Do fiscal reforms need a gender lens? Evidence from Indian states

October 28, 2025

Abstract

This study delves into the effects of adopting gender budgeting on fiscal policies within Indian states over the period spanning from 1991 to 2020. Employing a Difference-in-Differences (DiD) framework alongside alternative estimators, the research tackles concerns regarding endogeneity to ensure the robustness of its findings. The findings reveal that states embracing gender budgeting tend to allocate a larger share of their expenditures to sectors that promote gender inclusivity, notably education and infrastructure. Intriguingly, the adoption of gender budgeting appears to bolster the credibility of states in prioritizing pro-gender policies, facilitated notably by central government transfers, including Centrally Sponsored Schemes (CSS), which serve as vital transmission channels. However, while gender budgeting presents a promising approach to tackling gender disparities and bolstering social outcomes, its implementation may encroach upon fiscal autonomy, necessitating careful deliberation regarding resource allocation and intergovernmental relationships. The study's policy implications stress the significance of contextual factors and transparency in ensuring the effective and credible implementation of gender budgeting reforms, particularly within the diverse economic landscape of India. In summary, this research offers valuable insights into the nuanced impact of gender budgeting adoption on fiscal dynamics, highlighting the imperative for further exploration to navigate complexities and optimize policy outcomes.

Keywords: Regional Government, Regional Policy, Public Economics, Public Finance.

JEL Codes: H1; H7; J16; R5.

Conflict of interest: No conflict of interest.

Funding: No funding.

List of Figures

1 Pro-gender spending by treatment status 14
2 Stylized facts 15
3 Gender budgeting adoption by states 16
4 Parallel trend with honest DiD (Rambachan and Roth (2023)) 23
5 Event study results for “pro-gender” spending 25
6 Marginal effects of pro-gender public spending on IPV 35
7 Marginal effects of pro-gender public spending on IPV because of unfaithful . 37
8 Marginal effects of gender budgeting adoption on information access 41
A.1 Visual results of Callaway and Sant’Anna 50
A.2 Event study for "pro-gender" components (Callaway and Sant’Anna (2021)) 51
A.3 Results for Wooldridge estimator 52
A.4 Event study results for "pro-gender"spending 52
A.5 Event study results for "pro-gender"spending 53
A.6 Event study results for "pro-gender"spending with continuous treatment . . 53
B.1 Entropy balancing results 56
B.2 Entropy balancing Results 57
F.1 Results for a 3 years around period 60
F.2 Results for 5 years around period 60

List of Tables

1 outcomes Summary statistics . . . 12
2 Controls summary statistics . . . 13
3 Repartition of treatment . . . 17
4 Outcome means before the treatment (by status) . . . 21
5 Diff-in-Diff results . . . 24
6 Summary statistics . . . 34
7 Transmission channel . . . 38
A.1 Wooldridge Diff in Diff results . . . 51
B.1 Balance Statistics . . . 55
C.1 Dose Response Model results . . . 58
D.1 Placebo test Results for components . . . 58
E.1 Anticipation effects results . . . 59
F.1 Results for narrowing -3 ; +3 . . . 59
F.2 Results for narrowing -5 ; +5 . . . 59
G.1 Marginal effect on IPV due to unfaithful . . . 61
G.2 Marginal effect on IPV due to disrespect . . . 62
G.3 Marginal effect on IPV due to unfaithful in urban areas . . . 63
G.4 Marginal effect on IPV due to unfaithful in rural areas . . . 64

Contents

1 Introduction **4**

2 Conceptual framework **7**

3 Empirical methodology **10**
3.1 Data . . . 10
3.2 Stylized facts . . . 13
3.3 Identification strategy . . . 16
3.4 Parallel trend assumption . . . 20
3.5 Results . . . 23

4 Robustness Checks **25**
4.1 Alternative DiD Estimators . . . 25
4.2 Entropy Balancing . . . 27
4.3 Dose Response Function . . . 29
4.4 Placebo Test . . . 30
4.5 Anticipation effects . . . 30
4.6 Narrowing the control window . . . 31

5 Microeconomic Effects **32**
5.1 Data and empirical strategy . . . 33
5.2 Results . . . 35
5.3 Sensitivity Analysis . . . 36

6 Transmission channels **38**

7 Conclusion **41**

Appendices **50**
A Staggered DiD . . . 50
A.1 Callaway . . . 50
A.2 Wooldridge . . . 51
A.3 Borusyak . . . 52
A.4 Chaisemartin . . . 53
B Entropy balancing . . . 54
B.1 Correlation issue . . . 54
C Dose response model . . . 58
D Placebo Test . . . 58
E Anticipation effects . . . 59
F Narrowing the control window . . . 59
G Microeconomic effects . . . 61

1 Introduction

Despite a global decline in gender inequalities, they remain widespread, especially in developing countries where structural barriers (such as unequal access to education, health care, decent work, and political participation) continue to restrict women's opportunities. In response, Gender Budgeting (GB) has developed as a practical tool to integrate gender priorities into the design, planning, and monitoring of fiscal policies. This approach reframes budgets from neutral instruments into active levers that governments can use to challenge unequal power relations and redistribute resources more fairly. By analyzing how revenues are raised, how taxes impact men and women differently, and how spending is distributed across sectors, gender budgeting aims to make fiscal choices that directly reduce gender gaps. Making gender equality an explicit goal of public finance aligns budget processes with broader commitments to equity and sustainable development. Since its first introduction by Australia in 1974, GB has spread widely and today over 80 countries include a gender perspective in their budget processes (Rubin and Bartle (2023)).

Its relevance is even greater at the local level, where governments are directly responsible for delivering essential public services (such as schools, health centers, water supply, housing, and welfare programs) that have immediate impacts on women's daily lives and empowerment opportunities. Local governments decide how resources are allocated for these services, how priorities are set, and how responsive spending is to community needs. Implementing gender budgeting at this level means that fiscal policies can be shaped to address specific barriers faced by women and girls in their communities, ensuring that funds are reallocated where they are most needed to close gender gaps in access, quality, and outcomes. This local focus makes it possible to link fiscal choices directly to concrete improvements in women's social and economic empowerment such as better access to education, safer public spaces, or targeted support for women's livelihoods. Moreover, local gender budgeting helps strengthen democratic accountability by opening budget decisions to community participation and scrutiny, giving women a voice in how public money is spent and ensuring that fiscal policy

becomes an effective driver of gender equality where its impact is most tangible.

Most studies on gender budgeting still focus on the conditions that make its adoption possible, such as political will, administrative capacity, and civil society involvement (Chakraborty (2016); Quinn (2016); Jung (2022); Puig-Barrachina et al. (2017); Elomäki and Ylöstalo (2021)). However, they seldom explore how gender budgeting is implemented in practice or whether it actually shifts how public funds are spent (Stotsky and Zaman (2016)). Many works also overlook how expenditures are reallocated across sectors to tackle gender gaps. In addition, existing research often separates macroeconomic and microeconomic perspectives, analyzing overall fiscal trends without linking them to effects on households, or studying household impacts without connecting them to budgetary choices (Khera (2016); Das et al. (2015a); Buchmann et al. (2008); Kleven and Landais (2017); Mbodji (2023); Cannonier and Mocan (2018)). This gap limits a full understanding of how gender budgeting works in practice and how it can deliver real changes for gender equality, especially at the local level where its effects should be most visible.

This paper addresses a critical gap in the literature by providing an empirical assessment of the macro- and micro-level effects of gender budgeting in Indian states. The research question arises from a key concern: although many local governments announce gender budgeting measures, the absence of binding enforcement or sanctions means that these commitments may remain largely symbolic, producing limited or no real changes in how public funds are reallocated or in the everyday lives of women. In contexts like India, where the effectiveness of policy often depends on strong local implementation and accountability, gender budgeting risks becoming a mere rhetorical or technocratic tool—adopted to signal political commitment without translating into concrete shifts in spending priorities.

This makes it essential to examine whether gender budgeting actually leads to a reallocation of public resources and measurable improvements for the targeted populations. By focusing on Indian states—where subnational governments are responsible for delivering key public services that directly shape women's empowerment—this paper fills an important gap

by testing if gender budgeting can function as a credible instrument of fiscal policy rather than a policy label with little practical effect. It responds to three main shortcomings in the existing literature: the lack of evidence on how gender budgeting influences expenditure patterns in practice; the neglect of the reallocation dimension across sectors; and the frequent disconnect between macro-level fiscal analysis and micro-level impacts on households and communities. By combining these perspectives, this research clarifies whether gender budgeting, when implemented at the local level without strong enforcement, but with an obligation of transparency can still act as an effective mechanism for redirecting public spending (Gadenne (2017) and Besley and Smart (2007)) and advancing gender equality in a meaningful way.

To address these questions, this paper combines a theoretical framework with an empirical strategy that integrates both macroeconomic and microeconomic analyses. The empirical design relies on a difference-in-differences approach and entropy balancing to estimate the causal effects of gender budgeting adoption by comparing states that have implemented gender budgeting measures with those that have not, while controlling for potential selection bias and observable differences. At the macro level, this allows for the assessment of whether the introduction of gender budgeting results in significant changes in the allocation and composition of public expenditures. At the micro level, the analysis uses probit models to examine whether these fiscal changes translate into measurable impacts on individual and household outcomes related to women's empowerment. This mixed approach responds to data constraints typical of subnational studies while providing robust evidence on whether gender budgeting is linked to meaningful shifts in public spending and tangible improvements for the populations it targets.

The results show that states adopting gender budgeting allocate a larger share of their public spending to sectors that promote gender equality, particularly education and infrastructure, with these effects supported by central transfers that strengthen the credibility of local pro-gender commitments. At the microeconomic level, this reallocation translates into measurable improvements in women's empowerment, including positive changes in attitudes

towards gender equality and a reduction in tolerance for intimate partner violence, demonstrating that gender budgeting can produce concrete social outcomes alongside shifts in fiscal policy.

The remainder of this paper is structured as follows. The next section presents the conceptual framework that underpins the analysis. This is followed by the empirical strategy, detailing the identification framework used to estimate the effects of gender budgeting. The paper then discusses the robustness checks implemented to validate the results. The following section explores the microeconomic impacts on women's empowerment. Next, the transmission channels through which gender budgeting affects fiscal allocations are examined. Finally, the paper concludes with a discussion of the main findings and their policy implications.

2 Conceptual framework

In India's federal system, state governments play a central role in financing and delivering core public services like education, health, and infrastructures. These areas are crucial for reducing gender inequalities while also benefiting the wider population. Yet the discretionary power local governments hold over budgets, combined with weak citizen oversight, could often create information asymmetries that make it difficult for voters to verify whether announced policies translate into real spending shifts (Blagrave and Gonguet (2020) and Goetz and Jenkins (2001)). In this context, the principal-agent framework is particularly relevant: voters (principals) delegate authority to local leaders (agents) but face challenges in observing their true effort and intentions. Gender budgeting, if rigorously applied, could help close this gap by enforcing transparency, requiring states to disclose spending plans and report on how well they align with gender equality objectives. This transparency could act as a credible signal, lowering the informational advantage of local governments and enabling citizens to better monitor the match between commitments and action. Subsequently, the use of the term 'transparency' will refer to the implementation of gender budgeting and the transparency

resulting from its implementation. When transparency raises the political returns to visible, easy-to-monitor spending, leaders may have stronger incentives to reallocate budgets toward sectors like education and infrastructure that are both politically popular and clearly linked to women's empowerment. In this way, the model shows why, in the Indian context, gender budgeting has the potential to transform symbolic announcements into credible budgetary shifts that advance both gender goals and broad-based development.

The theoretical framework of this study is built upon the principal-agent theory of Meckling and Jensen (1976), which is particularly apt for examining the relationship between local leaders (agents) and their constituents (principals) as in Revelli (2002) and Vermeir and Heyndels (2006). This framework is chosen because voters, through the act of voting, delegate power to local leaders with the expectation that these leaders will implement public policies that align with the voters' expectations. The agent's behavior is influenced by a trade-off between the compensation received (such as political power and social status) and the effort exerted in promoting gender equality. Voters can observe and sanction the agent's actions, particularly during elections, by either re-electing the leader or negatively sanctioning them if expectations are not met.

A central aspect of this study is the role of transparency in reducing information asymmetry between voters and local governments. Transparency is modeled as a cost function that increases quadratically with the level of transparency (T):

$$C(T) = c \cdot T^2$$

where c represents the cost coefficient. Increased transparency allows voters to better observe the government's actions, thereby enhancing accountability. The voter's utility function is given by:

$$U_P = \alpha \cdot \theta_v + \beta \cdot T$$

where θ_v represents visible expenditures, and α and β are parameters reflecting the importance of visible expenditures and transparency, respectively.

The government's utility function, which includes political returns from transparency, is expressed as:

$$U_A = \gamma \cdot T \cdot (\delta \cdot \theta_v + (1 - \delta) \cdot \theta_{nv}) - c \cdot T^2$$

where γ measures political returns, δ captures the share of utility from visible expenditures, and θ_{nv} represents non-visible expenditures. The cost c is influenced by the local context, such as population size and urbanization, which affect the financial cost of transparency efforts.

The government's effort in promoting gender equality is modeled as an effort premium, where political support and activist actions are exchanged for the effort exerted. The optimal level of transparency (T^*) is derived by maximizing the government's utility function:

$$T^* = \frac{\gamma \cdot (1 - \delta)}{2c - \gamma \cdot (2\delta - 1)}$$

This solution shows that optimal transparency increases with political returns (γ) and decreases with costs (c). Visible expenditures (θ_v) are driven by transparency, highlighting the importance of transparency in political decision-making.

The implementation and effectiveness of gender budgeting are influenced by various contextual factors, including institutional frameworks, cultural norms, and intergovernmental relationships. For instance, cultural norms may affect the prioritization of gender-related expenditures, while intergovernmental relationships can impact resource allocation and policy outcomes.

The empirical analysis is closely linked to the theoretical framework, examining how gender budgeting affects public spending and political outcomes. The findings on the impact of gender budgeting on public spending are expected to align with theoretical expectations

regarding transparency, accountability, and political returns.

By addressing these points, the revised theoretical framework provides a clearer and more coherent explanation of the relationships and mechanisms underlying the study's hypotheses.

3 Empirical methodology

3.1 Data

This study relies on an original combination of various datasets that provide detailed and complementary information about economic, political and fiscal variables at state level. The core economic and fiscal data are sourced from the Reserve Bank of India (RBI), which offers a uniquely detailed and reliable account of the fiscal position and economic performance of each Indian State and Union Territory. The RBI's disaggregated statistics on revenues and expenditures allow for precise measurement of both revenue autonomy and expenditure autonomy, which are central to this analysis of fiscal federalism. Political variables are drawn from official electoral data provided by the Election Commission of India, ensuring accurate and consistent information on the political context at the state level. These data are crucial to control political factors that may influence both fiscal choices and Gender Budgeting adoption.

The Gender Budgeting adoption variable is a dummy which takes 1 if gender budgeting is implemented in a State and 0 otherwise. It comes from the paper of Stotsky and Zaman (2016) and has been updated by further research from literature and State governments disclaims.

As outcome variables we used "pro-gender" public spending (expressed as percentage of total expenditures). In this paper, we define "pro-gender" spending as the share of budget allocated to four key sectors: education, health, infrastructure, and water. These items are selected because they appear consistently in common gender budgeting templates and have been identified by adopting states as priority areas for advancing gender equality.

Education spending can directly reduce gender gaps by improving girls' enrollment, retention, and learning outcomes, which in turn expands women's future economic opportunities and bargaining power (Buchmann et al. (2008) and Kleven and Landais (2017)). Health expenditure is critical for ensuring women's access to maternal care, reproductive services, and basic health infrastructure, which lowers gender-specific barriers to well-being(Okojie (1994) and Sen and Östlin (2008)). Investment in infrastructure, such as roads and transport, eases women's mobility, connects them to markets and services, and can reduce time poverty linked to unpaid care work (Parikh et al. (2015); Das et al. (2015b); Calvo et al. (1994)) Finally, spending on water supply and sanitation addresses burdens that disproportionately fall on women and girls, freeing time and improving health and safety conditions, especially in rural areas (Weiss (1999)). Together, these sectors represent concrete channels through which public spending can be reoriented to address structural gender gaps and promote women's empowerment in daily life.

To measure the combined weight of these priority sectors in state budgets, we build an indicator that sums expenditures on education, health, water, and infrastructure and expresses them as a percentage of each state's total annual spending. Formally, this "pro-gender" share is calculated as:

$$Pro\_gender_{it} = \frac{(Education_{it} + Health_{it} + Water_{it} + Infrastructures_{it})}{Total\_expenditure_{it}} * 100 \quad (1)$$

This indicator allows us to assess whether gender budgeting is associated with a meaningful reallocation of public resources toward spending that can help close gender gaps. If these sectors become explicit priorities through gender budgeting, then adopting states should allocate a higher share of their total expenditure to this aggregate "pro-gender" spending than non-adopting states.

The next table summarizes the "pro-gender"variable and its components.

Table 1: outcomes Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.
pro_gender	5.193	2.858	0.338	16.794
infrastructures	2.311	1.474	0.053	12.235
education	0.73	0.852	0	5.517
health	0.582	0.626	0	5.849
water	1.534	1.461	-0.004	7.805
N		857		

The control variables are a set of covariates used in the literature on public spending composition which can also affect the likelihood of adopting gender budgeting.

Fiscal autonomy can affect spending size and composition and is associated with a smaller public sector size at the local level for Fiva (2006). Siwińska-Gorzelak et al. (2020) shows a U-shaped relationship between the ratio of own local taxes and the share of capital expenditures and a negative relationship between the former ratio and the share of education spending for Polish municipalities.

These states are also less dependent on central government transfers and are more autonomous in their political choices. The fiscal autonomy variable is a ratio between states' own local revenues and their total revenues (transfers and grants included).

The most urbanized states can generate some scale economies, or sometimes some congestion effects which make less effective and less efficient public spending and policies related to health and education issues. To avoid this, more urbanization may call for more expenditure centralization by attracting the rural population towards big centers, favoring a certain concentration of public expenditures(Sacchi and Salotti (2016)).

GDP per capita affects the sensibility to women rights and gender equality. Indeed, Doepke et al. (2012) found a strong connection between women's rights and economic development.

The share of seats held by women in local parliament influences the composition of public spending at the subnational level (Svaleryd (2009)). The presence of women in local parliament also affects the political decisions and the choice of gender budgeting adoption.

All the variables have a year lag to tackle or reduce the endogeneity.

The following table summarizes the main variables used in the estimation process.

Table 2: Controls summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
gender_budgeting	0.22	0.414	0	1	930
state autonomy	48.938	25.713	5.466	100	857
log(GDP per capita)	10.287	1.061	7.886	12.832	866
Urbanization (%)	33.568	19.098	7.98	99.900	736
trend	16.815	9.352	1	33	1023
women in parliement (%)	48.53	1.878	42.39	54.87	902
fiscal rule	0.381	0.486	0	1	930
Agriculture (%GDP)	26.575	20.109	0.052	130.834	866
log(population size)	20.847	0.125	20.608	21.025	787

3.2 Stylized facts

The graphs 1 and 2 highlight a comparison between the average share of “pro-gender” expenditures for treated (1) and untreated units (0). The adopters seem to allocate more of their funds to "pro-gender"items than the non-adopters. This result seems to suggest that gender budgeting adopters spend more on "pro-gender"items than others. However, this correlation does not mean anything in terms of causes and consequences because a correlation does not necessarily imply causality.

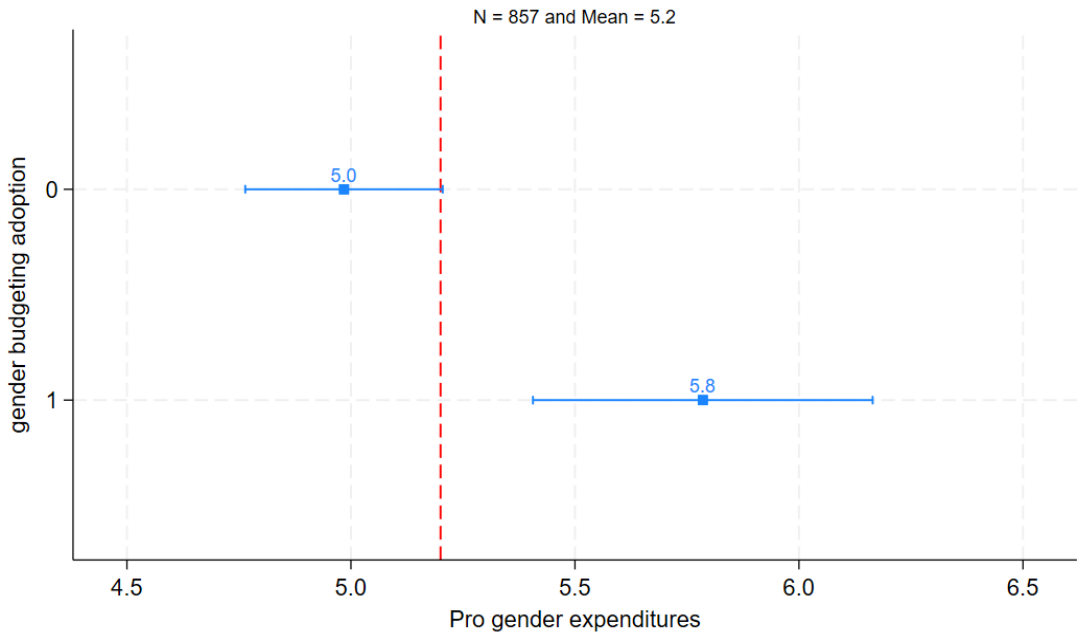
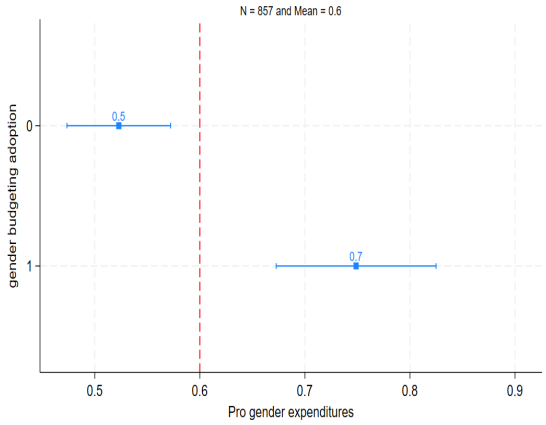


Figure 1: Pro-gender spending by treatment status

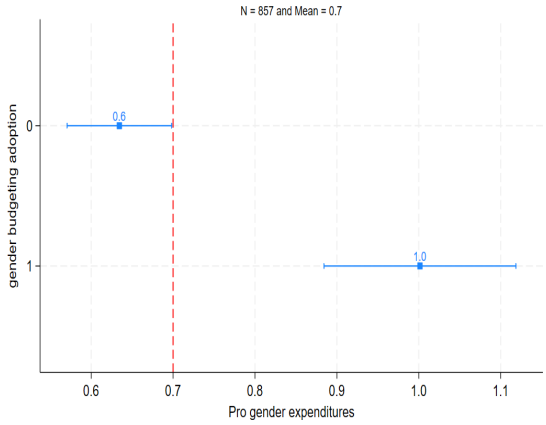
Always to analyze the potential difference in the average share of total expenditures between treated and untreated units, we computed the same statistics and made the same graph for the different components of "pro-gender" expenditures.

The graph 2 summarizes the average public expenditures for untreated (0) and treated units (1) by comparison to the global average of the sample (the vertical red line). The difference between both means suggests that the treated units seem to spend more for the three items (health, infrastructure, and education) than the untreated ones. In addition, their average expenditures in these items are more important than the average of the global sample. However, the situation is different for the water and sanitation services. Indeed, for this item, the treated units spend less on average on this item than the untreated ones. So, maybe the reallocation of public money is made to the detriment of water and sanitation services. However, we have to be cautious about this conclusion and the interpretations and need further investigations. All these elements tend to suggest that States that have adopted gender budgeting spend significantly more than the others. However, we can conclude that

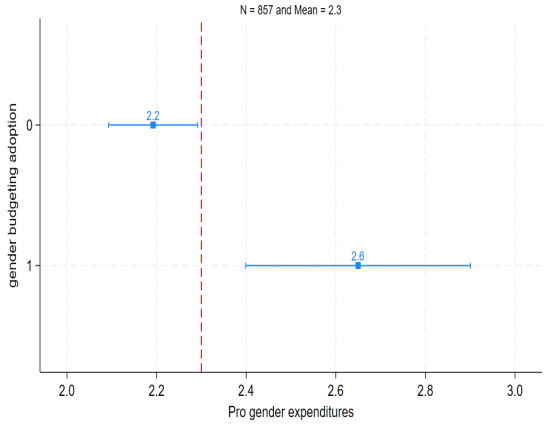
this difference is due to the gender budgeting adoption. The difference can be due to a simple correlation between the variables or to the fact that States which dedicated a greater share of their expenditures to these items got more incentives to adopt gender budgeting. This is why we have to go further in the analysis.



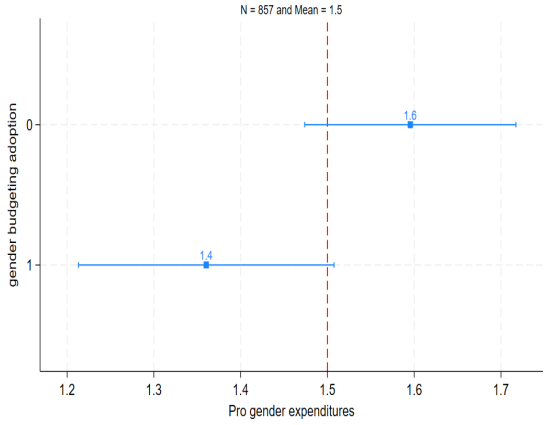
(a) Average expenditures share for health



(b) Average expenditures share for education



(c) Average expenditures share for infrastructures



(d) Average expenditures share for Water and sanitation services

Figure 2: Stylized facts

The graph 3 illustrates the staggered adoption of gender budgeting across various states in India. It highlights the timeline and sequence in which different states implemented gender budgeting practices, showcasing the varying pace of adoption. The data underscores how some states embraced the initiative earlier, while others followed more gradually, reflecting the diverse policy responses to gender equity concerns across the country.

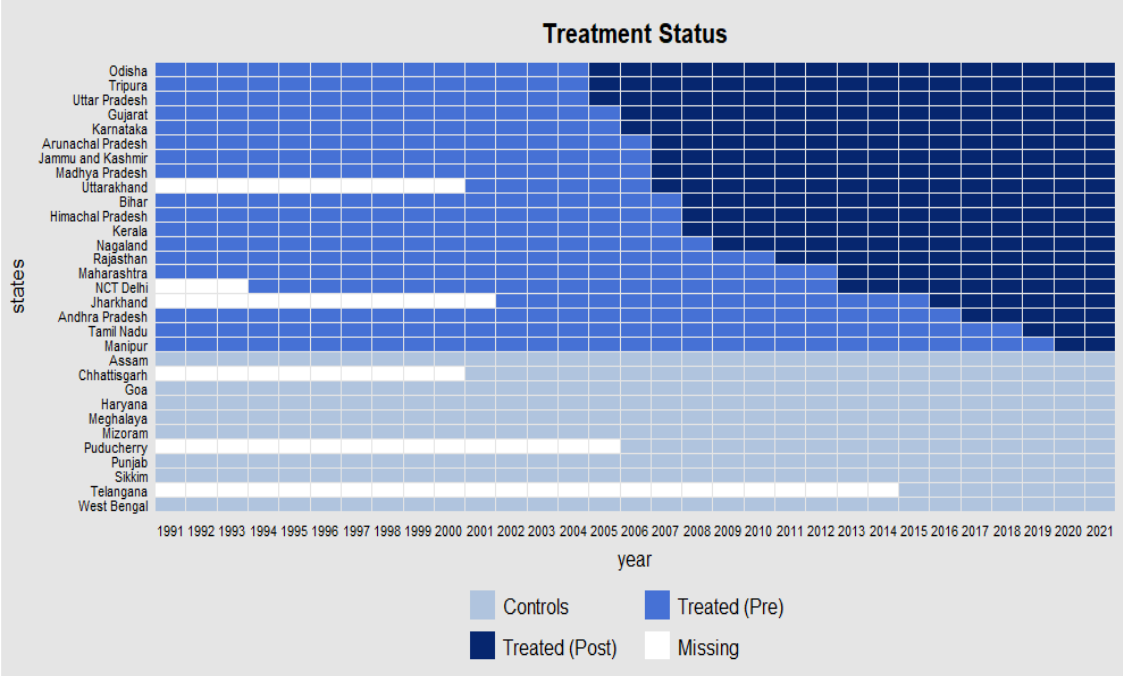


Figure 3: Gender budgeting adoption by states

Missing data often arises because some states did not exist prior to a certain point in time. Consequently, these states could not have been subject to any "treatment" immediately upon their creation. This situation ensures that there are "not yet treated" observations for all states, as newly formed states naturally fall into this category until they eventually receive treatment. This allows for a clearer comparison between treated and untreated states over time.

3.3 Identification strategy

The identification method used is a Difference in Difference (DiD) strategy, using a comprehensive panel dataset. I focus on the share of "pro-gender" public spending among the total expenditures for each State and each year through the period 1991-2020. The decision to adopt gender budgeting in each State is not random. Therefore, the main challenge is to correct for selection into the reform, i.e., to account for differences between adopter and non-adopter jurisdictions that could have influenced the outcome. The DiD identification

strategy makes it possible to correct for the initial difference in public expenditures and thus estimate the differential changes in these outcomes across states before and after each wave of adoption.

However, using several years of data makes our approach closer to two-way fixed effects (TWFE) linear regression. Recent methodological papers characterize the potential issues surrounding TWFE with multiple periods and multiple treatments (Callaway and Sant'Anna (2021), Borusyak et al. (2022) Goodman-Bacon (2021) and De Chaisemartin and d'Haultfoeuille (2020)). One issue addressed in this literature is the cross-unit heterogeneity of treatment. Other issues include the time-heterogeneity of treatment and the use of units that eventually become treated as control groups. When extending to 1991–2020, we try to capture longer-term effects and check if there is an increasing advantage of early adoption. I also acknowledge a group of states that have adopted gender budgeting after the first wave, which might slightly perturbate the control group as some units become treated. To address this, we suggest additional estimations where we explicitly account for the two types of treatment. In technical terms, we estimate the following equation in which Y_it is the outcome variable, i.e., public expenditures for State i in year t = 1, ..., T

$$Y_{it} = \alpha + \beta^W {D_{it}}^W + \rho X_{it} + \theta_i + \gamma_t + \epsilon_{it} \tag{2}$$

With the treatment dummy variable equal to 1 if the State i belongs to the group of states that have adopted gender budgeting in year k and are observed after that year.

Table 3: Repartition of treatment

Treated	223
Untreated	707

To slightly enhance the DiD setup, we use the Callaway and Sant'Anna (2021) DiD approach. The Callaway and Sant'Anna (2021) DiD estimator allows us to use inverse probability weighting as in Abadie (2005). As with Abadie (2005), we must estimate the

propensity score. However, because we have multiple treatment dates for multiple groups, there is a unique propensity score for every group. However, we do not have the luxury of a large reservoir of untreated units necessarily in many applications with multiple periods and differential timing. To create implicit pairings of units in the treatment and comparison groups, Callaway and Sant'Anna (2021) allows two options. I am using a pool of units as our comparison group who never are treated during the duration of the panel. Or we may use a pool of units that have simply not yet been treated by the time of treatment. Another key concept in Callaway and Sant'Anna (2021) is the group-time ATT. The group-time ATT is a unique ATT for a cohort of units treated at the same point in time.

The *csdid* package used for this estimation allows us to estimate with Callaway and Sant'Anna (2021) methods an estimator like Abadie (2005), but by considering the staggered adoption and heterogeneous effects. This type of approach usually brings flexibility to traditional DiD setups. Most importantly, it is used here to try to reduce unobserved time-varying differences between early- and late-gender budgeting-adopting states that could confound our results. For this, we are going to mobilize a set of variables X_{it} that are assumed to be correlated to some extent with time-varying confounders and that allow for comparing subgroups of treated and control states that are more alike.

For example, if states with the greatest GDP per capita are the ones that adopted gender budgeting first and, at the same time, are the ones that benefit from public expenditures (internal validity issue) or stand to benefit most from gender budgeting because their important GDP per capita can mean greatest interest for central government to rule this state. So, it can increase the discretionary transfers that are targeted at specific purposes (external validity issue), and then we might overstate the benefits of the gender budgeting adoption. Assuming that the unobservable advantages (e.g. economic and cultural dynamics, political leverage, or interest) are correlated with observable characteristics (e.g. population size, autonomy, GDP per capita), we could reduce the bias by comparing treated and control states that are most similar along a relevant set of observed characteristics of that sort. Rather than

using matching on many different characteristics, which brings a 'curse of dimensionality issue, we rely on a propensity score (PS) that concentrates all the useful information from these characteristics. The propensity score, denoted p hereafter, is obtained as the prediction of a first-stage estimation of a gender budgeting dummy on the set of relevant variables including key demographic dimensions such as urbanization ratio, density rate, GDP per capita, autonomy ratio (share of own revenues on total states revenues) and proportion of seats held by the women in State parliament. To consider treated and untreated states that are more like each other according to these different criteria simultaneously, we reweight observations using the inverse propensity score, as suggested by Abadie (2005) for the DiD approach. In this way, the modified estimation gives more weight to the late (early) gender budgeting adopters that are most similar to the early (late) gender budgeting adopters. I will also explore the heterogeneous impact of the reform by explicitly zooming in on groups with similar characteristics (e.g. treated and controlled states with high wealth). All estimations are clustered at the State level to account for autocorrelation.

The adoption of gender budgeting in Indian states was largely driven by institutional and political processes rather than short-term economic considerations directly linked to the outcomes studied. At the national level, the Women Component Plan of the late 1990s and the introduction of the Gender Budget Statement in 2005–06 provided a formal framework encouraging states to integrate a gender perspective into budget formulation and monitoring. States adopted gender budgeting at different points in time (Odisha in 2004, Karnataka in 2006/07, Kerala in 2008/09, among others) typically following the creation of dedicated gender cells, the publication of state-level gender budget statements, and the implementation of training programs and gender audits. Evidence from Stotsky and Zaman (2016) shows that adoption is more closely associated with political and institutional variables than with economic performance indicators, suggesting that these decisions were part of broader governance and equity initiatives rather than responses to anticipated changes in fiscal or social outcomes. This institutional narrative, combined with test showing no pre-treatment

differential trends and robustness checks supports the view that the timing of adoption can be treated as plausibly exogenous for identification purposes.

3.4 Parallel trend assumption

I compute a *t-test* to compare the mean of our outcome variable for both (adopters and non-adopters) before the first year of implementation. The results available in table 4 show that the mean of the outcome variable is relatively close for both (adopters and non-adopters) before the treatment was applied.

So, to compare treated and control states that are most similar, I also suggest DiD estimations adjusted by a quasi-matching strategy. Assuming that the matching variables are highly related to unobserved confounders, this approach should reduce the potential bias affecting trend differences between the groups of states that have adopted gender budgeting at different points in time.

Before adoption

Outcomes	Adopters	non Adopters	Difference
Pro-gender spending	5.04	4.911	
Education	0.71	0.54	
Health	0.50	0.55	
Infrastructures	2.26	2.11	
Water	1.52	1.68	

After adoption

Outcomes	Adopters	non Adopters	Difference
Pro-gender spending	5.72	4.76	***
Education	0.97	0.61	***
Health	0.73	0.53	***
Infrastructures	2.48	1.92	***
Water	1.49	1.68	*

Table 4: Outcome means before the treatment (by status)

The following graph (4) has been inspired by the work of Rambachan and Roth (2023) on a more credible approach to the parallel trend assumption. They propose some tools for robust inference in difference-in-differences and event-study designs where the parallel trends assumption may be violated. Instead of requiring that parallel trends hold exactly, they impose restrictions on how different the post-treatment violations of parallel trends can be from the pre-treatment differences in trends ("pre-trends"). They recommend that researchers use their methods to construct robust confidence intervals, under restrictions on the possible violations of parallel trends that are motivated by domain knowledge in their empirical setting. According to them, there are some key concerns about the pre-trend assumption. Despite

the statistical or visual results, it's important to consider some macroeconomic shocks that can disturb the pre-trend evolution. Figure 4 shows robust confidence sets for the treatment effect, using different values of Mbar [1]. The figure shows that if we impose Mbar$<$1, meaning that we restrict the post-treatment violations of parallel trends to be no larger than the maximal pre-treatment violation of parallel trends, then we obtain a robust confidence set for the causal effect on the expenditures share. This is wider than the original (without covariates) confidence interval, which is only valid if parallel trends hold exactly, but rule out a null effect on expenditures share.

The intuition for why the confidence sets are larger through time is that we have bound the violation of parallel trends across consecutive periods by Mbar times the max in the pre-treatment period. Thus, the identified set will be larger for later periods, since the treatment and control groups have more time to diverge. If we are willing to bound the magnitude of economic shocks by the max in the pre-treatment period, we will thus typically obtain wider confidence sets for parameters involving later periods. As suggested by Rambachan and Roth (2023) the table 4 available in the appendix summarizes the different bands of confidence interval according to the Mbar values.

[1] Mbar is a degree of smoothness, or how much we allow a violation of pre-trend assumption

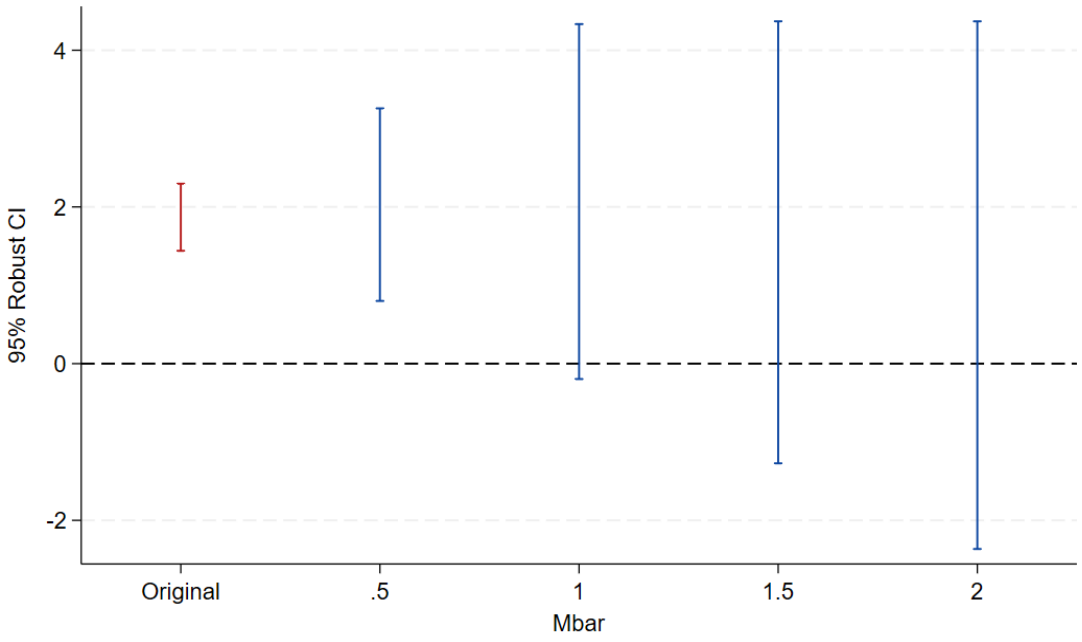


Figure 4: Parallel trend with honest DiD (Rambachan and Roth (2023))

3.5 Results

The results of the Average Treatment Effect on the Treated (ATT) are presented in Table 5. These results indicate that adopting gender budgeting has a positive and significant effect on the overall share of gender-friendly spending, as well as on specific items such as education and infrastructure for Indian states. This finding supports the idea that education and infrastructure are key sectors for reducing gender inequalities and are commonly highlighted as priorities in gender budgeting statements. This result is also consistent with Montes et al. (2019), who show that fiscal transparency can improve the allocation and efficiency of public spending, especially in democratic contexts. In India, where regular elections and political alternation strengthen vertical accountability, the disclosure obligations linked to gender budgeting increase transparency and can push local governments to reallocate more spending towards visible, pro-gender sectors to maintain credibility and voter support.

The lack of significant effects for health and water may reflect slower or less visible impact pathways compared to education and infrastructure. Improvements in health and water

typically require sustained investments and longer time horizons to generate outcomes that voters can clearly observe and attribute to local government action. In contrast, spending on education or infrastructure can deliver faster, more tangible results—such as schools, roads, or facilities—that are immediately visible and politically rewarding within electoral cycles. Furthermore, as health and water are basic services, non-adopter states could also maintain or gradually expand spending in these areas to meet minimum development standards, which reduces the difference between adopters and non-adopters and makes the reallocation harder to isolate over the study period.

Table 5: Diff-in-Diff results

	pro-gender	education	health	infrastructures	water
ATT	1.706**	0.371***	0.219	1.040***	0.102
	(2.41)	(3.59)	(1.28)	(2.69)	(0.25)
Observations	668	668	668	668	668

t statistics in parentheses

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

To examine the dynamics over time, we also estimate an event-study model. Figure 5 shows that the adoption of gender budgeting is associated with a clear, sustained increase in the share of "pro-gender" spending. States that adopt gender budgeting consistently devote a larger proportion of their budgets to priority sectors such as education and infrastructure compared to non-adopters. This positive effect appears to hold over time, indicating that gender budgeting can contribute to a lasting reorientation of spending toward gender-friendly priorities. The event-study by components are available in appendix section (c.f figure A.2).

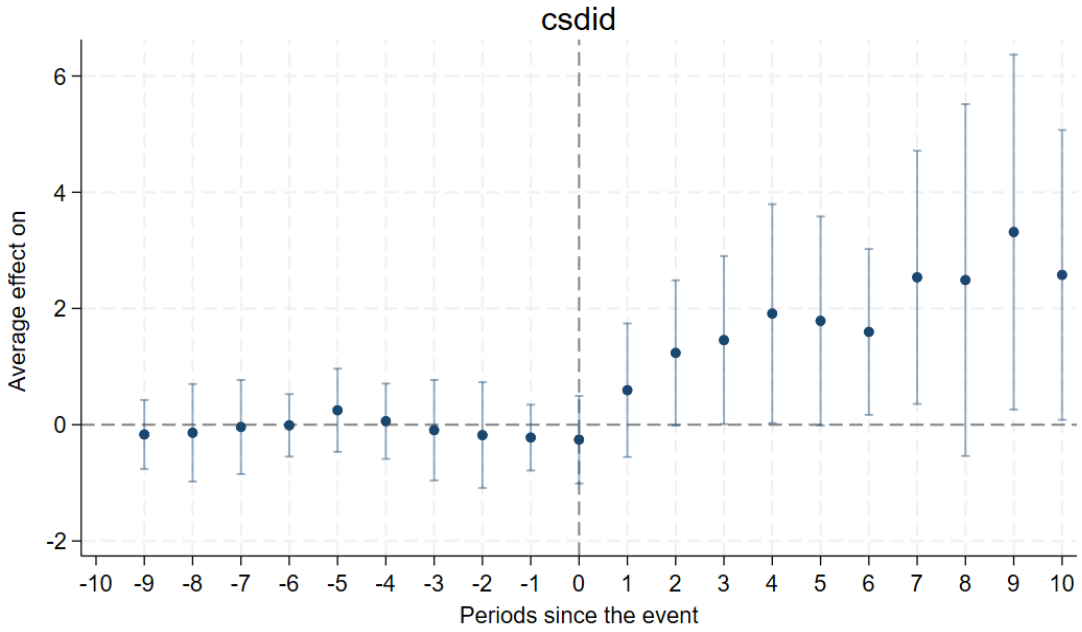


Figure 5: Event study results for "pro-gender" spending

4 Robustness Checks

4.1 Alternative DiD Estimators

As a robustness check, we complement our main results with additional Difference-in-Differences estimators that are also well suited to staggered adoption settings. First, we use the estimator proposed by Wooldridge (2021), which is designed for cases where treatment is implemented at different times across units. Wooldridge shows that the standard two-way fixed effects (TWFE) estimator is equivalent to a pooled OLS regression with unit-specific time averages and time-period-specific cross-sectional averages—what he calls the two-way Mundlak (TWM) regression. This flexible approach allows for considerable heterogeneity in treatment effects across time, treatment intensity, and covariates, and it can easily accommodate heterogeneous trends when the common trends assumption may not strictly hold. This makes the Wooldridge estimator a good fit for our context, where states adopt gender budgeting in different years and at different intensities. Table A.1 available in appendix

section shows that this estimator confirms a positive and significant effect of gender budgeting adoption on overall pro-gender spending as well as on education and infrastructure spending, supporting our main findings. In addition, figure A.3 also available in appendix section shows the event-study estimates using the Wooldridge framework, confirming that the positive effect is consistent over time.

To further strengthen our robustness checks, we also implement the estimators by Borusyak et al. (2022) and De Chaisemartin and d'Haultfoeuille (2020). The estimator of Borusyak et al. (2022) uses the average outcome from the entire pre-treatment period, which can increase precision but may be more sensitive to violations of the parallel trends assumption when trends diverge over longer horizons. In contrast, the estimator by Callaway and Sant'Anna (2021) uses only the last pre-treatment period as baseline, imposing weaker assumptions about parallel trends before treatment. Depending on the structure of pre-treatment dynamics, these choices help check the sensitivity of our results to alternative parallel trends assumptions. Finally, the estimator by De Chaisemartin and d'Haultfoeuille (2020) is especially relevant because it allows for treatment that can switch on and off and can handle both binary and continuous treatment. This flexibility is well suited for gender budgeting, which may have an evolving effect as states gain more experience with implementation over time. We define continuous treatment as the time since first adoption to capture whether earlier adopters show stronger or more persistent effects. The detailed results for Borusyak et al. (2022) are presented in Figure A.4 in the appendix. The results for the De Chaisemartin and d'Haultfoeuille (2020) estimator are provided for both the binary treatment in Figure A.5 and the continuous treatment in Figure A.6 also in the appendix. Together, these additional checks provide reassurance that our main findings are robust to alternative identification strategies and assumptions.

4.2 Entropy Balancing

For the robustness check, we also use the entropy balancing method of Hainmueller (2012), as applied by Baccini et al. (2018) in work on fiscal decentralization and tax competition between local jurisdictions. Entropy balancing is particularly relevant here because macroeconomic shocks and political dynamics may change voters' and local governments' expectations, independently of gender budgeting adoption. The mere announcement of gender budgeting could raise citizens' demands for better services, while non-adopting states may also adjust spending quality to stay electorally competitive. This competition effect can affect pre-trends and bias DiD estimates. Therefore, we complement several DiD estimators with entropy balancing, which does not rely on the parallel trends assumption and is also well suited for our setting.

In general, matching estimators aim to mimic the random assignment of treatment by constructing a control group that is statistically comparable to the treated group in terms of all relevant pre-treatment characteristics. Entropy balancing achieves this by pre-processing the data with a reweighting scheme that forces exact balance on specified moments of covariates. These weights can then be used in a regression framework just like sampling weights, ensuring that our treatment and control groups are directly comparable on observed characteristics that affect selection and outcomes. This is valuable for our panel data, where states differ in many structural factors that could bias the results if not well balanced.

Unlike traditional matching, which may struggle when the number of untreated units is limited or covariates are numerous, entropy balancing optimally adjusts weights to guarantee sufficient covariate balance. This reduces the risk of bias due to poor matches or model misspecification. Moreover, because it is non-parametric, entropy balancing does not require assumptions about the functional form of the treatment assignment process or the outcome model, which further limits risks of bias from incorrect specification.

Importantly, by combining reweighting with panel regressions, we can control for state and year fixed effects, accounting for unobserved heterogeneity across states and time. This

makes entropy balancing a robust complement to our DiD analysis, especially given the heterogeneity in adoption timing and the possibility of anticipatory effects or local shocks.

The Average Treatment Effect on the Treated (ATT) is estimated as:

$$\pi ATT(x) = E[Y(1)|T = 1, X = x] - E[Y(0)|T = 0, X = x] \tag{3}$$

where Y is the outcome, X is the vector of balanced pre-treatment covariates, and T indicates treatment.

Our empirical model combines the entropy weights with a fixed effects panel regression:

$$Y_{it} = \beta_1 T_{it} + \alpha_1 \log(GDP\_pc)_{it} + \alpha_2 \log(density)_{it} + \alpha_3 X_{it} + \mu_i + \psi_t + \epsilon_{it} \tag{4}$$

where Y_{it} is the outcome for state i at time t, T_{it} is the treatment variable (1 if gender budgeting is adopted, 0 otherwise), X_{it} includes other controls, μ_i are state fixed effects, ψ_t are year fixed effects, and ϵ_{it} is the error term.

Entropy balancing is therefore an appropriate robustness check for our context, as it ensures balanced groups, handles limited untreated units, and fits well with our unbalanced panel structure and the policy variation we study.

The entropy balancing results, shown in Figure B.2, confirm the main findings. They suggest that gender budgeting adoption is associated with a significant increase in the share of total spending devoted to pro-gender sectors, especially education and infrastructure. However, consistent with the other estimators, the effect for health remains statistically weak, suggesting that improvements in this sector may require sustained investment and more time to materialize. Notably, the negative and significant result for water spending indicates that this sector may become less of a priority for adopter states once other visible or politically rewarding sectors, like infrastructure, attract more funds. This shift could reflect strategic choices by local governments under limited budget constraints, prioritizing expenditures that are more visible and electorally beneficial. Overall, these findings reinforce the robustness of

our results by providing evidence that does not depend on the common trends assumption and by highlighting how gender budgeting can drive selective reallocation within the public budget.

4.3 Dose Response Function

To ensure the robustness of our findings, we also estimate a Dose Response Model (DRM). The DRM makes it possible to test whether the effect of gender budgeting varies with the intensity of the treatment, measured here by the duration since the first adoption. This is relevant because states that adopted gender budgeting earlier may have more experience implementing its principles and standard templates, which could reinforce its effects over time through institutional self-enforcement.

The DRM is particularly appropriate in this context as it allows us to examine the effect of a continuous treatment in the presence of possible endogenous selection, as developed by Cerulli (2015) and applied by Avenyo et al. (2019) and Janzen et al. (2023). This approach also complements and confirms the results obtained with the De Chaisemartin and d'Haultfoeuille (2020) estimator by testing the same identification strategy with a continuous version of the treatment variable. In this way, the DRM provides an additional layer of evidence to assess whether the duration of gender budgeting adoption strengthens the effects observed with binary treatment indicators.

Moreover, this framework allows us to test a learning by doing hypothesis, whereby the repeated application of gender budgeting practices and templates over time could gradually improve the alignment between fiscal allocations and gender equality objectives. The results confirm that a longer duration of gender budgeting adoption is associated with a significant increase in the share of spending allocated to pro-gender sectors, particularly education and infrastructure. The estimates also show a positive and significant effect for health spending when treatment duration is considered, while the effect for water remains statistically insignificant.

Overall, these findings support the idea that the impact of gender budgeting is reinforced over time through experience and institutionalisation, especially for priority sectors that are visible and politically salient. The detailed estimates from the DRM are presented in Table C.1 in the Appendix.

4.4 Placebo Test

I now examine whether there are confounding factors that could affect the results, which have remained stable so far (especially for education expenditures share). The empirical literature shows that the adoption of an economic policy is generally associated with parallel reforms, making the adoption of gender budgeting a non-random factor. One could therefore imagine that unobservable variables correlated with policy adoption and potentially with the outcome variable could affect the baseline results. While we are aware that the empirical — method used in this study aims to address these types of concerns, we still — strengthen the results by conducting a placebo test on gender budgeting adoption. To do this, we follow Apeti (2023) and Apeti and Edoh (2023) in setting placebo or arbitrary dates for gender budgeting, computed by randomly assigning gender budgeting episodes to countries in our sample after removing the actual adoption years. The main idea behind this test is that if the results are biased by unobservable variables, the placebo — test might also show significant effects. Random treatments within the sample do not affect both education and health expenditures share in total expenditures (Table D.1, in Appendix). Therefore, we can rule out the possibility of confounding — factors influencing our results.

4.5 Anticipation effects

Always to check the robustness of our results and be sure that the effects observed are due to the treatment adoption, we change the date of the adoption to test for potential anticipation effects. An example of anticipation effects could be the fact that the reform could be discussed in newspapers years before their adoption and that there are economic or political reasons for

rulers to change spending allocation before reforms. So, the anticipation effect can have an impact on the size of the outcome and the treatment effects estimation (Mertens and Ravn (2012) and Metiu (2021)) By construction, De Chaisemartin and d'Haultfoeuille (2020) use a placebo to estimate the pre-trend coefficients to assess the evolution of outcome if they were not treated. So, the fact that these coefficients are not significant during the three first periods before the adoption could mean an absence of anticipation effects.

However, we change the adoption wave date by considering that the treatment has been adopted two years before the effective date of adoption to test the presence or not of anticipation effects. The results obtained by using Callaway and Sant'Anna (2021) are presented in the appendix section at the table E.1.

The results show a non-significant effect for our alternative adoption waves. I can conclude that an absence of anticipation effects of gender budgeting adoption on the "pro-gender" public spending allocation. However, we found an existing anticipation effect for education allocation spending. This effect is less important than the effect after the adoption, and the anticipation effect didn't seem to explain all the results for education spending allocation.

4.6 Narrowing the control window

Finally, the effect captured in this work may suffer from some problems. Indeed, gender budgeting adoption can lead to a change in States' environments. In this sense, the effect captured may not be due to gender budgeting but to changes in institutional, political, social, or economic conditions after its adoption. Also, any other characteristic that may determine gender budgeting adoption may be a source of endogeneity. To circumvent these problems, we employ a similar approach as Neuenkirch and Neumeier (2015), Apeti (2023), and Apeti and Edoh (2023) by removing all observations before and after the initial year of adoption. Thus, we expect that this narrow time window characterizing our new treatment variable should provide a more robust estimate of its effect on public expenditures since the (generally slow-changing) institutional, political, social, and economic environment is more likely to be

stable over a narrow time window. In total, we explore the robustness of our findings with two modifications to the sample period. In addition to the first adoption period, we consider (i) a window of five years around it, and (ii) a window of three years around it.

Using entropy balancing with this narrow time window, table F.1 provides results that reinforce our previous findings. Thus, we can conclude that it seems unlikely that the estimated effects of gender budgeting are due to a coincidental change in the institutional, political, social, and economic environment in the gender budgeting adopters States' or to any other characteristics that may predict its adoption.

5 Microeconomic Effects

Beyond the effects that gender budgeting adoption can have on fiscal policy strategies and how it led to an increase of fiscal space dedicated to gender issues, we tried to assess how this can affect women daily lives. For example, Clots-Figueras (2011) finds that politicians' gender affects policy, but that their social position, i.e., their caste, should be consider as well. Female legislators in seats reserved for lower castes and disadvantaged tribes invest more in health and early education and favor "women-friendly" laws, such as amendments to the Hindu Succession Act, which was designed to give women the same inheritance rights as men. They also favor redistributive policies, such as land reforms. In contrast, female legislators from higher castes do not have any impact on "women-friendly" laws.

In addition, India is a very large country with very large states. Indeed, some Indian states like Rajhastan are greater and more populous than countries like Finland, Norway, or Ivory Coast. So, it could be interesting to check the potential effect at the very local and individual level. It's also important to notice that gender budgeting seems to become bottom-up approach. That means it is not the allocation of resources in the budget at national and or state levels that has to see but the resources that flow to and are available to women at the field level i.e. the women in the villages, cities and towns of the country that

need to be monitored (Sharma and Garg (2014)). To measure the effect of this policy reform of women empowerment and/or gender equality, we used intimate partner violence (IPV) as a measure of gender equality evolution and women empowerment. Indeed, we assumed that an improvement of women empowerment will lead to a decrease of the likelihood to accept or agree with IPV. Schuler and Nazneen (2018) findings suggest that women's empowerment has evolved in several ways that may be contributing to reductions in IPV: in its magnitude (for example, many women are earning more income than they previously did), in women's perceived exit options from abusive marriages, in the propensity of community members to intervene when IPV occurs, and in the normative status of empowerment (it is less likely to be seen as transgressive of gender norms). Dalal (2011) shows that economic empowerment is not the sole protective factor. Economic empowerment, together with higher education and modified cultural norms against women, may protect women from IPV. By focusing on the education and integrating ender issues into fiscal policy strategies and reflexion gender budgeting can help to reduce IPV. So, it could be an interesting outcome to assess the microeconomic effect (on women) of gender budgeting adoption.

5.1 Data and empirical strategy

Data on IPV come from the *Data Health Survey* (DHS), which have been conducted in Indian states since the 1990's. The DHS household surveys typically interview a representative sample of between 10,000 to 20,000 women (aged 15-49) and men (aged 15-59). By collecting answers about IPV among others from representative samples of the population, the DHS Program provides representative estimates of IPV tolerance rates among Indian states.

To assess the microeconomic effects of gender budgeting adoption, we use the three last waves of *Data health survey*. This choice is due to the availability of data about IPV tolerance from the respondents. I also merge the DHS repeated cross sections dataset with the previous dataset with macroeconomic indicators at states level. This process leaves me with a dataset combining macro and micro indicators for a sample of around 75,000 women in 31 Indian

States/UT The use of many waves allows to consider a potential time effect on the IPV tolerance among states and check the effect of the time since the first adoption wave.

The next table summarizes the main variables used for our probit regression analysis on the microeconomics effects of gender budgeting adoption.

Table 6: Summary statistics

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Domestic violence against wife:					
unfaithful	0.248	0.432	0	1	38483
disrespect	0.357	0.479	0	1	38540
roof materials	37.065	18.292	11	97	38540
number of children	2.36	1.345	0	14	45467
backward class or casts	2.585	1.089	1	8	43729
religion	2.628	10.637	1	96	45467
log(gdp per capita)	11.354	0.474	10.278	12.728	37669
urbanization	34.182	13.18	9.83	71.400	35257
women in parliement	48.546	1.637	44.47	52.49	37657
log(population)	3.344	1.731	-0.635	5.476	35257
dose	4.76	5.421	0	15	45467
partner education	2.624	1.588	0	8	74021

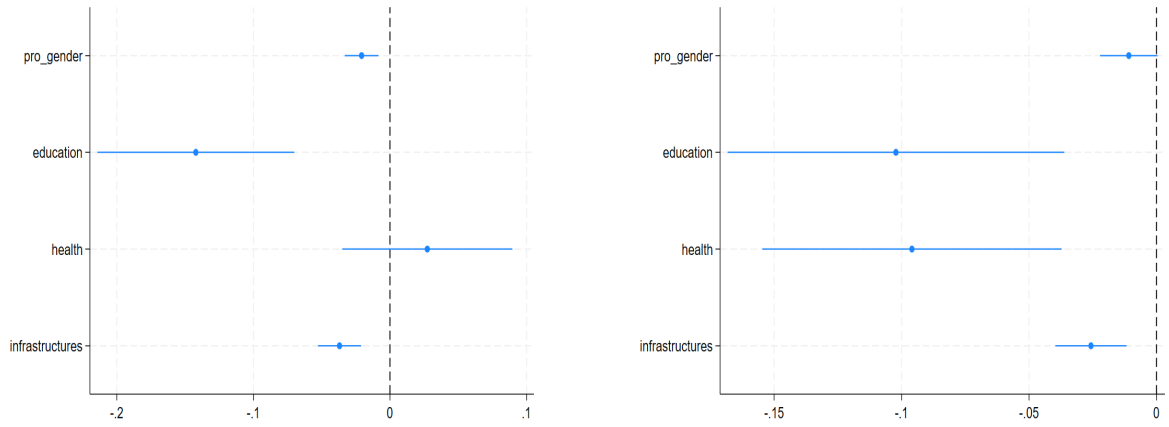
The dependent variable is a binary variable coded as 1 (if the respondent considers as normal to beat a wife for specific reasons regardless of high or low intensity) and 0 (otherwise). The variable of interest is the time (in year) since the first implementation of gender budgeting (to measure the intensity of the treatment). Given the qualitative nature of the dependent variable, the preferred estimation method for estimating equation (1) is the probit model. Compared to the linear probability method and the logit model, the probit model is the most effective and efficient in estimating the qualitative model. For the variable of interest, we used

the share of so called "pro-gender" spending and its components to ensure that this increase of the fiscal space dedicated to these items can effectively improve the women situation and help to their empowerment. Unlike the linear model, the coefficients from the Probit model estimations are not directly interpretable. They are interpreted in terms of marginal effects. The sign and significance of the parameters provide an indication of the impact of explanatory variables on the probability of observing the dependent variable's occurrence. The relevance of the identification strategy is verified through sensitivity analysis.

5.2 Results

The results suggest a negative effect of pro-gender and education public spending increase on the likelihood for a woman to consider as acceptable some Intimate Partner Violence (IPV) for unfaithful and disrespect. Women are also less likely to agree with the fact that their partners have sexual relationship with other women. The women who live in states that have implemented gender budgeting seem to be more aware of their rights. The results seem to be more important with the increase of public spending education spending.

The next graphs summarize the results for both outcomes.



(a) Marginal effect on IPV for unfaithful (b) Marginal effect on IPV for disrespect

Figure 6: Marginal effects of pro-gender public spending on IPV

The tables G.1 and G.2 available in appendix also show the results from a numerical

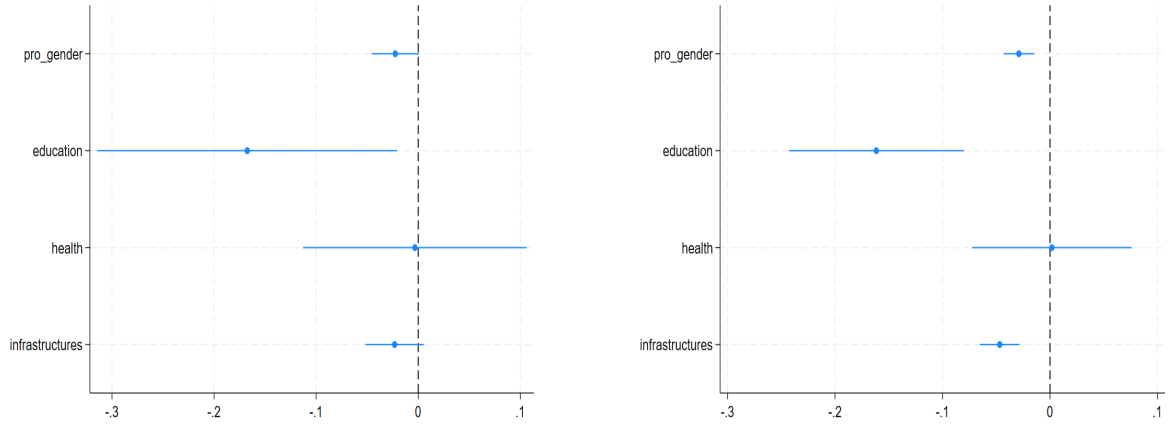
point of view.

5.3 Sensitivity Analysis

To assess the potential factors that can affect women's empowerment, we conducted a sensitivity analysis to see if living areas or educational attainment could influence my results. Indeed, living areas could significantly impact the effectiveness of gender budgeting. In rural regions, implementing and monitoring gender budgeting can pose challenges due to limited infrastructure, lower administrative capacity, and potential lack of awareness or political will. Additionally, funds designated for gender-focused initiatives may disproportionately benefit urban areas where resources and administrative capabilities are more concentrated. This potential urban bias can lead to an unequal distribution of benefits, undermining efforts to address gender disparities in rural communities. Consequently, the intended outcomes of gender budgeting—such as improved access to education, healthcare, and economic opportunities for women—may not be fully realized in rural areas, exacerbating existing inequalities (Bhana (2010)). So, it could be interesting to check if this factor among others can affect my results.

To examine these effects, we recompute the same equation, first using only observations from urban areas and then from rural areas. This allows me to determine if the results remain significant in both contexts and to compare the magnitude of these effects between urban and rural areas. By isolating urban and rural observations, we can assess whether the implementation and impact of gender budgeting differ across these living areas, providing insight into any disparities and helping to tailor more effective gender-focused policies for each context.

The next graphs (7a and 7b) summarize the results for IPV because of unfaithful from women and show that the effects remain positive and significant in both areas and are mainly driven by education public spending increase.



(a) Marginal effect on IPV in urban areas (b) Marginal effect on IPV in rural areas

Figure 7: Marginal effects of pro-gender public spending on IPV because of unfaithful

The tables available in appendix also show the results from a numerical point of view.

The results indicate that both overall pro-gender spending and education significantly impact domestic violence rates. However, spending on infrastructure, particularly roads and bridges, is notably more significant and exerts a stronger effect in rural areas. This heightened impact can be attributed to several factors. Firstly, improved roads and bridges enhance accessibility to essential services, such as healthcare and law enforcement, which are crucial in addressing and preventing domestic violence. Secondly, better transportation networks facilitate economic opportunities, reducing financial stress and associated violence. Lastly, enhanced infrastructure promotes social connectivity and support networks, which are vital in rural areas where isolation can exacerbate domestic violence situations. Therefore, infrastructure improvements in rural regions could play a critical role in mitigating domestic violence.

Education spending is also significant and exerts an important effect in rural areas. This heightened impact can be attributed to several factors. Firstly, improved education enhances awareness and understanding of domestic violence, equipping individuals with the knowledge to address and prevent it. Secondly, better educational opportunities lead to economic empowerment, reducing financial stress and associated violence. In urban areas,

education fosters diverse social connectivity and support networks, while in rural areas, it mitigates isolation that can exacerbate domestic violence situations. Therefore, education improvements play a critical role in mitigating domestic violence across both urban and rural regions.

In the next section, we have tried to check the potential transmission channels that can explain my results

6 Transmission channels

To identify the potential transmission channel, we built a ratio of *Centrally Sponsored Schemes* (CSS) on the State revenues and State expenditures. This construction aims to check if the increase in health and education expenditures can be due to an increase in transfers received by each state. Indeed, *Centrally Sponsored Schemes* are some transfers decided by central ministries and spent for some specific purposes such as education and health (which is on a *Concurrent List* between States and Central government). I am not able to collect data about the different schemes and only keep those related to health and education. The variables are summarised just below.

Table 7: Transmission channel

	CSS(% of revenues)	CSS (% of expenditures)	Credibility
Before adoption	4.08	4.18	4.13
After adoption	5.82	6.05	6.51
Non Gender Budgeting	4.54	4.60	5.33

I try to estimate the potential transmission channels by using the same process as Neuenkirch and Neumeier (2016) I compute the means of the two variables for (a) the treatment group during times when gender budgeting is in place, (b) the treatment group

focusing only on years before gender budgeting implementation, and (c) our synthetic control group obtained via entropy balancing. The results are outlined in table 7 just above. The descriptive statistics indicate some differences between the control group obtained via entropy balancing and states which apply gender budgeting. When comparing the control group to the treatment group before gender budgeting was applied, however, we find that the latter is characterized by a notably greater share of CSS for both measures.

Indeed, before the treatment, the treated units received less CSS in the percentage of revenues (4.08% versus 4.54%) and expenditures (4.18% versus 4.60%) than the non-adopter ones, but the situation became different after the adoption for revenues (5.82 vs 4.54) and expenditures (6.05 vs 4.60).

However, we can't conclude from this statistical test that gender budgeting adoption reduces the state's autonomy. The increase of CSS received by the States can be due to the wish of the central government to fund some projects decided by State governments to reach their objectives, but it can also be an incentive to adopt gender budgeting and mean for the central government to influence the state's decisions. I can only conclude that an increase in CSS received by the States could be a potential transmission channel to explain the greater share of health and education expenditures for the adopter States.

I also compute a kind of credibility index. To do it, we compute the difference between the share of "pro-gender"expenditures expected in the budget announcement and the share of "pro-gender"expenditures in the States financial account. I have assumed that this bias index will allow us to apprehend the performance of subnational administration. The differences between forecast and realization are possible and usual, but a systematic and important difference may mean a lower level of performance in its administration. I compute it as an absolute value. *The absolute value refers to the fact that we multiply the negative value by -1 to get only positive values* to consider the distance (bias) between the forecast and the realization. I made it because a systematic underestimation of expenditures in the forecast could be good news in terms of available funding, but it's not good news from the credibility

and local administration capacities point of view. The credibility index is computed as:

$$Bias_{it} = \left(\frac{Pro\_gender_{it}}{Total\_expenditure_{it}} - \frac{Pro\_gender\_expected_{it}}{Total\_expenditure\_expected_{it}}\right) * 100 \tag{5}$$

Where $Pro\_gender\_expected$ represents the expenditures previously considered as "pro-gender"in the budget announcement. They are expressed as a percentage of Total expenditures also expected in the budget announcement ($Total\_Expenditures\_expected$).

However, to make it easy to interpret and more intuitive, Itransform it under the form:

$$credibility_{it} = \frac{1}{Bias_{it}} \tag{6}$$

The results available in table 7 also suggest that Gender Budgeting adopters seem to become more credible than non-adopters.

As part of my analysis of potential transmission channels at microeconomic level, we conducted probit regressions to assess how the implementation of gender budgeting and the time since its adoption affect individuals' sources of information, particularly regarding family planning, which pertains to women's reproductive health. These sources of information are crucial as they enable women to make informed decisions and choices about their bodies. Given this context, examining these choices as an output is both relevant and necessary for understanding the broader impact of gender budgeting on women's autonomy in reproductive health decisions. The next graph (8) shows that individuals residing in states that have adopted gender budgeting are more likely to receive information about family planning through radio, newspapers, and television. This result may reflect a genuine commitment by these adopting states to implement measures that uphold their commitments and maintain credibility in their public announcements.

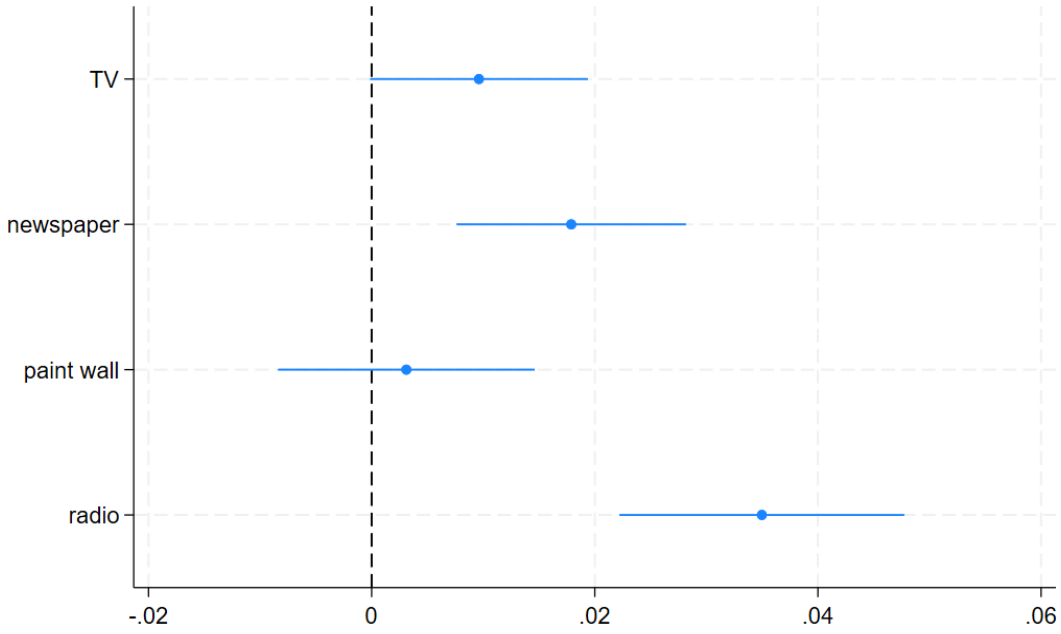


Figure 8: Marginal effects of gender budgeting adoption on information access

7 Conclusion

This paper has examined how the adoption of gender budgeting influences the composition of public spending in Indian states over the period 1991–2020, with particular attention to the share of resources allocated to so-called pro-gender sectors. The analysis started from the observation that while gender inequalities persist, the practical effectiveness of gender budgeting in reshaping budget priorities at the local level remains insufficiently studied. To address this, the paper combined a theoretical framework based on a principal-agent relationship and the role of transparency with an empirical strategy that uses multiple identification methods.

Using a range of Difference-in-Differences estimators adapted to staggered adoption, an entropy balancing approach to address possible selection bias and pre-trend concerns, and a Dose Response Model to capture the effects of treatment intensity over time. We provide robust evidence that states implementing gender budgeting tend, on average, to devote a

greater share of their budgets to sectors such as education and infrastructure. The findings for health spending remain more modest, while spending on water appears less prioritized or even reduced in relative terms, suggesting that not all sectors are equally affected by this reallocation.

These results confirm that gender budgeting can translate into concrete shifts in fiscal policy, particularly when supported by clear reporting requirements and ongoing assessment mechanisms that enhance governments' credibility. The study also shows that these spending reallocations are not merely symbolic but can be linked to micro-level improvements, including changes in attitudes towards women's rights and a decline in the acceptance of domestic violence. This highlights the potential of fiscal policy tools, when combined with transparency, to influence not only budget structures but also social outcomes (Samarakoon and Parinduri (2015) and Polzer et al. (2021)).

From a research perspective, this paper contributes by documenting the concrete budgetary consequences of gender budgeting in a large federal setting where states have significant discretion over public spending. It demonstrates the value of using multiple complementary estimation strategies to test the robustness of results when policies are rolled out at different times and with varying degrees of intensity.

In terms of policy, the findings suggest that gender budgeting can help align declared gender equality goals with actual spending decisions, provided transparency mechanisms are credible and effectively implemented. However, the results remain limited to the sectors and channels examined, and broader questions about fiscal autonomy and intergovernmental relations in India merit further investigation.

Future work could deepen this analysis by exploring additional transmission mechanisms, such as the role of local political competition, administrative capacity, or citizen participation in budget monitoring. Examining how repeated use of gender budgeting reinforces institutional learning and affects longer-term development outcomes would also be valuable.

Overall, this paper shows that gender budgeting, when genuinely adopted and accompanied

by credible transparency tools, can help redirect public spending toward sectors that advance gender equality and deliver tangible benefits for local populations.

References

Abadie, A. (2005). Semiparametric difference-in-differences estimators. *The review of economic studies*, 72(1):1–19.

Apeti, A. E. (2023). Household welfare in the digital age: Assessing the effect of mobile money on household consumption volatility in developing countries. *World Development*, 161:106110.

Apeti, A. E. and Edoh, E. D. (2023). Tax revenue and mobile money in developing countries. *Journal of Development Economics*, 161:103014.

Avenyo, E. K., Konte, M., and Mohnen, P. (2019). The employment impact of product innovations in sub-saharan africa: Firm-level evidence. *Research Policy*, 48(9):103806.

Baccini, L., Li, Q., Mirkina, I., and Johnson, K. (2018). Regional competition, business politicians, and subnational fiscal policy. *Business and Politics*, 20(3):410–437.

Besley, T. and Smart, M. (2007). Fiscal restraints and voter welfare. *Journal of public Economics*, 91(3-4):755–773.

Bhana, D. (2010). "here in the rural areas they don't say that men and women are equal!" contesting gender inequalities in the early years. *Agenda*, 24(84):9–18.

Blagrave, P. and Gonguet, F. (2020). Enhancing fiscal transparency and reporting in india.

Borusyak, K., Jaravel, X., and Spiess, J. (2022). Revisiting event study designs: Robust and efficient estimation. *Available at SSRN 2826228*.

Buchmann, C., DiPrete, T. A., and McDaniel, A. (2008). Gender inequalities in education. *Annu. Rev. Sociol*, 34:319–337.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

Calvo, C. M. et al. (1994). *Case study on intermediate means of transport: bicycles and rural women in Uganda*. Citeseer.

Cannonier, C. and Mocan, N. (2018). The impact of education on women's preferences for gender equality: Evidence from sierra leone. *Journal of Demographic Economics*, 84(1):3–40.

Cerulli, G. (2015). ctreatreg: Command for fitting dose–response models under exogenous and endogenous treatment. *The Stata Journal*, 15(4):1019–1045.

Chakraborty, L. (2016). Asia: A survey of gender budgeting efforts.

Clots-Figueras, I. (2011). Women in politics: Evidence from the indian states. *Journal of public Economics*, 95(7-8):664–690.

Dalal, K. (2011). Does economic empowerment protect women from intimate partner violence? *Journal of injury and violence research*, 3(1):35.

Das, M. S., Jain-Chandra, M. S., Kochhar, M. K., and Kumar, N. (2015a). *Women workers in India: why so few among so many?* International Monetary Fund.

Das, S., S., J.-C., K., K., and N., K. (2015b). Women workers in india: Why so few among so many.

De Chaisemartin, C. and d'Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, 110(9):2964–2996.

Doepke, M., Tertilt, M., and Voena, A. (2012). The economics and politics of women's rights. *Annu. Rev. Econ.*, 4(1):339–372.

Elomäki, A. and Ylöstalo, H. (2021). Gender budgeting in the crossroad of gender policy and public financial management: The finnish case. *Public Money & Management*, 41(7):516–526.

Fiva, J. H. (2006). New evidence on the effect of fiscal decentralization on the size and composition of government spending. *FinanzArchiv/Public Finance Analysis*, pages 250–280.

Gadenne, L. (2017). Tax me, but spend wisely? sources of public finance and government accountability. *American Economic Journal: Applied Economics*, pages 274–314.

Goetz, A. M. and Jenkins, R. (2001). Hybrid forms of accountability: citizen engagement in institutions of public-sector oversight in india. *Public management review*, 3(3):363–383.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277.

Hainmueller, J. (2012). Entropy balancing for causal effects: A multivariate reweighting method to produce balanced samples in observational studies. *Political analysis*, 20(1):25–46.

Janzen, J. P., Malone, T., Schaefer, K. A., and Scheitrum, D. P. (2023). Political returns to ad hoc farm payments? *Applied Economic Perspectives and Policy*, 45(1):555–578.

Jung, S.-M. (2022). Determinants of gender budgeting practices: Evidence from municipal governments in south korea. *Public Performance & Management Review*, pages 1–30.

Khera, P. (2016). *Macroeconomic impacts of gender inequality and informality in India*. International Monetary Fund.

Kleven, H. and Landais, C. (2017). Gender inequality and economic development: fertility, education and norms. *Economica*, 84(334):180–209.

Mbodji, Y. C. (2023). Effects of public expenditure on education on gender inequality in education in sub-saharan africa (ssa). *Review of Education*, 11(2):e3398.

Meckling, W. H. and Jensen, M. C. (1976). Theory of the firm: Managerial behavior, agency costs and ownership structure. *Journal of financial economics*, 3(4):305–360.

Mertens, K. and Ravn, M. O. (2012). Empirical evidence on the aggregate effects of anticipated and unanticipated us tax policy shocks. *American Economic Journal: Economic Policy*, 4(2):145–181.

Metiu, N. (2021). Anticipation effects of protectionist us trade policies. *Journal of International Economics*, 133:103536.

Montes, G. C., Bastos, J. C. A., and de Oliveira, A. J. (2019). Fiscal transparency, government effectiveness and government spending efficiency: Some international evidence based on panel data approach. *Economic Modelling*, 79:211–225.

Neuenkirch, M. and Neumeier, F. (2015). The impact of un and us economic sanctions on gdp growth. *European Journal of Political Economy*, 40:110–125.

Neuenkirch, M. and Neumeier, F. (2016). The impact of us sanctions on poverty. *Journal of Development Economics*, 121:110–119.

Okojie, C. E. (1994). Gender inequalities of health in the third world. *Social science & medicine*, 39(9):1237–1247.

Parikh, P., Fu, K., Parikh, H., McRobie, A., and George, G. (2015). Infrastructure provision, gender, and poverty in indian slums. *World Development*, 66:468–486.

Polzer, T., Nolte, I. M., and Seiwald, J. (2021). Gender budgeting in public financial management: a literature review and research agenda. *International Review of Administrative Sciences*, page 00208523211031796.

Puig-Barrachina, V., Ruiz, M. E., García-Calvente, M. d. M., Malmusi, D., Sánchez, E., Camprubí, L., Muntaner, C., Cortès-Franch, I., Artazcoz, L., and Borrell, C. (2017). How to resist austerity: The case of the gender budgeting strategy in andalusia. *Gender, Work & Organization*, 24(1):34–55.

Quinn, S. (2016). Europe: A survey of gender budgeting efforts.

Rambachan, A. and Roth, J. (2023). A more credible approach to parallel trends. *Review of Economic Studies*, page rdad018.

Revelli, F. (2002). Local taxes, national politics and spatial interactions in english district election results. *European journal of political economy*, 18(2):281–299.

Rubin, M. M. and Bartle, J. R. (2023). Gender-responsive budgeting: a budget reform to address gender inequity. *Public Administration*, 101(2):391–405.

Sacchi, A. and Salotti, S. (2016). A comprehensive analysis of expenditure decentralization and of the composition of local public spending. *Regional Studies*, 50(1):93–109.

Samarakoon, S. and Parinduri, R. A. (2015). Does education empower women? evidence from indonesia. *World Development*, 66:428–442.

Schuler, S. R. and Nazneen, S. (2018). Does intimate partner violence decline as women's empowerment becomes normative? perspectives of bangladeshi women. *World development*, 101:284–292.

Sen, G. and Östlin, P. (2008). Gender inequity in health: why it exists and how we can change it.

Sharma, P. and Garg, P. (2014). Women empowerment through gender budgeting in india. *International Research Journal of Human Resources and Social Sciences*, 1(6).

Siwińska-Gorzelak, J., Bukowska, G., and Wójcik, P. (2020). The impact of revenue autonomy on the composition of local public spending: Evidence from poland. *Local Government Studies*, 46(4):641–665.

Stotsky, M. J. G. and Zaman, M. A. (2016). *The influence of gender budgeting in Indian states on gender inequality and fiscal spending*. International Monetary Fund.

Svaleryd, H. (2009). Women's representation and public spending. *European Journal of Political Economy*, 25(2):186–198.

Vermeir, J. and Heyndels, B. (2006). Tax policy and yardstick voting in flemish municipal elections. *Applied Economics*, 38(19):2285–2298.

Weiss, J. (1999). *Infrastructure and economic development*. African Development Bank.

Wooldridge, J. M. (2021). Two-way fixed effects, the two-way mundlak regression, and difference-in-differences estimators. *Available at SSRN 3906345*.

Appendices

Appendix A

A Staggered DiD

A.1 Callaway

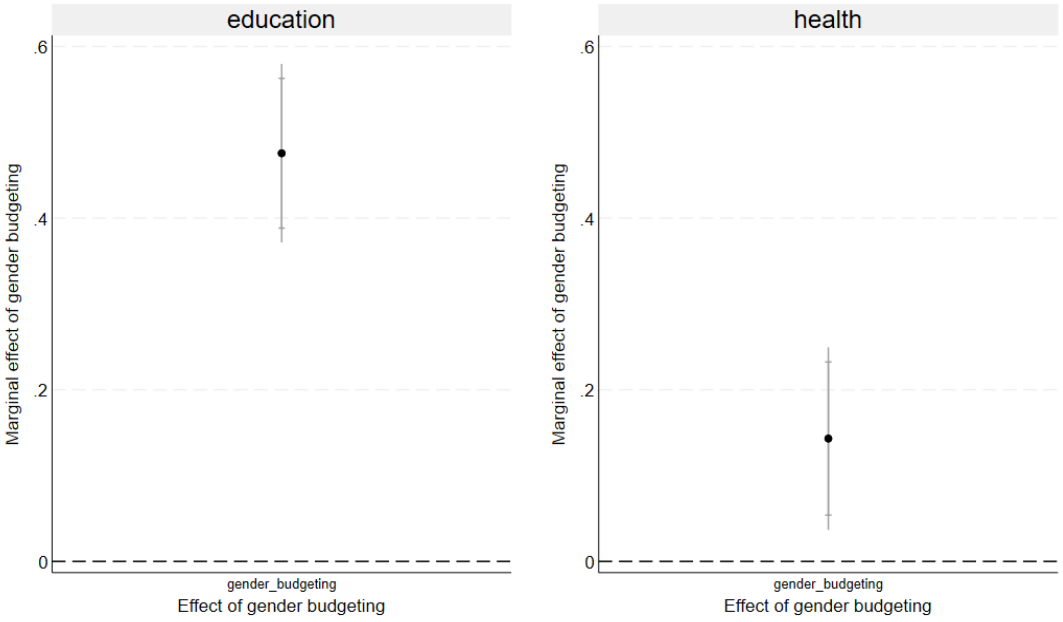
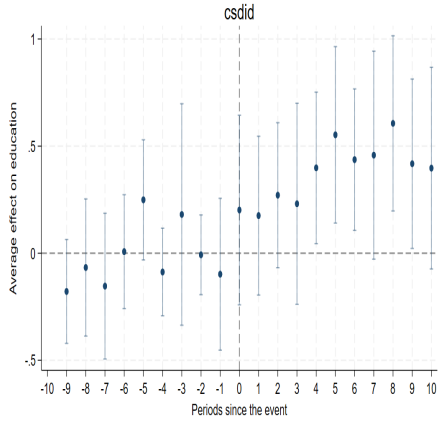
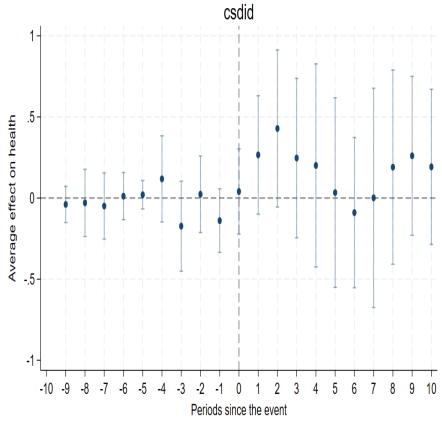


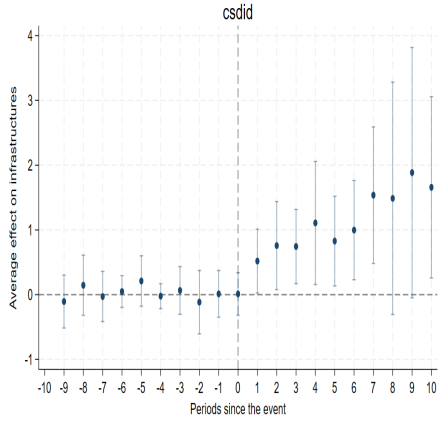
Figure A.1: Visual results of Callaway and Sant'Anna



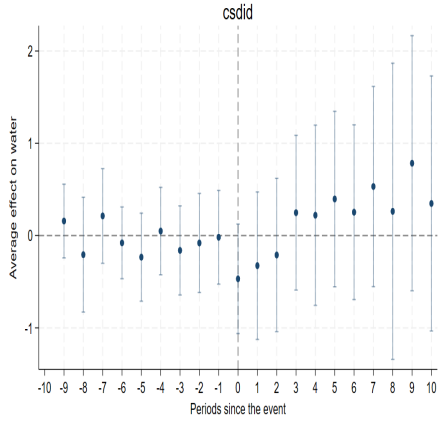
(a) Event study for education



(b) Event study health



(c) Event study infrastructures



(d) Event study water and sanitation

Figure A.2: Event study for "pro-gender" components (Callaway and Sant'Anna (2021))

A.2 Wooldridge

Table A.1: Wooldridge Diff in Diff results

	pro-gender	Education	Health	Infrastructures	Water
gender budgeting	1.151 *	0.387 ***	0.0921	0.912***	-0.229
	(1.68)	(5.02)	(0.51)	(3.47)	(-0.60)
Observations	668	668	668	668	668

t statistics in parentheses

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

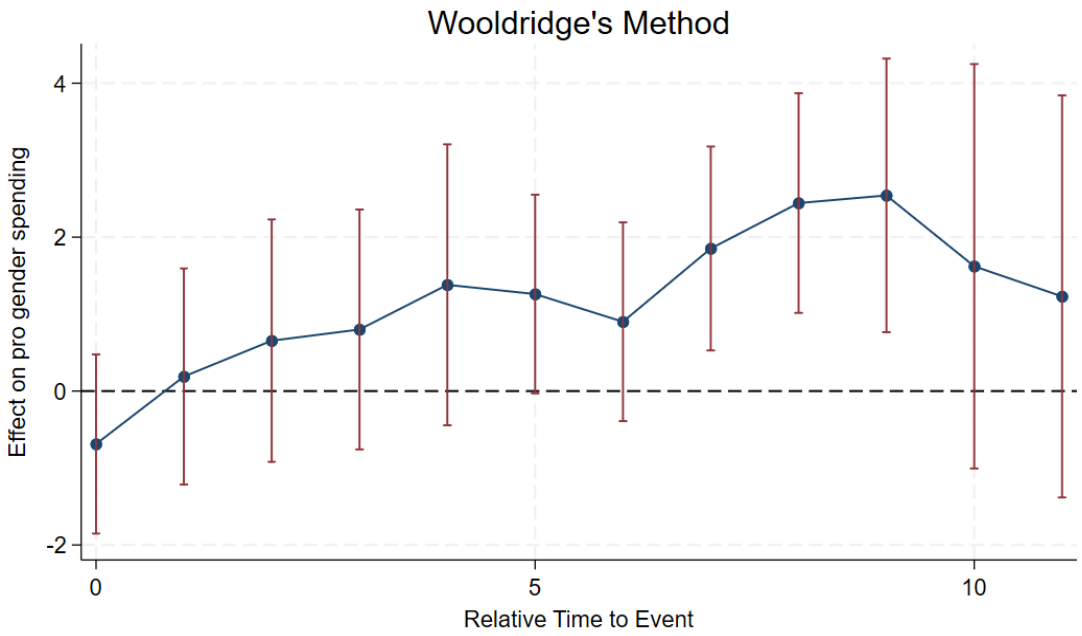


Figure A.3: Results for Wooldridge estimator

A.3 Borusyak

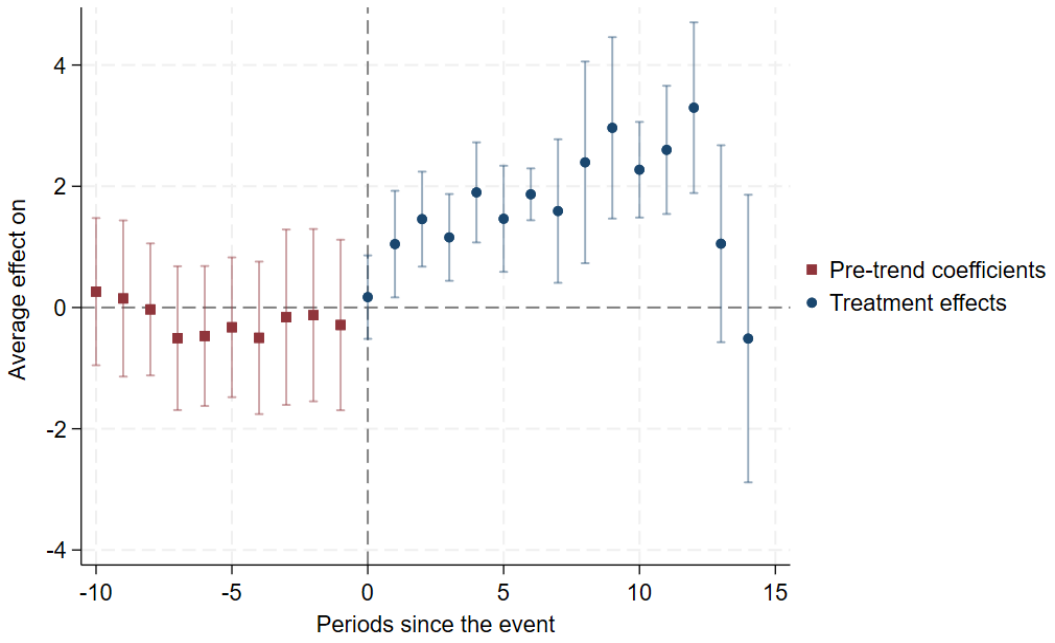


Figure A.4: Event study results for "pro-gender"spending

A.4 Chaisemartin

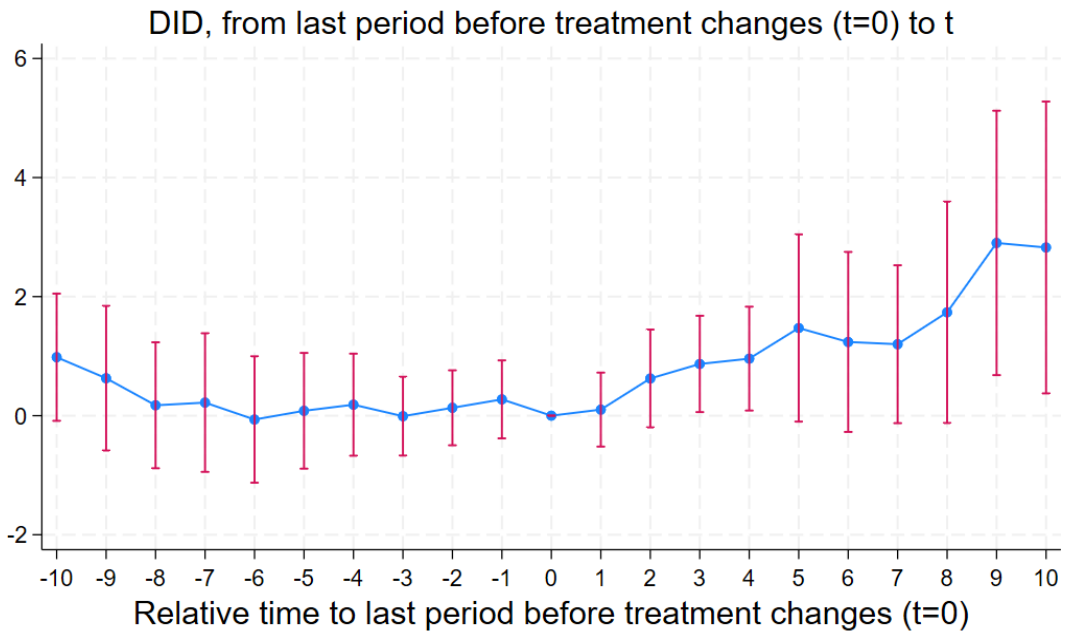


Figure A.5: Event study results for "pro-gender"spending

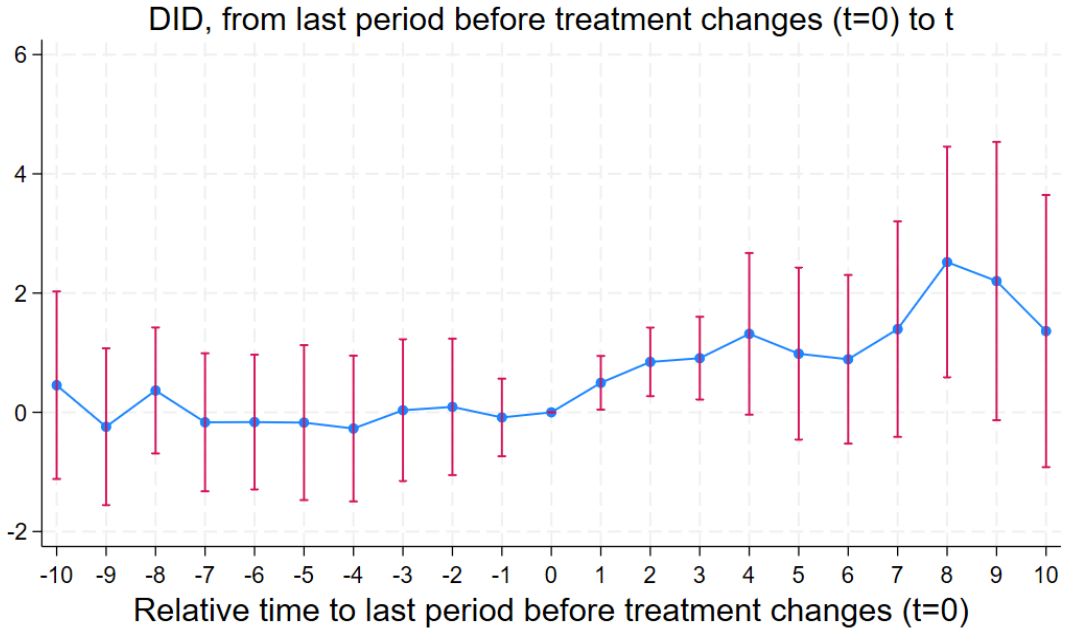


Figure A.6: Event study results for "pro-gender"spending with continuous treatment

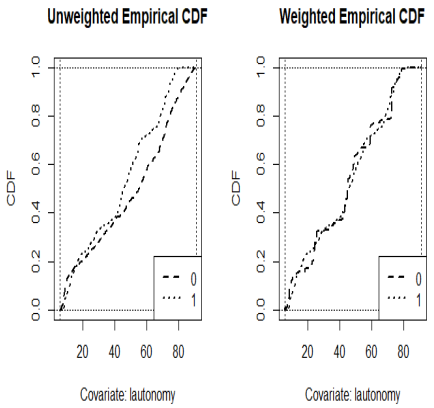
B Entropy balancing

B.1 Correlation issue

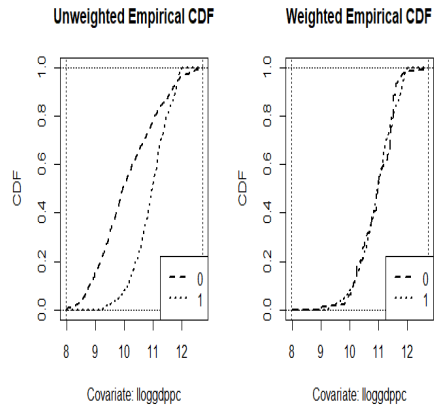
Table B.1 shows a simple comparison of pre-weighting sample means of all matching covariates between treated (Column [2]) and control (Column [1]) states, which represent the potential synthetic group. Column [5] shows significant differences between the two groups for all pre-treatment variables, as some p-values are below the threshold of 5%. Such differences could bias the true treatment effect due to a potential selection problem. Therefore, in Panel B (Column [1]), I compute a synthetic control group by re-weighting the control units, using the pre-treatment covariates from the benchmark specification. This approach allows us to make the means of the pre-treatment covariates of the synthetic group as comparable as possible to those of the treated units. As can be seen in Column [5] of Panel B, the weighting eliminated any significant pre-treatment difference between the means of the treated and synthetic covariates. Thus, I can consider the synthetic group as a perfect counterfactual of the treated group.

Table B.1: Balance Statistics

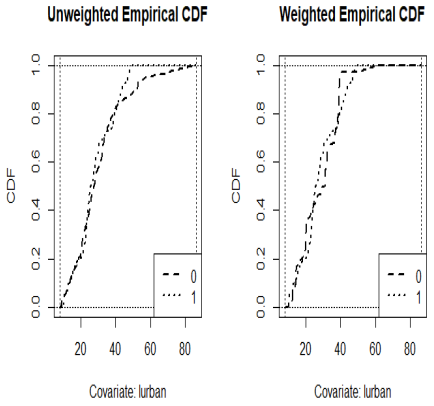
Variable	**treated**	**untreated**	**Difference**	**t statistics**	**p-value**
Unweighted Balance Statistics:					
	(1)	(2)	(3)	(4)	(5)
lautonomy	43.81	49.79	-5.98	2.4375	0.0156
lloggdppc	10.77	9.98	0.79	-11.2868	0.0000
lurban	27.65	29.54	-1.89	1.6248	0.1054
trend	22.76	14.18	8.58	-18.3878	0.0000
llocal_wip	48.46	48.67	-0.21	1.1481	0.2524
fiscal_rule	1.00	0.39	0.61	-26.46481	0.0000
lagri	27.33	31.13	-3.80	2.4178	0.0162
logpop	20.97	20.84	0.13	-19.3786	0.0000
Weighted Balance Statistics:					
	(1)	(2)	(3)	(4)	(5)
lautonomy	43.81	43.80	0.01	0.003	0.9922
lloggdppc	10.77	10.77	0.00	0.058	1.000
lurban	27.65	27.65	0.00	0.015	0.9473
trend	22.76	22.72	0.04	0.080	1.000
llocal_wip	48.46	48.46	0.00	0.776	0.8738
fiscal_rule	1.00	1.00	0.00	0.011	1.000
lagri	27.33	27.33	0.00	0.096	0.9919
logpop	20.97	20.96	0.01	-0.038	1.000



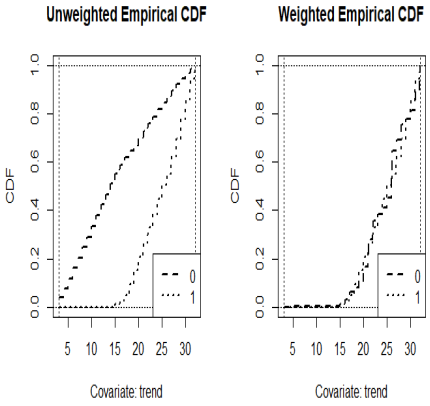
(a) Autonomy reweighted



(b) GDPpc reweighted



(c) Urbanization reweighted



(d) Trend reweighted

Figure B.1: Entropy balancing results

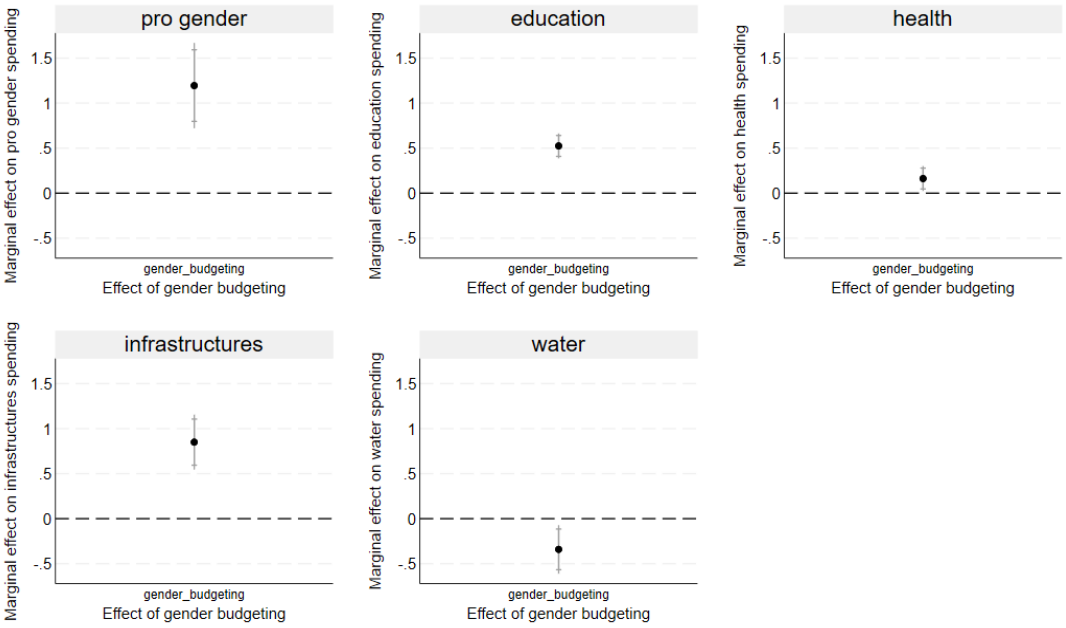


Figure B.2: Entropy balancing Results

Appendix B

C Dose response model

Table C.1: Dose Response Model results

	(1)	(2)	(3)	(4)	(5)
	pro_gender	education	health	infrastructures	water
gender_budgeting	2.077***	0.612***	0.510**	0.731***	0.237
	(0.556)	(0.200)	(0.190)	(0.244)	(0.319)
N	668	668	668	668	668
R^2	0.245	0.148	0.279	0.225	0.139

Standard errors in parentheses

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

D Placebo Test

Table D.1: Placebo test Results for components

	(1)	(2)	(3)	(4)	(5)
	pro_gender	education	health	infrastructures	water
placebo	0.106	0.0735	0.0224	0.0805	-0.0665
	(0.169)	(0.0553)	(0.0427)	(0.106)	(0.100)
N	668	668	668	668	668
R^2	0.435	0.242	0.242	0.242	0.207

Standard errors in parentheses

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

E Anticipation effects

Table E.1: Anticipation effects results

	pro-gender	Education	Health	Infrastructures	Water
ATT	0.640	0.293*	-0.117	0.444	0.0239
	(0.94)	(1.91)	(-0.49)	(1.55)	(0.06)
Observations	668	668	668	668	668

t statistics in parentheses

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

F Narrowing the control window

Table F.1: Results for narrowing -3 ; +3

	pro-gender	Education	Health	Infrastructures	Water
gender_budgeting	1.456***	0.298***	0.184**	1.443***	-0.410
	(3.60)	(3.83)	(2.27)	(5.93)	(-1.32)
Observations	166	166	166	166	166

t statistics in parentheses

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

Table F.2: Results for narrowing -5 ; +5

	pro-gender	Education	Health	Infrastructures	Water
gender_budgeting	1.551***	0.325***	0.153*	1.465***	-0.327
	(4.02)	(4.27)	(1.90)	(6.09)	(-1.13)
Observations	180	180	180	180	180

t statistics in parentheses

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

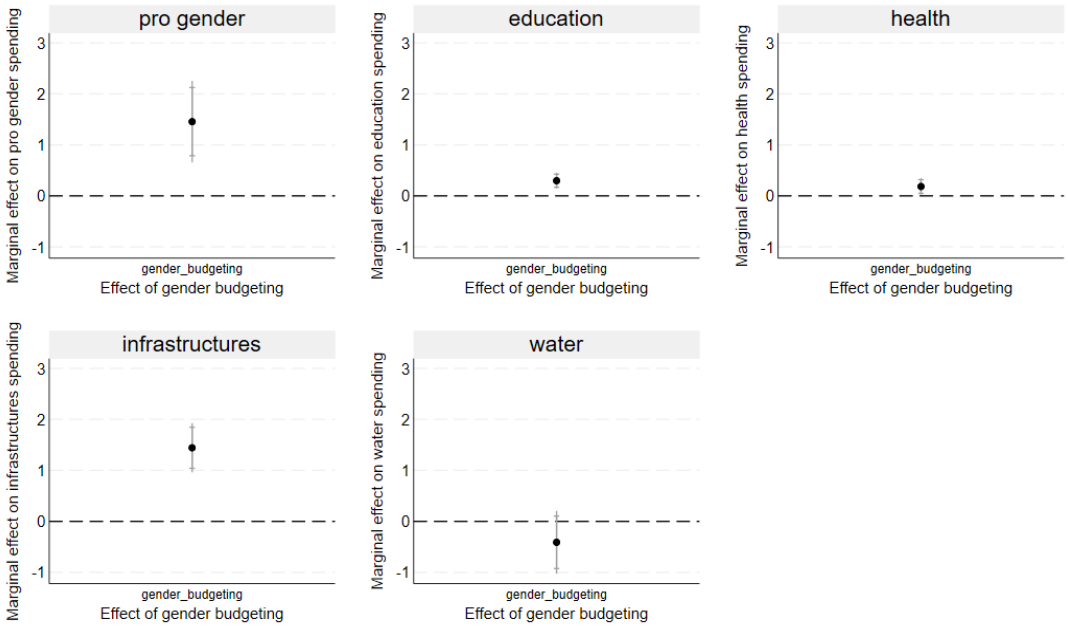


Figure F.1: Results for a 3 years around period

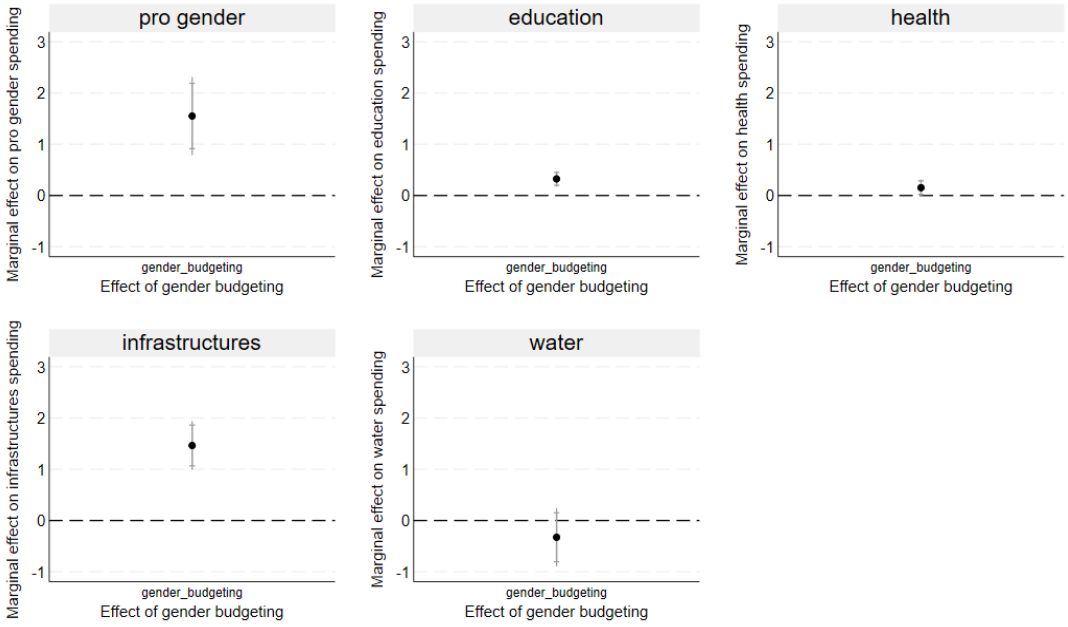


Figure F.2: Results for 5 years around period

Appendix C

G Microeconomic effects

Table G.1: Marginal effect on IPV due to unfaithful

	pro-gender	education	health	infrastructure
justifies domestic violence: wife unfaithful				
pro_gender	-0.0208***			
	(-3.29)			
education		-0.142***		
		(-3.86)		
health			0.0273	
			(0.86)	
infrastructures				-0.0369***
				(-4.58)
N	49294	49294	49294	49294

t statistics in parentheses

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

Table G.2: Marginal effect on IPV due to disrespect

	pro-gender	education	health	infrastructure
justifies domestic violence: wife disrespect				
pro_gender	-0.0109*			
	(-1.89)			
education		-0.102***		
		(-3.03)		
health			-0.0959***	
			(-3.20)	
infrastructures				-0.0257***
				(-3.60)
N	49351	49351	49351	49351

t statistics in parentheses

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

Table G.3: Marginal effect on IPV due to unfaithful in urban areas

	pro-gender	education	health	infrastructure
pro_gender	-0.0225*			
	(-1.89)			
education		-0.167**		
		(-2.24)		
health			-0.00329	
			(-0.06)	
infrastructures				-0.0231
				(-1.58)
N	11625	11625	11625	11625

t statistics in parentheses

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

Table G.4: Marginal effect on IPV due to unfaithful in rural areas

	pro-gender	education	health	infrastructure
pro_gender	-0.0289***			
	(-3.95)			
education		-0.162***		
		(-3.89)		
health			0.00167	
			(0.04)	
infrastructures				-0.0468***
				(-4.98)
N	38037	38037	38037	38037

t statistics in parentheses

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