as well as reducing the dataset to only counties of the 13 states with a fee (5). Estimating the Elasticity under a Bayesian framework we find that a 1% increase in the rate is associated only with an approximate 1% decrease. Also here increases of 0.5 % and decreases of 2.1 % are reasonably compatible with our data thus refraining from rejecting the hypothesis of a zero effect at the 5%-level (See Table 3).

If we compare the elasticity to estimates presented in the literature this result is far lower and more inelastic than in other circumstances. Yet as noted before this price elasticity estimate is more of a generic indication as the relationship is likely to be confounded by variables not present in our dataset such as groundwater replenishment or land use. Given our primary interest in assessing the causal impact of the policy tool WAF, we concentrate on instances where states adopted WAF

Table 3: Summary Elasticity - Bayesian Regression

Variable	Mean	SD	hdi_3%	hdi_97%	r_hat
log_rate_gw	-0.009	0.007	-0.021	0.005	1.000
log_gdp	0.082	0.033	0.019	0.145	1.000
log_mean_precip	0.024	0.023	-0.019	0.067	1.000
log_pop_density	0.083	0.038	0.015	0.159	1.000
log_externalwater	0.303	0.024	0.257	0.344	1.000
log_perc_gruene	0.002	0.025	-0.044	0.047	1.000
log_sw_area	0.215	0.033	0.149	0.275	1.000

legislation after previously having none. Therefore the goal in the following section is to determine whether this adoption significantly influenced water abstractions.

4.3 Effect of WAF Implementation

For the main analysis, I examine the border context of Rhineland-Palatinate and Saxony-Anhalt (hereinafter RPF & SA) and compare counties within a distance of approx. 50 km around the state frontiers. My dataset consists of 225 observations from a total of 45 counties over 5 years. These are structured in 115 untreated control and 105 treatment group observations. From briefly inspecting the set of covariates it seems that surface water resources seem more abundant in treated counties while economic production is higher in untreated Hesse & Thuringia (hereinafter HE & TH) counties. Turning to our main dependent variable it is visible that absolute mean abstractions are 217.340 m^3 in the treated RPF and SA counties higher than the 102.370 m^3 in the control group of HE and TH border counties. In logged terms the number of water abstractions per firm which is higher in counties with a fee and lies at a value of 4.18 (1.57) compared to 3.85(1.26) in the control group. Further comparing the states-wide numbers it becomes apparent that counties in SA have the highest abstractions (5.41), followed by HE (4.27), RPF (4.09), and lastly TH (3.56).

Table 4: Descriptive Statistics RPF SA by Treatment

Variable	Mean (T=0)	SD (T=0)	Mean (T=1)	SD (T=1)
GW Abstractions	102.37	137.32	217.34	486.52
log-GW Abstractions	3.85	1.26	4.18	1.57
GDP	8064639.34	12526628.90	4826385.01	2825239.16
Precipitation	749.46	135.63	710.77	144.74
Surface Water Area	776.20	569.13	1284.82	1148.54
Population density	667.67	748.71	548.00	663.50
External water supply	5885.30	16972.26	6244.42	17684.36

4.3.1 2x2 DID: Main Specification

The results from our specification in Equation 2 are presented in Table 5 where the first two columns specify the reduction in absolute numbers while the rest is expressed in log-terms.

The results of my main specification (4) suggest that the implementation of a WAF was associated with a 40.8% ($e^{-0.525} - 1$) decrease in firm groundwater abstractions in the RPF-SA context. Nonetheless, the confidence intervals indicate that differences that range from an 81% decline to an 86% increase in abstractions are also compatible with our data. If only year-fixed effects are accounted for, I find that the fee is associated with about 10% higher abstraction quantities (5) (95% CI:-6%; 28%). This is again reflecting the difference that is observable in the descriptive statistics. Once state-fixed effects are incorporated the effect turns negative in all of our specifications. The

Table 5: DiD: Fee Implementation in RPF-SA context

Dependent Variables:	GW Abstractions		log-GW Abstractions		
	Null	Full	Null_ln	Full_ln	Year
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
treat_post	-444.8	-457.3	-0.5342	-0.5253	0.0952
	(395.1)	(287.4)	(0.4351)	(0.3614)	(0.0568)
Fixed-effects					
state	Yes	Yes	Yes	Yes	
year	Yes	Yes	Yes	Yes	Yes
Observations	225	225	225	225	225
R^2	0.17259	0.36756	0.16750	0.39758	0.35123

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: The model equation used is: $ln(Y_{it}) = \alpha_0 + \alpha_1 T_i Post_t + \delta_s + \gamma_t + X_i + \epsilon_{it}$.

negative estimate in absolute terms testifies that the adoption of a fee was associated with a reduction in water abstractions. The estimate indicates that the fee implementation was linked with a 457.300 m^3 reduction in abstractions even though also a decrease of 1.37 million m^3 or an increase of 457.000 m^3 was compatible with our data (2). Overall my main logged specification for the effect of the WAF implementation around the border of RPF-HE & SA-THU thus indicates in their estimate that a firm in counties with a fee had 68 to 41 % lower firm groundwater abstractions. However, we do not find evidence for significance in the effect as they are subject to high uncertainty denoted in elevated standard errors. These measurements seem surprisingly large and raise the need for further testing that will be explored in detail in the robustness part.

As discussed above a precondition for the validity of our DiD approach is that treatment and control observations were following a similar trend before the adoption of the fee. Parallel trends seem plausible with regard to the graphical depiction of our main dependent variable of logged abstractions before 2013 in Figure 2 as well as the results drawn from prior trends and placebo testing that express no evidence for significant differences in the pre-treatment trend of both group.

The prior trends test does not find evidence for a significant difference in pre-trends *(p=0.65)* nor does the placebo test which simulates the effect for an earlier fee adoption in 2010. Its estimate is negative but not significantly different at a 5% level *(p=0.13)*. For my absolute measure of Abstractions, Parallel Trends seem rather debatable. This underscores the notion to focus on the results derived for the logged dependent variable. Furthermore, this approach relies on the assumption of the absence of other treatments at the adoption in 2013. Based on the information available to me, it appears that there may have only been a slight increase in the wastewater levy rate in Rhineland-Palatinate, which could distort my results. However, no consistent database can be found for this and the relative adjustment is rather small as I conclude. Against this background, I assume that requirement to be credible. However, the risk cannot be dismissed that the changes

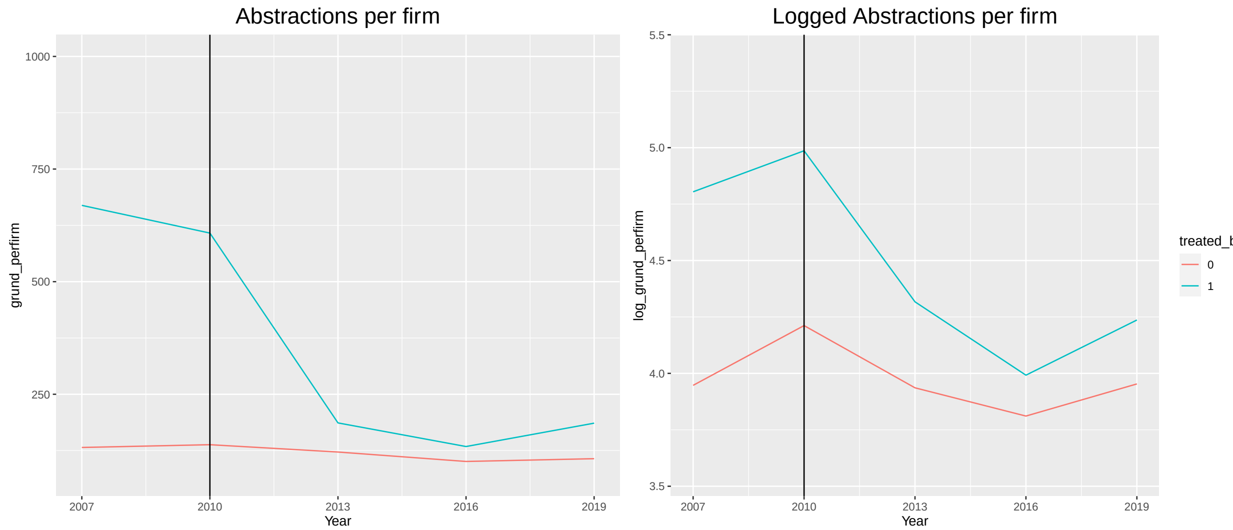

Figure 2: Absolute and Log-Abstractions 50 km RPF-SA context - Parallel Trends

to the respective WAF legislation are presented as a collection of changes, for example through simultaneous surface charges or additional exemptions, which could distort my results and limit the insight taken from my analysis.

4.3.2 Time-Varying Effect

Next, I investigate the question of whether the effect of the WAF adoption in the RPF & SA context varies over time. Especially in the light of the fact that abstraction data is only collected every three years the ATE presented before might be considered just a very broad approximation. It may be reasonable to assume that the adoption of a Fee might only set in with some delay or decay over time. Figure 3 shows the estimates graphically. Each of them is expressed relative to the reference period 2010, the last before the adoption of the fee. I find that the estimate before the adoption of treatment seems to be around zero which is in line with the common trend assumption. With the onset of the fee we can observe in the point estimate how abstractions seem to be affected negatively. In t=1 abstractions are 33% (-0.41) lower (95 % CI: -0.49;0.60). The effect estimate seems to be most pronounced in t=2 (2016) with a magnitude of a 43 % reduction (-0.566). For 2019 the absolute effect size demonstrates a 40% (-0.518) reduction a marginally less pronounced decrease. However for none of the estimates we find evidence that excludes the possibility of positive effect estimates in the Confidence Intervals thus it does not allow us to reject the hypothesis of a zero effect.

Thus I conclude that even though increases in abstractions remain plausible, the negative estimates point towards the notion that the WAF can potentially facilitate savings. Further, our dynamic measures suggest fairly stable savings over time. These observations raise the question of whether maybe my outcomes are particularly dependent on the geographical selection of RPF and SA and might differ for another selection of units. As we have seen in Table 3 choosing our

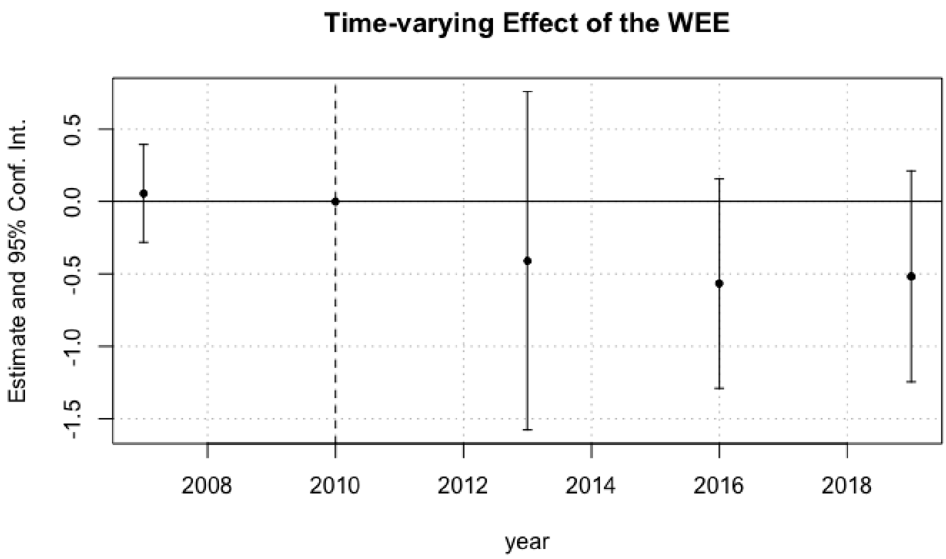

Figure 3: The Effect of a WAF Implementation over Time

set of fixed effects differently might also change the estimate significantly. Therefore especially the inclusion of county-fixed effects instead of state-fixed effects seems relevant.

4.3.3 Robustness

Testing the robustness of the results derived from the pooled RPF & SA context I investigate the impacts of choosing our geographical set differently. Therefore I reduced the dataset to counties that border directly (on RPF & SA context) (2), used the 50-km (3) and direct border comparison (4) to only the RPF (with HE) context. Further, I expanded the comparison to all counties of RPF (and HE) not only the bordering ones (5). The results of these variations are presented in Table 6.

The outcomes of these robustness tests make it apparent that the DiD estimate for the difference is very volatile. When we reduce the border contexts of RPF & SA to directly bordering counties (4) we observe that the effect appears to strengthen in size denoting a larger 63 % (1.01) reduction in abstractions. However, also 92% decreases and 77% increases are compatible with our data (95% CI: -2.58;0.57). When I limit our comparison to the RPF-HE context only, I find that the estimate reduces substantially to only 9.6%. We even establish that this estimate seems to be statistically significant from zero on a 5% level (95% CI Estimate: -0.19;-0.01). Similar to observations made before for the entire RPF & SA context, we observe that when we reduce the set to direct neighbors the effect increases in magnitude but only up to a decline of 16.4 % (-0.179; 95%CI: -2.32;1.96). Comparing all counties of RPF and HE the effect increases in size to about 26.8% (0.312; 95%CI: -1.13;0.51).

Especially with limiting ourselves to the set of only the RPF context we are worried that the state-fixed effects are capturing much of the existing variation in the data. Therefore the results

Table 6: Geographical Robustness of WAF effect

DV:	log-GW Abstractions				
	RPF & SA 50km	RPF&SA direct	RPF 50km	RPF direct	RPF all counties
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
treat_post	-0.5253	-1.008	-0.1006**	-0.1794	-0.3115
	(0.3614)	(0.4965)	(0.0073)	(0.1691)	(0.0643)
Fixed-effects					
state	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes
Observations	225	125	145	65	310
R^2	0.39758	0.38995	0.35263	0.50838	0.21683

Clustered (state) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

were tested regarding the inclusion of county-fixed effects which seem more plausible in the context of only comparing two states. Table 7 shows that our estimate again grows in absolute magnitude to 23.0 % (- 0.261) which is closer to the magnitude found in our basic specification. Nevertheless, also positive effect sizes of 36.1 % and decreases of 56.4 % are plausible.

Robustness tests seem to show large, volatile estimates. Nonetheless for most credible specifications (Table 6: 1 & 2 ; Table 7: 4) these include a zero effect. In light of these variations, it seems debatable whether the significant effect found under RPF incorporating State fixed effect (3) points towards the real existence of an effect. Most importantly county-fixed effect seems to capture the structure of our data in this two-state comparison. Further, it might be considered randomness, that as the increased number of specifications ran we come to discover some significant results eventually. This uncertainty in the results can be modeled more sensitively by exploiting the tool of Bayesian Analysis when we simulate our data observed just as one potential draw of outcomes. The results of this methodology I will explore in the following section.

Table 7: Robustness of WAF effect - County FE

DV:	log-GW Abstractions			
	RPF&SA State&Y	RPF&SA County&Y	RPF State&Y	RPF County&Y
Model:	(1)	(2)	(3)	(4)
Variables				
treat_post	-0.5253	-0.5468**	-0.1006**	-0.2613
	(0.3614)	(0.2388)	(0.0073)	(0.2780)
Fixed-effects				
state	Yes		Yes	
year	Yes	Yes	Yes	Yes
id_c		Yes		Yes
Observations	225	225	145	145
R^2	0.39758	0.84061	0.35263	0.87889

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

4.3.4 Bayesian Regression

The results for the DiD analysis using a Bayesian regression model are presented in Table 8. The coefficient d is denoting here our estimate for the fee implementation corresponding to α_1 from Equation 10. Using this methodology I find that the adoption of a fee is associated with a 24.6 % (-0.283) decrease in groundwater abstractions. However, the probabilistic characteristic of Bayesian statistics allows us to note that with a 95 % probability the effect size of a WAF adoption lies within a 52.2% (-0.739) decrease and a 23.0 % increase (0.207) in water abstractions. Thus the fee implementation does not seem to be significantly associated with abstraction reductions.

As done with Parallel Trends in the frequentist design we briefly investigate the proper functioning of the sampling process. As all *r_hat* are close to one, we gain confidence in the successful execution and convergence of the process. The inspection of the trace plots displays that the sampling process was properly functioning as the plots are stationary, well mixed indicated by condensed zig-zagging and the chains cover approximately the same region. Further, we check the model fit by establishing that most of the observed values are within the confidence intervals found by the sampling process as one can observe in Figure 4.

Table 8: Fee Implementation - Bayesian Regression DiD

Variable	Mean	SD	hdi_3%	hdi_97%	r_hat
d	-0.283	0.252	-0.739	0.207	1.000
log_gdp	0.069	0.065	-0.054	0.190	1.000
log_mean_precip	-0.059	0.061	-0.186	0.048	1.000
log_pop_density	0.183	0.073	0.047	0.321	1.000
log_externalwater	0.170	0.061	0.054	0.284	1.000
log_sw_area	0.131	0.064	0.004	0.243	1.000

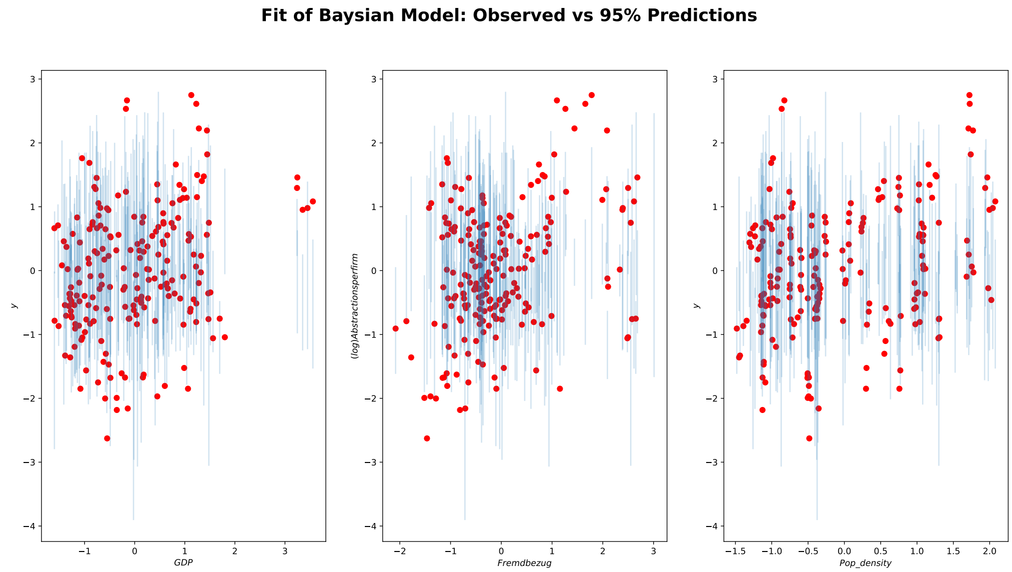

Figure 4: Checking the Model-fit by comparing Observations (red) vs 95 % Predictions (blue)

To summarize the results on the effect of a WAF implementation we have seen that the estimates are usually of negative sign and subject to substantial variation while being mostly insignificant. Frequentist estimates that reflect our panel and geographical structure point towards a negative direction of the relationship in line with intuition for a water savings incentive by adding a positive price to it. The point estimates indicate rather debatable large sizes around 40.8% in our basic context. Over time considered these savings seem relatively stable. However, we do not find evidence for the existence of an effect that allows us to reject the hypothesis of a zero effect.

The results of a Bayesian confirm the image that even though savings were not significant the fee implementation is if any associated with a negative impact in the SA & RPF context. The estimate indicating a 24.6 % reduction in abstractions points towards a negative impact and together with the robustness checks moreover suggests that my basic estimates are somewhat overstating the real size. Nevertheless, the analysis also clearly shows the great uncertainty underlying this estimate. The credibility intervals show that the actual value lies with 95% probability within a 52% decrease and a 23% increase, which is particularly wide and may give rise to questions about the specification of my priors.

As pointed out earlier there is far more variation in the design of German WAF policies than just the binary classification of whether one region had a fee or not. To understand how these varying designs might shape its effectiveness in conserving water resources, I will subsequently shed light on how rate increases have affected abstractions in the environment of eight German states.

Table 9: DiD - WAF Rate change

DV	log-GW Abstractions					
	rate8	rate6	rate8_county	rate_13	Interacted_8	Interacted_13
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
tp	0.0824	0.0816	0.1871	-0.0137	-1.131**	-0.2944
	(0.1750)	(0.1810)	(0.1622)	(0.1394)	(0.4618)	(0.5469)
ln_rate	-0.3625	-0.3540	-0.4739	-0.0551***	0.2520	-0.0557***
	(0.2150)	(0.2212)	(0.5805)	(0.0145)	(0.2660)	(0.0133)
tp × ln_rate					-0.4094*	-0.1053
					(0.1933)	(0.2225)
Fixed-effects						
state	Yes	Yes		Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes	Yes
id_c			Yes			
Observations	961	950	961	1,277	961	1,277
R^2	0.30551	0.30328	0.76801	0.32312	0.30601	0.32318

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1 ; Note : ATE was calculated using a mean(ln_rate) = -2.78*

4.4 Effect of WAF Rate Change

4.4.1 Basic DiD

For the basic analysis, our set consists in this situation of 961 observations from our eight states considered. States where the rate was increased include Brandenburg, Schleswig-Holstein, Lower Saxony, and North Rhine-Westphalia while it remained unchanged in Baden-Wuerttemberg, Saxony, Bremen, and Berlin.

Analyzing the effect of a changed rate within the context of these eight states with a positive fee I find that a rate change, when controlling for state and year fixed effects, is associated with an 8.5% increase in abstractions. Nonetheless, also a 28.2% (-0.331) decrease and a 64.2 % (0.496) rise are compatible with our data. This estimate seems robust to the exclusion of the cities of Berlin and Bremen which one might consider different from other counties (2).

The size of the positive effect surges to 20.6%(0.187) once we control for county-fixed effects (3). Also here negative and positive effects are within the results of our data and thus it can not be concluded that the effect truly is different from zero.

Also the results of the respective Bayesian regression point also in the direction of a positive estimate. I find that a rate change is associated with a 13.8% (0.129) increase in abstractions where with 95 % probability the true effect lies within increases of 178 % and decreases of 56.5 %(-0.833;1.025) which is once again particularly wide.

When we inspect the question of whether we can establish a different savings effect for higher rates of the WAF by analyzing the change-rate height interaction (5) we make several observations: First, the ATE of the rare change seems positive and lies at about 1%. Secondly, an interaction

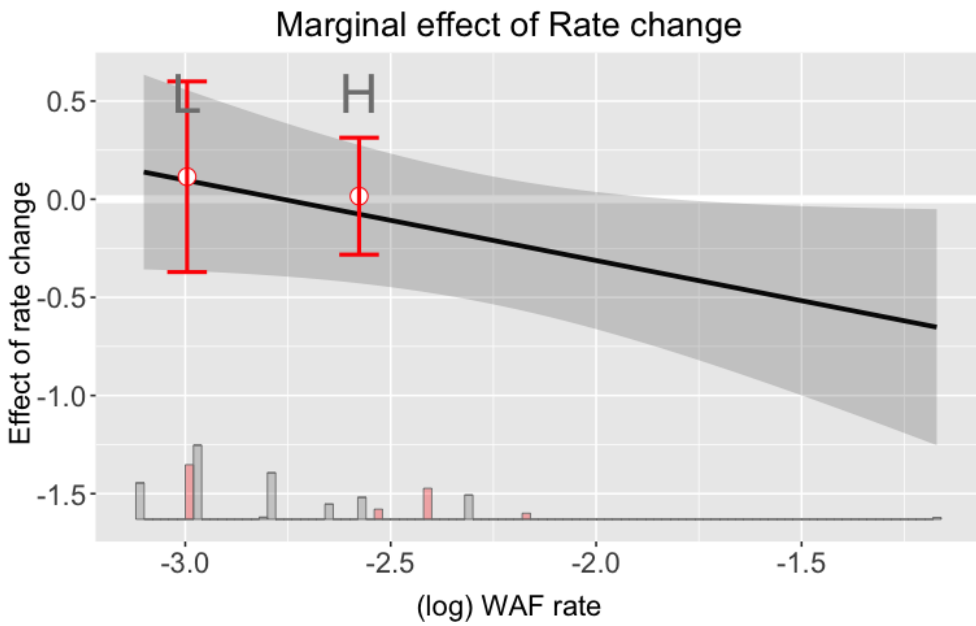

Figure 5: Effect of a Rate Change for higher (log) rates

seems to be absent at the 5% level as the insignificant interaction estimate denotes. Thirdly however the effect might grow negative for higher values of the rate. We see in Figure 5 that the marginal effect of a rate change seems to be moving towards a negative estimate thus a a more pronounced savings effect. Entirely based on eyeballing the marginal effect plot the Conditional Effects turn negative at a rate of 0.06 C/m^3(ln_rate = -2.75). It seems that for higher rates the savings effect of a rate change becomes indeed more pronounced as the negative interaction estimate indicates.

4.4.2 Event-study Rate Change

As the rate changes in the 4 states do not occur at the same point I use an event study design to analyze whether an effect is visible and its dynamics when we respect the time structure of the treatment. Therefore I use the Callaway and Sant'Anna [2021] Estimation technique which gives us an effect of the rate change on the treated thus for countries that experienced a rate change. Corresponding Results are presented in Table 10. In the simple specification analyzing the eight states with one rate change, I find that among those treated the rate change is associated with

Table 10: Event-Study Simple ATT Results - with 8 & 13 States

Model	Model_8	Model_13
ATT	-0.178	-0.370
Std_error	0.242	0.284
CI_low	-0.232	-0.273
CI_high	0.715	0.840
Obs	961	1277

a 16.3 % (-0.178) decrease in abstractions. However, also 20.7 % (-0.232) decreases and 104.4 % (0.715) increases are compatible with the data. The estimate seems to turn further negative when we use our broader dataset of 13 states that have a fee (*Model_ 13*).

Analyzing the results suggests a highly ambiguous image of the dynamics of the effect. One general hypothesis could be that savings might materialize in the shorter term but turn into increases in the longer term. With the implementation of a rate change abstractions decrease by 20.0% (- 0.223) at t= 0 which culminates in a 48.3 % (-0.660) decrease 3 years after the change. This direction of the effect completely turns however 6 years after the change where abstractions are on average 161 % higher (0.961). All estimates include zero effects and are subject to high standard errors thus also effects of opposite directions are compatible with our data thus making this observation rather speculative. A further reason for doubt in the validity of the underlying comparison is the observation that estimates, even though not statistically different from zero, seem not particularly close to zero in size indicating a lot of variation already before the rate change.

I hoped to gain some clarity on the effect by expanding our dataset to all 13 states with a fee, including the states Hamburg, Mecklenburg-Western Pomerania, Saarland, Rhineland-Palatinate, and Saxony-Anhalt. This gives us some indication about the nature of the effects which indicates again ongoing savings. According to Figure A7 (see Appendix) we observe a similar pattern around the event times τ = -6 and τ = 6 of some short-term savings which then turn to increases. On the left fringe of τ=-9, the estimate moves closer to zero. Inspecting the effect of a fee change 9 years after implementation I find that abstractions are lower compared to our reference period. As an additional measure to align more closely with our original specification, we model the dynamics of the effect including state-fixed effects. We find that using this approach ambiguity is marginally reduced. Estimates before the rate change seem now closer to zero as common trends would suggest and the rate change is associated with lower abstraction quantities for all after periods compared to the reference period. However, the size of the estimates remains unreasonably high (See Appendix Figure A8).

All in all, the results of the event study are of limited informative value. The unspecific identification strategy leads to contradictory and unrealistic results. Assuming that common trends prevail, which seems possible purely based on the statistical graph, a short-term savings effect could be identified at most. However, it is not possible to judge whether this is sustainable based on the present analysis. Overall this reinforces the opaque impression of the effect of a rate change gained before outside the event context which rather pointed toward a positive effect on abstractions.

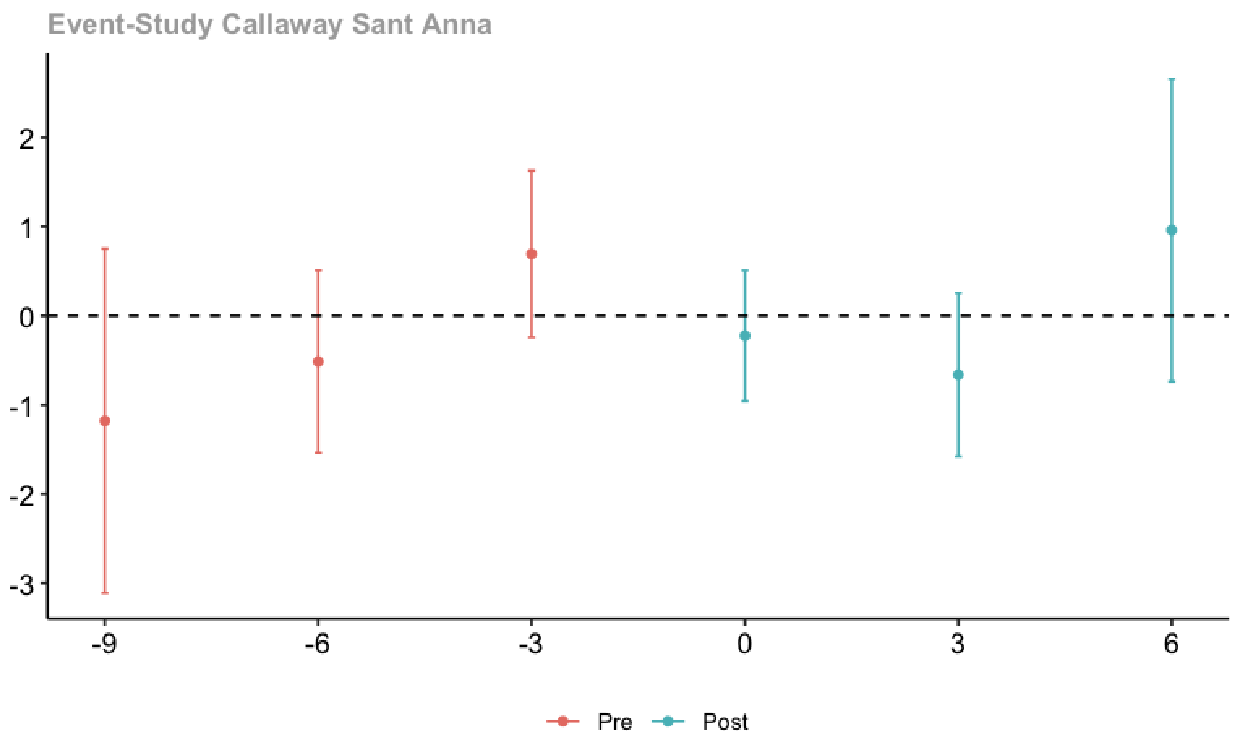

Figure 6: Dynamics of Rate Change Effect

5 Discussion

The results obtained are characterised by a high degree of ambiguity which requires nuanced consideration. Moreover, there are clear limitations to all of our three parts of the analysis that shall be depicted subsequently.

For my estimate on the price elasticity of -0.05 in Section 4.2 it is clear that even though it matches the academic consensus around water demand being inelastic, it is not only far lower than estimates provided in the literature but also limited in its worth. It is evident that relevant confounders are not adjusted for within my approach. Data on important variables such as temperature, existing groundwater resources, surface water abstractions and prices, the predominant industries active in the counties, or progress in production or water-usage technology such as recycling capabilities were not made available. Any work centered around the objective of estimating a reasonable elasticity would include these and choose a more nuanced identification strategy than our cross-country comparison of all counties. The main purpose of this part was rather to provide a general estimate of the German industrial context based on the obtained data.

The core of this study surrounds the impact of the WAF implementation in the RPF-SA context. The estimates found in our main DiD comparison indicate a volatile generally negative estimate which however does not translate in a significant savings effect. Thus we have to formally conclude on our first question that the implementation of the fee did not affect abstractions significantly neither in the frequentist nor the Bayesian regression framework. The frequentist benchmark of the RPF-SA context points towards a large however insignificant estimate indicating a 40% reduction or in the direct comparison of over 63% most likely overestimates the real size as variation in model specification and choice of geographical units show. The results from our Bayesian regression tend to confirm that the real estimate is most likely lower than found in the frequentist setting. The estimate denoting a 24.6 % decrease indicates that savings are potentially existent however insignificant in our setting. Nevertheless, our Bayesian results are crucially dependent on the choice of prior thus any numerical estimate should be considered with caution.

Part of the explanation of why estimates are large in the frequentist DiD might be captured in the above explanation around unobserved covariates such as existing groundwater resources and surface water abstractions. In the RPF & HE context it could be argued for instance that groundwater resources are more extensively available in Hessens border regions. Thus RPF counties have lower abstractions and thus stronger savings because there are not that abundant resources anyway. This would mean our estimate would overestimate the real effect. For the neighbor comparison, however, it appears that both are located within a comparable topographical region of the so-called Upper Rhine Plain, which has high groundwater reserves. In the case of surface water abstraction, comparability is somewhat more problematic because the Rhine, as a rich surface water body, represents the border region in the south but runs through Rhineland-Palatinate's administrative districts in the north. Although I adjust for the existence of these differences in the resources, this could be disproportionately compensated for in the actual abstractions of districts in the northeast of Rhineland-Palatinate. Further, it seems that this high estimate is explained by the choice of the geographical context. Lower estimates of 10-27 % for the RPF-only context point toward the explanation that the high estimate is in parts dependent on the inclusion of the SA-TH context. Yet there might be reasons why SA-TH might not be a suitable comparison. Inspecting Parallel trends just within SA it seems that it is somewhat less plausible indeed which might be read as an indication point towards the estimates in the RPF-only context. In addition to that, we see that estimates vary with regard to the inclusion of county-fixed effects. The inclusion of county-FE tends to yield slightly higher (absolute) estimates compared to state FE using the same counties. In particular, in the case of our reduced RPF-only context such specifications seem more plausible. This in turn yields us a lower estimate of a 26 % reduction (including zero nonetheless).

The dynamics of the estimate show they turn negative three years after the fee adoption and increase slightly in size until 2016 then approximately remain stable compared to the reference period. However, effects do not significantly differ from zero at any point. Hence we also have to conclude on our second question that there was no significance in the differences found however the course of the estimate seems negative and somewhat increasing until 6 years after the implementation and remains comparable 9 years after the implementation in 2019.

The estimates in the results regarding the effect of a rate change, our third question posed, are somewhat contradicting to results obtained in other parts of this work. Bayesian and Basic DiD point towards a positive insignificant effect of a rate change of 13.8 and 8.2 %. Although I pursue a Natural experiment research design in this case to avoid selection bias there remain substantial doubts regarding the identification strategy for the rate change. Compared to my main specification it is far broader by comparing all eight states with a WAF of which four legislated exactly one rate change and four that did none. These states might be considered different for various reasons. States like NI and BB with heavy agricultural water use might not be considered comparable to control states consisting of unchanged BW and Saxony which are more industrially shaped. As described industrial BW counties might use more river water and thus have less groundwater abstractions while agricultural NI and BB rely more on it. This might explain why the rate change correlates with higher abstractions in the Basic and Interacted frequentist and Bayesian settings. Hence we can only conclude from our third question that a rate change had, if any, a positive effect on abstractions and thus did not contribute to savings. On the fourth question posed we find that for higher levels of the rate abstractions indeed seem to decrease as the sign of our result estimates from the interaction and our graphical inspection of the marginal effect suggest but that this was not effective in creating significant savings at the 5% level. Turning to the question of whether this rate change effect varied over time, we overall again failed to establish significant differences in our Event-Study. Yet the negative event time estimates as found by this analysis could suggest that a rate change induces short-term savings but this insight is rather speculative. The estimate is $\tau = 6$ might further point to a turnaround in the longer term but also this is exclusively hypothetical.

Several points deserve discussion in the choice of my approach and could be considered limiting the conclusions drawn from my resources. First, the choice of the dependent variable and the connected dataset might be questioned regarding its ability to identify the effect in some imaginable ways. Essentially the non-public groundwater abstractions could be considered unable to capture industrial water demand as groundwater is not of high relevance in this sphere. As noted earlier groundwater accounts for only a small fraction of non-public water demand. River water is a considerably more important source for industrial water needs. However, they have not yet been accessible in sufficient detail and frequency for analysis. While there is data that collects particular abstractions for the mining and manufacturing sector which is undoubtedly resembling parts of the industrial water demand closer, I decided to use this dataset as it inherited less insecurity about treatment administration. As we have seen the WAF is designed vastly differently by states and sectors and so are the regulations, rates and exemptions for the mining and manufacturing sector that are extensive and lack consistency. This would have impaired the ability to effectively compare the application of WAF. Linking the general rate as my broad policy measure to a very specific part that it might not apply to anyways seemed not a suitable strategy. Thus this broader set matches better with the general nature of the instrument investigated.

Secondly also my method of data transformation and crucially the prior choice in the Bayesian approach might hamper the results. In using a logarithmic transformation in particular the handling of zero values is critical. As we observe a substantial number of zero values for the rate variable maybe a square root or arc sin transformation would have been better suited to deal with the skew for the elasticity and rate change analysis. Furthermore are the Bayesian analysis results heavily dependent on the choice of the priors. I run the limitation that these are set overly broad in my approach. Applying other priors under stricter priors denoted by a lower σ_{prior} heavily influences the result in the direction of finding a lower absolute estimate. Furthermore, does this widen the uncertainty around the parameter estimates and might have refrained me from establishing a significant effect. I chose this specification because it most closely matched the lack of evidence of effect size in the unfamiliar German context. However, it establishes a striking demand for further analysis of how a more nuanced choice would transform current estimates.

A third more substantial limitation to my results lies in the time structure of the relationship as well as the data. Extending the assumption that the effect not only varies over time it could also be lagged as firms adjust delayed to legislative WAF changes. This can be considered reasonable where production capacities especially in heavy industries like water or energy production are not always particularly flexible to adjust. Given that my data is only collected over 12 years in intervals of three years our estimate might be invalidated and we are unable to identify sub-period trends. Changes in the WAF legislation such as the WAF abolishment of Hesse that just took place in 2003 might still affect abstractions. In addition, I implicitly assume that SA and RPF have the same treatment date although SA actually introduced the WAF in 2012 due to the 3-year measurement cycle. However, this time shift cannot be mapped in our abstraction data. I might for example miss data for sub-periods like 2017 where, considering our Time-varying effect significant differences could have materialized, however without data on it we are not able to identify it. Further, it is difficult to establish the plausibility of common trends in the case of our main fee implementation DiD which is just based on two prior time points. This undermines the extent to which we can rely on the results drawn from our methodological approach.

6 Conclusion

In the light of ongoing depletion of water bodies in some regions due to climate-change related droughts and over-usage the efficient handling of water has gained importance. One instrument that has experienced a revival in institutions and academia due to its reevaluated conservation potential is water abstraction fees. In the course of this study, I examined under a traditional Frequentist and Bayesian Difference-in-Difference design the effectiveness of the German *Wasserentnahmeentgelt* to promote water savings among non-private actors on the county level in the period of 2007-2019. This is particularly relevant as local governments in Hesse and Bavaria have indicated plans to implement a fee in the upcoming years.

Expanding on a generic inelastic price elasticity estimation I find that a WAF implementation was not able to induce significant savings in the context of the states of Rheinland-Palatinate and Saxony-Anhalt which was my first question posed. However, estimates themselves indicate 40%-60% lower abstractions in my basic set-up and hence might suggest that the WAF implementations affected abstractions if any in a negative direction and were accompanied by potentially sizeable reductions. Tests of the robustness of these estimates and our Bayesian regression paint the picture that more realistic estimates lie somewhat lower and rather in the sphere of 10-25% which is in line with recent findings of studies such as Burlig et al. [2021], Bruno and Jessoe [2021] and Smith et al. [2017]. In particular, the Bayesian analysis uncovers the great uncertainty inherent to this effect size which in turn established a clear need for further investigation. Turning to the second question which involved the extent to which this effect varied over time, also this comparison does not yield significant differences. Point estimates seem to suggest relatively constant savings in the magnitude of about 40%. Nevertheless none of the estimates neither in the aggregate nor the dynamic analysis allow us to reject the hypothesis of a null effect.

The analysis of whether a change in the WAF rate was environmentally effective yields ambiguous results. Investigating the third question regarding the impact of rate changes in four German states Frequentist and Bayesian methods predominantly yield not-significant positive average estimates in the range of 1-12 %. This means rate adjustments were associated with on average higher abstractions in my setting even though also decreases remained plausible. As discussed these positive estimates are potentially due to underlying differences in the characteristics which call for further more nuanced investigation. Therefore the insights gained from the rate change design might be questioned in subsequent works. The event-study ATT that relies on a different specification of the control group delivers negative effects and hence opposes the previous results. The conditional treatment effect for the group that was treated indicates insignificant savings of about 16% and thus gives a rather negative hint to the question posed.

By running an interaction model with the respective rate in that state we investigated the question of how the effect varied for higher rate levels. My findings indicate that a significant interaction appears absent at the 5% level but that for higher rate levels estimates suggest to turn negative as one might reasonably expect. Thus we could only speculate that for policy makers to make sure savings occurred one had to choose higher rate levels. Eyeballing the marginal effect (5) plot indicates this reversal to occur at a rate of about 0.06 Cents/m^3.

Regarding the dynamics of this effect and thus relating to the fifth question posed earlier, one could hypothesize about a potential short-term savings pattern even though differences remain insignificant in this comparison. Estimates showing a decline in abstractions up to six years following adoption, which then shift to positive outcomes, raise this proposition. The nature and existence of such a hypothetical short-term effect might be considered worthwhile for future research projects.

The study of the effectiveness of water management policies in general and water abstraction charges in specific in multiple other contexts remains of valid interest for future research to fine-grain the estimates derived in this work. As existing works predominantly focus on the agricultural context, investigating the empirical effectiveness outside this sphere persists in high relevance. In the German context, a focus on mining and water for cooling purposes would be conceivable, as corresponding data is available. As data on public water abstractions exists until before 2000 studying private water demand (even though regarded as less elastic) seems worth studying under potentially a similar analysis which would allow to strengthen insights gained from a more robust design. Further, it is needed to extend the empirical understanding of the underlying mechanisms by which firms react to the installation of a WAF.

In addition, one could exploit the fact that policymakers have repeatedly adjusted their legislation on abstraction and water recycling charges in recent times. For example, the increase in rates in Lower Saxony or Mecklenburg-Western Pomerania in 2022 or the lifting of exemptions for mining in Brandenburg in 2011 represent interesting opportunities in the German context. Beyond this, a study of the abolition of the Dutch regulations around 2011 about its ecological effects could be intriguing.

To conclude, further empirical studies in different contexts are essential to understand the effectiveness of water pricing policies. This will enable policymakers and academia to determine if the reevaluated conservation potential of Water Abstraction Fees, which could not be causally identified in this work, translates to real-world outcomes.

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8 Appendix

Table A1: Summary of Sources

Variable	Source	Details
GW Abstractions	Regionalstatistik- regional statistical offices	Erhebung der nichtöffentlichen Wasserversorgung und Abwasserentsorgung
External water supply	Regionalstatistik- regional statistical offices	Erhebung der nichtöffentlichen Wasserversorgung und Abwasserentsorgung
Number of Firms	Regionalstatistik- regional statistical offices	Erhebung der nichtöffentlichen Wasserversorgung und Abwasserentsorgung
Surface water area	Regionalstatistik- regional statistical offices	Bodenfläche nach Art der tatsächlichen Nutzung » Wasserfläche; GENESIS-Tabelle: 3311101014
GDP	Regionalstatistik- regional statistical offices	VGR der Länder: Entstehungsrechnung » Bruttoinlandsprodukt/Bruttowertschöpfung nach Wirtschaftsbereichen - Jahressumme
Population Density	Regionalstatistik- regional statistical offices	Regionalatlas Deutschland Themenbereich "Bevölkerung" GENESIS-Tabelle: AI0021
Precipitation	DWD Climate Data Center (CDC)	Jahressumme der Stationsmessungen der Niederschlagshöhe in mm für Deutschland, ID : OBS_DEU_P1Y_RR

Table A2: Summary of Rate Sources

State	Source
Generally for most states not mentioned specifically	2019 BUND , 2016 IHK Brandenburg ; 2013 IHK Pfalz; 2010 UBA; 2007 UBA
Baden Würtermberg	2007 & 2010 Ministry of the Environment BW
NRW	2010 NRW Ministry of the Interior
Bremen	2010 Transparenzportal Bremen (TPP) - BremWEGG
Saarland	2007 Saarländisches Grundwasserentnahmeentgeltgesetz , Anlage zu § 2 Abs. 2
Saxony	2007 & 2010 Sächsisches Wassergesetz und Sächsische Wasserzuständigkeitsverordnung Anlage 2 2008

Counties selected for border comparison - 50km Benchmark

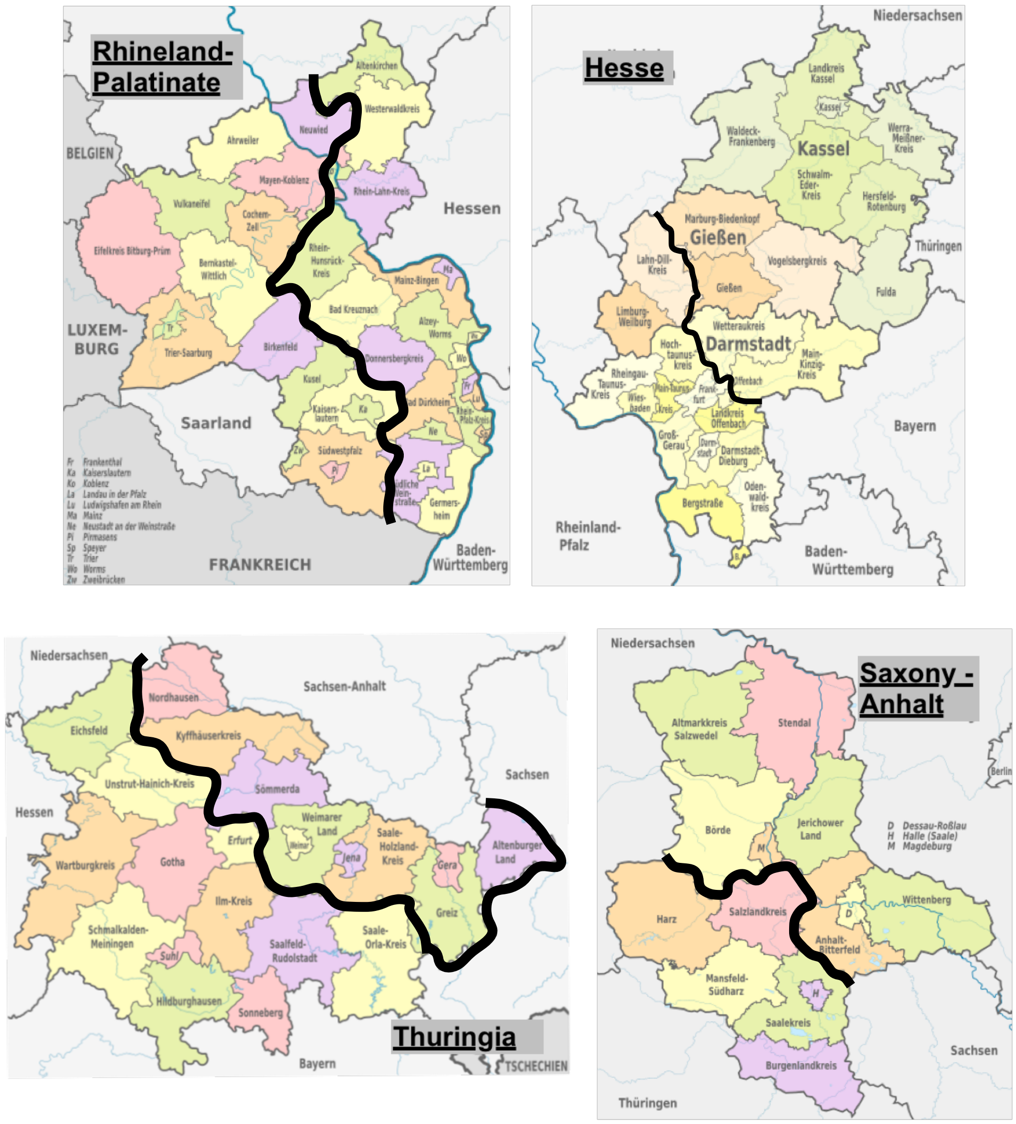

Figure A1: Counties Border context 50 km Benchmark

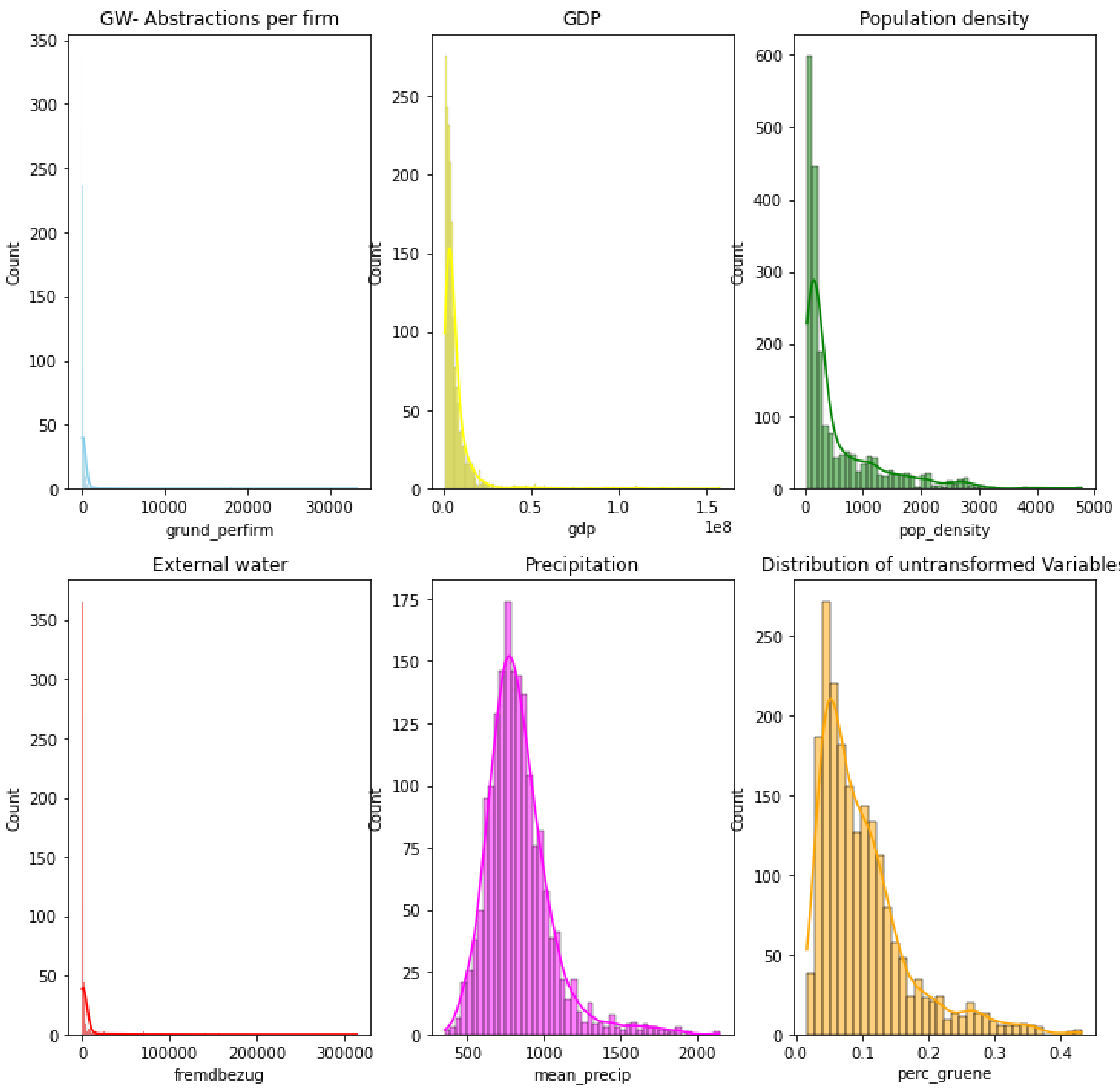

Figure A2: Variables Distribution - before Transformation

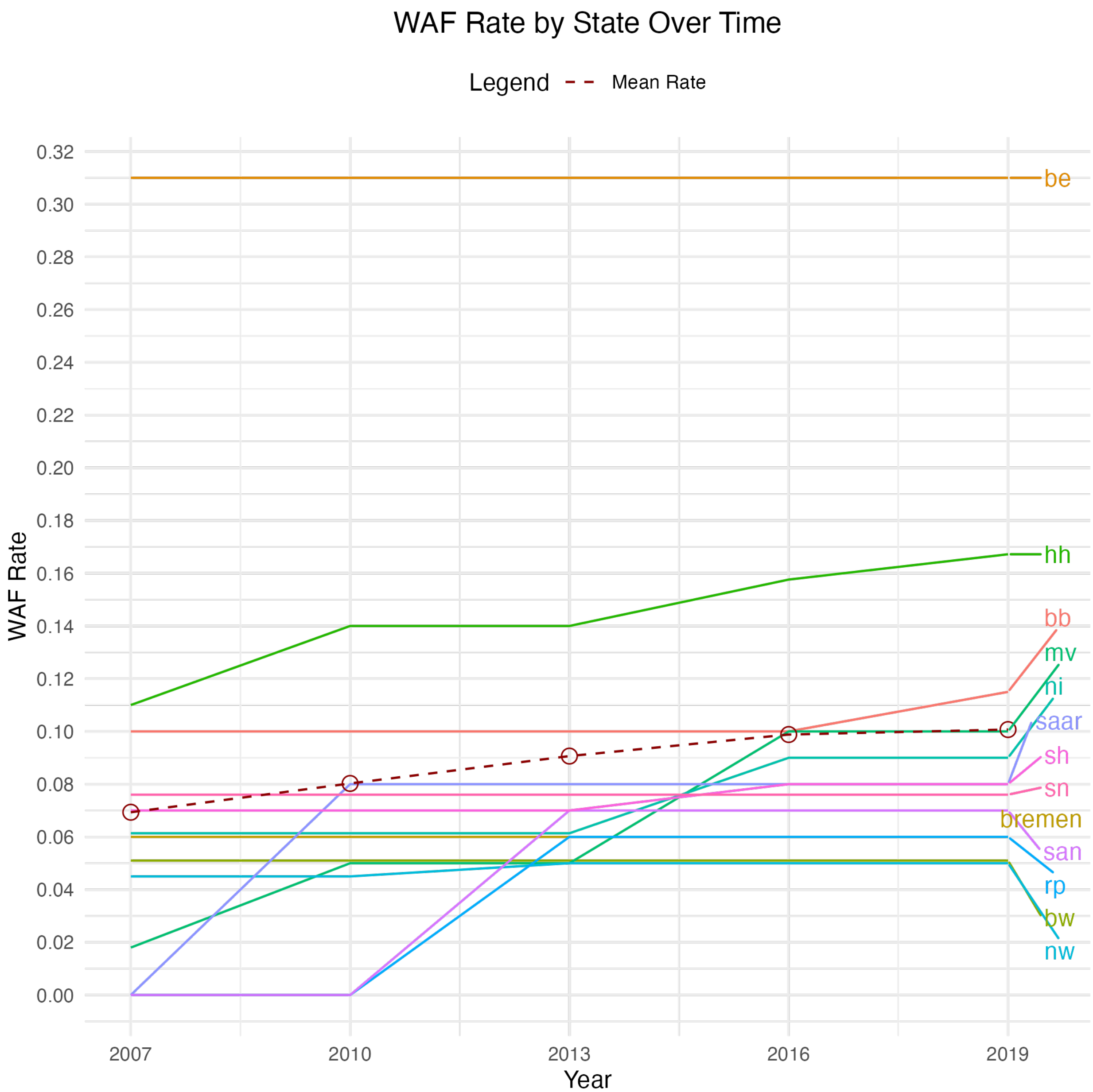

Figure A3: Rate by States 2007-2019

Note: be = Berlin; hh= Hamburg,bb= Brandenburg, mv= Mecklenburg-Western Pomerania, ni = Lower Saxony, saar = Saarland, sh = Schleswig-Holstein, sn = Saxony, san= Saxony-Anhalt, rp = Rhineland-Palatinate,bw = Baden-Württemberg, nw = North Rhine-Westphalia

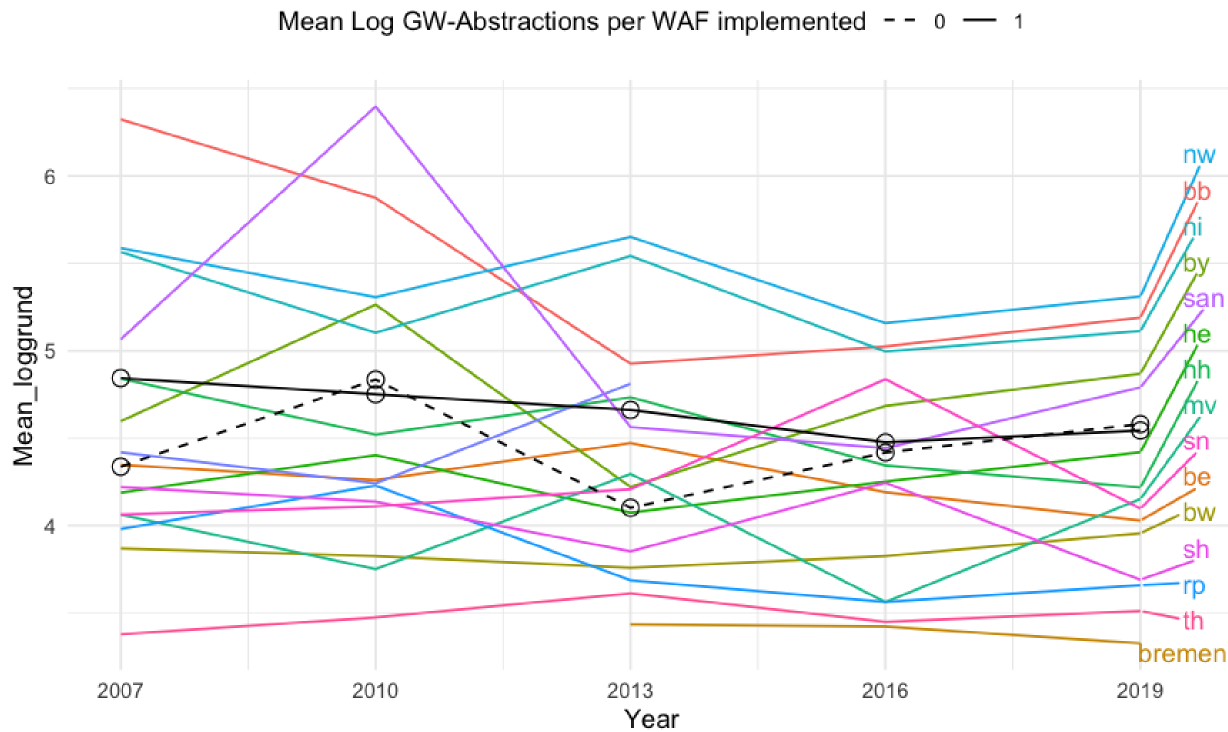

Figure A4: log GW-Abstractions 2007-2019

Note: Solid line denotes mean of states with implemented WAF while dashed mean of states without WAF

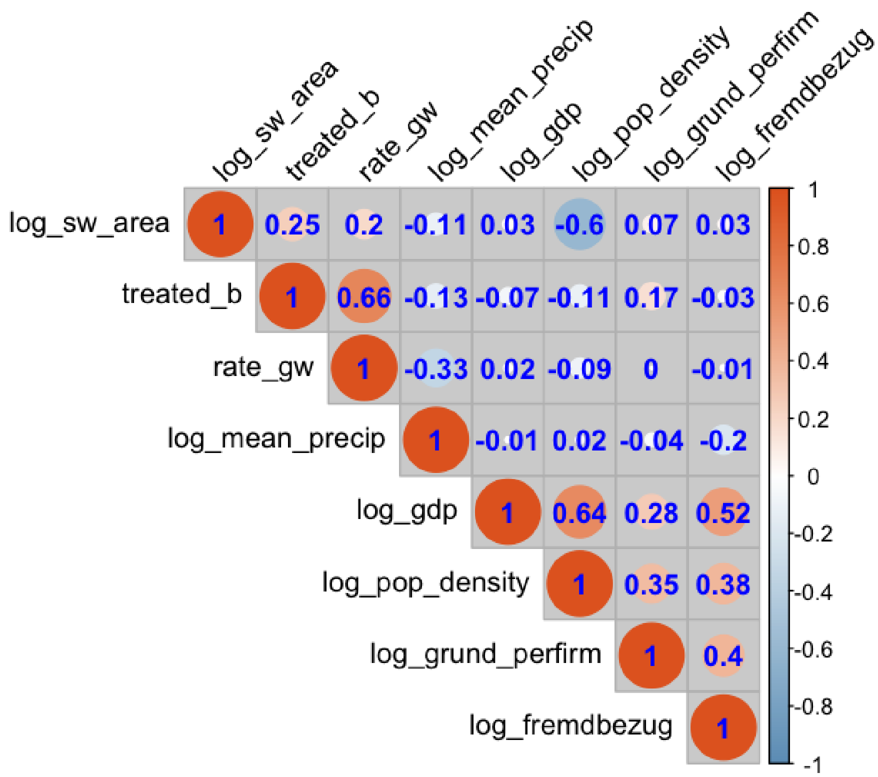

Figure A5: Correlation Matrix for RPF & SA context

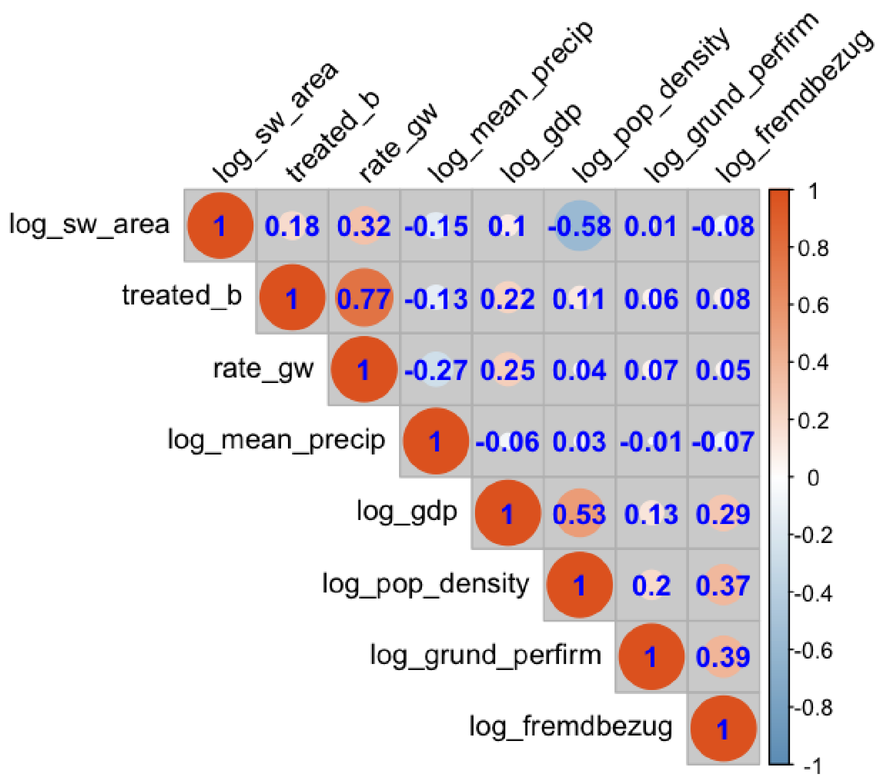

Figure A6: Correlation Matrix all Data

Table A3: Price Elasticity of Water Demand

Dependent Variable:	log_grund_perfirm				
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
log_rate	0.0273	-0.0524*	-0.0537*	-0.0598*	-0.0558
	(0.0187)	(0.0243)	(0.0218)	(0.0256)	(0.0331)
log_externalwater			0.3007***	0.2175**	0.3222***
			(0.0250)	(0.0697)	(0.0224)
log_pop_density			0.3425***	0.1834	0.4191***
			(0.0266)	(0.3588)	(0.0382)
log_gdp			-0.1814**	-0.4562	-0.2319**
			(0.0519)	(0.4330)	(0.0814)
log_mean_precip			0.0690	0.5024	-0.0661
			(0.1577)	(0.2924)	(0.2559)
log_sw_area			0.1850**	0.4708	0.1862***
			(0.0480)	(0.5102)	(0.0351)
log_perc_gruene			0.0837	0.1002	-0.1480
			(0.1180)	(0.1735)	(0.0793)
Fixed-effects					
year	Yes	Yes	Yes	Yes	Yes
state		Yes	Yes		Yes
id_c				Yes	
Observations	1,997	1,997	1,997	1,997	1,277
R^2	0.01238	0.18112	0.28482	0.73175	0.32275

Clustered (year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A4: DiD: Fee Implementation Full Table

Dependent Variables:	grund_perfirm			log_grund_perfirm		
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
treat_post	-444.8	-457.3	-0.5342	-0.5432	-0.5253	0.0952
	(395.1)	(287.4)	(0.4351)	(0.4210)	(0.3614)	(0.0568)
log_pop_density		409.0		1.221**	1.193**	1.220***
		(248.1)		(0.2598)	(0.2416)	(0.1235)
log_gdp		-224.8		-0.5190	-0.6765	-0.7965**
		(171.8)		(0.2553)	(0.3605)	(0.1763)
log_mean_precip		893.2		0.3043	0.5139	0.2021
		(393.8)		(0.7197)	(0.6061)	(0.4135)
log_sw_area		160.5		0.6848	0.7078	0.8834***
		(100.8)		(0.3449)	(0.3601)	(0.0609)
log_externalwater		75.23			0.1623	0.2686**
		(63.75)			(0.1626)	(0.0891)
Fixed-effects						
state	Yes	Yes	Yes	Yes	Yes	
year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	225	225	225	225	225	225
R^2	0.17259	0.36756	0.16750	0.38360	0.39758	0.35123
Within R^2	0.03312	0.26094	0.00927	0.26645	0.28308	0.33456

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A5: DiD: Fee Implementation Time-varying Estimates

Dependent Var	log GW-Abstractions
treated_b x year = 2007	0.0558 (0.1727)
treated_b x year = 2013	-0.4096 (0.5968)
treated_b x year = 2016	-0.5660 (0.3689)
treated_b x year = 2019	-0.5178 (0.3720)

Table A6: Rate Change - Summary Baysian - DiD

Variable	Mean	SD	$hdi_3\%$	$hdi_97\%$	r_hat
d	0.129	0.504	-0.833	1.025	1.000
log_rate_gw	0.022	0.076	-0.126	0.164	1.000
log_gdp	-0.064	0.037	-0.131	0.006	1.000
log_mean_precip	-0.006	0.031	-0.061	0.052	1.000
log_pop_density	0.052	0.046	-0.035	0.136	1.000
log_externalwater	0.349	0.031	0.291	0.408	1.000
log_sw_area	0.014	0.041	-0.061	0.092	1.000

Table A7: Even Study Dynamic Results M8

Eventtime	Estimate	Std_error	CI_low	CI_high
-9	-1.179	0.724	-3.011	0.654
-6	-0.513	0.413	-1.558	0.532
-3	0.695	0.349	-0.189	1.579
0	-0.223	0.319	-1.031	0.585
3	-0.660	0.344	-1.532	0.211
6	0.961	0.595	-0.545	2.467
ATT	0.026	0.267		

Event-Study Robustness 1

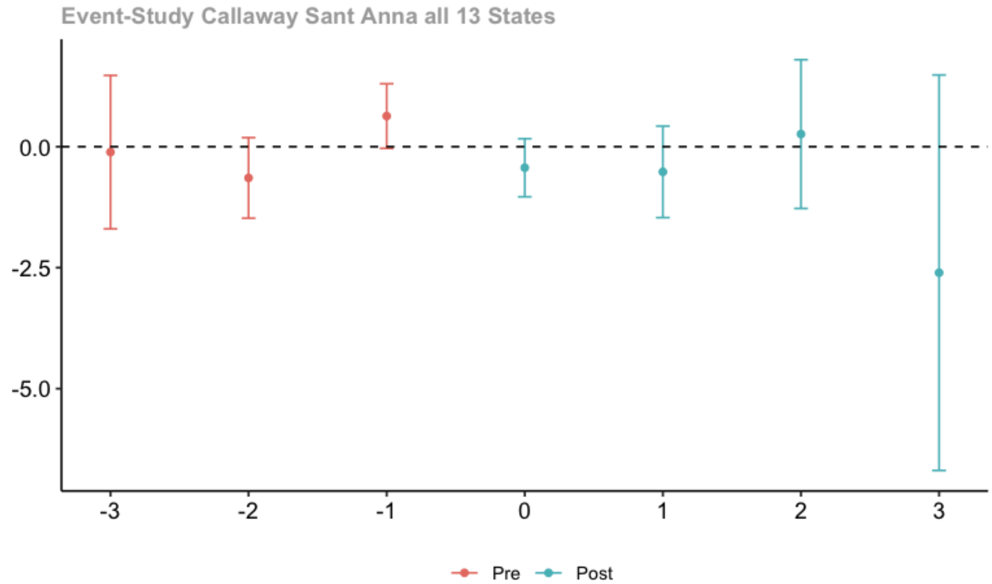

Figure A7: Dynamic Treatment Effects for all 13 states - County FE

Event-Study Robustness 2

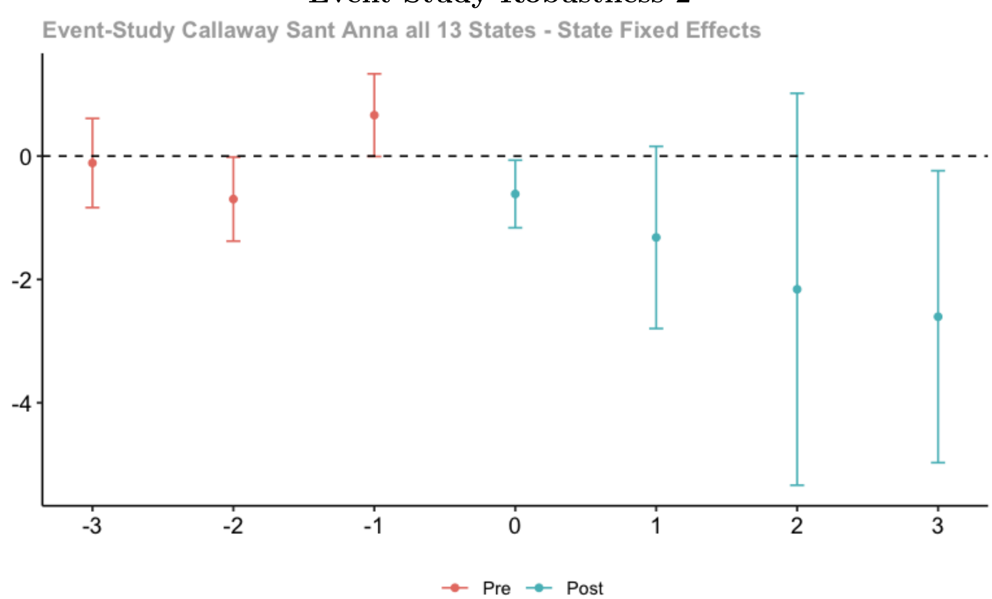

Figure A8: Dynamic Treatment Effects for all 13 states - State FE

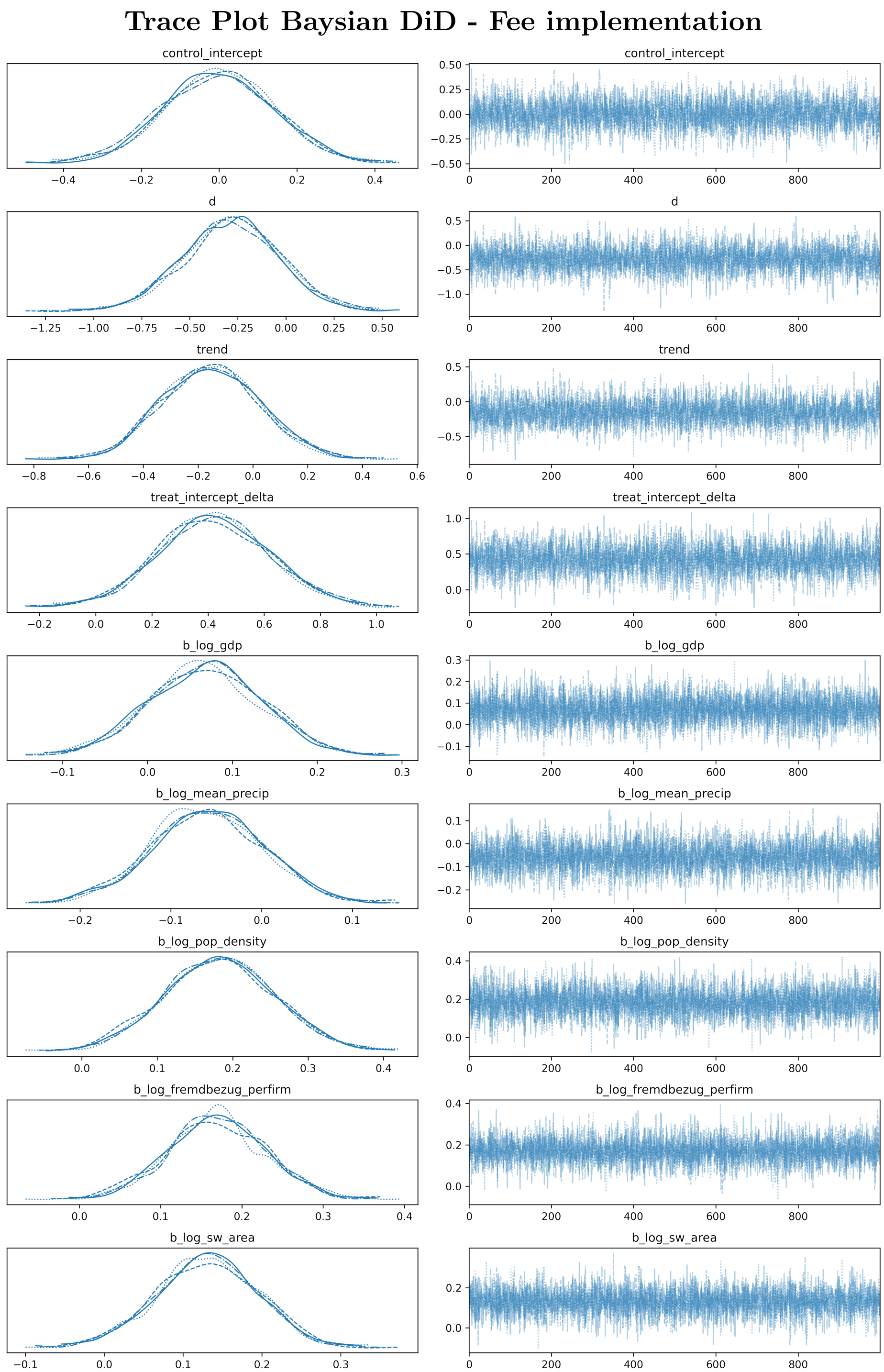

Figure A9: Trace Plots Fee Implementation