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Essays in Politics, Health, and Development

Rajani, Ishita

Princeton, NJ : Princeton University

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ESSAYS IN POLITICS, HEALTH, AND DEVELOPMENT

Ishita Rajani

A Dissertation
Presented to the Faculty
of Princeton University
in Candidacy for the Degree
of Doctor of Philosophy

Recommended for Acceptance
by the Department of Economics
Adviser: Professor Thomas Fujiwara

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Abstract

In my dissertation, I focus on two goals of development economics: understanding how political incentives distort public policies, and identifying policies that can improve women and children's health.

In Chapter 1, I study how political incentives unique to coalition governments affect the allocation of public funds with respect to the election cycle. I show that in countries with coalition governments a national incumbent allocates funds *both* to woo voters pre-election and to buy legislative support from smaller parties *post-election*. I show that in India, these two mechanisms give rise to two distinct patterns in the disbursement of funds: the "Buying Votes Cycle" (traditional cycle with a pre-election increase in the release of funds in swing districts for the national parties) and the "Patronage Cycle" (*previously undocumented cycle* with a post-election increase in the release of funds in safe districts for regional parties). Both distortions are large enough to produce cycles in household consumption.

In Chapter 2 (with Nikhil Gupta), I provide an explanation for the conflicting evidence regarding the impact of family planning programs on completed and short-run fertilities. Using a lifecycle model of fertility control choice, I theoretically show that improved contraceptive access can shift births earlier in the lifecycle due to improved control over fertility outcomes, increasing short-run fertility while decreasing completed fertility. I then empirically confirm the predictions of my model using the legalization of injectables in Zambia. My results suggest that access to modern contraceptives is welfare-improving for women, despite having counterintuitive implications for fertility.

In Chapter 3 (with Janet Currie), I study the effectiveness of Women, Infants, and Children (WIC) program in improving birth outcomes. I estimate models with mother fixed effects and directly investigate how time-varying characteristics of mothers affect selection into WIC, overcoming limitations of previous literature. I find that WIC is associated with reductions in both low birth weight and the probability that a child is "small for dates." I also find that women on WIC are more likely to be diagnosed with chronic conditions,

and to receive more intensive medical services, a finding that may reflect improved access to medical care.

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To my family.

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Chapter 1

Are Public Funds Used to Maintain Ruling Coalitions? Evidence from India

1.1 Introduction

Politically motivated disbursements of government resources that are large, inefficient, and welfare reducing are well-documented in developing countries. Past work has documented that manipulations in infrastructure provision (Baskaran et al. (2015)), agricultural credit (Cole (2009)), and government expenditures (Akhmedov and Zhuravskaya (2004)) arise right before elections, motivated by an incumbent's attempts to "buy" voters' support. However, most of the literature has studied political manipulations using predictions from *two-party models* (Cox and McCubbins (1986), Lindbeck and Weibull (1987), Dixit and Londregan (1996)). While this framework can describe political competition in countries with two political parties, 76 of the world's 145 democracies have coalition government systems, all featuring important smaller parties. Ignoring the unique incentives of central incumbents in

coalition governments can underestimate the extent of (and mischaracterize the nature of) politically motivated manipulations in multi-party settings.

I examine how the presence of politically relevant regional parties[1] in India, the world's largest democracy, affects policy manipulations used by a central incumbent in a *coalition government*. In particular, I study how the central incumbent manipulates the distribution of public funds to districts when it cares about both (a) winning subsequent state elections and (b) enjoying sufficient legislative support to maintain a governing coalition. I provide evidence of political manipulation by estimating political cycles in the release of funds, i.e., by tracking government expenditure against state electoral cycles and studying patterns in the distribution of funds relative to the time of state elections.[2] State elections are the appropriate setting to find evidence of the central incumbent "buying support" from regional parties because regional parties primarily care about winning state elections and use funds from the central government to fulfill their electoral promises.

I present a simple framework that formalizes how the two political incentives of the central incumbent affect the spatial allocation of funds across districts. This framework allows the central incumbent to have two goals: winning elections and maintaining ruling coalitions. To achieve these goals the central incumbent manipulates the disbursement of funds to either buy votes pre-election or to buy support from a regional party post-election. In any given district, the central incumbent will at most commit one of these two manipulations depending on the district's type of political competition. Specifically, my framework predicts that the

[1]Regional parties are smaller political parties that operate in sub-national geographical regions and have a loyal voter base organized on some common characteristic like language, religion, ethnicity, etc. Throughout this paper I use the terms smaller parties and regional parties interchangeably. For example, Samajwadi Party (India), Basque Nationalist Party (Spain), Pachakutik Plurinational Unity Movement (Ecuador), United Republican Party (Kenya), and Bloc Québécois (Canada).

[2]Using cross-sectional data instead of political cycles to find evidence of political manipulation in the disbursement of funds is less informative because it is difficult to determine if a district is receiving more funds out of necessity rather than political importance. Models of political cycles include Nordhaus (1975), Rogoff and Sibert (1988), Rogoff (1990), Shi and Svensson (2006). Drazen (2000a,b) survey the literature.

central incumbent will only buy votes pre-election in swing districts[3] for the two national parties and will only buy support post-election in districts where regional parties are strong.

I empirically demonstrate that, consistent with the predictions of my model, there are two types of political cycles in the disbursement of public funds for a government scheme by the Indian central incumbent: the "Buying Votes Cycle" and the "Patronage Cycle." The former is consistent with findings in the literature and involves a 60% increase in the release of funds by the central incumbent right before an election in districts where the two national parties compete with one another. The latter is a *heretofore undocumented* political cycle with increases of 55%-75% in the release of funds right *after the election* in districts where regional parties are strong.[4]

To show both types of cycles, I construct a unique monthly panel dataset covering 466 rural districts in India. This dataset combines administrative data from 2004–2014 on district-level, monthly release of funds under the Total Sanitation Campaign (TSC), a government scheme providing subsidized sanitation in rural areas; political data on 90 State Assembly Elections from 1994–2014; and district-level demographic data from Indian Censuses. The political setting in India during 2004–2014 is ideal to study manipulations by a central incumbent in a coalition government because it has the two characteristics needed to study the role of regional parties on political manipulations: strong regional parties and a central incumbent that needs the support of these regional parties.[5]

[3]I define swing districts as districts where each of the two political parties has won at least one election during a specified period.

[4]To the best of my knowledge, Cole (2009) is the only paper looking at political cycles that finds evidence of post-election political manipulations in government policy tools (an increase in write-off of bank loans after an election). However, he finds these manipulations in districts where the ruling party in a state wins and interprets them as the ruling party rewarding its loyal supporters. Other papers have found evidence of shuffling of government personnel after elections or appointment of new chief ministers for political reasons (e.g., Fagernäs and Pelkonen (2016), Iyer and Mani (2012)) but not in the context of incumbents buying support from regional parties.

[5]For example, in July 2008, the national coalition survived a no-confidence motion with the support of the Samajwadi Party, a political party active in only one state in India. In August 2013, support from 3 smaller parties, the Samajwadi Party, the Bahujan Samaj Party, and the Janata Dal (United) allowed the government to pass the Food Security Bill in the national legislative assembly.

I estimate political cycles *separately* in districts with different types of political competition to understand how different political incentives of the central incumbent across types of districts can lead to heterogeneous political manipulations across space. I exploit the unsynchronized nature of Indian state elections to estimate each political cycle using space and time fixed effects, accounting for district-specific time invariant characteristics and time-varying shocks that affect all districts with similar political competition equally. Intuitively, identification of political cycles comes from comparing changes in the disbursement of funds for two districts within the same category of political competition, at the same points in time, but at different points in their electoral cycle.

The cycles I estimate are consistent with the central incumbent having two different incentives across districts: buying support from regional parties and wooing voters. I show that the "Patronage Cycle" only exists in districts where the central incumbent never wins, confirming it is not driven by the central incumbent rewarding core supporters after winning an election itself. Instead, the "Patronage Cycle" only exists in districts where crucial allies of the central incumbent are politically competitive, confirming that it is a *consequence of the central incumbent rewarding regional party allies for their support.* I also show that the previously well-documented[6] "Buying Votes Cycle" only exists in districts that most closely resemble swing districts in a two-party setting (i.e., districts that are swing for the two national parties) and not in other types of districts. This suggests that the "Buying Votes Cycle" I document is caused by similar central incumbent motivations as in two-party models — manipulating policy to improve its chances of winning an upcoming election.

I construct a second novel dataset by combining all rounds of NSS consumption surveys during 2004–2014 with the political data described earlier, to show that both types of political manipulations are large enough to introduce political cycles in household consumption. To the best of my knowledge, political cycles using household consumption data have not

[6]For example, Drazen and Eslava (2010) on investment expenditure in Colombian regions, Faye and Niehaus (2012) on foreign aid to developing countries, Golden and Min (2013) on electricity loses in Uttar Pradesh, Alesina and Paradisi (2014) on the introduction of real estate taxes in Italy.

been previously documented. Consumption cycles indicate that government manipulations increase the volatility of household income, reducing household welfare compared to funds being released without political cycles.[7] I find that households in districts with the "Buying Votes Cycle" have a 6.3% increase in monthly per capita expenditure in the year prior to an election and households in districts with the "Patronage Cycle" have a 4.3% increase in monthly per capita expenditure post-election. These patterns in household consumption also suggest that both types of manipulations are not restricted to just the TSC but are a general pattern across various government schemes.

To the best of my knowledge, my paper is the first one to study political cycles arising from political incentives specific to a political system with *coalition governments*. The insight that coalition governments have different motivations for manipulating policy tools compared to a single-party government is not new (e.g., Rodden and Wilkinson (2004)). However, empirical work is limited to documenting how political appointments are used to maintain coalitions or how the strength of a coalition affects the magnitude of pre-election fiscal deficits.[8]

My work contributes to literatures on politically motivated redistribution and political cycles by showing that in a setting with coalition governments, disaggregating swing and safe districts by the identity of political parties that compete for them is crucial to account for differences in the way they are targeted by the central incumbent.[9] My paper provides further evidence that central governments use transfers to influence outcomes of local elections[10] by

[7]Political cycles have been shown to be inefficient and welfare reducing in other settings. For example, Akhmedov and Zhuravskaya (2004) find sizable increases and changes in the composition of public expenditure in the months prior to regional elections in Russia in a way that disproportionately affects poor people, Cole (2009) finds that increased agricultural lending in election years does not translate into increased agricultural output, Baskaran et al. (2015) find that increased electricity provision in special election years does not lead to improvements in GDP and may come at the expense of constituencies that do not have an upcoming election.

[8]For example, Ansolabehere et al. (2005), Hanusch (2010), Tutar and Tansel (2000), Chang (2008), Balassone and Giordano (2001), Hanusch (2012).

[9]The literature on politically motivated redistribution (e.g., Levitt and Snyder (1997), Case (2001), Johansson (2003), Brender and Drazen (2008), Eslava (2011), Klomp and de Haan (2013)) either focuses on two-party settings, or collapses multi-party settings into the framework of two-party settings, to study whether an incumbent targets swing or safe districts, generally pre-election.

[10]For example, Dahlberg and Johansson (2002), Bracco et al. (2015), and Arulampalam et al. (2009) show that central governments are more likely to give grants to swing Swedish municipalities, aligned Italian municipalities, and aligned and swing Indian states, respectively. Solé-Ollé and Sorribas-Navarro (2008)

documenting how central transfers are correlated with state electoral cycles. Finally, my findings complement the literature that documents political cycles in the actions of Indian state governments before state elections[11] by showing that the central government also uses its resources for political gain in these elections.

The rest of the paper is organized as follows. Section 1.2 gives the institutional background of the setting of this paper — details about the political setting in India. Section 1.3 proposes a simple framework to predict how a central incumbent in a coalition government disburses funds across districts with different types of political competition. Sections 1.4 and 1.5 discuss the data and empirical strategy used to test these predictions, respectively. Section 1.6 states the main empirical results along with robustness checks. Section 1.7 provides further evidence that the "Patronage Cycle" is consistent with the central incumbent buying support from regional parties. Section 1.8 discusses how the two types of political manipulations lead to cycles in household consumption. Section 1.9 concludes.

1.2 Institutional Background

To study how the presence of regional parties affects policy manipulations by the central incumbent, I need a political setting with both strong regional parties and coalition governments. India during 2004–2014 possessed both features: it had strong and important regional parties and the central incumbent ruled a coalition of several political parties and needed to maintain inter-party relationships. In the following sub-sections, I describe these two institutional features in detail. I also discuss that using state elections to study pre- and post-election manipulations by the central incumbent is appropriate in the Indian context because the outcomes of state elections in India are important for both the central incumbent and its coalition partners.

show that Spanish municipalities where coalition partners of the national incumbent win are also more likely to receive grants from the incumbent.

[11]For example, Khemani (2004) finds increases in proportion of spending on public investment projects, Cole (2009) finds increases in the provision of agricultural credit by government banks, and Min and Golden (2014) find increases in electricity loses in the year before a state election.

1.2.1 Political Parties and Coalition Governments in India

In this section, I describe the role of regional parties in Indian politics. In particular, I discuss the political setting in India during 2004–2014 when the national government was a coalition government and many regional parties were giving outside support to the government. I also discuss political events that suggest the central incumbent had a constant need to prevent defections.

Regional parties are an important force in Indian politics (Ziegfeld (2016)). There are over 50 regional parties in India, in addition to the two main national parties,[12] the Indian National Congress (INC) and the Bharatiya Janata Party (BJP). Most regional parties in India are caste, religion, or language based parties that enjoy the support of a core group of voters living in a region.[13] Most, but not all of them are non-ideological and some of them have formed alliances with both national parties at different points in time. For example, the Dravida Munnetra Kazhagam was a coalition partner of the BJP at the central government during 1999–2004 and of the INC during 2004–2014.

Regional parties play two important roles in Indian politics. First, regional parties are competitive in the majority of state elections in India. For example, at present 19 out of 29 states in India either have a regional party as the ruling party or as a member of the ruling coalition. Second, if national parties fail to win the majority of seats in a national election, regional parties can play the role of kingmaker in national coalitions. For example, the Pattali Makkal Katchi (PMK), a political party with only 6 out of 543 seats at the national legislative assembly was given a cabinet position in the government and was an important member of the national coalition from 2004–2009.

During the period studied in this paper, 2004–2014, the INC was the single largest party in the national elections, however, it only won 26.3% and 38% constituencies in the national

[12]I call a political party a national party if it is active in almost all states in India and if it has ever won the largest number of constituencies in a national election. This implies that in a coalition government the national party will be the largest member.

[13]Examples of Indian regional parties include, the Samajwadi Party, the Bahujan Samaj Party, the Dravida Munnetra Kazhagam, the Pattali Makkal Katchi, etc.

elections in 2004 and 2009, respectively. It formed a minority (coalition) government that constantly needed the support of regional parties to survive no-confidence motions or to pass bills it was working towards. For example, in July 2008, the central government survived a no-confidence motion with the support of two regional parties, the Samajwadi Party (SP) and the Jharkhand Mukti Morcha. Similarly, in December 2012, the central government could only pass a legislation on Foreign Direct Investment in retail because the SP and the Bahujan Samaj Party (BSP) either abstained or voted for the bill.[14] These regular threats to the stability of the government ensured that the INC had a constant need to keep its allies (regional parties) satisfied during its tenure.

This minority government received support from two types of regional parties, those that were officially part of the coalition (e.g., the PMK) and others that provided outside support (e.g., the SP). A political party gives outside support to a government by agreeing to not vote against the government in case of a no-confidence motion or by helping the government pass bills in the legislative assembly, while not officially joining the government. Regional parties giving outside support to the government can not be "rewarded" with cabinet positions for their support,[15] and the central incumbent has to use other means to prevent them from defecting (Rodden and Wilkinson (2004)). In this paper, I argue that the central incumbent can buy support of such regional parties by allocating funds under centrally-sponsored schemes to districts where regional parties win.

1.2.2 State Elections in India

A central incumbent only manipulates policies around an election if it cares about the outcome of the election and if the election occurs at a time when the central incumbent has

[14] Additional examples include: In July 2010, the central government defeated a *cut motion* on petroleum prices with the help of the BSP, the SP, and the Rashtriya Janata Dal; in August 2013, support from three regional parties the SP, the BSP, and, the Janata Dal (United) allowed the government to pass the Food Security Bill in the Lok Sabha (the lower house of Parliament).

[15] There is a large literature on incumbents rewarding their coalition partners with cabinet positions in the government, including Warwick and Druckman (2001), Ansolabehere et al. (2005), Carroll and Cox (2007), Bäck et al. (2011), Laver et al. (2011).

sufficient resources to influence the outcome. State elections in India are an appropriate setting to study manipulations by the central incumbent because they satisfy both these conditions.

Indian state elections occur every five years and are usually contested by both national and regional parties. Elections are unsynchronized across states implying that each state has an election during the tenure of an elected central incumbent and state elections are separated in time both across years and within a year. For example, during the tenure of a single central incumbent (May 2009 – April 2014), there were several state elections, including Kerala in April 2011, and Punjab in January 2012, and Gujarat in December 2012. The staggered nature of state elections allows the central incumbent greater latitude to manipulate election outcomes.

Central incumbents in India care about winning state elections for several reasons. First, state elections directly impact the legislative power of the central incumbent by changing the composition of the Upper House of Parliament. For example, if the central incumbent does not win a single assembly constituency[16] in any state election, it will hold zero seats in the Upper House and would have to rely on other political parties to pass any legislation.[17] Second, alignment between state and central governments helps the central incumbent better achieve its policy goals without opposition from state governments. Third, state elections are seen as a referendum on the central incumbent's popularity during its term. When a central incumbent loses elections across several states during its tenure, voters see it as a sign of the national incumbent's weakening popularity, thus lowering its chances of winning the next national election.[18] The former two reasons ensure the central incumbent would also want its allies to win wherever it is too weak to win itself.[19]

[16]Assembly constituencies are the electoral districts, each electing a single member of the state legislative assembly by plurality rule.

[17]The number of seats a party holds in the Rajya Sabha increases with their seat share in state elections, implying that national parties have an incentive to win as many state legislative assembly seats as possible.

[18]For example, Dettman (2004), "Assembly polls in 5 states: Will it be referendum on Central govt?", Hindustan Times, March 9, 2016.

[19]For example, "Rajya Sabha polls: BJP gains edge in Upper House but still short of majority", The Indian Express, June 12, 2016.

Finally, state elections are the appropriate setting to find evidence of the central incumbent buying support from allied regional parties. Regional parties care primarily about successfully contesting state elections, in part by signaling to voters in their state their ability to extract resources from the center (Rodden and Wilkinson (2004), Ziegfeld (2012)). For example, in 2012 Akhilesh Yadav (SP), the Chief Minister of UP, met with the Prime Minister to demand smooth transfer of funds for various Center sponsored schemes;[20] in 2015 Nitish Kumar (Janata Dal (United)), the Chief Minister of Bihar, announced his support for an upcoming bill at the national legislative assembly and asked the Prime Minister for central funds to help fulfill his electoral promises.[21] As regional parties in part fulfill their electoral promises to voters using funds released by the central incumbent to their state, we expect the release of funds to be correlated with a *state* election cycle.

1.3 Theoretical Framework

I propose a simple framework with two national parties and a regional party that predicts how a central incumbent manipulates the spatial allocation of funds under a government sponsored scheme in order to sway the results of state elections and to maintain national legislative support from regional parties. I show that the central incumbent releases funds pre-election only in swing districts for the two national parties and releases funds post-election only in districts where the regional party is competitive. This framework provides testable predictions for different types of political cycles that can arise across space due to manipulations by the central incumbent. I test these predictions in Section 1.6.

My framework focuses only on the spatial dimension of the national incumbent's political manipulations. For simplicity, I have abstracted away from endogenous budgets and

[20] "Akhilesh meets PM, seeks smooth transfer of funds for UP", rediff NEWS, 14th April 2012.
[21] "Nitish Backs GST Bill to Woo Central Funds", The Indian Express, 13th December 2015.

the possibility of the national opposition party also attempting to woo the regional party. Inclusion of these features does not qualitatively change the predictions of my model.[22]

1.3.1 Setting of Model

Consider a three period model with parties I (for Incumbent), O (for Opposition), and Z (for Regional Party). Parties I and O are national parties with national voter bases and differ from each other on some ideological grounds. Both these parties compete in central and state level elections. Party Z is a regional party concerned with the issues of people living in the region it is representing.[23] It can never win the majority of seats in the central election but can get enough votes that its support is important for a coalition central government.

Multiple state elections occur during the tenure of an elected central government, and all three parties compete for the up-for-grabs state governments. The central incumbent, I, is interested in winning state-level elections and enjoying sufficient legislative support at the national level to be able to pass legislation. The central government can use transfers to increase the party's chances of winning in as many state elections as possible and can also use these funds to buy legislative support at the national level from party Z.

Party Z is interested in winning state-level elections, while party O is interested in both winning state-level elections and maintaining sufficient ideological difference from party I to be competitive in the next national election. The central incumbent needs the support of party Z to pass national legislations and can promise to reward the supporters of party

[22]Allocation of funds for government schemes are determined in a two-step process: choice of overall budget, and choice of spatial allocation. At the second step, the budget is fixed. Extending the model to have an endogenous budget will impact the size of the budget in a given year, but not the incumbent's incentives for different manipulations across space at the time of allocation. Thus, endogenous budgets can affect the magnitude of the cycles, but will not qualitatively alter the results.

My model focuses on a central incumbent's manipulations around state elections. As the national opposition party does not control the national government, it can only keep promises it makes to the regional party if it wins a subsequent national election and has incentives to honor those promises post-victory. Therefore, the national opposition party's offers to the regional party have greater uncertainty than offers made by the national incumbent, and allowing the national opposition party to make offers to the regional party will not qualitatively affect the results of the model, given the low commitment associated with such offers.

[23]In case of India we can think of Z as a set of regional parties where each of these parties has a loyal vote base in the region it is active in.

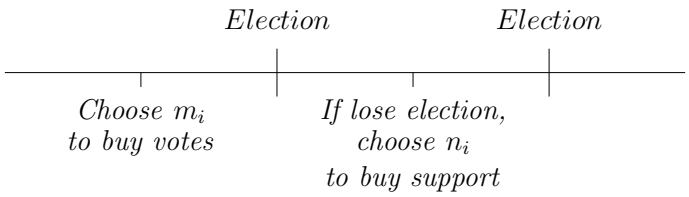


Figure 1.1: Timing of the central incumbent's actions in district i

Z after a state election in return for its support at the center. The central incumbent can make a similar proposal to party O, but is always rejected given O's preference for ideological purity.

I divide districts into six groups based on their level of political competition and the identity of the winning party within a level of political competitiveness. Districts that are safe for parties I, O, and Z are denoted by D_I, D_O, and D_Z, respectively. Districts that are swing for parties, I and O, O and Z, and I and Z, are labelled D_{IO}, D_{OZ}, and D_{IZ}, respectively. Denote by $\mathcal{D}$ all the districts in these six categories.[24] Let $\mathcal{J} \equiv \{I, O, Z, IO, OZ, IZ\}$.

1.3.2 Timing of Model

There are three periods in the model. Elections are held in all districts at the end of period 1 and the beginning of period 3. Figure 1.1 describes the timing of the model in each district.

Period 1: In each district i party I chooses to release an amount of funds m_i to increase its probability of winning the upcoming election. I refer to this behavior of party I as 'buying votes' in the rest of the paper. If party I wins the election in a district, it gets a fixed payoff u that is the same across all districts. I interpret u as the utility party I gets from capturing a district's seat in the state legislative assembly. I denote by $p_j^y(m_i)$ the probability that party $y \in \{I, O, Z\}$ wins the election in district i in category j.

Period 2: If party I does not win the election in district i, it decides whether to 'buy support' from the district's winning party $x \in \{O, Z\}$ by releasing funds n_i to the district.

[24] I assume that there are no districts where all three political parties can win elections.

Party I's utility from receiving support from party x is given by a fixed utility v across all districts. I denote by $q_j^x(n_i)$ the probability that party I receives support from wining party x in district i after releasing funds n_i.

Period 3: There is a second election at the beginning of period three. Party I's probability of winning this election in district i is $s(m_i, n_i)$ and its utility from winning this election is W. The probability $s(m_i, n_i)$ increases if party I releases more funds to buy votes in a district in period 1 and it decreases in the amount of funds it uses to buy support from the winning party.

1.3.3 Central Incumbent's Maximization Problem

In every district i, the central incumbent has two choice variables: funds m_i used to buy votes before the first election, and funds n_i used to buy support from the winning party in the district if the central incumbent does not win the first election. The total budget that can be manipulated by the central incumbent under this scheme is given by M and is fixed for a given fiscal year. The central incumbent maximizes expected future utility

$$\max_{m_i,n_i} \sum_{j\in\mathcal{J}} \sum_{i\in D_j} \left[p_j^I(m_i)u + \delta \sum_{k\in\{O,Z\}} \left[p_j^k(m_i)q_j^k(n_i)\right] v + \delta^2 s(m_i,n_i)W \right] \tag{1.1}$$

subject to the budget constraint:

$$\sum_{i\in\mathcal{D}} [m_i + n_i] \leq M \text{ and } m_i \geq 0,\ n_i \geq 0 \tag{1.2}$$

With reasonable restrictions on probabilities[25] the central incumbent maximizes its expected utility by employing three different types of manipulations across six groups of districts defined by different types of political competition. The solution to the maximization problem is formally stated in Proposition 1.

Proposition 1. *Under reasonable restrictions on probabilities, the central incumbent's maximization problem has a solution such that:*

1. *In swing districts for parties I and O, $m_i \geq 0$ and $n_i = 0$.*
2. *In safe districts for party Z and in swing districts for parties O and Z, $m_i = 0$ and $n_i \geq 0$.*
3. *In safe districts for parties I and O and in swing districts for parties I and Z, $m_i = n_i = 0$.*

Proof: See Appendix A. □

Given the timing of the three types of manipulations conducted by an optimizing central incumbent, I can predict three types of political cycles in the release of funds in a district around the first election.

Predictions. *If the total budget M is large enough, all three types of actions predicted in Proposition 1 occur in the optimal solution and there are three testable empirical predictions about the shapes of political cycles observed in different types of districts:*

P1: There is a ***"Buying Votes Cycle"*** *in the release of funds with a peak pre-election in swing districts for parties I and O.*

[25] I assume that all probabilities are continuously differentiable in their elements to ensure that the above maximization problem has a well-defined solution. Party I's probabilities of winning both elections and successfully buying support increase in the magnitude of funds it uses to buy votes and buy support, respectively and its probability of winning the subsequent election in a district decreases if it buys support from the party that won the previous election. Party I's funds are better used buying votes in swing districts for parties I and O than in swing districts for parties I and Z. Party I's benefit from using an additional unit of funds to buy support from party Z is less than the cost it has to bear (reduced probability of winning) in the next election. I formalize these assumptions in Appendix A.

*P2: There is a **"Patronage Cycle"** in the release of funds with a peak post-election in districts that are safe for party Z and in swing districts for parties O and Z.*

*P3: There are **No Political Cycles** in the release of funds in districts that are safe for parties I and O, and in swing districts for parties I and Z.*

The mapping between the types of manipulations predicted in Proposition 1 and the corresponding political cycles described above is summarized in Table 1.1.

Table 1.1: Predicted Political Cycles in Various Districts

Type of District	Pre-election Manipulation	Post-election Manipulation	Predicted Cycle
Swing for parties I and O	Yes	No	Pre-Election Peak ("Buying Votes Cycle")
Safe for party Z Swing for parties O and Z	No	Yes	Post-Election Peak ("Patronage Cycle")
Safe for party I Safe for party O Swing for parties I and Z	No	No	No Cycle

Below, I provide intuition for why the central incumbent's manipulations differ across districts with different types of political competitions. Party I will only choose to 'buy votes' in districts where it faces uncertainty about winning the election at the end of period 1 – districts that are swing for parties I and O and parties I and Z. In the optimal solution, party I only chooses to 'buy votes' in the former districts, where pre-election manipulations have a higher chance of success. In the latter districts, core supporters of party Z will never vote for party I, reducing the effectiveness of pre-election manipulations.

Party I will only choose to 'buy support' in districts where the regional party can win the election at the end of period 1 – districts that are safe for party Z, and districts that are swing for parties O and Z and parties I and Z. In the optimal solution, party I only chooses to 'buy support' in the first two types of districts, where it can never win and buying support cannot impede its chances of winning future elections. In the third type of district party I

is directly competing with party Z, and post-election releases of funds in these districts will hurt party I's chances of winning subsequent elections as voters can interpret post-election disbursement of funds as a signal of party Z's competence.

Corollary 1. *The aggregate cycle in the release of funds (pooled across all districts) can take one of four forms: no cycle, a cycle with a peak post-election, a cycle with a peak pre-election, and a cycle with peaks both before and after an election.*

Given the predictions of the three types of cycles across districts, the shape of the aggregate cycle depends on the number of districts and the magnitude of manipulation in each category of districts. Since the literature has focused on aggregate cycles, estimating an aggregate cycle will allow me to compare my results with previous work. I discuss results of empirically estimating the aggregate cycle in Section 1.6.

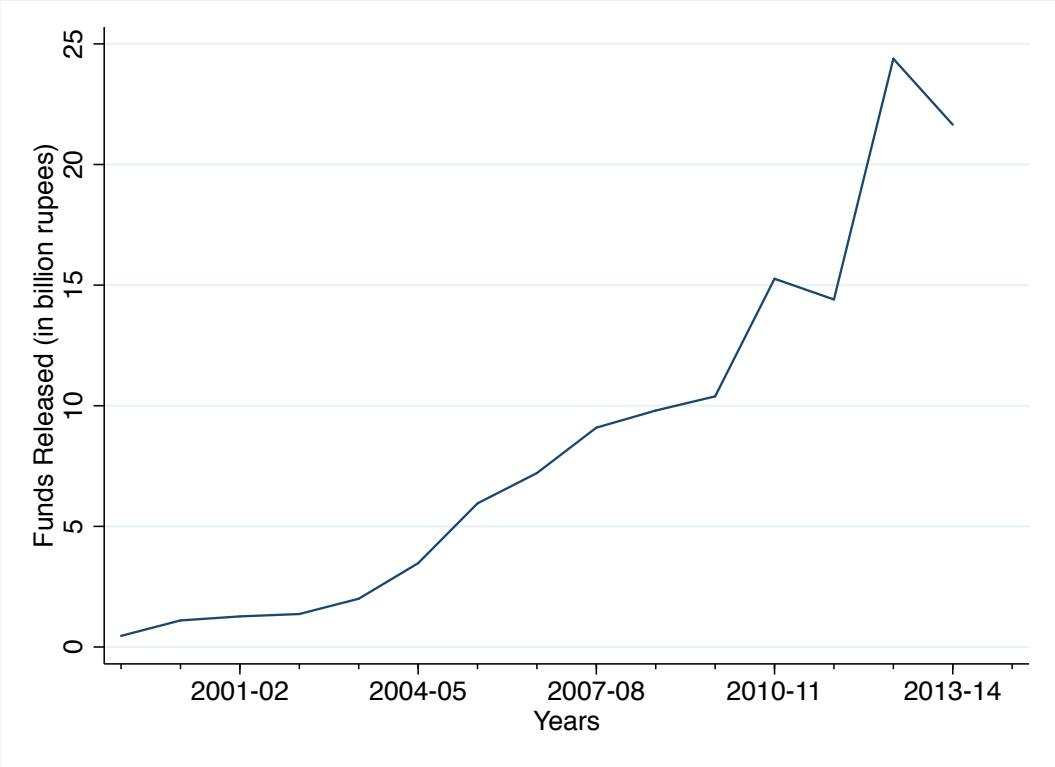
In Appendix A, I extend the model to include the presence of two regional parties instead of one to capture a political setting where there are several regional parties and the central incumbent only needs the support of a few regional parties to maintain its national coalition. In this extension, the central incumbent only tries to buy support of regional parties that are likely to be its allies at the national level, a prediction I confirm in Section 1.7.

1.4 Data

One contribution of my paper is the construction of the panel data I use to test the presence of politically motivated manipulations in the disbursement of public funds. I construct a unique panel dataset that matches district-level administrative data on the monthly release of funds under the Total Sanitation Campaign (TSC) during 2004–2014 with data on demographic characteristics of districts from Census 2001 and Census 2011, and political data on 90 state assembly elections during 1994–2014.[26] To the best of my knowledge, this is the first paper

[26]To coincide with the tenure of the coalition government I restrict my analysis to data from May 2004 to February 2014.

Figure 1.2: Release of Funds for the Total Sanitation Campaign



Notes: This graph plots the yearly release of funds in India for the Total Sanitation Campaign during 1999–2000 to 2013–2014. Data Source: TSC website, Ministry of Drinking Water and Sanitation, Government of India.

to use all these data sets together. This dataset covers 466 rural districts across 18 states in India.[27] Since the TSC is a rural sanitation scheme all my analysis is restricted to rural districts in India. To test if the political manipulations in the disbursement of government funds are large enough to affect household consumption, I construct a second dataset that combines data on household consumption expenditure from six rounds of National Sample Surveys during 2004–2014 with the political data mentioned above.

TSC Funds Data: Under the TSC, central and state governments in India give subsidies to households in rural areas to construct sanitation facilities. Launched in April 1999, the

[27]The states in my sample include Andhra Pradesh, Bihar, Chhattisgarh, Haryana, Himachal Pradesh, Gujarat, Jharkhand, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Orissa, Punjab, Rajasthan, Tamil Nadu, Uttar Pradesh, Uttarakhand, and West Bengal. These states accounted for over 93% of India's population according to Census 2001.

TSC's budget increased from Rs. 1.65 billion in 2002–03 to Rs. 32 billion in 2012–13 (see Figure 1.2), with the central government contributing 60-70% of total funds.[28]

There are four features of the TSC that provide me with clean opportunities to test the predictions of my model. First, the TSC is a centrally-sponsored scheme for which the central government determines the allocation of funds after reviewing financial plans submitted by districts. This structure of the scheme allows me study manipulations by the central government[29] in the disbursement of funds.[30] Second, high-frequency data for a period of over 10 years of a government scheme, rare in developing countries, enables me to use an identification strategy with very few assumptions on the unobservables by exploiting the staggered timing of state elections in India. Third, previous research has not shown large increases in latrine construction (Stopnitzky (2012), Hueso and Bell (2013)), raising questions about the effectiveness of this scheme and where the allocated funds are being spent. Fourth, relative to other schemes, the size of the TSC was still small during 2004–2014. As such, the TSC was likely subject to less scrutiny compared to the larger schemes and less of a priority for the government to hide political manipulations under this scheme.

The data on monthly release of funds under the TSC for each district come from the website of the Ministry of Drinking Water and Sanitation, Government of India.[31]

Political Data: I construct political variables using data on 90 state assembly elections across 3395 assembly constituencies during 1994–2014. This data is available from the website of the Election Commission of India. I use the following information about each election: timing of the election (month and year), identity of the winning party in each assembly con-

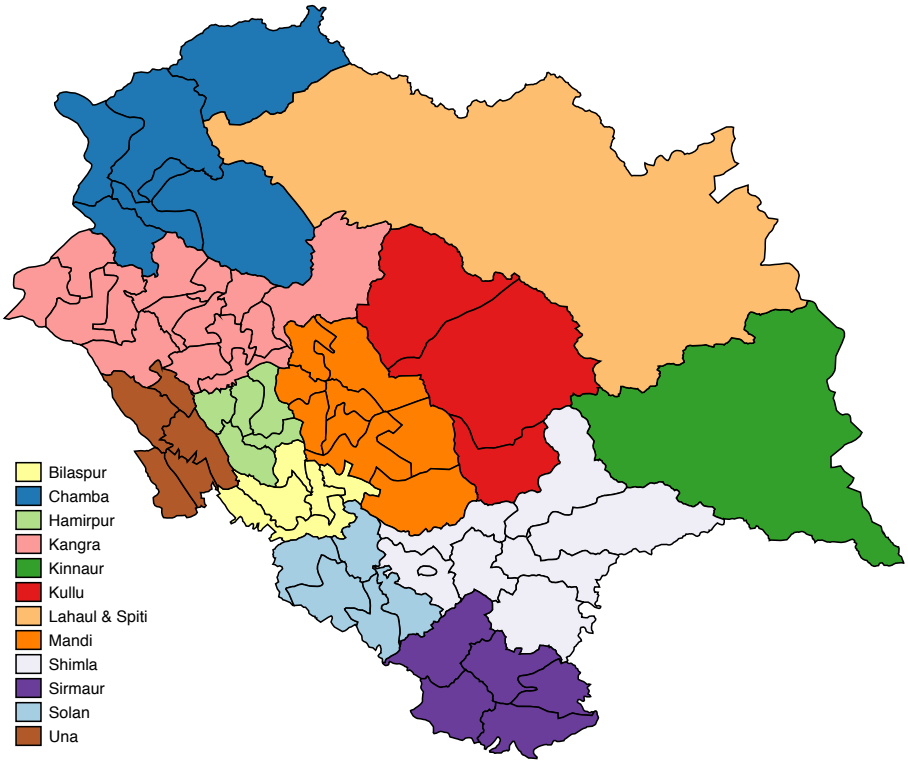
[28]Since I am interested in the behavior of the central government in this paper, I will only use data for the share of funds allocated by the central government to each district.

[29]For example, Rao and Singh (2003) discuss that among various types of transfers by the central government in India, centrally-sponsored schemes are the most susceptible to political discretion.

[30]During each fiscal year districts prepare a financial plan for the upcoming fiscal year based on the data reported by the Gram Panchayats (Village Councils). These district-level plans are then aggregated by the state governments and sent to the central government. The central government then decides the allocation of funds across districts at the beginning of each fiscal year, based on the financial plans submitted by the districts.

[31]This data was collected during March 2014 from http://sbm.gov.in/tsc/NBA/NBAHome.aspx.

Figure 1.3: Administrative and Political divisions within Himachal Pradesh



Notes: This map shows the boundaries of assembly constituencies within the state of Himachal Pradesh. The twelve districts of the state have been shaded with different colors.

stituency, margin of victory in each assembly constituency, and reservation status of each assembly constituency.[32]

Demographic Data: I use district-level demographic characteristics including population, Scheduled Caste population, Scheduled Tribe population, and religious composition of a district from Census of India (2001).

Household Surveys: I also construct a repeated cross-section dataset using six rounds of NSS Consumption Surveys during 2004–2014, to test for the presence of political cycles in household consumption (see Section 1.8 for details).

Geographic Unit of Analysis: While the TSC is implemented at the district level, state elections occur at the level of assembly constituencies that are typically smaller than

[32]During each election a share of assembly constituencies are reserved for two groups, Scheduled Castes and Scheduled Tribes, based on their population in a state.

a district.[33] For example, Figure 1.3 shows 68 assembly constituencies (as boundaries) and 12 districts (shaded in different colors) for the state of Himachal Pradesh. To construct my dataset, I aggregate political information from assembly constituencies to the district level and define the winning party in a district as the political party that wins the largest proportion of assembly constituencies in a district.[34] The number and boundaries of districts changed during the period of analysis as new districts were created. As such, I adhere to the district boundaries found in the Census 2001 as closely as possible.

1.5 Empirical Methodology

I exploit the unsynchronized nature of Indian state elections and use a space and time fixed effects regression strategy to test for the existence of different types of political cycles in the release of funds in districts with different types of political competition. I also discuss how I partition Indian districts into the six categories from Section 1.3 and use pre-period election data to address the endogeneity in the classification of districts that arises from using contemporaneous political variables.

1.5.1 Strategy to Estimate Political Cycles in the Data

As the theoretical framework in Section 1.3 predicts that the political cycle observed in a district depends on the type of political competition in a district, I estimate the following regression model *separately in the six categories of districts* to establish the existence of different types of political cycles in the release of funds for the TSC in India. Specifically, for each district category I estimate:

[33] The median district in my data has 8 assembly constituencies.

[34] Appendix B discusses an example of how I define the winning party in a district in more detail. One limitation with doing political analysis at the district level versus assembly constituency level is that there may be greater variation in which party gets elected at the assembly constituency level in the latter that can get averaged when I aggregate constituencies to districts. To deal with the aggregation issue, I have tried defining the winning party in a district in different ways as robustness checks. I discuss one of these alternative definitions in Appendix C.2.

$$Exp_{dt} = \alpha + \sum_{i=2}^{5} \beta_i Year_{(+i)dt} + \gamma X_{dt} + \delta_d + \theta_t + \epsilon_{dt} \tag{1.3}$$

where the dependent variable is the per capita funds released for the TSC in a district d at time (year-month) t, X_{dt} is a vector of controls that includes the ratio of assembly constituencies reserved for Scheduled Castes in a district and the ratio of assembly constituencies reserved for Scheduled Tribes in a district,[35] δ_d and θ_t are district and monthly time fixed effects respectively, and $Year_{(+i)dt}$ is a dummy which takes the value 1 if time t falls in the i^{th} year after state assembly elections in district d.[36] For example, if state elections are held in district d in December 2000 then $Year_{(+1)dt}$ takes the value 1 in the year 2001 and 0 otherwise, $Year_{(+2)dt}$ takes the value 1 in the year 2002 and 0 otherwise, etc (see Figure 1.4). As elections are held every 5 years, i takes values from 1 to 5.[37] I show all political cycles with $Year_{(+1)}$ as the base category.

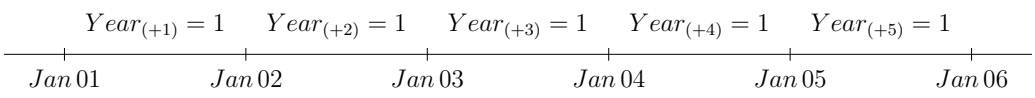


Figure 1.4: An example of the definition of electoral cycle dummies

I include district fixed effects and monthly time fixed effects for each month during 2004–2014 to control for time invariant characteristics of a district that can affect the release of funds and for trends in the release of funds in each type of district over time, respectively. In particular, monthly time fixed effects deal with the national election cycle. Similarly, district fixed effects account for baseline levels of latrines in a district and thus for the baseline need

[35] Reservations for Scheduled Castes and Scheduled Tribes have been shown to affect spending patterns by the government (e.g., Pande (2003)).

[36] Results are robust to using Half-Yearly or Quarterly dummies (Appendix C.4).

[37] Elections are scheduled by the Election Commission of India around 60 months after the previous election. However, there is a some variation in the exact month in which an election is held, therefore, in my data 93% of elections occur during 56-61 months after the last election. To deal with the variation in the exact period between two elections, I define dummies $Year_{(+1)}$, $Year_{(+2)}$, $Year_{(+3)}$, $Year_{(+4)}$, and $Year_{(+5)}$ as taking value 1 in months 1-12 after the election month, 13-24 after the election month, 25-36 after the election month, 37-48 after the election month, and 48 to the next election after the election month, respectively. I drop all election months from my analysis.

for public funds in a district. Additionally, estimating specification (1.3) separately in the six categories of districts allows districts in different categories to have completely different time trends.

The variables of interest are the coefficients β_i. Each β_i is the average monthly release of funds in a district in the i^{th} year since the last state election, controlling for time-varying district characteristics, and district and monthly time fixed effects. If β_2, β_3, β_4, and β_5 are statistically distinguishable from β_1 (the base category) then I conclude there is a political cycle in the release of funds.

The identification of each β_i comes from comparing changes in the release of funds for two districts in the same category at the same points in time but at different points in their electoral cycle.[38] The unsynchronized nature of state elections in India is crucial for the identification of political cycles in the above specification. If all elections were synchronized with each other it would not be possible to flexibly account for trends in the release of funds using monthly time fixed effects. For example, it would not be possible to control for the potential effects of national election cycle on the release of funds. Thus, exploiting the staggered nature of state elections allows me to estimate political cycles with much weaker identification assumptions compared to a scenario where all elections are scheduled simultaneously.

Standard errors are clustered at the district level to account for serial correlation in error terms in a given district. As a robustness check, I also report results for two-way clustering of standard errors (Cameron et al. (2011)) at district and state-year level in the Appendix. The two-way clustering accounts for spatial correlation in the release of funds in a state at a given point in time in its electoral cycle in addition to the serial correlation in error terms in a given district.

[38]This means that I am comparing two districts such that the previous elections in these two districts did not occur at the same time.

Table 1.2: Mapping of Different Types of Districts Between the Model and Data

	Type of District in the Data		Number of
Type of District in the Model	Safe	Swing	Districts
Safe for Party I	INC		22
Safe for Party O	BJP		47
Safe for Party Z	Regional Party		118
Swing for Parties I and O		INC/BJP	87
Swing for Parties O and Z		BJP/RP	62
Swing for Parties I and Z		INC/RP	90

1.5.2 Defining Sub-Samples to Test the Existence of Different Types of Political Cycles

To estimate specification (1.3) separately in districts with different types of political competition I partition Indian districts into six categories to match the categories from Section 1.3. I first divide districts into two categories based on their level of political competitiveness: *Safe* if the same political party has won all the elections in the district during 1993–2003 and *Swing* if two different political parties have won at least one election each in the district during 1993–2003. I define the winning party in a district in an election as the political party winning the largest proportion of assembly constituencies in a district in that election.[39] I then further divide *Safe* and *Swing* districts into three groups each based on the identity of the winning political party in a district. Parties I (Incumbent), O (Opposition), and Z (Regional Party) correspond to the INC, the BJP, and regional parties in the data, respectively. The mapping of the six categories of districts between the theoretical framework (Section 1.3) and Indian data along with the number of districts classified in each category is summarized in Table 1.2.[40]

[39]My results are robust to using other definitions of *Safe* districts. For example, in Appendix C.2 I report the results for classifying a district as *Safe* if the same political party wins at least 60% of the assembly constituencies in a district in all elections during 1993–2003.

[40]There can be a seventh category of districts, those that are swing for all three political parties. In Appendix D.1, I show that there are no political cycles in the release of funds in such districts.

Using political data and release of funds data from the same period to classify districts and estimate political cycles, respectively, can introduce endogeneity in categorizing districts. For example, one can argue that swing districts for the INC and the BJP are classified as such because the central incumbent is releasing funds in these districts right before an election, and otherwise they would be safe districts for one of the two parties. To deal with the potentially endogenous classification of districts, I use data from 1993–2003 to categorize districts and data from 2004–2014 to estimate political cycles in the release of funds. Using pre-period (1993–2003) political data to classify districts addresses endogeneity but introduces noise from districts changing classification over time (see Table 1.3 for a transition matrix between districts categorized using election data from the pre-period and contemporaneous period). Pre-period data predicts districts that are safe for regional parties and swing districts for the INC and the BJP in the contemporaneous period quite well but a large proportion of districts in the other four categories change classification over time. Since my model has the strongest predictions for the first two categories of districts and pre-period data deals with endogenous classification of districts, I use the pre-period categorization of districts as my preferred specification. All my results are robust to using contemporaneous election data to define district categories.

1.6 Results

I show that the central incumbent in India was manipulating the spatial allocation of public funds to both influence the outcomes of state elections by pre-election disbursement of funds and by buying support from regional parties. I confirm that the "Buying Votes Cycle" only exists in swing districts for the two national parties, and has a 60.2% increase in the release of funds in the year before an election.[41] Similarly, I confirm that the "Patronage Cycle" only exists in safe districts for regional parties and swing districts for the BJP and regional

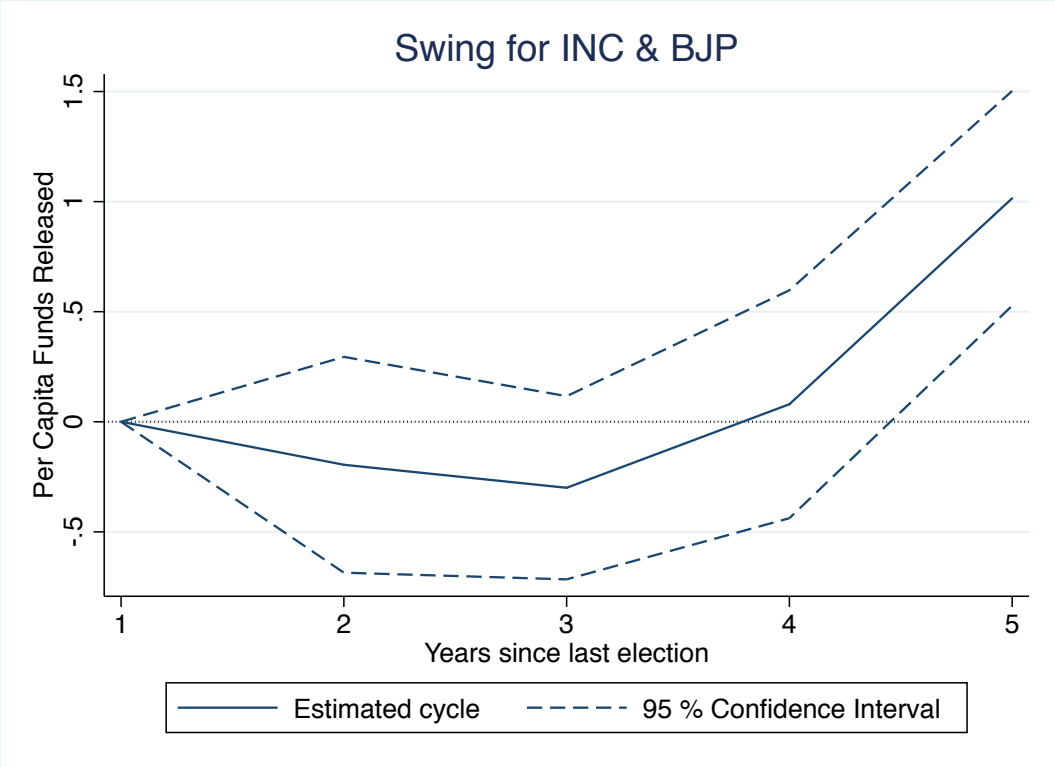
[41] This increase is calculated relative to the mean release of funds over the five-year election cycle.

Table 1.3: Transition Matrix for Districts Between Categories Defined Using Pre-Period Data and Current Period Data.

Categories using	Categories using current period election data							
pre-period election data	INC Safe	BJP Safe	RP Safe	INC/BJP	BJP/RP	INC/RP	INC/BJP/RP	Total
INC Safe	**0.09**	0.00	0.00	0.46	0.00	0.27	0.18	1.00
BJP Safe	0.00	**0.32**	0.00	0.51	0.11	0.00	0.06	1.00
RP Safe	0.00	0.00	**0.65**	0.00	0.14	0.19	0.02	1.00
INC/BJP	0.00	0.13	0.00	**0.69**	0.03	0.01	0.14	1.00
BJP/RP	0.00	0.02	0.31	0.03	**0.37**	0.08	0.19	1.00
INC/RP	0.09	0.00	0.16	0.09	0.02	**0.56**	0.08	1.00
INC/BJP/RP	0.00	0.03	0.10	0.15	0.12	0.20	**0.40**	1.00
Total	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00

Notes: Each cell shows the probability of a district transitioning from a category defined using pre-period data to a category defined using current period data. Districts safe for INC, BJP, and Regional Parties have been labelled INC Safe, BJP Safe, and RP Safe, respectively. Swing districts for INC and BJP, for BJP and Regional Parties, and for INC and Regional Parties have been labelled INC/BJP, BJP/RP, and INC/RP, respectively. Swing districts for INC, BJP, and Regional Parties have been labelled INC/BJP/RP.

Figure 1.5: Buying Votes Cycle in the Release of Funds in Swing Districts for the INC and the BJP



Notes: This graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. It has been plotted using coefficients in column (1) of Table 1.4. Districts that are swing for the INC and the BJP in the state elections during 1993–2003 have been labeled *Swing for INC and BJP*. The omitted category is $Year_{(+1)}$. Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1993–2014.

parties, where there is a 54.9% and 74.9% post-election increases in the release of funds, respectively.

The rest of the Section is organized as follows. I first confirm the existence of the three types of political cycles predicted by hypotheses (P1)-(P3) by estimating specification (1.3) in the six types of districts defined in Section 1.5.2, separately.[42] Then I discuss the aggregate

[42]Hypothesis (P1) predicted that swing districts for the two national parties (the INC and the BJP) should have a pre-election peak in the release of funds, hypothesis (P2) predicted that safe districts for regional parties and swing districts for the opposition (the BJP) and regional parties should have a post-election

political cycle that exists in all districts pooled together to compare my results to other papers in the literature. Finally, I discuss robustness.

1.6.1 Existence of Three Types of Political Cycles

Consistent with hypothesis (P1), swing districts for the INC and the BJP exhibit the "Buying Votes Cycle" with a 60.2% increase in the release of funds in the year prior to an election. The shape of the "Buying Votes Cycle" is consistent with the central incumbent trying to enhance its chances of winning seats in upcoming state elections by disbursing additional funds prior to elections in swing districts for the two national parties. I have plotted the "Buying Votes Cycle" in Figure 1.5 using the estimation results reported in column (1) of Table 1.4. In each figure of a political cycle, I plot the coefficients of interest, the $\beta_i s$, against years since election. I normalize the coefficient on the first year after an election (β_1) to be zero and measure all other coefficients relative to β_1.

Consistent with hypothesis (P2), safe districts for regional parties and swing districts for the BJP and regional parties, exhibit the "Patronage Cycle" with 54.9% and 74.9% increase in the release of funds in the second year after an election, respectively. Using the estimation results reported in columns (2) and (3) of Table 1.4, I plot the "Patronage Cycle" exhibited by these two types of districts in Figure 1.6. The post-election increase in disbursement of funds is consistent with the central incumbent buying support from regional parties. Specifically, the central incumbent is buying support from its regional party allies (see Section 1.7.1). The precise timing of the peak in the "Patronage Cycle" coincides with the beginning of a new fiscal year after a state election, when the central incumbent decides how to allocate funds across districts under the TSC (see Section 1.7.2). Swing districts for the BJP and regional parties have a larger peak in the release of funds compared to safe districts for regional parties, a difference driven by the fact that most regional parties

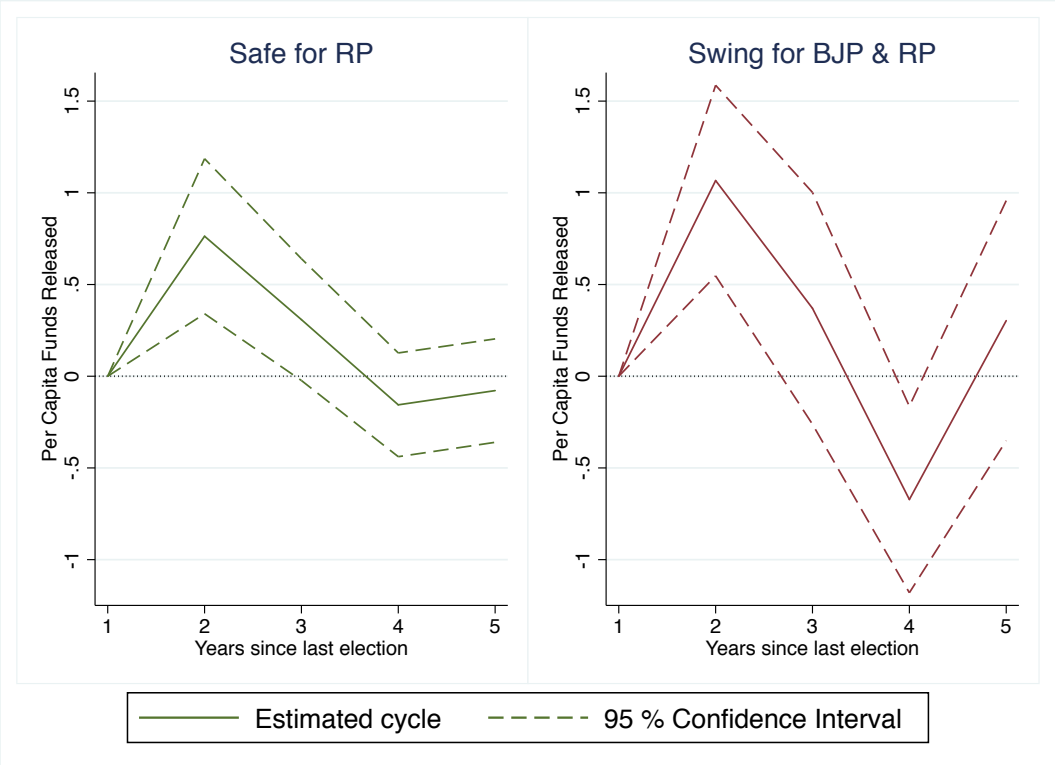
peak in the release of funds, and hypothesis (P3) predicted that safe districts for either of the two national parties, and swing districts for the incumbent (the INC) and regional parties should have no cycles in the release of funds.

Table 1.4: Political Cycles in Districts with Different Political Competition

	District Categories					
	Swing for INC & BJP	Safe for RP	Swing for BJP & RP	Safe for INC	Safe for BJP	Swing for INC & RP
	(1)	(2)	(3)	(4)	(5)	(6)
Year(+2)	-0.195	0.763***	1.067***	-0.050	0.428**	0.229
	(0.247)	(0.213)	(0.260)	(0.384)	(0.191)	(0.175)
Year(+3)	-0.300	0.309*	0.370	0.423	0.160	0.143
	(0.209)	(0.168)	(0.316)	(0.429)	(0.206)	(0.163)
Year(+4)	0.080	-0.156	-0.672**	-0.751	-0.018	0.025
	(0.260)	(0.143)	(0.254)	(0.452)	(0.303)	(0.212)
Year(+5)	1.015***	-0.078	0.303	0.901	0.577**	0.233
	(0.245)	(0.142)	(0.327)	(0.828)	(0.247)	(0.212)
Observations	9,941	13,308	7,004	2,512	5,368	10,296
R-squared	0.204	0.160	0.146	0.127	0.173	0.110
District and Time FE	Y	Y	Y	Y	Y	Y
Mean	1.686	1.390	1.425	1.641	1.305	1.055
Std. Dev	7.749	6.513	6.888	8.706	5.942	5.905
Clusters	87	118	62	22	47	90

Notes: The unit of observation is a district-month. The dependent variable is the monthly per capita expenditure for the TSC in a district. Each column reports the results for estimating model (1.3) in one of six categories of districts. Column (1) reports the estimated political cycle for swing districts for the INC and the BJP. Columns (2) and (3) report estimated political cycles for safe districts for regional parties, and swing districts for the BJP and regional parties, respectively. Columns (4)-(6) report estimated political cycles for safe districts for the INC and the BJP, and swing districts for the INC and regional parties, respectively. The omitted category is Year(+1) in each regression. All regressions control for the ratio of ACs in a district reserved for SCs and STs, and district and time (year-month) fixed effects. Robust standard errors clustered at district level are reported in the parentheses. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1993–2014.
** p<0.01, ** p<0.05, * p<0.1

Figure 1.6: Patronage Cycle in the Release of Funds in Safe Districts for Regional Parties, and Swing Districts for the BJP and Regional Parties



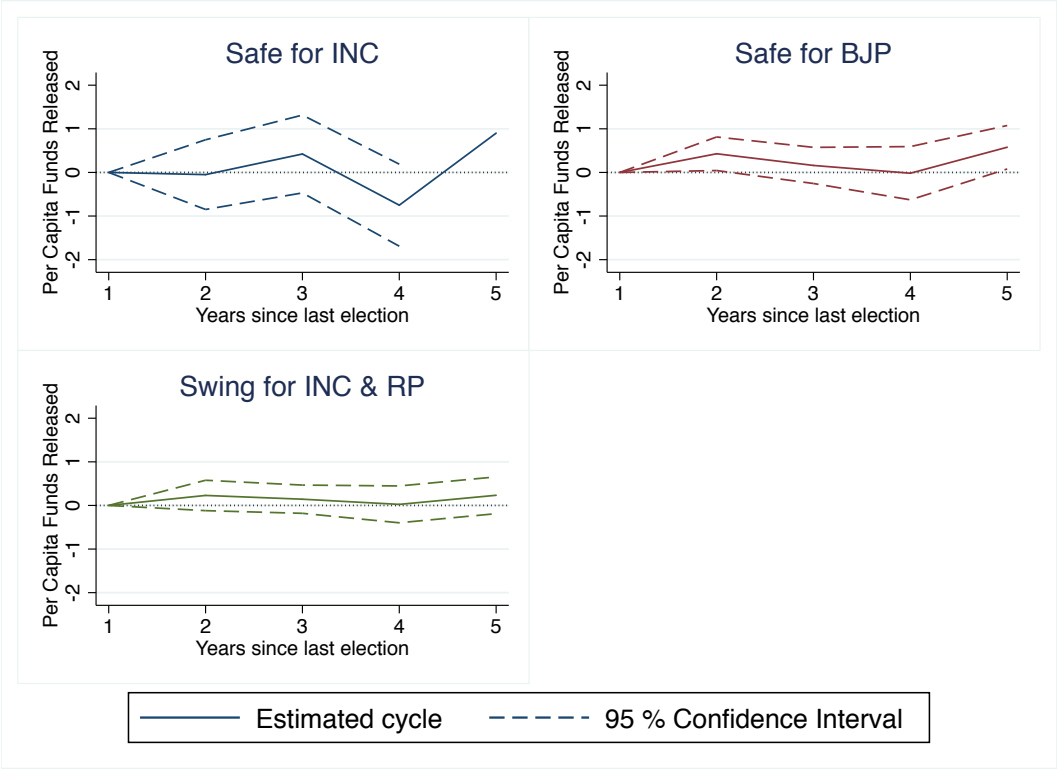
Notes: Each graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. These graphs have been estimated using coefficients in columns (2) and (3) of Table 1.4, respectively. Districts where a regional party won the largest proportion of assembly constituencies in all the state elections during 1993–2003 have been labeled *Safe for RP*. Districts that are swing for the BJP and regional parties in state elections during 1993–2003 have been labeled *Swing for BJP & RP*. The omitted category is $Year_{(+1)}$ in all graphs. Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1993–2014.

that win in the first category of districts are allied with the central incumbent, while the same is not true for regional parties that win in the second category of districts. Since the post-election peak in the release of funds is being driven by the central incumbent rewarding its allies, the second category of districts show a smaller increase in the release of funds (see Appendix E.1).

Consistent with hypothesis (P3), there are no cycles in the release of funds in safe districts for the INC, and in swing districts for the INC and regional parties. These results are reported in columns (4) and (6) of Table 1.4, respectively, and the political cycles are plotted in Figure 1.7. However, safe districts for the BJP show increases in the release of funds in the second and fifth years after an election. These peaks are not robust to defining political competition using contemporaneous election data (see Appendix C.1) or using a stricter definition of a safe district for a political party (see Appendices C.2 and C.3), and are being driven by districts that change definition between the pre-period and the contemporaneous period. Specifically, 62% of districts classified as safe for the BJP transition into being swing districts for the INC and the BJP, or swing districts for the BJP and regional parties (see Table 1.3), suggesting that using pre-period election data to define these districts does not predict the political competition being observed by the central incumbent when it is allocating funds during 2004–2014.

My results both confirm the existence of pre-election political manipulations as documented by past literature and contribute to the literature by showing the existence of a previously undocumented political cycle, the "Patronage Cycle", caused by post-election manipulations to compensate regional parties for their support. In two-party settings previous work has found evidence that swing districts for the two parties are most likely to receive funds from the incumbent before an election to increase its chances of winning these districts. The existence of the "Buying Votes Cycle" in swing districts for the two national parties confirms the findings of the literature in that these swing districts are the most similar to the competitive districts in the two-party settings. Unlike two-party models, where post-election political manipulations are consistent with the central incumbent rewarding loyal voters because they take the form of post-election benefits to safe districts for the incumbent, the "Patronage Cycle" is found in districts where the central incumbent never wins, ruling out the possibility that this cycle is about rewarding loyal voters.

Figure 1.7: No Political cycles in the Release of Funds in Districts that are Safe for INC or BJP, and in Swing Districts for the INC and Regional Parties



Notes: Each graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. These graphs have been plotted using coefficients in columns (4), (5), and (6) of Table 1.4, respectively. Districts where the INC and the BJP won the largest proportion of assembly constituencies in all state elections during 1993–2003 have been labeled *Safe for INC* and *Safe for BJP* respectively. Districts that are swing for the INC and regional parties in the state elections during 1993–2003 have been labeled *Swing for INC & RP*. The omitted category is $Year_{(+1)}$ in all graphs. I have omitted the 95% confidence interval for $Year_{(+5)}$ in safe districts for the INC to make the axis similar to Figures 1.5 and 1.6 for comparison. The coefficient for $Year_{(+5)}$ is not statistically significantly different from zero. Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1993–2014.

Other papers studying political cycles have not looked at political cycles in districts disaggregated by the six types of political competition discussed above. To better compare my results to past literature I also look at the political cycle that exists across all districts pooled together (the aggregate cycle). I find that the shape of the 'aggregate cycle' looks very different from the ones previously documented because it has two peaks: one pre-election and one post-election, confirming that the effects of both the "Buying Votes Cycle" and the "Patronage Cycle" are large enough to appear in the aggregate data (see Figure 1.8). However, the magnitudes of the pre-election (18.1%) and post-election (26.1%) peaks are much smaller than the ones in the disaggregated data, suggesting that only examining an aggregate cycle underestimates the size of manipulations relative to manipulations experienced by certain categories of districts.

1.6.2 Robustness

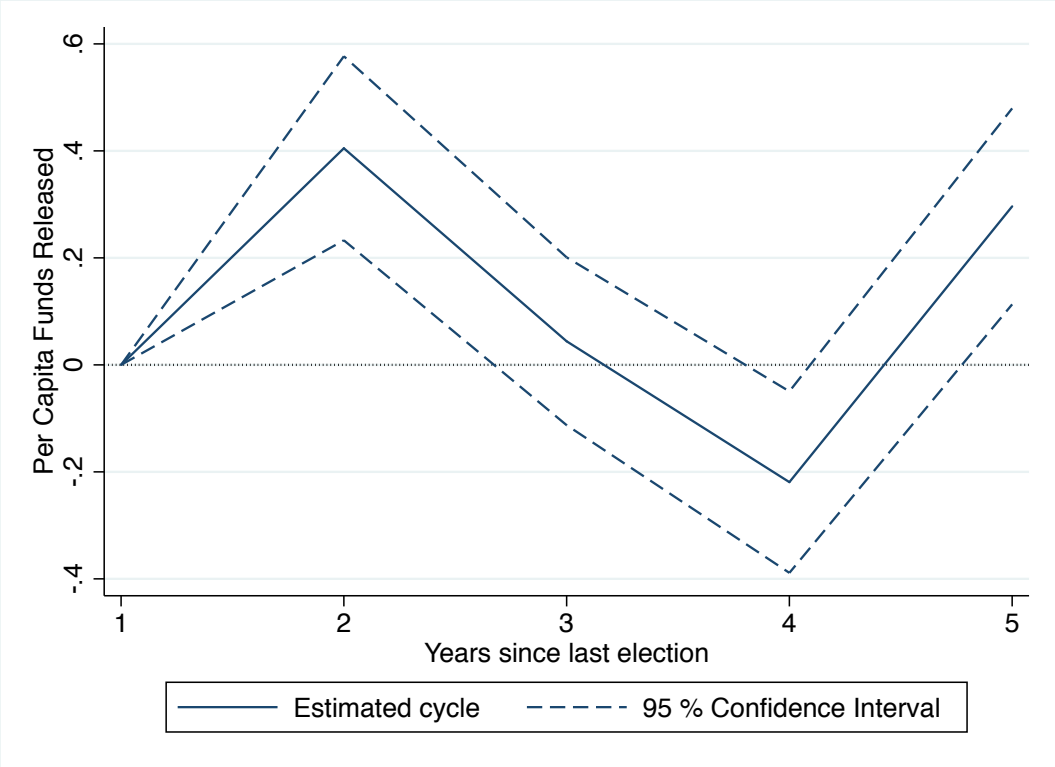
All three types of cycles are robust to: defining district categories using contemporaneous election data (Appendix C.1), using an alternate definition of categorizing a district as *Safe* or *Swing* (Appendix C.2), estimating political cycles using half-yearly (6-month interval) dummies for the electoral cycle instead of yearly dummies (Appendix C.4), estimating political cycles by aggregating the data to half-yearly levels (Appendix C.5),[43] and including district specific linear time trends (Appendix C.6). All cycles are also robust to using election results from 1993–2014 to categorize districts (Appendix C.3) and to dropping data for earlier than scheduled elections.[44] I discuss some of these briefly below and all of them in detail in Appendix C.

The political cycles in all six categories of districts are robust to using contemporaneous election data instead of pre-period election data to define district categories. While using pre-period election data allows me to more exogenously define district categories, it introduces

[43]Results are also robust to using quarters instead of half-years.

[44]Results are also robust to dropping Bihar and Jharkhand where the election in February 2005 gave a fractured verdict and led to re-election in October 2005, and to dropping all time-varying controls. Results are also robust to dropping one state at a time.

Figure 1.8: Political Cycle in the Release of Funds for all Districts Pooled Together



Notes: This graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. The omitted category is $Year_{(+1)}$. Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 2004–2014.

noise in the categorization of districts because districts transition between categories from the pre-period to the contemporaneous period. To confirm that noisy categorization of districts is not driving my results, I reclassify districts using contemporaneous election data and find that all six political cycles have the same shape as before. Additionally, safe districts for the BJP now have no political cycles and swing districts for the BJP and regional parties have no pre-election increase in the release of funds, confirming that these features of political cycles estimated using district categorizations from pre-period electoral data were a result of the noisy definition of these two types of districts.

The shapes of political cycles are not driven by estimating yearly political cycles using monthly frequency release of funds data. My preferred specification uses monthly frequency release of funds data and estimates political cycles using yearly electoral cycle dummies. Using monthly frequency data leads to better identification of political cycles as I can exploit the differences in specific timing of elections across districts. Using yearly electoral cycle dummies captures the institutional feature of the TSC where each district receives only one or two transfers in a given year; in most months a district does not receive any funds. To deal with the concern that choice of aggregation drives the shapes of political cycles, I show that all six political cycles are robust to: (a) using half-yearly or quarterly electoral cycle dummies, and (b) aggregating the data to half-yearly (yearly) level and estimating half-yearly (yearly) political cycles. Estimating monthly political cycles is inappropriate given the institutional features of the scheme. I discuss the details of robustness checks (a) and (b) in Appendices C.4 and C.5, respectively.

Political cycles are not driven by electoral cycles where the election occurred earlier than the scheduled date. As state governments in India can call elections earlier than the scheduled date, using actual election dates to define electoral cycle dummies can upwardly bias the $Year_{(+5)}$ coefficient if state governments systematically call elections early when they observe more funds being allocated to districts in the state.[45] However, early elections are rare in my sample: only 6 out of 59 state assembly occur before the scheduled date,[46] of which only two were called by the existing state government, and estimated cycles are robust to dropping these early election cycles.

In the following sections I provide additional results to show that the "Patronage Cycle" is in fact consistent with the central incumbent buying support from regional parties that are its allies and discuss suggestive evidence that both the "Buying Votes Cycle" and the

[45]The central government can also recommend the imposition of President's rule and removal of a state government (Article 356 of the Constitution of India), leading to early elections that can be correlated with the economic situation of a state.

[46]The early state elections are Bihar (2005), Haryana (2009), Himachal Pradesh (2007), Jharkhand (2009), Karnataka (2008), and Orissa (2004).

"Patronage Cycle" are large enough to affect household consumption in districts where these cycles exists.

1.7 Is the Patronage Cycle consistent with Buying Support?

I show that the "Patronage Cycle" is being driven by the central incumbent buying support from regional parties, ruling out two alternative explanations for the post-election increase in the release of funds. I also discuss why the peak occurs in the second year after an election. The "Patronage Cycle" is not consistent with the central incumbent rewarding its loyal voters because the post-election peak is only found in districts where the central incumbent never wins (Section 1.6).[47] Similarly, the post-election peak is not driven by some underlying characteristic of the district that also makes the district safe for regional parties as the post-election increases in the release of funds are specifically targeted towards districts where allies of the central incumbent win. Additionally, I provide evidence that the timing of the post-election peak corresponds to the first fiscal year after an election, the first opportunity for a central incumbent to manipulate the allocation of funds across districts after an election. As I am studying the "Patronage Cycle" in this section, all analysis is restricted to districts that exhibit this cycle: districts that are safe for regional parties and swing districts for the BJP and regional parties.

1.7.1 Buying Support from Allies

The post-election peak in the release of funds is driven by the central incumbent rewarding its regional party allies, whose support it needs to keep its coalition together.[48] Importantly, the

[47] In contrast, Cole (2009) finds evidence of state governments in India rewarding districts aligned with them post-election.

[48] Rodden and Wilkinson (2004) and Solé-Ollé and Sorribas-Navarro (2008) also find evidence of states and municipalities aligned with the national coalitions in India and Spain receiving more funds compared to unaligned states and municipalities, respectively.

Table 1.5: Patronage Cycle in Districts where Allies and Opposition of the Central Incumbent Win

	Safe for RP		Swing for BJP & RP	
	Allies	Opposition	Allies	Opposition
Year(+2)	1.152***	-0.237	1.968***	0.148
	(0.335)	(0.210)	(0.525)	(0.282)
Year(+3)	0.306	-0.077	0.262	0.316
	(0.266)	(0.219)	(0.676)	(0.681)
Year(+4)	0.166	0.019	-0.584	-1.346**
	(0.225)	(0.248)	(0.694)	(0.493)
Year(+5)	0.318	-0.764***	0.811	-0.051
	(0.195)	(0.225)	(0.569)	(0.512)
Observations	9,037	4,271	4,593	2,411
R-squared	0.189	0.215	0.242	0.167
District and Time FE	Y	Y	Y	Y
Mean	1.530	1.095	1.405	1.465
Std. Dev	6.935	5.503	6.109	8.169
Clusters	81	37	41	21

Notes: The unit of observation is a district-month. The dependent variable is the monthly per capita expenditure under TSC in a district. The sample for columns (1) and (2) are safe districts for regional parties. The sample for columns (3) and (4) are swing districts for the BJP and regional parties. Columns (1) and (3) report the estimation of model (1.3) in Bihar, Tamil Nadu, Uttar Pradesh, and West Bengal. Columns (2) and (4) report the estimation of model (1.3) in the remaining states. The omitted category is Year(+1) in each regression. All regressions control for the ratio of ACs in a district reserved for SCs and STs, and district and time (year-month) fixed effects. Robust standard errors clustered at district level are reported in the parentheses. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014; political data on state assembly elections during 1993–2014. *** p<0.01, ** p<0.05, * p<0.1

peak in the "Patronage Cycle" is not being driven by some underlying characteristic common to all districts where regional parties win. I show this by estimating the "Patronage Cycle" separately in districts where regional parties allied to and opposed to the central incumbent win and finding that only the first set of districts have the post-election peak.

To test if the post-election peak only exists in a subset of the districts, I first divide districts into two categories based on the identity of the regional parties that win them: districts where regional party allies of the central incumbent win and districts where the rest of the regional parties win.[49] The first category includes districts in the states of Bihar, Tamil Nadu, Uttar Pradesh, and West Bengal and the second category includes districts in the remaining states. I label these two categories *Allies* and *Opposition*, respectively.[50] It is interesting to note that most of these allied regional parties gave outside support to the central incumbent at various points in time during 2004–2014 instead of being a part of the coalition government and thus could not be rewarded with cabinet positions in the government.[51] Most of these regional parties were also not giving support to the central incumbent because of ideological reasons, and therefore their threats to withdraw support from the coalition were credible.

I estimate specification (1.3) separately in districts categorized as *Allies* and *Opposition* above, and find that the post-election peak in the "Patronage Cycle" only exists in the first set of districts. For both, safe districts for regional parties, and swing districts for the BJP and regional parties, only districts classified as *Allies* show a post-election peak in the release of funds (columns (1) and (3) of Table 1.5), while districts classified as *Opposition* do not show any peak in the release of funds (columns (2) and (4) of Table 1.5). These results are

[49]I classify a regional party as an ally of the central incumbent if for any period of time during 2004–2014 it was either a coalition partner of the central incumbent or provided outside support to the coalition.

[50]Even though there some regional parties in states classified as *Opposition* were allied with the central incumbent, I classify only four states as *Allies* because these states had some of the most important (in terms of number of seats in the Parliament) regional parties that were allied with the central incumbent at the federal/central level during this period. For example, the Samajwadi Party and the Bahujan Samaj Party in Uttar Pradesh; the Rashtriya Janata Dal in Bihar; the Dravida Munnetra Kazhagam in Tamil Nadu; the Communist Party of India (Marxist) and the All India Trinamool Congress in West Bengal.

[51]I have discussed outside support in the context of coalition governments in Section 1.2.1.

consistent with hypotheses (P4) and (P5) derived from an extension of my model that adds an additional regional party unallied with the central incumbent. These hypotheses predict that only those districts where regional parties allied with the central incumbent win, exhibit the "Patronage Cycle" (See Appendix A.2).

To deal with the issue of endogeneity in classifying a specific district as allied to or opposed to the central incumbent, I choose to define *Allies* as a broad category that includes four states that had important allies of the central incumbent since the beginning of the tenure of the central incumbent.[52] The above categorization is likely to underestimate the magnitude of the "Patronage Cycle" in the districts classified as *Allies* because while I classify all districts[53] in a state as being allied to the central incumbent, there are instances where there are multiple regional parties in a state and some of them are allied to the central incumbent while others are not. Therefore, the cycle I observe is a combination of the cycles for allied and non-allied regional parties in these states. If the central incumbent is not buying support from the latter, then the overall cycle should have a smaller peak compared to one restricted only to regional parties giving support to the central incumbent.

Another way to test if the "Patronage Cycle" is driven by districts where regional parties more likely to be important allies of the central incumbent are competitive, is to use the demographic characteristics of districts that are associated with core supporters of potential allies. In particular, several regional parties in India have a strong Muslim and Scheduled Caste (SC) vote base (Chandra (2004), Mehra (2010)) and these parties were also more likely to be allied with the INC during 2004–2014. Therefore, I classify districts with a larger proportion of Muslim and SC populations as districts[54] where allied regional parties are more likely to be competitive. I show that the post-election peak in the "Patronage

[52]New regional parties were formed between the pre-period and contemporaneous period making it difficult to use pre-period data to classify districts as safe for a specific regional party. Additionally, it is difficult to use pre-period election data to classify a district as safe for a particular regional party because of strong competition among regional parties.

[53]Safe districts for regional parties or swing districts for the BJP and regional parties.

[54]I classify districts as having a low Muslim (SC) population if the proportion of Muslim (SC) population in a district is less than the median level in Census 2001.

Cycle" is being driven by districts with high Muslim and high SC populations (see Table 1.6), while districts with low Muslim and low SC populations do not show any post-election increases in the release of funds. I can further show that the peaks in both political cycles are not just being driven by the presence of these groups because the pre-election peak in the "Buying Votes" cycle only exists in districts with low Muslim and low SC populations.

1.7.2 Timing of the Peak in the Patronage Cycle

The timing of the peak in the "Patronage Cycle" coincides with the first 2-3 installments of funds released by the central incumbent after the beginning of a new fiscal year after an election.[55] Given the institutional structure of the TSC, this is the first opportunity after an election for the central incumbent to manipulate the allocation of funds across districts.

There are three institutional features of the TSC that affect the precise timing of the peak in the "Patronage Cycle". First, districts have to prepare a financial plan for each upcoming fiscal year and submit it to the state and central governments by February for approval. This implies that if a state election occurs right before the start of a new financial year (e.g., in February) then districts do not have enough time to prepare a new financial plan that reflects the priorities of the changed political control over the district.[56] Second, the central government makes decisions regarding the allocation of funds across districts for the upcoming financial year in March of every year. Therefore, if a state has an election in September, the central government can only allocate funds to its allies beginning in March of next year. Third, funds are typically released to a district one or two times in a year so the release of additional funds is spread over the first 2-3 installments (generally, 12 to 18 months) once the new budget is approved.

I test if the peak in the "Patronage Cycle" coincides with the beginning of a new fiscal year after an election by estimating the following regression separately in districts that are safe for regional parties and districts with turnover between the BJP and regional parties:

[55]Fiscal years in India start in April every year.
[56]See Government of India (2007)

Table 1.6: The Patronage Cycle Across Demographic Groups

	Safe for RP				Swing for INC & BJP			
	High SC	Low SC	High Mus	Low Mus	High SC	Low SC	High Mus	Low Mus
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Year(+2)	1.084***	0.291	1.172***	0.072	-0.231	-0.296	0.371	-0.400
	(0.340)	(0.225)	(0.279)	(0.328)	(0.519)	(0.231)	(0.496)	(0.283)
Year(+3)	0.511*	0.096	0.409**	0.353	-0.081	-0.502**	-0.288	-0.313
	(0.260)	(0.220)	(0.199)	(0.313)	(0.365)	(0.243)	(0.304)	(0.284)
Year(+4)	0.084	-0.378*	-0.047	-0.346	0.332	-0.065	-0.276	0.395
	(0.185)	(0.202)	(0.186)	(0.251)	(0.358)	(0.375)	(0.291)	(0.442)
Year(+5)	0.138	-0.471**	-0.068	-0.200	0.750*	1.251***	0.598	1.062***
	(0.222)	(0.191)	(0.178)	(0.299)	(0.370)	(0.330)	(0.561)	(0.270)
Observations	8,187	5,121	8,597	4,711	3,313	6,628	2,516	7,425
R-squared	0.166	0.214	0.157	0.232	0.217	0.216	0.149	0.257
District and Time FE	Y	Y	Y	Y	Y	Y	Y	Y
Mean	1.527	1.172	1.320	1.519	1.712	1.672	1.192	1.853
Std. Dev	7.132	5.371	6.526	6.488	8.045	7.597	5.734	8.315
Clusters	72	46	77	41	29	58	22	65

Notes: The unit of observation is a district-month. The dependent variable is the monthly per capita expenditure for the TSC in a district. The sample for columns (1)–(4) are safe districts for regional parties. The sample for columns (5)–(8) are swing districts for the INC and the BJP. The sample for columns labeled High (Low) SC are districts where the proportion of SC population is above (below) the median, according to Census 2001. The sample for columns labeled High (Low) Muslim are defined analogously. The omitted category is Year(+1) in each regression. All regressions control for the ratio of ACs in a district reserved for SCs and STs, and district and time (year-month) fixed effects. Robust standard errors clustered at district level are reported in the parentheses. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1993–2014. *** p<0.01, ** p<0.05, * p<0.1

$$Exp_{dt} = \alpha + \kappa Fiscal_{dt} + \sum_{i=2}^{5} \beta_i Year_{(+i)dt} + \gamma X_{dt} + \delta_d + \theta_t + \epsilon_{dt} \tag{1.4}$$

where *Fiscal* is a dummy takes the value 1 for the 15 months beginning in April of the next year if district d had an election between January to October, takes the value 1 for the 15 months beginning in April of the next to next year, if district d had an election in November or December, and takes the value 0 otherwise.[57] For example, if a district had an election in May 2005, then *Fiscal* takes the value 1 from April 2006–July 2007, and if a district had an election in December 2005, *Fiscal* takes the value 1 from April 2007–July 2008. While most of the increases in the release of funds occur in the first fiscal year (12 months), some districts get a third higher installment as well. To capture the peak in such districts well, I allow the dummy *Fiscal* to take the value 1 in the first 15 months after the beginning of a fiscal year.[58] The remaining variables are the same as defined in specification (1.3).

I show that the timing of the post-election peak in both types of districts with the "Patronage Cycle" can be explained by the *Fiscal* dummy. I can include both the electoral cycle dummies and the *Fiscal* dummy in specification (1.4) because different states in India have elections in different months, which implies that the dummy *Fiscal* is not collinear with any given year in the election cycle. Including both types of dummies allows me to test which one can explain the precise timing of the peak in the "Patronage Cycle." I find that upon including the *Fiscal* dummy in my regression specification, the timing of the post-election peak is explained by the *Fiscal* dummy and the dummies for years since election show no increases in the release of funds. The results for estimation of model (1.4) are reported in Table 1.7. Columns (1) and (3) report the original "Patronage Cycle" in the two types of districts from my earlier estimation of specification (1.3). Columns (2) and (4) report the results for the estimation of specification (1.4).

[57] I use this definition of *Fiscal* because I find that districts with elections in November–February do not get rewarded in the fiscal year beginning next April. I attribute this to the first institutional feature of the scheme, i.e., districts have to submit a financial plan by February and it takes some time to fully change this plan in a few months.

[58] See Table D.2 for results with *Fiscal* defined as the first 12 months of a new fiscal year.

Table 1.7: Timing of the Peak in the Patronage Cycle

	Safe for RP		Swing for BJP & RP	
	(1)	(2)	(3)	(4)
Fiscal		0.577**		0.951***
		(0.223)		(0.322)
Year(+2)	0.763***	0.352	1.067***	0.354
	(0.213)	(0.258)	(0.260)	(0.326)
Year(+3)	0.309*	0.268	0.370	0.189
	(0.168)	(0.166)	(0.316)	(0.279)
Year(+4)	-0.156	-0.043	-0.672**	-0.498*
	(0.143)	(0.152)	(0.254)	(0.258)
Year(+5)	-0.078	0.007	0.303	0.367
	(0.142)	(0.147)	(0.327)	(0.317)
Observations	13,308	13,308	7,004	7,004
R-squared	0.160	0.161	0.146	0.146
District and Time FE	Y	Y	Y	Y
Mean	1.390	1.390	1.425	1.425
Std. Dev	6.513	6.513	6.888	6.888
Clusters	118	118	62	62

Notes: The unit of observation is a district-month. The dependent variable is the monthly per capita expenditure under TSC in a district. The sample for columns (1) and (2) are safe districts for regional parties. The sample for columns (3) and (4) are swing districts for the BJP and regional parties. Columns (1) and (3) report the estimation of model (1.4) without including the *Fiscal* dummy. Columns (2) and (4) report the estimation of model (1.4) including the *Fiscal* dummy. The omitted category is Year(+1) in each regression. All regressions control for the ratio of ACs in a district reserved for SCs and STs, and district and time fixed effects. Robust standard errors clustered at district level are reported in the parentheses. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1993–2014.
*** p<0.01, ** p<0.05, * p<0.1

1.8 Effect of Political Cycles on Households

I use data on household consumption expenditure to provide evidence about the magnitude of political manipulations discussed above and their potential effects on household welfare. I find that monthly per capita consumption of households in districts that exhibit the "Buying Votes Cycle" increases by 6.2% in the year before an election, and monthly per capita consumption of households in districts that exhibit the "Patronage Cycle" increases by 4.3% post-election. Cycles in consumption are absent in the remaining districts.

Political cycles in household consumption suggest that: first, funds disbursed under both the "Buying Votes Cycle" and the "Patronage Cycle" reach households;[59] second, political cycles in the release of funds for the TSC are indicative of a general pattern of government behavior since the funds involved in the TSC alone are not large enough to affect aggregate household consumption, and third, political cycles in the disbursement of funds reduce household welfare compared to funds being released without cycles by introducing volatility in household consumption.

1.8.1 Data and Estimation Strategy

I use all National Sample Survey (NSS) Household Consumption Rounds during 2004–2014 to construct a repeated cross-section dataset of monthly household consumption in rural districts.[60] In each of these surveys, households are asked about consumption of various goods and services during a 30 day or 365 day recall period.[61] The NSS surveys are representative at the district-rural level and were conducted annually during 2004–2008, and once every two years from 2008 to 2012.

[59] Ideally I would directly test whether households receive funds under the TSC. However, there is only one household survey (the IHDS) that asks this question and it can not be used to comment on the timing of the receipt of funds.

[60] I use NSS Survey Rounds 61 (2004–05), 62 (2005–06), 63 (2006–07), 64 (2007–08), 66 (2009–10), and 68 (2011–12).

[61] Items with 30 day recall period include foodstuff, tobacco, intoxicants, fuel, medical (non-institutional) services, rents, consumer taxes, etc. Items with 365 day recall period include clothing, bedding, footwear, education and medical (institutional) services, durable goods, etc.

To test if household consumption is affected by the "Buying Votes Cycle" and the "Patronage Cycle," I estimate the following regression separately in districts[62] that exhibit these cycles:

$$y_{hdt} = \alpha + \sum_{i=2}^{5} \beta_i Year_{(+i)dt} + \theta X_{hdt} + \pi_t + \delta_d + \epsilon_{hdt} \tag{1.5}$$

where h indexes households, d indexes districts, and t indexes date (year-month) of survey. My preferred dependent variable, y_{hdt}, is the monthly per capita expenditure on items with a 30-day recall period only. The short recall period on these items allows me to determine the precise timing of consumption relative to an election, and so to estimate political cycles in consumption accurately.[63] The variable of interest, $Year_{(+i)dt}$, is an indicator variable that takes value 1 if district d at time t is in its i^{th} year since the last state election. I include year-month fixed effects, π_t, district fixed effects, δ_d, and a vector of household-level controls, X_{hdt}.[64] Standard errors are clustered at the district level.

The two types of cycles should affect household income differently. Since funds disbursed under the "Buying Votes Cycle" are used to influence election outcomes, funds should (a) reach households and (b) arrive before an election. Thus, household income should increase in the year prior to an election in these districts. By contrast, it is not necessary that funds disbursed under the "Patronage Cycle" should reach households, as funds can be diverted by local politicians. Moreover, even if regional parties release funds to households, it is unclear whether they have an incentive to do so without delay. Thus, if there is an increase in household income in districts exhibiting the "Patronage Cycle," I can only predict that it must occur during or after the second year post-election (i.e., after the central incumbent has released funds).

[62] The "Buying Votes Cycle" only exists in swing districts for the INC and the BJP. The "Patronage Cycle" only exists in safe districts for regional parties and in swing districts for the BJP and regional parties. The remaining districts do not exhibit any political cycles.

[63] As a robustness check, I also report results using the monthly per capita expenditure reported in the survey that includes expenditure on items with both 30 day and 365 day recall periods.

[64] X_{hdt} includes controls for: religion, caste group, quantity of land possessed, age, sex, and education of household head, proportion of household members who are women, proportion of household members who are under 15, and an indicator variable for whether or not the household had a ceremony in the last 30 days.

I expect political cycles in funds released to households to affect consumption patterns of households because rural household are unable to completely smooth their consumption against the income shocks caused by political cycles. In particular, political cycles in the release of funds to households can cause unpredictable and transient income shocks: households do not know whether they will receive a transfer, households do not know the amount of transfer, and households do not know the precise timing of transfer (beyond that it will be prior to an election in districts with the "Buying Votes Cycle"). Additionally, it has been documented that most poor households cannot save easily despite having money because lack of access to formal credit and savings institutions, temptation, lack of safe place to save, transfers to friends and family, etc. (e.g., Banerjee and Duflo (2007), Ashraf et al. (2010), Dupas and Robinson (2013)) may prevent households from saving. Both these features together imply that transient income shocks to households will affect their consumption and not just savings, producing cyclical patterns in household consumption expenditure.

1.8.2 Results

Relative to the first year after an election, there is a 6.2% increase in monthly per capita expenditure in the year prior to an election among households in districts that exhibit the "Buying Votes Cycle," and a 4.3% increase in the monthly per capita expenditure post-election among households in districts that exhibit the "Patronage Cycle."[65] In addition, districts that exhibit no cycle in the release of funds under the TSC, do not show any political cycle in monthly per capita expenditure (see Table 1.8). All cycles are remarkably similar in shape to the patterns in the release of TSC funds in these districts, though the timing of the post-election increase suggests a potential delay between funds being released by the central incumbent and funds reaching households in districts exhibiting the "Patronage Cycle" (see Figure 1.9).

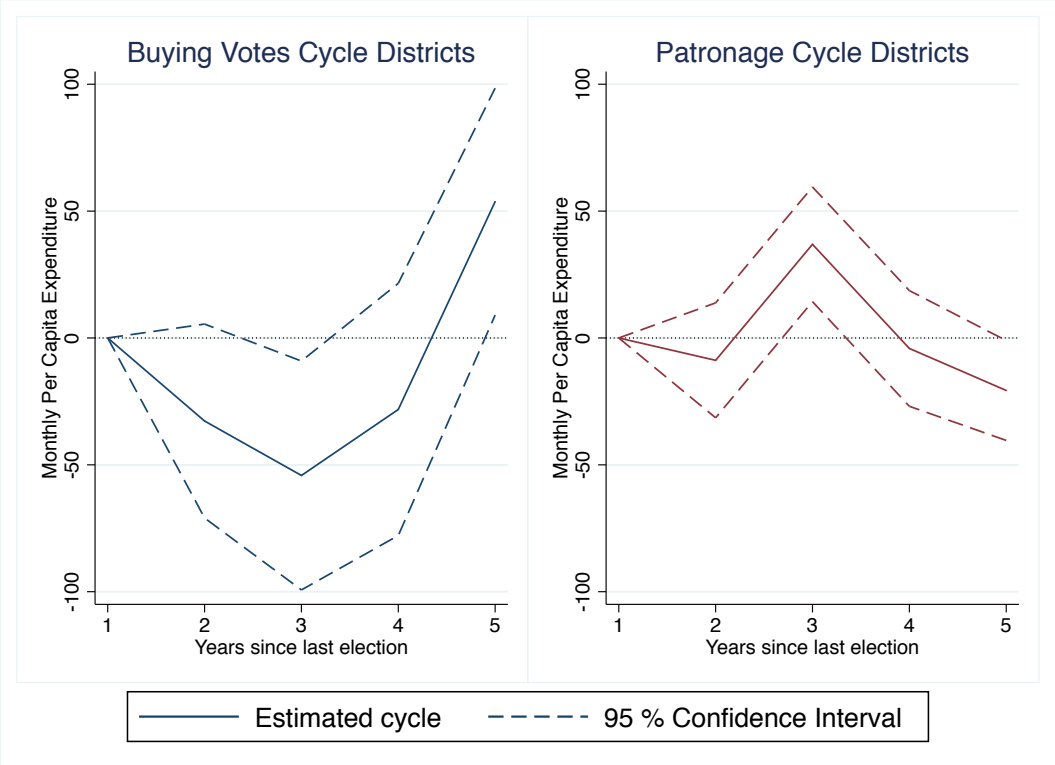
[65]When using items with both 30-day and 365-day recall, I find households in districts that exhibit the "Buying Votes Cycle" show an increase (8.35%) in the monthly per capita expenditure in the year prior to

Table 1.8: Buying Votes and Patronage Cycles in Household Consumption Expenditure

	BV Cycle	Patronage Cycle	No Cycle
Year(+2)	-32.731*	-8.812	-14.865
	(19.224)	(11.476)	(12.169)
Year(+3)	-54.189**	36.909***	-13.987
	(22.690)	(11.449)	(13.820)
Year(+4)	-28.179	-4.126	-24.181
	(24.998)	(11.554)	(17.817)
Year(+5)	53.729**	-20.693**	3.866
	(22.497)	(9.972)	(15.041)
Observations	32,353	97,625	71,212
R-squared	0.333	0.289	0.294
District and Survey FE	Y	Y	Y
Mean	853.2	861.5	988.1
Std. Dev	789.7	766.2	971.8
Clusters	87	180	157

Notes: The unit of observation is a household. The dependent variable is the monthly per capita consumption expenditure of a household on items with a 30-day recall period. Columns (1), (2), and (3) report the estimation results of specification (1.5) in districts with the "Buying Votes Cycle" (swing districts for the INC and the BJP), districts with the "Patronage Cycle" (safe districts for regional parties and swing districts for the BJP and regional parties), and districts with the no political cycles (safe districts for the INC, safe districts for the BJP, and swing districts for the INC and regional parties). The omitted category is Year(+1). All regressions include district and time fixed effects. Standard errors are clustered by district. All regressions control for household characteristics like religion; caste group; quantity of land possessed; age, sex, and education of household head; proportion of household members who are women; proportion of household members who are under 15; and an indicator variable for whether or not the household had a ceremony in the last 30 days. Datasets used in this analysis: NSS Consumption Survey Rounds 61 (2004-05), 62 (2005-06), 63 (2006-07), 64 (2007-08), 66 (2009-10), and 68 (2011-12) and political data on state assembly elections during 1993-2014. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Figure 1.9: Political cycles in Household Consumption in Districts with the Buying Votes Cycle and in Districts with the Patronage Cycle



Notes: Each graph plots the monthly per capita expenditure (on items with a 30 day recall) of households during an electoral cycle controlling for household characteristics, and district and time fixed effects. These graphs have been plotted using coefficients reported in columns (1) and (2) of Table 1.8, respectively. The left graph shows the political cycle for swing districts for the INC and the BJP. The right graph shows the political cycle for safe districts for regional parties and swing districts for the BJP and regional parties. The omitted category is Year(+1) in both graphs. Datasets used in this analysis: NSS Household Consumption Surveys (Rounds 61, 62, 63, 64, 66, and 68); political data on state assembly elections during 1994-2014.

My results suggest that funds disbursed under both cycles reach households, are common to multiple government schemes, and affect household welfare. First, political cycles in household consumption match the political cycles I document in government expenditures suggesting that these funds (or some proportion of funds) reach households. It is unlikely that these peaks in consumption are driven solely by other determinants of households' demand such as ceremonies, festivities, or rainfall, given that there is no reason to believe these determinants are correlated with electoral cycles during the period I study. Second, given that the release of funds for the TSC is small relative to household consumption, political cycles in household consumption appear to be driven by common government action across multiple schemes. Third, political cycles in government transfers to households introduce volatility into household income and lead to increased volatility in consumption. In addition to the peaks in household consumption discussed above, I find that households have significantly lower per capita consumption at other points in the electoral cycle. For example, household consumption is significantly lower in the second and third years since election, and in the fifth year since election in districts that exhibit the "Buying Votes Cycle" and the "Patronage Cycle," respectively (see Table 1.8). While political manipulations increase volatility of household consumption, they do not lead to an increase in the average consumption of households because the sum of coefficients on the election cycle dummies is statistically indistinguishable from zero in all three types of districts. Therefore, if households prefer consumption smoothing, but lack access to saving devices, political cycles in the release of government funds can be welfare reducing compared to a scenario where households receive fixed payments from government schemes each year.

an election and households in districts that exhibit the "Patronage Cycle" show an increase (4.8%) in the monthly per capita expenditure post-election.

1.9 Conclusion

In this paper, I show that the central incumbent in a coalition government can use spatial allocation of public funds to both enhance its chances of winning upcoming state elections *and* maintain legislative support from regional parties. In particular, I document the existence of two types of political cycles in the release of public funds for the TSC by the central incumbent in India during 2004–2014: the previously well-documented "Buying Votes Cycle" and the *heretofore undocumented* "Patronage Cycle". The "Buying Votes Cycle" only exists in swing districts for the two national parties, causes a 60% increase in the release of funds in the year prior to an election, and is consistent with the literature that finds politically motivated pre-election increases in the disbursement of government funds, provision of infrastructure, etc., in swing districts for the incumbent in two-party settings. The "Patronage Cycle" only exists in safe districts for regional parties and in swing districts for the national opposition party and regional parties and leads to a 55-75% increase in the release of funds post-election depending on the type of political competition in a district.

I link the "Patronage Cycle" with the specific political environment in India during 2004–2014 by showing that this cycle is driven by the central incumbent buying support from regional parties that were giving outside support to the central incumbent and threatened to leave the coalition at several points in time (e.g., the SP, the BSP, the DMK, etc.). The existence of the "Patronage Cycle" provides the insight that ignoring the unique features and goals of coalition governments can miss sizable manipulations and underestimate the extent of political manipulations in multi-party democracies. Specifically, my results suggest that it is important to characterize specific forms of political competition, beyond swing and safe or aligned and unaligned districts, to study political manipulations in multi-party systems.

Furthermore, my paper is the first to show that both types political manipulations are large enough to impact the timing of household consumption in India. Household consumption increases by 6.2% in pre-election in districts exhibiting the "Buying Votes Cycle" and by 4.3% post-election in districts exhibiting the "Patronage Cycle." My results indicate that

both types of political manipulations (a) are not limited to one government scheme, but are indicative of a general pattern of government behavior, and (b) introduce volatility into the consumption patterns of households, reducing household welfare compared to funds being released without a cycle.

A Mathematical Appendix

A.1 Assumptions and Proofs

I place the following assumptions on the probabilities of the model.

Assumption 1. *The probabilities of the model satisfy:*

1. *Probabilities $p_j^y(\cdot)$, $q_j^k(\cdot)$, and $s(\cdot,\cdot)$ are continuously differentiable in their elements for all j, k, y.*

2. *Probabilities $p_j^I(\cdot)$ and $q_j^Z(\cdot)$ are strictly increasing in their elements for all j. Probability $s(m_i, n_i)$ is increasing in m_i, and decreasing in n_i for all i.*

3. $\max_m p_{IZ}^{I\prime}(m) < \min_m p_{IO}^{I\prime}(m)$.

4. $-\delta W \frac{\partial s(m_i,n_i)}{\partial n_i} > \frac{dq_{IZ}^Z(n_i)}{dn_i} v \ \forall \ n_i$ *and* $i \in D_{IZ}$.

I discuss the above assumptions in detail below. The probability of party I winning in district i is strictly increasing in the funds released in district i pre-election, m_i. These funds can be used to sway the election result towards the central incumbent using several tactics. For example, direct cash transfers to voters, construction of public work projects in a district, mobilizing the local leaders in the district etc. Party I's probability of receiving support from party Z in district i is strictly increasing in the amount of funds offered to district i, n_i. Release of additional funds in districts after party Z has won there, helps party Z keeps its electoral promises and indicates its ability to extract resources from the central incumbent to its voters. The probability of party I winning the second election in district i is strictly decreasing if it lost the first election and bought support from the winning party in district i because if party I releases funds in a district after another party has won there, then some benefits of these additional funds accrue to the winning party, enhancing its chances of winning the next election while decreasing party I's chances of winning the next election.

Party I is better off spending additional funds in districts where it directly competes with party O compared to districts where it directly competes with party Z. The latter districts have some loyal supporters of party Z who can't be wooed by party I by giving them benefits. These voters support party Z based on similarities in demographic characteristics, for example, language, religion, caste etc. and it is not easy for party I to sway their votes. For example, in Tamil Nadu the two major regional parties the DMK and the AIADMK, are both linguistic based parties. It would be very hard for the national parties to sway the votes of people who are voting along linguistic lines just by giving them some benefits pre-election. Therefore, while 'buying votes' pre-election increases party I's chances of winning in the district, these funds can be better used in other districts (for example, where parties I and O compete) where party I has better chances of winning.

In districts with turnover between parties I and Z, while buying support from party Z increases party I's utility in Period 2, it reduces the probability of party I winning the next election because some benefits of these additional funds released in a district where party Z has won, accrue to party Z and not to party I. I assume that the benefits from spending an additional unit of funds on buying support from party Z are always less than the cost of these funds in terms of reducing party I's probability of winning the next election in the district.

Proof of Proposition 1:

1. Let $i \in D_{IO}$. In these districts $p^I_{IO}(m_i) > p^I_{IO}(0)$ and $s(m_i, n_i) > s(0, n_i)\ \forall\ m_i > 0$. Therefore, $m_i \geq 0$. If party I wins, it does not need to buy support from party O in this district, thus $n_i = 0$. If party I loses, party O will never accept party I's offer to buy support, therefore again $n_i = 0$.

2. Let $i \in D_Z$. In these districts $p^I_Z(m_i) = 0$ and $s(m_i, n_i) = 0\ \forall\ m_i$. Suppose $\exists\ i \in D_Z$, such that $m_i > 0$, then party I can increase its expected utility by allocating this m_i

to buy votes in $i' \in D_{IO}$. Thus, $\nexists\ i \in D_Z$, such that $m_i > 0$. However, $q_Z^Z(n_i) > q_Z^Z(0)$ $\forall\ n_i > 0$, therefore $n_i \geq 0$.

Let $i \in D_{OZ}$. In these districts $p_{OZ}^I(m_i) = 0$ and $s(m_i, n_i) = 0\ \forall\ m_i$. Suppose $\exists\ i \in D_{OZ}$, such that $m_i > 0$, then party I can increase its expected utility by allocating this m_i to buy votes in $i' \in D_{IO}$. Thus, $\nexists\ i \in D_{OZ}$, such that $m_i > 0$. If party O wins in district i, it will never accept party I's offer to buy support, therefore $n_i = 0$. If party Z wins, $q_{OZ}^Z(n_i) > q_{OZ}^Z(0)\ \forall\ n_i > 0$, therefore $n_i \geq 0$.

3. Let $i \in D_I$. In these districts $p_I^I(0) = 1$ and $s(0, n_i) = 1$. Suppose $\exists\ i \in D_I$, such that $m_i > 0$, then party I can increase its expected utility by allocating this m_i to buy votes in $i' \in D_{IO}$. Thus, $\nexists\ i \in D_I$, such that $m_i > 0$. Since party I always wins in these districts, it does not need to buy support from the winning party post-election. Therefore, $n_i = 0$.

   Let $i \in D_O$. In these districts $p_O^I(m_i) = 0$ and $s(m_i, n_i) = 0\ \forall\ m_i$. Suppose $\exists\ i \in D_O$, such that $m_i > 0$, then party I can increase its expected utility by allocating this m_i to buy votes in $i' \in D_{IO}$. Thus, $\nexists\ i \in D_O$, such that $m_i > 0$. Party O never accepts party I's offer to buy support, therefore $n_i = 0$.

   Let $i \in D_{IZ}$. Party I's expected utility from district i is given by: $p_{IZ}^I(m_i)u + \delta[1 - p_{IZ}^I(m_i)]q_{IZ}^Z(n_i)v + \delta^2 s(m_i, n_i)W$. If there is an interior solution, the expected utility is maximized at n_i^* if $[1 - p_{IZ}^I(m_i)]\ \frac{dq_{IZ}^Z(n_i^*)}{dn_i}v = -\delta W \frac{\partial s(m_i, n_i^*)}{\partial n_i}$. By assumption 1, $[1 - p_{IZ}^I(m_i)]\frac{dq_{IZ}^Z(n_i^*)}{dn_i}v < -\delta W \frac{\partial s(m_i, n_i^*)}{\partial n_i}$. Therefore party I will choose $n_i = 0$, because the cost of spending an additional unit of funds in district i is greater than the benefit.

   Given that $n_i = 0$, the expected utility of party I from district i is $p_{IZ}^I(m_i)u + \delta^2 W s(m_i, 0)$. Suppose party I decides to spend m_i^* funds in district i and m_j^* funds in district j where $j \in D_{IO}$. Now if it moves μ funds from district i to district j, its expected utility in district i will be $p_{IO}^I(m_i^* - \mu)u + \delta^2 W s(m_i^* - \mu, 0)$ and its expected

utility in district j will be $p^I_{IO}(m^*_j + \mu)u + \delta^2 Ws(m^*_j + \mu, 0)$. By assumption 1, party I is now better off. Therefore, it will choose $m_i = 0$.

A.2 Extension 1: Adding a Second Regional Party

Suppose now there is a second regional party, Z_1, that is not an ally of the central incumbent. The timing of the model is the same as before, except now both parties O and Z_1 will never give support to party I.

I divide districts into ten groups based on their level of political competition and the identity of the winning party within a level of political competitiveness. Districts that are safe for parties I, O, Z, and Z_1 are denoted by D_I, D_O, D_Z, and D_{Z_1}, respectively. Districts that are swing for parties, I and O, O and Z, O and Z_1, Z and Z_1, I and Z, and I and Z_1, are labelled D_{IO}, D_{OZ}, D_{OZ_1}, D_{ZZ_1}, D_{IZ} and D_{IZ_1}, respectively. Denote by $\mathcal{D}_1$ all the districts in these ten categories.[66] Let $\mathcal{J}_1 \equiv \{I, O, Z, Z_1, IO, OZ, OZ_1, ZZ_1, IZ, IZ_1\}$.

The central incumbent again maximizes its expected utility (1.1) subject to its budget constraint. The solution to this problem is summarized below:

Proposition 2. *Under Assumption 1, the central incumbent's maximization problem has a solution such that:*

1. *In swing districts for parties I and O, $m_i \geq 0$ and $n_i = 0$.*

2. *In safe districts for party Z and in swing districts for parties O and Z and parties Z and Z_1, $m_i = 0$ and $n_i \geq 0$.*

3. *In safe districts for parties I, O, and Z_1 and in swing districts for parties I and Z and parties O and Z_1, $m_i = n_i = 0$.*

4. *In swing districts for parties I and Z_1, $n_i = 0$ and it is ambiguous if party I buys votes pre-election.*

[66] I again assume that there are no districts where all three political parties can win elections.

The central incumbent's actions in all districts where party Z_1 never wins are the same as before. For districts that exhibit the "Patronage Cycle," the above proposition gives two additional predictions that I test in Section 1.7.1:

Predictions. *If the total budget M is large enough, actions predicted in Proposition 2 occur in the optimal solution and there are two additional testable empirical predictions about the shapes of political cycles observed in different types of districts:*

P4: Among safe districts for regional parties, districts that are safe for party Z_1 show no cycle in the release of funds while the remaining districts exhibit the "Patronage Cycle".

P5: Among swing districts for party O and regional parties, only swing districts for parties O and Z exhibit the "Patronage Cycle" and the remaining districts exhibit no political cycle in the release of funds.

Intuitively, the central incumbent only buys support from regional party Z and not from party Z_1, because party Z_1 never gives support to party I. Therefore, the "Patronage Cycle" is only driven by districts where party Z is competitive. The actions of party I in swing districts for parties I and Z_1 are ambiguous because the central incumbent will only choose to buy votes in these districts if buying vote in these districts increases party I's utility more than buying votes in swing districts for parties I and O.

Proof of Proposition 2:

1. Let $i \in D_{ZZ_1}$. In these districts $p^I_{ZZ_1}(m_i) = 0$ and $s(m_i, n_i) = 0 \; \forall \; m_i$. Suppose $\exists \; i \in D_{ZZ_1}$, such that $m_i > 0$, then party I can increase its expected utility by allocating this m_i to buy votes in $i' \in D_{IO}$. Thus, $\nexists \; i \; \in \; D_{ZZ_1}$, such that $m_i > 0$. If party Z_1 wins in district i, it will never accept party I's offer to buy support, therefore $n_i = 0$. If party Z wins, $q^Z_{ZZ_1}(n_i) > q^Z_{ZZ_1}(0) \; \forall \; n_i > 0$, therefore $n_i \geq 0$.

2. Let $i \in D_{Z_1}$. In these districts $p^I_{Z_1}(m_i) = 0$ and $s(m_i, n_i) = 0 \; \forall \; m_i$. Suppose $\exists \; i \in D_{Z_1}$, such that $m_i > 0$, then party I can increase its expected utility by allocating this m_i

to buy votes in $i' \in D_{IO}$. Thus, $\nexists\ i \in D_{Z_1}$, such that $m_i > 0$. Party Z_1 never accepts party I's offer to buy support, therefore $n_i = 0$.

Let $i \in D_{OZ_1}$. In these districts $p^I_{OZ_1}(m_i) = 0$ and $s(m_i, n_i) = 0\ \forall\ m_i$. Suppose $\exists\ i \in D_{OZ_1}$, such that $m_i > 0$, then party I can increase its expected utility by allocating this m_i to buy votes in $i' \in D_{IO}$. Thus, $\nexists\ i \in D_{OZ_1}$, such that $m_i > 0$. Parties O and Z_1 never accepts party I's offer to buy support, therefore $n_i = 0$.

Let $i \in D_{IZ_1}$. Party Z_1 never accepts party I's offer to buy support, therefore $n_i = 0$. Given that $n_i = 0$, the expected utility of party I from district i is $p^I_{IZ_1}(m_i)u + \delta^2 W s(m_i, 0)$. Party I decides to spend $m_i^* > 0$ funds in district i if it is better off spending these funds in district i compared to spending these funds in district j where $j \in D_{IO}$. Otherwise, it will choose $m_i = 0$.

Proof of Proposition 2 in districts D_I, D_O, D_Z, D_{IO}, D_{OZ}, and D_{IZ} is the same as for Proposition 1.

B Construction of the Political Variables

As an example, Figure B.1 shows the geographical and political divisions for one hypothetical state in India. The state is divided into three districts: $District_1$, $District_2$, and $District_3$. Each district is further divided into assembly constituencies and elections are held in these assembly constituencies. There are two assembly constituencies in $District_1$, AC_1^1 and AC_2^1, one assembly constituency in $District_2$, AC_1^2, and four assembly constituencies in $District_3$, AC_1^3 to AC_4^3. If party I wins in the single constituency in $District_2$ and party O wins in three constituencies in $District_3$, I define party I as the winner in $District_2$ and party O as the winner in $District_3$. If parties I and O win in one constituency each in $District_1$, I define $District_1$ as a swing district for parties I and O.

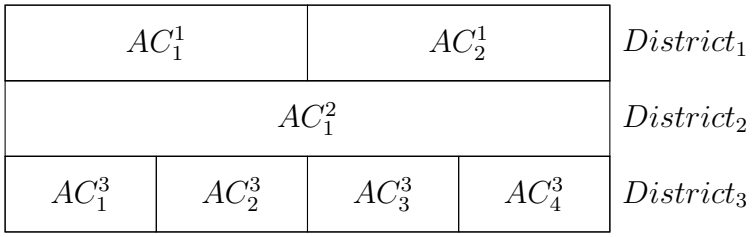


Figure B.1: Administrative and Political divisions within a state

C Robustness

C.1 Estimating Political Cycles in District Categories Defined using Contemporaneous Election Data

Because I use pre-period election data to categorize districts into six categories, I show that the shapes of political cycles observed in Section 1.6.1 are not being driven by how districts transition between categories from the pre-period and to the contemporaneous period. I use election data from 2003–2014 to divide districts into the six categories defined in Section 1.5.2.[67] I then test hypotheses (P1)-(P3) by estimating specification (1.3) separately in the six categories of districts defined using contemporaneous election data. The results for these estimations are reported in Figures C.1, C.2, and C.3, respectively. The shapes of political cycles estimated using this alternate definition of categorizing districts are very similar to those reported in Figures 1.5, 1.6, and 1.7, respectively.

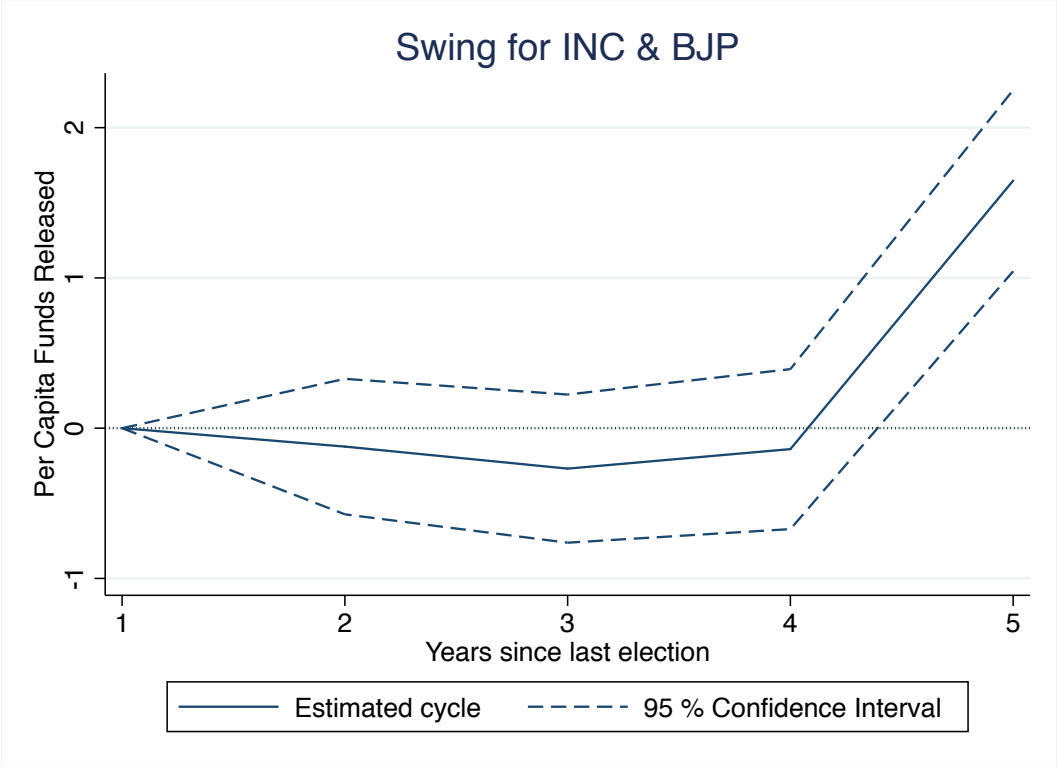
There are two differences between the cycles reported in this Section and those reported in Section 1.6.1. First, the cycle in safe districts for the BJP (see Figure C.3) now does not have any peaks in the release of funds and is consistent with hypothesis (P3). Second, there is no pre-election increase in the release of funds in swing districts for the BJP and regional parties (see Figure 1.6). These results suggest that these peaks are being driven by districts which change definition from the pre-period to the contemporaneous period. The political cycle in the release of funds in safe districts for the INC looks noisy because there are only 8 districts in this category when I use election data from 2003–2014 to classify districts into various categories.

Magnitudes of the peaks in both the "Buying Votes Cycle" and the "Patronage Cycle" are now even larger than those reported in Section 1.6.1. The "Buying Votes Cycle" now shows an increase of 111% in the release of funds over the mean per capita monthly release of

[67] I use electoral data beginning in 2003 and not 2004 to ensure that I have data on at least three state elections in every state to classify districts.

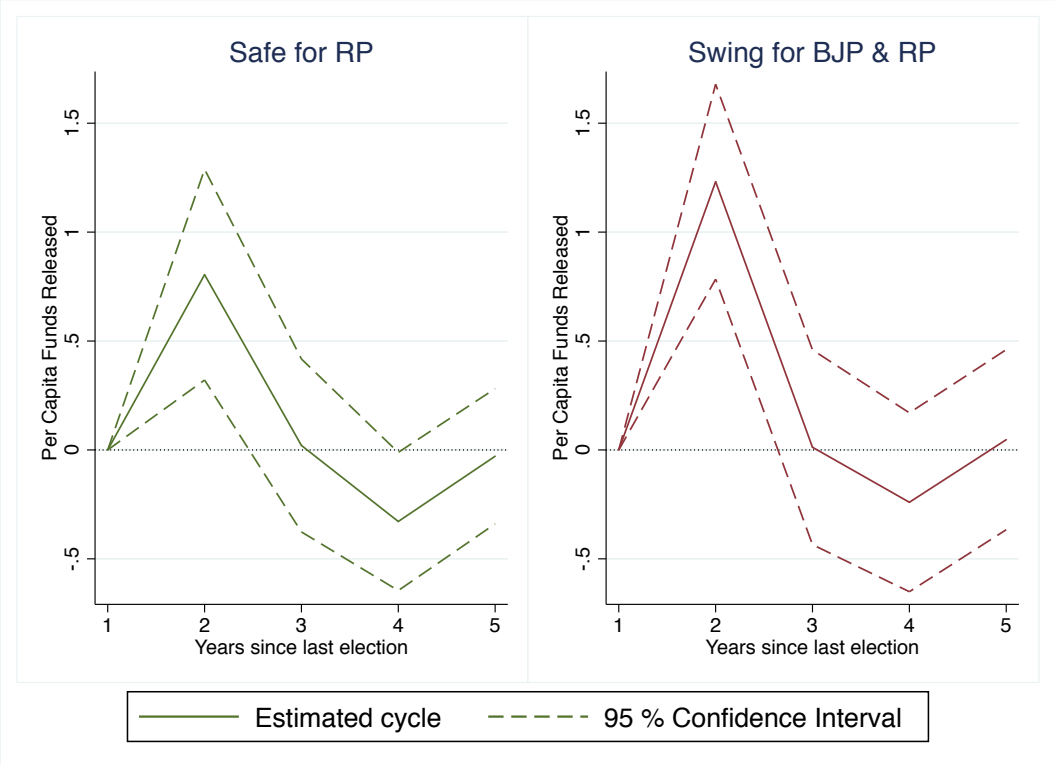
funds in swing districts for the INC and the BJP. The "Patronage Cycle" now shows a 62% and 107% increase in the release of funds over the mean per capita monthly release of funds in safe districts for regional parties and swing districts for the BJP and regional parties, respectively. Both types of cycles show larger peaks when using current data suggesting that the central incumbent only considers recent elections to determine the type of political competition in a district.

Figure C.1: Buying Votes Cycle in the Release of Funds in Swing Districts for the INC and the BJP



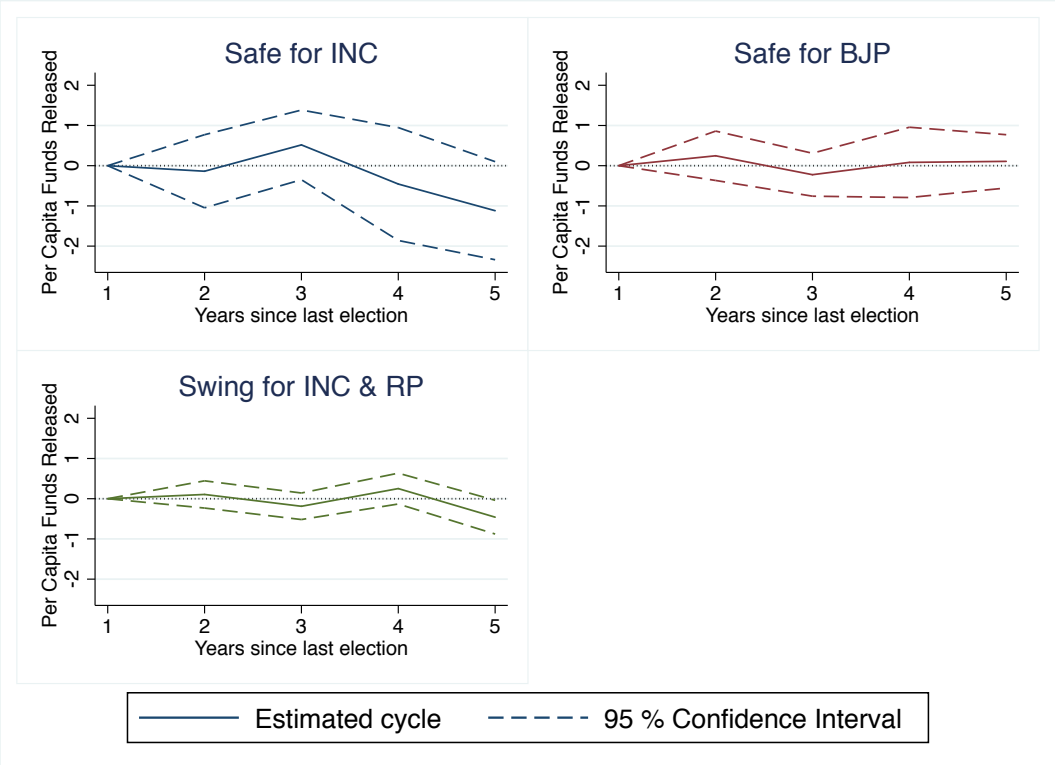
Notes: This graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. Districts that are swing for the INC and the BJP in the state elections during 2003–2014 have been labeled *Swing for INC and BJP*. The omitted category is Year(+1). Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 2003–2014.

Figure C.2: Patronage Cycle in the Release of Funds in Safe Districts for Regional Parties and Swing Districts for the BJP and Regional Parties



Notes: Each graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. Districts where a regional party won the largest proportion of assembly constituencies in all the state elections during 2003–2014 have been labeled *Safe for RP*. Districts that are swing for the BJP and regional parties in the state elections during 2003–2014 have been labeled *Swing for BJP & RP*. The omitted category is Year(+1) in all graphs. Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 2003–2014.

Figure C.3: No Political Cycles in the Release of Funds in Safe Districts for the INC and the BJP and Swing Districts for the INC and Regional Parties



Notes: Each graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. Districts where the INC and the BJP won the largest proportion of assembly constituencies in all state elections during 2003–2014 have been labeled *Safe for INC* and *Safe for BJP* respectively. Districts that are swing for the INC and regional parties in the state elections during 2003–2014 have been labeled *Swing for INC & RP*. The omitted category is Year(+1) in all graphs. Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 2003–2014.

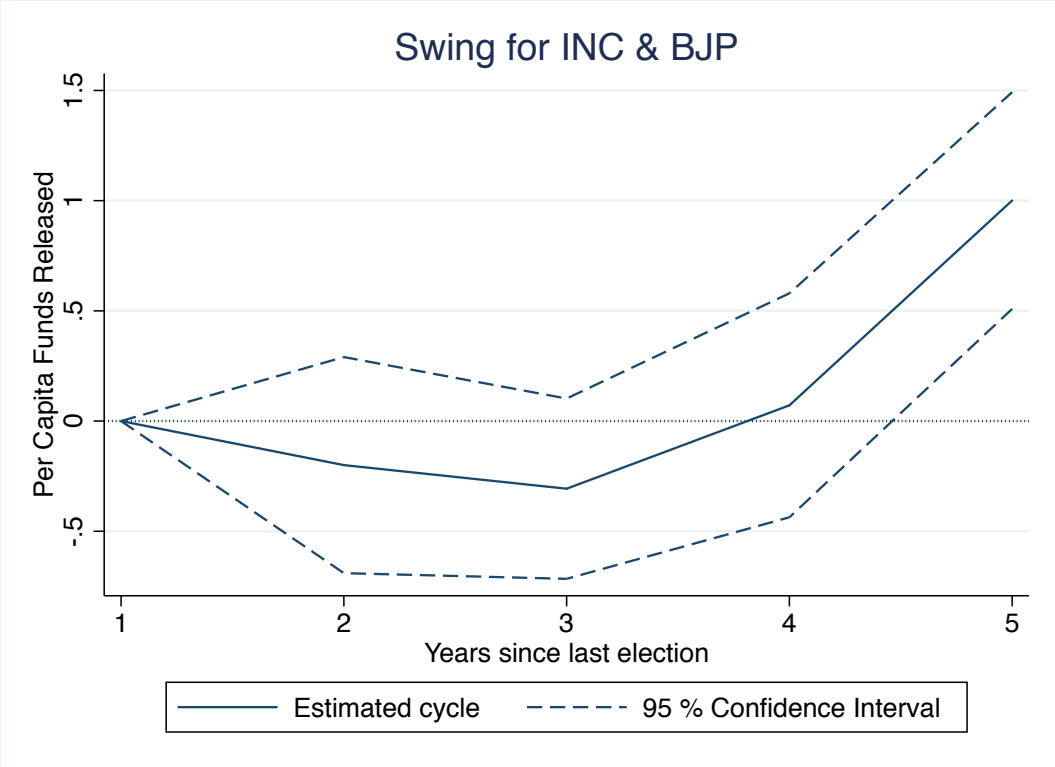
C.2 Political Cycles Using an Alternate Definition of Winning in a District

I test if my results are robust to a stricter definition of classifying a district "Safe" for a political party. Using this new definition, I classify a district as "Safe" for a political party if that party wins at least 60% of the assembly constituencies (and not just the largest proportion) in a district in every election during 1994–2004. The new definition leads to several districts that were previously classified as "Safe", as now being classified as swing districts based on the identities of political parties that win the first and second largest proportion of assembly constituencies in the district.

I test hypotheses (P1)-(P3) by estimating specification (1.3) in the six types of districts using this new definition of "Safe" districts and show that the political cycles have the same shape as Figures 1.5, 1.6, and 1.7. The only difference is that safe districts for the BJP now show no peaks in the release of funds and is consistent with hypothesis (P3). This result suggests that the peaks in safe districts for the BJP in Figure 1.7 are caused by districts that are more likely to be classified as swing using the stricter definition of "Safe" districts. The results for the estimation of the new political cycles are reported in Figures C.4, C.5 and C.6.

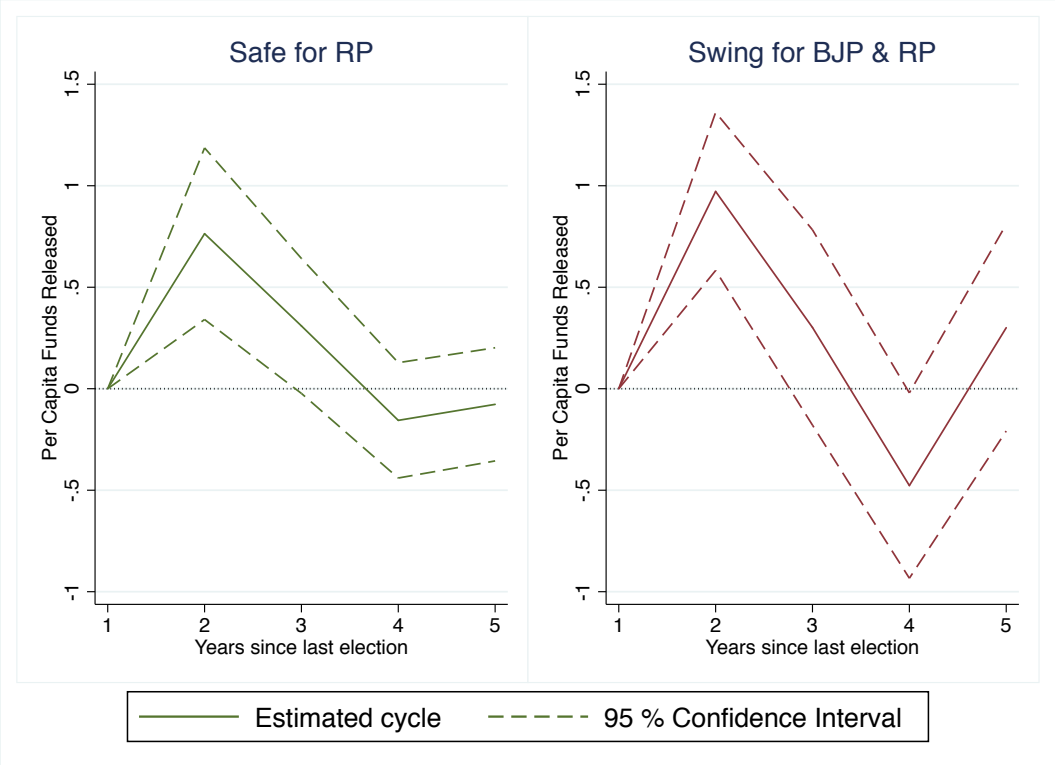
Safe districts for the INC and the BJP, and swing districts for the INC and regional parties have no political cycles in the release of funds. Swing districts for the INC and the BJP exhibit the "Buying Votes Cycle" with a 56% increase in the release of funds (over the mean monthly release of funds) in the year before an election. Safe districts for regional parties and swing districts for the BJP and regional parties exhibit the "Patronage Cycle" with 48% and 78% increases in the release of funds (over the mean monthly release of funds) in the second year after an election, respectively.

Figure C.4: Buying Votes Cycle in the Release of Funds in Swing Districts for the INC and the BJP



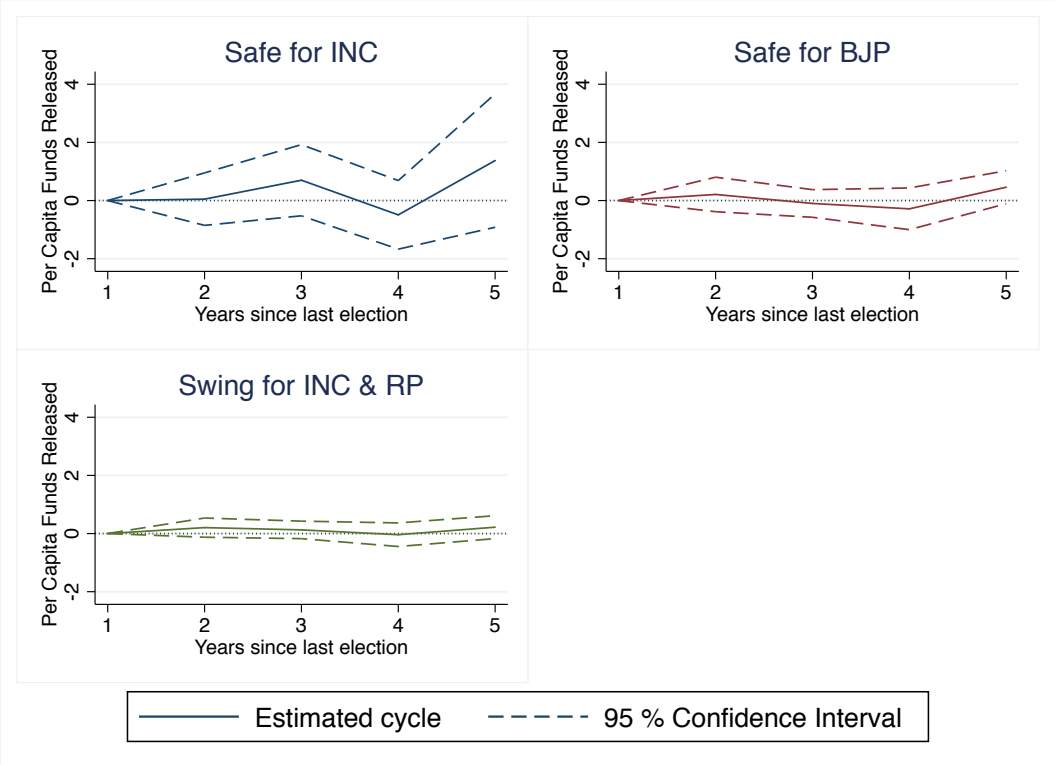
Notes: This graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. Districts that are swing for the INC and the BJP in the state elections during 1994–2004 have been labeled *Swing for INC and BJP*. The omitted category is Year(+1). Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1994–2014.

Figure C.5: Patronage Cycle in the Release of Funds in Safe Districts for Regional Parties and Swing Districts for the BJP and Regional Parties



Notes: Each graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. Districts where a regional party won the largest proportion of assembly constituencies in all the state elections during 1994–2004 have been labeled *Safe for RP*. Districts that are swing for the BJP and regional parties in the state elections during 1994–2004 have been labeled *Swing for BJP & RP*. The omitted category is Year(+1) in all graphs. Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1994–2014.

Figure C.6: No Political Cycles in the Release of Funds in Safe Districts for the INC and the BJP, and Swing Districts for the INC and Regional Parties

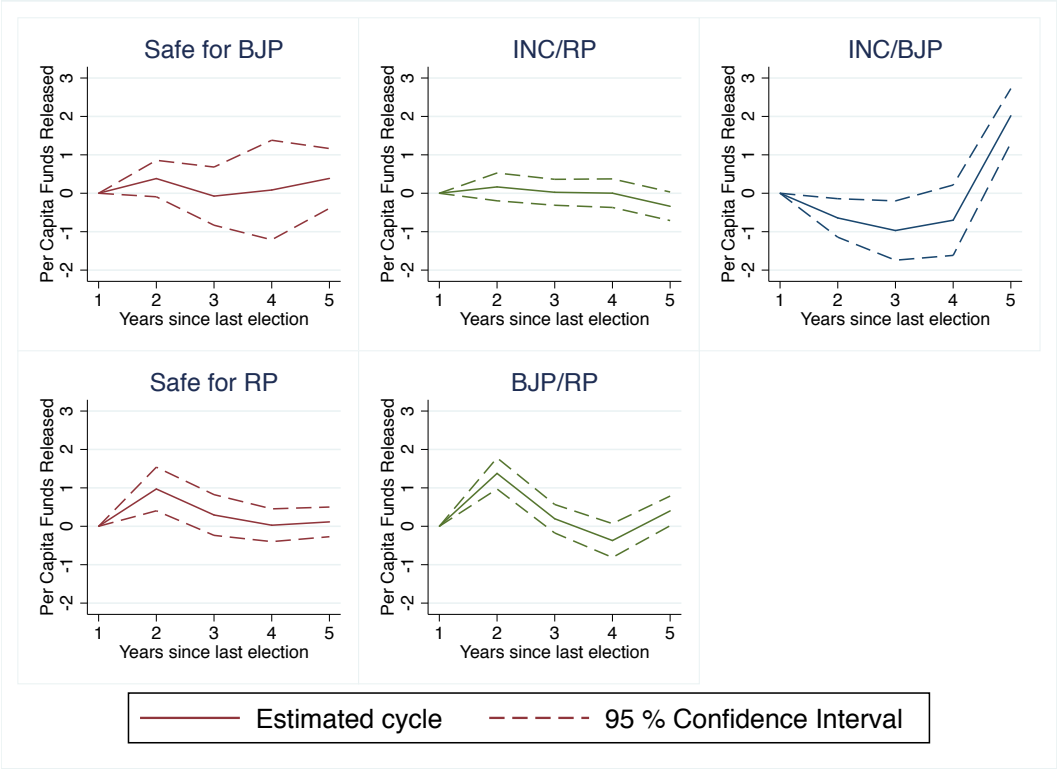


Notes: Each graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. Districts where the INC and the BJP won the largest proportion of assembly constituencies in all state elections during 1994–2004 have been labeled *Safe for INC* and *Safe for BJP* respectively. Districts that are swing for the INC and regional parties in the state elections during 1994–2004 have been labeled *Swing for INC & RP*. The omitted category is Year(+1) in all graphs. Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1994–2014.

C.3 Political Cycles Using Election Data from 1994–2014 to Categorize Districts

I show that my results are robust to categorizing districts into the six groups described in Section 1.5.2 using another definition of "Safe" districts. I use election results for the entire period (pre-period and contemporaneous period) and classify a district as "Safe" only if it has been safe for a political party for the 20 year period (1994–2004).

Figure C.7: Political Cycles in Five Types of Districts



Notes: Each graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. Districts where the BJP and regional parties won the majority of assembly constituencies in all the state elections during 1994–2014 have been labeled *Safe for BJP* and *Safe for RP*, respectively. Districts which experienced a turnover between the INC and the BJP, the INC and regional parties, and the BJP and regional parties in the state elections during 1994–2014 have been labeled *INC/BJP*, *INC/RP*, and *BJP/RP*, respectively. The omitted category is Year(+1) in all graphs. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014; political data on state assembly elections during 1994–2014.

The shapes of all cycles (see Figure C.7) look the same as those in Figures 1.5, 1.6, and 1.7, respectively. The only difference is that the political cycle in safe districts for the BJP has no peaks and is consistent with hypothesis (P3). Swing districts for the INC and the BJP exhibit the "Buying Votes Cycle" with an increase of 105% in the release of funds (over the mean monthly release of funds) in the year before an election. Safe districts for regional parties and swing districts for the BJP and regional parties exhibit the "Patronage Cycle" with 42% and 117% increases in the release of funds (over the mean monthly release of funds) after an election, respectively. I do not report the political cycle for safe districts for the INC because there are only two districts in this category when I use this new definition of a "Safe" district.

C.4 Political Cycles Using Half-Yearly Electoral Dummies

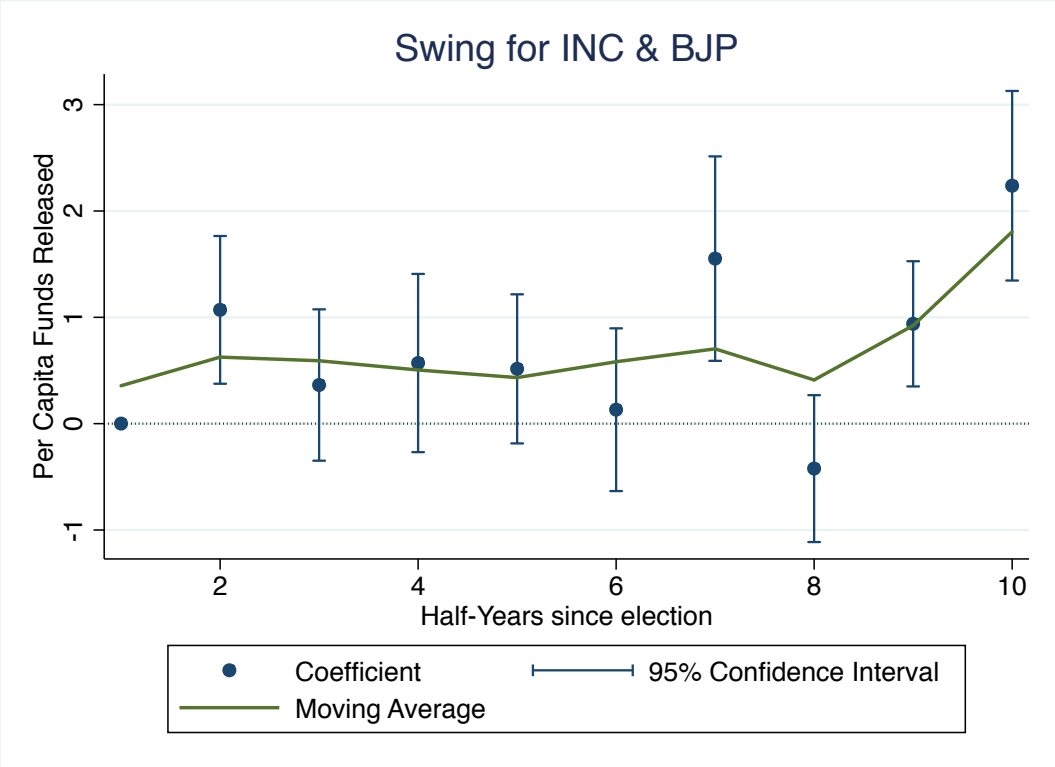
I show that the political cycles estimated using dummies for years since election are robust to using electoral dummies for shorter intervals, ruling out that the shapes of yearly cycles are a result of the way I choose to aggregate the data. In this section, I estimate political cycles using dummies for half-years (6 month intervals) since an election. In particular, I estimate the following specification separately in the six types of districts defined in Section 1.5.2:

$$Exp_{dt} = \alpha + \sum_{i=2}^{10} \beta_i HY_{(+i)dt} + \gamma X_{dt} + \delta_d + \theta_t + \epsilon_{dt} \tag{C.1}$$

where $HY_{(+i)dt}$ is a dummy that takes the value 1 if district d at time t is in the i^{th} half-year since an election. Elections occur every five years in India, therefore there are 10 half-years between two elections. $HY_{(+1)}$ is the omitted category. All other variables are the same as defined in specification (1.3).

The shapes of all six cycles (see Figures C.8, C.9, and C.10) look very similar to the ones reported in Figures 1.5, 1.6, and 1.7, respectively. Swing districts for the INC and the

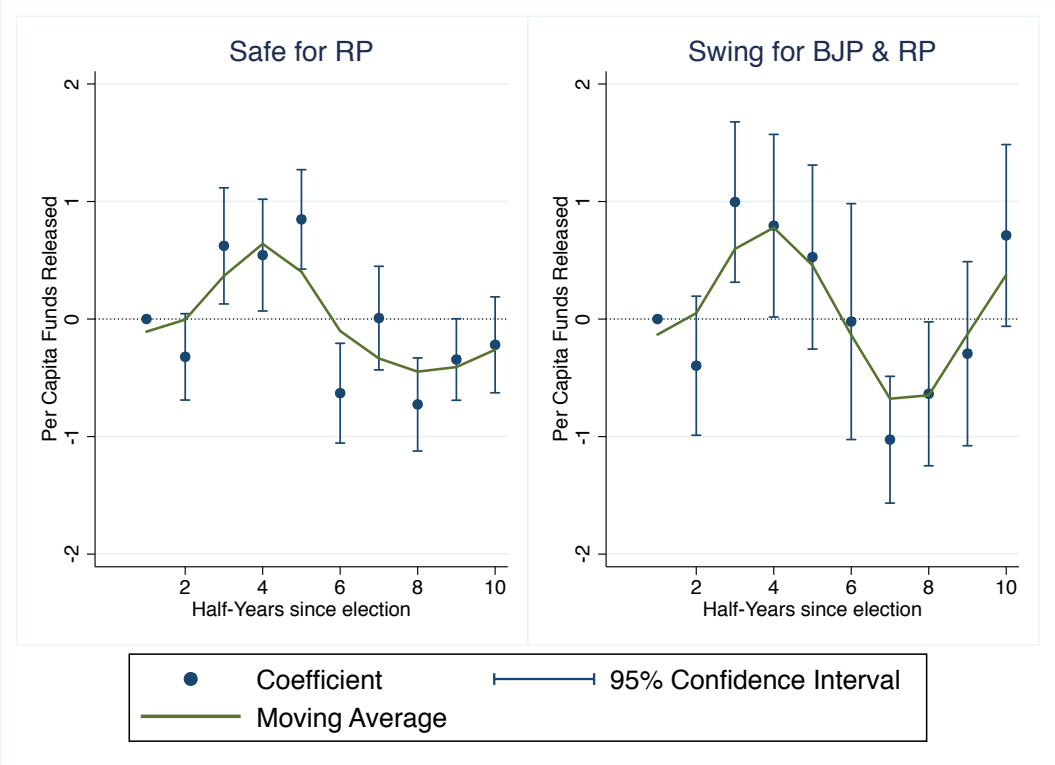
Figure C.8: Buying Votes Cycle in the Release of Funds in Swing Districts for the INC and the BJP



Notes: This graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time fixed effects. Districts that experienced a turnover between the INC and the BJP in state elections during 1994–2004 have been labeled *Swing for INC & BJP*. The omitted category is HY(+1). The green line shows the weighted moving average of the coefficients using 1 as the weight for one lagged and one forward term, and using 2 as the weight for the current term. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1994–2014.

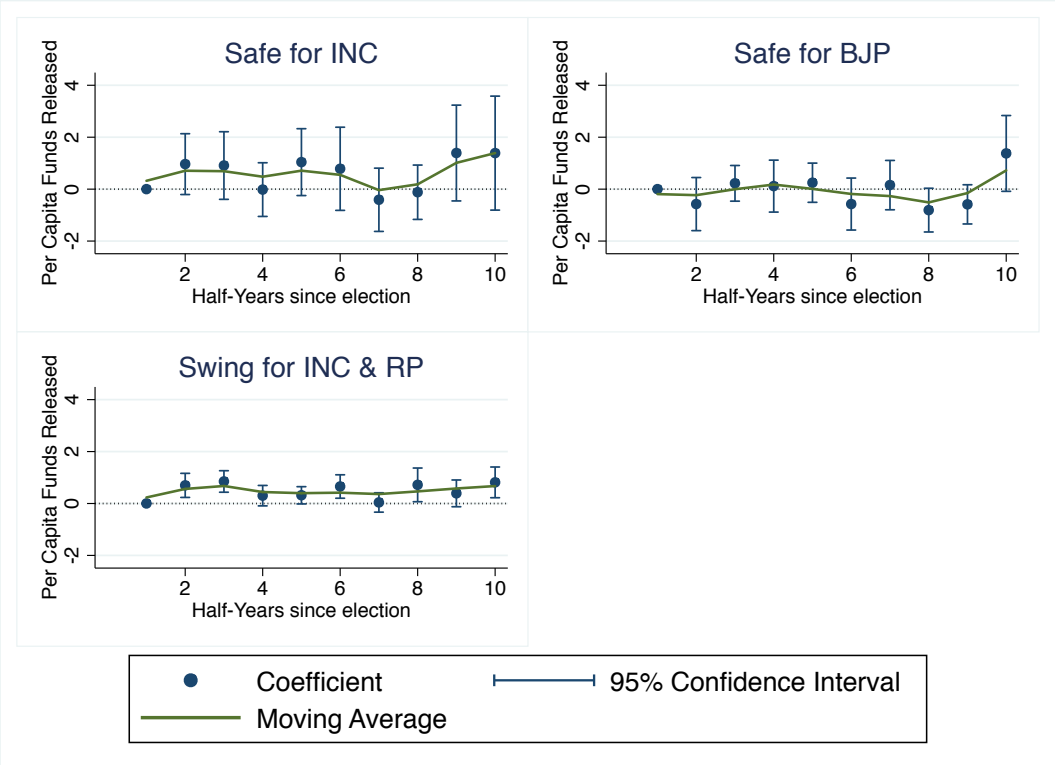
BJP exhibit the "Buying Votes Cycle" with an increase in the release of funds in the last year before an election and especially in the last half-year before an election (an increase of 133% over the mean monthly release of funds). Safe districts for regional parties and swing districts for the BJP and regional parties exhibit the "Patronage Cycle" with an increase in the release of funds in the third, fourth, and fifth half-years. These increases are between 39-61% in safe districts for regional parties and between 56-70% in swing districts for the BJP and regional parties.

Figure C.9: Patronage cycle in the release of funds in Safe Districts for Regional Parties and Swing Districts for BJP and Regional Parties



Notes: Each graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time fixed effects. Districts where a regional party won the majority of assembly constituencies in all state elections during 1994–2004 have been labeled *Safe for RP*. Districts that experienced a turnover between the BJP and regional parties in the state elections during this period have been labeled *Swing for BJP & RP*. The omitted category is HY(+1) in all graphs. The green line shows the weighted moving average of the coefficients using 1 as the weight for one lagged and one forward term, and using 2 as the weight for the current term. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1994–2014.

Figure C.10: No Political Cycles in the Release of Funds in Safe Districts for the INC and the BJP, and Swing Districts for the INC and Regional Parties



Notes: Each graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time fixed effects. Districts where INC and BJP won the majority of assembly constituencies in all the state elections during 1994–2004 have been labeled *Safe for INC* and *Safe for BJP*, respectively. Districts that experienced a turnover between the INC and regional parties in state elections during 1994–2004 have been labeled *Swing for INC & RP*. The omitted category is HY(+1) in all graphs. The green line shows the weighted moving average of the coefficients using 1 as the weight for one lagged and one forward term, and using 2 as the weight for the current term. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1994–2014.

C.5 Political Cycles Using Data Aggregated to Half-Years

The shapes of political cycles are not driven by estimating yearly political cycles using monthly frequency release of funds data. My preferred specification uses monthly frequency release of funds data and estimates political cycles using yearly electoral cycle dummies. Using monthly frequency data leads to better identification of political cycles as I can exploit the differences in specific timing of elections across districts. Using yearly electoral cycle dummies captures the institutional feature of the TSC where each district receives only one or two transfers in a given year; in most months a district does not receive any funds. To deal with the concern that choice of aggregation drives the shapes of political cycles, aggregating the data to half-yearly level and estimating half-yearly political cycles.

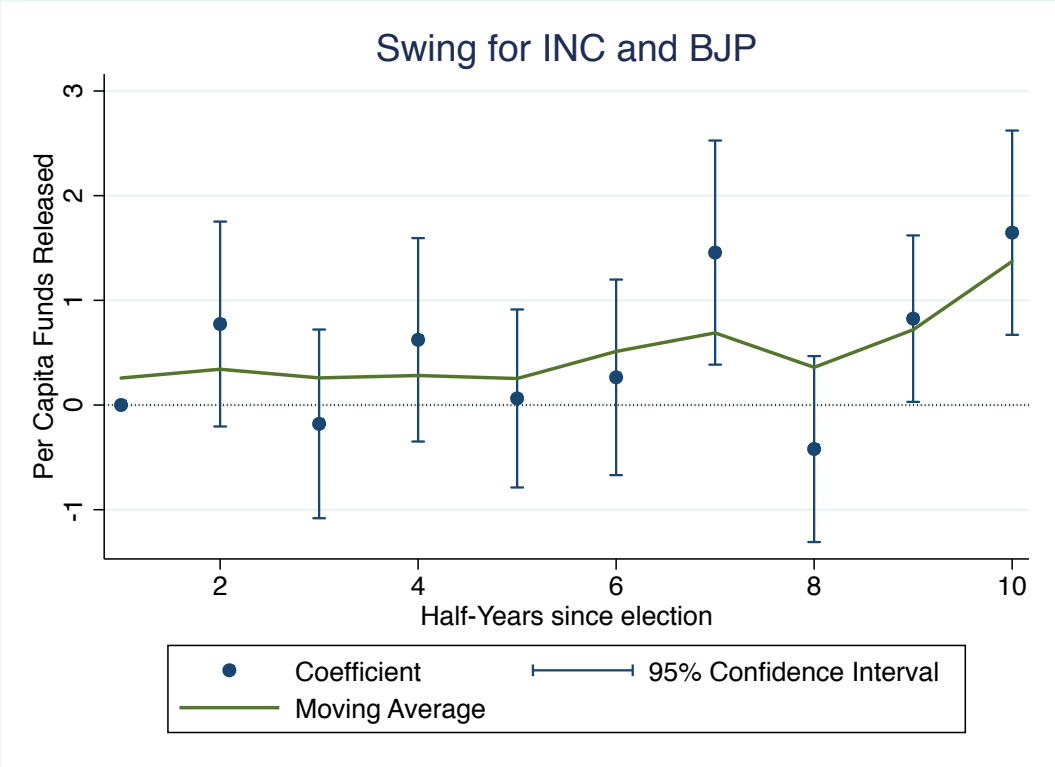
To test hypotheses (P1)-(P3), I estimate the following model separately in the six types of districts defined in Section 1.5.2:

$$Exp_{dy} = \alpha + \sum_{i=2}^{10} \beta_i HY_{(+i)dy} + \gamma X_{dy} + \delta_d + \theta_y + \epsilon_{dy} \tag{C.2}$$

where Exp_{dy} is the average per capita expenditure in district d in the half-year y, X_{dy} is a vector of time varying controls: ratio of assembly constituencies reserved for SCs and ratio of assembly constituencies reserved for STs in district d at the beginning of the half-year y, δ_d and θ_y are district and half-year fixed effects respectively, and $HY_{(+i)dy}$ is a dummy that takes the value 1 if district d in the first month of half-year y is in the i^{th} half-year after an election. $HY_{(+1)}$ is the omitted category. The coefficients of interest are $\beta_i s$.

All three types of political cycles are robust to using the above estimation. Swing districts for the INC and the BJP exhibit the "Buying Votes Cycle" (see Figure C.11) with an increase of 68% and 132% of the mean monthly release of funds in the second-last and last half-year before an election, respectively. Safe districts for regional parties and swing districts for the BJP and regional parties show the "Patronage cycle" (see Figure C.12) with peaks in the third, fourth, and fifth half-years after an election. These peaks correspond to the second and

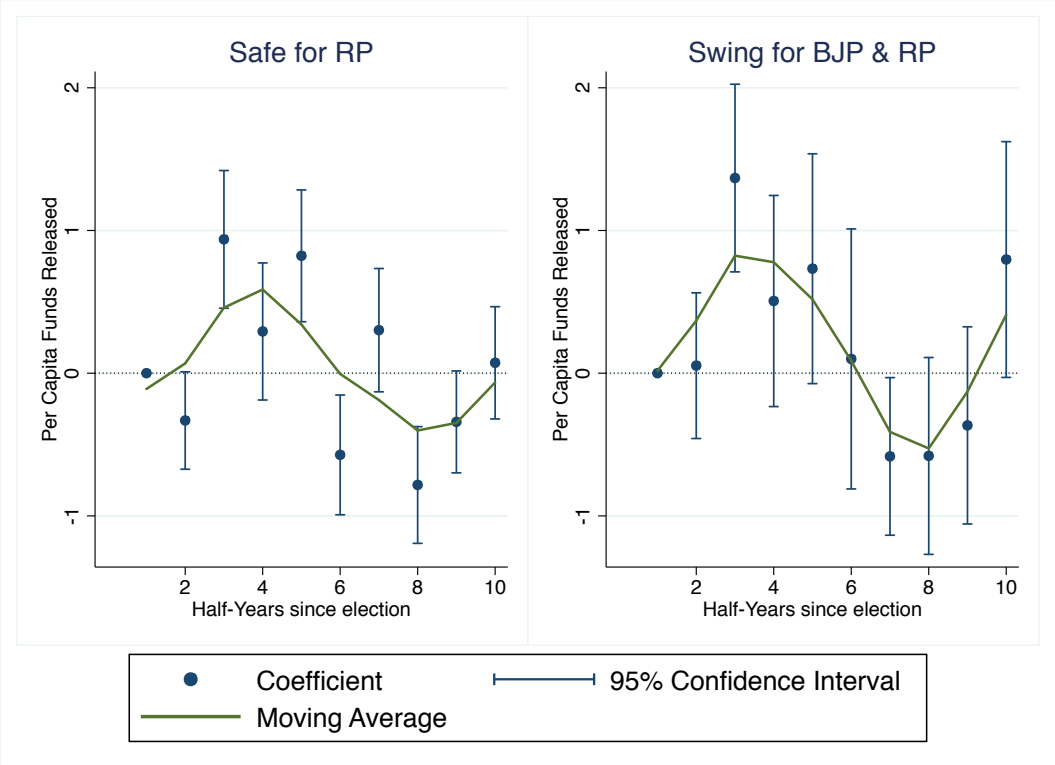
Figure C.11: Buying Votes Cycle in the Release of Funds in Swing Districts for the INC and the BJP



Notes: This graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. Districts that are swing for the INC and the BJP in the state elections during 1994–2004 have been labeled *Swing for INC and BJP*. The omitted category is Year(+1). Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1994–2014.

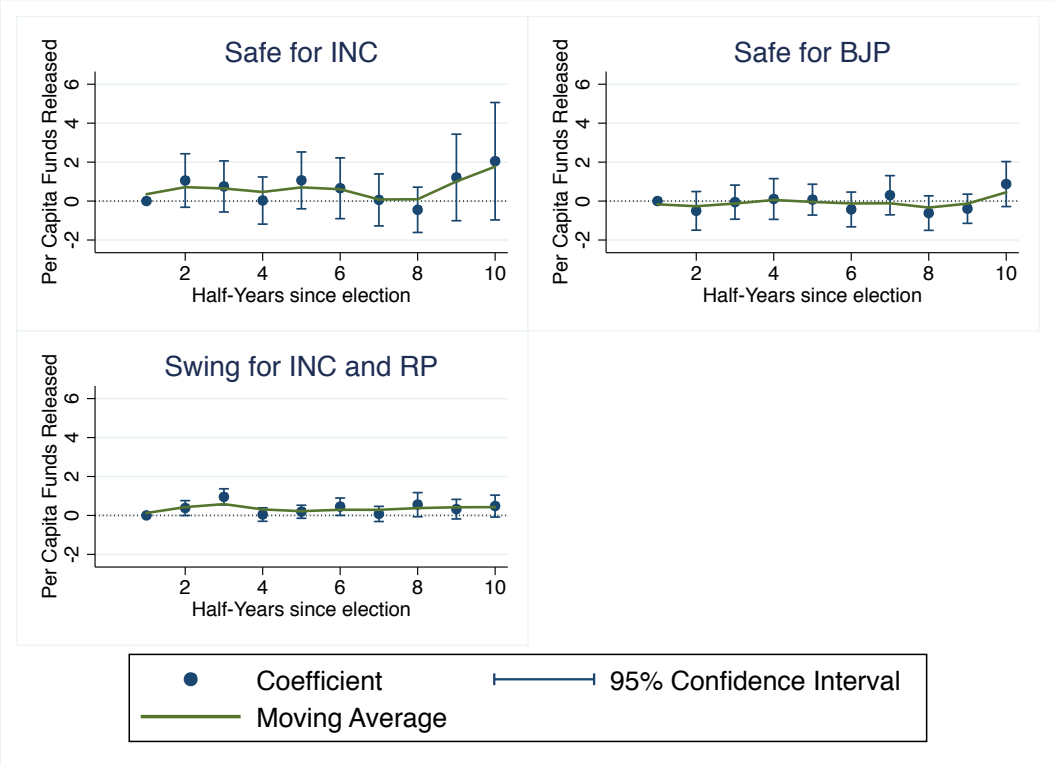
third years after an election. The magnitude of the peaks in safe districts for regional parties and swing districts for the BJP and regional parties are 58-70% and between 53%-107% of the mean monthly release of funds, respectively. Safe districts for the INC and the BJP show no political cycles in the release of funds of an electoral cycle (see Figure C.13). Swing districts for the INC and regional parties show an increase in the second and third half-years since election. However, this peak is not robust to using other specifications. The shapes of all political cycles are also robust to aggregating the data to years instead of half-years.

Figure C.12: Patronage Cycle in the Release of Funds in Safe Districts for Regional Parties and Swing Districts for the BJP and Regional Parties



Notes: Each graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. Districts where a regional party won the largest proportion of assembly constituencies in all the state elections during 1994–2004 have been labeled *Safe for RP*. Districts that are swing for the BJP and regional parties in the state elections during 1994–2004 have been labeled *Swing for BJP & RP*. The omitted category is Year(+1) in all graphs. Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1994–2014.

Figure C.13: No Political Cycles in the Release of Funds in Safe districts for the INC and the BJP, and Swing Districts for the INC and Regional Parties



Notes: Each graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. Districts where the INC and the BJP won the largest proportion of assembly constituencies in all state elections during 1994–2004 have been labeled *Safe for INC* and *Safe for BJP* respectively. Districts that are swing for the INC and regional parties in the state elections during 1994–2004 have been labeled *Swing for INC & RP*. The omitted category is Year(+1) in all graphs. Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1994–2014.

C.6 Political Cycles Including District-Specific Time Trends

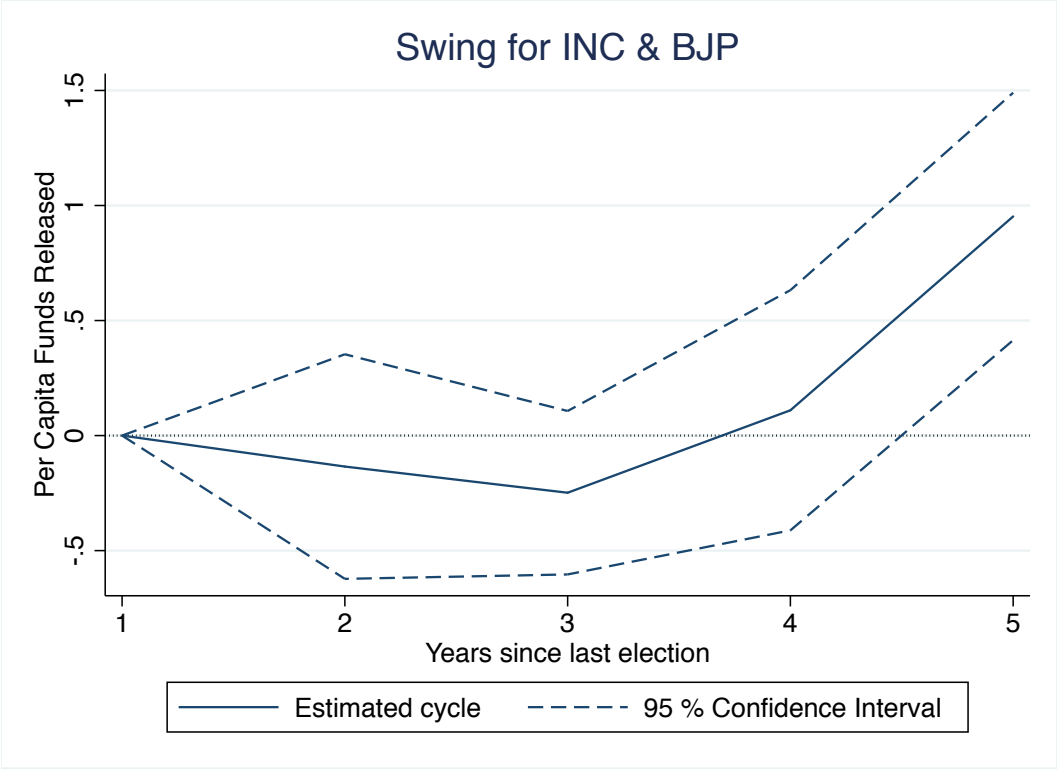
My results are robust to including district-specific linear time trends. To show this I estimate the following model separately in the six categories of districts defined in Section 1.6.1:

$$Exp_{dt} = \alpha + \sum_{i=2}^{10} \beta_i Year_{(+i)dt} + \gamma X_{dt} + \delta_d + \theta_t + \delta_d * t + \epsilon_{dt} \tag{C.3}$$

where $\delta_d * t$ is the district-specific linear time trend and the rest of the variables are the same as in model (1.3). The results of these estimations are reported in Figures C.14, C.15, and C.16. All cycles have the same shape as the ones in Figures 1.5, 1.6, and 1.7. The only difference is that the political cycle in safe districts for the BJP has no peaks and is consistent with hypothesis (P3).

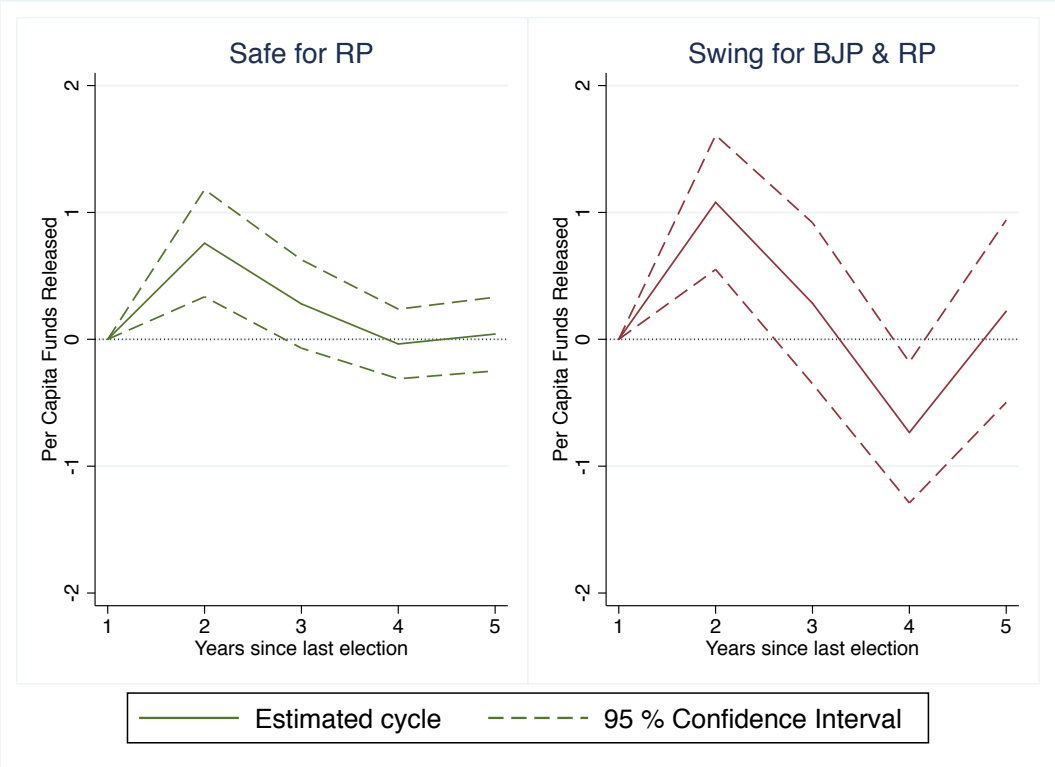
The magnitude of the pre-election peak in swing districts for the INC and the BJP is 56% of the average monthly release of funds. The magnitudes of the post-election peaks in safe districts for regional parties and swing districts for the BJP and regional parties are 55% and 76% of the average monthly release of funds, respectively. Safe districts for the INC and the BJP, and swing districts for the BJP and regional parties exhibit no cycles in the release of funds.

Figure C.14: Buying Votes Cycle in the Release of Funds in Swing Districts for the INC and the BJP



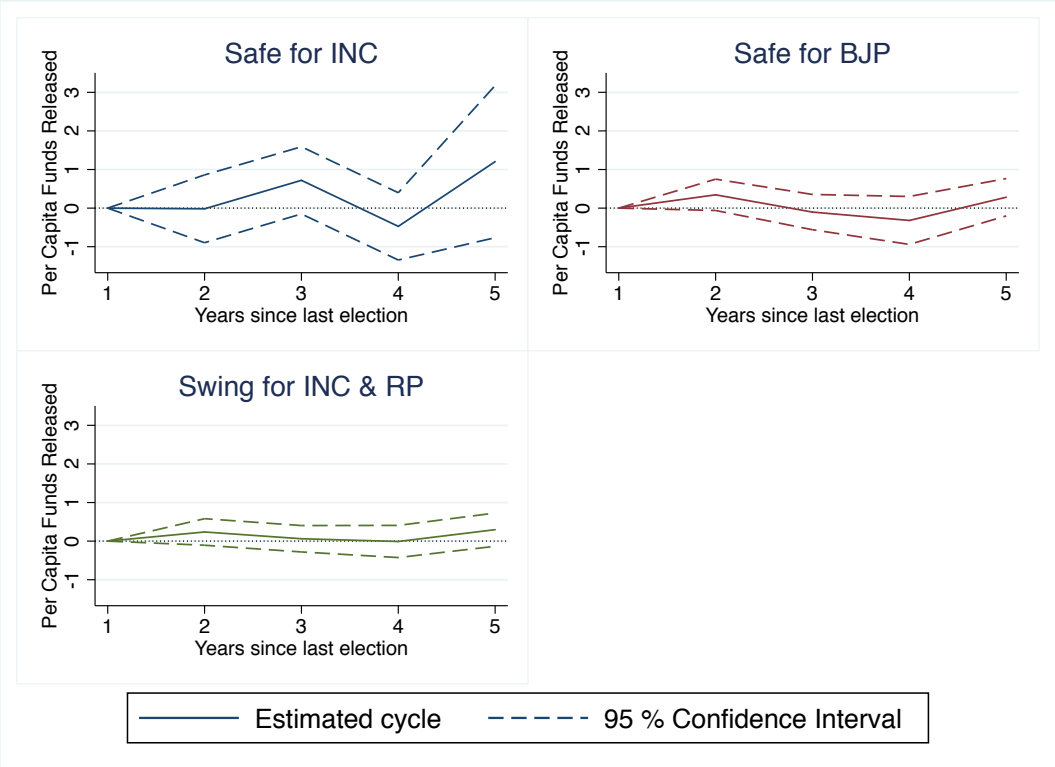
Notes: This graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, district-specific linear time trend, and district and time (year-month) fixed effects. Districts that are swing for the INC and the BJP in the state elections during 1994–2004 have been labeled *Swing for INC and BJP*. The omitted category is Year(+1). Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1994–2014.

Figure C.15: Patronage Cycle in the Release of Funds in Safe Districts for Regional Parties and Swing Districts for the BJP and Regional Parties



Notes: Each graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, district-specific linear time trends, and district and time (year-month) fixed effects. Districts where a regional party won the largest proportion of assembly constituencies in all the state elections during 1994–2004 have been labeled *Safe for RP*. Districts that are swing for the BJP and regional parties in the state elections during 1994–2004 have been labeled *Swing for BJP & RP*. The omitted category is Year(+1) in all graphs. Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1994–2014.

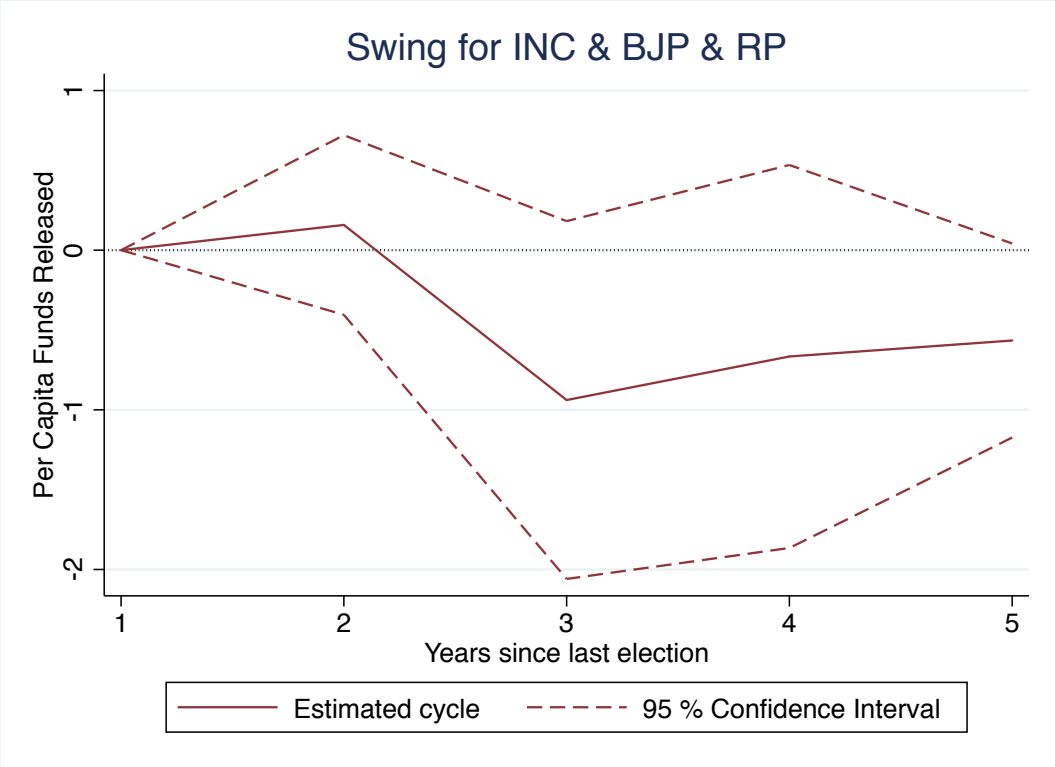
Figure C.16: No Political Cycles in the Release of Funds in Safe districts for the INC and the BJP, and Swing Districts for the INC and Regional Parties



Notes: Each graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. Districts where the INC and the BJP won the largest proportion of assembly constituencies in all state elections during 1994–2004 have been labeled *Safe for INC* and *Safe for BJP* respectively. Districts that are swing for the INC and regional parties in the state elections during 1994–2004 have been labeled *Swing for INC & RP*. The omitted category is Year(+1) in all graphs. Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1994–2014.

D Supplementary Figures and Tables

Figure D.1: Political cycle in the Release of Funds in Swing Districts for All Three Political Parties



Notes: This graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. Districts which experienced a turnover between the INC, the BJP, and regional parties in the state elections during 1994–2004 have been labeled *Swing for INC & BJP & RP*. The omitted category is Year(+1) in all graphs. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1994–2014.

Table D.1: Political Cycles in Districts with Different Political Competition

	District Categories					
	Swing for INC & BJP	Safe for RP	Swing for BJP & RP	Safe for INC	Safe for BJP	Swing for INC & RP
	(1)	(2)	(3)	(4)	(5)	(6)
Year(+2)	-0.195	0.763*	1.067***	-0.050	0.428	0.229
	(0.397)	(0.427)	(0.381)	(0.431)	(0.291)	(0.283)
Year(+3)	-0.300	0.309	0.370	0.423	0.160	0.143
	(0.424)	(0.318)	(0.389)	(0.483)	(0.337)	(0.282)
Year(+4)	0.080	-0.156	-0.672**	-0.751	-0.018	0.025
	(0.477)	(0.325)	(0.339)	(0.498)	(0.433)	(0.429)
Year(+5)	1.015*	-0.078	0.303	0.901	0.577	0.233
	(0.594)	(0.305)	(0.435)	(0.891)	(0.539)	(0.402)
Observations	9,941	13,308	7,004	2,512	5,368	10,296
R-squared	0.204	0.160	0.146	0.127	0.173	0.110
District and Time FE	Y	Y	Y	Y	Y	Y
Mean	1.710	1.347	1.388	1.631	1.353	1.040
Std. Dev	7.803	6.407	6.785	8.639	6.458	5.858
District Clusters	87	118	62	22	47	90
State-Year Clusters	121	110	99	121	88	143

Notes: The unit of observation is a district-month. The dependent variable is the monthly per capita expenditure for the TSC in a district. Each column reports the results for estimating model (1.3) in one of six categories of districts. Column (1) reports the estimated political cycle for districts that are swing for INC and BJP. Columns (2) and (3) report the estimated political cycles for districts that are safe for regional parties, and districts that are swing for BJP and regional parties, respectively. Columns (4)-(6) report the estimated political cycles for districts that are safe for INC, safe for BJP, and that are swing for INC and regional parties, respectively. The omitted category is Year(+1) in each regression. All regressions control for the ratio of ACs in a district reserved for SCs and STs, and district and time fixed effects. Robust standard errors clustered at district and state-year levels are reported in the parentheses. Datasets used in this analysis: monthly release of funds under TSC during the years 2004-2014 and political data on state assembly elections during 1994-2014. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table D.2: Timing of the Peak in the Patronage Cycle

	Safe for RP		Swing for BJP and RP	
	(1)	(2)	(3)	(4)
Fiscal		0.886***		0.934**
		(0.260)		(0.353)
Year(+2)	0.787***	0.163	1.128***	0.473*
	(0.216)	(0.264)	(0.265)	(0.264)
Year(+3)	0.305*	0.260	0.383	0.252
	(0.167)	(0.169)	(0.317)	(0.332)
Year(+4)	-0.152	-0.065	-0.659**	-0.608**
	(0.144)	(0.143)	(0.255)	(0.263)
Year(+5)	-0.107	-0.077	0.314	0.304
	(0.144)	(0.143)	(0.333)	(0.328)
Observations	13,072	13,072	6,880	6,880
R-squared	0.161	0.162	0.146	0.146
District and Time FE	Y	Y	Y	Y
Mean	1.390	1.390	1.390	1.390
Std. Dev	6.513	6.513	6.513	6.513
Clusters	118	118	62	62

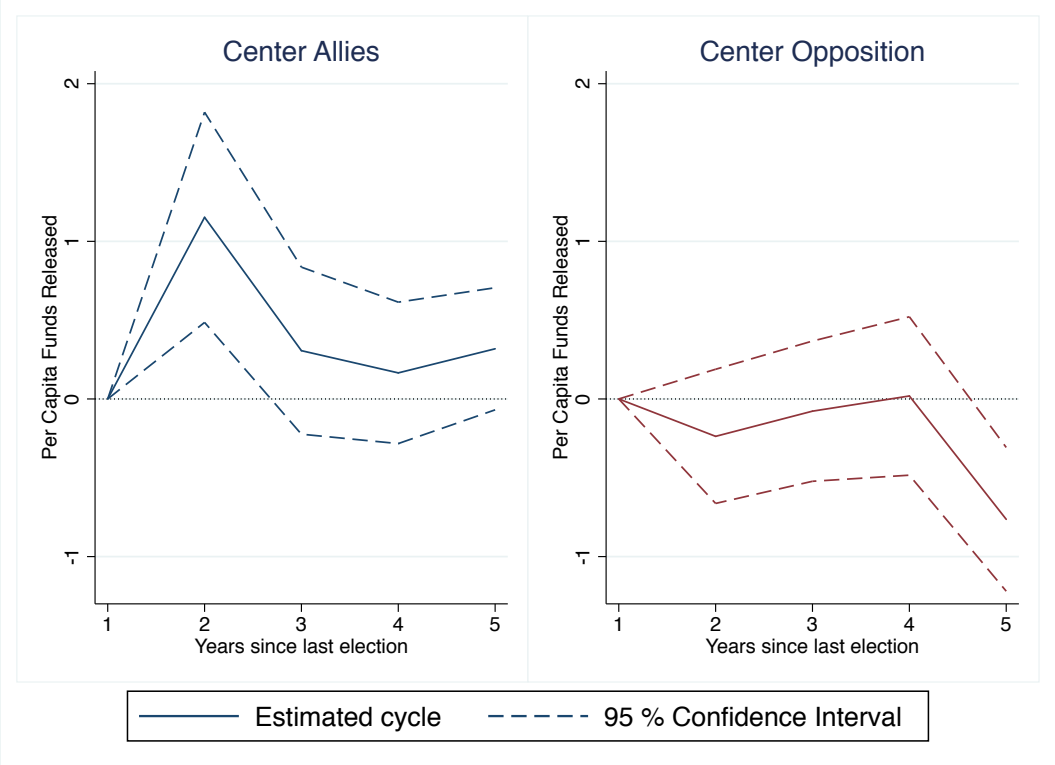
Notes: The unit of observation is a district-month. The dependent variable is the monthly per capita expenditure under TSC in a district. The *Fiscal* dummy now takes the value 1 in the first 12 months after an election. The sample for columns (1) and (2) are safe districts for regional parties. The sample for columns (3) and (4) are swing districts for the BJP and regional parties. Columns (1) and (3) report the estimation of model (1.4) without including the *Fiscal* dummy. Columns (2) and (4) report the estimation of model (1.4) including the *Fiscal* dummy. The omitted category is Year(+1) in each regression. All regressions control for the ratio of ACs in a district reserved for SCs and STs, and district and time fixed effects. Robust standard errors clustered at district level are reported in the parentheses. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1994–2014.
*** p<0.01, ** p<0.05, * p<0.1

E Supplementary Analysis

E.1 Magnitude of the Patronage Cycle in Swing Districts for the BJP and Regional Parties

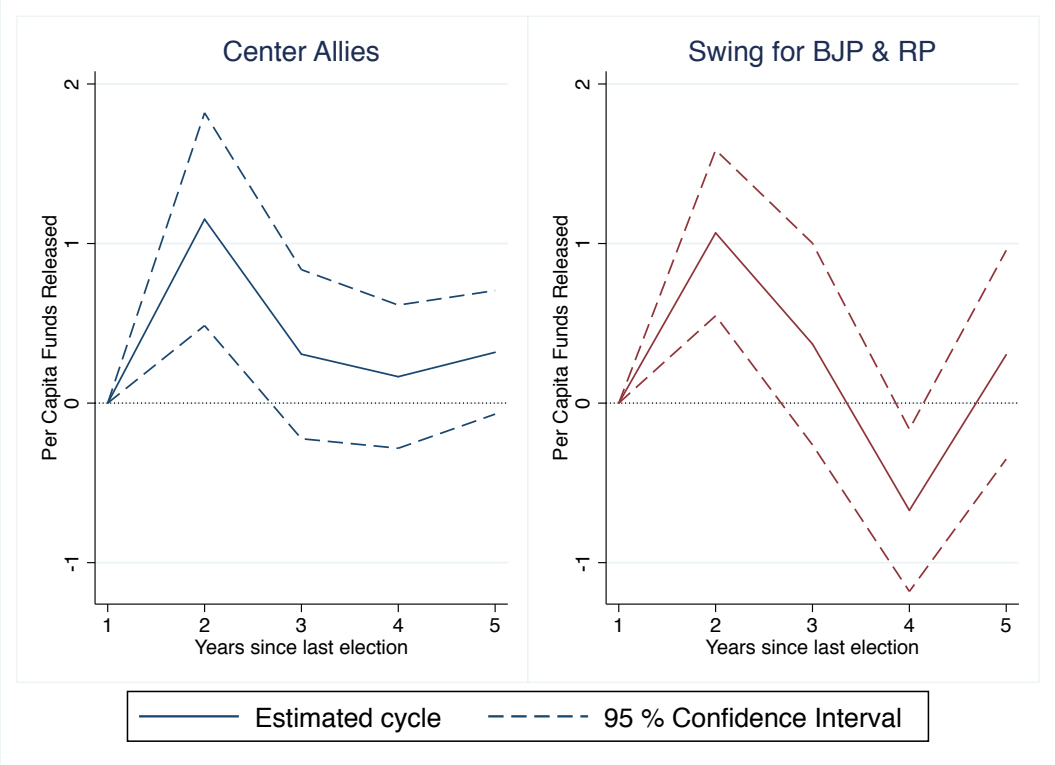
The magnitude of the peak in the "Patronage cycle" is much larger in swing districts for the BJP and regional parties compared to safe districts for regional parties (see Figure 1.6). This difference in the magnitude of peaks is coming from the difference in the composition of regional parties in the two categories of districts. Specifically, most of the regional parties that won in swing districts for the BJP and regional parties were allies of the central incumbent: the BSP, the SP, the NCP, and the RJD won around 76% of the districts won by regional parties in these districts. In contrast, regional parties that won in safe districts for regional parties were a combination of allies and opponents of the central incumbent. Thus, the "Patronage cycle" observed in safe districts for regional parties is a combination of the cycles for Allies and Opponents (see Figure E.1) and is smaller in magnitude compared to the one observed in swing districts for the BJP and regional parties that is driven by allies of the central incumbent. As further proof of this argument, the "Patronage cycle" in swing districts for the BJP and regional parties looks similar in magnitude to the one observed in safe districts for regional parties where allies of the central incumbent win (see Figure E.2).

Figure E.1: Political Cycles in the Release of Funds in Safe Districts for Regional Parties by Alliance with Central Incumbent



Notes: Each graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. These graphs have been plotted using coefficients in columns (1) and (2) of Table 1.5. The left and right graphs show political cycles for districts that are safe for important allies of the central incumbent and districts that are safe for the rest of the regional parties, respectively. Districts labeled *Center Allies* are districts in Bihar, Tamil Nadu, Uttar Pradesh, and West Bengal where regional parties always win. Districts labeled *Center Opposition* are the rest of the safe districts for regional parties. The omitted category is Year(+1) in both graphs. Elections occur just before the start of the cycle and at the end of the cycle. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1994–2014.

Figure E.2: Patronage cycle in the release of funds in Districts Safe for Allies of the Central Incumbent and Swing Districts for BJP and Regional Parties)



Notes: Each graph plots the average monthly release of funds in a district during an electoral cycle controlling for time-varying district characteristics, and district and time (year-month) fixed effects. Districts in Bihar, Jharkhand, Tamil Nadu, UP, and West Bengal where a regional party won the majority of assembly constituencies in all the state elections during this period have been labeled *Allies of Center*. Districts which experienced a turnover between the BJP and regional parties in the state elections during this period have been labeled *Swing for BJP & RP*. The graph on the right is the same one as in Figure 1.6 and the graph on the left is the same one as in Figure E.1. The omitted category is Year(+1) in all graphs. Datasets used in this analysis: monthly release of funds for the TSC during 2004–2014 and political data on state assembly elections during 1994–2014.

E.2 Evidence From IHDS Household Surveys

Using IHDS-2 (2011-12) Survey, I test if the probability of households in districts with the "Buying Votes Cycle" receiving funds under the TSC is higher than the probability of households in districts with the "Patronage Cycle" receiving funds under the TSC. I restrict the sample to only the three types of districts that have either the "Buying Votes Cycle" or the "Patronage Cycle" and estimate the following regression:

$$y_{hds} = \alpha + \beta INC/BJP_d + \theta X_{hds} + \delta_s + \epsilon_{hds} \tag{E.1}$$

where y_{htds} is an indicator variable for household h in district d and state s receiving government funds to build a toilet, INC/BJP_d is a dummy for district d being a swing district for the INC and the BJP, X_{hds} includes household level socio-economic controls,[68] δ_s are state fixed effects.

Households in districts with the "Buying Votes Cycle" are 10.1 percentage points more likely to say they received funds to build a toilet compared to households in districts with the "Patronage Cycle" (see Column (1) in Table E.1). To refute that the above result is only being driven by greater demand for funds in swing districts for the INC and the BJP due to lack of sanitation facilities compared to districts with the "Patronage Cycle," I restrict the sample to only households that did not have a toilet in the first round of the survey and repeat the above analysis. I find that households in districts with the "Buying Votes Cycle" are still 9.1 percentage points more likely to say they received funds to build a toilet compared to households in districts with the "Patronage Cycle" (see Column (2) in Table E.1).

In the TSC administrative data on allocation of funds, swing districts for the INC and the BJP receive the most funds on average and from household surveys I find that households

[68] X_{hds} includes controls for caste, religion, household assets, household size, percentage of household members who are adults, BPL status, size of land owned, per capita income, whether the household knows anyone in the panchayat, main income source, percentage of female household members, and sex of household head.

in these districts are also most likely to say that they received government funds to build toilets. Additionally, among districts with the "Buying Votes Cycle" and the "Patronage Cycle" households in the former districts are more likely to receive funds to build toilets, also suggesting that funds in swing districts for the INC and the BJP are more likely to actually reach households and not just go to local politicians.

Table E.1: Results for Cross-Sectional Allocation of Funds Across Households in Safe Districts for Regional Parties and Swing Districts for the INC and the BJP

	All households	Households with no toilet in Round 1
	(1)	(2)
Swing for INC & BJP	0.101***	0.092***
	(0.021)	(0.020)
BPL	0.012	0.010
	(0.008)	(0.008)
Panchayat	0.014*	0.016*
	(0.007)	(0.008)
Observations	14,248	11,027
R-squared	0.048	0.054
State FE	Y	Y
Mean	0.0713	0.0714

Notes: The unit of observation is a household. The dependent variable is an indicator for whether or not the household received funds from the government to build a latrine. Column (1) reports the results of estimation of specification (E.1) for all households. Columns (2) reports the results of estimation of specification (E.1) only for households that reported not having a latrine in the previous round of the IHDS survey. The omitted category in both regressions is districts safe for regional parties. Both regressions include controls for caste, religion, household assets, household size, percentage of household members who are adults, BPL status, size of land owned, per capita income, whether the household knows anyone in the panchayat, main income source, percentage of female household members, and sex of household head. Standard errors are clustered by district. Datasets used in this analysis: IHDS (2011-12) and political data on state assembly elections during 1994-2003. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Chapter 2

Lifecycle Fertility and the Impact of Family Planning Programs: Evidence from Southern Africa

with Nikhil Gupta

2.1 Introduction

Policymakers and scholars have long been concerned with fertility declines in developing countries given the potentially welfare-improving behavioral changes associated with fertility reduction — e.g., increases in women's labor force participation (Bloom et al. (2009), Bailey (2006)) and human capital accumulation (Goldin and Katz (2002)) — and the potential for a positive feedback loop between fertility declines and macroeconomic growth (Galor and Weil (1996), Galor (2005)). However, the importance of family planning programs in facilitating fertility declines is contested between supply-side and demand-side proponents. The former emphasize that improved access to family planning reduces fertility (e.g., Bongaarts (1994), Bailey (2012)), while the latter emphasize that reductions in desired fertility, rather than improvements to family planning programs, drive fertility declines (e.g., Pritch-

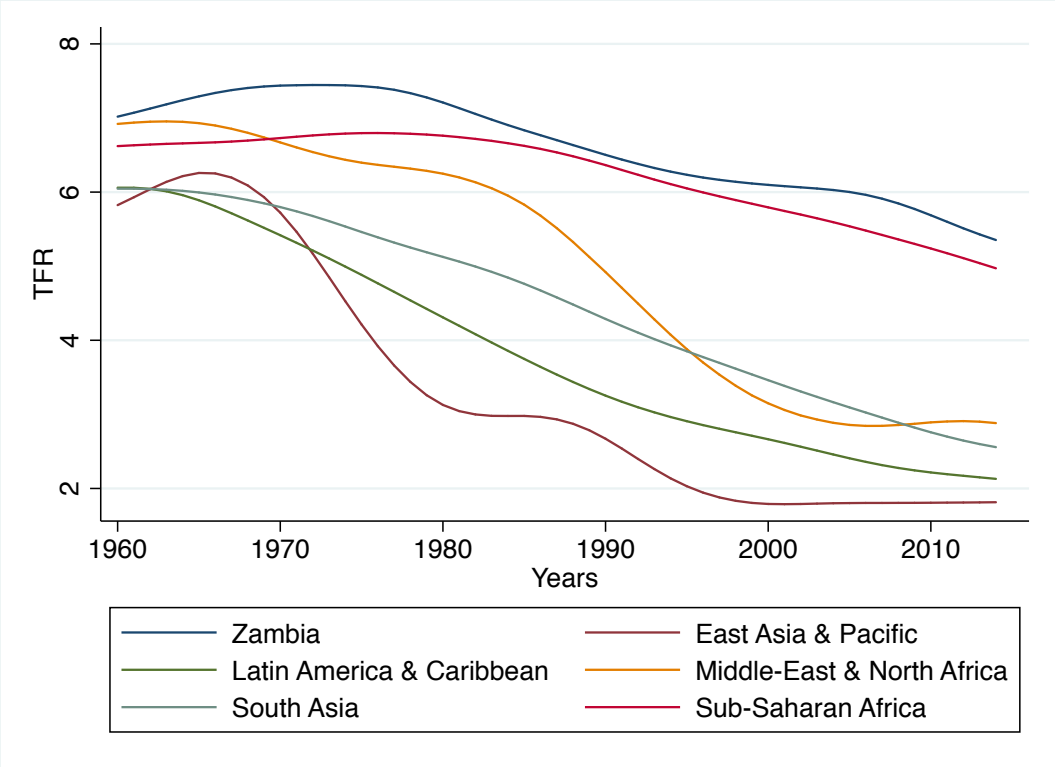
ett (1994)). Empirical evidence has yet to resolve this debate, with evaluations of supply-side interventions yielding puzzling and ambiguous results — access to family planning reduces completed fertility (e.g., Miller (2010), Angeles et al. (1998), and Bailey (2012)), but has no clear impact on short-term fertility (e.g., Ashraf et al. (2013)). Determining how family planning programs impact fertility has assumed greater importance as the fertility transition has stalled[1] in sub-Saharan Africa (Bongaarts and Casterline (2013)).

In this paper, we theoretically and empirically assess the impact of access to family planning on fertility control choice, completed fertility, and short-run fertility within a *lifecycle framework*. Specifically, we examine how women's lifecycle fertility choices and lifecycle traditional methods use respond to a broad expansion in the type and availability of modern contraceptives (i.e., the legalization of injectables) in Zambia and surrounding countries, a region of sub-Saharan Africa typical of the stalled transition (Figure 2.1). We theoretically show that correctly identifying the impact of improved access on completed and short-run fertility *requires* embedding the evaluation of family planning programs within a unified lifecycle model of fertility control choice because access to modern contraceptives changes women's *intertemporal fertility incentives* by eliminating the risk of unwanted (future) pregnancies. Using predictions of our lifecycle model, we empirically confirm that the differential impacts of improved contraceptive access on completed and short-run fertility are *consistent* with a single behavioral response — an intertemporal substitution of births to earlier in the lifecycle.

We build a dynamic model of fertility control choice where women choose costly pre-pregnancy and post-pregnancy fertility control methods in each of their fertile periods in order to achieve their *ex ante* randomly assigned target fertilities. At the end of their fertile life, women incur symmetric penalties for exceeding or falling short of their desired number of children. These intertemporal incentives create a *precautionary motive* for women with intermediate target fertilities to delay wanted births when expected losses from overshoot-

[1]Fertility rates in sub-Saharan Africa remain higher, and fertility declines have progressed more slowly, than the rest of the developing world.

Figure 2.1: Total Fertility Rates Around the World



Notes: The graph plots estimated Total Fertility Rates (TFR) for Zambia and several regions of the developing world from the World Development Indicators Database maintained by the World Bank. Aggregates for regions are constructed excluding high income countries. All series include TFR rates that are imputed.

ing target fertilities outweigh expected losses from undershooting target fertilities. Access to modern contraceptives allows women to perfectly limit unwanted births, relaxing any precautionary motives and causing: (a) substitution away from traditional methods of fertility control; (b) reductions in short-run and completed fertility among women already at or above their target fertility; and (c) increases in short-run, but not completed, fertility among women who were previously delaying having children. The latter two consequences exert *opposite forces* on short-run, but not completed, fertility, and when sufficiently many women delay wanted births in the absence of modern contraceptives, improved contraceptive access *increases short-run fertility*, while (weakly) decreasing completed fertility.

We test the predictions of our model using difference-in-difference techniques that exploit the fact that all countries surrounding Zambia had legalized injectables at least ten years prior to Zambia's 2004 legalization. We construct four unique data sets that pool across Zambia and administrative regions bordering Zambia in five neighboring countries[2] using 32 rounds of Demographic and Health Surveys (DHS), and exploit the staggered legalization of injectables to identify causal estimates for the effect of expanded modern contraceptive access on fertility, birth-spacing, abortions, and traditional methods of fertility control. Using legal access to injectables as our treatment variable allows us to avoid both selection into modern contraceptive use and endogeneity arising from unobserved factors (e.g., policymakers' decisions) affecting the location of family planning programs. Our specification flexibly controls for differential time trends in the age-profile of urban and rural women's contraceptive and fertility choices to better identify treatment effects. We supplement our analysis with within-woman estimates of treatment effects when estimating responses for outcome variables with rich panel dimensions (i.e., fertility and birth-spacing).

We find that women respond to Zambia's 2004 legalization of injectables by: (a) increasing use of injectables (350%) and modern contraceptives (50%); (b) decreasing reliance on unsafe abortions (-30%), coital infrequency (-10%), and postpartum protection from pregnancy risk during the second year post-birth (-23%); and (c) increasing short-run fertility (10%), and decreasing birth-spacing. Importantly, completed fertility (i.e., the stock of births for women 45-49) does not increase in response to injectable legalization, confirming that fertility increases reflect changes in the timing of births across the lifecycle (i.e., *tempo* effects) rather than changes in the total number of births (i.e., *quantum* effects). These results are consistent with the predictions of our model discussed above, but cannot be explained by the existing framework for understanding the relationship between modern contraceptive access and fertility. In particular, neither supply-side (Bongaarts (1994)) nor demand-side (Pritchett (1994)) explanations for fertility reductions differentiate between short-run and

[2]These countries are Tanzania, Malawi, Mozambique, Zimbabwe, & Namibia.

completed fertility responses, and both predict that fertility decreases when contraceptive use increases, contrary to our findings.

We further exploit that rural and urban women had differential access to modern contraceptives prior to legalization,[3] and that women were at different ages and parity levels when legalization occurred, to generate empirical predictions for how adoption, substitution, and fertility responses should differ across subsamples defined by urban or rural sector, age, and parity. We find that contraceptive adoption and substitution away from abortions is largest for women at higher parities, consistent with our predictions that women at or above their target fertilities switch to using modern contraceptives instead of abortions to limit unwanted births. Similarly, we find that substitutions away from traditional pre-pregnancy methods of fertility control and increases in fertility (decreases in birth-spacing) occur only for rural women, consistent with our prediction of differential impact in rural and urban areas given higher pre-legalization use of modern contraceptives by urban women. Importantly, short-run fertility increases are confined to rural women 20–45, with rural childless women older than 20 particularly likely to increase short-run fertility. These fertility responses are consistent with women shifting births earlier in their lifecycle, with women 15-19 having additional reasons to delay births (e.g., human capital formation) and women older than 45 having no reason to delay births prior to legalization.

To the best of our knowledge, we are the first to theoretically formulate how access to modern contraceptives impacts fertility when women have precautionary motives to delay wanted births and the first to empirically confirm that intertemporal incentives to increase fertility can dominate contemporaneous gains in limiting unwanted births. In doing so, we contribute to a large literature exploring how contraceptive access impacts fertility, both

[3]Urban women had substantially more access to modern contraceptives than rural women prior to legalization. Correspondingly, injectable legalization was a positive shock to any contraceptive access in rural areas, but only a positive shock to method type access in urban areas.

in the developing world[4] and in the developed world.[5] Specifically, we complement existing work showing that access to modern contraceptives delayed onset of childbearing (e.g., Goldin and Katz (2002)) and increased birth-spacing intervals (e.g., Cleland et al. (2006)) by showing that in a context where women can achieve lengthy birth-spacing intervals using only traditional methods, modern contraceptives can actually decrease birth-spacing and increase short-run fertility as women shift births earlier in the lifecycle. Moreover, by showing that modern contraceptive access changes women's intertemporal fertility incentives, we help reconcile the differential impacts of contraceptive access on completed fertility and short-term fertility found in the literature.

Our paper also contributes to a large literature on why the fertility transition stalled in sub-Saharan Africa[6] by arguing that the uniquely high reliance on traditional methods of fertility control in sub-Saharan Africa (e.g., lengthy durations of postpartum abstinence and breastfeeding) makes the region particularly likely to have had a large fraction of women successfully delaying wanted births prior to contraceptive access, thereby suggesting larger short-term increases in fertility when contraceptive access improves relative to other regions in the world. Finally, our paper contributes to literatures studying the effect of contraceptive access on breastfeeding (e.g., Rous (2001), Jayachandran (2014)), abortions (e.g., Miller and Valente (2016), Jones (2015)), and abstinence (e.g., Cleland and Ali (2006)) by confirming that modern contraceptives are substitutes with these traditional methods of fertility control in the Zambian context.

[4]For example, Miller (2010) shows that the rollout of family planning services in Colombia in the 1960s and 1970s (moderately) reduced completed fertility; Ashraf et al. (2013) and Ashraf et al. (2014) study the effects of a randomized contraceptive voucher program in Zambia, finding limited evidence that fertility decreases for women randomly assigned the voucher treatment; Desai and Tarozzi (2011) find no fertility effects when women are randomly assigned to increased family planning outreach programs in Ethiopia; Angeles et al. (1998) uses a random effects model to show large reductions in fertility associated with community-level family planning program placement in Tanzania. See Miller and Babiarz (2016) for a comprehensive survey of the literature.

[5]See, for example, Goldin and Katz (2002), Bailey (2006), and Bailey (2012).

[6]A large number of explanations that have been proposed for this phenomenon, including: limited contraceptive access (Bongaarts (1994)); high desired fertility (Pritchett (1994)); high infant mortality rates (Caldwell et al. (1992)); low levels of women's education (Lloyd et al. (2000), Kravdal (2002)); and a unique cultural model of sexual behavior and fertility in sub-Saharan Africa that rewards high fertility (Caldwell and Caldwell (1987), Caldwell et al. (1989)).

Our paper is organized as follows. We detail the context of fertility control methods in Zambia in Section 2.2 and generate testable empirical predictions from a dynamic model of fertility control choice in Section 2.3. We describe our data and outline our empirical strategy in Sections 2.4 and 2.5, respectively. We present results regarding the impact of injectable legalization on contraceptive adoption, substitution away from traditional methods, and fertility in Section 2.6. We discuss the implications of our results and conclude in Section 2.7.

2.2 Background

We study the relationship between fertility declines and contraceptive access in Zambia for two reasons. First, Zambia and surrounding countries have high fertility, high desired fertility, extensive use of traditional methods of fertility control, and low baseline use of modern contraceptives. Second, the legalization of injectables in Zambia is delayed relative to all its neighboring countries, creating a shock to contraceptive access that we exploit for identification in Section 2.5. These features allow us to study the relationship between contraceptive access and fertility decline in a context ideal to test our model, as: (a) women are likely to be using traditional methods of fertility control to delay having children and, thereby, avoid exceeding target fertilities; and (b) a large shock to contraceptive access can eliminate precautionary motives to delay childbirth. Below, we provide details of the predominant methods of fertility control in Zambia and surrounding countries and provide details of how legal barriers affected the diffusion of injectable contraceptives throughout the region.

2.2.1 Methods of Fertility Control in Zambia & Surrounds

Women in the regions we study have access to a variety of fertility control technologies, both modern and traditional, with heterogeneous effectiveness at preventing births. Unlike

modern methods, many of the traditional methods can only be used at certain moments in a woman's life (e.g., post-birth). Below, we discuss the major fertility control technologies available to these women, when during their lives they can use these technologies, and the mechanisms by which these technologies reduce fertility.

The primary methods of modern contraceptives used in countries in our sample are injectables, hormonal birth control pills, and condoms.[7] Injectables — a progestin-only form of hormonal birth control administered via injection that provide three months of protection from pregnancy risk — have unique features that render them particularly effective for fertility reduction in the sub-Saharan African context. Specifically, injectables have lower failure rates[8] and fewer adherence concerns relative to pills and condoms because: (a) users do not need to remember to use injectables daily or at time of intercourse; (b) users do not need to negotiate with male partners to use injectables; and (c) users can easily conceal injectables from male partners.[9]

The primary methods of traditional pre-pregnancy fertility control are postponed initiation into sexual activity, coital infrequency, and prolonged durations of postpartum abstinence and breastfeeding. Postponing first intercourse, available primarily to younger women, and coital infrequency, available to women who can negotiate sexual frequency with their partners, both directly reduce pregnancy risk.[10] Postpartum technologies are only available

[7]For example, DHS survey data from 1995-1999 in control group countries suggests that pills, injectables, and condoms have the three highest method mix shares, respectively. Similarly, the 1996 Zambia DHS shows pills and condoms having the largest method mix share. Towards the end of the period we study, female sterilization and hormonal implants begin to increase method mix share in all countries in our sample.

[8]Polis et al. (2016) estimate yearly typical use failure rates in Eastern & Southern Africa for injectables, pills, and condoms to be 1.9%, 4.7%, and 3.5%, respectively, though their sample of countries does not include Zambia and their methodology most likely understates failure rates. Within the US, the CDC estimates yearly typical use failure rates of injectables, pills, and condoms to be 6%, 9%, and 18%, respectively. Both sets of numbers confirm that injectables have lower failure rates compared to condoms and birth control pills.

[9]Ashraf et al. (2014) randomize access to contraceptive vouchers among women in Lusaka, Zambia, finding that women who receive contraceptive vouchers absent their husband's knowledge are more likely to use contraceptives and choose injectables than their peers who received vouchers in the presence of their husband. Their work confirms that male surveillance of women's behaviors is an important determinant of contraceptive use and that injectable contraceptives are one way of ameliorating that surveillance.

[10]In the sub-Saharan African context, marriage and initiation into sexual activity are not strongly linked, making initiation into sexual activity more relevant than entering marriage. See Caldwell et al. (1989) and Caldwell et al. (1991) for discussions of African sexual practices and how they differ from Eurasian practices.

following a birth and reduce pregnancy risk by either delaying a woman's return to ovulation or delaying a woman's return to sexual activity. There are four postpartum outcomes associated with traditional methods of fertility control: breastfeeding, abstinence, amenorrhea,[11] and the protection period.[12] Both breastfeeding duration and abstention duration are choice variables for women[13] post-birth that reduce pregnancy risk: breastfeeding duration delays return of menses, reduces conception probability once menses has returned, and can result in reduced coital frequency in societies (like Zambia) with taboos against sexual intercourse with breastfeeding mothers;[14] postpartum abstinence directly reduces pregnancy risk by delaying return to sexual intercourse. Duration of amenorrhea and duration of the protection period are outcomes that are functions of women's choice behavior and correspond to reduced pregnancy risk: amenorrhea directly prevents pregnancy and is primarily determined by duration and intensity of breastfeeding;[15] the postpartum protection period, by construction, corresponds to a period with zero pregnancy risk and is primarily determined by abstention and breastfeeding behaviors.

Abortions, a traditional post-pregnancy method of fertility control (Joffe (2009)), are widely used in Zambia & surrounds.[16] Abortion restrictions vary across the countries in our

[11]Postpartum amenorrhea refers to the period of time following a live birth when a woman does not experience her period and, hence, has (minimal) zero pregnancy risk.

[12]The postpartum protection period is the period of time following a live birth when a woman cannot get pregnant because she is either abstaining or amenorrheic. The duration of the postpartum protection period is the maximum of the duration of abstention and the duration of amenorrhea.

[13]Intimate partner violence and strong social norms regarding postpartum behavior can mitigate the extent of choice women have over postpartum behavior. Nonetheless, we can view breastfeeding and abstention as functions of household bargaining, where the degree to which a woman's preferences are respected correspond to her bargaining power. It will only be in limit cases, then, that these behavioral outcomes are orthogonal to women's choices.

[14]See Blackburn (2007) for a discussion of physiological responses to breastfeeding. See Bleek (1976)) for a discussion of social norms regarding sexual intercourse and breastfeeding among groups in sub-Saharan Africa. Jayachandran (2014) has an informative discussion of the mechanisms by which breastfeeding can reduce fecundity.

[15]See Blackburn (2007) and McNeilly (2001) for discussions of the physiological relationship between breastfeeding behavior and postpartum amenorrhea. Intensity of breastfeeding is typically measured by number of feedings per day and intensity of suckling.

[16]Sedgh et al. (2012), whose methodology is likely to underestimate abortion use in countries where women seeking abortions face legal obstacles and social stigma, estimate that Eastern Africa (which includes Zambia) had the highest abortion rates of any sub-region in the world barring Eastern Europe and the Caribbean in 2008.

sample (see Table F.1), but, importantly for our identification strategy, remain unchanged during the time period we consider. Voluntary abortions are illegal in all countries in our sample and substantial barriers exist to acquiring a legal abortion. For example, in Zambia abortions are permitted when there is substantial risk to the physical or mental health of the mother, but legally require the signature of multiple physicians, including a specialist, and must take place in specific government facilities.[17] The high costs to procuring legal abortions suggest that a large fraction of the abortions that do take place are illegal and unsafe.[18]

2.2.2 Injectable Legalization in Zambia & Surrounds

The rollout of injectable contraceptives was staggered in Zambia relative to surrounding countries. Countries surrounding Zambia lifted bans on injectables and began distribution following FDA approval of injectables in 1992 (Sutherland et al. (2011)). In contrast, Zambia maintained a legal ban on injectables until late 2004, when Depo-Provera (the most common brand of injectable) was formally registered by the Zambia Drug and Poisons Board (Solo et al. (2005)).

Legalization led to a large expansion in contraceptive supply in Zambia despite imperfect enforcement of the ban on injectables.[19] Post-legalization, the Zambian government, NGOs, and private clinics began to distribute injectables, the former two at zero cost, in a gradual country-wide rollout, with aid agencies providing large shipments of injectables directly to the Zambian Ministry of Health. Implementation of the rollout improved over time as com-

[17] See Likwa et al. (2010) for a discussion of abortion use in Zambia.

[18] Sedgh et al. (2012) estimate that over 95% of abortions in Eastern Africa are unsafe in 2008; that estimate rises to 100% during 1995-2003. Likwa et al. (2010) provide comparable figures: the authors use hospital records from five major hospitals around Zambia to document that approximately 85 times as many women are treated for abortion-related complications as obtain safe induced abortions during 2003–2008.

[19] Notably, some aid and donor agencies conducted interventions to expand method mix choice (including injectables) in the predominantly urban Lusaka and Copperbelt regions during the late 1990s in order to demonstrate to the Zambian government that Zambian women had strong unmet demand for injectables. These interventions suggest that urban Zambian women had (some) access to injectables prior to 2004. Rural women, particularly those residing outside of the Lusaka and Copperbelt regions, did not. See Skibiak et al. (2007) for details.

munity health workers were trained and authorized to administer injectable contraceptives to users following changes in WHO guidelines in 2009 (Chin-Quee et al. (2013)). However, documenting the precise stages of the rollout is difficult for at least two reasons: 1) injectables can be out-of-stock at local health centers (Ali et al. (2008)), making measurement of contraceptive supply at a local level difficult; and 2) a public scare in January 2008 based on rumors that injectables were contaminated with HIV led to a three-month withdrawal of injectables from health centers.[20] For these reasons, we analyze injectable legalization, rather than rollout, as a positive shock to contraceptive access.

2.3 A Model of Contraceptive Access & Dynamic Fertility Choice

To discipline our thoughts on how women respond to improved contraceptive access, we propose a model where women with finite opportunities to have children choose pre-pregnancy and post-pregnancy methods of fertility control each period to achieve their *ex ante* randomly assigned target fertilities. Women without access to modern contraceptives have asymmetric control over achieving target fertilities — their ability to restrict unwanted pregnancies without substantial disutility is poor relative to their ability to have another child. Asymmetric control causes women with intermediate target fertilities to have a precautionary motive to delay wanted births in order to avoid exceeding their target fertilities. Expanded access to modern contraceptives improves women's ability to avoid unwanted pregnancies,[21] changing their fertility behavior as: (a) women with intermediate target fertilities increase fertility in the short-run because they have better ability to limit unwanted pregnancies in the future; and (b) women already at their target fertility, who were using traditional methods (e.g.,

[20]These rumors were publicly debunked by the Zambian Ministry of Health, and injectables were reintroduced to all health facilities in the months following the withdrawal.

[21]The CDC estimates typical-use annual failure rates for modern contraceptives such as hormonal implants and IUDs below 1%, substantially lower than comparable rates for traditional methods such as withdrawal (22%) or periodic abstinence (24%).

abortions) to limit births, substitute away from traditional methods towards modern contraceptives in order to more successfully limit unwanted pregnancies. These two consequences of improved contraceptive access exert opposite forces on aggregate fertility. In contrast, when women's fertility control in the absence of modern contraceptives approaches symmetry, women have no precautionary motives to delay fertility and improved contraceptive access *reduces fertility*, conforming to the predictions of a repeated static model of fertility control.[22]

Our model highlights how access to modern contraceptives can have counterintuitive implications when women make dynamic fertility control choices and contributes to existing models of dynamic stochastic fertility control (e.g., Rous (2001), Newman (1988)) by formalizing a precautionary motive to delay having children and analyzing how this motive is impacted by improved contraceptive access. Our model abstracts away from the trade-off between human capital formation and initiation into sexual activity and childbearing for adolescents, limiting our ability to comment on fertility behaviors for women aged 15-19. Our model also abstracts from preferences over birth-spacing, differential access to modern contraceptives prior to legalization, and infant mortality. These additional forces attenuate, but do not necessarily eliminate, precautionary motives to delay births.[23] Below, we present a simple three-period version of our model to provide intuition for how asymmetric fertility control can generate precautionary motives to avoid having children and can lead to ambiguous fertility responses to improved modern contraceptive access.

[22]Consider a model where women are instead assigned a fertility preference (i.e. want a child now, do not want a child now) at the beginning of a each period using i.i.d draws from an underlying distribution and rewarded at the end of each period based on how successful they were at achieving their preference. Then, women's fertility choices in a given period are independent of the future (i.e. static), and access to modern contraceptives *must reduce fertility*.

[23]For example, including preferences for birth-spacing intervals ensures that women have precautionary motives to delay births only if women are already meeting their birth-spacing targets and still face overshooting risks.

2.3.1 Description of Model

Consider a world where women live four periods, indexed by $t \in \{0, 1, 2, 3\}$, and have three fertile periods corresponding to $t \in \{1, 2, 3\}$. At the onset of the model ($t = 0$) women are randomly and independently assigned a target fertility $n_i \in \{0, 1, 2, 3\}$,[24] and a disutility from abortion use $c_i \sim F(0, \infty)$, where F is a cumulative density function for a continuous random variable. A woman makes decisions about fertility control in each period based on two state variables: (a) the relationship between her stock of children and her desired target fertility, and (b) her personal cost of abortions. Denote by s_t the stock of children that a woman has at the end of period t, with $s_0 \equiv 0$, and define the difference between a woman's stock of children and her desired fertility target as $\gamma_t^i \equiv s_t - n_i$. At the beginning of period t, then, the state variables for individual i are (γ_{t-1}^i, c_i).

Within each of her fertile periods, a woman first chooses pre-pregnancy methods of fertility control $b \in [0, 1]$ or $m \in \{0, 1\}$ corresponding to traditional methods of fertility control (e.g., coital infrequency, postpartum behaviors, etc.) and modern contraceptives (injectables), respectively. Conditional on a pregnancy realizing, she then chooses $a \in \{0, 1\}$, corresponding to keeping or aborting the pregnancy, respectively. When using traditional methods b, a woman conceives with probability $p(b)$; if she chooses to use injectables ($m = 1$), she does not conceive. A woman utilizing no methods of fertility control will conceive with probability $\bar{p} < 1$. Abortions successfully terminate a pregnancy with probability one.

Women experience instantaneous disutility from use of fertility control methods and a final payoff that penalizes them from deviating from their target fertility. Within each period, women receive fixed utility $\bar{u}$, disutility $\phi(b)$ from using traditional pre-pregnancy methods of fertility control, disutility $\bar{\phi}$ from using injectables, and personal disutility c_i from aborting a pregnancy. At the end of $t = 3$, women are penalized for deviating from their target fertility

[24] These target fertilities can be construed as broader fertility categories. In the Zambian context, we correspond these target fertilities to 0 kids, 1–3 kids, 4–6 kids, and 7+ kids, respectively.

with symmetric loss function $U(\gamma_3^i) \equiv -r(|\gamma_3^i|)$. For convenience, we normalize $\phi(0) = 0$ and set $r(0) = 0$.

2.3.2 Maximization Problem and Solution

Within each period, women maximize the sum of current and expected utility by choice of fertility control methods. We characterize how women's fertility and fertility control behaviors change as contraceptive access expands by comparing women's optimal behavior when they cannot use modern contraceptives (i.e., they are constrained to choose $m = 0$) to women's optimal behavior when they can use modern contraceptives (i.e., they may choose $m \in \{0, 1\}$). Throughout our exposition, we focus on the intuition and forces driving our results; a formal presentation of our model can be found in Appendix A.

World without modern contraceptives

When women cannot choose modern contraceptives, women at $t \in \{1, 2, 3\}$ solve:

$$V_t(\gamma_{t-1}^i) = \max_{b \in [0,1]} \Big\{ \bar{u} - \phi(b) + p(b) \max \Big\{ V_{t+1}(\gamma_{t-1}^i + 1), V_{t+1}(\gamma_{t-1}^i) - c_i \Big\} + (1 - p(b)) V_{t+1}(\gamma_{t-1}^i) \Big\}$$

where the value function $V_t(\gamma_{t-1}^i) \equiv V_t(\gamma_{t-1}^i, c_i)$ for $t = 1, 2, 3$ suppresses dependence on c_i for conciseness, and $V_4(\gamma_2^i) \equiv U(\gamma_2^i)$. Women choose traditional fertility control methods b while accounting for future abortion decisions in the event of pregnancy (the inner maximum), balancing the immediate disutility from using traditional fertility control methods with (potential) disutility from pregnancy.

With standard assumptions on cost functions and probabilities,[25] we fully characterize women's optimal behavior when they do not have access to modern contraceptives (see Lemma 1 in Appendix A). Women with high target fertilities, $n_i = 3$, will try to have children every period; they will use neither traditional methods of fertility control nor abortions and will, in expectation, have $3\bar{p}$ children. Women with low target fertilities, $n_i = 0$, will try to avoid having children every period, aborting pregnancies if and only if their abortion disutility is low and choosing an intensity of traditional method use that increases in their abortion disutility. Intuitively, women with low disutility of abortion use know they will terminate any pregnancy that arises and have less incentive to use (costly) fertility control methods to avoid pregnancy.

Women with intermediate target fertilities, $n_i \in \{1, 2\}$, choose to delay having desired births when their expected losses from overshooting outweigh their expected losses from undershooting (i.e., when their failure rate while trying to avoid pregnancy, $p(b^*)$, and their personal disutility of abortions, c_i, is high relative to their failure rate while trying to get pregnant, $1 - \bar{p}$), and vice-versa.[26] As women's control over fertility outcomes becomes increasingly asymmetric (i.e., $\bar{p}$ increases), more women delay having children to avoid potential overshooting losses (formalized in Lemma 2 in Appendix A).

[25] We place smoothness restrictions on the cost functions $\phi(\cdot)$ and $r(\cdot)$ and the probability $p(\cdot)$ to ensure that solutions exist to the above maximization problems. We place convexity restrictions on all three functions to ensure that women's optimal choice of traditional fertility control method is weakly increasing in the disutility of abortions, c_i. Finally, we assume that cost functions are strictly increasing, and that the probability function is strictly decreasing. We formalize these restrictions in Assumption 2 in Appendix A.

[26] Women with low disutility from using abortions face minimal expected losses from overshooting as their future selves will terminate any unwanted pregnancy.

World with modern contraceptives

Women with access to modern contraceptives maximize:

$$V_t^I(\gamma_{t-1}^i) = \max \begin{cases} \bar{u} - \bar{\phi} + V_{t+1}^I(\gamma_{t-1}^i), \\ \max_b \begin{cases} \bar{u} - \phi(b) + p(b) \left[V_{t+1}^I(\gamma_{t-1}^i + 1) \vee \left(V_{t+1}^I(\gamma_{t-1}^i) - c_i \right) \right] \\ +(1 - p(b)) V_{t+1}^I(\gamma_{t-1}^i) \end{cases} \end{cases}$$

where the $\vee$ operator denotes a maximum and $V_4(\gamma_2^i) \equiv U(\gamma_2^i)$. Modern contraceptive access provides women with the additional choice to forego using traditional methods in favor of modern contraceptives. Below, we characterize women's optimal behaviors when they can utilize modern contraceptives (see Lemma 3 in Appendix A).

Women with high desired fertility, $n_i = 3$, have the same behavior as before. Women with low desired fertility, $n_i = 0$, again try to avoid having children in each period but will switch to using modern contraceptives if the disutility of modern contraceptives, $\bar{\phi}$, is low relative to the disutility of using traditional methods and abortions. Abortions and modern contraceptives are substitutes: modern contraceptive use is a (weakly) increasing function of abortion disutility; and the probability of a woman using abortions is a (weakly) decreasing function of modern contraceptive disutility. Women with intermediate desired fertility, $n_i \in \{1, 2\}$, no longer delay births when the disutility of using modern contraceptives is low. Intuitively, when modern contraceptives have low disutility of use, women know that their future selves can use modern contraceptives and avoid unwanted children with certainty, relaxing any precautionary motive to avoid having children.

2.3.3 Empirical Predictions

We derive testable empirical predictions of the impact of injectable legalization on fertility behavior by extending our model to allow cohorts of women to age through the lifecycle (see

Corollaries 2 & 3, and Proposition 3 in Appendix A).[27] Let time periods t correspond to different ages for women, with $\bar{N}$ women alive in each of the three fertile ages corresponding to different birth cohorts. Let $y \in \{y_0, y_1\}$ denote years. As time progresses, y increases from y_0 to y_1, and women age from t to $t+1$. A complete age-profile of women are alive at each value of y: women at the end of their fertile lives ($t = 3$) exit the model, and a new cohort of $\bar{N}$ women enter childbearing with new draws of n_i and c_i at $t = 1$. Injectables are legalized between y_0 and y_1.

Our predictions require four assumptions: (a) the disutility of modern contraceptives is low relative to that of traditional methods; (b) sufficiently many women have intermediate target fertilities compared to the maximum number of children they are able to bear; (c) failure rates when trying to avoid pregnancy are higher than failure rates when trying to have another child; and (d) the fraction of women with access to modern contraceptives prior to legalization is low. While we cannot test the first assumption, the latter three assumptions are consistent with the Zambian context.[28]

Prediction 1: Women adopt contraceptives post-legalization. Adoption is higher for women at higher parities.

Prediction 2: Use of traditional methods and abortions decline. Substitutions away from abortions are largest among older women and women at higher parities.

[27] This extension more accurately captures the reality of injectable legalization in Zambia, where many women were exposed to legalization during the middle of their childbearing years.

[28] *Assumption (b):* Using a DHS survey question that overestimates target fertilities, the average rural (urban) woman in 2002 reported her ideal number of children to be 5.24 (4.05). Assuming (conservatively) that women are fertile from 16–40 and can bear a child every two years, women can bear a maximum of 12 children, well above their desired target fertility.

Assumption (c): Chandra et al. (2013) use the National Survey of Family Growth in the US and estimate that 80% of non-sterilized women aged 40–45 with at least one live birth are fecund. The CDC estimates typical use failure rates of traditional methods of fertility control like withdrawal and periodic abstinence to be 22–28%. Hence, Zambian women over the majority of their lifecycle are likely to have higher failure rates when trying to avoid pregnancy than when trying to conceive.

Assumption (d): Rural women had essentially no access to modern contraceptives pre-legalization (Skibiak et al. (2007)) and constituted 66% of Zambian women aged 15–49 in 2002.

Women use modern contraceptives to reduce pregnancy risk when they do not want to have additional children, substituting away from both traditional methods of fertility control and abortions. As women only abort when they are at or above their target fertilities, we expect larger substitutions away from abortions among subgroups of women who are more likely to have already reached their target fertilities — e.g., older women and women at higher parities. Contraceptive adoption is higher at higher levels of parity because incentives to limit births are stronger than (absent) incentives to space births in our model.

Prediction 3: Fertility increases and birth-spacing decreases for women prior to their final fertile period (i.e., women 15–45) in response to injectable legalization.

Prediction 4: Childless women who desire at least one child do not adopt contraceptives and increase fertility.

Prediction 5: Fertility does not increase for women in their terminal fertility period (i.e., women 45–49). Completed fertility does not increase.

Intuitively, modern contraceptives increase fertility by relaxing the precautionary motive to delay having children *and* decrease fertility by aiding women in avoiding unwanted children. When a large enough fraction of women have intermediate target fertilities (i.e. a precautionary motive to delay births) fertility responses are positive. Childless women who desire at least one child will no longer delay wanted births, eschewing modern contraceptives and increasing fertility. Fertility for women in their final fertile period cannot increase as these women never delay wanted children. Completed fertility cannot increase because women's ability to limit unwanted births has improved and overshooting risk outweighs undershooting risk prior to improvements in contraceptive access.

Prediction 6: Declines in use of traditional methods and abortions are smaller in magnitude in urban areas than rural areas.

Prediction 7: Fertility increases are confined to rural areas; fertility does not increase in urban areas. Decreases in birth-spacing change analogously.

We generate two additional predictions by considering an extension to our model that allows urban women to have access to modern contraceptives prior to injectable legalization. Intuitively, women with prior access to modern contraceptives would have already experienced the largest gains in fertility control relative to traditional methods. They would no longer have precautionary motives to delay births and will have already made the largest substitutions away from traditional methods and abortions. Hence, the magnitude of substitution away from traditional methods should be smaller and fertility should not increase.

2.4 Data

To study the relationship between contraceptive access and fertility behaviors, we use all 32 rounds of publicly available Demographic & Health Surveys (DHS) across six countries[29] between 1988 and 2014 that collect information on birth histories or contraceptive use.[30] All surveys we utilize are representative samples of women 15-49 at the administrative region level for both urban and rural sectors.

We use the DHS surveys to construct four unique datasets consisting of women residing at survey date in Zambia and bordering administrative regions in neighboring countries. The first is a *repeated cross-section* of 98,825 women that we create by pooling data across DHS rounds and countries. We study contraceptive adoption, the abortion use, and coital frequency with this cross-section. The remaining three datasets are panel datasets we use

[29]The countries are Zambia, Tanzania, Malawi, Mozambique, Zimbabwe, & Namibia. Our sample includes data from all countries bordering Zambia bar two. We exclude Angola and the Democratic Republic of the Congo (DRC) for two reasons: (a) the DHS does not survey Angola; and (b) all women in the DRC surveyed by the DHS were interviewed post-2007, resulting in little or no pre-2004 data for most outcomes of interest.

[30]We use information from three types of surveys: the standard DHS survey, the AIDS Information Survey (AIS), and the Knowledge, Attitudes, & Practice (KAP) survey. The AIS and KAP do not ask all modules from the standard rounds, but include information on birth histories and contraceptive use.

Table 2.1: Summary Description of Datasets

Dataset	**Unit of Observation**	**Outcomes Studied**
Repeated Cross-Section	Woman	Contraceptive Use, Coital Frequency, Abortions
Postpartum Panel	Woman-Child-Month	Postpartum Behaviors
Birth-history Panel	Woman-Year	Flow Fertility
Birth-spacing Panel	Woman-Birth-Month	Birth-spacing

to study postpartum and fertility behaviors. We describe them in more detail below, and summarize the outcome variables we study with each dataset in Table 2.1.

We use information collected on postpartum outcomes for each live birth during the 5-6 years prior to survey date to construct a *postpartum panel* — a child-level monthly frequency panel dataset that we use to study the durations of postpartum outcomes. It is inappropriate to study the duration of postpartum outcomes using a cross-section of births because of censoring that arises from mothers still breastfeeding (or abstaining, etc.) at the time of a child's death or at the time of interview. We address this censoring by having the panel cover each month from a child's birth to the date of survey and dropping all months following a child's death, ensuring that all months corresponding to a living child are weighted equally. DHS survey questionnaires are careful to collect information on the month of birth and, if a child died, the age at death in months, that is complete and accurate for all births within 5-6 years of survey date.

We use the complete retrospective birth histories[31] elicited from all interviewed women by the DHS to construct two panel datasets: a *birth-history panel* and a *birth-spacing panel*. The former is a woman-level yearly frequency panel dataset containing information on the timing, survival information, and sex of each birth that we use to study fertility. This panel

[31]Though collected retrospectively, the birth histories collected by the DHS in sub-Saharan Africa are known to be accurate and complete (Pullum and Becker (2014)).

covers each year from the year of a woman's 10th birthday to the year of survey, and we drop all survey years in which women are interviewed prior to December.[32] The latter is a birth-level monthly frequency panel dataset that we use to study birth-spacing intervals. As with our postpartum variables, it is inappropriate to study birth-spacing using a cross-section of births because of the censoring that arises from mothers being surveyed while in the middle of a birth-spacing interval. We address this censoring by having the panel cover each month following a live birth until either the subsequent live birth or the survey date, with every month following a live birth weighted equally.

2.5 Empirical Strategy

We use difference-in-difference techniques to exploit the staggered legalization of injectable contraceptives in Zambia and surrounding countries and identify causal estimates for the effect of expanded modern contraceptive access on a variety of fertility behaviors. Given the timing of injectable law changes discussed in Section 2.2.2, we classify women in Zambia prior to 2004 as untreated and classify women in Zambia post-2004 as treated. Women in countries surrounding Zambia, who form the control group, are always classified as treated because all our regression samples only include information from 1996 onwards.

Formally, our main regression specification is as follows:

$$y_{irsat} = \alpha + \beta T_{rt} + \delta_{sar} + \gamma_{sat} + \xi X_{irsat} + \varepsilon_{irsat} \tag{2.1}$$

where i indexes individuals, r indexes regions, s indexes urban or rural place of residence, a indexes age group, and t indexes time. The unit of observation in specification (2.1) varies with the dataset used, as summarized in Table 2.1, but the regression and identification

[32] When constructing year-level measures of fertility, including incomplete years for women will result in underestimates of fertility.

strategy remain the same. We discuss specifics for different datasets and dependent variables as we present our results in later sections.

The main regressor of interest is the treatment variable, T_{irsat}, an indicator variable taking value one if a woman has legal access to injectables. Using legal access to injectables as our treatment variable allows us to avoid bias due to selection into use of modern contraceptives. Similarly, abstracting from the specific pattern of injectable rollout in Zambia eliminates bias arising from the endogeneity of family planning program locations (i.e., programs are more likely to be placed in high fertility areas). To test predictions of our model, we also estimate treatment effects for subsamples defined by urbanity, age, parity, and education level by estimating specification (2.1) separately for subsamples defined along these dimensions.

We control for (potentially time-varying) individual characteristics such as marital status,[33] years of schooling,[34] and parity[35] using X_{irsat}. The set of controls varies with our outcome variable, and we discuss these specifics when presenting results in later sections. We present all results both with and without the inclusion of controls.

Our specification flexibly controls for urbanity and age group by including space and time fixed effects interacted with age group a and urban or rural sector s.[36] Controlling flexibly for sector and age is necessary given that time trends for contraceptive and fertility behaviors differ in urban and rural areas and over the lifecycle. Sector-age-region fixed effects, δ_{sar}, control for time-invariant determinants of the dependent variable within each subsample sar (e.g., baseline measures of y for women in region r, sector s, and age group a). Similarly, sector-age-time fixed effects, γ_{sat}, control for space-invariant determinants of the dependent variable within each subsample sat (e.g., a common shock to y at time t for all women in sector s and age group a).

[33]There are five categories measured by the DHS: never married, currently in union, divorced, widowed, and separated (not living together). In panel datasets, the only time-varying measure of marital status we can create is a measure of ever having been married or in union.

[34]We group years of schooling into four categories: 0 years, 1–4 years, 5–8 years, and 9+ years.

[35]We group levels of parity into four categories: 0 births, 1–3 births, 4–6 births, and 7+ births.

[36]Women are coded as living in a certain region and in an urban or rural place of residence based on their response at survey date. Women are grouped into six age buckets: 15–19, 20–24, 25–29, 30–34, 35–39, and 40–44.

As Zambia and its neighboring countries do not have synchronized survey times, we choose regression samples and time windows for our time fixed effects carefully to ensure that information from Zambia and a minimum of four control group countries are present in each time window.[37] For our main regressions, we restrict regression samples to information from women aged 15–44 after the year 1996 to examine women during prime childbearing ages and to allow for the initial effects of injectable legalization in countries surrounding Zambia to take hold. We study women aged 45–49 separately because we expect women in their terminal fertile period to respond differently. We present details of the precise regression sample we use for each outcome variable when we present our results.

We cluster all standard errors at the sub-national region level. There are a total of eighteen clusters in our dataset that are unbalanced in size. It is well documented that standard cluster-robust variance estimation on a sample with both unbalanced clusters and clusters fewer than twenty in number introduces downward bias in the estimator for the variance-covariance matrix (Bertrand et al. (2004), Cameron and Miller (2015)). To address this, we confirm results are robust to estimating standard errors using the wild cluster bootstrap technique of Cameron et al. (2008).

Regression specification (2.1) identifies β by comparing changes in outcomes for Zambian women in a given age group and sector at two times on either side of the 2004 legalization to changes in outcomes for non-Zambian women in the same age group and sector at the same two times. For datasets with a panel dimension, identification for β comes from both comparisons across women and comparisons within woman. When studying fertility and birth-spacing, we complement our evidence from specification (2.1) by estimating individual fixed effect specifications that identify β using only within-woman variation in fertility.

Our estimation strategy identifies the casual effect of improved contraceptive access on our outcome variables as long as the *parallel trends assumption* holds — as long as out-

[37]When using the repeated cross-section dataset, we do so by constructing five-year time bins: 1990–94, 1995–99, 2000–04, 2005–09, & 2010–2014. When using the panel datasets, we estimate specification (2.1) with more granular time fixed effects (i.e., year or year-month), but restrict the years or months we include in our regression samples.

come variables for women in a given sector and age group in both Zambia and surrounding regions would have had parallel time trends in the absence of exposure to treatment. We assess the validity of the parallel time trends assumption in four ways. First, we plot event studies for all our outcome variables to determine whether or not outcomes for women in Zambia and surrounding countries are on parallel time trends prior to either group of women gaining access to injectables (i.e., prior to 1992). Second, we check that observed covariates (e.g., desired fertility) are on similar time trends in both Zambia and surrounding countries throughout the period we study. Third, we estimate paired-region specifications that restrict identification of β to women living within smaller spatial units, who are more likely to have similar time trends in unobserved covariates (e.g., local shocks to access). Fourth, we address any country-specific policies in our control group that affect contraceptive access by estimating our results dropping one control-group country at a time.

When examining event studies, we ascertain: (a) that time trends in outcome variables are parallel prior to 1992; and (b) that our estimated coefficients are driven by changes in Zambia post-2004 rather than changes in control group countries. For all outcome variables we study, we construct subsamples zsa of our data disaggregated by Zambia z, age group a, and urban or rural sector s, and regress:

$$y_{irt}^{zsa} = \alpha^{zsa} + \delta_r^{zsa} + \gamma_t^{zsa} + \xi^{zsa} X_{irt}^{zsa} + \varepsilon_{irt}^{zsa} \tag{2.2}$$

where i indexes individuals, r indexes regions, t indexes time, z indexes living in Zambia or elsewhere, s indexes sector, and a indexes age group. Our event studies are plots of the time coefficients γ_t^{zsa}.

The two most important covariates of contraceptive use and fertility behavior are desired fertility, and level of education.[38] We ascertain whether or not there are differential time trends for these covariates in Zambia and surrounding countries by estimating specification

[38]See Pritchett (1994) for desired fertility, and Kravdal (2002) for education.

(2.2). Desired fertility[39] is not differentially changing between control group countries and Zambia for women across age-groups and sectors of residence (see Figures D.1 and D.2). In contrast, rural women aged 15–19 in Zambia increase years of schooling post-2004 at a slightly faster rate than their counterparts in control group countries (see Figure D.3). Urban women aged 15–19 in Zambia and surrounding countries have parallel trends in years of schooling.[40] We cannot disentangle whether higher years of completed schooling in Zambia are due to a school construction program from 1996–2010 (Ashraf et al. (2016)) or due to improved contraceptive access increasing perceived returns to schooling for girls. We control for this as best we can by showing all our results are robust to including education controls and showing our results are consistent (when estimated separately) across levels of educational attainment.

Specification (2.1) permits comparisons of geographically distant pairs of women when identifying β. Spatially distant women can be more dissimilar in unobserved time-varying characteristics than women living close to each other (e.g. time-varying local shocks to contraceptive access), threatening identification. We ensure our results are not driven by such comparisons by confirming that all results are robust to using a paired region regression specification that estimates β using only variation within zones on either side of the Zambian border (for details see Appendix E).

Other policy changes that differentially impact contraceptive use or fertility behavior during the period we study can also threaten our identification strategy. We address such concerns by confirming our results are robust to dropping each control group country one at a time, ensuring that our results are not driven by any country-specific shocks (e.g., country-

[39]Using the repeated cross-section dataset, we measure desired fertility from women's responses to the survey question "Would you like to have another child, or would you prefer to not to have any more children?" We construct two dependent variables from women's responses: an indicator variable taking value one if a woman responds she wants to have another child at survey date and an indicator variable taking value one if a woman responds she wants no more children at survey date. The two indicator variables are not complements: women who are sterilized, infecund, have never had sex, or have undecided fertility preferences are coded separately.

[40]Examining time trends in educational attainment for adolescents informs us of changes in *contemporaneous* levels of schooling; examining time trends for older women informs us of changes in *past* levels of schooling (potentially pre-1990).

specific policies) or by any country-specific responses to global shocks.[41] In the paper, we only present results for samples that exclude Malawi, as Malawi is the only country that directly changes policies affecting contraceptive access during the time period we study.[42]

2.6 Results

Increased access to modern contraceptives leads to adoption of modern contraceptives, substitution away from traditional methods of fertility control, and increases in fertility. Increases in fertility are confined to rural women younger than 45, while completed fertility does not increase. Substitutions away from traditional methods of fertility control are larger for rural women, and substitutions away from abortions are larger for older women (30–44) and women at higher parities (4+ kids). Childless women eschew modern contraceptives in favor of childbearing. These results confirm Predictions 1–7 of our model.

2.6.1 Modern Contraceptive Adoption

Women increase use of injectables (360%) and use of any modern contraceptive (49.5%) post-legalization. Usage of birth control pills and condoms does not change. Injectable and modern contraceptive adoption is higher for women at higher parities, consistent with Prediction 1. Time trends confirm that adoption of injectables is driven by the staggered legalization of injectables through the region, and confirm that while legalization is a positive shock to any contraceptive access in rural areas, it is only a positive shock to method type access in urban areas.

[41] The component of global policy changes (e.g., changes in the global gag rule) that affect women of a given age and sector equally in all the regions we study are addressed by our time fixed effects.

[42] In 2005, the Malawi government eased restrictions on who can perform female sterilizations and committed funding to expanding the number of health care personnel and family planning workers. Both these policy changes, by expanding contraceptive access in control group countries post-2004, should lead us to underestimate responses to injectable legalization.

Methods

To study the effect of injectable legalization on contraceptive use and test Prediction 1, we estimate specification (2.1) on our *repeated cross-section dataset*. The dependent variable is an indicator variable taking value one if a woman is using one of four types of contraceptive technologies at survey date: injectables, birth control pills, condoms, and a catch-all category for modern contraceptives.[43] Modern contraceptives include IUDs, diaphragms, female sterilizations, male sterilizations, and hormonal implants in addition to injectables, birth control pills, and condoms.[44] We estimate specification (2.1) on two samples of women: (a) all women surveyed, and (b) all women using modern contraceptives when surveyed. Analyzing the first sample informs us of how contraceptive use changes post-legalization; analyzing the second informs us of how contraceptive users' choice among contraceptive methods changes post-legalization.

Results

We find a 6.4 percentage point increase in the use of both injectables and modern contraceptives by Zambian women post-legalization, representing 360% and 49.5% increases over pre-legalization levels (see Table 2.2).[45] There is no significant change in their use of condoms or pills. Zambian users also alter their choice of method: users are 28 percentage

[43]We study women's contraceptive choice using a flow measure of contraceptive usage at survey date because it is superior to either either ever-use of contraceptives or contraceptive calendar information for three reasons: (a) it allows us to better determine timing of use; (b) it suffers from less recall error; and (c) it is asked in a consistent manner across all countries and survey rounds during our period of analysis.

[44]We do not examine IUDs, diaphragms, and male sterilizations because use of these methods is minimal (during 1992-2014, a total of 187 Zambian women report using these methods at survey date, respectively, despite the DHS interviewing 46,286 Zambian women). We do not examine female sterilizations and hormonal implants because both these methods are the subject of additional policy changes during our period of study: Malawi expanded access to female sterilizations post-2005 and USAID expanded distribution of Norplant throughout sub-Saharan Africa post-2007. As discussed in Section 2.5, we address these policy changes by confirming our results are robust to dropping each control group country one at a time.

[45]Our results on contraceptive adoption are robust to excluding control group countries one at at time (see Table C.1), to including controls for marital status and years of schooling, and to using a paired-region regression specification that identifies β only using variation within smaller geographical areas (see Table C.1). As we exclude control group countries one at a time, point estimates for injectable adoption range from 5.8 to 9.1 percentage points, and point estimates for modern contraceptive use range from 5.5 to 11.5 percentage points, consistent with strong take-up.

Table 2.2: Contraceptive Adoption

Panel A: Changes in Contraceptive Choice

	Modern Contraceptives		Injectables		Condoms		Pills	
Post Inj	0.064***	0.066***	0.064***	0.064***	-0.008	-0.008	0.011*	0.011*
	(0.020)	(0.020)	(0.013)	(0.013)	(0.005)	(0.005)	(0.006)	(0.006)
Obs.	76,126	75,493	76,126	75,493	76,126	75,493	76,126	75,493
Mean	0.255	0.255	0.109	0.109	0.0355	0.0355	0.0706	0.0706
Clusters	18	18	18	18	18	18	18	18
Controls	N	Y	N	Y	N	Y	N	Y

Panel B: Changes in Contraceptive Choice Among Contraceptive Users

Post Inj	0.288***	0.281***	-0.145***	-0.138***	-0.145***	-0.145***
	(0.038)	(0.037)	(0.024)	(0.024)	(0.023)	(0.022)
Obs.	19,378	19,248	19,378	19,248	19,378	19,248
Mean	0.430	0.430	0.140	0.140	0.277	0.277
Clusters	18	18	18	18	18	18
Controls	N	Y	N	Y	N	Y

Notes: The dependent variable in these regressions is an indicator variable that takes value 1 if a woman is using (a type of) contraception at survey date. The reported coefficient and standard errors are for the term β in regression (2.1). All regression samples are restricted to women 15–44 interviewed during 1996–2014. The regression sample in *Panel A* consists of this full sample; *Panel B* is restricted to women using contraceptives at survey date. The category modern contraceptives include injectables, norplant, pills, condoms, male & female sterilization, IUDs, & diaphragms. Controls include dummy variables for marital status and years of schooling. All regressions are clustered at the region level. *** p<0.01, ** p<0.05, * p<0.1

points more likely to be using injectables and 13–14 percentage points less likely to use pills or condoms post-legalization. These results do not suggest that women who were using pills or condoms ceased doing so post-legalization. Rather, they suggest that contraceptive users make different choices with regards to method type post-legalization: when given legal access to contraceptives with lower failure rates (i.e., injectables), Zambian women using modern contraceptives are more likely to choose more effective methods of fertility control.

Contraceptive adoption is a broad phenomenon, suggesting injectable legalization is an excellent candidate to study the impact of improved access to contraceptives on aggregate fertility behavior. Adoption of injectables and modern contraceptives is consistent with our pooled estimates for women in urban and rural areas (see Table 2.3) and for women of different educational attainments (see Figure B.2). Contraceptive adoption is stronger for women during prime childbearing years (see Figure B.1): injectable and modern contraceptive use increases by approximately 8 percentage points for women 20–39. Consistent with Prediction 5, contraceptive adoption is stronger for women at higher levels of parity (see Figure B.3): injectable and modern contraceptive use does not increase for childless women, increases by 8 percentage points for women with 1–6 live births, and increases by 11 percentage points for women with more than 7 live births.

Time trends for injectable use (see Figure 2.2) in both rural and urban areas, and time trends of modern contraceptive use (see Figure 2.3) in rural, but not urban, areas, are consistent with the staggered legalization of injectables leading to staggered adoption of contraceptive technologies.[46] From baseline rates of injectable use near zero,[47] control group countries exhibit much sharper take-up during the 1990s compared to Zambia across age groups and place of residence. Despite lagging in injectable use during the 1990s, Zambian

[46]Event studies for method mix changes can be found in Appendix D.

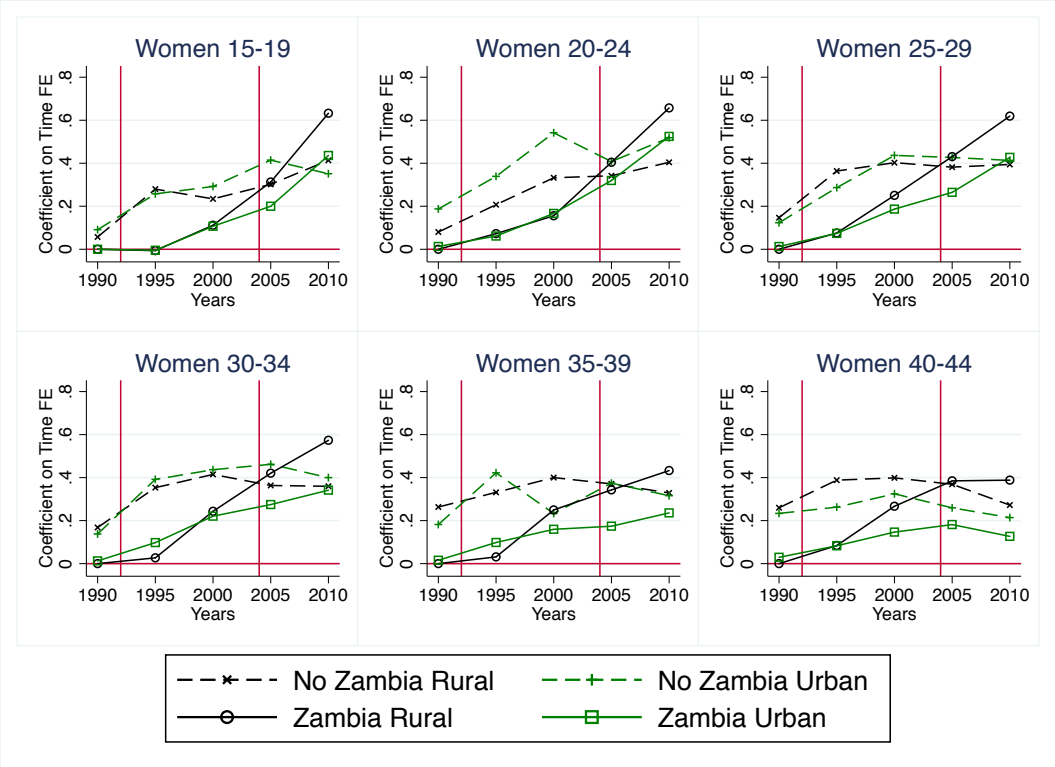
[47]Low baseline rates of injectable use (fewer than 0.1% of women in Zambia in 1992 were using injectables, while 1.93% of women in control group countries during 1988–1992 used injectables) reflect parallel time trends of near zero injectable use prior to 1992 in both Zambia and control group countries: low baseline use in both areas can only reflect divergent pre-trends if injectable use *was declining* in either Zambia or control group countries during the late 1980s, a fact that is inconsistent with a broad literature on contraception in sub-Saharan Africa (e.g., Bertrand et al. (1993), Caldwell and Caldwell (1987)).

Table 2.3: Heterogeneous Contraceptive Adoption Across Sectors

	Injectables		Modern Contraceptives		Condoms		Pills	
Panel A: Rural Women								
Post Inj	0.066***	0.066***	0.065***	0.066***	-0.006	-0.006	0.011	0.011
	(0.016)	(0.015)	(0.021)	(0.020)	(0.006)	(0.006)	(0.007)	(0.007)
Obs.	52,660	52,162	52,660	52,162	52,660	52,162	52,660	52,162
Mean	0.109	0.109	0.235	0.235	0.0282	0.0282	0.0619	0.0619
Clusters	18	18	18	18	18	18	18	18
Controls	N	Y	N	Y	N	Y	N	Y
Panel B: Urban Women								
Post Inj	0.057***	0.059***	0.063**	0.066**	-0.013	-0.013	0.012	0.012
	(0.014)	(0.013)	(0.028)	(0.028)	(0.013)	(0.012)	(0.013)	(0.013)
Obs.	23,466	23,331	23,466	23,331	23,466	23,331	23,466	23,331
Mean	0.111	0.111	0.299	0.299	0.0519	0.0519	0.0900	0.0900
Clusters	18	18	18	18	18	18	18	18
Controls	N	Y	N	Y	N	Y	N	Y

Notes: The dependent variable in these regressions is an indicator variable that takes value 1 if a woman is using (a type of) contraception at survey date. The reported coefficient and standard errors are for the term β in regression (2.1). All regression samples are restricted to women 15–44 interviewed during 1996–2014. The category modern contraceptives include injectables, norplant, pills, condoms, male & female sterilization, IUDs, & diaphragms. Controls include dummy variables for marital status and years of schooling. All regressions are clustered at the region level.
*** p<0.01, ** p<0.05, * p<0.1

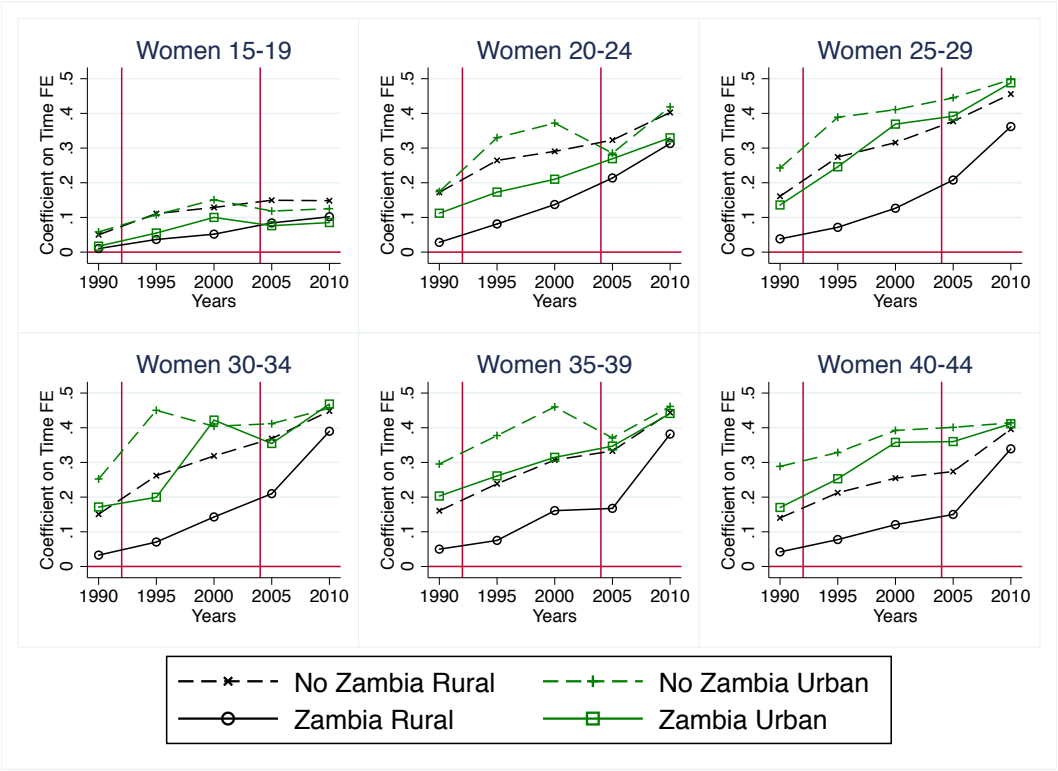
Figure 2.2: Event Studies for Injectable Use Across Age Groups



Notes: The graphs plot coefficients for γ_t^{zsa} from regression specification (2.2). The dependent variable in specification (2.2) is an indicator variable that takes value 1 if a woman is using injectables at survey date. All coefficients are shifted upward by the fraction of women using injectables in sample *zsa* in the first time period 1990–95 to capture differences in baseline usage by sample. Regression samples include all women 15–44 interviewed during 1990–2014. The left-most vertical red line corresponds to the 1992 legalization of injectable contraceptives in countries surrounding Zambia. The right-most vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

women "catch-up" and have similar rates of injectable use as control group countries by 2010–14. This pattern is particularly strong in rural areas, where pre-legalization use of injectables in Zambia is minimal, and for women of prime childbearing ages (20–39), where take-up is particularly strong. Rural women aged 25–39 show similar patterns for modern contraceptives. In contrast, urban women in Zambia and control group countries appear to be on the same upwards trend in modern contraceptive use throughout 1990–2014.

Figure 2.3: Event Studies for Modern Contraceptive Use Across Age Groups



Notes: The graphs plot coefficients for γ_t^{zsa} from regression specification (2.2). The dependent variable in specification (2.2) is an indicator variable that takes value 1 if a woman is using modern contraceptives at survey date. All coefficients are shifted upward by the fraction of women using modern contraceptives in sample zsa in the first time period 1990-95 to capture differences in baseline usage by sample. Regression samples include all women 15–44 interviewed during 1990–2014. The left-most vertical red line corresponds to the 1992 legalization of injectable contraceptives in countries surrounding Zambia. The right-most vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

Together the event studies for injectables and modern contraceptives confirm that injectable legalization has different impacts in rural and urban areas. In rural areas, injectable legalization is consistent with a shock to *modern contraceptive access as a whole* given that both injectable use and modern contraceptive use sharply increase post-legalization. In urban areas, injectable legalization is instead consistent with a shock to *access to different methods* of contraception and is only a minor shock to access to modern contraceptives — urban Zambian women use modern contraceptives at similar rates as women in control countries despite limited access to injectables during the 1990s.

2.6.2 Contraceptive Access & Traditional Methods

Strong take-up of modern contraceptives post-legalization in Zambia is accompanied by women *reducing reliance* on traditional pre-pregnancy and post-pregnancy methods of fertility control. Rural women have the strongest substitutions away from traditional methods, increasing coital frequency (12.7%), and decreasing reliance on abortions (-35%) and postpartum methods of fertility control. In contrast, urban women only decrease use of postpartum methods. Reductions in abortion use are driven by women older than 30 and by women with more than four live births. These results confirm Predictions 2 & 6 of our model.

Methods

We use two different data sets and two different regression specifications to study three classes of traditional methods of fertility control: coital frequency, abortions, and postpartum behaviors and outcomes. Below, we discuss the data and regression specifications we use for each method of traditional fertility control.

Coital Frequency: We study how coital frequency changes in response to improved contraceptive access by estimating specification (2.1) on our *repeated cross-section* dataset. We measure coital frequency using an indicator variable that takes value one if a woman reports having had sexual intercourse during the week prior to survey date. As this information is

only collected for sexually active women, we restrict our sample to women who report ever having had sexual intercourse. As robustness, we also examine alternate measures of coital frequency including sexual intercourse during the month prior to survey date and initiation into sexual activity.

Abortions: We study how abortion use changes in response to improved contraceptive access by estimating specification (2.1) on our *repeated cross-section* dataset. We measure abortion use with an indicator variable that takes value one if a woman reports ever having had a terminated pregnancy at survey date. As asked, terminated pregnancies include miscarriages, stillbirths, and abortions.[48] By bundling responses for all types of terminated pregnancies, the survey question minimizes underreporting of abortions due to stigma but makes it difficult to isolate changes in abortions independent of changes in miscarriages and stillbirths. In particular, if women idiosyncratically vary in their ability to carry pregnancies to term and the composition of women choosing to have pregnancies changes in response to contraceptive legalization, then reductions in reports of ever having had a terminated pregnancy can be driven by *both* reductions in abortions *and* reductions in the fraction of pregnant women who experience miscarriages. Given that miscarriage rates are highest in the first seven weeks, and that first trimester miscarriages are the least likely to be self-reported by women[49] it is unlikely that any changes we find in reports of ever having had a terminated pregnancy are driven solely by changes in reported miscarriages.

Postpartum Variables: We study postpartum breastfeeding, abstinence, amenorrhea, and protection from pregnancy risk using our *postpartum panel*, which addresses the censoring of postpartum duration variables by creating a panel for each live birth and weighting each month after a live birth where the child is alive equally. On this dataset, we estimate specification (2.1) with additional controls for different hazard rates for women in different age-groups and sectors — we include fixed effects for months since birth interacted with fixed

[48]Specifically, DHS survey questionnaires ask: "Have you ever had a pregnancy that miscarried, was aborted, or ended in a stillbirth?"

[49]Most of these miscarriages present as a late period with heavier menstrual flow.

effects for age-group and sector.[50] We assess the validity of the parallel trends assumption by providing event studies for postpartum variables using a similarly modified version of specification (2.2). For each of the postpartum outcomes we study, the dependent variable is an indicator variable taking value one in a given month since birth if a woman is still experiencing that postpartum outcome.

We estimate this specification on two samples — 1–12 months after a live birth, and 13–24 months after a live birth — to better capture substitution patterns between postpartum behaviors and modern contraception. We expect to see larger substitutions in the second year since birth relative to the first if: (a) postpartum behaviors have positive impacts on the health of both mother and child that are largest immediately following birth, decline in magnitude over time, and are unaffected by improvements in contraceptive access; and (b) the cost of persisting with a postpartum behavior is increasing in its duration.[51] Hence, pooling across both years since birth may mask reductions in the duration of postpartum behavior.

Results

Below, we present results for each class of traditional fertility control we study and confirm Prediction 2.

Coital Frequency: Sexually active Zambian women are 5.4 percentage points more likely to report having sexual intercourse within one week of survey date post-legalization (see Table 2.4), consistent with Prediction 2.[52] Consistent with Prediction 6, the change in sex-

[50] For a formal presentation of the regression specification, refer to Appendix E.2.

[51] In contrast to our hypothesis, Jayachandran (2014) provides evidence from Indonesia that breastfeeding and modern contraceptives are substitutes immediately post-birth, but are complements 13–24 months post-birth. She argues that this occurs because during the second year post-birth, contraception delays the onset of pregnancy and allows for longer breastfeeding durations (as many women wean when they become pregnant). In the Zambian context, this argument is less likely to be true because birth-spacing intervals were long prior to legalization: the mean birth-spacing interval among Zambian women 15–44 prior to legalization was 34.7 months, implying that on average women began a pregnancy resulting in a live birth 25 months after giving birth.

[52] Results are robust to the inclusion of fixed effects for marital status and educational attainment, through our preferred specification excludes these controls as they may be endogenous. Results are also robust to

ual behavior is driven by rural women, who are 6.2 percentage points (12.7%) more likely to report increased coital frequency post-legalization. Urban women show no substantial changes in coital frequency — point estimates are positive but smaller (1.8–3 percentage points). Point estimates are qualitatively consistent but smaller in magnitude when we examine coital frequency during the month prior to survey date (see Table C.2), suggesting that women are substituting away from both abstinence durations longer than a month and abstinence durations longer than a week but shorter than a month. There is no change in women's initiation into sexual activity in either rural or urban areas (see Table C.2).

Abortions: Zambian women are 5.3 percentage points less likely to report ever having a terminated pregnancy at survey date post-legalization of injectables (see Table 2.4).[53] Consistent with Prediction 6, the reduction in reports of ever experiencing a terminated pregnancy is driven by rural women, who are 6.1 percentage points (35%) less likely to report ever having had a terminated pregnancy post-legalization. Urban women show no substantial changes in ever experiencing a terminated pregnancy — point estimates are negative but only half as large as in rural areas (-3.1 percentage points). Consistent with Prediction 2, substitutions away from abortions are larger amongst rural women older than 30 (see Figure B.7), and amongst rural women with more than four children (see Figure B.9). Declines in abortion use are similar across levels of educational attainment (see Figure B.8).

dropping each control group country one at at time and to using a paired region regression specification (see Table C.3). Event studies for coital frequency (see Figure D.7) suggest that the parallel trends assumption holds (time trends for rural women in Zambia and control group countries are parallel and overlapping from 1990–2004) and confirm that our estimated treatment effects are driven by increases in coital frequency among rural Zambian women post-2004.

[53]Results are also robust to dropping one country at at time and to using a paired-region regression specification (see Table C.4), and to the inclusion of dummy variables for marital status and education in our specification. Event studies for ever reporting a terminated pregnancy (see Figure D.8) confirm that our estimated treatment effects are driven by decreases in reports of terminated pregnancies in Zambia post-legalization rather than any changes in control group countries. As questions on terminated pregnancies are only asked of women interviewed post-2000 and countries only have a single survey round during 2000–2004, it is not possible to assess time trends pre-legalization.

Table 2.4: Changes in Coital Frequency & Abortion Use

	All Areas		Rural Areas		Urban Areas	
Panel A: Changes in Coital Frequency						
Post Inj	0.0537**	0.0451**	0.0617**	0.0534**	0.0309	0.0183
	(0.0222)	(0.0202)	(0.0268)	(0.0243)	(0.0184)	(0.0135)
Obs.	66,757	66,213	46,996	46,559	19,761	19,654
Mean	0.542	0.542	0.557	0.557	0.507	0.507
Clusters	18	18	18	18	18	18
Controls	N	Y	N	Y	N	Y
Panel B: Changes in Ever-Use of Abortions						
Post Inj	-0.0531***	-0.0519***	-0.0607***	-0.0604***	-0.0316	-0.0265
	(0.0158)	(0.0151)	(0.0169)	(0.0163)	(0.0210)	(0.0202)
Obs.	63,622	63,581	44,421	44,390	19,201	19,191
Mean	0.112	0.112	0.118	0.118	0.0959	0.0959
Clusters	18	18	18	18	18	18
Controls	N	Y	N	Y	N	Y

Notes: The dependent variable in the regressions in Panel A is an indicator variable that takes value 1 if a woman reports having had sexual intercourse during the week prior to the interview date. The dependent variable in the regressions in Panel B is an indicator variable that takes value 1 if a woman reports ever having had a terminated pregnancy (miscarriage, stillbirths, and abortions) at survey date. The reported coefficient and standard errors are for the term β in regression (2.1). All regression samples are restricted to women 15–44 interviewed during 1996–2014. Controls include dummy variables for marital status and educational attainment. All standard errors are clustered at the region level. *** p<0.01, ** p<0.05, * p<0.1

Our results are robust to studying an alternate measure of abortion use — the fraction of pregnancies borne by a woman during the 5-6 years prior to interview that result in termination (see Table E.1). Specifically, by combining information on birth histories with information collected on the timing of each terminated pregnancy during the 5-6 years prior to survey date, we construct a *pregnancy panel* consisting of all pregnancies borne by a women during the 5-6 years prior to interview. We use this dataset to estimate specification (2.1) while flexibly controlling for recall error, finding that rural women are 32.2% less likely to report a pregnancy as terminated post-legalization. See Appendix E.4 for details.

Postpartum variables: Both rural and urban Zambian women show substantial reductions in the probability they are amenorrheic or protected from pregnancy risk in any given month during the second year post-birth (see Table 2.5).[54] Specifically, rural Zambian women are 7.4 percentage points (21%) and 8.9 percentage points (21%) less likely to be amenorrheic or protected from pregnancy risk in a given month during the second year post-birth, respectively. Urban women are 9.3 percentage points (36%) less likely to be amenorrheic and 9.8 percentage points (28%) less likely to be protected from pregnancy risk during the second year post-birth. These reductions in postpartum outcomes are driven by different behavioral changes in urban and rural areas — rural women are 3.8 percentage points (25%) less likely to be abstaining, while urban women are 8.7 percentage points (12%) less likely to be breastfeeding, in a given month during the second year post-birth. Women in both areas are likely reducing the intensity with which they breastfeed children, as evidenced by the reduction in duration of postpartum amenorrhea.

Reductions in the use of traditional methods are smaller in the first year post-birth, consistent with our expectations. Both rural and urban women experience declines in protection

[54] Our results are robust to the inclusion of dummy variables for marital status and educational attainment (see Table C.5), to excluding one control group country at a time (see Table C.6), and to using a paired-region regression specification. Event studies (see Figures D.9–D.14) confirm that estimated treatment effects are driven by reductions in the durations of postpartum outcomes in Zambia post-legalization and that Zambia and surrounding countries are on similar trends during 1996–2004. Assessing time trends prior to 1992 is not feasible as there are insufficient months with data from Zambia and four control group countries.

Table 2.5: Changes in Postpartum Behaviors

Panel A: 1–12 Months Post-Birth

	Rural Areas				Urban Areas			
	Breast-feeding	Postpartum Abstinence	Postpartum Amenorrhea	Postpartum Protection	Breast-feeding	Postpartum Abstinence	Postpartum Amenorrhea	Postpartum Protection
Post Inj	-0.0007	-0.0118	-0.0188	-0.0231**	-0.0134	0.0010	-0.0481**	-0.0374**
	(0.0099)	(0.0188)	(0.0110)	(0.0095)	(0.0181)	(0.0216)	(0.0227)	(0.0138)
Obs.	259,855	354,859	354,859	354,859	65,170	93,503	93,503	93,503
Mean	0.968	0.585	0.784	0.856	0.963	0.575	0.708	0.807
Clusters	18	18	18	18	18	18	18	18

Panel B: 13–24 Months Post-Birth

Post Inj	0.0182	-0.0380***	-0.0735***	-0.0886***	-0.0869**	-0.0111	-0.0925***	-0.0978***
	(0.0179)	(0.0125)	(0.0138)	(0.0148)	(0.0306)	(0.0299)	(0.0227)	(0.0312)
Obs.	80,956	109,717	109,717	109,717	21,100	30,503	30,503	30,503
Mean	0.765	0.132	0.298	0.370	0.715	0.134	0.228	0.316
Clusters	18	18	18	18	18	18	18	18

Notes: The dependent variable in these regressions is an indicator variable that takes value 1 if a woman is breastfeeding, abstaining, amenorrheic, or experiencing the postpartum protection period at a given month following a live birth. The reported coefficient and standard errors are for the term β in regression (E.2). All regression samples are restricted to information from women 15–44 during 1996–2010. No regressions include individual level controls. All standard errors are clustered at the region level. *** p<0.01, ** p<0.05, * p<0.1

of pregnancy risk of 2.3 and 3.7 percentage points, respectively, reflecting only 27% and 38% of the reduction in use during the second year post-birth. While the sign of point estimates are similar, women in rural areas no longer significantly reduce reliance on postpartum abstinence and no longer experience significantly reduced periods of postpartum amenorrhea. Similarly urban women do not significantly reduce breastfeeding duration.

2.6.3 Contraceptive Access & Realized Fertility

Using both across-women and within-woman difference-in-difference techniques on our *birth-history panel* and *birth-spacing panel* datasets, we find that rural women post-2004 increase fertility and decrease birth-spacing, while urban women do not. Fertility increases post-legalization are confined to rural women younger than 45, with childless rural women older than 20 exhibiting particularly large increases in fertility. Importantly, neither short-term fertility for women 45–49, nor completed fertility (i.e., women's stock of live births when 45–49) increase, consistent with contraceptive access inducing women to have births earlier. Taken together, these results are consistent with Predictions 3–5 and Prediction 7 of our model.

Methods

We first estimate fertility responses to injectable legalization on our yearly-frequency birth history dataset using specification (2.1) with flow fertility as the dependent variable. Specifically, y is an indicator variable taking value one for woman i in year t if she reports a live birth during that year. We present results both with and without controls for marital status, years of schooling, and parity. We restrict our regression sample to all women in our sample observed during the years 1996–2010 to ensure a consistent group of control group countries in all years.

We then estimate treatment effects unbiased by any confounding factors that are woman-specific and time-invariant (e.g., a target fertility) using an *individual fixed effects* estimation

strategy:

$$y_{isbt} = \alpha + \beta T_{isbt} + \gamma_{st} + \lambda_b + \xi X_{isbt} + \phi_i + \varepsilon_{isbt} \tag{2.3}$$

where i indexes individuals, s indexes urban or rural place of residence, b indexes age in years, and t indexes years. The dependent variable and treatment variable in specification (2.3) are the same as in specification (2.1). We present results with and without controls for time-varying individual determinants of fertility such as martial status and parity.[55] We identify β only using comparisons of women in the same sector by interacting time fixed effects with urbanity, and we flexibly control for a woman's age-profile of flow fertility using fixed effects for age in years, λ_b. We restrict our regression sample to only include women interviewed post-2005 — for whom we have information from before and after injectable legalization in Zambia — and use information from these women corresponding to the years 1996–2010.

Identification of β in specification (2.3) comes from examining a rural (urban) Zambian woman at two points in time, one after injectable legalization and one before, and comparing her to a rural (urban) woman from outside of Zambia with the same birth-year at the same two points in time. This identification strategy is valid provided that women in Zambia and control group countries would have had parallel time trends in flow fertility in the absence of treatment. We provide evidence for this by constructing subsamples zs of our data disaggregated by Zambia z and sector s and estimating:

$$y_{ibt}^{zs} = \alpha^{zs} + \gamma_t^{zs} + \lambda_b^{zs} + \phi_i^{zs} + \varepsilon_{ibt}^{zs} \tag{2.4}$$

where i indexes individuals, b indexes age in years, and t indexes year. We estimate specification (2.4) on all women in our sample observed during 1985–2010; our event studies plot the coefficients γ_t^{zs}.

[55] Measure of education in our sample is time-invariant, and captured by individual fixed effects.

We assess the impact of injectable legalization on birth-spacing using estimation strategies analogous to specifications (2.1) and (2.3) on our *birth-spacing panel* that additionally control for differential hazard rates by sector and age — we include fixed effects for months since last birth interacted with fixed effects for sector and age. The dependent variable in both specifications is an indicator taking value one if a woman reports a live birth in a given month. We estimate how the probability of giving birth changes post-legalization separately for each of the seven years following a live birth to determine how birth-spacing changes.[56] Hence, *increases* in probability of giving birth post-legalization in a given year post-birth correspond to *decreases* in birth-spacing. For formal regression equations, see Appendix E.3.

Results

Estimates from across-woman and within-woman specifications (see Table 2.6) both show *increases in flow fertility* post-legalization of 2.1–2.5 percentage points (9.6–11.2%), consistent with Prediction 3.[57] Fertility increases are driven solely by rural women, who increase fertility by 2.8–3.1 percentage points (11.2–12.1%). In contrast, urban women show no change in fertility: point estimates are slightly positive and statistically indistinguishable from zero. These heterogeneous responses by sector are consistent with Prediction 7. Fertility increases in rural areas are consistent among women older than 20 (see Figure B.10), among women with more than one child (see Figure B.12), and among women with fewer than 8 years of completed schooling (see Figure B.11).[58] As women 15–19 are the most likely to be childless

[56] We assume that women cannot have a subsequent birth during the nine months immediately following a live birth: the first "year" after a live birth corresponds to 9–20 months post-birth, etc.

[57] Fertility responses are robust to excluding one control group country at at time (see Table C.7), to the inclusion of controls for education, parity, and marital status, and to estimating treatment effects using paired region specifications (see Table C.8). Event studies using both specification (2.2) and specification (2.4) confirm that: (a) women in Zambia and surrounding countries have parallel time trends in flow fertility prior to 1992; and (b) positive fertility responses in rural areas are driven by increases in flow fertility in Zambia post-2004 (see Figures D.15 – D.18). Identification appears particularly strong in rural areas, where time trends of flow fertility overlap and move synchronously in response to minor shocks prior to 1992.

[58] Assessing heterogeneous fertility responses among women in subgroups based on ever-use of modern contraceptives is highly endogenous, leading us to cut our data along age, parity, and educational dimensions. One concern with our method is that the subgroup of women who are unwilling to use contraceptives — a group that constitutes a larger share of never-users post-legalization — increase fertility in the years following injectable legalization, resulting in our aggregated estimates masking fertility declines among women who are

Table 2.6: Fertility Responses For Women 15 – 44

Panel A: Across-Women Fertility Responses

	All Areas		Rural Areas		Urban Areas	
Post Inj	0.0213***	0.0250***	0.0281***	0.0312***	0.0040	0.0097
	(0.0061)	(0.0061)	(0.0055)	(0.0055)	(0.0077)	(0.0080)
Obs.	525,528	525,221	372,202	371,986	153,326	153,235
Mean	0.211	0.211	0.229	0.229	0.167	0.167
Clusters	18	18	18	18	18	18
Controls	N	Y	N	Y	N	Y

Panel B: Within-Woman Fertility Responses

Post Inj	0.0250***	0.0439***	0.0311***	0.0509***	0.0090	0.0247**
	(0.0063)	(0.0099)	(0.0064)	(0.0103)	(0.0069)	(0.0090)
Obs.	417,976	417,976	288,419	288,419	129,557	129,557
# Women	44,227	44,227	30,242	30,242	13,985	13,985
Mean	0.211	0.211	0.231	0.231	0.168	0.168
Clusters	18	18	18	18	18	18
Controls	N	Y	N	Y	N	Y

Notes: The dependent variable in all regressions is an indicator variable that takes value 1 if a woman reports having a live birth in a given month. *Panel A* reports coefficients and standard errors for β in regression (2.1). Controls include dummy variables for educational attainment, for ever having been married in a given year, and for parity in a given year. Regression samples are restricted to women 15–44 observed during 1996–2010. *Panel B* reports coefficients and standard errors for β in regression (2.3). Controls include dummy variables for whether or not a woman has ever been married in a given year and for parity in a given year. Regression samples are restricted to women 15–44 interviewed post-2005 observed during 1996–2010. All standard errors are clustered at the region level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

and to have more than 9 years of completed schooling, these results suggest that improved modern contraceptive access eliminated precautionary motives to delay births for women during their prime childbearing years. Adolescent women have additional incentives to delay births (i.e., human capital formation) that are not eroded by improved access to contraceptives.

Similarly, both across-woman and within-woman specifications for birth-spacing (see Tables 2.7 & 2.8) confirm that rural, but not urban, women are reducing birth-spacing durations, consistent with Predictions 3 & 7. For example, rural women are 0.76–0.81 percentage points (17.9–18.5%) more likely to have a subsequent birth in any given month during their second year (21–32 months) post-birth; increases for urban women during their second year post-birth are insignificant and one-tenth in magnitude. These reductions in birth-spacing provide direct evidence that women are less likely to delay childbirth. Moreover, increases in the likelihood of having a subsequent birth during the first four years post-birth suggest that fertility increases are not being driven by either childless women entering childbearing or women who had been avoiding childbirth for lengthy periods of time (and, hence, may be selected on some unobservable dimension) choosing to have an additional child.

Childless women eschew modern contraceptives and increase fertility (see Table 2.9), consistent with Prediction 4. In particular, rural childless women aged 20 and older show no take-up of injectable contraceptives (point estimates are negative and insignificant) and increase flow fertility by 4.5 percentage points, nearly double the increase for rural women as a whole. Results are qualitatively similar, but slightly attenuated when we include childless women aged 15–19, consistent with adolescent girls having additional incentives to delay childbearing due to tradeoffs between entering childbearing and human capital formation.

Consistent with Predictions 3 & 5, fertility increases are confined to rural women younger than 45 (see Figure B.10), and do not increase among women aged 45–49 (see Table 2.10).

willing to use modern contraceptives. Time trends for subsamples of Zambian women defined by ever-use of modern contraceptives within five years of survey date reveals this to be false as contraceptive users display fertility increases post-legalization.

Table 2.7: Across-Women Birth-Spacing Responses For Women 15 – 44

	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7
Panel A: Rural Women							
Post Inj	.0033***	.0076***	.0077***	.0061***	.0009	.0063***	.0010
	(.0006)	(.0020)	(.0020)	(.0017)	(.0019)	(.0015)	(.0029)
Obs.	978,223	735,720	397,532	215,287	132,831	89,874	62,743
Mean	.0086	.0356	.0488	.0376	.0276	.0202	.0152
Clusters	18	18	18	18	18	18	18
Panel B: Urban Women							
Post Inj	-.0012	.0024	.0057**	.0030	.0039	-.0009	-.0026
	(.0011)	(.0027)	(.0024)	(.0022)	(.0023)	(.0029)	(.0022)
Obs.	295,396	236,086	150,640	96,028	65,545	47,182	34,231
Mean	.0068	.0260	.0332	.0262	.0195	.0160	.0119
Clusters	18	18	18	18	18	18	18

Notes: The dependent variable in all regressions is an indicator variable that takes value 1 if a woman reports having a live birth in a given month. "Year 1" corresponds to 9–20 months post-birth; "Year 2" corresponds to 21–32 months post-birth, etc. All reported coefficients and standard errors for β in regression (E.3). Controls include dummy variables for educational attainment, for ever having been married in a given year, and for parity in a given year. Regression samples are restricted to women 15–44 observed during 1996–2010. All standard errors are clustered at the region level.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.8: Within-Woman Birth-Spacing Responses For Women 15 – 44

Panel A: Rural Women

	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7
Post Inj	.0030***	.0081**	.0087*	.0192***	.0098	.0208**	.0003
	(.0007)	(.0029)	(.0042)	(.0045)	(.0079)	(.0078)	(.0088)
Obs	742,457	556,371	301,976	163,019	99,421	66,008	46,084
# Women	22836	21566	18650	13283	8864	6149	4360
Mean	.0088	.0357	.0481	.0379	.0281	.0213	.0155
Clusters	18	18	18	18	18	18	18

Panel B: Urban Women

Post Inj	-.0004	.0008	.0064	.0027	-.0023	-.0101	-.0037
	(.0010)	(.0040)	(.0062)	(.0067)	(.0078)	(.0156)	(.0181)
Obs.	242,655	192,902	123,616	78,737	53,544	38,128	27,937
# Women	9094	8564	7581	6050	4550	3461	2581
Mean	.0070	.0259	.0328	.0268	.0202	.0169	.0129
Clusters	18	18	18	18	18	18	18

Notes: The dependent variable in all regressions is an indicator variable that takes value 1 if a woman reports having a live birth in a given month. "Year 1" corresponds to 9–20 months post-birth; "Year 2" corresponds to 21–32 months post-birth, etc. The reported coefficients and standard errors for β in specification (E.4). Regression samples are restricted to women 15–44 interviewed post-2005 observed during 1996–2014. Controls include dummy variables for whether or not a woman has ever been married in a given year and fixed effects for parity in a given year. All standard errors are clustered at the region level. *** p<0.01, ** p<0.05, * p<0.1

Table 2.9: Contraceptive Adoption & Fertility Responses for Childless Women

Panel A: Childless Women Age 20 and Older

	Rural Areas				Urban Areas			
	Injectables		Flow Fertility		Injectables		Flow Fertility	
Post Inj	-0.0055	-0.0044	0.0453***	0.0527***	0.0246	0.0251	0.0083	0.0111
	(0.0111)	(0.0102)	(0.0145)	(0.0102)	(0.0165)	(0.0171)	(0.0221)	(0.0177)
Obs.	2,339	2,309	22,483	22,466	2,449	2,433	17,231	17,217
Mean	0.0180	0.0180	0.272	0.272	0.0196	0.0196	0.175	0.175
Clusters	18	18	18	18	18	18	18	18
Controls	N	Y	N	Y	N	Y	N	Y

Panel B: All Childless Women

Post Inj	0.0050	0.0044	0.0129*	0.0249***	0.0083	0.0084	-0.0017	0.0106
	(0.0030)	(0.0032)	(0.0066)	(0.0045)	(0.0063)	(0.0065)	(0.0085)	(0.0070)
Obs.	11,701	11,597	99,841	99,753	7,305	7,262	55,616	55,579
Mean	0.0109	0.0109	0.180	0.180	0.0118	0.0118	0.132	0.132
Clusters	18	18	18	18	18	18	18	18
Controls	N	Y	N	Y	N	Y	N	Y

Notes: The dependent variable in regressions for injectable use is an indicator variable that takes value 1 if a woman reports using injectables at survey date. Regressions are estimated using our *repeated cross-section* dataset on samples consisting of childless women aged 15–44 interviewed during 1996–2014. Controls include dummy variables for marital status and educational attainment. The dependent variables in regressions for flow fertility is an indicator variable that takes value 1 if a woman reports having a live birth in a given year. Regressions are estimated using our *birth-history panel* on samples consisting of childless women aged 15–44 observed during 1996–2010. Controls include dummy variables for ever having been married in a given year, educational attainment at survey date, and parity in a given year. All standard errors are clustered at the region level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Specifically, rural women over 45 increase use of injectables by 2.7 percentage points (329%) post-legalization, have no changes in flow fertility, and have negative (insignificant) point estimates for completed fertility (i.e., their stock of live births). That completed fertility has negative point estimates suggests, as in our model, that fertility increases in response to modern contraceptives reflect changes in the timing of births across the lifecycle (i.e., *tempo* effects) rather than changes in the total number of births (i.e., *quantum effects*). More speculatively, these results suggest that by eliminating the risk of exceeding target fertilities, improved contraceptive access reduces fertility in the long-run.

Taken together, these results confirm that improved modern contraceptive access can cause positive short-run fertility responses by relaxing precautionary motives to delay having children.

2.7 Conclusion

In this paper, we theoretically and empirically investigate the impact of a broad expansion of contraceptive type and availability in Zambia. By building a lifecycle model of fertility control choice, we highlight that improved contraceptive access can change women's intertemporal fertility incentives, leading them to shift births earlier in the lifecycle. Expanded contraceptive access improves women's ability to restrict unwanted births, resulting in: (a) increases in flow, but not completed, fertility as women with intermediate target fertilities substitute away from traditional fertility control methods and no longer delay wanted births to avoid exceeding target fertilities; and (b) decreases in flow and completed fertility as women who were already at their target fertilities can better restrict unwanted births. When sufficiently many women have precautionary motives to delay childbearing, modern contraceptive access leads to increases in flow, but not completed, fertility. We use our model to generate different predictions for substitution and fertility responses for women in urban and rural sectors, for women at different ages, and for women at different parities.

Table 2.10: Contraceptive Adoption & Fertility Responses for Women 45–49

	Injectable Adoption		Completed Fertility		Flow Fertility	
Panel A: Rural Women						
Post Inj	0.027***	0.026***	-0.242	-0.259	0.0035	0.0020
	(0.008)	(0.008)	(0.206)	(0.195)	(0.0115)	(0.0110)
Obs.	4,082	4,048	4,185	4,151	6,115	6,115
Mean	0.0394	0.0394	7.188	7.188	0.0366	0.0366
Clusters	18	18	18	18	18	18
Controls	N	Y	N	Y	N	Y
Panel B: Urban Women						
Post Inj	0.032	0.024	-0.424	-0.247	-0.0064	-0.0096
	(0.024)	(0.021)	(0.326)	(0.267)	(0.0145)	(0.0153)
Obs.	1,314	1,306	1,333	1,325	1,576	1,576
Mean	0.0419	0.0419	6.385	6.385	0.0133	0.0133
Clusters	18	18	18	18	18	18
Controls	N	Y	N	Y	N	Y

Notes: The dependent variable in regressions for injectable use is an indicator variable that takes value 1 if a woman reports using injectables at survey date. The dependent variable in regressions for completed fertility is the stock of live births a woman reports at survey date. Regressions are estimated using our *repeated cross-section* dataset on samples consisting of women aged 45–49 interviewed during 1996–2014. Controls include dummy variables for marital status and educational attainment. The dependent variables in regressions for flow fertility is an indicator variable that takes value 1 if a woman reports having a live birth in a given year. Regressions are estimated using our *birth-history panel* on samples consisting of women aged 45–49 observed during 1996–2010. Controls include dummy variables for ever having been married in a given year, educational attainment at survey date, and parity in a given year. All standard errors are clustered at the region level.
*** p<0.01, ** p<0.05, * p<0.1

We empirically confirm that women in Zambia responded to improved contraceptive access by adopting modern contraceptives, substituting away from traditional methods of fertility control, and increasing short-run fertility. Consistent with the predictions of our model, we find that: (a) adoption of modern contraceptives is higher for women at higher parities; (b) substitution away from traditional methods is stronger in rural areas; (c) reductions in abortion use are stronger among older women and women with higher parities; (d) increases in fertility (and decreases in birth-spacing) occur only for rural women younger than 45; (e) women older than 45 do not increase fertility; (f) childless women eschew modern contraceptives in favor of increasing fertility; and (g) completed fertility does not increase.

Our results highlight the importance of evaluating contraceptive interventions using life-cycle models of fertility choice, and can be used to reconcile the differential impacts of improved contraceptive access on completed and short-run fertility found in the literature. In particular, our results suggest that the ambiguous short-run relationship may be driven by intertemporal incentives to increase births dominating immediate gains in women's ability to limit unwanted births. Finally, the substitution patterns we observe — both intertemporally and across methods of fertility control — suggest that contraceptive access is *welfare improving* for women, despite having counterintuitive implications for fertility.

A Mathematical Appendix

A.1 Formal Presentation of Main Model

World Without Modern Contraceptives

Recall the maximization problems in periods $t = 1, 2, 3$ in a world without modern contraceptives:

$$V_t(\gamma_{t-1}^i) = \max_{b\in[0,1]} \left\{ \bar{u} - \phi(b) + p(b) \max \left\{ V_{t+1}(\gamma_{t-1}^i + 1), V_{t+1}(\gamma_{t-1}^i) - c_i \right\} \right.$$
$$\left. + (1 - p(b)) V_{t+1}(\gamma_{t-1}^i) \right\}$$

where $V_4(\gamma_2^i) \equiv U(\gamma_2^i)$. The above maximization problem can be rewritten as:

$$V_t(\gamma_{t-1}^i) = \bar{u} + V_t(\gamma_{t-1}^i, c_i) - \min_{b\in[0,1]} \left\{ \phi(b) + p(b) \min \left\{ V_t(\gamma_t^i, c_i) - V_t(\gamma_{t-1}^i + 1, c_i), c_i \right\} \right\}$$

We define

$$\tilde{b}(\alpha) = \operatorname*{argmin}_{b\in[0,1]} \{\phi(b) + \alpha p(b)\} \quad \forall \alpha \in \mathbb{R}$$

$$B(\alpha) = \min_{b\in[0,1]} \{\phi(b) + \alpha p(b)\} \quad \forall \alpha \in \mathbb{R}$$

and place the following restrictions on cost functions and probabilities.

Assumption 2. *We assume the following properties about payoffs and probabilities:*

1. *The functions $\phi(\cdot)$, $r(\cdot)$ and $p(\cdot)$ are all twice continuously differentiable.*
2. *The cost functions $\phi(\cdot)$ and $r(\cdot)$ are strictly increasing and strictly convex. The probability $p(\cdot)$ is strictly decreasing and strictly convex.*

With this notation, we characterize women's optimal behavior when they do not have access to modern contraceptives.

Lemma 1. *Let Assumption 1 hold. Then,*

1. *Both $\tilde{b}(\alpha)$ and $\tilde{B}(\alpha)$ are well-defined and increasing in α. $\tilde{b}(\alpha)$ is strictly increasing in α for $\alpha \geq 0$.*

2. *Women's optimal choice of abortions and traditional methods of fertility control satisfy:*

$$a_t^*(\gamma_{t-1}^i, c_i) = \mathbb{1}_{\{c_i \leq V_{t+1}(\gamma_t^i) - V_{t+1}(\gamma_t^i+1)\}} \quad \forall \;\; t \in \{1,2,3\}$$
$$b_t^*(\gamma_1^i, c_i) = \tilde{b}\left(c_i \wedge \left[V_{t+1}(\gamma_{t-1}^i) - V_{t+1}(\gamma_{t-1}^i+1)\right]\right) \quad \forall \;\; t \in \{1,2,3\}$$

Given this optimization solution, we can characterize women's precautionary motives to delay fertility.

Definition 1. *A woman has a precautionary motive to delay fertility in period t whenever she desires at least one more child in period t (i.e., $\gamma_{t-1}^i < 0$), but chooses $b_t^*(\gamma_{t-1}^i, c_i) > 0$.*

Lemma 2. *Let Assumption 1 hold. Then in period t, no women with $\gamma_{t-1}^i \leq t-4$ has a precautionary motive to delay fertility. Moreover, there exist scalars q_1, q_2, and q_3 with $q_1 < q_2 < q_3$ and threshold abortion cost values c_1^*, c_2^*, and c_1^+ with $c_1^* < c_2^* < c_1^+$ such that*

1. *When $q_1 > \bar{p}$, no women have a precautionary motive to delay having children and women's fertility behavior is identical to behavior in a model of repeated static fertility choice.*

2. *When $\bar{p} > q_2$, women with $\gamma_1^i = -1$ and $c_i > c_2^*$ or women with $\gamma_0^i = -1$ and $c_i > c_1^*$ delay having wanted children in periods 2 and 1, respectively.*

3. *When $\bar{p} > q_3$, women with $\gamma_1^i = -1$ and $c_i > c_2^*$ delay having wanted children in period 2. Similarly, women with $\gamma_0^i = -1$ and $c_i > c_1^*$ or women with $\gamma_0^i = -2$ and $c_i > c_1^+$ delay having wanted children in periods 1.*

Denote by $N(t,\gamma)$ the fraction of women at time t with for whom $\gamma_{t-1}^i = \gamma$ and observe that $N(1,-x) = N_x$ for $x \in \{0,1,2,3\}$. Below, we analyze women's fertility behavior in the extreme case when $\bar{p} > q_3$ (and, hence, the largest fraction of women delay wanted births).

Corollary 2. *Let Assumption 1 hold and let $\bar{p} > q_3$. Define*

$$g(c_i,\gamma,t) \equiv p\left(\tilde{b}\left(c_i \wedge [V_{t+1}(\gamma) - V_{t+1}(\gamma+1)]\right)\right)$$

Then at time t, the fraction of women having abortions, A_t, is:

$$\begin{aligned} A_1 =& N_0 \int_0^{d_1} p(\tilde{b}(c_i))dF \\ A_2 =& \sum_{\gamma=0}^{1} N(2,\gamma) \int_0^{d_2(\gamma)} p(\tilde{b}(c_i))dF \end{aligned}$$

with $d_1 = V_2(0,\Delta r(2)) - V_2(1,\Delta r(3))$ and $d_2(\gamma) = V_3(\gamma,\Delta r(\gamma)) - V_3(\gamma+1,\Delta r(\gamma+1))$. The fraction of women using traditional methods, T_t, is:

$$\begin{aligned} T_1 =& N_0 + N_1(1-F(c_1^*)) + N_2(1-F(c_1^+)) \\ T_2 =& N(2,1) + N(2,0) + N(2,-1)(1-F(c_2^*)) \end{aligned}$$

and the fraction of women giving birth, F_t, is:

$$\begin{aligned} F_1 =& N_0 \int_{d_1}^{\infty} g(c_i,0,1)dF + N_1 \int_{c_1^*}^{\infty} g(c_i,-1,1)dF + N_2 \int_{c_1^+}^{\infty} g(c_i,-2,1)dF \\ &+ (N_1 F(c_1^*) + N_2 F(c_1^+) + N_3)\bar{p} \\ F_2 =& N(2,1) \int_{d_2(1)}^{\infty} g(c_i,1,2)dF + N(2,0) \int_{d_2(0)}^{\infty} g(c_i,0,2)dF \\ &+ N(2,-1) \int_{c_2^*}^{\infty} g(c_i,-1,2)dF + (N(2,-3) + N(2,-2) + N(2,-1)F(c_2^*))\,\bar{p} \end{aligned}$$

World With Modern Contraceptives

When women have access to modern contraceptives, their maximization problems change:

$$V_t^I(\gamma_{t-1}^i, c_i) = \bar{u} + V_{t+1}^I(\gamma_{t-1}^i, c_i) - \min\left\{\bar{\phi}, B\left(c_i \wedge \left[V_{t+1}^I(\gamma_{t-1}^i, c_i) - V_{t+1}^I(\gamma_{t-1}^i + 1, c_i)\right]\right)\right\}$$

We solve for women's optimal decisions when they have access to modern contraceptives. The size of the disutility of modern contraceptives, $\bar{\phi}$, is an important determinant of women's behaviors. We first consider the case when the cost of contraceptives is low; then we consider a case where the cost of contraceptives is high.

Assumption 3. *Assume that* $\bar{\phi} = 0$.

Denote by c^I the threshold value of abortions costs satisfying $\bar{\phi} = B(c^I)$.

Lemma 3. *Let Assumption 2 & 3 hold. Then,* $c^I = 0$*, and women's optimal choice of abortions and traditional methods of fertility control satisfy:*

$$\begin{aligned}
a_t^{I,*}(\gamma_{t-1}^i, c_i) =& \mathbb{1}_{\{c_i \leq V_{t+1}(\gamma_t^i) - V_{t+1}(\gamma_t^i + 1)\}} \quad \forall \;\; t \in \{1,2,3\} \\
m_t^{I,*}(\gamma_{t-1}^i, c_i) =& \mathbb{1}_{\{\gamma_{t-1}^i \geq 0\}} \; \mathbb{1}_{\{c_i \geq c^I\}} \quad \forall \;\; t \in \{1,2,3\} \\
b_t^{I,*}(\gamma_{t-1}^i, c_i) =& \mathbb{1}_{\{\gamma_{t-1}^i \geq 0\}} \; \mathbb{1}_{\{c_i < c^I\}} \tilde{b}\left(c_i\right) \quad \forall \;\; t \in \{1,2,3\}
\end{aligned}$$

Moreover, if $\bar{p} > q_3$*, modern contraceptive access eliminates any precautionary motives for delaying children.*

Women's optimal behavior with modern contraceptives allows us to derive expressions for the fraction of women having abortions, the fraction of women using traditional methods, and the fraction of women giving birth in any given period.

Corollary 3. *Let Assumptions 2 & 3 hold. Denote by* $N^I(t, \gamma)$ *the fraction of women at time* t *with for whom* $\gamma_{t-1}^i = \gamma$ *and observe that* $N^I(1, -x) = N_x$ *for* $x \in \{0,1,2,3\}$*. Then at time* t*, the fraction of women having abortions,* A_t*, is* $A_1^I = A_2^I = 0$*; the fraction of women*

using traditional methods, T_t, is $T_1^I = T_2^I = 0$; and the fraction of women giving birth, F_t, is:

$$F_1^I = (N_1 + N_2 + N_3)\bar{p}$$
$$F_2^I = [N^I(2,-1) + N^I(2,-2) + N^I(2,-3)]\bar{p}$$

In contrast, when the cost of modern contraceptives is sufficiently high, women's optimal behavior remains unchanged from the world without modern contraceptives.

Corollary 4. *Let Assumption 1 hold. Suppose $\bar{\phi} > r(3)$. Then, no one adopts modern contraceptives and women's fertility behavior is unchanged.*

Injectable Legalization

To derive testable empirical predictions from our model, we consider an extension of our model where cohorts of women age through the lifecycle to more accurately capture the reality of injectable legalization in Zambia. Prior to modern contraceptive access, $\bar{N}$ women are alive in each of the three fertile periods in our model, corresponding to different cohorts. When we move to the next time period, the $\bar{N}$ women at the end of their fertile lives exit the model, and a new cohort of $\bar{N}$ women enter their childbearing year ($t = 1$) with new draws of n_i and c_i, ensuring that there exists a complete age-profile of women during each period.

We consider how women's fertility behavior changes when injectables are legalized by assuming that injectable legalization occurs between two periods. Thus, women at $t = 3$ prior to legalization will see no benefits, while women at $t = 1$ prior to legalization will be able to adjust their behavior and women newly entering their fertile life will have access to modern contraceptives during all periods.

Proposition 3. *Let Assumptions 1 & 2 hold. Recall the notation $N(t,\gamma)$ and the expressions for these quantities derived in the proof of Corollary 2. Define*

$$g(c_i,\gamma,t) \equiv p\left(\tilde{b}\left(c_i \wedge [V_{t+1}(\gamma+1) - V_{t+1}(\gamma)]\right)\right)$$

Legalizing injectables increases use of modern contraceptives, decreases use of abortions,

$$\begin{aligned} 0 > A_1^I - A_1 &= -A_1 \\ 0 > A_2^I - A_2 &= -A_2 \\ 0 > A_3^I - A_3 &= -A_3 \end{aligned}$$

decreases use of traditional methods of fertility control:

$$\begin{aligned} 0 > T_1^I - T_1 &= -T_1 \\ 0 > T_2^I - T_2 &= -T_2 \\ 0 > T_3^I - T_3 &= -T_3 \end{aligned}$$

and has ambiguous effects on fertility during the first two periods:

$$\begin{aligned} F_1^I - F_1 = {} & N_1\left[\bar{p}(1-F(c_1^*)) + \int_{c_1^*}^{\infty}(\bar{p} - g(c_i,-1,1))dF\right] \\ & + N_2\left[\bar{p}(1-F(c_1^+)) + \int_{c_1^+}^{\infty}(\bar{p} - g(c_i,-2,1))dF\right] - N_0\int_{d_1}^{\infty} g(c_i,0,1)dF \\ F_2^I - F_2 = {} & N(2,-1)\left[\bar{p}(1-F(c_2^*)) + \int_{c_2^*}^{\infty}(\bar{p} - g(c_i,-1,2))dF\right] \\ & - N(2,0)\int_{d_2(0)}^{\infty} g(c_i,0,2)dF - N(2,1)\int_{d_2(1)}^{\infty} g(c_i,1,2)dF \\ F_3^I - F_3 = {} & -N(3,0)\int_{r(1)}^{\infty} g(c_i,0,3)dF - N(3,1)\int_{r(2)-r(1)}^{\infty} g(c_i,1,3)dF \\ & - N(3,2)\int_{r(3)-r(2)}^{\infty} g(c_i,2,3)dF \end{aligned}$$

Proposition 3 characterizes how the fraction of women using abortions and traditional methods, and the fraction of women having births, changes over the lifecycle in response to modern contraceptive legalization. It allows us to generate empirical predictions for how these quantities change over the lifecycle:

Corollary 5. *Reductions in abortion use are larger at older ages:*

$$A_3^I - A_3 < A_2^I - A_2 < A_1^I - A_1 < 0$$

Corollary 6. *For N_0 small, fertility increases for younger ages ($t = 1$) in response to injectable legalization. When, additionally, $N(2,-1)$ is small relative to $N(2,0)$, fertility also increases for older ages ($t = 2$) in response to injectable legalization.*

Corollary 7. *Fertility decreases for women in their terminal fertile period ($t = 3$).*

Corollary 8. *Women's expected completed fertility declines.*

A.2 Proofs

Proof of Lemma 1: Weierstrass' Extreme Value Theorem guarantees existence of $\tilde{b}(\alpha)$ and $B(\alpha)$. That $\tilde{b}(\alpha)$ is increasing in α follows from the increasing convexity of ϕ and the decreasing convexity of p. In particular, $\tilde{b}(\alpha) = 0$ for all $\alpha \leq 0$, and satisfies

$$-\frac{\phi'(\tilde{b}(\alpha))}{p'(\tilde{b}(\alpha))} = \alpha$$

for all $\alpha > 0$. The expression $-\phi'(b)/p'(b)$ is increasing in b; as α increases, the optimal choice of b must also strictly increase to ensure the first-order condition is satisfied.

$B(\alpha)$ is increasing in α by the Envelope Theorem, applicable because both ϕ and p are twice continuously differentiable.

Deriving expressions for a woman's optimal decisions requires backward induction and algebra. We examine optimal policies in each period in turn, below.

In period 3, women abort a realized pregnancy if and only if $-c_i - r(|\gamma_2^i|) > -r(|\gamma_2^i + 1|)$ or, equivalently, if and only if

$$c_i < r(\left|\gamma_2^i + 1\right|) - r(\left|\gamma_2^i\right|) \equiv \Delta r(\left|\gamma_2^i + 1\right|)$$

Substituting the abortion decision into women's value functions at the pregnancy node yields the following optimization problem:

$$\max_b \left\{ \bar{u} - \phi(b) - r(\left|\gamma_2^i\right|) - p(b) \min\{c_i, \Delta r(\left|\gamma_2^i + 1\right|)\} \right\}$$

Hence, the optimal policy choice is $b_3^*(c_i, \gamma_2^i) = \tilde{b}(c_i \wedge \Delta r(|\gamma_2^i + 1|))$, and we write the value function

$$V_3(c_i, \gamma_2^i) = \bar{u} - r(\left|\gamma_2^i\right|) - B(c_i \wedge \Delta r(\left|\gamma_2^i + 1\right|))$$

The period 2 abortion node decision mirrors that of period 3. Specifically, women abort in period 2 if and only if $c_i < V_3(\gamma_1^i, c_i) - V_3(\gamma_1^i + 1, c_i)$. Backwards inducing yields that $b_2^*(c_i, \gamma_2^i) = \tilde{b}(c_i \wedge -\Delta V_3(\gamma_1^i, c_i)$.

The period 1 decisions are analogous. □.

Proof of Lemma 2: In period 3, women with $\gamma_2^i \leq -1$ have only a single period to have another child. Hence, they cannot overshoot their target fertilities, while facing penalties for undershooting their target fertilities. Evaluating their optimal policies confirms that they never delay trying to have a child. Analogous arguments hold for women in period 3 with $\gamma_1^i \leq -2$ and women in period 1 with $\gamma_1^i \leq -3$.

The only women in period 2 who can possibly delay having wanted children are those with $\gamma_1^i = -1$. These women delay if and only if $b_2^*(-1, c_i) > 0$ or $a_2^*(-1, c_i) = 1$. Equivalently, these women delay if and only if $V_3(-1, c_i) > V_3(0, c_i)$. Expanding value functions and simplifying yields the necessary and sufficient condition for delaying a wanted child in period

2 for these women:

$$(1-\bar{p})r(1) < B(c_i \wedge r(1)) \tag{5}$$

Observe that we can write $p(b) = \min\{\bar{p}, p(b)\}$ for all b. Then, the Intermediate Value Theorem (IVT) guarantees that there exists a scalar q_2 such that $(1-q_2)r(1) = B(r(1))$. Similarly, the IVT guarantees the existence of a threshold value c_2^* such that $(1-\bar{p})r(1) = B(c_2^*)$. Hence, $\bar{p} > q_2$ is a sufficient condition for women in period 2 with $\gamma_1^i = -1$ and $c_i > c_2^*$ to delay wanted children. Women with $c_i < c_2^*$ have sufficiently small abortion costs that their risk of overshooting target fertilities is mitigated (they anticipate using abortions).

The analogous necessary and sufficient condition for delaying a wanted child in period 1 for women with $\gamma_0^i = -1$ is: $V_2(-1, c_i) > V_2(0, c_i)$. Equivalently, these women delay wanted children if and only if

$$V_3(-1) - V_3(0) > B(c_i \wedge (V_3(-1) - V_3(0))) - B(c_i \wedge (V_3(0) - V_3(1))) \tag{6}$$

where we have suppressed the dependence of the value functions on c_i for concision. Expanding value functions yields that $V_3(0) - V_3(1) > V_3(-1) - V_3(0)$. Consequently, the right hand side of (6) is always less than or equal to zero. Hence, $\bar{p} > q_2$ is a sufficient condition for women in period 1 with $c_i > c_2^*$ and $\gamma_0^i = -1$ to delay wanted children. For women with $c_i < c_2^*$, the above condition reduces to

$$(1-\bar{p})(V_3(-1) - V_3(0)) > -B(c_i \wedge (V_3(0) - V_3(1)))$$

As $V_3(0) - V_3(1) \geq r(1)$, a sufficient condition for the above inequality to hold is

$$(1-\bar{p})(B(c_i \wedge r(1)) - (1-\bar{p})r(1)) > -B(c_i \wedge r(1))$$

or equivalently, that

$$B(c_i) > \frac{(1-\bar{p})^2 r(1)}{2-\bar{p}} \tag{7}$$

Hence, there exists a threshold value c_1^* satisfying (7) with equality by the IVT. As the fraction on the right hand side of (7) is strictly smaller than $(1-\bar{p})r(1)$, $c_1^* < c_2^*$. Thus, $\bar{p} > q_2$ is a sufficient condition for women in period 1 and $\gamma_0^i = -1$ to delay wanted children so long as $c_i > c_1^*$.

The analogous necessary and sufficient condition for delaying a wanted child in period 1 for women with $\gamma_0^i = -2$ is $V_2(-2) > V_2(-1)$, where we have suppressed the dependence of the value functions on c_i for concision. Equivalently, these women delay wanted children if and only if

$$(1-\bar{p})(\bar{p}r(1) + (1-\bar{p})(r(2)-r(1))) < B(c_i \wedge (V_3(-1) - V_3(0))) \tag{8}$$

As $r(2) - r(1) > r(1)$ by the convexity of $r(\cdot)$, the left hand side of (8) is strictly larger than $(1-\bar{p})r(1)$. Furthermore, when $\bar{p} = q_2$, $V_3(-1) - V_3(0) = 0$, and the right hand side collapses to zero. Hence, at $\bar{p} = q_2$, $\bar{p}$ is too small for inequality (8) to hold. An analogous appeal to the Intermediate Value Theorem ensures there exists a q_3 such that when $\bar{p} > q_3$, inequality (8) holds. By the above reasoning, $q_3 > q_2$. Similarly, there exists a $c_1^+ > c_2^*$ such that whenever $\bar{p} > q_3$, women in period 1 with $\gamma_0^i = -2$ and $c_i > c_1^+$ delay wanted children.

A cursory examination of inequalities (5), (6), and (8) reveal that when $\bar{p} = 0$ none of them hold. By continuity, then, there exists a scalar $q_1 > 0$ such that none of these inequalities hold for $\bar{p} < q_1$. By construction $q_1 < q_2$. $\square$.

Proof of Corollary 2: This immediately follows from Lemmas 1 & 2. Simply take expectations. Expressions for the $N(t,\gamma)$ are given by:

$$
\begin{aligned}
N(2,-3) &= N_3(1-\bar{p}) \\
N(2,-2) &= N_3\bar{p} + N_2\left[F(c_1^+)(1-\bar{p}) + \int_{c_1^+}^{\infty}(1-g(c_i,-2,1)dF\right] \\
N(2,-1) &= N_1\left[F(c_1^*)(1-\bar{p}) + \int_{c_1^*}^{\infty}(1-g(c_i,-1,1))dF\right] \\
&\quad + N_2\left(F(c_1^+)\bar{p} + \int_{c_1^+}^{\infty} g(c_i,-2,1)dF\right) \\
N(2,0) &= N_0\left(F(r(1)) + \int_{d_1}^{\infty}(1-g(c_i,0,1))dF\right) \\
&\quad + N_1\left(F(c_1^*)\bar{p} + \int_{c_1^*}^{\infty} g(c_i,-1,1)dF\right) \\
N(2,1) &= N_0\int_{d_1}^{\infty} g(c_i,0,1)dF \qquad \square.
\end{aligned}
$$

Proof of Lemma 3: This proof is entirely analogous to the proof of Lemmas 1 and 2. Introducing costless, perfect fertility control eliminates overshooting risk and eliminates any precautionary motives. $\square$.

Proof of Corollary 3: Immediate from Lemma 3. Take expectations. Expressions for $N^I(t,\gamma)$ are given by:

$$
\begin{aligned}
N^I(2,-3) &= N_3(1-\bar{p}) \\
N^I(2,-2) &= N_3\bar{p} + N_2(1-\bar{p}) \\
N^I(2,-1) &= N_2\bar{p} + N_1(1-\bar{p}) \\
N^I(2,0) &= N_0 + N_1\bar{p} \qquad \square.
\end{aligned}
$$

Proof of Corollary 4: This is immediate from Lemmas 1, 2, and 3. As $r(3)$ is the maximum possible overshooting penalty with 3 periods, when $\bar{\phi} > r(3)$, the disutility of modern contraceptive use dwarfs any penalties for missing target fertilities. $\square$.

Proof of Proposition 3: Use the expressions derived in Corollaries 2 & 3. Note that because injectable legalization is happening during women's lives, the composition of women at $t = 2, 3$ is determined by fertility behavior in a pre-legalization world. $\square$.

Proof of Corollary 5: Immediate from Proposition 3 and 2 as $A_3 > A_2 > A_1$ by virtue of more women having reached their target fertilities in later periods. $\square$.

Proof of Corollary 6: Immediate from Proposition 3 and manipulation of the formulas for $N(t, \gamma)$ found in Corollary 2. $\square$.

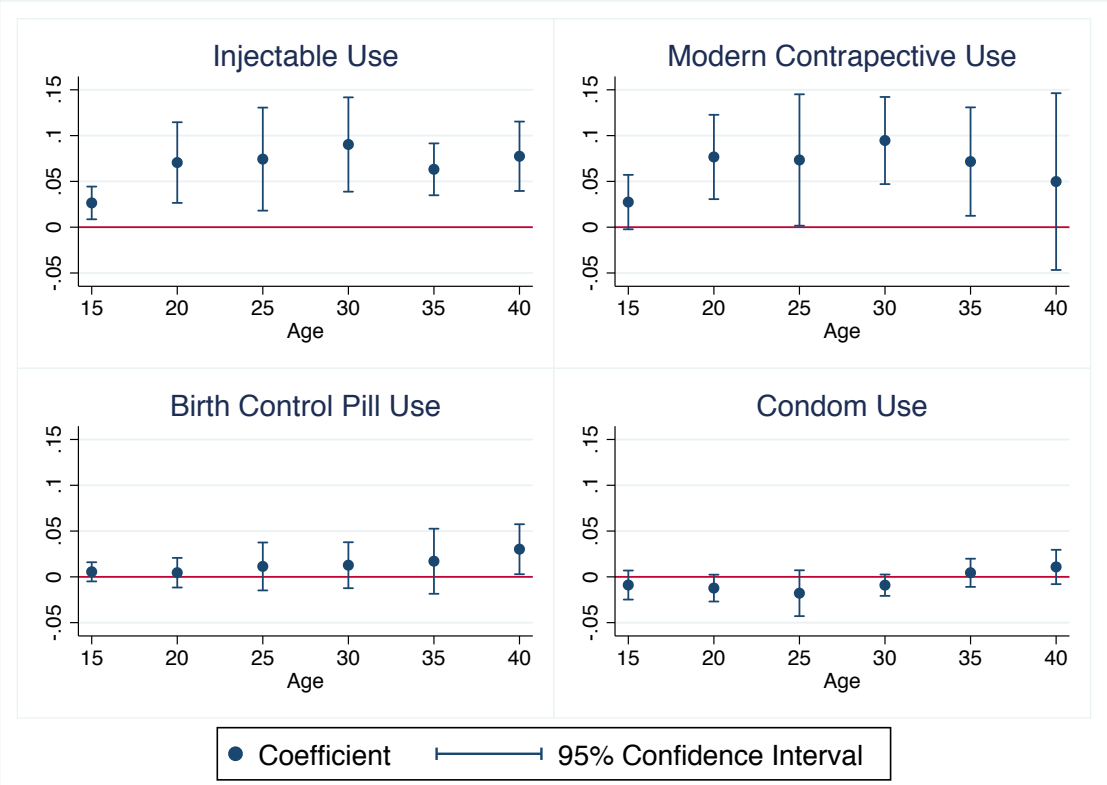
Proof of Corollary 7: Immediate from Proposition 3. $\square$.

Proof of Corollary 8: Immediate from Proposition 3. With costless modern contraceptives, women attempt to have children until they reach their target fertilities, then perfectly limit births. Hence, overshooting risk is eliminated, and expected completed fertility falls. $\square$.

B Analysis of Heterogeneous Treatment Effects

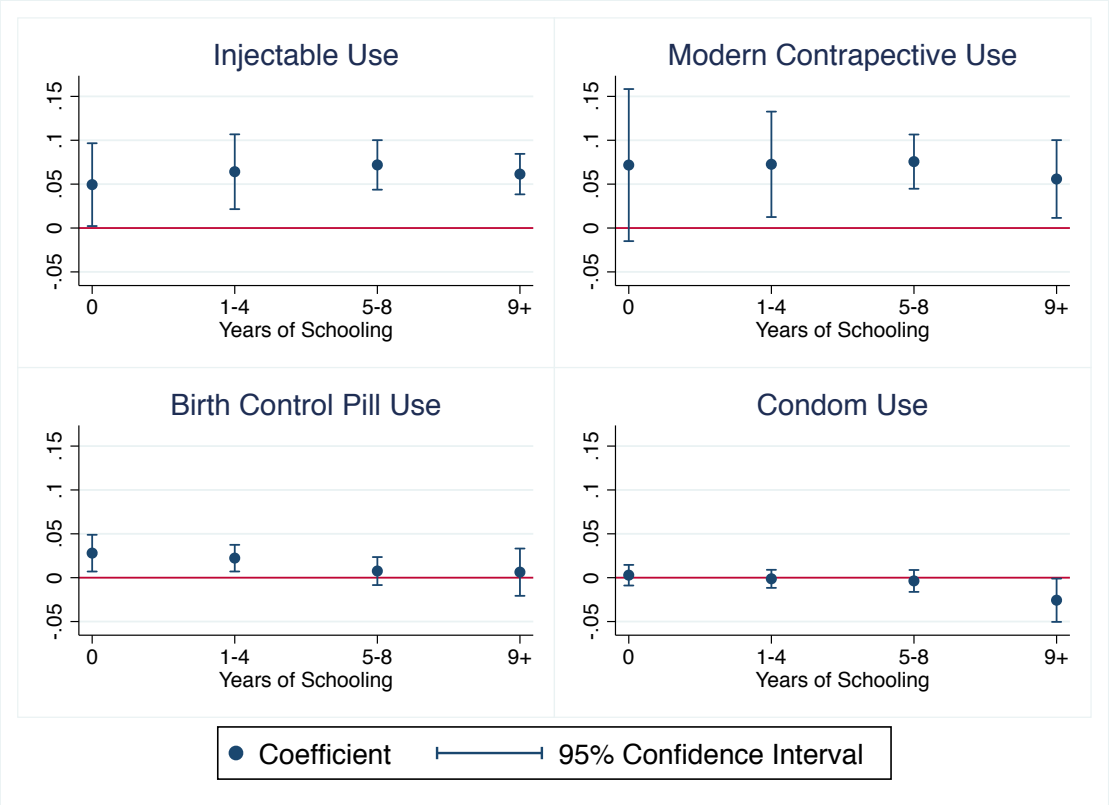
B.1 Contraceptive Adoption

Figure B.1: Heterogeneous Contraceptive Take-up Across Age Group



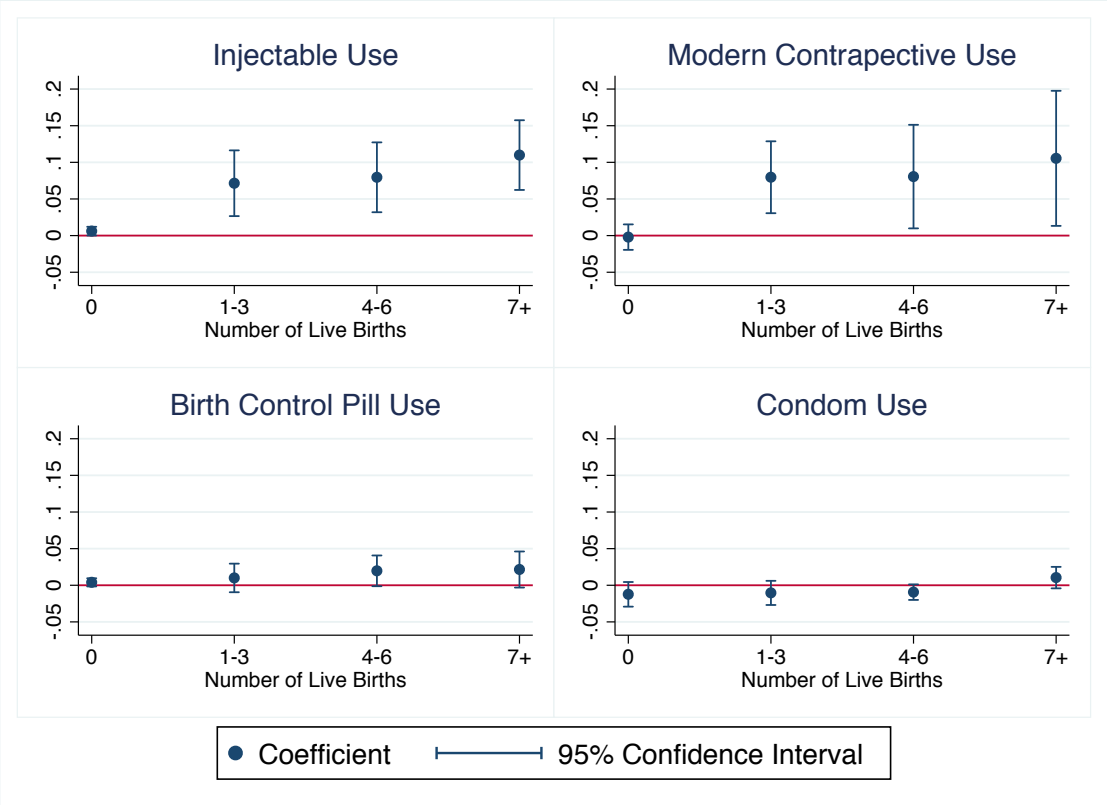
Notes: The graphs plot coefficient estimates and 95% confidence intervals for the treatment effect β from the regression specification (2.1) estimated separately on subsamples defined by age. The dependent variable is an indicator variable that takes value one if a woman is using a type of contraceptive at survey date. All standard errors are clustered at the region level.

Figure B.2: Heterogeneous Contraceptive Take-up Across Education Level



Notes: The graphs plot coefficient estimates and 95% confidence intervals for the treatment effect β from the regression specification (2.1) estimated separately on subsamples defined by years of schooling. The dependent variable is an indicator variable that takes value one if a woman is using a type of contraceptive at survey date. All standard errors are clustered at the region level.

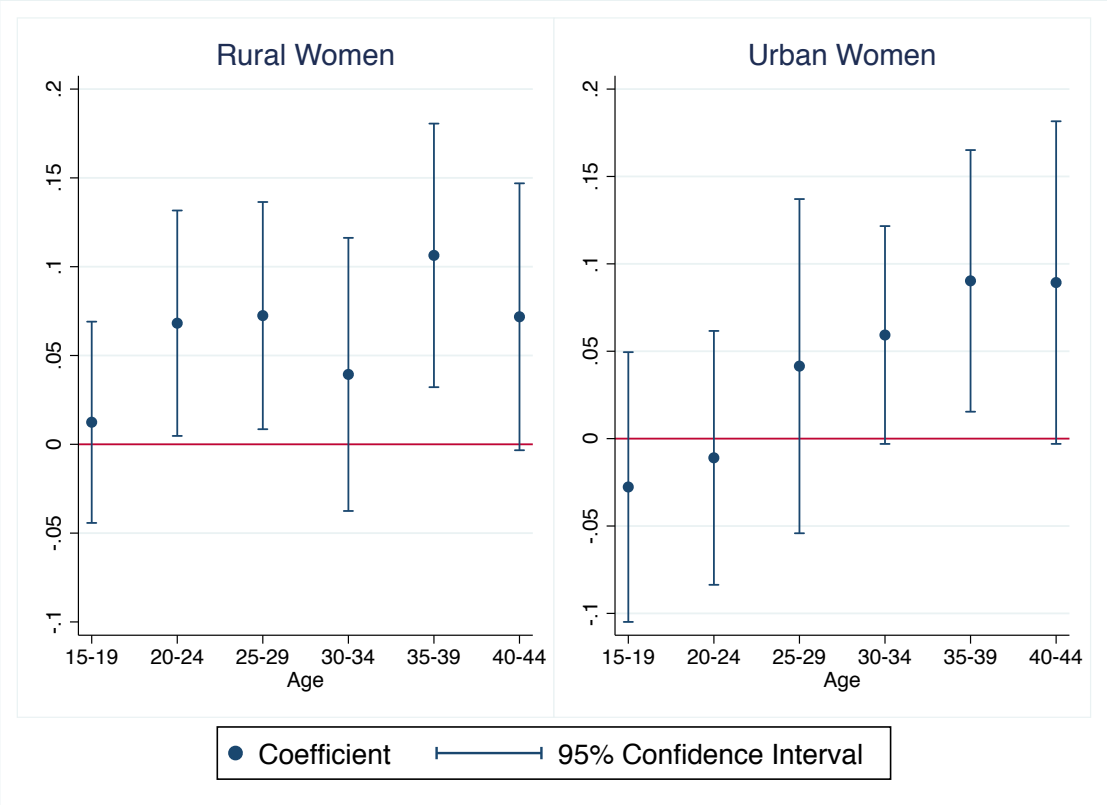
Figure B.3: Heterogeneous Contraceptive Take-up Across Parity



Notes: The graphs plot coefficient estimates and 95% confidence intervals for the treatment effect β from the regression specification (2.1) estimated separately on subsamples defined by parity. The dependent variable is an indicator variable that takes value one if a woman is using a type of contraceptive at survey date. All standard errors are clustered at the region level.

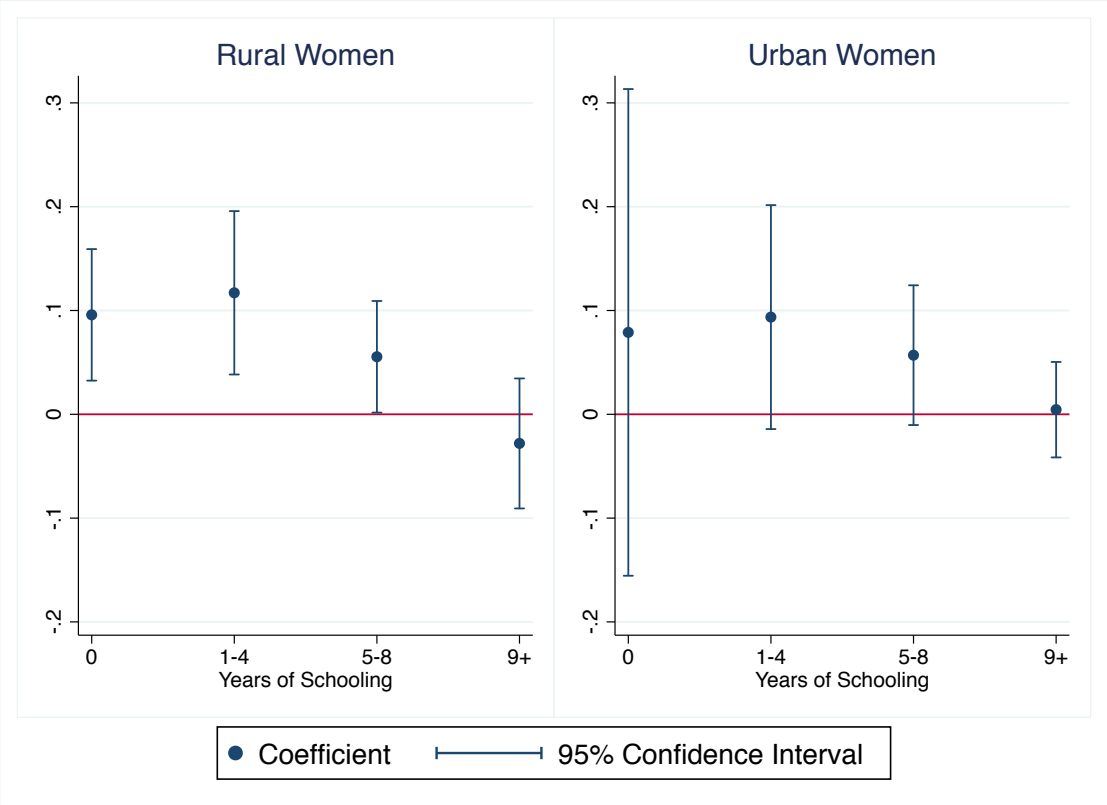
B.2 Coital Frequency

Figure B.4: Heterogeneous Coital Frequency Response Across Age Groups



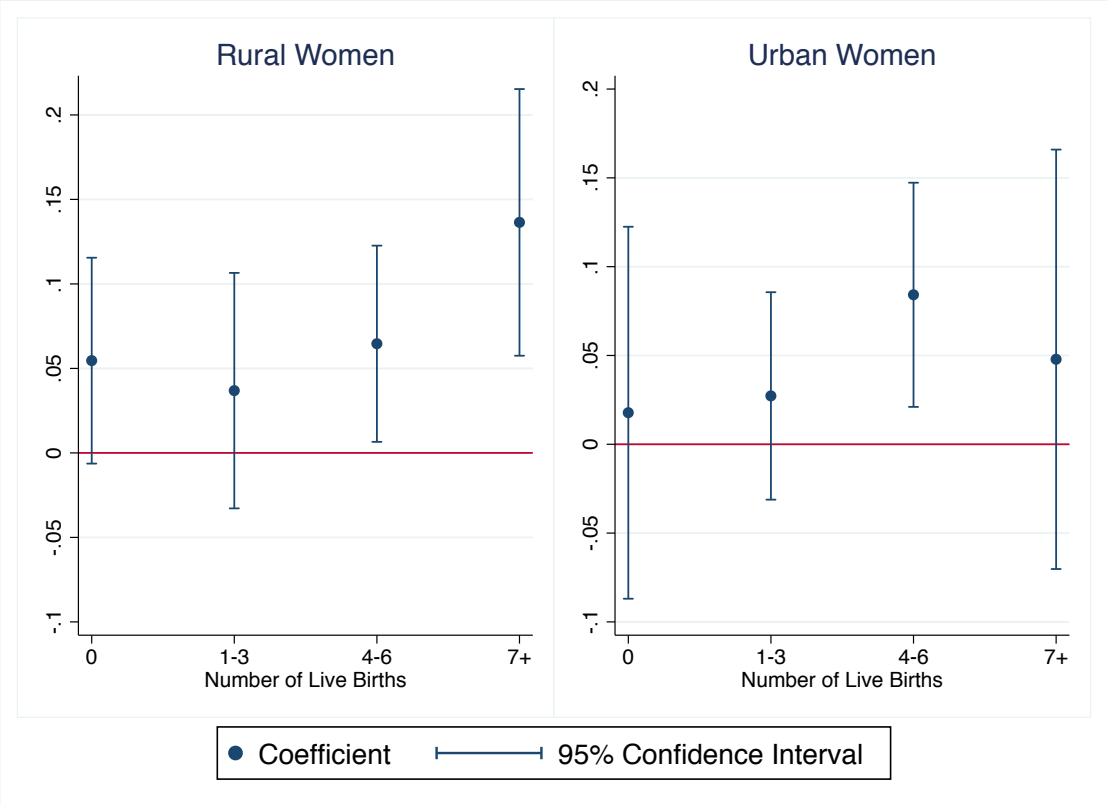
Notes: The graphs plot coefficient estimates and 95% confidence intervals for the treatment effect β from the regression specification (2.1) estimated separately on subsamples defined by age. The dependent variable is an indicator variable that takes value one if a woman reports having had sex during the week prior to survey date. All standard errors are clustered at the region level.

Figure B.5: Heterogeneous Coital Frequency Response Across Educational Attainment



Notes: The graphs plot coefficient estimates and 95% confidence intervals for the treatment effect β from the regression specification (2.1) estimated separately on subsamples defined by years of schooling. The dependent variable is an indicator variable that takes value one if a woman reports having had sex during the week prior to survey date. All standard errors are clustered at the region level.

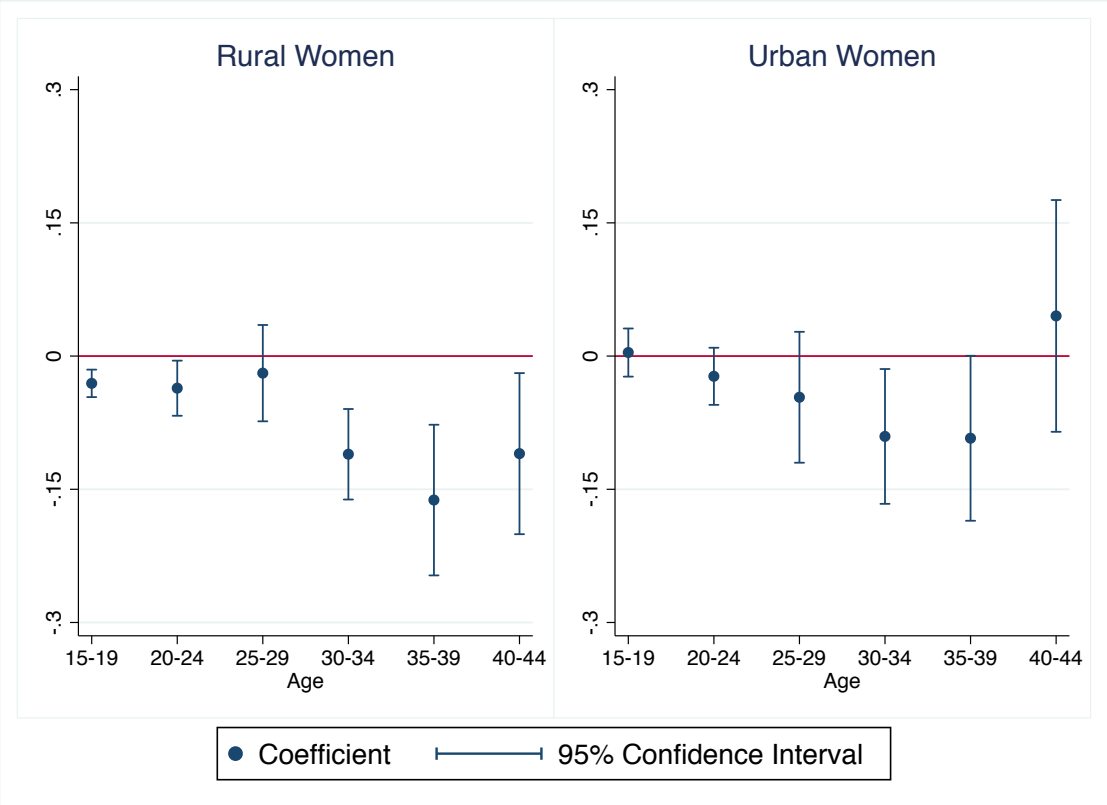
Figure B.6: Heterogeneous Coital Frequency Response Across Parity



Notes: The graphs plot coefficient estimates and 95% confidence intervals for the treatment effect β from the regression specification (2.1) estimated separately on subsamples defined by parity. The dependent variable is an indicator variable that takes value one if a woman reports having had sex during the week prior to survey date. All standard errors are clustered at the region level.

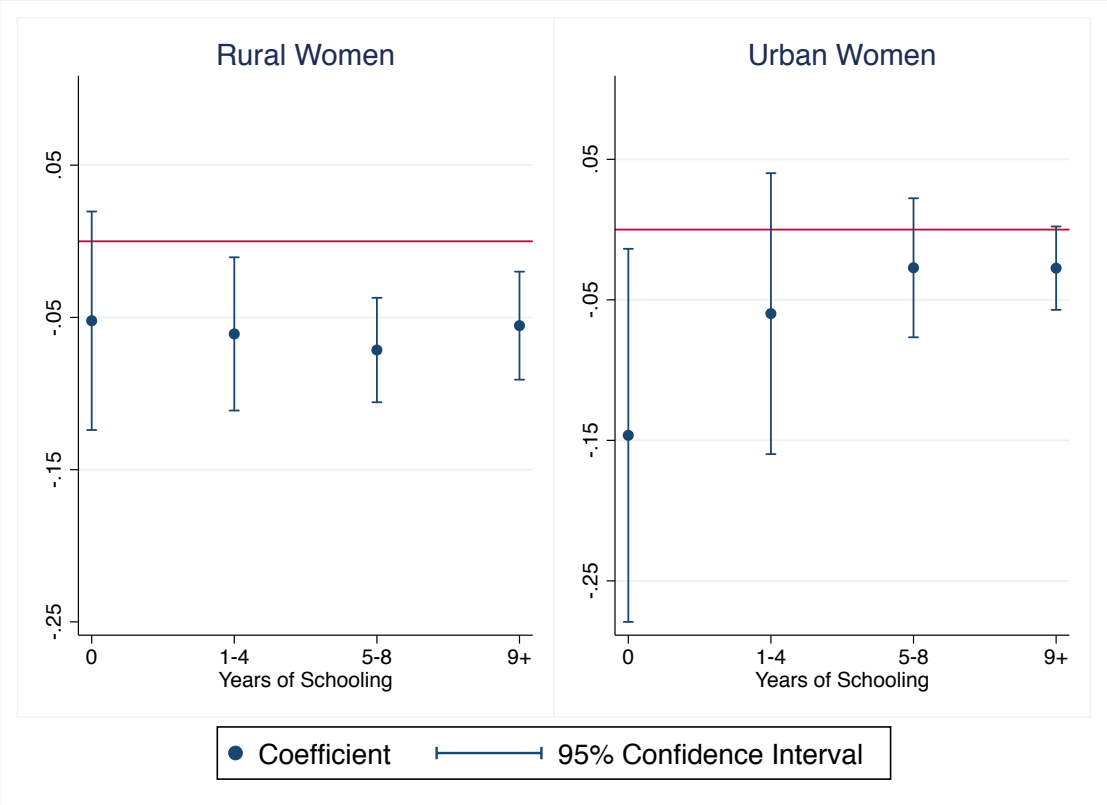
B.3 Abortions

Figure B.7: Heterogeneous Abortion Response Across Age Groups



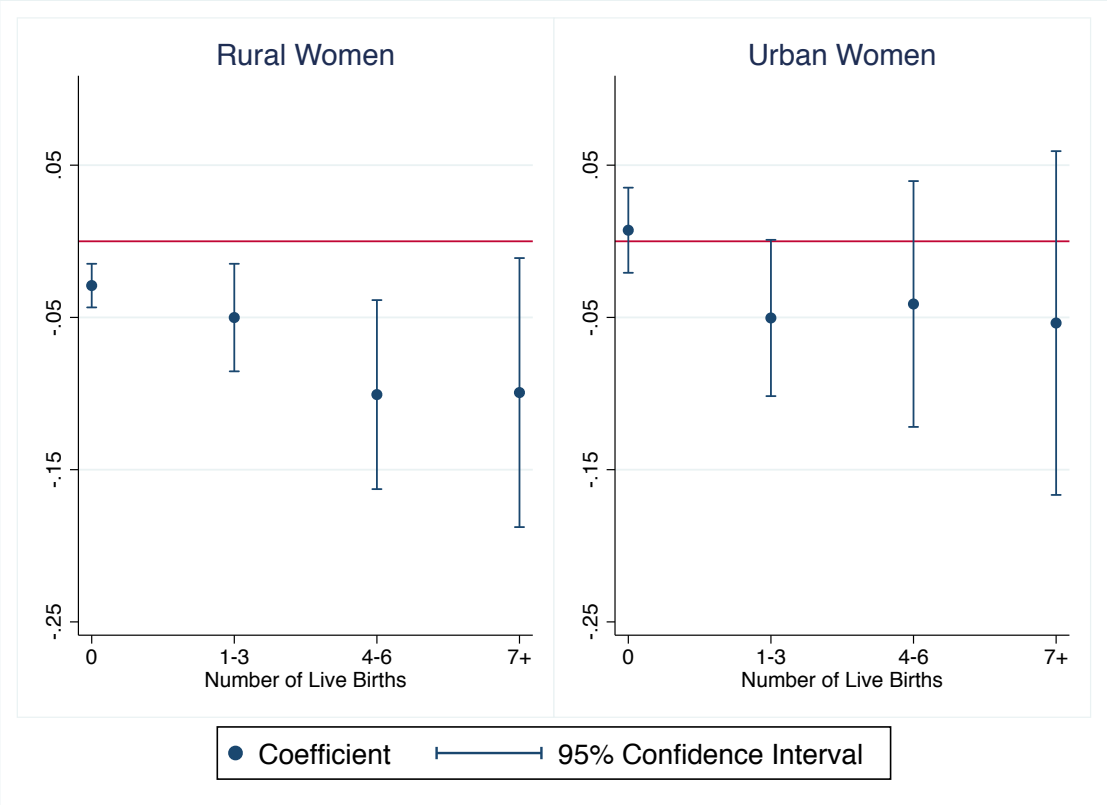
Notes: The graphs plot coefficient estimates and 95% confidence intervals for the treatment effect β from the regression specification (2.1) estimated separately on subsamples defined by age. The dependent variable is an indicator variable that takes value one if a woman reports ever having had a terminated pregnancy at survey date. All standard errors are clustered at the region level.

Figure B.8: Heterogeneous Abortion Response Across Educational Attainment



Notes: The graphs plot coefficient estimates and 95% confidence intervals for the treatment effect β from the regression specification (2.1) estimated separately on subsamples defined by years of schooling. The dependent variable is an indicator variable that takes value one if a woman reports ever having had a terminated pregnancy at survey date. All standard errors are clustered at the region level.

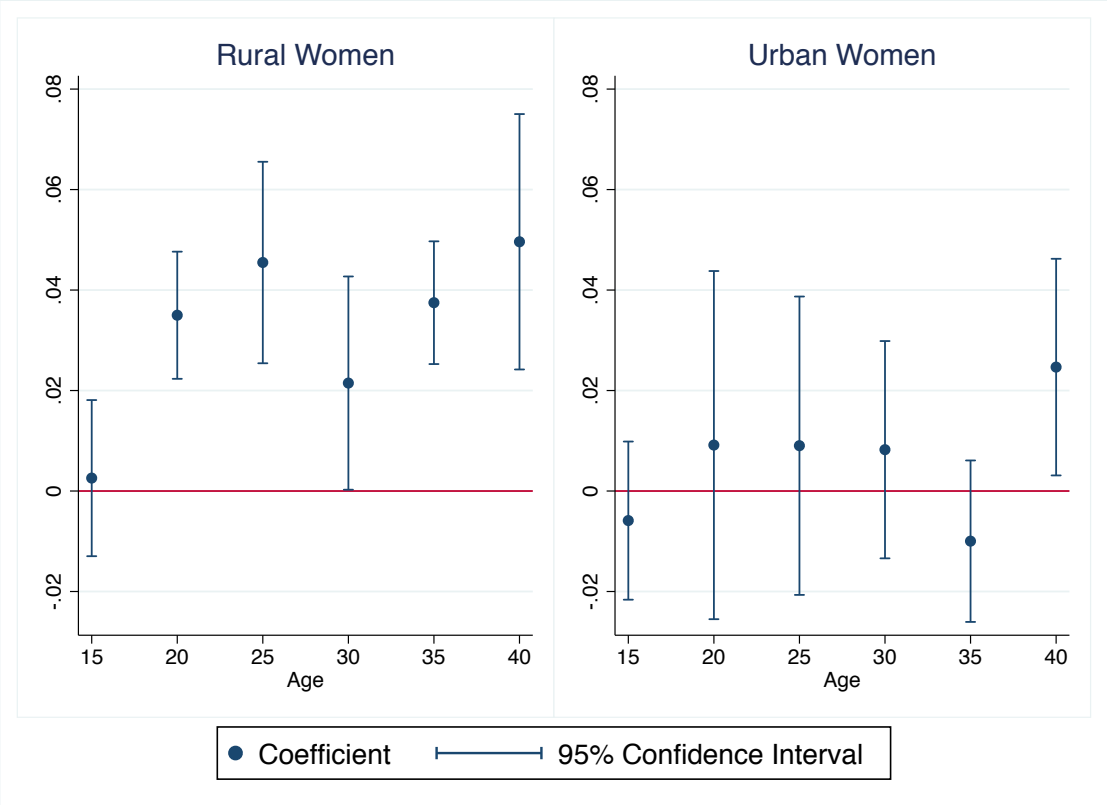
Figure B.9: Heterogeneous Abortion Response Across Parity



Notes: The graphs plot coefficient estimates and 95% confidence intervals for the treatment effect β from the regression specification (2.1) estimated separately on subsamples defined by parity. The dependent variable is an indicator variable that takes value one if a woman reports ever having had a terminated pregnancy at survey date. All standard errors are clustered at the region level.

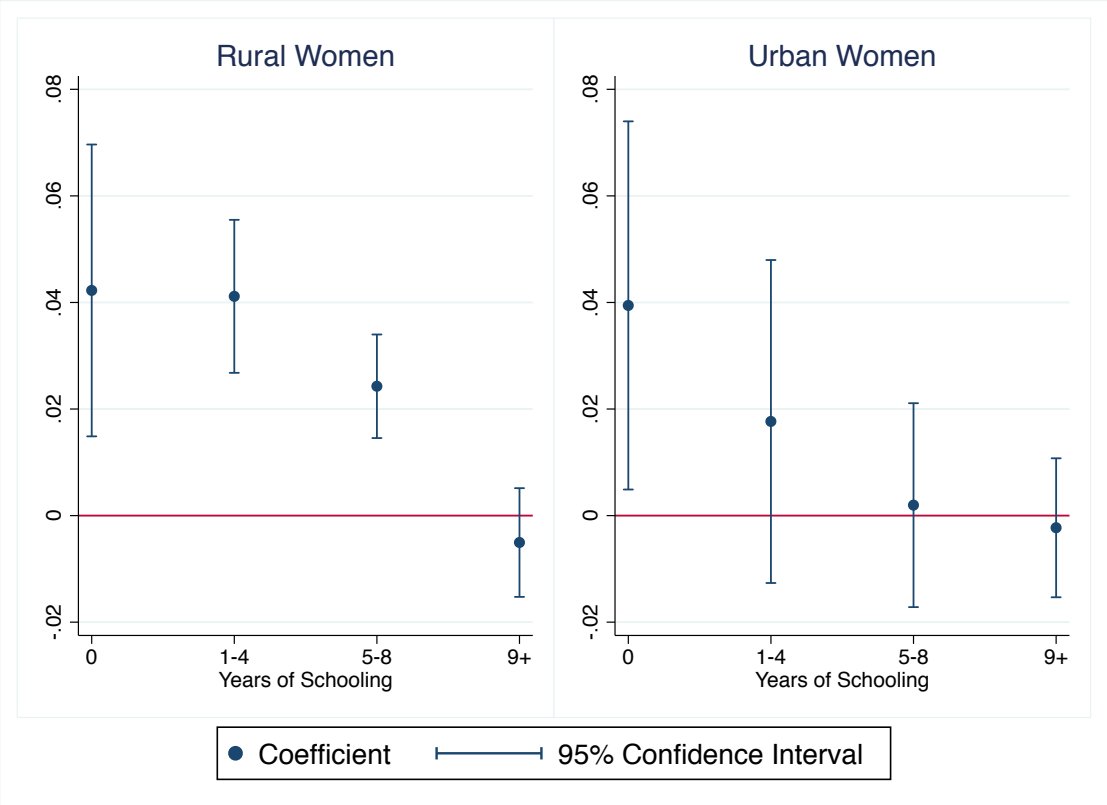
B.4 Fertility

Figure B.10: Heterogeneous Fertility Response Across Age Groups



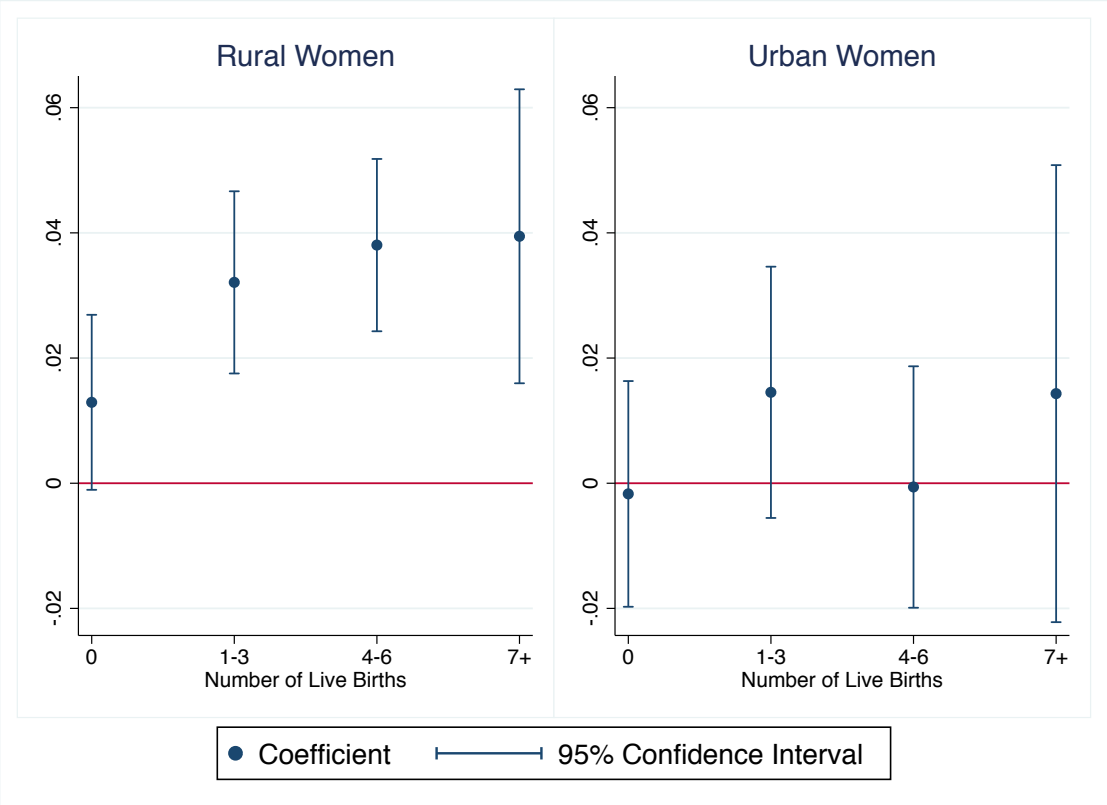
Notes: The graphs plot coefficient estimates and 95% confidence intervals for the treatment effect β from the regression specification (2.1) estimated separately on subsamples defined by age. The dependent variable is an indicator variable taking value one in a year in which a woman gives birth. All standard errors are clustered at the region level.

Figure B.11: Heterogeneous Fertility Response Across Educational Attainment



Notes: The graphs plot coefficient estimates and 95% confidence intervals for the treatment effect β from the regression specification (2.1) estimated separately on subsamples defined by years of schooling. The dependent variable is an indicator variable taking value one in a year in which a woman gives birth. All standard errors are clustered at the region level.

Figure B.12: Heterogeneous Fertility Response Across Parity



Notes: The graphs plot coefficient estimates and 95% confidence intervals for the treatment effect β from the regression specification (2.1) estimated separately on subsamples defined by parity. The dependent variable is an indicator variable taking value one in a year in which a woman gives birth. All standard errors are clustered at the region level.

C Robustness Tables

Table C.1: Contraceptive Adoption Robustness

Panel A: Contraceptive Adoption Excluding Malawi

	Injectables		Modern Contraceptives		Condoms		Pills	
Post Inj	0.090***	0.091***	0.116***	0.123***	-0.007	-0.007	0.014	0.020
	(0.017)	(0.017)	(0.021)	(0.024)	(0.009)	(0.008)	(0.012)	(0.012)
Obs.	51,731	51,101	51,731	51,101	51,731	51,101	51,731	51,101
Mean	0.0939	0.0939	0.254	0.254	0.0376	0.0376	0.0940	0.0940
Clusters	16	16	16	16	16	16	16	16
Controls	N	Y	N	Y	N	Y	N	Y

Panel B: Contraceptive Adoption Using a Paired-Region Regression Specification

Post Inj	0.062**	0.063**	0.085**	0.088**	-0.000	0.001	0.020*	0.021*
	(0.022)	(0.021)	(0.030)	(0.029)	(0.003)	(0.002)	(0.010)	(0.010)
Obs.	61,280	60,655	61,280	60,655	61,280	60,655	61,280	60,655
Mean	0.117	0.117	0.266	0.266	0.0367	0.0367	0.0725	0.0725
Clusters	14	14	14	14	14	14	14	14
Controls	N	Y	N	Y	N	Y	N	Y

Notes: The dependent variable in these regressions is a dummy variable that takes value 1 if a woman reports using the specific type (or types) of contraception at survey date and takes value zero otherwise. The category modern contraceptives include injectables, norplant, pills, condoms, male & female sterilization, IUDs, & diaphragms. *Panel A* reports coefficients and standard errors are for the term β in regression (2.1) excluding Malawi. *Panel B* reports coefficients and standard errors are for the term β in regression (E.1). Controls include fixed effects for marital status and educational attainment. All standard errors are clustered at the region level. *** p<0.01, ** p<0.05, *p<0.1

Table C.2: Alternate Measures of Coital Frequency

	All Areas		Rural Areas		Urban Areas	
Panel A: Ever Had Sexual Intercourse						
Post Inj	0.0017	0.0101	0.0099	0.0159**	-0.0202	-0.0073
	(0.0068)	(0.0071)	(0.0066)	(0.0075)	(0.0129)	(0.0109)
Obs.	77,569	76,936	53,787	53,289	23,782	23,647
Mean	0.865	0.865	0.877	0.877	0.837	0.837
Clusters	18	18	18	18	18	18
Controls	N	Y	N	Y	N	Y
Panel B: Sex Within Last Month						
Post Inj	0.0440**	0.0329*	0.0470*	0.0362	0.0355*	0.0218
	(0.0192)	(0.0177)	(0.0225)	(0.0210)	(0.0201)	(0.0177)
Obs.	66,757	66,213	46,996	46,559	19,761	19,654
Mean	0.684	0.684	0.696	0.696	0.658	0.658
Clusters	18	18	18	18	18	18
Controls	N	Y	N	Y	N	Y

Notes: All coefficients and standard errors are for the term β in regression (2.1). The dependent variable in *Panel A* is an indicator variable that takes value one if a woman reports ever having had sexual intercourse at survey date. The dependent variable in *Panel B* is an indicator variable taking value one if a woman reports having had sex during the past month at survey date. Controls include fixed effects for marital status and educational attainment. All standard errors are clustered at the region level. *** p<0.01, ** p<0.05, *p<0.1

Table C.3: Coital Frequency Robustness

Panel A: Coital Frequency Responses Excluding Malawi

	All Areas		Rural Areas		Urban Areas	
Post Inj	0.0576**	0.0567**	0.0713**	0.0676**	0.0105	0.0108
	(0.0226)	(0.0212)	(0.0281)	(0.0253)	(0.0221)	(0.0222)
Obs.	45,990	45,449	29,788	29,351	16,202	16,098
Mean	0.519	0.519	0.535	0.535	0.489	0.489
Clusters	16	16	16	16	16	16
Controls	N	Y	N	Y	N	Y

Panel B: Coital Frequency Responses Using a Paired-Region Regression Specification

Post Inj	0.0930***	0.0835***	0.1000***	0.0873***	0.0628**	0.0630***
	(0.0161)	(0.0120)	(0.0191)	(0.0154)	(0.0236)	(0.0136)
Obs.	54,041	53,503	39,932	39,499	14,109	14,004
Mean	0.549	0.549	0.561	0.561	0.514	0.514
Clusters	14	14	14	14	14	14
Controls	N	Y	N	Y	N	Y

Notes: The dependent variable in these regressions is a dummy variable that takes value 1 if a woman reports having had sexual intercourse during the week prior to survey date. *Panel A* reports coefficients and standard errors are for the term β in regression (2.1) excluding Malawi. *Panel B* reports coefficients and standard errors are for the term β in regression (E.1). Controls include fixed effects for marital status and educational attainment. All standard errors are clustered at the region level.
*** p<0.01, ** p<0.05, *p<0.1

Table C.4: Abortion Use Robustness

	All Areas		Rural Areas		Urban Areas	
Panel A: Abortion Responses Excluding Malawi						
Post Inj	-0.0265	-0.0264	-0.0433	-0.0448	0.0176	0.0220
	(0.0397)	(0.0383)	(0.0399)	(0.0388)	(0.0448)	(0.0437)
Obs.	40,780	40,740	25,038	25,007	15,742	15,733
Mean	0.110	0.110	0.117	0.117	0.0969	0.0969
Clusters	16	16	16	16	16	16
Controls	N	Y	N	Y	N	Y
Panel B: Abortion Responses Using a Paired-Region Specification						
Post Inj	-0.0464	-0.0469*	-0.0570*	-0.0574*	0.0041	0.0048
	(0.0264)	(0.0252)	(0.0281)	(0.0273)	(0.0204)	(0.0181)
Obs.	51,914	51,880	38,172	38,145	13,742	13,735
Mean	0.108	0.108	0.115	0.115	0.0893	0.0893
Clusters	14	14	14	14	14	14
Controls	N	Y	N	Y	N	Y

Notes: The dependent variable in these regressions is a dummy variable that takes value 1 if a woman reports ever having had a terminated pregnancy prior to survey date. *Panel A* reports coefficients and standard errors are for the term β in regression (2.1) excluding Malawi. *Panel B* reports coefficients and standard errors are for the term β in regression (E.1). Controls include fixed effects for marital status and educational attainment. All standard errors are clustered at the region level.
*** p<0.01, ** p<0.05, *p<0.1

Table C.5: Changes in Postpartum Outcomes With Controls

	Rural Areas				Urban Areas			
	Breast-feeding	Postpartum Abstinence	Postpartum Amenorrhea	Postpartum Protection	Breast-feeding	Postpartum Abstinence	Postpartum Amenorrhea	Postpartum Protection
Panel A: 1–12 Months Post-Birth								
Post Inj	-0.0003	-0.0134	-0.0196*	-0.0239**	-0.0140	-0.0075	-0.0536**	-0.0447***
	(0.0099)	(0.0183)	(0.0108)	(0.0091)	(0.0188)	(0.0196)	(0.0200)	(0.0136)
Obs.	259,657	354,635	354,635	354,635	65,144	93,465	93,465	93,465
Mean	0.968	0.585	0.784	0.856	0.963	0.575	0.708	0.807
Clusters	18	18	18	18	18	18	18	18
Panel B: 13–24 Months Post-Birth								
Post Inj	0.0188	-0.0387***	-0.0742***	-0.0892***	-0.0889***	-0.0185	-0.0995***	-0.1090***
	(0.0177)	(0.0127)	(0.0137)	(0.0143)	(0.0300)	(0.0354)	(0.0217)	(0.0357)
Obs.	80,915	109,676	109,676	109,676	21,100	30,498	30,498	30,498
Mean	0.765	0.132	0.298	0.370	0.715	0.134	0.228	0.316
Clusters	18	18	18	18	18	18	18	18

Notes: The dependent variable in these regressions is a dummy variable that takes value 1 if a woman reports that she is breastfeeding, abstaining, or experiencing amenorrhea or the postpartum protection period in a given month post-birth. All coefficients and standard errors are for the term β in regression (E.2). *Panel A* reports treatment effects for the first 12 months post-birth. *Panel B* reports treatment effects for the second 12 months post-birth. All regressions include as controls fixed effects for marital status and educational attainment. All standard errors are clustered at the region level. *** p<0.01, ** p<0.05, *p<0.1

Table C.6: Changes in Postpartum Outcomes Excluding Malawi

	Rural Areas				Urban Areas			
	Breast-feeding	Postpartum Abstinence	Postpartum Amenorrhea	Postpartum Protection	Breast-feeding	Postpartum Abstinence	Postpartum Amenorrhea	Postpartum Protection
Panel A: 1–12 Months Post-Birth								
Post Inj	-0.0080	-0.0381	-0.0419**	-0.0392**	-0.0345	-0.0299	-0.0621**	-0.0485*
	(0.0171)	(0.0264)	(0.0194)	(0.0157)	(0.0234)	(0.0296)	(0.0241)	(0.0244)
Obs.	142,619	192,765	192,765	192,765	46,691	70,628	70,628	70,628
Mean	0.968	0.588	0.786	0.860	0.958	0.565	0.697	0.799
Clusters	16	16	16	16	16	16	16	16
Panel B: 13–24 Months Post-Birth								
Post Inj	0.0211	-0.0608**	-0.0928***	-0.1141***	-0.0705	0.0155	-0.0614**	-0.0457
	(0.0233)	(0.0234)	(0.0280)	(0.0317)	(0.0420)	(0.0434)	(0.0257)	(0.0357)
Obs.	43,863	59,843	59,843	59,843	14,238	22,305	22,305	22,305
Mean	0.753	0.138	0.316	0.387	0.702	0.139	0.228	0.320
Clusters	16	16	16	16	16	16	16	16

Notes: The dependent variable in these regressions is a dummy variable that takes value 1 if a woman reports that she is breastfeeding, abstaining, or experiencing amenorrhea or the postpartum protection period in a given month post-birth. All coefficients and standard errors are for the term β in regression (E.2) excluding Malawi. *Panel A* reports treatment effects for the first 12 months post-birth. *Panel B* reports treatment effects for the second 12 months post-birth. All standard errors are clustered at the region level. *** p<0.01, ** p<0.05, *p<0.1

Table C.7: Fertility Responses Excluding Malawi

Panel A: Across-Women Fertility Responses

	All Areas		Rural Areas		Urban Areas	
Post Inj	0.0131*	0.0164**	0.0202***	0.0229***	-0.0040	0.0013
	(0.0069)	(0.0068)	(0.0059)	(0.0056)	(0.0083)	(0.0088)
Obs.	350,957	350,659	221,332	221,116	129,625	129,543
Mean	0.206	0.206	0.228	0.228	0.167	0.167
Clusters	16	16	16	16	16	16
Controls	N	Y	N	Y	N	Y

Panel B: Within-Woman Fertility Responses

Post Inj	0.0168*	0.0521***	0.0230**	0.0630***	0.0023	0.0263**
	(0.0080)	(0.0138)	(0.0082)	(0.0156)	(0.0074)	(0.0113)
Obs.	294,177	294,177	181,212	181,212	112,965	112,965
# Women	31,356	31,356	19,185	19,185	12,171	12,171
Mean	0.206	0.206	0.230	0.230	0.168	0.168
Clusters	16	16	16	16	16	16
Controls	N	Y	N	Y	N	Y

Notes: The dependent variable in all regressions is an indicator variable that takes value 1 if a woman reports having a live birth in a given month. *Panel A* reports coefficients and standard errors for β in regression (2.1) excluding Malawi. Controls include fixed effects for educational attainment, for ever having been married in a given year, and for parity in a given year. *Panel B* reports coefficients and standard errors for β in regression (2.3) excluding Malawi. Controls include fixed effects for whether or not a woman has ever been married in a given year and fixed effects for parity in a given year. All standard errors are clustered at the region level.
*** p<0.01, ** p<0.05, * p<0.1

Table C.8: Fertility Responses Using a Paired Regression Specification

Panel A: Across-Women Fertility Responses

	All Areas		Rural Areas		Urban Areas	
Post Inj	0.0193**	0.0217***	0.0264***	0.0280***	0.0025	0.0070
	(0.0071)	(0.0059)	(0.0064)	(0.0057)	(0.0087)	(0.0072)
Obs.	430,963	430,705	320,662	320,464	110,301	110,241
Mean	0.208	0.208	0.224	0.224	0.162	0.162
Clusters	14	14	14	14	14	14
Controls	N	Y	N	Y	N	Y

Panel B: Within-Woman Fertility Responses

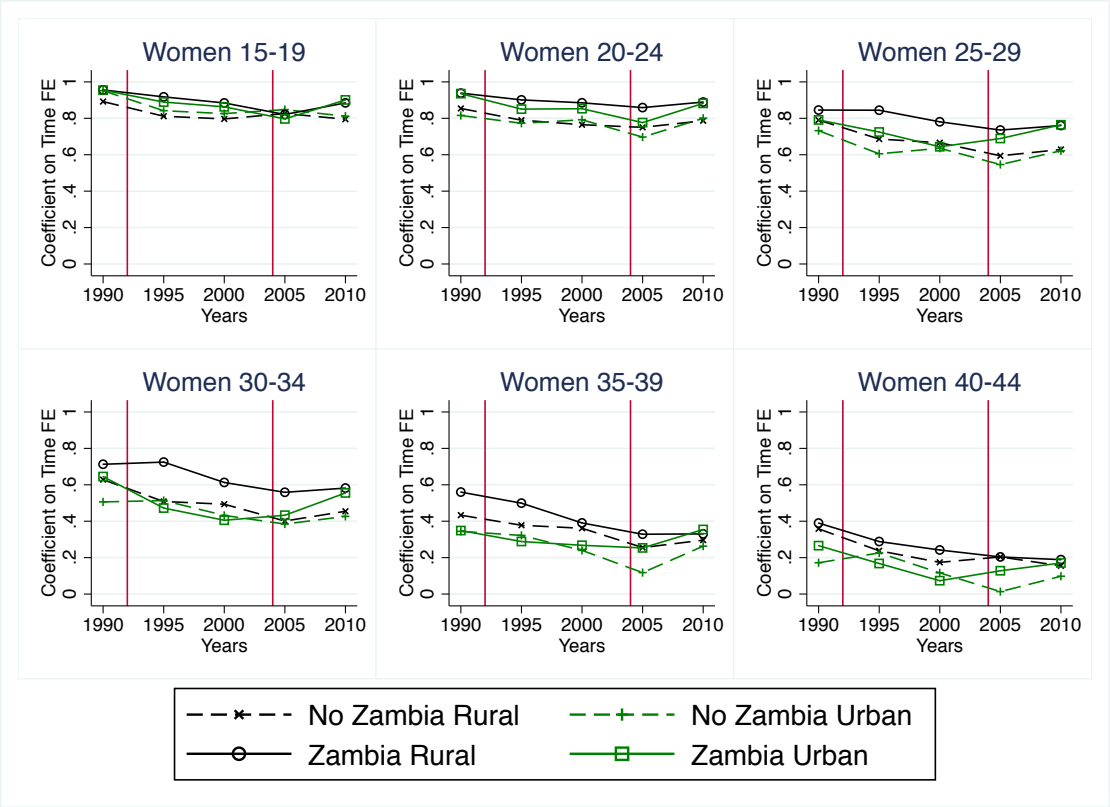
Post Inj	0.0202**	0.0373***	0.0290***	0.0596***	0.0085	0.0294**
	(0.0076)	(0.0099)	(0.0080)	(0.0116)	(0.0094)	(0.0132)
Obs.	339,888	339,888	247,188	247,188	92,700	92,700
# Women	36,127	36,127	26,073	26,073	10,054	10,054
Mean	0.208	0.208	0.226	0.226	0.162	0.162
Clusters	14	14	14	14	14	14
Controls	N	Y	N	Y	N	Y

Notes: The dependent variable in all regressions is an indicator variable that takes value 1 if a woman reports having a live birth in a given month. *Panel A* reports coefficients and standard errors for β in regression (E.1). Controls include fixed effects for educational attainment, for ever having been married in a given year, and for parity in a given year. *Panel B* reports coefficients and standard errors for β in regression (2.3) adapted so that the time fixed effects are interacted with effects for the three region zones (Western, Eastern, and Southern). Controls include fixed effects for whether or not a woman has ever been married in a given year and fixed effects for parity in a given year. All standard errors are clustered at the region level. *** p<0.01, ** p<0.05, * p<0.1

D Additional Event Studies

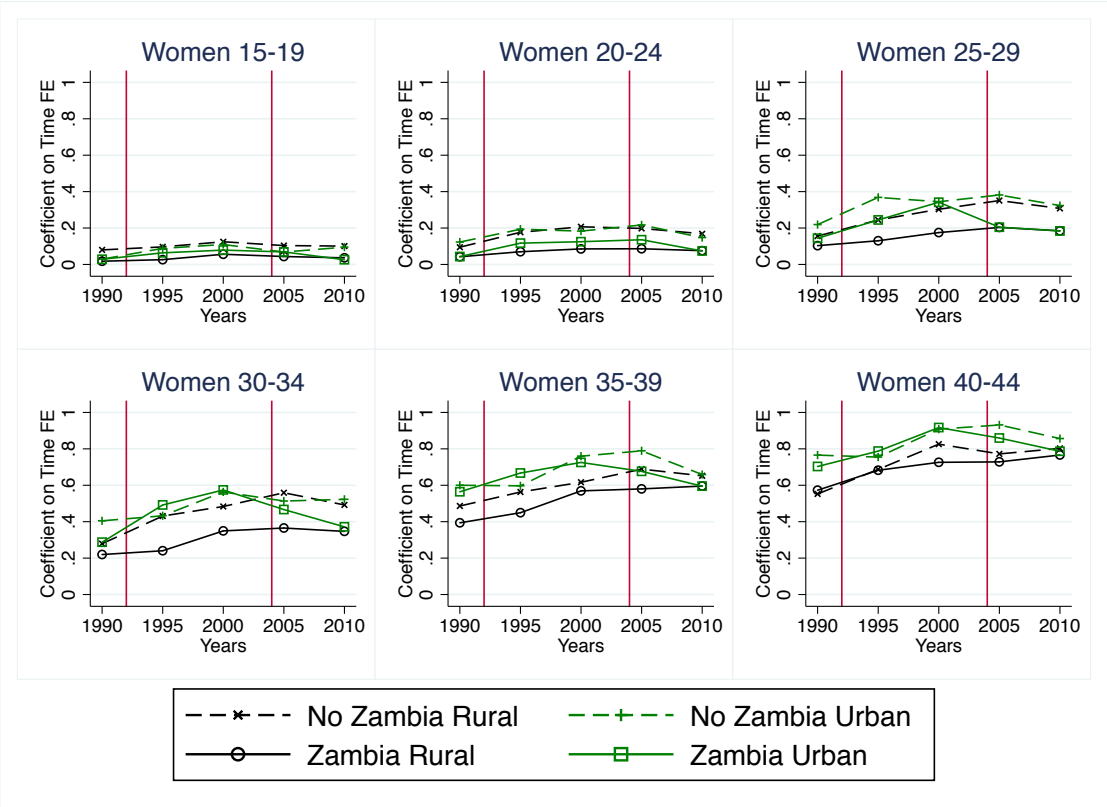
D.1 Event Studies for Desired Fertility & Education

Figure D.1: Event Studies for Desiring Another Child Across Age Groups



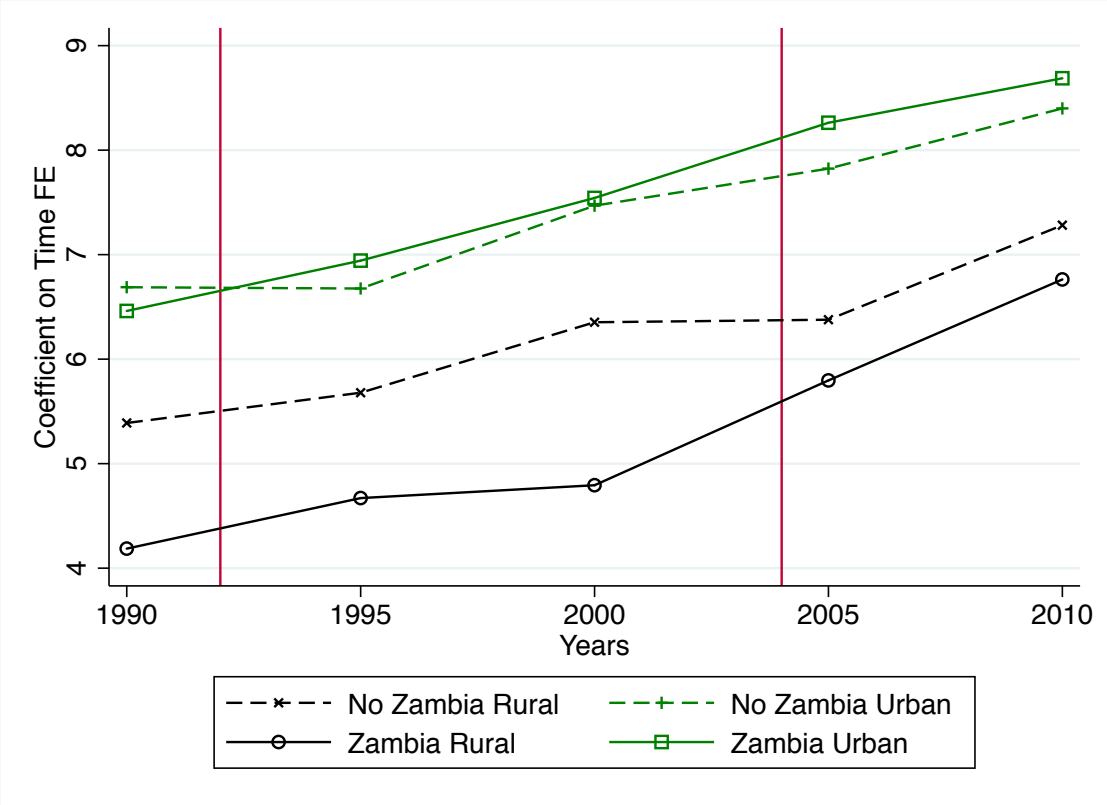
Notes: The graphs plot coefficients for γ_t^{zsa} from regression specification (2.2). The dependent variable is an indicator variable that takes value 1 if a woman reports wanting another child at survey date. All coefficients are shifted upward by the fraction of women who report wanting another child at survey date in sample zsa during the first time period 1990-95 to capture differences in baseline desire by sample. The left-most vertical red line corresponds to the 1992 legalization of injectable contraceptives in countries surrounding Zambia. The right-most vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

Figure D.2: Event Studies for Desiring No More Children Across Age Groups



Notes: The graphs plot coefficients for γ_t^{zsa} from regression specification (2.2). The dependent variable is an indicator variable that takes value 1 if a woman reports wanting no more children at survey date. All coefficients are shifted upward by the fraction of women who report wanting no more children at survey date in sample zsa during the first time period 1990–95 to capture differences in baseline desire by sample. The left-most vertical red line corresponds to the 1992 legalization of injectable contraceptives in countries surrounding Zambia. The right-most vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

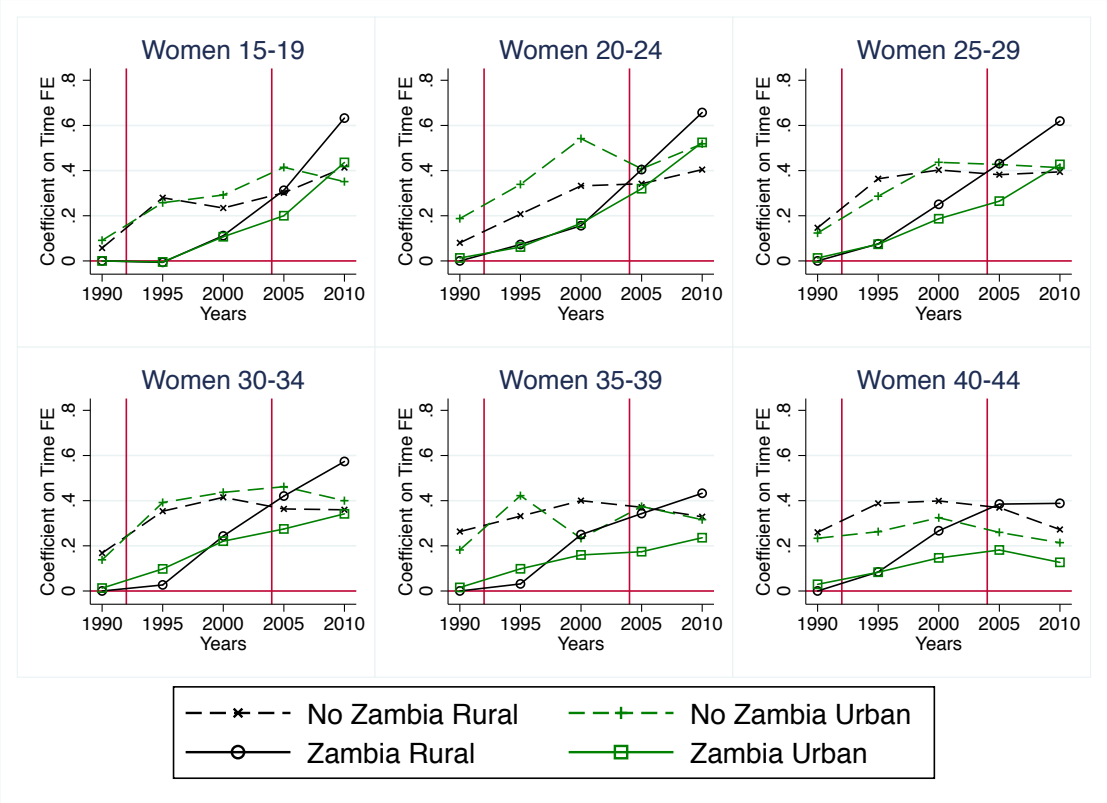
Figure D.3: Event Studies for Years of Schooling for Adolescent (15–19) Girls



Notes: The graphs plot coefficients for γ_t^{zsa} from regression specification (2.2). The dependent variable is a variable measuring women's self-reported completed years of schooling. All coefficients are shifted upward by the mean years of schooling completed by survey date by women in sample zsa during the first time period 1990–95 to capture differences in baseline education by sample. The left-most vertical red line corresponds to the 1992 legalization of injectable contraceptives in countries surrounding Zambia. The right-most vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

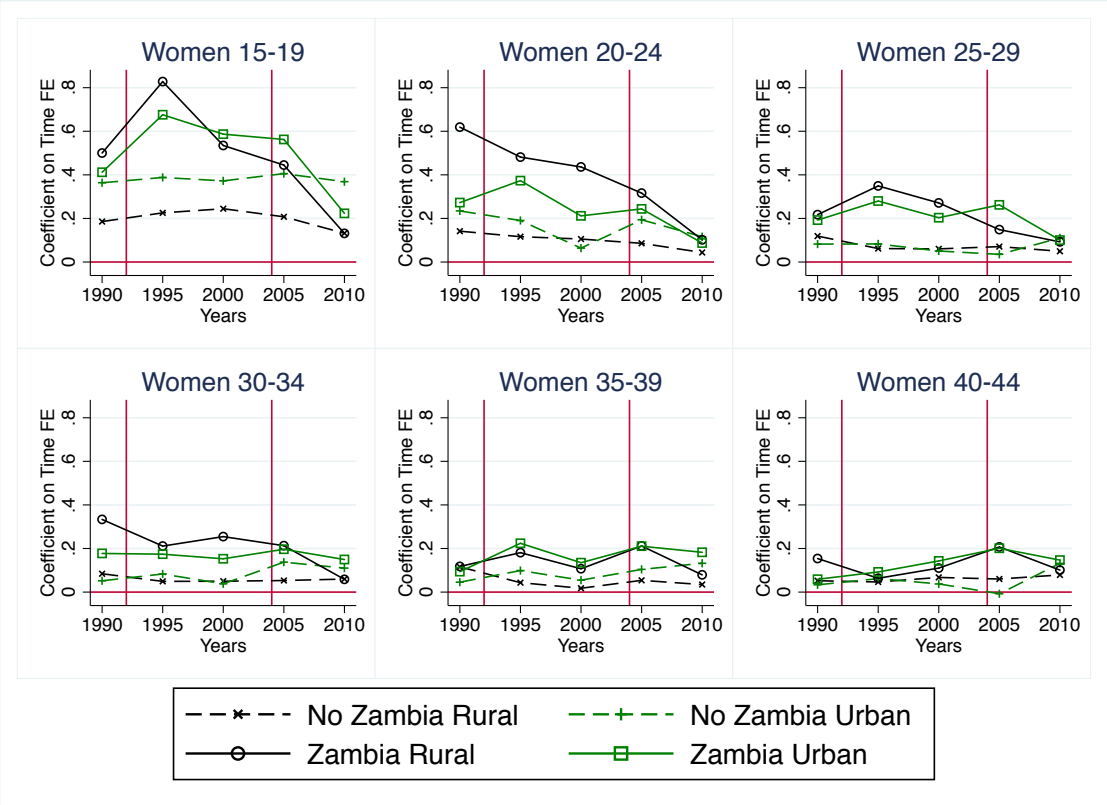
D.2 Event Studies for Method Mix Results

Figure D.4: Event Studies for Injectable Use Across Age Groups Among Modern Contraceptive Users



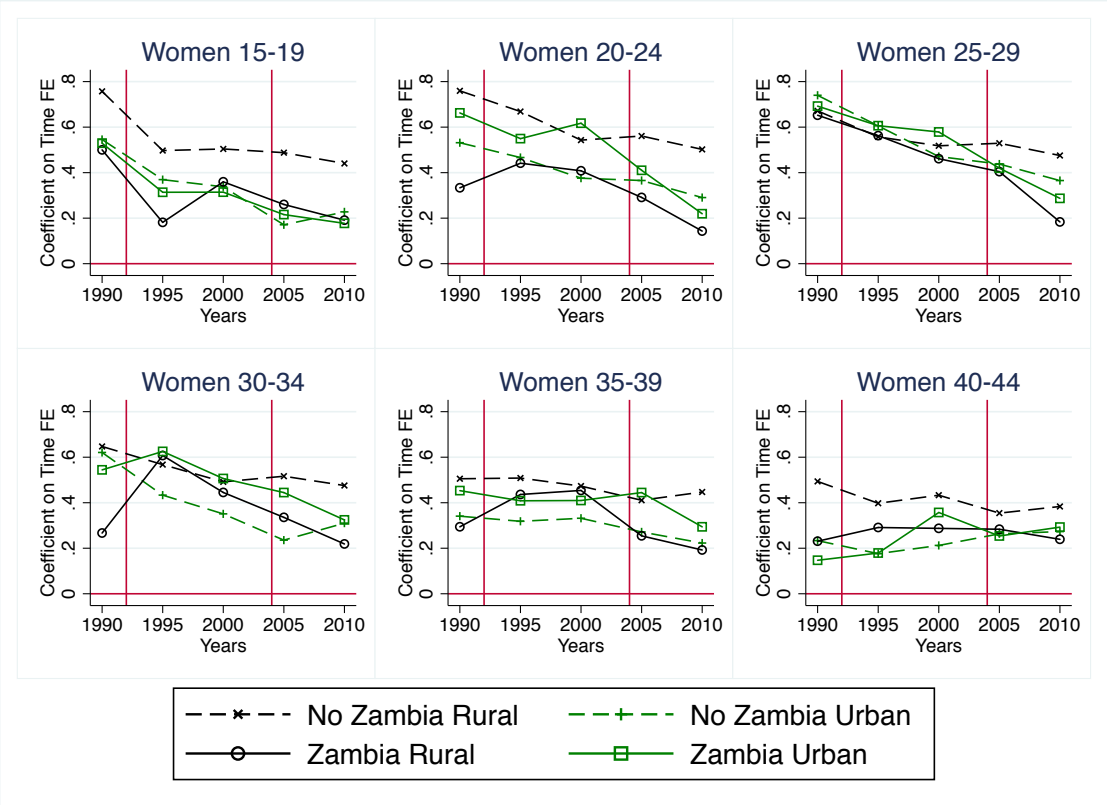
Notes: The graphs plot coefficients for γ_t^{zsa} from regression specification (2.2) restricted to the sample of women using modern contraceptives at interview date. All coefficients are shifted upward by the average injectable use among modern contraceptive users in sample zsa during the first time period 1990–95 to capture differences in baseline usage by sample. The dependent variable in specification (2.2) is an indicator variable that takes value 1 if a woman is using injectables at survey date. The left-most vertical red line corresponds to the 1992 legalization of injectable contraceptives in countries surrounding Zambia. The right-most vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

Figure D.5: Event Studies for Condom Use Across Age Groups Among Modern Contraceptive Users



Notes: The graphs plot coefficients for γ_t^{zsa} from regression specification (2.2) restricted to the sample of women using modern contraceptives at interview date. All coefficients are shifted upward by the average condom use among modern contraceptive users in sample *zsa* during the first time period 1990–95 to capture differences in baseline usage by sample. The dependent variable in specification (2.2) is an indicator variable that takes value 1 if a woman is using condoms at survey date. The left-most vertical red line corresponds to the 1992 legalization of injectable contraceptives in countries surrounding Zambia. The right-most vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

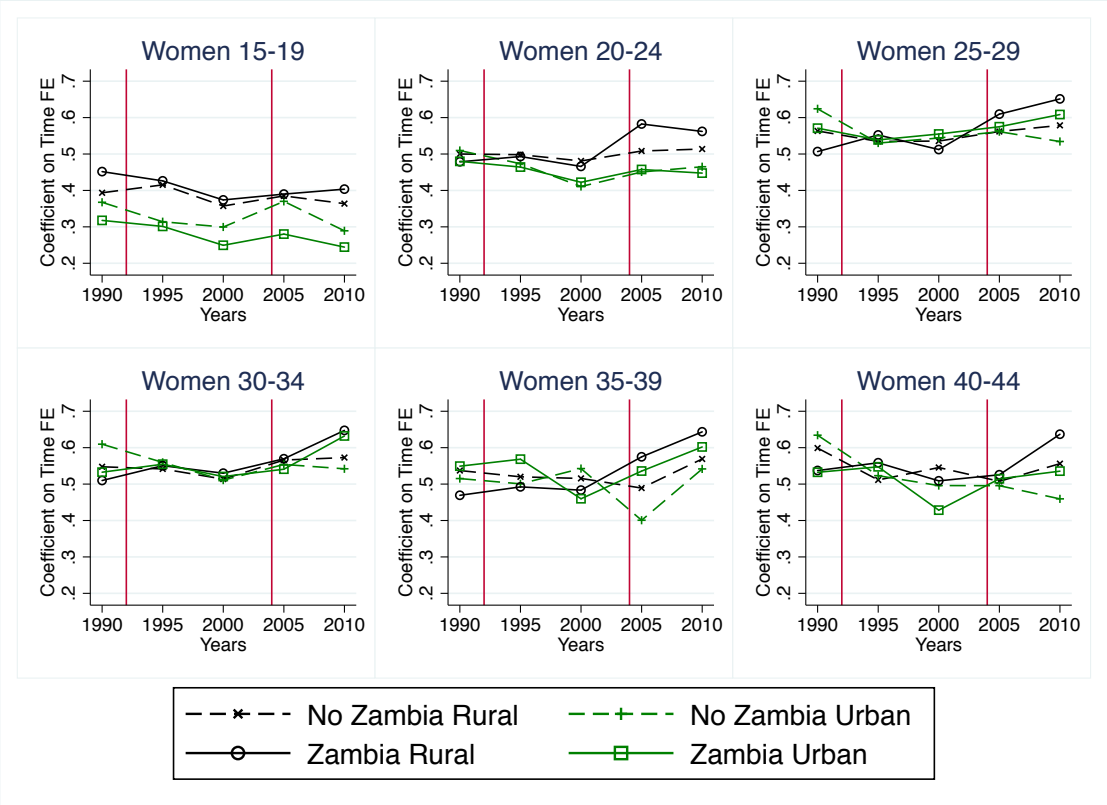
Figure D.6: Event Studies for Pill Use Across Age Groups Among Modern Contraceptive Users



Notes: The graphs plot coefficients for γ_t^{zsa} from regression specification (2.2) restricted to the sample of women using modern contraceptives at interview date. All coefficients are shifted upward by the average pill use among modern contraceptive users in sample *zsa* during the first time period 1990–95 to capture differences in baseline usage by sample. The dependent variable in specification (2.2) is an indicator variable that takes value 1 if a woman is using birth control pills at survey date. The left-most vertical red line corresponds to the 1992 legalization of injectable contraceptives in countries surrounding Zambia. The right-most vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

D.3 Event Studies for Traditional Fertility Control Methods

Figure D.7: Event Studies for Coital Frequency for Women 15–44



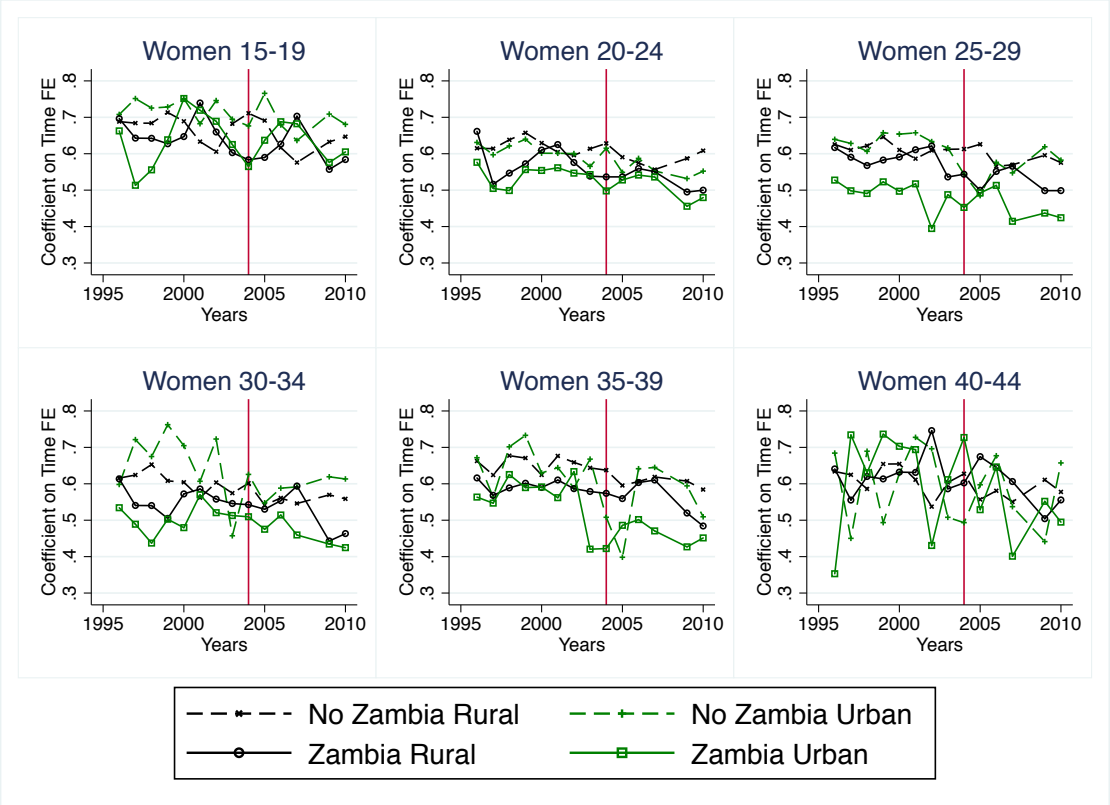
Notes: The graphs plot coefficients for γ_t^{zsa} from regression specification (2.2). The dependent variable in specification (2.2) is an indicator variable that takes value 1 if a woman reports having had sexual intercourse during the week prior to survey. All coefficients are shifted upward by the fraction of women reporting sexual intercourse during the past week in sample zsa in the first time period 1990–95 to capture differences in baseline coital frequency by sample. The left-most vertical red line corresponds to the 1992 legalization of injectable contraceptives in countries surrounding Zambia. The right-most vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

Figure D.8: Event Studies for Terminated Pregnancies for Women 15–44



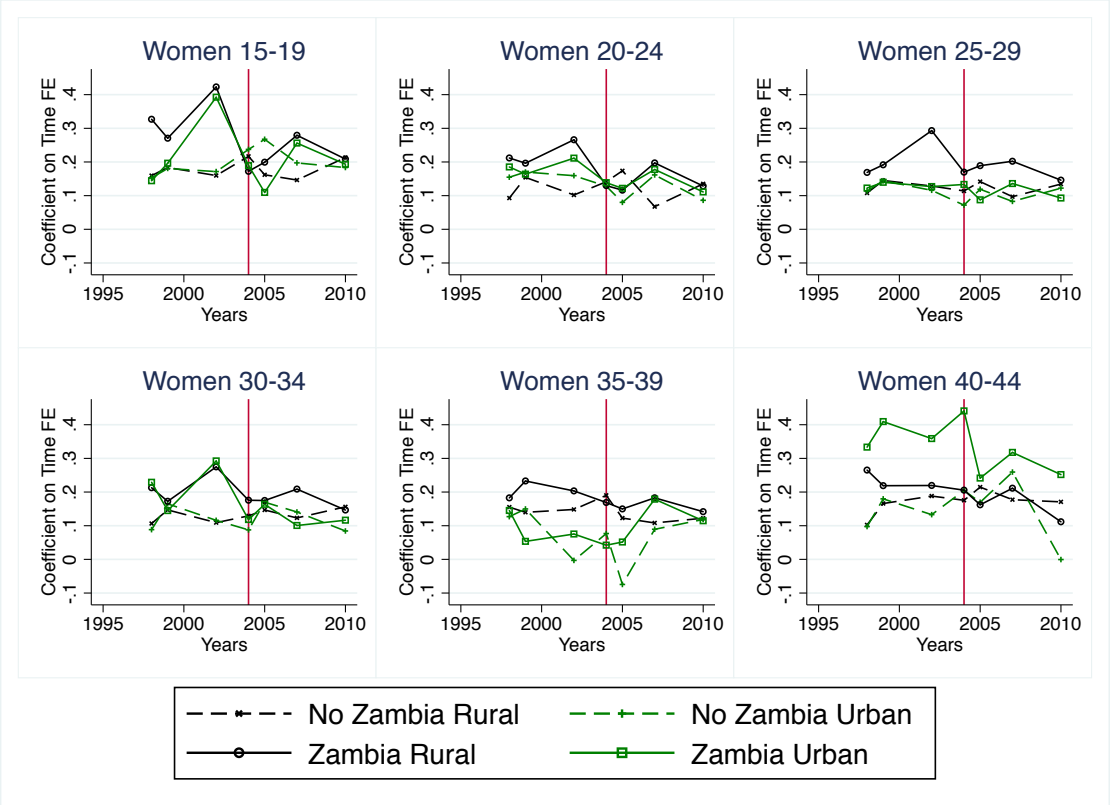
Notes: The graphs plot coefficients for γ_t^{zsa} from regression specification (2.2). The dependent variable in specification (2.2) is an indicator variable that takes value 1 if a woman reports ever having had a terminated pregnancy at survey date. All coefficients are shifted upward by the fraction of women reporting a terminated pregnancy in sample zsa in the first time fixed effect 2000–2004 to capture differences in baseline usage by sample. The vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

Figure D.9: Event Studies for Postpartum Abstinence (1–12 Months Post-Birth)



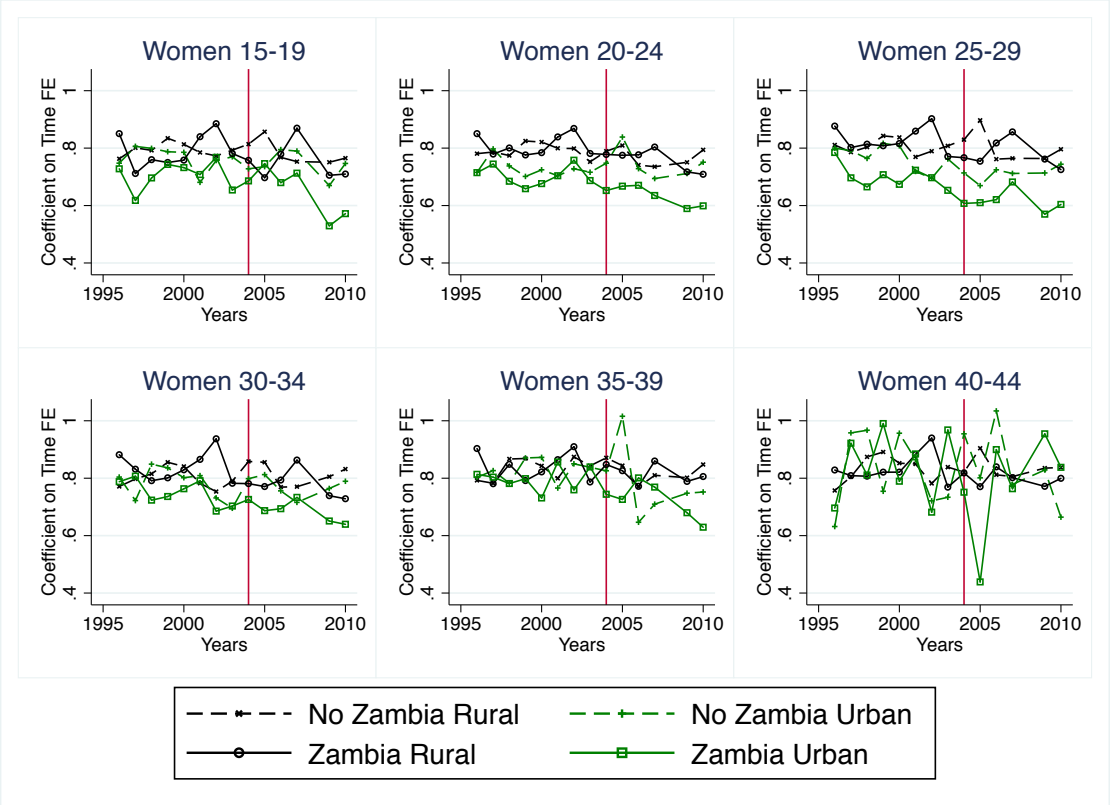
Notes: The graphs plot coefficients for γ_t^{zsa} from an event-study version of regression specification (E.2) that omits our treatment variable. The dependent variable is an indicator variable that takes value 1 if a woman reports postpartum abstaining in a given month postbirth. All coefficients are shifted upward by the fraction of women reporting that they are abstaining in sample zsa in 1996 to capture differences in baseline usage by sample. The vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

Figure D.10: Event Studies for Postpartum Abstinence (13–24 Months Post-Birth)



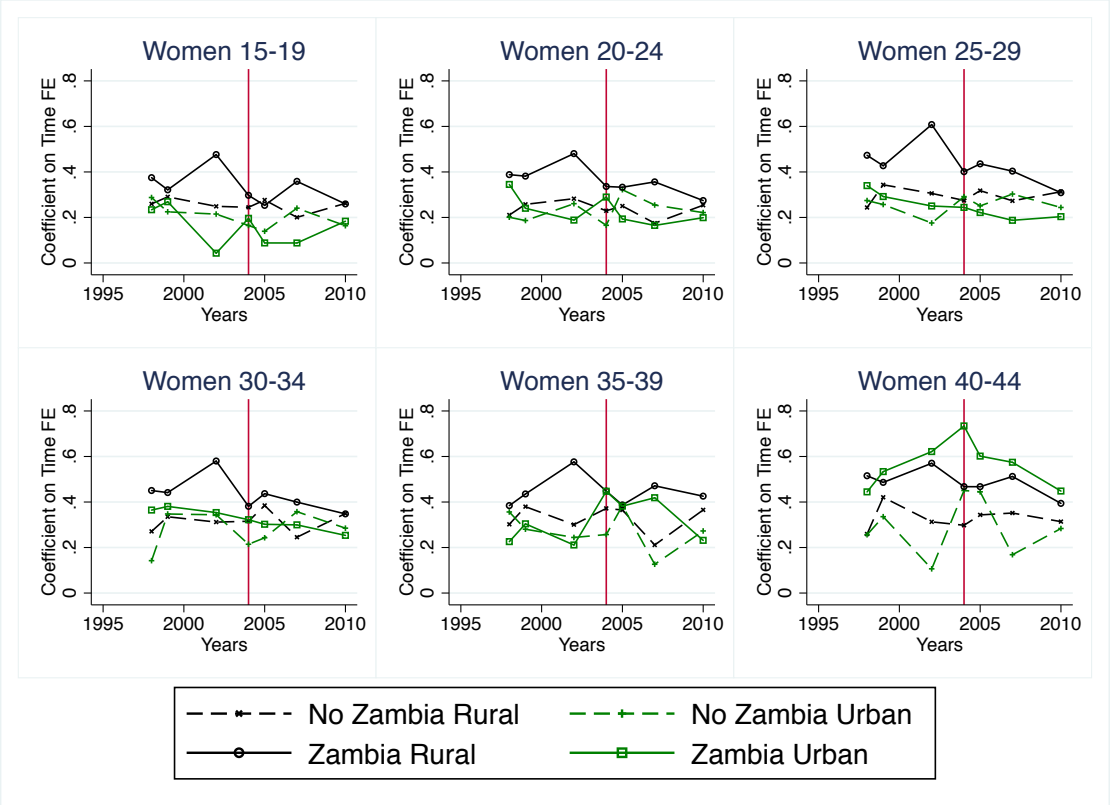
Notes: The graphs plot coefficients for γ_t^{zsa} from an event-study version of regression specification (E.2) that omits our treatment variable. The dependent variable is an indicator variable that takes value 1 if a woman reports postpartum abstaining in a given month postbirth. All coefficients are shifted upward by the fraction of women reporting that they are abstaining in sample zsa in 1996 to capture differences in baseline usage by sample. The vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

Figure D.11: Event Studies for Postpartum Amenorrhea (During 1–12 Months Post-Birth)



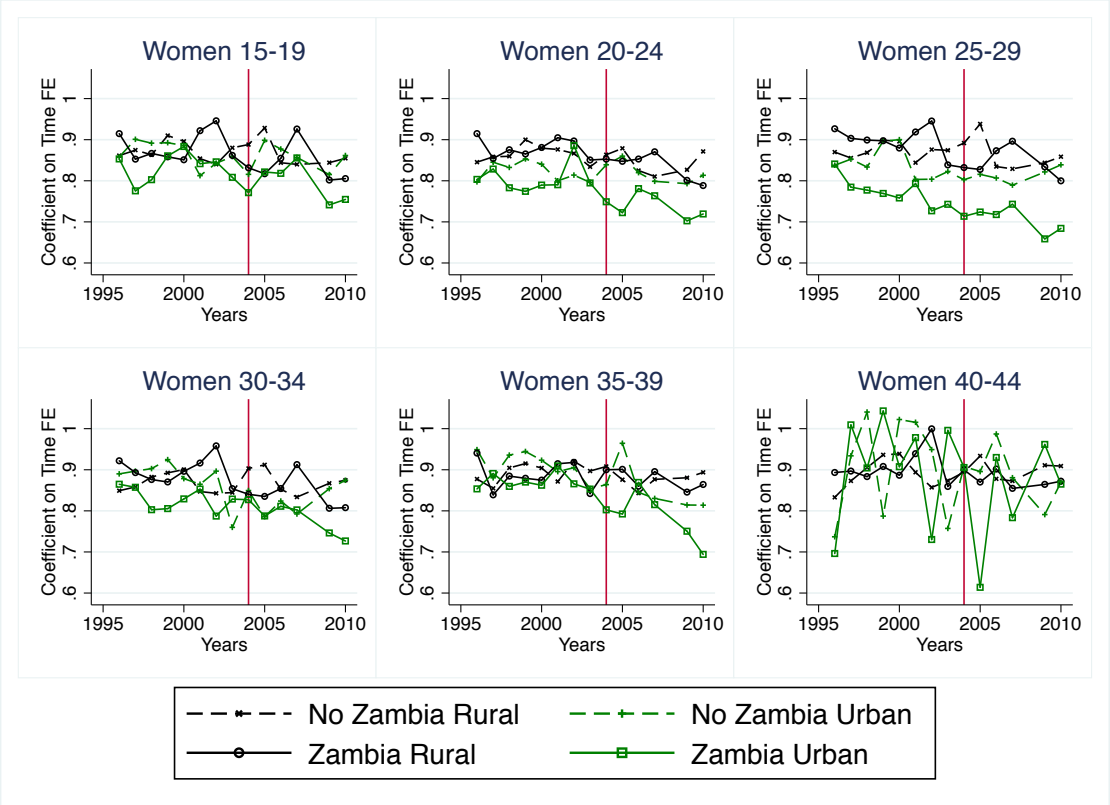
Notes: The graphs plot coefficients for γ_t^{zsa} from an event-study version of regression specification (E.2) that omits our treatment variable. The dependent variable is an indicator variable that takes value 1 if a woman reports experiencing amenorrhea in a given month post-birth. All coefficients are shifted upward by the fraction of women reporting that they are amenorrheic in sample zsa in 1996 to capture differences in baseline usage by sample. The vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

Figure D.12: Event Studies for Postpartum Amenorrhea (13-24 Months Post-Birth)



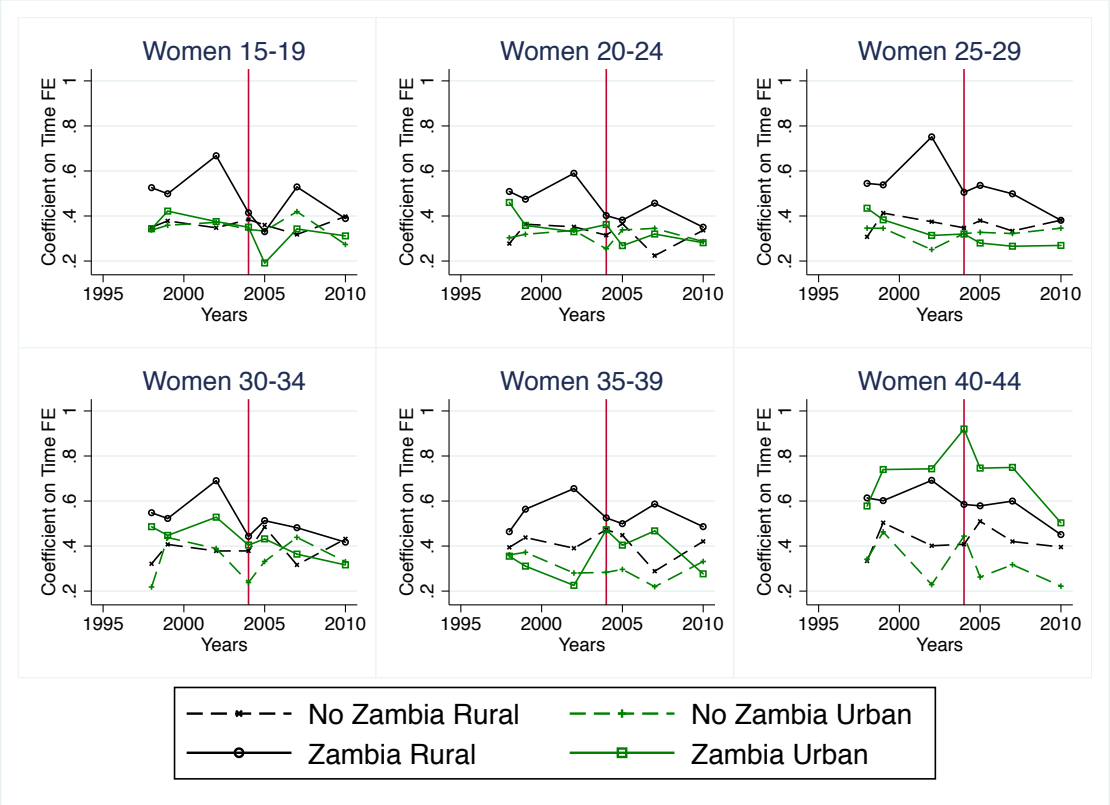
Notes: The graphs plot coefficients for γ_t^{zsa} from an event-study version of regression specification (E.2) that omits our treatment variable. The dependent variable is an indicator variable that takes value 1 if a woman reports experiencing amenorrhea in a given month post-birth. All coefficients are shifted upward by the fraction of women reporting that they are amenorrheic in sample zsa in 1996 to capture differences in baseline usage by sample. The vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

Figure D.13: Event Studies for Postpartum Protection Period During 1–12 Months Post-Birth



Notes: The graphs plot coefficients for γ_t^{zsa} from an event-study version of regression specification (E.2) that omits our treatment variable. The dependent variable is an indicator variable that takes value 1 if a woman reports that she is either abstaining or amenorrheic in a given month post-birth. All coefficients are shifted upward by the fraction of women reporting that they are either abstaining or amenorrheic in sample zsa in 1996 to capture differences in baseline usage by sample. The vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

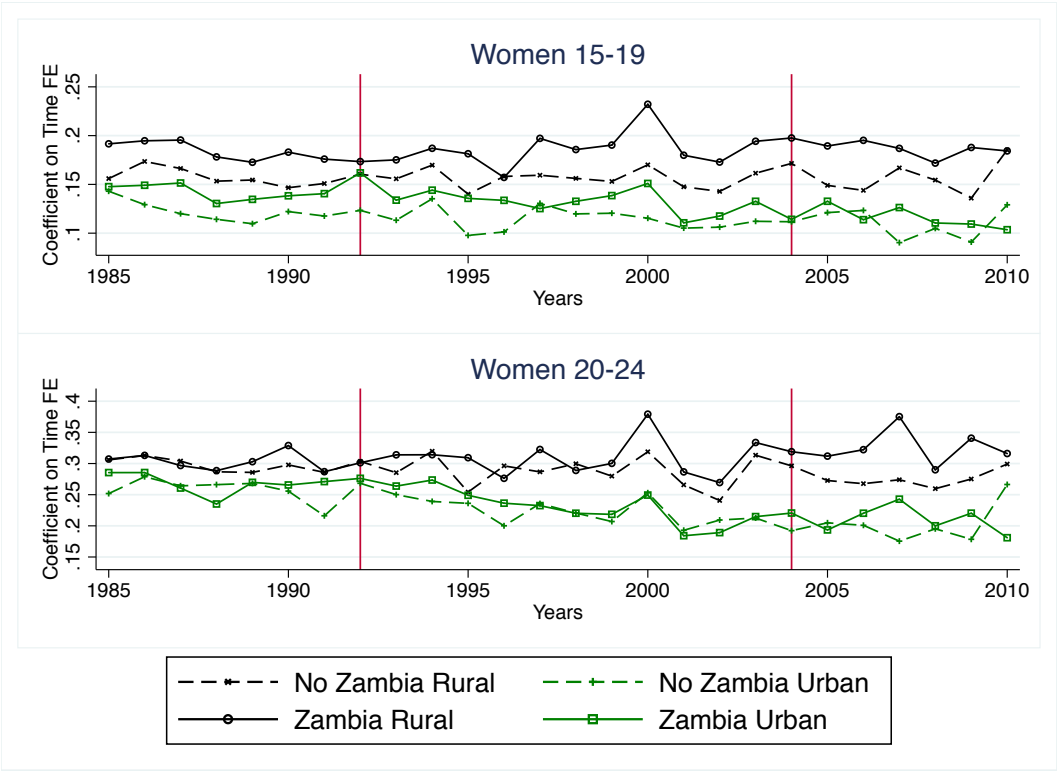
Figure D.14: Event Studies for Postpartum Protection Period During 13–24 Months Post-Birth



Notes: The graphs plot coefficients for γ_t^{zsa} from an event-study version of regression specification (E.2) that omits our treatment variable. The dependent variable is an indicator variable that takes value 1 if a woman reports that she is either abstaining or amenorrheic in a given month post-birth. All coefficients are shifted upward by the fraction of women reporting that they are either abstaining or amenorrheic in sample zsa in 1996 to capture differences in baseline usage by sample. The vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

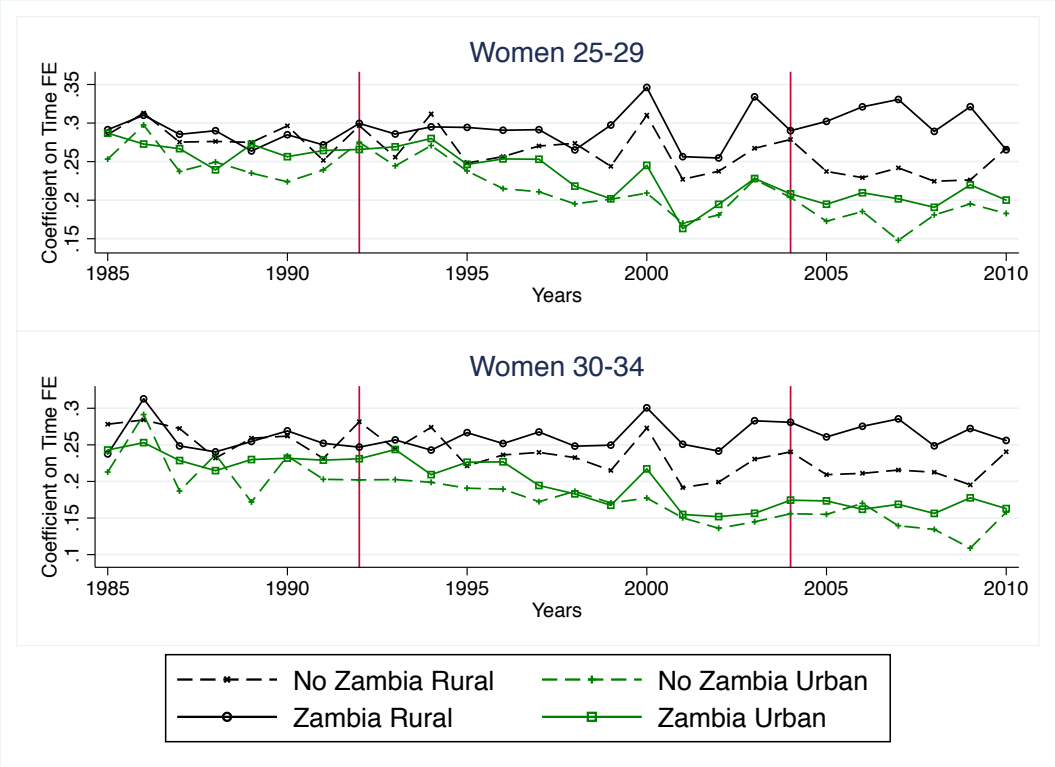
D.4 Event Studies for Fertility Results

Figure D.15: Event Studies for Flow Fertility For Women 15–24



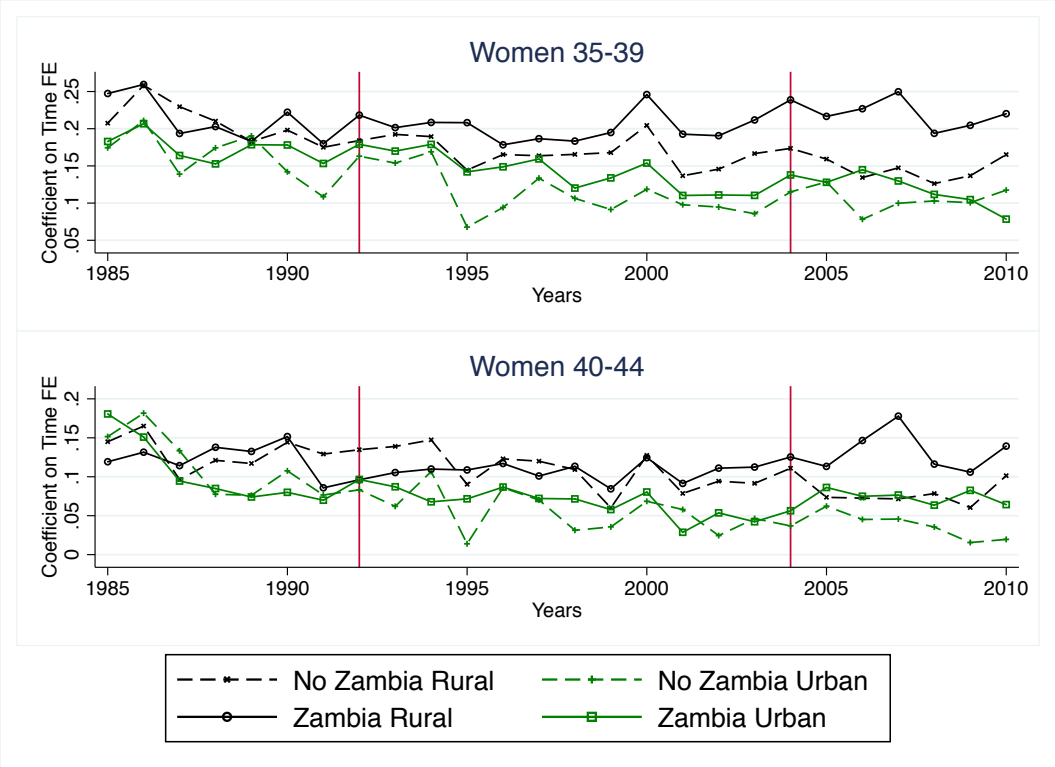
Notes: The graphs plot coefficients for γ_t^{zsa} from regression specification (2.2). All coefficients are shifted upward by the mean probability of giving birth in sample zsa in the first year 1985 to capture differences in baseline usage by sample. The dependent variable in specification (2.2) is an indicator variable that takes value 1 has a live birth in a given year. The left-most vertical red line corresponds to the 1992 legalization of injectable contraceptives in countries surrounding Zambia. The right-most vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

Figure D.16: Event Studies for Flow Fertility For Women 25–34



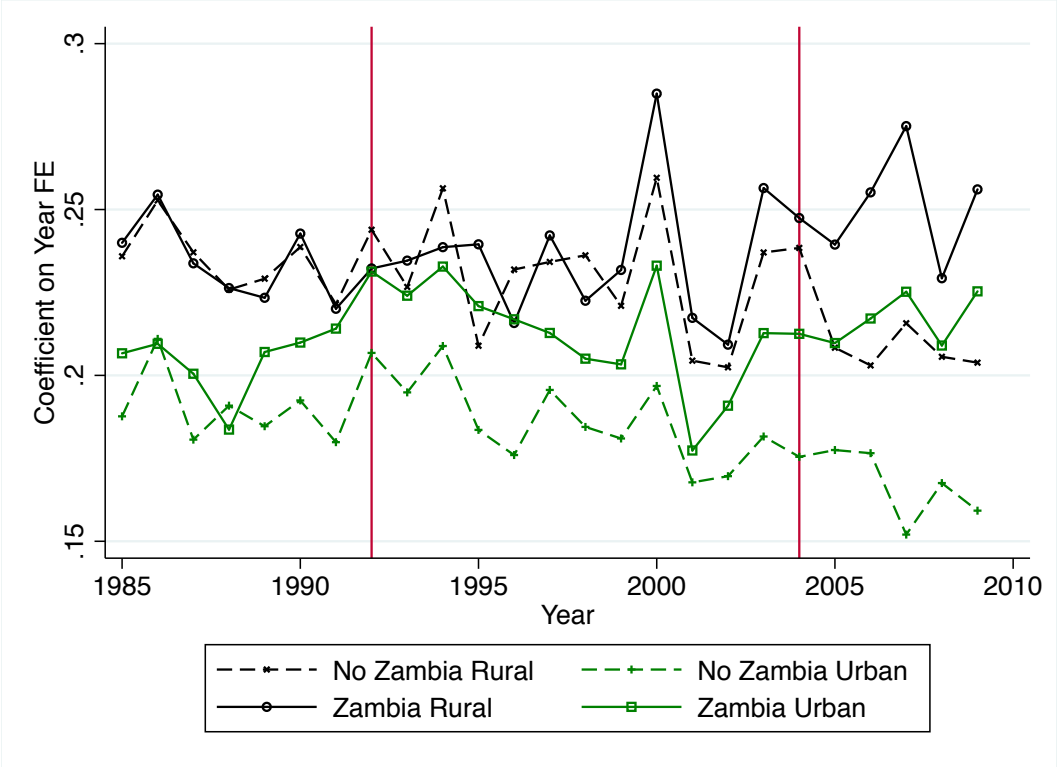
Notes: The graphs plot coefficients for γ_t^{zsa} from regression specification (2.2). All coefficients are shifted upward by the mean probability of giving birth in sample zsa in the first year 1985 to capture differences in baseline usage by sample. The dependent variable in specification (2.2) is an indicator variable that takes value 1 has a live birth in a given year. The left-most vertical red line corresponds to the 1992 legalization of injectable contraceptives in countries surrounding Zambia. The right-most vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

Figure D.17: Event Studies for Flow Fertility For Women 35–44



Notes: The graphs plot coefficients for γ_t^{zsa} from regression specification (2.2). All coefficients are shifted upward by the mean probability of giving birth in sample zsa in the first year 1985 to capture differences in baseline usage by sample. The dependent variable in specification (2.2) is an indicator variable that takes value 1 has a live birth in a given year. The left-most vertical red line corresponds to the 1992 legalization of injectable contraceptives in countries surrounding Zambia. The right-most vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

Figure D.18: Event Studies for Flow Fertility With Individual FE



Notes: The graphs plot coefficients for γ_t^{zs} from regression specification (2.3). All coefficients are shifted upward by the mean probability of giving birth in sample zs in the first year 1985 to capture differences in baseline usage by sample. The dependent variable in specification (2.4) is an indicator variable that takes value 1 has a live birth in a given year. The left-most vertical red line corresponds to the 1992 legalization of injectable contraceptives in countries surrounding Zambia. The right-most vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

E Supplementary Analysis

E.1 Paired Region Regression Specification

We estimate paired region regression specification by excluding Zambian regions bordering only the Congo, matching the remaining regions on either side of the Zambian border into three zones, and interacting the time fixed effects in specification (2.1) with indicator variables for our zones. Formally, the paired region regression specification is:

$$y_{irsapt} = \alpha + \beta T_{ir} + \delta_{sar} + \gamma_{sapt} + \xi X_{irsapt} + \varepsilon_{irsapt} \tag{E.1}$$

where i indexes individuals, r indexes regions, s indexes urbanity, a indexes age group, p indexes zone, and t indexes time. Identification for β is now additionally constrained to come from comparisons between pairs of women in the same zone, unlike in specification (2.1). The three paired region zones correspond to a Western zone, a Southern zone, and an Eastern zone. All other categories and controls are the same as in specification (2.1).

E.2 Postpartum Variables Regression Specification

On our *postpartum panel*, we estimate:

$$y_{irsamt} = \alpha + \beta T_{rt} + \delta_{sar} + \gamma_{sat} + \lambda_{sam} + \xi X_{irsamt} + \varepsilon_{irsamt} \tag{E.2}$$

where i indexes individual, r indexes region, s indexes sector, a indexes age-group, m indexes months since birth, and t indexes time (year-month). For each of the postpartum outcomes we study, the dependent variable is an indicator variable taking value one in a given month since birth if a woman is still experiencing that postpartum outcome. The treatment variable, time and space fixed effects, and individual controls are the same as in specification (2.1). We interact fixed effects for months since birth with fixed effects for sector and age-group to flexibly control for different hazard rates for women in different age groups and sectors. We

assess the validity of the parallel trends assumption by providing event studies for postpartum variables using a modified version of specification (2.2) that similarly controls for months since birth.

As questions on the durations of postpartum variables are only asked of live births during a 5–6 year window prior to survey date, when examining postpartum behaviors during the first year (two years) post-birth, we must omit the first year (two years) of the survey window to ensure that every time fixed effect includes observations for women in each month since last birth. Our regression samples consist of all information from 1996–2010, excluding the relevant portions of the survey window and excluding times for which information from at least four control group countries is missing due to non-synchronous survey rounds.

E.3 Birthspacing Regression Specification

We assess the impact of injectable legalization on birth-spacing using the following specifications on our *birth-spacing panel*:

$$y_{irsamt} = \alpha + \beta T_{rt} + \delta_{sar} + \gamma_{sat} + \tau_{sam} + \xi X_{irsamt} + \varepsilon_{irsamt} \tag{E.3}$$

$$y_{isbmt} = \alpha + \beta T_{rt} + \gamma_{st} + \lambda_b + \tau_m + \xi X_{isbmt} + \phi_i + \varepsilon_{isbmt} \tag{E.4}$$

where i indexes individual, r indexes region, s indexes sector, a indexes age-group, b indexes age in years, m indexes month since last birth, and t indexes time (year-month). We estimate our birth-spacing specifications separately for each of the seven years following a live birth.[59] The dependent variable in both specifications is an indicator taking value one if a woman reports a live birth in a given month. As birth-spacing is a duration outcome, we flexibly control for months since last birth (interacted with fixed effects for sector and age group in specification (E.3)) to control for hazard rates. The treatment variable, fixed effects, and controls are all identical to specifications (2.1) and (2.3). Identification of β in both

[59] We assume that women cannot have a subsequent birth during the nine months immediately following a live birth. Hence, the first "year" after a live birth corresponds to 9–20 months post-birth.

specifications is analogous to our identification when studying flow fertility, with specification (E.3) using both across-women and within-woman variation to identify β, and specification (E.4) using only within-woman variation.

E.4 Changes in the Intensive Margin of Abortion Use

We utilize information from contraceptive calendars and birth histories to construct a *pregnancy panel* that records pregnancies borne by a woman during the 5–6 years prior to survey date. This dataset includes information on whether each pregnancy resulted in a live birth or was terminated.[60] Terminated pregnancies are underreported,[61] and the *pregnancy panel* includes only a subset of all pregnancies experienced by interviewed women during the 5–6 years prior to survey.

We use our *pregnancy panel* to study whether the fraction of pregnancies reported as terminated changes in response to improved contraceptive access. The contraceptive calendar information used to construct our pregnancy panel contains substantial recall error: plotting the time trend for the fraction of pregnancies reported as terminated while controlling flexibly for region and age yields a distinct sawtooth pattern (see Figure E.1). To address this, we estimate a specification similar to (2.1) that flexibly accounts for recall error:

$$y_{irsadt} = \alpha + \beta T_{rt} + \delta_{sar} + \gamma_{sat} + \lambda_{sad} + \xi X_{irsadt} + \varepsilon_{irsadt} \tag{E.5}$$

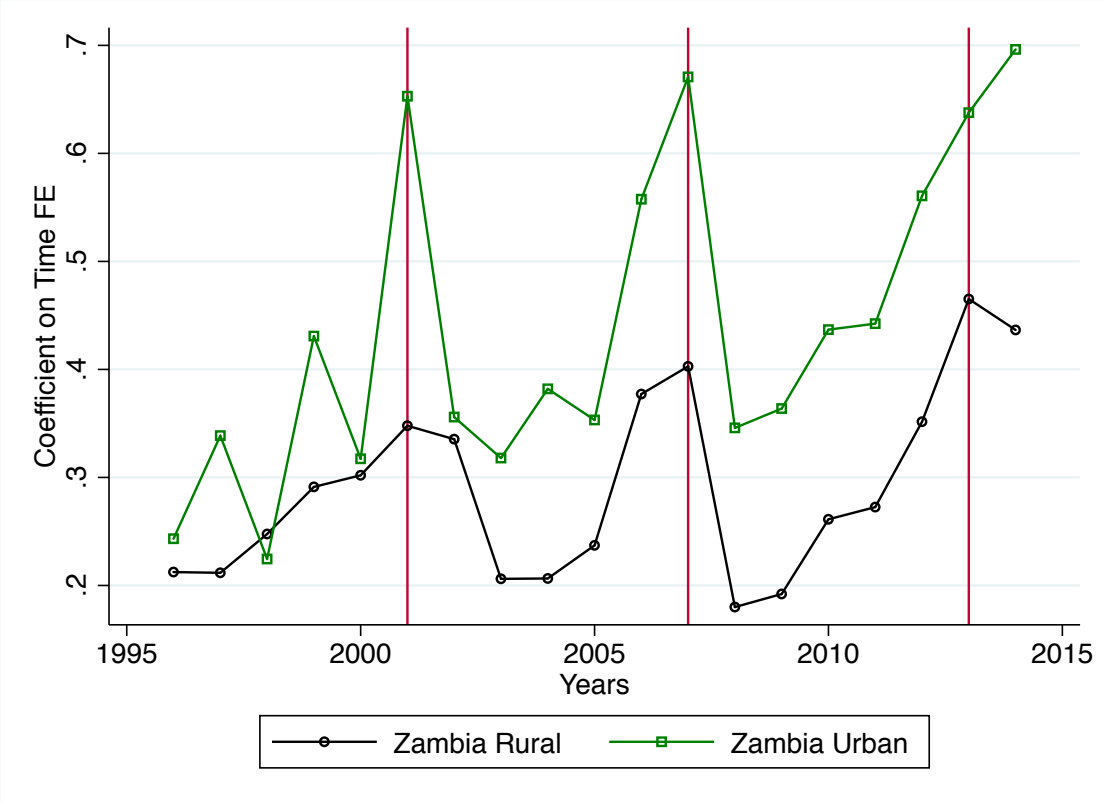
where i indexes individual, r indexes region, s indexes sector, a indexes age group, d indexes recall year, and t indexes year. The dependent variable is an indicator variable taking value one if a women reports a given pregnancy as resulting in a termination. The treatment variable, time and space fixed effects, and individual-level controls are the same as in specification (2.1). We measure recall year as years to date of survey, and exploit staggered months

[60] Terminated pregnancies include abortions, miscarriages, and stillbirths.

[61] For example, miscarriages that occur during the first trimester are often under-reported as respondents may not realize they are pregnant at the time of miscarriage and the miscarriage presents as a delayed, severe menstrual cycle. Similarly, abortions may be under-reported due to stigma.

of survey within each survey round to include fixed effects for both recall year and year.[62] We interact fixed effects for recall year with effects for sector and age-group to flexibly control for different recall patterns by sector and age-group. We restrict our regression sample to pregnancies during 2002–2007, years corresponding to the recall period for the 2007 Zambian DHS round, to examine changes in the intensive margin of terminated pregnancies in a window with three years of information prior to injectable legalization and three years of information after injectable legalization.

Figure E.1: Recall Effects in Contraceptive Calendar



Notes: The graphs plot coefficients on year fixed effects from regression specification (2.2) for the urban and rural sectors in Zambia, pooling across age groups. All coefficients are shifted upward by the mean probability of reporting a terminated pregnancy in a given sector in 1996. The dependent variable is an indicator variable taking value one if a pregnancy was reported as terminated. The three vertical red lines correspond to the three survey years in Zambia for which we have information on terminated pregnancies.

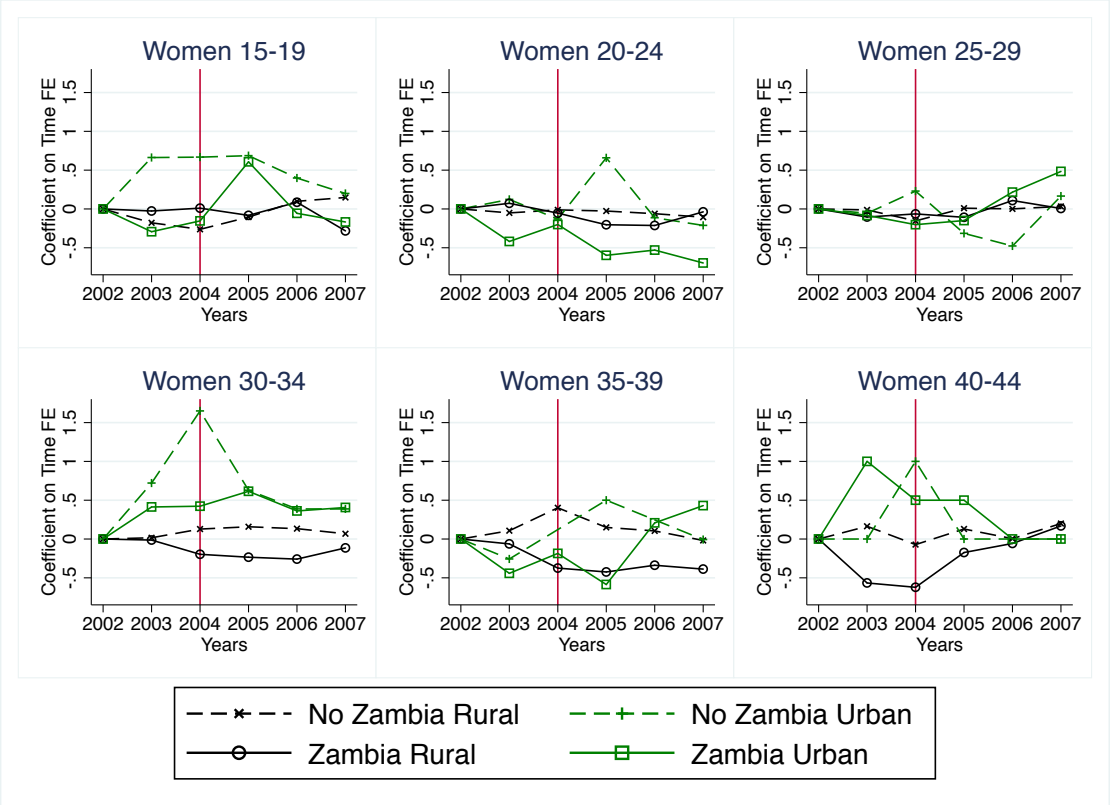
[62]For example, a woman interviewed in June of 2007 will be in her first recall year from July 2006 – June 2007, in her second recall year from July 2005 – June 2006, etc.

Table E.1: Changes in Flow Abortion Use During 2002–2007

	All Areas		Rural Areas		Urban Areas	
Post Inj	-0.0568	-0.0632+	-0.0961**	-0.1003***	0.1888	0.1616
	(0.0330)	(0.0307)	(0.0351)	(0.0340)	(0.2128)	(0.1750)
Observations	3,962	3,962	3,230	3,230	732	732
Mean	0.318	0.318	0.298	0.298	0.407	0.407
Clusters	18	18	18	18	18	18
Controls	N	Y	N	Y	N	Y

Notes: The dependent variable in these regressions is an indicator variable that takes value 1 if a woman reports ever having had a terminated pregnancy (miscarriage, stillbirths, and abortions) at survey date. The reported coefficient and standard errors are for the term β in regression (2.1). All standard errors are clustered at the region level. *** p<0.01, ** p<0.05, $^{+}$ p<0.1

Figure E.2: Event Studies for Terminated Pregnancies Using Contraceptive Calendar Information



Notes: The graphs plot coefficients for γ_t^{zsa} from regression specification (2.2) adapted to include controls for recall year. All coefficients are shifted upward by the mean probability of reporting a terminated pregnancy in sample zsa in the first time fixed effect 2000–2004 to capture differences in baseline usage by sample. The dependent variable is an indicator variable that takes value 1 if a woman reports ever having had a terminated pregnancy at survey date. The vertical red line corresponds to the 2004 legalization of injectable contraception in Zambia.

F Data Appendix

Table F.1: Criterion for Legal Abortions

Abortions Legal for:	Zambia	Tanzania	Malawi	Mozambique	Zimbabwe	Namibia
Risk to life	Y	Y	Y	Y	Y	Y
Physical Health	Y	Y	N	Y	Y	Y
Mental Health	Y	Y	N	Y	N	Y
Rape/Incest	N	N	N	N	Y	Y
Fetal Impairment	Y	N	N	N	Y	Y
Socioeconomic Grounds	Y	N	N	N	N	N
By choice	N	N	N	N	N	N

Notes: Criterion taken from the UN.

Chapter 3

Within-Mother Estimates of the Effects of WIC on Birth Outcomes in New York City

*with Janet Currie**

3.1 Introduction

There is growing acceptance of the idea that prenatal conditions can have health effects not only at the time of the birth, but also lasting into adulthood (Currie and Almond (2011)). This observation leads people to question whether there are "shovel ready" policies that can be implemented to improve children's life chances. The U.S. Supplemental Nutrition Program for Women, Infants, and Children (called WIC) would appear to be ideally suited to play such a role. The program provides nutritious food, counseling, and assistance with access to medical care to pregnant women, as well as to their infants and young children. Bitler et al. (2003) show that the program serves a large fraction of pregnant women in the United States. In fiscal year 2011, 9 million participants were served each month including

*This chapter has been published as Currie and Rajani (2015).

2.1 million pregnant or lactating women, which can be compared with the roughly 4 million births in that year (USDA (2012)).

Yet, despite decades of research suggesting that "WIC works" in the sense that women who use WIC during pregnancy have healthier babies than other similar women, the suspicion persists that the observed positive association between WIC participation and outcomes might not be causal.[1] There are two limitations to most WIC research.[2] First, as women choose whether or not to participate in WIC and when to begin participating, it is possible that women who seek out WIC benefits are different from other observationally similar women in some unobservable way that is conducive to better birth outcomes. Despite this concern however, there has been little direct examination of the determinants of selection into the program. Second, the longer a woman is pregnant, the longer she has to enroll in WIC. Hence, a positive correlation between WIC and length of gestation could reflect this "longer window of opportunity" effect. Moreover, as the fetus gains most of its weight in the third trimester, if women with longer gestations are more likely to sign up for WIC, then we would also expect their babies to have higher birth weights.

This article addresses these limitations of the previous literature. Starting with a sample of all New York City births from 1994 to 2004, we focus on women with more than one singleton birth over the sample period and estimate models with mother fixed effects. In these models, the effect of WIC is identified by comparing births to the same mother, with and without WIC. This specification controls for the fact that women who use WIC may be different in some unobservable fixed respect than women who do not. It is still possible however, that time-varying characteristics of mothers are associated with both WIC participation and positive birth outcomes. Hence, we also use our data to directly investigate this type of selection and ask when mothers are most likely to use WIC in terms of these time varying characteristics.

[1] See Currie (2003) for a review of the older WIC literature and a discussion of its limitations. Also see Besharov and Germanis (2001) and Joyce et al. (2005) for further discussion of the limitations of the WIC literature.

[2] See Ludwig and Miller (2005).

In order to deal with the possibility of a mechanical correlation between gestation length and probability of WIC enrollment, we focus much of our attention on full-term births as well as asking whether infants were "small for dates" (i.e., below the 10th percentile of national weight standards given gestational age). We also ask whether the effects of WIC are bigger for first born children. Women pregnant with their first born child may lack information about healthy practices and may not know that they are at risk for poor birth outcomes, so it is possible that WIC would have particularly large effects in this group.

We are able to document important facts about selection into the WIC program — mothers who change WIC status between births are more likely to use WIC when they are younger, unemployed, and/or unmarried. Women are also much more likely to be referred to WIC if they have had a previous poor pregnancy outcome. However, mothers are also more likely to be diagnosed with chronic hypertension or chronic diabetes when they are on WIC suggesting that WIC may improve access to medical care and/or screening for these conditions.

We find that WIC is associated with reduction in low birth weight, even among full-term infants, and with reductions in the probability that a child is "small for dates." These improvements are associated with a reduction in the probability that the mother gained too little weight during pregnancy. Effects on low birth weight are concentrated in first born infants, although effects on being small for dates and weight gain are present for all WIC mothers. These findings suggest that WIC counseling may be particularly useful for first time mothers. We also find that women on WIC and their infants receive more intensive medical services, a finding that may reflect improved access to medical care.

3.2 Background

This section does not attempt to review all of the voluminous literature on WIC (see Currie (2003) for a review of the older literature). Instead, we first provide an overview of the

program, and then focus on studies that have attempted to address some or all of the empirical problems outlined above, and discuss how our study builds on and adds to these efforts.

WIC is a supplemental feeding program for pregnant and lactating women, infants, and children up to age 5. In addition to belonging to one of these groups, a WIC participant must either have income less than 185% of the federal poverty line, or must have Medicaid coverage (available to pregnant women with incomes less than 200% of the federal poverty line. In New York, the threshold was about $38,000 for a family of three in 2012). Women must also be evaluated and deemed to be "at nutritional risk," though in practice this requirement does not appear to be very binding (Bitler et al. (2003)).

WIC clients receive coupons or a debit card which can be used to purchase only specifically selected nutritious food items.[3] In 2012, New York WIC provided food benefits worth an average of $55 per participant per month.[4] To maintain their benefits, clients are required to attend nutritional counseling and breastfeeding education sessions at regular intervals. One of the open questions about WIC is why it works (if indeed it does) given that the benefits are small relative to people's monthly food budgets. Possible answers are that the healthy WIC foods available or the nutritional counseling nudge people's diets in a healthier direction, or that the availability of WIC benefits helps to facilitate access to medical care.

As discussed above, most studies of WIC have difficulty dealing with possible nonrandom selection of women into WIC benefits. If women who enroll in WIC are, for example, more concerned about their child's health, or more motivated to change their behaviors than other women, then the measured positive effects of WIC could reflect these unobserved characteristics of mothers who enroll in the program.

[3]As an example, the New York regulations state the following about infant cereal: "This is the only brand of cereal allowed for infants up to 12 months old: Gerber Cereal for Baby; Plain varieties only: barley, oatmeal, rice, whole wheat, or mixed; 8 and 16-ounce containers are allowed; NOT ALLOWED: Organic, extra ingredients such as DHA, fruit, formula, or added protein."

[4]https://www.fns.usda.gov/pd/25wifyavgfd.htm.

In view of this concern, it is surprising that few WIC studies have estimated models with maternal fixed effects because these models offer a way to control for all observed and unobserved fixed characteristics of mothers.[5] The study closest in spirit to ours is Kowaleski-Jones and Duncan (2002) who use data from the Children of the National Longitudinal Survey of Youth and estimate mother fixed effects models. Unfortunately, they had only 104 sibling pairs, and only 71 with discordant WIC participation. Nevertheless, they found a statistically significant effect on birth weight (of 7 ounces) and a positive effect on temperament scores. They did not find a statistically significant effect on measures of motor and social skills.

Rossin-Slater (2013) estimates maternal fixed effects models in a study of the effects of the closure of WIC clinics in Texas. She finds that having a WIC clinic in one's zip code increases maternal weight gain and birth weight, but is also associated with more diagnoses of diabetes and hypertension during pregnancy. She speculates that this latter result might reflect better access to medical care, which in turn leads to higher diagnosis rates.

Joyce et al. (2005) also examine birth records for New York City. In order to deal with selection, they focus on relatively narrowly defined groups, for example, Hispanic mothers with a first pregnancy, and use matching models with propensity scores. In order to deal with the mechanical correlation between gestation and the window available for WIC enrollment (i.e., the fact that longer pregnancies both allow more time to enroll and also generally result in healthier infants), they focus on birth weight conditional on gestational age. Joyce et al. (2008) examine data from nine states and ask whether earlier initiation of WIC is associated with better maternal behaviors and better birth outcomes. Both papers argue that a positive association between WIC and gestation is suspect for the reasons given above. Both of these papers do however find positive effects of WIC on infant outcomes, though they argue that the effects are smaller than those found in the previous literature.

[5] A third study, by Hicks and Langham (1985) focuses on the beginning of the WIC program in Louisiana. They look at 19 sibling pairs in which, due to the timing of the phase in, one child received WIC in utero, and the other did not receive it until they were already a year old. They find positive effects of prenatal WIC participation on IQ and cognitive test scores.

Bitler and Currie (2005) and Figlio et al. (2009) also attempt to deal with selection by narrowing the comparison group. Bitler and Currie (2005) focus on a large national sample of mothers whose births were paid for by Medicaid (so that all of the mothers were adjunctively eligible for WIC). Figlio et al. (2009) use administrative data from Florida and focus on families in a narrow income band around the WIC eligibility threshold. They also use a tightening of WIC income documentation requirements to try to identify the effect of WIC receipt. Using this design, they find a strong effect of WIC on the incidence of low birth weight (birth weight less than 2,500 g) even though, as they point out, the marginal mother eligible for WIC may benefit less than inframarginal recipients.

Hoynes et al. (2011) focus on the roll out of the WIC program in the late 1970s in a difference-in-differences framework and show that in counties that adopted WIC, the incidence of lower birth weight fell. However, arguably the factors affecting the nutritional status of pregnant women could be quite different today than they were in the 1970s, given sharp declines in the relative price of food over time, as well as large increases in obesity.

This brief review indicates that our study is one of very few to examine the effects of WIC in models that control for mother fixed effects. It is also the first large-scale analysis of selection into WIC, and the first to examine effects of WIC by birth order. The exploration of within-mother selection into WIC and of the effects of WIC on firstborns is of independent interest and sheds light on the way that "WIC works."

3.3 Methods

In order to investigate the way that time varying maternal characteristics are associated with participation in the WIC program, we estimate models of the form:

$$Z_{it} = a_i + a_1 WIC + a_2 X_{it} + a_3 Month + a_4 Year + a_5 Neighborhood + e_{it}, \qquad (3.1)$$

where Z_{it} is one of a vector of maternal characteristics including prepregnancy weight, whether she has chronic diabetes, whether she has chronic hypertension, age, whether she smoked at all during the pregnancy, whether she was employed, whether she was married, and whether the mother had a previous poor pregnancy outcome (the latter outcome can only be examined in mothers who had a previous birth that is included in our data set). The vector a_i represents a fixed effect for each mother. The vector X_{it} includes time varying characteristics of the mother and child which might be associated with selection into the program including child gender, maternal education (<12, 12, 13–15, 16+, missing), parity (1, 2, 3, 4+, missing), and maternal age (<20, 20–24, 25–29, 30–34, 35+, missing). (Note that these age dummies are omitted when the dependent variable in question is maternal age.) The coefficient of interest is a_1, which measures the correlation between changes in the time-varying dependent variables and changes in WIC status between pregnancies.

We also include dummy variables for each month and year of birth in order to control for time trends in these variables, and for maternal neighborhoods within New York City, because neighborhood is an important indicator of economic and social status.[6] Finally, e_{it} represents a random error term.

Our object in estimating equation (3.1) is to determine whether time-varying characteristics of mothers are systematically related to WIC enrollment. If we find that a mother is more likely to be on WIC when she has a dangerous chronic condition that complicates her pregnancy, then we should take this information into account when interpreting the within-mother estimates of the effects of WIC participation on birth outcomes. For example, chronic diabetes is associated both with a higher risk of a small and preterm baby, and with a higher risk of a very large baby with a complicated delivery. Children of diabetic mothers also have a higher incidence of congenital anomalies and so may be at higher risk of negative developmental outcomes (Nold and Georgieff (2004)). If mothers diagnosed with

[6]Neighborhoods are defined by the Department of City Planning and are at the level of areas such as "Brighton Beach," "Clinton Hill," or "Hunts Point." See http://www.nyc.gov/html/dcp/html/bytes/dwn_npa.shtml.

diabetes were more likely to be referred to WIC, then this type of selection could bias the estimated effect of WIC on birth outcomes.

Turning to outcomes of pregnancy that we might expect to be affected by the WIC program, we estimate models of the form:

$$Y_{it} = a_i + a_2 WIC_{it} + a_3 Z'_{it} + a_4 X_{it} + a_5 Month + a_6 Year + a_7 Neighborhood + e_{it}, \quad (3.2)$$

where Y_{it} is a vector of outcomes including whether the baby is low birth weight, whether the baby is small for dates, whether the baby had a low APGAR (Appearance, Pulse, Grimace, Activity, Respiration) score, whether the baby was preterm (gestation less than 38 weeks), whether the baby was admitted to the neonatal intensive care unit (NICU), and whether there were complications of labor and delivery; WIC is an indicator for whether the mother used WIC during the pregnancy, and Z' includes the following time-varying characteristics of the mother: prepregnancy weight, whether she has chronic diabetes, whether she has chronic hypertension, whether she smoked at all during the pregnancy, whether she was employed, whether she was married. The other variables are defined as in equation (3.1) except that X_{it} includes one additional variable, which is whether the information about the father is missing from the birth certificate. This variable can be viewed as an indicator of father involvement.

Finally, we also estimate models that allow the effects of WIC to differ for first born children. There are two reasons why this exercise is of interest. One is that first born children may be more affected by WIC because their mothers do not know what to expect and so the guidance offered by WIC might be of particular value. Second, as we show below, mothers who have had a previous poor pregnancy outcome (such as an infant death) are more likely to use WIC in the current pregnancy. Failure to control for this negative selection into WIC could result in downward bias in the estimated effect on infant health; therefore, it is of

interest to compare the effects of WIC on firstborns with the effects of WIC for subsequent pregnancies. These models take the form:

$$Y_{it} = a_i + a_2 WIC_{it} + a_3 WIC_{it} \times Firstborn + a_4 Z'_{it} + a_5 X_{it} + a_6 Month + a_7 Year + a_8 Neighborhood + e_{it} \tag{3.3}$$

where *Firstborn* is an indicator equal to one if the child is the mother's first born, and all other variables are defined as described above. In this regression, a_2 is the estimated effect of WIC on all children, and a significant value for a_3 indicates that the effect is different for firstborns.

We conduct our analyses for all mothers, and for subsamples of mothers defined by Medicaid coverage, education less than 12 years, and by race. We examine these subsamples largely for the sake of comparability with previous research. Some previous analyses have found larger effects of WIC among African American mothers. As discussed above, several previous analyses have confined their attention to mothers with Medicaid coverage. Finally, one might think that the counseling aspect of the WIC program could be particularly valuable to less educated mothers, so we also examine this group separately.

3.4 Data

The data for this study come from the New York City Department of Health and Mental Hygiene's Vital Statistics Natality system for collecting and recording information from the certificate of live birth. Data for these certificates come from two worksheets. One is completed by the mother and asks information about her circumstances and behaviors (such as marital status, smoking during pregnancy, and prepregnancy weight). The other worksheet is completed by the medical facility where the birth takes place using medical records. This worksheet includes information about prenatal care visits, risk factors for the

Table 3.1: Variable Means

Sample	Full Sample	Mothers w>1 Child	Mothers w>1 Child, on WIC	Mothers w>1 Child, not on WIC
On WIC during pregnancy	0.42	0.423	1	0
Number of observations	[1,232,007]	[559,935]	[236,680]	[323,255]
Outcomes				
Low birth weight	0.068	0.061	0.068	0.055
Infant small for dates	0.099	0.093	0.102	0.086
Gestation < 37 weeks	0.08	0.074	0.079	0.07
Weight gain < 6 kg	0.068	0.072	0.085	0.062
Complications of labor	0.348	0.322	0.345	0.306
Infant to NICU	0.081	0.072	0.094	0.056
Mother and child characteristics				
Child female	0.488	0.49	0.491	0.49
Child's parity	1.994	2.275	2.357	2.216
Mother education	12.475	12.344	11.093	13.259
Mother age	27.923	27.492	25.802	28.729
Marital status	0.511	0.546	0.347	0.691
Father's information missing	0.212	0.19	0.266	0.135
Employed	0.342	0.32	0.153	0.444
Smoked during pregnancy	0.039	0.039	0.052	0.03
Prepregnancy weight (kg)	64.943	65.179	65.888	64.655
Chronic diabetes	0.003	0.003	0.004	0.002
Chronic hypertension	0.009	0.007	0.009	0.007
African American	0.278	0.259	0.323	0.211
Medicaid coverage	0.604	0.669	0.968	0.451
First trimester prenatal care	0.645	0.651	0.567	0.714
Previous poor outcomes[a]	0.056	0.109	0.137	0.091

Notes: [a]Applies only to 746,491 births for which we observe a previous birth in our sample.

pregnancy, complications of labor and delivery, and newborn health. We start with all live singleton births in New York City between 1994 and 2004, which yields approximately 1.2 million records.

Table 3.1 provides an overview of our data. In the full sample, 42% of the births were to mothers who used WIC at some point during pregnancy. Column (2) of Table 3.1 shows means for those children with a sibling in the sample, roughly 45% of the sample births. The remaining two columns focus on this subsample of sibling births, and divide them into births in which the mother used WIC and births in which she did not. Focusing on these last two columns, we can see that the incidence of negative outcomes is generally higher for

the WIC infants: they are more likely to be low birth weight, small for dates, to have low APGAR scores (a measure of the health of the infant at birth that is taken out of 10), and to be premature.[7] Strikingly, the WIC infants are almost twice as likely as the non-WIC infants to end up in the NICU.

Of course much of the gap between the WIC infants and the non-WIC infants is likely to be due to differing maternal characteristics. Table 3.1 indicates that mothers of the WIC infants were much less educated (a mean difference of more than 2 years), younger, half as likely to be married, twice as likely to be missing the father's information on the birth certificate (an indicator of paternal involvement), and more likely to smoke. They also have higher incidence of chronic diabetes and chronic hypertension, are less likely to receive prenatal care in the first trimester of pregnancy, and are almost entirely dependent on Medicaid coverage. The WIC mothers are a little heavier on average before the pregnancy but less likely to have gained an adequate amount of weight during pregnancy.[8] They are also more likely to have complications of labor and delivery. Finally WIC mothers who have already had one child are more likely to have had a poor outcome in a previous pregnancy. We define a poor outcome to include: A previous infant death (very rare), a prior child admitted to the NICU, a previous infant who was premature, or a previous low birth weight infant. These stark differences between the circumstances of WIC and non-WIC infants illustrate the importance of adequately controlling for the characteristics of WIC mothers when evaluating the effects of the program on outcomes.

3.5 Results

Table 3.2 examines the relationship between maternal characteristics and participation in the WIC program using mother fixed effects models. Therefore, these estimates rely on the

[7]Birth weights < 700 g and $> 7{,}000$ g have been recoded as missing. The dummy for gestation < 37 weeks has been recoded as missing for gestation < 20 weeks and > 45 weeks.

[8]Prepregnancy weight has missing values in the data and in addition values < 91 pounds have been coded as missing when this is used as an outcome.

Table 3.2: Mother Fixed Effects Estimates Describing Selection into the WIC Program (Coefficient, (SE), Percentage Effect) (All Coefficients and Standard Errors Multiplied by 100)

Dependent Variable	Married	Employed	Age	Prepregnancy Weight	Smoked	Chronic Diabetes	Chronic Hypertension	Prenatal Care First Trimester	Poor Previous Outcome[a]
All births	-2.454	-6.094	-0.535	-8.518	0.582	0.071	0.12	0.62	2.536
N =1,226,026	(0.127)	(0.165)	(0.109)	(3.732)	(0.078)	(0.023)	(0.037)	(0.235)	(0.223)
	-4.80%	-17.78%	-5.53%	-0.13%	15.08%	21.80%	13.51%	0.96%	13.64%
Gestation >37,	-2.551	-6.277	-0.455	-9.954	0.638	0.05	0.053	0.488	2.976
<42 weeks	(0.137)	(0.179)	(0.117)	(4.007)	(0.080)	(0.022)	(0.036)	(0.253)	(2.243)
N =1,119,928	-4.90%	-18.20%	-4.80%	-0.13%	18.03%	17.90%	7.23%	0.75%	17.66%
Medicaid coverage	-2.536	-6.370	-0.660	-8.845	0.584	0.06	0.106	0.852	1.884
N =739,990	(0.141)	(0.164)	(0.128)	(4.180)	(0.090)	(0.025)	(0.040)	(0.262)	(0.265)
	-7.05%	-37.96%	-4.80%	-0.14%	12.33%	17.12%	11.86%	1.52%	8.75%
Mother's	-1.566	-3.453	-0.077	-13.287	0.629	0.042	0.05	1.305	2.684
education<=12	(0.154)	(0.183)	(0.151)	(4.829)	(0.109)	(0.028)	(0.044)	(0.303)	(0.285)
N =732,852	-3.83%	-17.90%	-0.52%	-0.20%	12.59%	12.35%	5.83%	2.25%	12.63%
African American	-3.262	-7.712	-1.641	31.649	0.941	0.067	0.16	0.491	2.149
N =340,650	(0.240)	(0.304)	(0.216)	(7.906)	(0.181)	(0.047)	(0.091)	(0.450)	(0.457)
	-10.86%	-21.07%	-12.51%	0.45%	15.16%	9.53%	9.72%	0.84%	8.14%
Number nonmissing	1,225,999	1,166,803	1,225,837	1,092,221	1,217,216	1,226,026	1,226,026	1,096,100	163,639

Notes: Each entry shows the coefficient on WIC from a separate regression of equation (3.1). Standard errors in parentheses. All models controlled for the child's gender, maternal education (<12, 12, 13–15, 16+ years), child parity (1, 2, 3, 4+), maternal age (<20, 20–24, 25–29, 30–34, 35+), month of birth, year of birth, 193 neighborhoods, and indicators for missing values of these variables.

[a]This variable is defined only for women with a previous birth that is included in our data set.

sample of mothers who used WIC for one pregnancy but did not use it for another to tell us when they were relatively more likely to use WIC. The table shows estimates for all births, full-term births only, and for different groups of mothers defined by insurance status, race, and education.

The first three columns of Table 3.2 show that mothers tend to use WIC when they are not employed, when they are unmarried, and when they are younger. These differences exist for all the groups we examine, except that there is no difference in age among less educated mothers. All but African American mothers have lower prepregnancy weights when they go on WIC, whereas African American mothers, who are the highest mean weight group, are more likely to use WIC when they are relatively heavy. Together these results suggest a scenario in which mothers use WIC (and smoke) when they are relatively young, unemployed, and unmarried. As they grow older they may be more likely to quit smoking, become employed, and/or marry and they become less likely to use WIC.

It is surprising then that we also find that women are more likely to have chronic diabetes and chronic hypertension when they are on WIC because these are conditions that increase in prevalence with age. An examination of the subgroups shows that this relationship is strongest among African American women. We think that, consistent with Rossin-Slater's interpretation of her results from Texas, women on WIC may be more likely to be properly diagnosed and treated if they have chronic diabetes or hypertension, so that these conditions are more likely to be subsequently recorded on the birth certificate. Consistent with this interpretation, Table 3.2 also shows that women on WIC are more likely to get prenatal care in the first trimester.[9]

The last column of Table 3.2 shows that in the subset of women who have already had a birth that we can observe, women who had a previous poor pregnancy outcome are much more likely to be on WIC. This finding could reflect women being more motivated to try to

[9]An alternative possibility is that women on WIC are being unnecessarily diagnosed with these conditions and treated, but we think this is unlikely given the clear diagnostic criterion and lack of debate about appropriate treatment for high blood pressure and diabetes.

improve their pregnancy outcomes, or a pattern of referring women with poor prior outcomes to WIC.

Changes in these time-varying maternal characteristics are likely to bias estimated WIC effects if they are not controlled. The fact that women use WIC when they are relatively disadvantaged (young, unmarried, and unemployed) would be expected to bias estimated effects of WIC downward in the absence of controls for these variables. Similarly, if women who know that they are at risk of future poor outcomes are more likely to use WIC, then estimates that do not take this selection into account would understate the benefits of WIC. If women on WIC are really more likely to have chronic diabetes and/or hypertension, then this would also tend to be reflected in poorer birth outcomes. However, as discussed above, it is possible that this pattern reflects differential reporting of chronic conditions for women on WIC rather than an actual increased prevalence of these conditions.

Table 3.3 shows estimated effects of WIC on infant outcomes. The estimates in this table and in Table 3.4 are based on equation (3.2) and include observable time-varying characteristics associated with selection into the WIC program: maternal age, employment, marital status, smoking during pregnancy, prepregnancy weight,[10] chronic diabetes, chronic hypertension, and whether women received prenatal care in the first trimester. We have also estimated these models excluding the prenatal care variable, and find that this exclusion has little effect on the estimates.

As we only observe previous poor pregnancy outcomes for a subset of women in our data set, we have not controlled for that variable here. Below, we show estimates which allow the effect of WIC to vary for first born children and other children. These estimates will shed light on the potential importance of any information that mothers may have gained from previous pregnancies, because no mother has such information for first births.

[10]When used as a control, indicators for the following categories of prepregnancy weight have been included: missing, [90, 110) pounds, [110,120) pounds, [120,135) pounds, [135,159) pounds, [159,185) pounds, and $\geq$ 185 pounds.

Table 3.3: Mother Fixed Effects Estimates of the Effects of WIC Participation on Outcomes

	Low Birth Weight	**Gestation <37 Weeks**	**Small for Dates**	**Low Weight Gain**	**Complications of Labor**	**Any NICU**
All births, N =1,226,026						
Coeff × 100	-0.414	-0.800	-0.288	-0.625	2.115	1.143
(SE) × 100	(0.106)	(0.118)	(0.131)	(0.131)	(0.217)	(0.125)
Mean dep. var.	0.067	0.08	0.099	0.068	0.348	0.081
Implied % change	-6.12%	-10.05%	-2.92%	-9.18%	6.07%	14.17%
Medicaid covered mothers only, N =739,990						
Coeff × 100	-0.426	-0.862	-0.265	-0.676	2.285	1.222
(SE) × 100	(0.116)	(0.129)	(0.143)	(0.150)	(0.227)	(0.139)
Mean dep. var.	0.073	0.084	0.106	0.08	0.348	0.091
Implied % change	-5.81%	-10.21%	-2.50%	-8.47%	6.56%	13.38%
Mothers with ≤ 12 years of education only, N =732,852						
Coeff × 100	-0.451	-0.966	-0.187	-0.767	2.224	1.334
(SE) × 100	(0.135)	(0.150)	(0.168)	(0.176)	(0.264)	(0.159)
Mean dep. var.	0.073	0.084	0.106	0.08	0.341	0.089
Implied % change	-6.15%	-11.44%	—	-9.54%	6.53%	14.95%
African American mothers only, N =340,650						
Coeff × 100	-1.142	-1.437	-0.398	-0.875	0.386	0.856
(SE) × 100	(0.228)	(0.249)	(0.263)	(0.268)	(0.401)	(0.272)
Mean dep. var.	0.099	0.11	0.124	0.088	0.345	0.116
Implied % change	-11.50%	-13.03%	—	-9.90%	—	7.35%
# Nonmissing	1,223,284	1,216,781	1,212,655	1,077,828	1,226,026	1,112,908

Notes: Each coefficient is from a separate regression of equation (3.2). Standard errors in parentheses. In addition to the variables listed in the notes to Table 3.2, these regressions controlled for chronic diabetes, chronic hypertension, prepregnancy weight, maternal smoking, maternal employment, marital status, whether the mother obtained prenatal care in the first trimester, and whether the father was listed on the birth certificate. Implied % change in the dependent variable is shown if the coefficient estimate is significantly different than zero.

Table 3.4: Mother Fixed Effects Estimates of the Effects of WIC Participation on Outcomes (Gestation >37 Weeks Only)

	Low Birth Weight	**Small for Dates**	**Low Weight Gain**	**Complications of Labor**	**Any NICU**
All births, N = 1,119,928					
Coeff × 100	-0.160	-0.482	-0.511	2.147	1.268
(SE) × 100	(0.077)	(0.138)	(0.136)	(0.233)	(0.110)
Mean dep. var.	0.028	0.099	0.063	0.336	0.051
Implied % change	-5.63%	-4.88%	-8.17%	6.39%	24.80%
Medicaid covered mothers only, N = 672,213					
Coeff × 100	-0.155	-0.447	-0.590	2.324	1.327
(SE) × 100	(0.085)	(0.150)	(0.156)	(0.243)	(0.123)
Mean dep. var.	0.031	0.107	0.073	0.336	0.06
Implied % change	-4.97%	-4.18%	-8.03%	6.92%	22.10%
Mothers with <= 12 years of education only, N = 664,951					
Coeff × 100	-0.136	-0.412	-0.682	2.352	1.384
(SE) × 100	(0.100)	(0.177)	(0.183)	(0.283)	(0.141)
Mean dep. var.	0.031	0.107	0.074	0.328	0.058
Implied % change	—	-3.84%	-9.21%	7.18%	24.04%
African American mothers only, N = 300,423					
Coeff × 100	-0.376	-0.630	-0.798	0.528	1.425
(SE) × 100	(0.169)	(0.281)	(0.285)	(0.440)	(0.246)
Mean dep. var.	0.04	0.126	0.08	0.368	0.071
Implied % change	-9.37%	-5.02%	-9.95%	—	20.19%
Number Nonmissing	1,119,723	1,119,723	990,317	1,119,928	1,016,222

Notes: Each coefficient is from a separate regression of Equation (3.2). Standard errors in parentheses. In addition to the variables listed in the notes to Table 3.2, these regressions controlled for chronic diabetes, chronic hypertension, prepregnancy weight, maternal smoking, maternal employment, marital status, whether the mother obtained prenatal care in the first trimester, and whether the father was listed on the birth certificate. Implied % change in the dependent variable is shown if the coefficient estimate is significantly different than zero.

Column (1) in the first panel suggests that consistent with the vast majority of WIC studies, WIC participation is estimated to have a negative effect on the incidence of birth weight less than 2,500 g: The coefficient in the first row and column of .0041 can be compared with the fraction low birth weight which is also given in the table and thus implies a 6.12% reduction in the overall probability of low birth weight. However, it is possible that some of the estimated "effect" is driven by the mechanical correlation between gestation and the probability of enrollment in WIC (due to having a longer time period in which to enroll) that was discussed earlier. Column (2) suggests that WIC has a large effect on prematurity, reducing it by 10.05% which again, may reflect this mechanical correlation. Hence, column (3) shows estimates of whether the baby was "small for dates." Conditioning on gestation length in this way suggests that WIC reduces the overall probability of a small for dates baby by 2.92%. This smaller effect suggests that some of the estimated WIC effect in low birth weight and prematurity is indeed due to the mechanical relationships between gestation length, a longer window in which to enroll in WIC, and positive birth outcomes. However, the significant effect on whether the baby is small for dates indicates that WIC has a significant effect even conditional on gestation. The fourth column provides some evidence regarding potential mechanisms, suggesting that the probability of low weight gain during pregnancy (defined as weight gain less than 6 kg) is significantly reduced by participation in WIC, falling by 9.18%.

Columns (5) and (6) examine complications of labor and delivery, and the probability that the infant was admitted to the NICU, respectively. Surprisingly, the estimated effect of WIC is significantly positive in both cases, indicating that receiving WIC prenatally increases the probability of these outcomes. Because not even the harshest critics of WIC have suggested that it actually harms women and children, it appears likely that these estimates reflect improved access to medical care during and after the birth among WIC recipients rather than a causal effect of prenatal nutritional assistance or counseling.

The second panel of Table 3.3 shows estimates for mothers who were covered by Medicaid in both pregnancies. The estimated effects of WIC are almost identical to those in the full sample of mothers for all the outcomes examined. Hence, it appears that although Medicaid participants are adjunctively eligible for the WIC program, WIC participation is not simply a proxy for Medicaid coverage. Hence any improvements in access to medical care that are associated with WIC are not coming solely through the provision of health insurance, but through some other channel. The estimates for mothers with less than 12 years of education are very similar to those for Medicaid covered mothers.

Turning to the estimates by race, the effect on "small for dates," which is the most reliable measure of infant health in this table, is about one-third larger in terms of the point estimate for African Americans, but is not precisely estimated given the smaller subsample. The effect of WIC on the probability that the mother gained too little weight is also negative and about a third larger than for the sample as a whole. This estimate is statistically significant. Thus, the table provides some support for the hypothesis that the effects of WIC are larger for African American women than for other groups.

Aside from the regressions using "small for dates" as the dependent variable, the estimates in Table 3.3 are potentially colored by the mechanical correlation between gestation and WIC enrollment that has been discussed. Hence, Table 3.4 repeats the same analyses on the subset of infants who are full term, that is, born after 37 weeks gestation. Perhaps surprisingly, these estimates are similar to those in Table 3.3, and in some cases are larger. For example, in this sample WIC is estimated to reduce the probability that an infant is small for dates by 4.88%. Turning to the subsamples, the effect is significant for Medicaid mothers and less educated mothers. When we look by race, the impact of WIC on low birth weight is over twice as high for African Americans as for other mothers. The effects are also larger for African Americans in the models of low weight gain, small for dates, and NICU use.

Table 3.5 shows estimates from model (3.3), which allows the effect of WIC to be different for first born children than for children of higher birth order. Columns (2) through (5) suggest

Table 3.5: Mother Fixed Effects Estimates of the Effects of WIC Participation on Outcomes (Gestation >37 Weeks Only)

	Low Birth Weight	**Small for Dates**	**Low Weight Gain**	**Complications of Labor**	**Any NICU**
All births, N = 1,119,928					
WIC Coeff × 100	0.077	-0.475	-0.382	2.354	1.296
(SE) × 100	(0.078)	(0.147)	(0.146)	(0.250)	(0.104)
WIC Coeff × first birth × 100	-1.103	-0.171	-0.498	-0.886	-0.431
(SE) × 100	(0.111)	(0.209)	(0.204)	(0.357)	(0.148)
Mean dep. var.	0.028	0.099	0.063	0.336	0.051
Implied % change	—	-4.81%	-6.11%	7.00%	25.34%
% Change of first birth	-36.06%	—	-14.08%	4.37%	16.91%
Medicaid covered mothers only, N = 672,213					
WIC Coeff × 100	0.022	-0.573	-0.536	1.954	1.273
(SE) × 100	(0.089)	(0.165)	(0.173)	(0.270)	(0.121)
WIC Coeff × first birth × 100	-0.908	0.325	-0.210	1.33	-0.187
(SE) × 100	(0.152)	(0.281)	(0.294)	(0.460)	(0.206)
Mean dep. var.	0.031	0.107	0.073	0.336	0.06
Implied % change	—	-5.36%	-7.30%	5.82%	21.20%
% Change of first birth	-29.03%	—	—	9.79%	—
Mothers with <=12 years of education only, N = 664,951					
WIC Coeff × 100	0.063	-0.454	-0.611	2.171	1.244
(SE) × 100	(0.102)	(0.189)	(0.198)	(0.306)	(0.137)
WIC Coeff × first birth × 100	-1.100	0.037	-0.288	0.611	0.075
(SE) × 100	(0.163)	(0.301)	(0.310)	(0.486)	(0.217)
Mean dep. var.	0.031	0.107	0.074	0.328	0.058
Implied % change	—	-4.23%	-8.26%	6.63%	21.61%
% Change of first birth	-33.14%	—	—	—	—
African American mothers only, N = 300,423					
WIC Coeff × 100	-0.129	-0.679	-0.597	0.866	1.716
(SE) × 100	(0.176)	(0.305)	(0.312)	(0.482)	(0.240)
WIC Coeff × first birth × 100	-0.971	0.217	-0.814	-1.352	-1.087
(SE) × 100	(0.290)	(0.504)	(0.506)	(0.796)	(0.395)
Mean dep. var.	0.04	0.126	0.08	0.368	0.071
Implied % change	—	-5.41%	-7.44%	—	24.31%
% Change of first birth	-27.39%	—	—	—	8.91%
Number nonmissing	1,119,723	1,119,723	990,317	1,119,928	1,016,222

Notes: Each set of coefficients is from a separate regression of equation (3.3). Standard errors in parentheses. In addition to the variables listed in the notes to Table 3.2, these models controlled for chronic diabetes, chronic hypertension, prepregnancy weight, maternal smoking, maternal employment, marital status, whether the mother obtained prenatal care in the first trimester, and whether the father is listed on the birth certificate. Implied % change in the dependent variable is shown if the coefficient estimate is significantly different than zero. % Change for first births shown if significantly different than all births.

that the estimates are, for the most part, quite similar to Table 3.4. There is no significant difference in the effect of WIC on being small for dates between first born and other children: Both groups of children are 4.81% less likely to be small for dates compared with siblings who did not receive WIC in utero. The effects of WIC on low weight gain are somewhat larger for firstborns, but statistically significant for all children.

Moreover, there appear to be significant effects of WIC on complications of labor and delivery and the probability of NICU use for most children, including first born children. The sole exception is among African American mothers. In this group, WIC has no significant effect on complications of labor and delivery, and has a much weaker effect on NICU use among first borns than among other children. Recall, that we presented some evidence earlier suggesting that women with previous poor pregnancy outcomes are referred to WIC. Because mothers of first born children have not had previous poor pregnancy outcomes, the effect of WIC on the use of these medical services cannot be driven by such referrals in this group. Stronger effects of WIC on the utilization of medical care among later born African American children are consistent with such a pattern of referrals, however.

One result that is quite different for firstborns than for other children appears in column (1). Only first born children have significantly lower probabilities of being low birth weight when they received WIC. Moreover, WIC is estimated to reduce the probability of low birth weight in this group by a third.

3.6 Discussion and Conclusions

This article presents the first large-sample mother fixed effects estimates of the effects of WIC on infant outcomes and use of health care. Because fixed effects estimates only control for fixed characteristics of mothers, we also pay careful attention to time-varying factors associated with within-mother changes in prenatal WIC participation. The portrait that emerges is one in which young, unmarried, unemployed women use WIC initially, but are less

likely to use it for subsequent pregnancies in which they are older and married or employed. These results also show that investigation of the determinants of WIC participation is useful when interpreting its estimated effects.

The evidence suggests that women receive more intensive medical care when they are on WIC, at least in New York City. Women are more likely to be diagnosed with chronic diabetes and hypertension when they are on WIC despite being younger; they are more likely to be diagnosed with complications of labor and delivery; and their infants are more likely than non-WIC siblings to spend any time in the NICU. These results contrast with earlier estimates (e.g., Bitler and Currie (2005)) which suggested that WIC resulted in immediate cost savings due to reductions in use of medical care among women and newborns. Moreover, even firstborns are more likely to be diagnosed with complications of labor and delivery and to use the NICU if their mothers were on WIC during pregnancy, which suggests that the overall effect cannot just be an artifact of women with poor previous pregnancy outcomes being referred to WIC, though it is possible that the pattern among African Americans reflects such a referral pattern.

We also find, consistent with most previous WIC studies, that WIC participation has positive effects on infant health: WIC infants are 5.6% less likely to be low birth weight and 4.9% less likely to be small for dates (even when we only look at full-term infants). However, consistent with the critiques of Joyce et al. (2005), Joyce et al. (2008), and others, these estimates are quite a bit smaller than many that are in the literature.[11] An exception is among firstborns, where WIC reduces the probability of low birth weight by a third. Thus, the results suggest that WIC promotes weight gain and reduces the probability that a child is "small for dates" among most women, but has especially strong effects on women delivering for the first time.

[11] For example, Bitler and Currie (2005) find a 29% reduction in the incidence of low birth weight. It may be that the effects of WIC are quite different in different populations, depending in part on their underlying nutritional status and access to medical care.

One possible caveat to this study is that the estimated effects may actually be too small. Measurement error that was independent of WIC status, which is self-reported by the mother, could bias the estimated coefficients toward zero. Also, it is not clear that one should expect the estimated effects of WIC to be the same in every time and place as they must depend on the average health and nutritional status of women in the program, the composition of program benefits, and the medical and nutritional benefits available outside the WIC program.

In summary, we find that infants whose mothers used WIC during pregnancy are healthier than siblings born when the mothers were not using WIC. Some of the effect appears to come through a reduced probability of low weight gain during pregnancy, which is reflected in heavier babies and a lower incidence of "small for dates" babies. WIC mothers in our sample were also heavier users of medical care, and were more likely to be diagnosed with chronic conditions (probably reflecting this more intensive use of care). We believe that mother fixed effects techniques applied to richer administrative data (such as future mergers of educational records with birth records) could yield even greater insights into the long-term benefits of WIC.

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