

Essays in Applied Economics

Kunyu Wang

Thesis submitted to the Faculty of Graduate and Postdoctoral Studies
in partial fulfilment of the requirements for the degree:
Doctorate in Philosophy Economics

Department of Economics
University of Ottawa
Ottawa, Ontario, Canada

© Kunyu Wang, Ottawa, Canada, 2018

Abstract

Chapter 1 Does the party of government influence the amount and type of inward foreign investment? The results of a number of correlational studies provide inconsistent evidence. However none of these studies - for any level of government or any jurisdiction - have used methods that allow them to speak to causal effects. Regression discontinuity (RD) method is applied to a set of narrow-margin US gubernatorial elections. Over the course of a four-year term the election of a Republican governor causes a 21% boost in the growth of manufacturing-oriented FDI stock, compared to a Democrat. This effect is robust to a series of challenges. However, the same approach provides no evidence that partisanship matters for the overall level of FDI.

Chapter 2 Does an economic shock open a window of opportunity for reform, and if it does, how does the institution of a state play a role? The paper investigates how economic shocks affect the structural reforms in various institutions. This paper addresses this issue by using the exogenous variation in the international price of large commodity goods to generate the exogenous change in national income. The analysis relies on a unique mapping between new annual data from 1962 to 2005

on economic shocks from commodity prices and structural reforms in 111 countries. I find significant heterogeneous effects across sectors in autocratic countries. In autocracies, positive economic shocks promote reforms in real sectors, but deter reforms in financial sectors. However the impact of economic shocks on structural reform in democratic countries is nil.

Chapter 3 The deregulation of branch banking across the United States substantially increased the availability of credit to existing borrowers and others who has previously been excluded. Exploiting the staggered timing of changes across states for identification it is estimated that deregulation caused a 3.3% increase in rates of suicide and a 4.7% increase in rates of divorce. This is consistent with a large body of evidence linking excess debt to various measures of individual and relationship distress. Results are in most cases statistically significant at levels much higher than 1%, and prove resilient in a battery of robustness checks and falsification exercises.

Acknowledgements

I am grateful to my thesis supervisor, Anthony Heyes for his guidance, encouragement and advice. I would like to thank my committee members, Zhihao Yu, Roland Pongou Jason Garred and Anindya Sen, for their reviews and suggestions. I acknowledge valuable comments by Abel Brodeur, Paul Makdissi, Aggey Semenov, Louis-Philippe Morin and Pierre Brochu. I would also like to thank Christopher Ksoll, Zhiqi Chen, Rose Anne Devlin and Victoria Barham for supporting me during my study. The research reported in chapters 1 and 3 form the basis for coauthored papers with Anthony Heyes.

Table of Contents

Abstract	ii
Acknowledgements	iv
General Introduction	1
1 Subnational Politics and Foreign Direct Investment (FDI): First Causal Evidence	5
1.1 Introduction	5
1.2 Research Design	10
1.2.1 Methods	10
1.2.2 Study Setting	15
1.2.3 Data	16
1.3 Results	19
1.3.1 Main results	19
1.3.2 Validity	21
1.3.3 Methodological robustness	23
1.4 Possible Channels	26
1.5 Conclusion	27
2 Commodity Price Shocks, Institutions and Windows of Opportunity for Structural Reform	44
2.1 Introduction	44
2.2 Literature Review	47

2.3	Data	50
2.3.1	Data on reforms	50
2.3.2	Data on commodity prices	52
2.3.3	Data on institutions	53
2.4	Research Design	54
2.4.1	Empirical Strategy	54
2.4.2	Model	55
2.5	Results	57
2.5.1	Main Results	57
2.5.2	Robustness Checks	60
2.6	Conclusion	62
3	The Social Footprint of Bank Regulation: Natural Experimental Evidence from the US	72
3.1	Introduction	72
3.2	Literature Review	76
3.3	Setting	79
3.4	Data	80
3.5	Empirical Strategy	81
3.6	Results	83
3.6.1	Main Results: Suicide	83
3.6.2	Main Results: Divorce	84
3.6.3	Robustness	84
3.6.4	Falsification: Evidence from 6000 Placebos	88
3.7	Conclusion	91
3.8	Appendix	104
	Bibliography	109

List of Tables

1.1	Summary Statistics	29
1.2	Statistics on Gubernatorial Elections	30
1.3	RD Estimates for FDI Per Capita	31
1.4	RD Estimates for FDI Manufacturing Per Capita	32
1.5	Covariate Balance Tests for Gubernatorial RD Design	33
1.6	RD Estimates for FDI Manufacturing Per Capita: Senate/House/President	34
1.7	RD Estimates for FDI Manufacturing Per Capita: 5% & 10% Bandwidth	35
1.8	RD Estimates for FDI Manufacturing Per Capita: Second Order Poly- nomial	36
1.9	RD Estimates for FDI Manufacturing Per Capita: 99% & 95% Win- sorizing	37
1.10	RD Estimates for FDI Manufacturing as Percent of GSP	38
2.1	Summary Statistic	65
2.2	Estimation for Financial Market Reform	66
2.3	Estimation for Product Market Reform	67
2.4	Robustness Check for Financial Market Reform: Exclude Big Pro- ducer & Big Regulation Indices & Control for Shares	68
2.5	Robustness Check for Product Market Reform: Exclude Big Producer & Big Regulation Indices & Control for Shares	69
2.6	Robustness Check for Financial Market Reform: Export Share . . .	70
2.7	Robustness Check for Product Market Reform: Export Share	71

3.1	Year Intrastate Bank Branching First Allowed	93
3.2	Summary Statistics	94
3.3	Main Result: Suicide	95
3.4	Main Result: Divorce	96
3.5	Robustness Check: Suicide	97
3.6	Robustness Check: Divorce	98
3.7	Exclude Large States - Suicide	104
3.8	Exclude Large States - Divorce	105
3.9	Inclusion of Lagged Unemployment Rate - Suicide	106
3.10	Inclusion of Lagged Unemployment Rate - Divorce	107

List of Figures

1.1	Histogram of victory margin	39
1.2	RD estimates on FDI, 1 to 4 years after election	40
1.3	RD estimates on FDI Manufacturing, 1 to 4 years after election . . .	41
1.4	The effect of electing a Republican governor on change in FDI in Manufacturing	42
1.5	McCrary density of victory margin	43
3.1	Timing of intrastate branching deregulation	99
3.2	Timing of bank deregulation and pre-existing suicide rate	100
3.3	Timing of bank deregulation and pre-existing divorce rate	101
3.4	Suicide placebo tests	102
3.5	Divorce placebo tests	103
3.6	Timing of bank deregulation and unilateral divorce law	108

General Introduction

My thesis lies in applied economic analysis on the partisanship effects on foreign direct investment, commodity price shocks and window of opportunity for structural reforms, and social costs of bank de-regulation laws. Given the varied nature of these topics, I apply a diverse set of tools drawn from applied econometrics by exploiting natural experiments. Establishing causal inference is one of the most pressing challenges in modern applied economics as oftentimes, observational associations between variables of interest are inadequate in unraveling causal pathways. I investigate these issues by including institutional and geographic context and exploiting political and economic shocks, ultimately isolating causal concepts in a clear manner.

My first chapter has contributed the first empirical investigation of a causal link from political partisanship to foreign direct investment in the United States. While there are many correlational studies relating the political parties to the attraction of foreign direct investment, none of these studies established a causal effects. This is the gap that the first chapter seeks to fill. The partisan differences of economic policy in the US are pronounced. Democratic administrations seek to promote growth through a consumption-driven approach, while Republican administrations tend to

adopt an investment-driven growth strategy. The divergent growth strategies tend to create different business environments for foreign direct investors.

To obtain the plausibly exogenous variation in political party in power, the regression discontinuity design is adopted, exploiting the discontinuity generated by the first-past-the-post election system. The identifying assumption that underpins the application of RDD to close elections is that when one party or the other wins by a sufficiently narrow margin, then the partisanship of the victory can be regarded as being random. The evidence points to Republican governors causing a substantial and sustained upward bump in foreign investment into manufacturing activities. Over the course of a four-year term the election of a Republican governor causes a 21% boost in the growth of manufacturing-oriented FDI stock, compared to a Democrat. This paper articulates and defends the essential role of partisanship in attracting foreign direct investment and also sheds light on the evaluation on the economic outcomes of ideological growth strategies.

Finding a window of opportunity to conduct economic reform is one of the fundamental aims in political economy. Economic reform is difficult to implement, even when we considered those that are efficiency-enhancing. Unequal distributions between benefits and costs exist both economically and politically, underlining the non-neutrality feature of reform. Often-times, we witness reforms that are postponed or adopted after long delays, because reforms are rarely being Pareto improvement, resulting in winners and losers. Does an economic shock open a window of opportunity for reform, and if it does, how does the institution of a state play a role? The second chapter investigates how economic shocks affect the structural reforms

in various institutions.

Previous research is limited to case studies, the use of pre-existing data sources that cover a relatively narrow set of reforms and countries. An expanded dataset on structural reform that includes many countries across broader time-frames is investigated in this chapter. Additionally, the economic literature ignores the fact that national income is endogenous to economic reform, and overwhelmingly fails to find a causal effect between economic conditions and economic reform. This chapter addresses this issue by using the exogenous variation in the international price of large commodity goods to generate the exogenous change in national income. The analysis relies on a unique mapping between new annual data from 1962 to 2005 on economic shocks from commodity prices and structural reforms in 111 countries. I use exogenous shocks from principal commodity world prices to identify the plausible causal effects from economic shocks on structural reform. I find significant heterogeneous effects across sectors in autocratic countries. In autocracies, positive economic shocks promote reforms in real sectors, but deter reforms in financial sectors. However the impact of economic shocks on structural reform in democratic countries is nil.

In the last chapter, the social impact of financial development is investigated. In particular, this chapter estimates the social cost in possible suicide and divorce from banking deregulation in the U.S. Banking industry used to be a highly regulated industry in US. Throughout the last three decades, the restrictions on banks' abilities to conduct business in different geographic areas have almost vanished. In the 1980s and 1990s, most states removed geographic restrictions on bank branching. Over the last few years, deregulation of geographic bank branching restrictions has become a

widely analyzed policy change.

The economic impact of financial development had been well documented in a large number of literature in economics and finance, while little looked at the social impact of the financial development. The cross-state, cross-time variation in bank deregulation across the US states is used to assess how improvements in banking systems have affected two indicators of social distress, namely, suicide and divorce rates. The results from a difference-in-differences approach show that bank deregulation leads to 3.2% increase in the state level suicide rate and 4.7% increase in the state level divorce rate. The results derived from this chapter highlights the importance the social cost of financial development.

Chapter 1

Subnational Politics and Foreign Direct Investment (FDI): First Causal Evidence

1.1 Introduction

Political leaders - local and national - are not reticent in taking credit for investment flowing into their jurisdictions. This is not surprising given that foreign direct investment (FDI) contributes significantly to the vitality of most modern economies. For example, in the United States a 2016 report by the International Trade Administration estimated that (a) 12 million jobs were directly attributable to FDI (employing over 10% of the workforce), a number that is growing and, (b) FDI also has substantial positive spillover effects on the productivity and innovation of domestic firms

([Office of Trade and Economic Analysis \(2016\)](#)). The ability to attract and retain outside investment is often an important part of the prospectus of candidates from both major parties in US gubernatorial elections.

While there are many some correlational studies relating the political ‘type’ of those holding office to the vigor of foreign direct investment (FDI) into states, countries and cities, there is no empirical evidence (from the US or anywhere else) of any *causal* link. This is the gap that we seek to fill in this paper. More concretely our research question is: Are Democrat or Republican state Governors better at attracting FDI?

Anecdotal evidence points to Republican superiority. For example, all five of the 2017 *Golden Shovel Awards* - given annually by the Area Development magazine to acknowledge outstanding performance in this sphere - went to states with Republican governors (Illinois, Georgia, Arizona, Kentucky and Mississippi).¹

However, empirical evidence on the matter is mixed. Using a pooled cross-section data, [Halvorsen and Jakobsen \(2013\)](#) find that on average FDI is higher in Republican governed states. That effect is not statistically significant, however, leading them to conclude that “... foreign direct investors seem relatively agnostic with respect to the question of which party controls the state government” (page 182). [McMillan \(2009\)](#) finds a significant and positive relationship between Democratic governorship and FDI. Using a slightly different measure [Fox \(1996\)](#) also finds a positive and significant association between Democratic governorship and the foreign firm location

¹<http://www.areadevelopment.com/Gold-Shovel-Econdev-Awards/Q2-2017/states-compiling-rosters-of-new-expanded-facilities.shtml>

decisions.² With a focus on national governments [Pinto and Pinto \(2008\)](#) find that right-leaning governments attract more total FDI but that incumbent government partisanship also correlates with the *type* of investment. In particular jurisdictions with left-leaning (right-leaning) governments experience greater inflow into sectors where investment can be expected to complement (substitute) labor in production.³

The central problem with existing studies such as these is that they point only to correlations and are unable to speak to issues of causation. This is a big limitation. For example, states that have unobserved characteristics that predispose them to elect Republican governors also have characteristics (perhaps the same characteristics, perhaps others) that make them attractive targets for inward investment. At the same time there may be causal effects running in the opposite direction. A state that is successful in attracting much foreign investment might (for whatever reason) develop economically or socially in a way that makes it more likely to vote for a particular type of leader.

²[Fox \(1996\)](#) provides a detailed discussion of why the effect of party could go either way, and in setting up her hypotheses is non-committal on expected sign. “The party of the governor is a dummy variable equal to 1 for Democratic governors and 0 for Republican governors. A priori it is difficult to determine what the expected relationship between the party of the governor and firm location decisions should be. On the one hand, by setting the state’s economic development priorities, Democratic governors may be more likely to take interventionist approaches to the economy by actively pursuing economic development policies that result in favourable business climates. This suggests a positive relationship between Democratic governors and firm location decisions. However, a negative relationship will result if firms are attracted to states with Republican governors, where Republican governors can affect business climates by being more ‘pro-business’ ([Hansen \(1989\)](#)). Given the viability of each of these options no specific relationship is hypothesized up front” ([Fox \(1996\)](#)).

³There is a much larger literature on how politics affects wider economic outcomes. At the Federal level, for example, [Blinder and Watson \(2016\)](#) report a positive association between US economic performance (especially in terms of GDP growth and productivity improvements) and President of the United States has been Democrat rather than Republican. Our ambitions in this paper are much more focused.

We apply a regression discontinuity design (RDD) to a set of narrow margin gubernatorial elections between 1977 and 2004 to generate the first evidence of a causal link from the party affiliation of the governor of a state to the level and pattern of FDI that flows into that state.⁴

The perennial challenge in causal inference is ensuring random or quasi-random assignment of treatment, in this case the party affiliation of the Governor. A perfect experimental design would involve randomly assigning a Democratic or Republican governorship to a large sample of states by toss of a coin, then observing the subsequent pattern of FDI across the two groups. Any difference in patterns could then be causally attributed to the outcome of the toss. Of course, such a randomized controlled trial would not be feasible in our setting. The identifying assumption that underpins the application of RDD to close elections is that when one party or the other wins by a sufficiently narrow margin then the partisanship of the victory can be regarded as being (close to) random (Lee (2008), Eggers et al. (2015)).

Methods will be detailed below. It is well-known that the RDD researcher faces a number of choices in conducting his analysis (Lee and Lemieux (2010)), and recent evidence is that those choices may sometimes be made - perhaps inadvertently - in way that ensures the statistical significance of results exceeds some critical thresholds (Brodeur et al. (2016)). To avoid the risk of such manipulation - and to make defensible a claim that we are adhering to best practice in the conduct of the study - our central estimates are based on the widely-accepted methods summarized in Calonico

⁴RDD methods were first applied by Thistlethwaite and Campbell (1960). A popular survey of methods and applications is by Lee and Lemieux (2010). Lee (2008) and Angrist and Pischke (2008) also provide good overviews.

[et al. \(2014\)](#). In particular the bandwidth underpinning our central estimates are optimally chosen to minimize the asymptotic mean square error of the RD estimator, rather than selected arbitrarily, and robust nonparametric confidence intervals are reported.

In brief our results are as follows. The election of a Republican as governor has a statistically significant positive effect on net FDI inflows in manufacturing industries to a state. The effect is substantial - 21% in aggregate dollars in our preferred specification - and is sustained throughout the term of office. Interestingly it makes no significant difference to the total value of FDI inflow. The results are relatively undisturbed by inclusion or exclusion of a range of controls, and prove robust to a variety of robustness tests.

We will point to various strands of related literature ([de la Cuesta and Imai \(2016\)](#), [Eggers et al. \(2015\)](#), [Grimmer et al. \(2011\)](#)). However we deliberately exclude claims of the channels through which party-affiliation might matter. Indeed one attraction of the RDD method is that it allows the researcher to remain agnostic as to the mechanism or mechanisms at play. The powers of a Governor are manifold, and the partisanship of the officeholder might influence investment flows through various channels. Some of these might be direct, such as inducements and trade visits. Others indirect. Foreign investors may be attracted to places with low taxes, flexible regulators, promise a responsive workforce or offer cohesive social settings ([Head et al. \(1999\)](#)). There are often-claimed partisan differences in approach to economic policy which might influence the type of investor to which a state appeals. According to [Quinn and Shapiro \(1991\)](#) Democratic administrations typically seek to promote

growth via consumption-driven approaches, while Republican administrations favor supply-side strategies. Democratic administrations also seek to shift the tax burden toward corporations and owners of capital. In addition to manipulation of state-level policy, [Innes and Mitra \(2015\)](#) find that Republican (compared to Democratic) representation in the US House of Representatives reduces EPA inspection rates in a state, suggesting partisan politicians are able to influence not just the evolution of policy but also its implementation by Federal Agencies.⁵ An appeal of RDD is that it will absorb all of these effects and delivers a ‘net’ or aggregate measure.

The rest of the paper is organized as follows. Section 2 describes methods and data. Section 3 reports results. Section 4 concludes.

1.2 Research Design

In this section we outline our methods and data sources, including discussion of assumptions and potential challenges to compelling identification.

1.2.1 Methods

The regression discontinuity method exploits artificial thresholds that divide entities arrayed along some inherently continuous dimension into discrete groups.

In a two-candidate election the majority-wins rule provides a sharp threshold. If a candidate gathers 49.99% of admissible vote he loses, if 50.01% of votes he wins. For a margin of victory sufficiently close the assignment of winner in such an election

⁵Our empirical strategy is close to [Innes and Mitra \(2015\)](#) who exploit close-call congressional elections.

can, under plausible assumptions, be regarded as being as-good-as-random. This provides a natural-experimental basis for identifying of the causal effects of election outcomes on other variables of interest, avoiding the problems of methods reliant on selection on observables (Grimmer et al. (2011)). In a nutshell the approach involves looking at outcomes of interest after elections in which a particular party narrowly won, with those after elections in which they narrowly lost.

Early applications of RDD to close election datasets demonstrated the possibility of incumbency advantage (Lee (2008)), policy responsiveness (Lee et al. (2004)) and the rents from holding office (Eggers and Hainmueller (2009)). Several authors have used the method to explore the effects of partisanship on other economic outcomes. For example, Ferreira et al. (2009), Gerber and Hopkins (2011) and de Benedictis-Kessner and Warshaw (2016) present corresponding empirical evidence on close US mayor election and the size of government. Innes and Mitra (2015) looked at election and regulatory outcomes; Beland (2015) at labour market outcomes; and Leigh (2008) at numerous policy settings, including minimum wage, post-tax inequality, and unemployment rate.⁶

Our preferred results rely on the non-parametric local polynomial estimation methods due to Calonico et al. (2014). Helpfully, the same authors at the same time developed and published code that allows for execution of their methods in STATA (Calonico et al. (2014)) and R (Calonico et al. (2015)).

The method fits a weighted polynomial function to observations above and below the discontinuity within a particular bandwidth. The polynomial function usually

⁶There are non-RDD papers looking at effect of political parties on economic outcomes, for instance, Snowberg et al. (2007), List and Sturm (2006), Reed (2006), Besley and Case (1995).

takes order 1 or 2, and the weights are determined endogenously, not by researcher discretion, by a kernel function that performs the computation based on the distance of observations from the discontinuity. Within the bandwidth, the closer the observation is to the discontinuity the more heavily it is weighted. An election with a winning margin of 0.03% is given greater weight than an election won by 3%. This implementation does not impose a parametric form of regression functions (Skovron and Titunik (2015)). Excluding the observations far away from the cut-off prevent the distortion of the approximation near the cut-off (Gelman and Imbens (2014)).

The steps in estimation are as follows.

First, a bandwidth h is selected. The bandwidth is the width of the interval around the discontinuity within which the local polynomial is fitted. Typically this choice has been made arbitrarily, and for election-based studies has been set at 5% or 10% winning margins (de la Cuesta and Imai (2016), Erikson et al. (2015), Beland (2015)). In choosing bandwidth the researcher faces a trade-off between bias and variance.⁷ We follow the procedure developed in Calonico et al. (2014) for choosing the optimal bandwidth - that which minimizes the asymptotic mean squared error (MSE) of the regression discontinuity estimator, where MSE is the sum of the bias squared and variance of the estimator. The choice of optimal bandwidth also means that we avoid *ad hoc* decisions and the risk of (conscious or inadvertent!) specification searching.

Second, a kernel function $K(\cdot)$ is chosen. The function assigns non-negative

⁷As observations fall further from the discontinuity - margin of victory is greater - the as-good-as-random assignment assumption becomes less palatable, introducing the risk of bias. A broader bandwidth, however, means more data points.

weights to each observation x_i around the cut-off c and within the bandwidth. The commonly used kernel function is the triangular kernel function which applies more weight to observations closer to the cut-off. Using the kernel function we compute a weight w_i for each observation where

$$w_i = K((x_i - c)/h).$$

Third, weighted least squares regression is run separately on the set of observations that are above the cut-off but within the bandwidth and those below the cut-off but within the bandwidth on the choice of the polynomial. The order of the polynomial should be kept low and high order of the polynomials tends to lead to approximation error due to the overfitting and biases at the boundary points ([Skovron and Titiunik \(2015\)](#)).

Finally, we take the difference of the two estimated intercepts and get the regression discontinuity estimate. In effect the size of the ‘jump’ in the outcome variable that occurs at the discontinuity. Once we get the point estimate, we are interested in constructing the confidence interval and testing the hypothesis. Under the MSE optimal local polynomial estimation, the conventional inference method has been shown to be invalid ([Skovron and Titiunik \(2015\)](#)), so we adopt the robust confidence intervals proposed in [Calonico et al. \(2014\)](#).

To implement the local linear version we fit weighted linear regression functions to the observations within a bandwidth h on either side of the cut-off point c . In other words,

$$\underset{\alpha_1, \beta_1}{\text{minimize}} \quad \sum_{i: c-h < x_i < c} (y_i - \alpha_1 - \beta_1(x_i - c))^2 w_i \quad (1)$$

and

$$\underset{\alpha_2, \beta_2}{\text{minimize}} \quad \sum_{i: c < x_i < c+h} (y_i - \alpha_2 - \beta_2(x_i - c))^2 w_i \quad (2)$$

Given the estimates of $\hat{\alpha}_1$ and $\hat{\alpha}_2$, the estimated average treatment effect is

$$\hat{\tau}_{rd} = \hat{\alpha}_2 - \hat{\alpha}_1 \quad (3)$$

In this paper, we estimate the local treatment effect, following the methodology proposed by [Calonico et al. \(2014\)](#).⁸ The local linear regression offers flexibility with little loss of statistical power. Given the goal to estimate the expected values of potential outcomes at the discontinuity threshold, another particular advantages of the local linear regression estimators is attributed to better theoretical properties at the boundary when compared to other popular approaches ([Fan and Gijbels \(1996\)](#), [de la Cuesta and Imai \(2016\)](#)).

In summary we have adopted what we believe to be current best practice in RDD to model the relationship between assignment and outcome variable, using local linear estimation with an optimal MSE (mean-square-error), bandwidth and robust confidence intervals.

⁸[Imbens and Kalyanaraman \(2011\)](#) derive the bandwidth selection procedure that minimizes the approximate mean squared error at the threshold. [Calonico et al. \(2014\)](#) improve [Imbens and Kalyanaraman \(2011\)](#) and construct a bias-corrected estimator with robust confidence intervals.

1.2.2 Study Setting

States are the primary subdivisions of the US and have a high degree of autonomy in how they govern themselves. Each state possesses a number of important powers including the operation of local government, regulating business, levying taxes and spending tax revenues. The head of a state is called a Governor. He or she controls the governmental budget, appoints many officials, and has a plethora of other powers. Governors can veto bills and, in many cases, have the power of the line-item veto on appropriation bills. In addition to hard authority, the governor can also bring to bear significant ‘soft’ power through the authority given to him by his office. In summary, governors are influential players in US politics.

A governor may run his or her state in a way that makes it more or less attractive to a prospective international investor. In addition, in recent decades they have been increasingly visible players on the international scene. External state-promotional activity dates back to the 1970s ([Fry \(1998\)](#), [Watson \(1995\)](#)) and has focussed on promoting trade and attracting inward investment. As regards FDI in particular, states take the lead role in recruitment of inward investment, with the role of the federal government much smaller. Many state-operated international offices and governor-led overseas missions are set up to attract FDI ([McMillan \(2009\)](#)). US state officials claim that the international trade and investment is the largest category of state international engagement ([Whatley \(2003\)](#)). As a result, the governor of a state acts as the chief economic ‘ambassador’ in appealing to prospective investors.

1.2.3 Data

We obtain data from several sources.

Our outcome variable of interest is FDI. State level FDI data is drawn from US Bureau of Economic Analysis. We first obtain the total monetary amount of FDI stock and the FDI stock in manufacturing sectors in each US State for each year from 1977 to 2004. The FDI stock refers to the real book value of gross property, plant, and equipment (PPE) of all nonbank affiliates. This includes the value of buildings, structures, machinery, and equipment, etc., but excludes inventories and intangible assets. It corresponds to the standard definition of FDI in the US. We are interested in FDI flows, so we take the difference between the FDI measures in the year governor was elected (elections almost always occur in November) and each of the four years following where governor took the office respectively. So for example, if the a governor wins a close election in November 2005 we take the difference between FDI stock in 2005 with that stock in 2006, 2007, 2008 and 2009. This allows us to answer four slightly different questions: How much ‘extra’ FDI does a governor of a particular political persuasion attract in his first year in office, first two years in office, etc. In most cases the fourth variant can be taken to approximate the extra FDI across the whole term in office. In addition, taking changes in the outcome variable (rather than working with levels) serves to increase the statistical efficiency of our regression discontinuity design ([Lee and Lemieux \(2010\)](#)).

Table 1.1 presents summary statistics for FDI and other covariates. The mean FDI stock per capita across the whole of the US is 2796 USD and the mean FDI

stock per capita in the manufacturing sector 921 USD.

The data on gubernatorial elections are obtained from two sources. The election data from 1977 to 1990 is drawn from dataset *Candidate and Constituency Statistics of Elections in the United States, 1788-1990* (ICPSR 7757) from the Inter-university Consortium for Political and Social Research. The remaining election data comes from Dave Leip's Atlas of U.S. Presidential Elections ([Leip \(2008\)](#)).

Gubernatorial elections usually take place in November and the governor elected takes power in the following January. A governor's term usually is four years.⁹ We define the election margin to be the percentage of votes obtained by the Republican candidate minus the percentage obtained by the Democrat. Following that convention the discontinuity is at zero (we can ignore third candidates). If the election margin is positive (negative), the Republican (Democrat) has won.

Table 1.2 summarizes outcomes in the 361 elections in our sample. Of those Republicans won 186, Democrats won 175 times. In terms of close elections we count for the purpose of this summary table those with winning margins of 5% and 10%. Within a 5% interval around the cut-off, we get 81 elections of which Republicans won 42 times, Democrats won 39 times. Of the 157 elections within 10% of the cut-off Republicans won in 80, Democrats in 77. So by each of these metrics the sample is roughly balanced, no party seems systematically more likely to prevail when result margins are narrow. If we look at the elections in terms of incumbents and challengers, we can see that incumbents win more often. However, in the case of close elections, the winning frequency of incumbents and challengers are balanced.

⁹The exceptions to this are New Hampshire and Vermont, where terms are two years. They do not feature in our dataset.

Incumbents won rough same number of times as challengers in 5% margins, while challengers won slightly more times in 10% margins. The close-to-symmetric shape of the barchart of density of winning margins in figure 1.1 is consistent with this. Together these numbers suggest that there is no precise manipulation of selection into the treatment, which would threaten the validity of the identification assumption on which application of the RDD rests. To back-up this ‘eye-ball’ test, we will conduct and present the results of more formal tests later.

To improve precision other control variables are included. In particular, we control for state measures for population, percentage of the state workforce under union contract, average hourly earnings in manufacturing, unemployment rate and education. In addition, we add some state specific controls including farmland and urbanization. Farmland is the percentage of each state’s total acreage that is farmland in year 2004 and urbanization refers to the percentage of population in urbanized areas and urban clusters in the year 2000.

Data for the control variables are obtained from several sources. The labour-related variables are from U.S. Bureau of Labour Statistics. Figures for educational expenditures and urbanization are from U.S. Census Bureau. Farmland is from the U.S. Department of Agriculture Economic Research Service.

1.3 Results

1.3.1 Main results

Table 1.3 and table 1.4 present main results on the causal impact of the election of a Republican on (1) FDI per capita and (2) FDI in manufacturing per capita. Significance levels are adjusted by robust inference methods proposed by [Calonico et al. \(2014\)](#). The robust p-values are regarded as conservative, so the significance of the results reported will also hold if we were to use conventional p-values. Standard errors are clustered at state level.

Table 1.3 relates to FDI per capita. The top number in each column is the estimated discontinuity - the additional FDI causally attributed to a Republican win. In each panel, column (1) derives from estimates with no controls. Column (2) includes a control for whether the election was associated with a change in ruling party. Column (3) adds party change and other controls. The estimates are generally not significant. The second panel identifies a positive and significant effect of a Republican win on FDI inflow in the two year window following an election win, but the value becomes smaller and significance is lost (even at 10%) once controls are added. Overall the analysis reported in table 1.3 points to no discernible effect, positive or negative, of a Republican governorship.

Table 1.4 reports the results of conducting the same exercise but specialized on flows of FDI per capita into the manufacturing sectors. We find a significant positive effect of a Republican governor on these flows, which can be interpreted causally.

The effect sustains whole term of governor. The results are relatively insensitive to exclusion or inclusion of the control for party change or other controls.

Panel 1 implies that in his first year in office a Republican governor, other things equal, attracts an additional 97.11 USD dollars of FDI in manufacturing per head compared to his Democratic counterpart. In the first two years he attracts an additional 156.51 USD. In his first three years 262.88 USD. And in the full four years of his term an additional 195.59 USD. Note that these are not ‘within year’ flows, but rather the cumulative effect over four different time horizons. The state-level average FDI per capita in manufacturing in our sample is 920.82 USD so against that benchmark, the growth in FDI stock in manufacturing activities is 21% higher under a Republican governor during a four year term compared to a the counter-factual of a Democratic governor.

Figure 1.2 and figure 1.3 plot the estimates RD estimates of the effects of a Republican governorship on cumulative FDI per capita (Figure 1.2) and FDI in manufacturing per capita (Figure 1.3) across each of the four time horizons. The plotted estimates are based on our preferred specifications with all controls and the confidence levels are presented at 90% and 95%.

Figure 1.4 present the fitted curves either side of the discontinuity for each of the four exercises (recall that each exercise refers to a different time horizon over which the impact of the governorship on cumulative FDI is assessed). The ‘jump’ in the vicinity of the discontinuity is the effect presented in the earlier tables, and it can be seen here to be positive in each of the four panels. The size of each dot is determined by the number of election data points in each winning margin bucket. While the

specifications reported are estimated over a wider interval, we can see comparatively large dots in the immediate vicinity either side of the discontinuity, and that visually there is a very apparent step up from those just to the left of the discontinuity to those just to the right. While the data points further from the discontinuity are included in the estimates, they carry correspondingly lower weight.

To summarize; (1) we find no evidence that the political affiliation of the state governor has a causal impact on total FDI inflow. However, (2) there is a significant (at 1%), positive and substantial effect of a Republican governorship on inflows of manufacturing FDI, an effect that is sustained across the whole of the governor’s term in office.

1.3.2 Validity

In this section, we challenge our study design by performing three validity checks.

A key assumption of RDD is the continuity assumption - that the only change which occurs at the point of discontinuity is the shift in the treatment status.¹⁰ This would be compromised if, for example, one party or the other were able to manipulate the winning margin such as to be ‘just over the line’. In our setting, the violation of the continuity assumption would require the eventual winner be able to predict vote shares with extreme precision and then deploy necessary resources to win the close elections. Existing evidence suggests that this is not a significant risk in our setting

¹⁰[de la Cuesta and Imai \(2016\)](#) distinguish the continuity assumption from the local randomization assumption, where the latter one is more restricted. For the local randomization assumption to be met, within a window of pre-specified size around the discontinuity threshold, whether or not an observation receives the treatment is essentially randomly determined. [de la Cuesta and Imai \(2016\)](#) argue that the local randomization assumption is not required for the RD design to be valid.

([Eggers et al. \(2015\)](#), [de la Cuesta and Imai \(2016\)](#)). However, the following validity checks confirm that there is no evidence of such sorting behavior.

The standard approach to challenging the random selection into treatment assumption is the McCrary test ([McCrary \(2008\)](#)). The McCrary test essentially tests for whether there is discontinuity in the density of the assignment variable in the vicinity of the discontinuity being used for identification. The McCrary graph is presented in figure 1.5. If the parties can manipulate the election results in close elections, we should expect the proportion of observations just to the left of the cut-point to be meaningfully different from those to the right. Sorting, if it exist, would produce a discontinuity in the density of the forcing variable. We can easily see that the density is smooth around the cut-off and there is no unusual jump in the figure. It indicates there is no clear manipulation of the assignment variable. It corresponds to the claim in [Lee \(2008\)](#) that there is no discernible manipulation by one party near the cut-off. That is to say that the number of Democratic governors and Republicans are balanced. Consistent with [Erikson et al. \(2015\)](#) and [Eggers et al. \(2015\)](#), we find no significant discontinuities in the gubernatorial election, suggesting that there is no evidence of sorting.¹¹

We also conduct the density test proposed by [Cattaneo et al. \(2015\)](#), another continuity-based test of design. It uses a local polynomial density estimator that does not require the pre-binning of the data and leads to a size and power improvement

¹¹[Grimmer et al. \(2011\)](#) criticize the use of close elections because of the potential for imbalance near the threshold that distinguishes winners from losers. But their argument has been shown to apply only to the U.S. House of Representatives during the post-war period. [Eggers et al. \(2015\)](#) find no similar sorting occurs in other electoral settings, including the U.S. House in other time periods, statewide, state legislative, and mayoral races in the US.

([Skovron and Titiunik \(2015\)](#)). We are able to reject the null hypothesis that the density is discontinuous at the cut-off with an associated P-value of 0.4496.

To further examine whether the key identification assumption of the RD design is credible, we conduct placebo tests on a number of covariates. In particular, we perform the estimation on the covariates following the same methodology, and the results are presented in table 1.5. We find no significance, indicating that the covariates are balanced.¹² Results show that party affiliation of the governor has no effect on these variables.

1.3.3 Methodological robustness

In the main results section we explored the robustness of our main estimates to exclusion and inclusion of a variety of state-level controls. In this section, we perform sensitivity tests to examine the robustness of our RDD results as they apply to FDI in manufacturing to changes in modelling assumptions.¹³

Senate/House & President

In our preferred specification, we add state and time-varying characteristics including state population, percentage of the state workforce under union contract, average hourly earnings in manufacturing, unemployment rate, education expenses, farmland

¹²[de la Cuesta and Imai \(2016\)](#) argue that under the continuity assumption, observations on either side of the discontinuity threshold can systematically differ from each other in many aspects, even by a large magnitude. The imbalance of barely-winners and barely-losers near the threshold does not necessarily invalidate the application of the RDD. Nonetheless we find the covariates are balanced in our setting and the continuity assumption holds.

¹³We conducted similar exercises on the aggregate FDI data (that tested the robustness of the results in table 1.3) and obtain similar results. In other words the non-significance result in that case proves robust. We do not report these in detail here.

and urbanization in order to control for possible confounding factors that might influence the results.

To better isolate the impact of the gubernatorial election, we include more factors that may play roles in shaping foreign economic policies. In addition to state governor, state legislatures are also regarded powerful on international issues ([McMillan \(2009\)](#)). In table 1.6 we include the variables indicating which party controls the house and which controls the senate. Also, the party holds office at national level matters for setting the economic climate. In table 1.6 we control that the variables indicating which party the president belongs to. The results are robust to the inclusion of dummy variables for having Republicans control the state senate, for Republicans controlling the state house, and for the president being a Republican.

Bandwidth

For our preferred estimates we adopted a optimal bandwidth following [Calonico et al. \(2014\)](#). The optimal bandwidth selected by the method varies between about 7% and 10% across the specifications. This is similar to the bandwidth chosen (usually arbitrarily) in other applications of RDD methods to close elections.¹⁴ Nonetheless, as robustness checks we re-run each regression imposing first a 5% and then a 10% bandwidth. The results of these exercises are presented in table 1.7. The estimates retain sign and significance and are similar in value to those from the preferred specifications in table 1.4.

¹⁴Our bandwidth is close to [Caughey et al. \(2016\)](#) who study the effect of gubernatorial election outcomes on policy liberalism and [de Benedictis-Kessner and Warshaw \(2016\)](#) who use similar bandwidth when examining how the size of government reacts to outcomes of US mayoral election. Our bandwidth is tighter than, for example, [Klašnja and Titiunik \(2014\)](#).

Order of polynomial

In our base specification we fitted the data linearly on either side of the discontinuity. This is the preferred approach because [Gelman and Imbens \(2014\)](#) argue that estimators for causal effects based on high order (third, fourth, or higher) polynomials can be misleading in the RDD setting. [Gelman and Imbens \(2014\)](#) recommend instead use estimators based on local linear or quadratic polynomials.

In table 1.8 we report the results of repeating the exercise but fitting a second-order polynomial. Again we can see that the size of the estimated effects are little disturbed, and in all cases the sign and significance of the effect is sustained.

Outliers

To allay concerns that the result is being driven by a small number of extreme observations we perform outlier analysis by winsorizing data at 99% and 95% level. Winsorization is the statistical transformation of the data by limiting extreme values in the data to reduce the effect of (possibly spurious) outliers (winsorization is widely used by economists ([Alesina et al. \(2015\)](#), [Dick and Lehnert \(2010\)](#))). A 99% winsorization, for example, would see all data below the 1st percentile set to the 1st percentile, and data above the 99th percentile set to the 99th percentile. Winsorization methods are usually more robust to outliers, although there are alternatives, such as trimming, that will achieve a similar effect. One advantage of winsorization is that the transformation limits the impact of outliers, without losing observations. The results are presented in the table 1.9. The results are little disturbed by the winsorization suggesting that they are not overly driven by a few extreme-valued

outliers.

Alternative denominator for dependent variable

Our central analysis took as dependent variable FDI per capita as our measure of FDI intensity. An alternative and equally sensible approach would have been to work with FDI per unit of percentage of state level GSP. To confirm that this would not significantly have disturbed conclusions we re-estimate our preferred specification on that basis. The results of this exercise are reported in table 1.10.

In this version, a Republican governor - compared to the benchmark of a Democrat - causes a 12% increase in FDI stock in manufacturing as a percentage of the gross state product after one year and an increase of 27% over the four-year term in office. These are qualitatively similar to our main results, though somewhat larger in size.

1.4 Possible Channels

Economic theory suggests a large variety of potential mechanisms that relate political parties and attraction of FDI. Political parties can influence the location choices of multinationals in both direct and indirect ways. On one hand, state governments can influence the foreign investors directly by offering investment incentives of various kinds, like tax credits, cheap loans, access to foreign trade zones, and a variety of subsidies. On the other hand, the attraction policy can be indirect when a state's general economic development policies are shaped by political choices concerning

infrastructure development, investment in education, and labor-market regulation policies. But the question of exactly how and to what degree each targeting policies matter is still largely unresolved ([Halvorsen and Jakobsen \(2013\)](#)).

The literature on FDI determinants documents a number of factors multinational companies consider. The factors may include, market size and growth potential ([Resmini \(2000\)](#)), industry clustering ([Wheeler and Mody \(1992\)](#)), labor market flexibility and financial depth ([Yu and Walsh \(2010\)](#)), infrastructure, institutional quality, tax level, political risk ([Dunning \(1998\)](#)). But the relative importance of those factors to the international investors of these different traits is complex. The multinationals with long time horizons and multifaceted location motives, rarely count solely on any single incentive offered by states ([Halvorsen and Jakobsen \(2013\)](#)).

It is outside the scope of our analysis to investigate the exact mechanisms of impact of political parties on FDI. Foreign investors' decision could be influenced by various economic policies adopted by political parties in office. We take advantage of the RDD method by uncovering the net impact of those implemented economic policies.

1.5 Conclusion

Foreign investment plays a crucial role in the American - and other - economies. But how influential is the type of government in a place in determining the levels or patterns of inward investment?

In this paper, we present what we believe to be the first empirical investigation

of a causal link from political partisanship to FDI. As such we provide a further empirical point-of-connection between political and economic outcomes, with a direction of effect. To obtain plausibly exogenous assignment of treatment (political party in power) we use a regression discontinuity design, exploiting the discontinuity generated by the first-past-the-post election system.

The evidence points to Republican governors causing a substantial and sustained upward bump in foreign investment into manufacturing activities, when compared to their Democratic counterparts. However, we find no evidence one way or the other on total FDI flows, although those effects are much less precisely estimated. Rather than study particular mechanisms, an advantage of the RDD approach is that it allows us to be agnostic regarding mechanism(s). It seems likely that Republicans do some things that are attractive to investors, while Democrats may do other things, and the analysis here estimates the net effect of those interventions when added together.

This line of research could fruitfully be taken forward in different ways. One would be to explore the partisan effects on more finely classified activities - which types of economic activity are more or less sensitive to political events than others? A second is to explore the role of left- and right-leaning governments in settings other than the US, with alternative political, social and economic landscapes.

Table 1.1: Summary Statistics

	Mean (1)	Std. Dev (2)	Min (3)	Max (4)
FDI Stock per Capita	2795.70	4972.94	71.77	47463.41
FDI Stock per Capita in Manufacturing	920.82	1017.17	16.61	8322.05
FDI Stock / GSP	0.112	0.143	0.009	1.844
FDI Stock in Manufacturing / GSP	0.041	0.036	0.002	0.275
Population	4986858	5483532	397363	35842038
Union	15.741	7.142	2.8	38.3
Wage	13.454	1.740	9.589	21.329
Unemployment	2.924	0.948	1.081	12.166
Education Expense	32.889	6.268	15.957	50.201
Farm Land	41	24.3	1	93
Urbanization	71.70	14.76	38.2	94.4

Note: Union: percentage of the state workforce under union contract; Wage: average hourly earnings in manufacturing; Education expense: total state expenditure on education as a percentage of total government expenditure; Farm Land: the percentage of each state's total acreage that is farmland in 2004; Urbanization: percentage of population in urbanized areas and urban clusters in 2000.

Source: FDI-related variables are from U.S. Bureau of Economic Analysis; labour-related variables are from U.S. Bureau of labour statistics; Population, Education and Urbanization variable is from U.S. Census Bureau; Farmland is from U.S. Department of Agriculture Economic Research Service.

Table 1.2: Statistics on Gubernatorial Elections

Number of Elections	5%	10%	Total
All elections	81	157	361
Republican won	42	80	186
Democratic won	39	77	175
Incumbent won	41	74	222
Challenger won	40	83	139

Sources: Candidate and Constituency Statistics of Elections in the United States, 1788-1990 (ICPSR 7757) and Dave Leip's Atlas of U.S. Presidential Elections.

Table 1.3: RD Estimates for FDI Per Capita

	One year after election			Two years after election		
	(1)	(2)	(3)	(1)	(2)	(3)
Repub won	88.377 (114.89)	58.828 (109.49)	98.089 (104.7)	245.47** (144.82)	235.94** (149.07)	173.69 (148.46)
Bandwidth	0.180	0.209	0.188	0.131	0.135	0.138
Party Change		✓	✓		✓	✓
Controls			✓			✓
Effective Observations	215	237	213	171	177	173
Total Observations	341	341	334	336	336	329
	Three years after election			Four years after election		
	(1)	(2)	(3)	(1)	(2)	(3)
Repub won	80.088 (225.01)	54.995 (230.11)	91.52 (225.7)	-197.73 (272.41)	-131.58 (278.62)	-148.6 (275.16)
Bandwidth	0.179	0.199	0.181	0.193	0.192	0.211
Party Change		✓	✓		✓	✓
Controls			✓			✓
Effective Observations	186	201	183	192	192	198
Total Observations	305	305	301	299	299	295

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Estimated using local-linear regression, optimal bandwidth, triangular kernel and robust confidence intervals calculated using *rdrobust* (Calonico, Cattaneo and Titiunik, 2014)

Table 1.4: RD Estimates for FDI Manufacturing Per Capita

	One year after election			Two years after election		
	(1)	(2)	(3)	(1)	(2)	(3)
Repub won	103.04*** (38.706)	102.84*** (38.193)	97.107*** (32.027)	177.56*** (59.49)	176.31*** (59.394)	156.51*** (44.857)
Bandwidth	0.072	0.074	0.075	0.076	0.077	0.081
Party Change		✓	✓		✓	✓
Controls			✓			✓
Effective Observations	95	98	97	99	100	99
Total Observations	303	303	299	301	301	297
	Three years after election			Four years after election		
	(1)	(2)	(3)	(1)	(2)	(3)
Repub won	240.06*** (59.21)	242.18*** (59.034)	262.88*** (53.244)	145.97** (66.121)	144.53** (65.789)	195.59*** (60.975)
Bandwidth	0.110	0.110	0.084	0.115	0.115	0.102
Party Change		✓	✓		✓	✓
Controls			✓			✓
Effective Observations	126	125	95	129	129	113
Total Observations	288	288	284	284	284	280

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Estimated using local-linear regression, optimal bandwidth, triangular kernel and robust confidence intervals calculated using *rdrobust* (Calonico, Cattaneo and Titiunik, 2014)

Table 1.5: Covariate Balance Tests for Gubernatorial RD Design

	Estimate	BW	CI	Pr > z 	N
Population	94453	0.142	(-199154, 486529)	0.411	345
Union	-0.597	0.142	(-2.845, 1.808)	0.662	309
Wage	-0.111	0.119	(-0.658, 0.608)	0.939	305
Unemployment	0.314	0.149	(-0.405, 1.134)	0.353	309
Education Expense	-0.429	0.195	(-1.910, 1.437)	0.782	309
Farm land	-4.475	0.160	(-25.522, 14.323)	0.582	363
Urbanization	5.188	0.147	(-4.531, 18.136)	0.239	363

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Estimated using local-linear regression, optimal bandwidth, triangular kernel and robust confidence intervals calculated using *rdrobust* (Calonico, Cattaneo and Titiunik, 2014)

Table 1.6: RD Estimates for FDI Manufacturing Per Capita: Senate/House/President

	One year after election		Two years after election	
	(1)	(2)	(1)	(2)
Repub won	103.56*** (32.117)	100*** (31.918)	165.76*** (45.618)	152.74*** (44.052)
Bandwidth	0.079	0.075	0.080	0.081
Party Change	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Senate/House	✓		✓	
President		✓		✓
Effective Observations	94	97	95	99
Total Observations	291	299	289	297
	Three years after election		Four years after election	
	(1)	(2)	(1)	(2)
Repub won	311.86*** (55.642)	269.73*** (52.496)	228.33*** (63.863)	215.66*** (58.701)
Bandwidth	0.079	0.084	0.090	0.090
Party Change	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Senate & House	✓		✓	
President		✓		✓
Effective Observations	89	95	100	103
Observations	276	284	273	280

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Estimated using local-linear regression, optimal bandwidth, triangular kernel and robust confidence intervals calculated using *rdrobust* (Calonico, Cattaneo and Titiunik, 2014)

Table 1.7: RD Estimates for FDI Manufacturing Per Capita: 5% & 10% Bandwidth

	One year after election		Two years after election	
	5%	10%	5%	10 %
Repub won	70.17** (27.105)	89.12** (28.731)	118.4*** (47.622)	172.89*** (42.267)
Bandwidth	0.05	0.1	0.05	0.1
Party Change	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Effective Observations	63	121	63	120
Total Observations	299	299	297	297
	Three years after election		Four years after election	
	5%	10%	5%	10 %
Repub won	310.76*** (62.86)	256.21*** (51.737)	346.5*** (60.498)	198*** (61.107)
Bandwidth	0.05	0.1	0.05	0.1
Party Change	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Effective Observations	60	115	58	113
Observations	284	284	280	280

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Estimated using local-linear regression, optimal bandwidth, triangular kernel and robust confidence intervals calculated using *rdrobust* (Calonico, Cattaneo and Titiunik, 2014)

Table 1.8: RD Estimates for FDI Manufacturing Per Capita: Second Order Polynomial

	One year after election	Two years after election
Repub won	119.85*** (43.12)	221.93*** (56.847)
Bandwidth	0.130	0.139
Party Change	✓	✓
Controls	✓	✓
Effective Observations	144	153
Observations	299	297
	Three years after election	Four years after election
Repub won	336.41*** (72.113)	342.95*** (67.613)
Bandwidth	0.116	0.115
Party Change	✓	✓
Controls	✓	✓
Effective Observations	127	126
Observations	284	280

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Estimated using local-linear regression, optimal bandwidth, triangular kernel and robust confidence intervals calculated using *rdrobust* (Calonico, Cattaneo and Titiunik, 2014)

Table 1.9: RD Estimates for FDI Manufacturing Per Capita: 99% & 95% Winsorizing

	One year after election		Two years after election	
	99%	95%	99%	95%
Repub won	90.211*** (26.412)	66.918*** (24.292)	171.65*** (41.948)	154.24*** (39.71)
Bandwidth	0.093	0.128	0.098	0.121
Party Change	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Effective Observations	114	143	118	137
Observations	299	299	297	297
	Three years after election		Four year after election	
	99%	95%	99%	95%
Repub won	257.29*** (51.596)	254.22*** (46.739)	209.26*** (60.343)	202.95*** (52.03)
Bandwidth	0.097	0.098	0.093	0.112
Party Change	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Effective Observations	113	114	106	123
Observations	284	284	280	280

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Estimated using local-linear regression, optimal bandwidth, triangular kernel and robust confidence intervals calculated using *rdrobust* (Calonico, Cattaneo and Titiunik, 2014)

Table 1.10: RD Estimates for FDI Manufacturing as Percent of GSP

	One year after election	Two years after election
Repub won	0.0049*** (0.0013)	0.0089*** (0.0020)
Bandwidth	0.074	0.105
Party Change	✓	✓
Controls	✓	✓
Effective Observations	95	125
Observations	299	297
	Three years after election	Four years after election
Repub won	0.0132*** (0.0021)	0.0111*** (0.0039)
Bandwidth	0.104	0.115
Party Change	✓	✓
Controls	✓	✓
Effective Observations	118	126
Observations	284	280

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Estimated using local-linear regression, optimal bandwidth, triangular kernel and robust confidence intervals calculated using *rdrobust* (Calonico, Cattaneo and Titiunik, 2014)

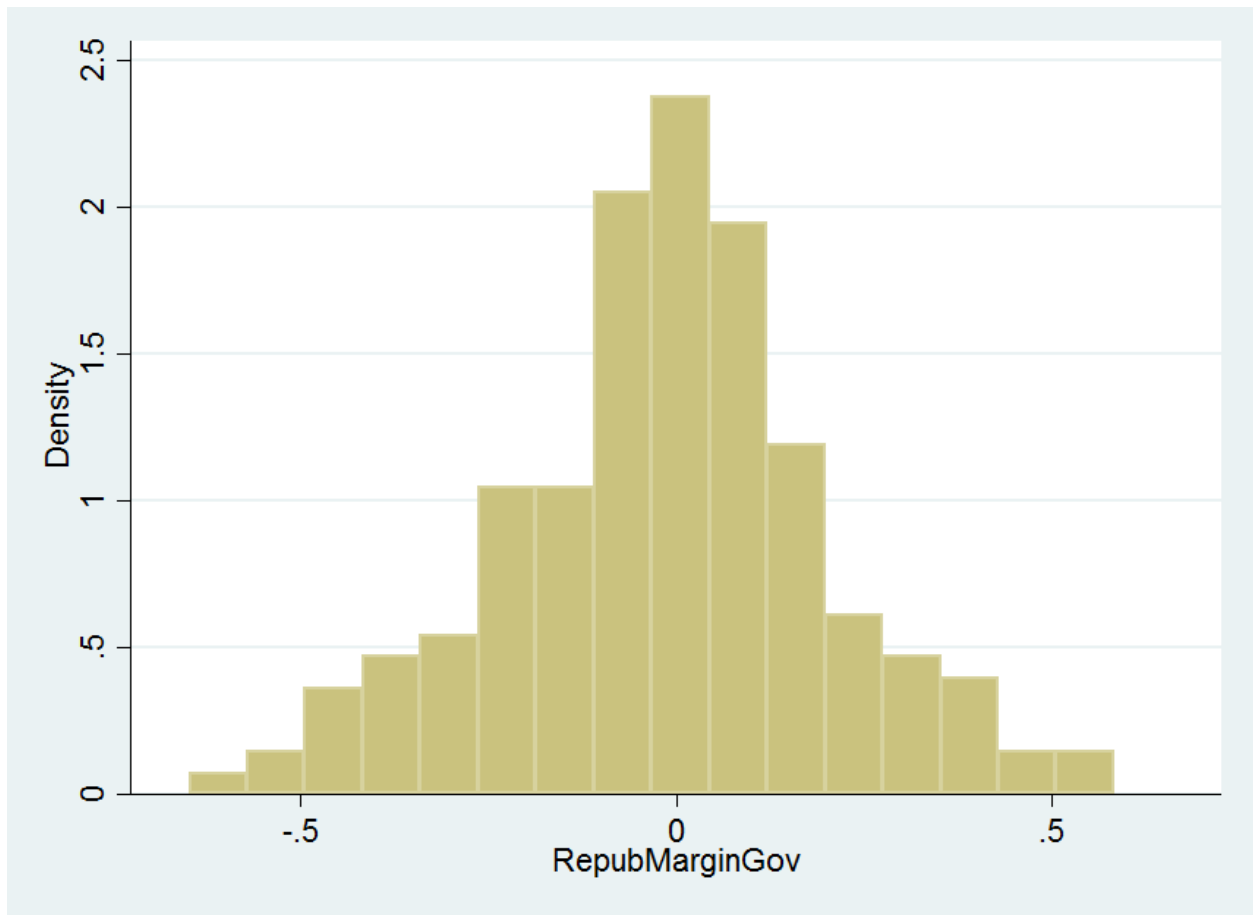
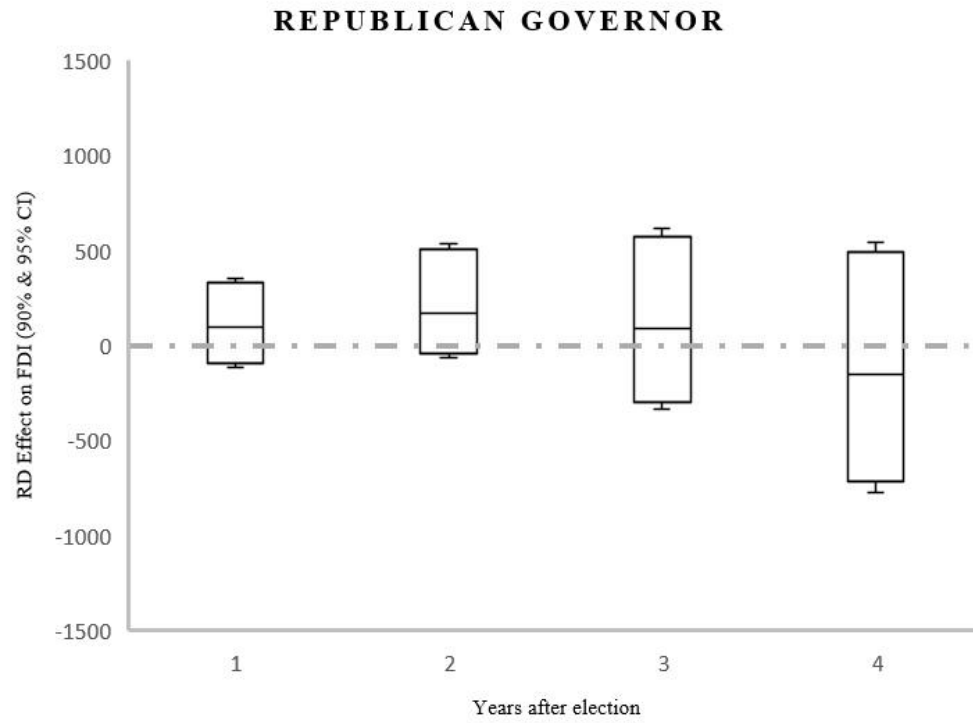
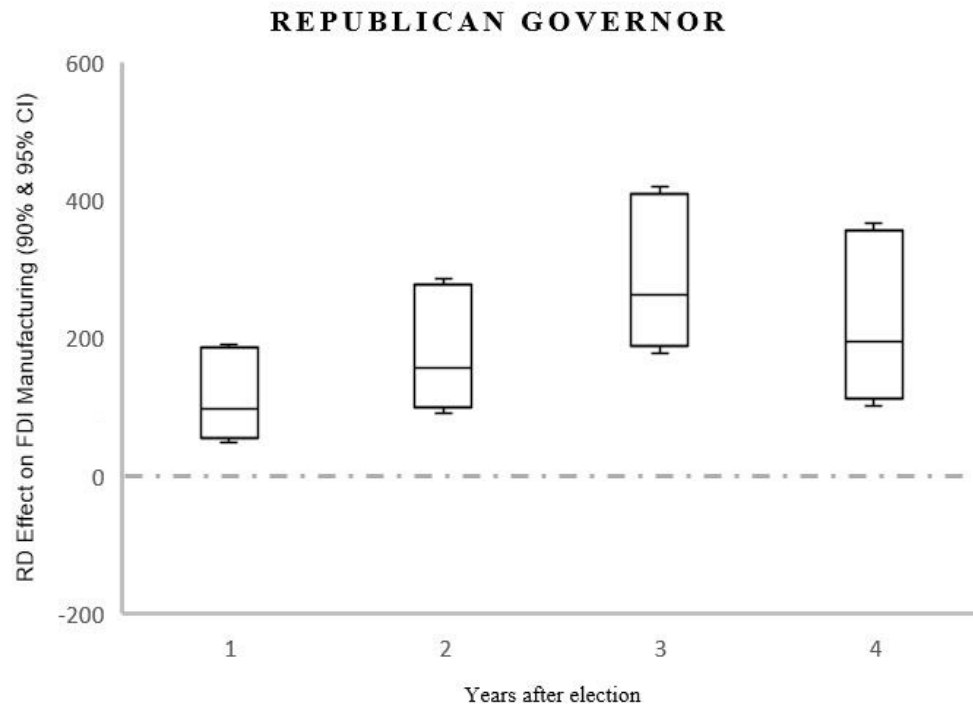


Figure 1.1: Histogram of victory margin



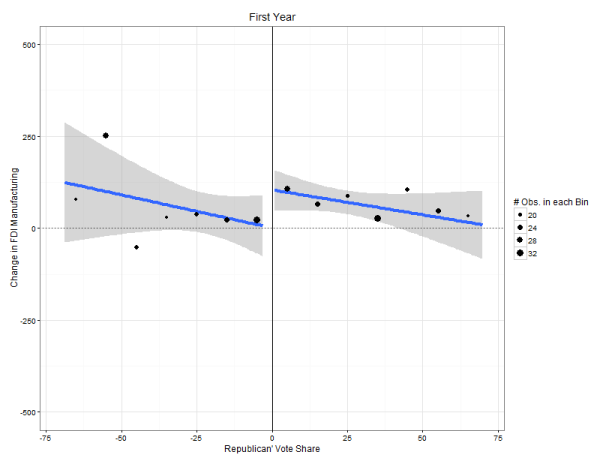
Note: The plotted estimates are based on the preferred specifications with all controls and the confidence levels are presented at 90% and 95%.

Figure 1.2: RD estimates on FDI, 1 to 4 years after election

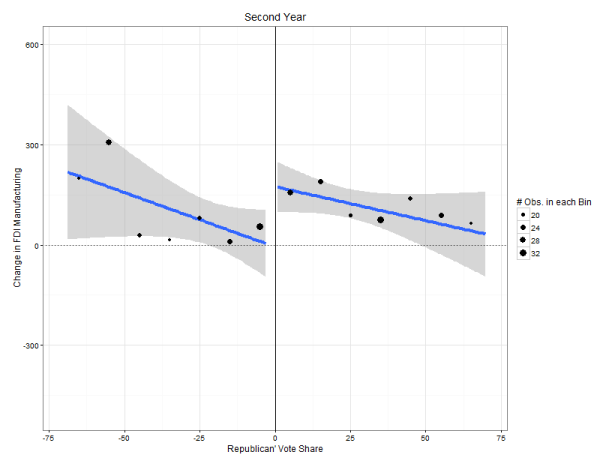


Note: The plotted estimates are based on the preferred specifications with all controls and the confidence levels are presented at 90% and 95%.

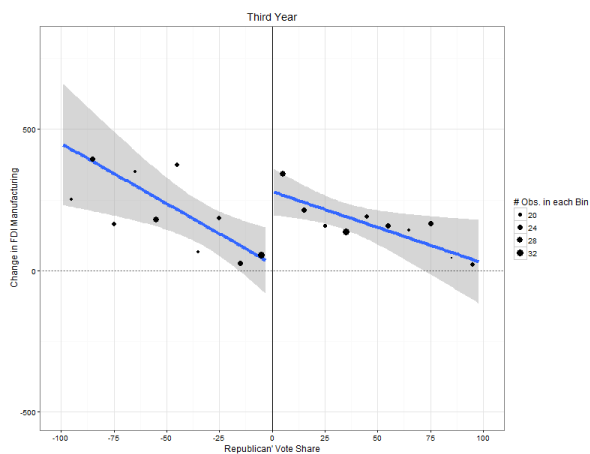
Figure 1.3: RD estimates on FDI Manufacturing, 1 to 4 years after election



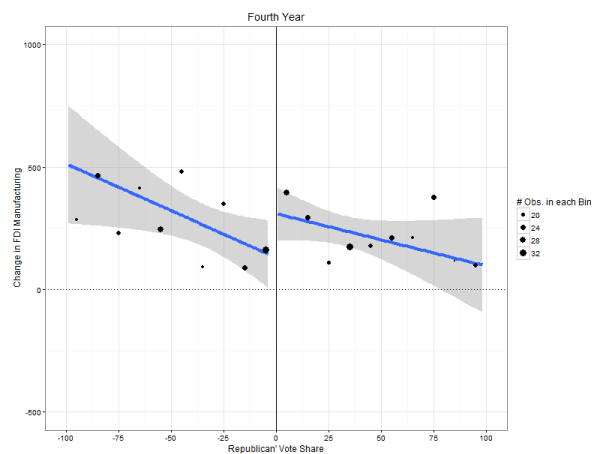
One year after election



Two years after election



Three years after election



Four years after election

Note: The vote share is multiplied by 1000. The black dots are averages in 0.5% bins and shaded are 95% confidence intervals.

Figure 1.4: The effect of electing a Republican governor on change in FDI in Manufacturing

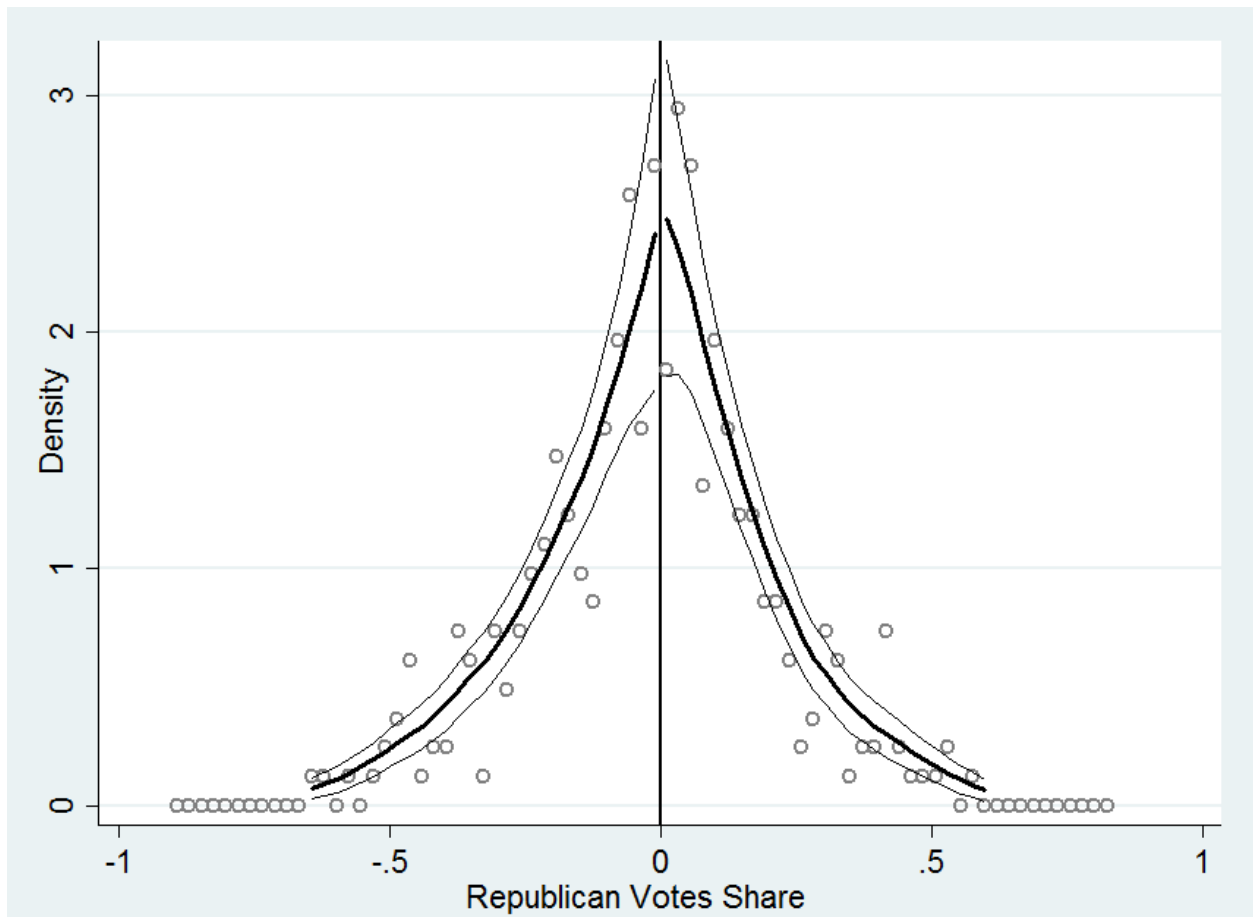


Figure 1.5: McCrary density of victory margin

Chapter 2

Commodity Price Shocks, Institutions and Windows of Opportunity for Structural Reform

2.1 Introduction

Finding a window of opportunity to conduct economic reform is one of the fundamental aims in political economy. Economic reform is difficult to implement, even when we considered those that are efficiency-enhancing. Often times, we witness reforms that are postponed or adopted after long delays, because reforms are rarely being Pareto improvement, resulting in winners and losers. Unequal distributions between benefits and costs exist both economically and politically, underlining the non-neutrality feature of reform ([Fernandez and Rodrik \(1991\)](#)). The large distri-

butional shifts of economic reform raise the stakes for every citizen in the political process, thus the state plays an important role in shaping the regulatory environment and in promoting economic reform.

Two important questions remain unanswered in this field. First, does output contraction induce reform? Does crisis shape the window of reform? The anticipated effect of output on reform hinges on two countervailing factors. On one hand, some evidence shows that crisis promotes reform due to the high cost of inaction and low political cost ([Alesina et al. \(2006\)](#)). On the other hand, crisis reduces the resources available to compensate losers, and thus deters reform from occurring ([Bonfiglioli and Gancia \(2015\)](#)). Furthermore, large negative economic shocks could radically change one's belief on the current policy and in turn, promote reforms. Meanwhile negative shocks raise the uncertainty and disagreement on the future economy and may even lead to political gridlock ([Abiad and Mody \(2005\)](#)). The net effect of economic shocks on reforms depend on the respective magnitudes of the effects.

Second, how do institutions decide when to conduct reforms? There are significant findings in the literature that emphasize the role of states and suggest that political freedom and economic freedom go hand in hand ([Persson and Tabellini \(2006\)](#)). Theoretically, democratic regimes implement more reforms if reforms create more winners than losers. Indeed, democratic regimes make decisions within a complex interaction between shifting voter preferences, strategic lobbying, and special interest politics ([Grossman and Helpman \(2001\)](#)). Democratic regimes could lead to fewer reforms if the electoral system creates a pivotal voter with veto power. The uncertainty about the impact of economic reforms at the individual level could also

lead to votes against reforms even if a majority of them would benefit from the reform. Moreover, if we turn to look back to the reforms in East Asian countries, many successful reforms were conducted in autocratic countries such as, South Korea and Singapore. The evidence is still mixed and far from reaching a conclusion.

Despite the growing attention of both economists and policy makers on the topic, little effort has been devoted to studying the effect of economic shocks on the adoption of structural reforms. This paper analyzes the relationship between economic shocks and structural reforms using unique data in 111 countries from 1962 to 2005. Previous research is limited to case studies, the use of pre-existing data sources that cover a relatively narrow set of reforms and countries. This paper addresses this issue by using an expanded dataset on structural reform that includes many countries across broader time-frame. Additionally, the economic literature ignores the fact that national income is endogenous to economic reform and overwhelmingly fails to find a causal effect between economic conditions and economic reform. This paper addresses this issue by using the exogenous variation in the international price of large commodity goods to generate the exogenous change in national income.

In the paper, we separately examine two types of structural reforms, namely, reforms in financial market sector and reforms in the product market sector. Product market sector reform is different from financial market sector reform in a number of ways. Due to the nature of the market structure of the product market, the cost of product market reform tends to concentrate on a few players. Thus, the target groups who lose from the reform are often easy to identified and get compensated. It is often not the case in the financial market. In addition, the impact of interest groups may be

amplified by several institutional factors, when industry representatives participate in regulatory decisions in product markets ([Høj et al. \(2006\)](#), [Faccio \(2006\)](#)).

This paper contributes to the literature in two ways. First, we take advantage of the changes in the international price of the principal commodity of a country as exogenous sources of income fluctuation to identify the effect of national income on reform. Secondly, this paper aims to investigate how economic shocks affect the windows of reform under different institutional regimes. The findings in this paper suggest that the reforms in autocratic countries are heavily affected by national income shocks while it is not the case in democratic countries. In addition, the product market reforms and the financial reforms tend to respond differently to the economic shocks. Positive economic shocks promote product market reforms but deter the financial reforms. The results are robust to a variety of robustness checks.

The paper proceeds as follows. Section 2 highlights the related literature. Section 3 describes the data. Section 4 illustrate the empirical methodology. Section 5 reports our results and Section 6 concludes.

2.2 Literature Review

The political economy of reform has been documented both theoretically and empirically. Theoretically, [Fernandez and Rodrik \(1991\)](#) look at the reform decisions regarding democratic voting mechanisms and propose that the source of preventing reforms is uncertainty regarding the distribution of gains and losses from the reform. In particular, there is a bias toward the status quo whenever the individual win-

ners and losers from reform cannot be identified in advance. While on the empirical side, [Giuliano et al. \(2013\)](#) find that an increase in the quality of democratic institutions has a positive and significant impact on the adoption of economic reforms. Also, [Alesina et al. \(2006\)](#) find that reforms tend to follow periods of inflationary and budgetary crisis, when new governments take office and when governments are strong.

Concerning the timing of reforms, [Rancière and Tornell \(2016\)](#) is in favor of the hypothesis that crises facilitate economic reforms. Specifically, they present a model where rent-seeking leads to economic decline, which, in turn, will make a future reform inevitable when times will already be bad enough. Furthermore, they use the case of trade liberalization as a prime example of structural reform to show that reforms are induced by severe crises. In addition, [Tommasi and Velasco \(1996\)](#) suggest that structural reforms tend to occur in crises rather than during prosperous times based on the experiences of developing countries with market-oriented reforms.

On the other hand, [Mian et al. \(2014\)](#) show that banking, currency, inflation, or debt crises lead to greater ideological polarization in society, greater fractionalization of the legislative body, and a decrease in the size of the working majority of the ruling coalition. The raising political polarization could weaken incumbent governments and hinder the adoption of the financial reforms.

Moreover, [Drazen and Easterly \(2001\)](#) find mixed evidence of crisis-induced reforms. In particular, they find that the reforms are more likely to be conducted following episodes of high inflation rate and black market premium, but no evidence show that high current account deficit, high budget deficit, and negative per capita

growth rate is correlated with reforms.

The effectiveness of reforms is another strand of related literature. Several empirical papers have looked at the outcomes of reforms. [Prati et al. \(2013\)](#) suggest that both real sector and financial sector reforms are promoting economic growth, but the positive effect of reform is influenced by a country's constraints on the authority of the executive power and by its distance from the technological frontier. Even though, the evaluation of the reforms is beyond the scope of this paper, the systematic empirical work in this field conveys an important message that reforms are mostly complicated issues, and thus the adoption of reforms could differ in timing and institutions.

The literature on economic shocks from commodity prices is also rich. The effect of economic shocks on political-economy outcomes like conflicts and democratic change are widely investigated. [Brückner and Ciccone \(2010\)](#) examine the effect of economic conditions on civil war, and find a robust effect of commodity price downturns on the outbreak of civil wars, and they also find that Sub-Saharan countries are more likely to engage in civil wars following economic downturns in their main OECD export destinations.

[Caselli and Tesei \(2016\)](#) document the effect of natural resource windfalls on political regimes. Their results indicate that natural resource windfalls have no effect on the political system when they occur in democracies, while windfalls have significant political consequences in autocracies. In deeply entrenched autocracies, the effect of windfalls on politics is little, while in moderately entrenched autocracies, the effect is significantly larger.

[Brückner et al. \(2012\)](#) look at the oil price shock on the democratic change. They find that countries with greater net oil exports over GDP see improvements in democratic institutions following upturns in international oil prices. However, [Burke and Leigh \(2010\)](#) use commodity price changes as instruments for income changes and find insignificant effects of commodity-driven income changes on political regimes.

Numerous paper attempts to understand the determinants of structural reform and how various economic conditions affect the likelihood of the adoption of reforms, few of them establish a causal effect, which is where we want to fill the gap. Moreover, the role of the interactions of institutions has received little attention when analyzing the impact of economic conditions. Institutions play a significant role in economic development ([Acemoglu et al. \(2001\)](#)). In fact, the political nature of an institutional equilibrium that makes it very difficult to reform economic institutions ([Acemoglu et al. \(2008\)](#), [Acemoglu and Robinson \(2008\)](#)). In this paper, we introduced the role of institutions to analyze the timing of structural reform facing economic shocks.

2.3 Data

In this section, the data used in this paper is described and the data is obtained from different sources. The sample period is from 1962 to 2005.

2.3.1 Data on reforms

The data on reforms comes from a completely new and extensive dataset, compiled by the Research Department of the International Monetary Fund (IMF), describing

the degree of regulation for a sample of 150 industrial and developing countries. [Prati et al. \(2013\)](#) describes all data sources of the reform indicators and full details of the construction of the indicators.

Reform is usually defined as liberalization of markets and changes in the regulatory environment ([Bonfiglioli and Gancia \(2015\)](#), [Giuliano et al. \(2013\)](#), [Høj et al. \(2006\)](#)). The following paragraphs present brief definitions and construction of the reform indicators used in this paper. Reform indicators cover both financial and real sectors. Financial sector reform indicators include reforms pertaining to domestic financial markets, while real sector structural reform indicators include measures of product markets, including telecommunication and electricity markets. The indicators are constructed such that, firstly, both sectors contains different sub-indices summarizing different dimensions of the regulatory environment in each sector. Secondly, the sub-indices are then aggregated into indices and normalized between 0 and 1 and the higher values represent greater degree of liberalization.

The measures of financial sector reforms are mainly for the domestic financial sector. The domestic financial sector liberalization indicator in turn includes measures of regulation indicators of banking sector and securities markets. The banking system index contains (1) credit controls, such as directed credit and subsidized lending, (2) interest rate controls, such as floors or ceilings, (3) competition restrictions, such as limits on branches and entry barriers in the banking market, including licensing requirements or limits on foreign banks), (4) public ownership of banks. (5) quality of banking supervision and regulation, including the power and independence of bank supervisors, the adoption of Basel capital standards, and the presence of a

framework for bank inspections.

The security markets index captures the quality of the market framework, including the degree of legal restrictions on the development of domestic bonds and equity markets as well as the existence of independent regulators. Turning to the real sector, the product market indicator covers the degree of liberalization in the telecommunication and electricity markets, including the extent of competition in the provision of these services, the presence of an independent regulatory authority, and degree of privatization.

We use the variation in the absolute degree of economic liberalization within each sector to measure the reform in the corresponding sector across countries and over time. To be noted, the differences in the value of the indices across sectors do not indicate a precise quantitative measure of whether one sector is more liberalized than another due to the different methodology used to construct each index. A positive difference between the index in the financial sector and the real sector does not necessarily mean that the financial sector is more liberalized than the real sector.

2.3.2 Data on commodity prices

To construct a country level economic shock based on commodity price, we follow the method used in [Caselli and Tesei \(2016\)](#). A country's principal commodity is identified as the commodity that is ranked first by its value of exports in the largest number of years. Then the principal commodity is matched with the corresponding world prices. The commodities include both agricultural and mineral commodities. The principal commodity export shares and prices data are from UN Comtrade data

set and IMF IFS data set. The detailed data sources and construction are well-documented in [Caselli and Tesei \(2016\)](#).

2.3.3 Data on institutions

Institutions are measured using the standard, well-established measure of democracy taken from the Polity IV database, which measures the longitudinal patterns of the political regime characteristics and transitions. The data on institutions is based on the Polity2 variable in the Polity IV database. The Polity2 variable captures the electoral rules and openness of the political institutions. The value determination is obtained on three main building blocks: (1) Existence of institutions through which citizens can express preference over different politics and leaders; (2) Existence of institutional constraints on the exercise of power of the executive in charge; (3) Guarantee of civil liberties to all citizens both in their daily life and in their political participation.

In particular, we use the combined Polity2 index ranging from -10 to 10 (-10=high autocracy; 10=high democracy). The threshold for democracy is conventionally set at strictly positive values of the variable. In our setting, we adopt the current best practice in the political science literature that the democratic countries hold positive values, while the autocratic countries hold negative values.

2.4 Research Design

In this section we outline research methods, including discussion of assumptions and potential challenges to compelling identification.

2.4.1 Empirical Strategy

The empirical strategy we adopt in this paper is to employ the economic shock from the change in the price of country’s principal commodity. Our strategy is close to [Caselli and Tesei \(2016\)](#). It relies on two critical assumptions, which are plausible in our setting. Firstly, the main export commodity of a country should be mostly driven by geographic features so that it is exogenous to the political and economic environment. Second, as small and open economies, the vast majority of countries’ principal export commodity individually account for a relatively small share of the world output, so it is very unlikely that structural reform in one country will have a significant effect on international prices.

Some related literature ([Burke and Leigh \(2010\)](#)) use the price shocks as an instrument for income changes. We do not follow the instrument variables approach, because commodity shocks could affect reforms through many economic channels, like household incomes, state revenues, inequality ([Bazzi and Blattman \(2014\)](#)). We maintain the widely accepted hypothesis that the main effect is upon income, but we do not rule out the possibility of other plausible channels. As a result, we prefer a reduced-form specification to estimate the direct effect of the commodity price shocks on reforms.

2.4.2 Model

Our baseline specification is as follows:

$$\Delta Y_{it} = \alpha_i + \beta_1 \Delta X_{i(t-1,t-3)} + \delta_t + \varepsilon_{it} \quad (2.1)$$

ΔY_{it} is the outcome of interest: first difference in regulation indices in country i and year t , measuring structural reform; $\Delta X_{i(t-1,t-3)}$ is the average growth rate in a country's principal commodity over the last three years horizon; α_i are country fixed effects; δ_t are year fixed effects; ε_{it} is an error term that is clustered at the country level and can be correlated across years within the same country.

Reforms are measured by change of the regulation indices between year $t-1$ and t . We estimate the structural reform in financial sector and product market sector separately. The change in commodity prices is the average growth rate in the price of principal commodity over the year $t-4$, $t-3$, $t-2$, and $t-1$. The measure of commodity shock by using the average three-years growth in international price has also been used extensively in previous literature ([Brückner and Ciccone \(2010\)](#), [Caselli and Tesei \(2016\)](#), [Deaton \(1999\)](#)).

There are several reasons that we use lagged average changes in commodity prices. First, it will defuse concerns about reverse causation. Second, by allowing more possible lags, we expand the time that the economic reform may take place to react to economic events. Last, averaging the prices change will reduce the role of extremely transitory shocks and reduce possible measurement error. The average three-years growth can capture temporary deviations from the average growth process, while

being enough persistent to influence the reform dynamics of a country.

To account for the serial correlation in the estimates introduced by the rolling windows by construction, the standard errors are clustered at the country level in all regressions, allowing for heteroscedasticity and arbitrary correlation in the error term. Also, we include country fixed effects in our dynamic panel regression model in order to ensure underlying country-specific trends common to changes in principal-commodity prices and adoption of reforms will not drive our results to being biased.

In order to account for the role of institutions, we introduce the interaction term between economic shocks from price changes and dummy variables indicating if the country is a democratic country. In particular, we estimate the following equation:

$$\Delta Y_{it} = \alpha_i + \beta_1 \Delta X_{i(t-1,t-3)}|A + \beta_2 \Delta X_{i(t-1,t-3)}|D + \delta_t + \varepsilon_{it} \quad (2.2)$$

In this equation, $\Delta X_{i(t-1,t-3)}|A$ is the interaction term of commodity price change with the autocratic dummy, while $\Delta X_{i(t-1,t-3)}|D$ is the interaction term of commodity price change with the democratic dummy. Also, we control for the dummy capturing whether the country is a democratic country before the economic shock in the regression. The lagged level of the reform index is also controlled to identify the existence of convergence toward some possible country specific levels of regulation.

Meanwhile, by including lagged level of dependent variable, the model is a dynamic panel data model with fixed effects. There is concern of bias. The bias of the estimator in a dynamic model is generally known as dynamic panel bias or Nickell bias [Nickell \(1981\)](#).¹ In the literature, to get consistent estimates in the dynamic

¹The bias is caused by eliminating the unknown individual effects from each observation, which

panel data model with fixed effects, the System-GMM is usually adopted by taking first differences and instrumenting the differenced variables with all their available lags in levels and differences (Caselli and Tesei (2016)). The GMM estimator is derived by using suitably lagged levels of the dependent and predetermined independent variables as instruments for the equation in first differences (Arellano and Bond (1991)).

In our sample, the long time span may lead to bias in the asymptotic results of system-GMM, since the instrument count grows large relative to the sample size, over-fitting the instrumented variables and failing to expunge their endogenous components. The number of instruments must be limited (Caselli and Tesei (2016)). To solve this problem, we follow the practice in Beck and Levine (2004) and Calderon et al. (2002) to combine instruments through addition into smaller sets so that the instrument counts linear in T and no information is discarded (Roodman (2009)).

2.5 Results

In this section, we present summary statistics of our sample as well as the estimation result and robustness checks.

2.5.1 Main Results

Table 2.1 shows the summary statistics of the main variable used in the model. Price shock refers to the average growth rate in the price of the principal commodity over

creates a correlation of order $(1/T)$ between the explanatory variables and the residuals in the transformed model.

a three-year window, while the reform is the one-year difference in regulation indices. We can see that there is a big variation in prices shocks. There are more positive commodity price shocks than negative ones in the sample. 63% of our sample are democratic countries.

Table 2.2 presents the estimation results of financial market reform. Column (1) presents the first specification. Our dependent variable is one-year change in regulation index which represents reform. Our independent variable is lagged price change in principal commodity averaged over three years. Regressing reform indices on lagged price change in principal commodity yields a negative but statistically insignificant coefficient. Column (2) presents the results of adding democratic dummy as a control. The impact of the price shock is more precisely estimated but remains negative.

In column (3), we test our model in a more detailed way to uncover the differential impact of institutions. In particular, we look at the effect of price shocks on democracies and autocracies separately. The price shock is interacted with the dummy variables indicating whether the country is democratic and autocratic. Then the coefficient of interest becomes the coefficient of the interaction terms. We can see that the coefficient on the autocracies is negative and statistically significant at 1% level, while coefficient on the democracies is negative and statistically insignificant. The estimated coefficients imply that the impact of price shock in autocracies is much stronger than in democracies. Column (3) is our preferred specification. The coefficient on the the autocracies implies that a one standard deviation increase in the price of the principal commodity of a country reduces the financial reform indices

by 22%.

As predicted in the discussion in the previous section, the institutions do play an important role in shaping the choice of reform during varying economic conditions. The negative relationship suggests that the economic shock deters the reform in financial market.

As we mention in the empirical strategy section, there is a concern of bias in the dynamic panel regression with fixed effects when we include the lagged level of the reform index in column (4). However, the Nickell bias in the OLS specification is not a big concern in our setting, since the sample we use has a large time dimension. As a result, we should not expect the OLS estimates to be significantly different from system-GMM estimates. Nonetheless, we provide results from both estimation methods. We can see from column (4) and column (5) of table 2.2, the estimation from system-GMM and OLS are typically very similar to each other, which is not surprising.

In fact, we prefer the OLS estimates to the system-GMM estimates. As [Roodman \(2009\)](#) points out, if the time span is large, the bias from the dynamic panel regression becomes insignificant, and a more straightforward fixed-effects estimator will perform well.² So we prefer to present OLS estimates.

Now we turn to the estimation results of product market reforms in table 2.3. We see a positive and significant coefficient, suggesting that the average effect of price shock is positive related to product market reform. In addition, the results in

²At the same time, the number of instruments in difference and system-GMM tends to explode as the years of observation grow. If the number of observations is small, the cluster-robust standard errors and the Arellano-Bond autocorrelation test become unreliable.

column (3) decompose the effect of price shock. As we can see the coefficient on the autocracies is positive and statistically significant at 1% level, and the coefficient on the democracies is also positive but much less significant than the estimates on the autocracies. The estimation indicates that the positive impact of price shock on product market is still largely driven by the autocratic countries. Column (3) is again our preferred specification. A one standard deviation increase in the price of the principal commodity of a country increase the product market reform indices by 21%. Economic shocks open windows for product market reform.

2.5.2 Robustness Checks

In this section, we perform robustness checks of our baseline results. One plausible concern is that current commodity prices are affected by expectations of future economic reforms in the main world producers. One possible solution is to investigate robustness by dropping the largest producers in the sample. In column 1 and column 2 of table 2.4 and table 2.5, we address this concern by excluding from all countries belonging to OPEC and those accounting for more than 3% of the total world production of their principal commodity. Due to the significant drop in sample size, we lose some levels of significance, but the results stay quantitatively similar. Dropping these countries has little material effects on our results.

Additionally, for countries which already have a highly liberalized market, the economic shock would play little role of pushing the reform further. To account of this concern of dynamics of reform, in column 3 of table 2.4 and table 2.5, we drop the countries which has the regulation indices equals to one. The coefficient is

significant at 1% level and similar in magnitude to our main results.

Furthermore we address the concern that principal commodities may account for a large share of GDP in autocratic countries. If this is true, the role of the institution may be just a result of a large economic shocks due to higher share of principal commodity. To check this, we control the share of principal commodity in the regression and the results are presented in last columns in table 2.4 and table 2.5. The results are significant, suggesting that this is not a major concern for our results.

For the countries whose principal commodity accounts for only a small share of GDP, it is unlikely that a commodity price change represents an important economic shock to the national income. So we exclude the countries with small principal-commodity shares. In table 2.6 and table 2.7, column 1 excludes countries in the first quartile; column 2 excludes all countries below the median average share. All of the estimation results remain close to the baseline estimates and significance level and are thus quite robust.

Last, we further test whether the impact of principal commodity price change is through the national income channel by running placebo regressions which only include the countries whose principal commodity accounts for only a small share of GDP. In table 2.6 and table 2.7, column 3 only includes countries in the first quartile; column 4 only includes all countries below the median average share. The estimation results show little significance both for financial sector reform and product sector reform. It suggests that the price change of principal commodity take effects by affecting national income.

2.6 Conclusion

Political economists research the time windows to conduct economic reforms. Economic shocks are one of the main causes investigated. The question of whether economic shocks bring opportunities for conducting economic reforms is central to the political economy literature. Unfortunately, the empirical evidence is limited to small set of countries, to restricted reforms and to short periods, while the finding to this question remain tentative. More importantly, the central problem with existing studies is that they provide evidence only of correlation and often fail to address the issues of causation. This is a big limitation. For example, it might be plausible that some unobserved factors drives two series. At the same time there may be causal effects running in the opposite direction to the one implicitly assumed. For instance, a country adopting certain forms of reforms may internalize the economic shocks.

We use novel dataset on structural reforms, which encompasses several sectors and many countries over a large number of years to generate the first evidence of a causal link from economics shock to windows of reform. In particular, we identify an economic shock in a country as the change in the price of the country's principal commodity, since both the identity of a country's principal commodity and its price in international markets are largely exogenous to the country's choice of economic reform.

The main conclusions of this paper are that (1) we find that major economic reforms following economic shocks are often conducted in autocratic countries; (2) The effect of economic shocks in autocracies is heterogeneous across sectors. An

economic shock is negatively associated with the reform in financial sectors, while positively related to the reforms in real sectors. (3) By contrast, there is little effect on reforms in democratic regimes. Democratic regimes are essentially unaffected by economic shocks, contrasting to the consequences in autocratic regimes, suggesting that institution types are important in determining the economic reform.

Our results are similar to the literature on the political and economic effects of natural resource wealth. [Andersen and Aslaksen \(2013\)](#) investigate the relationship between resource wealth and the duration of a political leadership. They find that wealth derived from natural resources affects political survival in intermediate and autocratic, but not in democratic polities. Our results suggest that reforms are only sensitive to commodity price shock in autocratic countries. A possible channel is that the revenue flow from resource rents make the autocratic government takes a strategic action in reform to increase its probability of remaining in power, when facing commodity shocks.

While the literature has identified several explanations for the timing of windows of reform, with many of them identifying single factors as cornerstones, until now the role of interaction between economic shocks and institutions has received little attention. As a result, existing theories often lack sharp predictions on the window choice of reform. The analysis in this paper uncovers the asymmetric impact of economic shock on structural reforms in democracies and autocracies, as well as the differential impact within autocracies.

This paper offers insights for the broader literature on the political economy of reform. The function of the institution is of great significance to the window

of structural reform. Furthermore, the findings suggest that additional research on structural reform will only be fruitful by considering reform in particular areas, rather than focusing on broad reform episodes. More detailed work awaits for further exploration.

Table 2.1: Summary Statistic

	Mean (1)	Std. Deviation (2)	Min (3)	Max (4)
Price Shock	0.068	0.187	-0.366	1.044
Financial Market Reform	0.020	0.051	-0.278	0.333
Financial Market Reform(Democracy)	0.021	0.050	-0.222	0.278
Financial Market Reform(Autocracy)	0.018	0.053	-0.278	0.333
Product Market Reform	0.010	0.048	0	0.727
Product Market Reform(Democracy)	0.019	0.066	0	0.727
Product Market Reform(Autocracy)	0.006	0.032	0	0.454
Democracy	0.627	0.484	0	1

Note: Reforms are measured by change of the regulation indices between year t-1 and t. Price shock is the average growth rate in the price of principal commodity over the year t-4, t-3, t-2, and t-1.

Table 2.2: Estimation for Financial Market Reform

	Model 1 OLS	Model 2 OLS	Model 3 OLS	Model 4 OLS	Model 5 GMM
Price shock	-0.011 (0.007)	-0.014** (0.006)			
Price shock in Autocracy			-0.023*** (0.008)	-0.023*** (0.007)	-0.017** (0.007)
Price shock in Democracy			-0.003 (0.009)	-0.004 (0.009)	-0.003 (0.009)
Democracy		0.006 (0.004)	0.005 (0.004)	0.004 (0.004)	0.0004 (0.002)
Reform _{t-1}				0.063** (0.025)	0.120*** (0.026)
Country FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Number of countries	81	81	81	81	81
Observations	2,302	2,246	2,246	2,158	2,158

Note: The table presents estimates of the impact of commodity price shocks on Financial Market Reform. Dependent variable is reform index measured by change of the regulation indices. Price shock is the average growth rate in the price of principal commodity over three years. All models control for state and year fixed effects. Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 2.3: Estimation for Product Market Reform

	Model 1 OLS	Model 2 OLS	Model 3 OLS	Model 4 OLS	Model 5 GMM
Price shock	0.011* (0.006)	0.014** (0.006)			
Price shock in Autocracy			0.011*** (0.004)	0.011** (0.004)	0.008* (0.004)
Price shock in Democracy			0.017* (0.013)	0.016 (0.013)	0.008 (0.012)
Democracy		-0.000 (0.003)	-0.001 (0.003)	-0.0007 (0.003)	0.009*** (0.002)
Reform _{t-1}				0.054 (0.037)	0.112*** (0.041)
Country FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Number of countries	103	103	103	103	103
Observations	3,828	3,413	3,413	3,412	3,412

Note: The table presents estimates of the impact of commodity price shocks on Product Market Reform. Dependent variable is reform index measured by change of the regulation indices. Price shock is the average growth rate in the price of principal commodity over three years. All models control for state and year fixed effects. Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 2.4: Robustness Check for Financial Market Reform: Exclude Big Producer & Big Regulation Indices & Control for Shares

	Exclude OPEC	Exclude Big Producer	Exclude Big Indices	Control Shares
Price shock in Autocracy	-0.021** (0.009)	-0.021** (0.009)	-0.023*** (0.008)	-0.030*** (0.009)
Price shock in Democracy	-0.003 (0.010)	-0.006 (0.014)	-0.004 (0.010)	-0.008 (0.009)
Democracy	0.005 (0.004)	0.003 (0.005)	0.005 (0.004)	0.006 (0.004)
Share				0.047 (0.057)
Country FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Number of countries	77	51	81	81
Observations	2,118	1,337	2,158	2,006

Note: The table presents estimates of the impact of commodity price shocks on Financial Market Reform. Dependent variable is reform index measured by change of the regulation indices. Price shock is the average growth rate in the price of principal commodity over three years. All models control for state and year fixed effects. Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 2.5: Robustness Check for Product Market Reform: Exclude Big Producer & Big Regulation Indices & Control for Shares

	Exclude OPEC	Exclude Big Producer	Exclude Big Indices	Control Shares
Price shock in Autocracy	0.010** (0.004)	0.005 (0.004)	0.011*** (0.004)	0.014*** (0.005)
Price shock in Democracy	0.020 (0.014)	0.009 (0.014)	0.017 (0.013)	0.020 (0.014)
Democracy	-0.002 (0.034)	0.002 (0.004)	-0.001 (0.003)	-0.001 (0.004)
Share				0.009 (0.021)
Country FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Number of countries	96	68	103	102
Observations	3,133	2,131	3,411	2,687

Note: The table presents estimates of the impact of commodity price shocks on Product Market Reform. Dependent variable is reform index measured by change of the regulation indices. Price shock is the average growth rate in the price of principal commodity over three years. All models control for state and year fixed effects. Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 2.6: Robustness Check for Financial Market Reform: Export Share

	Exclude First Quartile	Exclude Below Median	Placebo First Quartile	Placebo Below Median
Price shock in Autocracy	-0.020** (0.009)	-0.031*** (0.010)	-0.020 (0.023)	-0.013 (0.013)
Price shock in Democracy	-0.006 (0.011)	-0.024* (0.012)	0.012 (0.013)	0.011 (0.012)
Democracy	0.004 (0.004)	0.004 (0.005)	0.007 (0.012)	0.006 (0.006)
Country FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Number of countries	56	30	25	51
Observations	1,573	845	673	1401

Note: The table presents estimates of the impact of commodity price shocks on Financial Market Reform. Dependent variable is reform index measured by change of the regulation indices. Price shock is the average growth rate in the price of principal commodity over three years. All models control for state and year fixed effects. Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 2.7: Robustness Check for Product Market Reform: Export Share

	Exclude First Quartile	Exclude Below Median	Placebo First Quartile	Placebo Below Median
Price shock in Autocracy	0.015*** (0.004)	0.015** (0.006)	-0.006 (0.013)	0.009 (0.007)
Price shock in Democracy	0.036** (0.016)	0.034 (0.024)	-0.028 (0.018)	0.006 (0.016)
Democracy	-0.003 (0.003)	0.002 (0.004)	0.010 (0.009)	-0.005 (0.005)
Country FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Number of countries	75	47	28	56
Observations	2,519	1,574	894	1839

Note: The table presents estimates of the impact of commodity price shocks on Product Market Reform. Dependent variable is reform index measured by change of the regulation indices. Price shock is the average growth rate in the price of principal commodity over three years. All models control for state and year fixed effects. Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Chapter 3

The Social Footprint of Bank Regulation: Natural Experimental Evidence from the US

3.1 Introduction

The large-scale deregulation of retail banking in the US in the second half of the twentieth century has been causally linked to economic development ([Jayaratne and Strahan \(1996\)](#)), more business start-ups ([Black and Strahan \(2002\)](#)), raised efficiency in the banking sector ([Jayaratne and Strahan \(1998\)](#)), reduced income inequality ([Beck et al. \(2010\)](#)), improved access to college education of children from credit-constrained (lower income) households ([Sun and Yannelis \(2016\)](#)) and other outcomes.

While the economic benefits of deregulation of retail banking have often been discussed in both academic and popular media, the social impacts are much less well understood.¹ In the US it lead, through increased competition in the local retail banking markets, to a substantial increase in credit extended both to existing borrowers and also to previously excluded households ([Dick and Lehnert \(2010\)](#)). Given the central role that debt and financial stress have been shown to play in depression ([Bridges and Disney \(2010\)](#)), mental health problems ([Fitch et al. \(2007\)](#)), self-harm and suicide ([Hatcher \(1994\)](#)), suicidal thoughts ([Hintikka et al. \(1998\)](#)), marital dissatisfaction ([Grable et al. \(2007\)](#)), intra-household conflict ([Papp et al. \(2009\)](#)), domestic violence ([Fox et al. \(2002\)](#)) and other measures of personal or family distress, it is natural to ask: Are there previously unaccounted for impacts of the relaxation of regulation on important human distress metrics?

It is to this debate - what are the aggregate social consequence of freer access to credit - that we aim to contribute here. Motivated by the research just cited we focus on two suggestive metrics, namely; (1) rates of suicide and, (2) rates of divorce. Noting the centrality of debt and financial circumstances in popular and practitioner discussion of both outcome and divorce, and the important impact that bank deregulation had on credit availability across the US, it is perhaps surprising that the possible causal link between the two has not been explored rigorously before.

Cross-sectional analysis could be used to uncover any correlation between stringency of banking regulation in a jurisdiction and rates of suicide and divorce. How-

¹The two papers that speak to negative social impact are: (1) [Dick and Lehnert \(2010\)](#) estimate that lifting of intrastate branching restrictions - which we will focus on here - caused a 10 to 16% increase in personal bankruptcies. (2) [Garmaise and Moskowitz \(2006\)](#) estimate that the same reform caused a decrease in property crime.

ever, deducing a causal relationship from any such correlation would be fraught with danger. Jurisdictions with unobserved characteristics that lead to them to have less regulated banking might plausibly have characteristics (the same, or other) that make inhabitants of those jurisdictions more prone to suicide and/or divorce.

Here we overcome this methodological challenge by exploiting the staggered deregulation of intrastate banking rules across states during the 1970s, 1980s and 1990s.² Importantly the timing of adoption of deregulation has been shown to be unrelated to actual or anticipated state-level economic circumstances ([Jayaratne and Strahan \(1996\)](#)).³ We will also confirm that timing of deregulation was uncorrelated with state-level suicide and divorce rates, and carry out an unusually thorough set of placebo exercises in which we repeatedly falsely assign treatment dates to states. A significant number of papers, some already cited, have used the exogenous variation in timing of treatment to underpin causal inference using a differences-in-differences design. We do the same here, following the empirical approach of [Sun and Yannelis \(2016\)](#), [Dick and Lehnert \(2010\)](#) and others more or less exactly.

So what do we expect to find? As should be clear, there are mechanisms that plausibly push in opposing directions on our outcomes of interest. Making access to credit easier can aid consumption smoothing - help individuals and families to remain liquid during a temporary fall in income - can plausibly mitigate distress. On the other hand looser credit can feed indebtedness problems and over-borrowing under predatory lending practices ([Bond et al. \(2009\)](#)) or among those with self-control

²We will outline the institutional detail of this and other components of the deregulation of retail banking later in the paper.

³[Kroszner and Strahan \(1999\)](#) provide a careful analysis of the economic and political reasons for the exact timing of state branching deregulations in various states.

problems (Gathergood (2012)), while Brown et al. (2005) and others evidence the psychological burden of debt more generally. In this way, bank branching deregulation could have negative social impacts. This makes the overall impact on distress through diverse channels is an *empirical* question.⁴

As a preview of our main findings, we identify deregulation of intrastate branching rules as overall causing a 3.3% increase in state-level suicide rate and a 4.7% increase in the state-level divorce rate. To give an approximate feel for what that means, the suicide number converts to around 1515 additional suicide deaths (and 37 100 additional suicide attempts) across the country per annum at an estimated cost of USD 2.27 billion.⁵ Results are in most cases statistically significant at levels much higher than 1%, and prove resilient in a variety of robustness checks and falsification exercises.

The rest of the paper is organized as follows. Section 2 summarizes some pertinent literature on finance and psychological health, suicide and divorce. Section 3 describes our research setting - retail banking in the US and its deregulation between 1970 and the 1990s. Section 4 outlines data. Section 5 outlines empirical strategy. Section 6 reports main results and those from a number of robustness checks and falsification exercises. Section 7 concludes.

⁴Deregulation also spawned innovation and novel practices that could also have had positive or negative impacts on personal distress. For example Melzer (2011) estimate that the advent and growth of the ‘payday loan’ industry, providing very short-term consumer loans at very high annualized interest rates, exacerbated indebtedness problems causing increased delinquency on mortgage and rent payments and deferral of needed health care.

⁵For these indicative numbers we simply interact the estimated percentage change with summary national statistics and cost estimates for 2016 at afsp.org.

3.2 Literature Review

There are several strands of research that make plausible a causal link from availability of credit and the levels of suicide and divorce. While we will remain largely agnostic on causal channels it is sensible to review mechanisms consistent with the statistical results that we will present later in the paper. In this section we provide brief sketches of research that links credit availability (and debt) to psychological health, suicide and divorce.

[Fitch et al. \(2007\)](#) find that the greater availability of credit in the UK during the past two decades has resulted in more mental health problems relating to increasing debt. Expanded credit in the UK is mainly attributed to the deregulation of financial markets in the 1980s and the entry of the US lenders into the UK market in 1990s. Deregulation led to intensified competition, aggressive marketing and targeting of new customer groups, including low income individuals. They report that one in four people with mental health problems in the UK report having debt or arrears, three times the prevalence compared to individuals without similar conditions. In similar vein [Bridges and Disney \(2010\)](#) study the effect of household financial indebtedness on psychological well-being using a large household survey of families with children in Britain. The results show a strong association between being in a depressed psychological state and metrics for indebtedness and financial stress. [Gathergood \(2011\)](#) use changes in local housing prices as exogenous variation in the severity of household debt (difficulties servicing debt) examine the causal impact of problem debt on psychological health, finding the similar results on the link from indebtedness

to psychological stress.

In the medical literature, some studies suggest that debt is linked to psychological distress including depression and anxiety. [Reading and Reynolds \(2001\)](#) suggest that financial adversity has a central place in the association between socioeconomic hardship and maternal depression. They use longitudinal data, collected over six months, on 271 families with young children and find that stress about debt was the strongest independent socioeconomic predictor of the depression score in repeated interviews.

A large literature points to an association between debt problems and suicide. [Hatcher \(1994\)](#) provides a descriptive survey of financial difficulties in a consecutive series of patients who deliberately poisoned themselves in the UK. Their study suggests that the patients in debt were more likely to harm themselves with greater suicidal intent and subsequently to report more symptoms of depression and hopelessness. [Hintikka et al. \(1998\)](#) use data from Finnish general population and also find that difficulties in repaying debts were associated with suicidal ideation and suicide attempts. [Yip et al. \(2007\)](#) find that the presence of indebtedness is a risk factor that can trigger stressed individuals to contemplate suicide, using data from Hong Kong. They further attribute unmanageable debt to increasing credit availability in that jurisdiction, particularly to previously excluded groups.⁶

Evidence also show that suicide rates increase during periods of economic reces-

⁶The relationship between debt and suicide is also found in the developing world. [Merriott \(2016\)](#) reviews a broad set of literature concerning factors associated with the farmer suicide crisis in India, where an estimated 16 000 farmers commit suicide annually (far above the rate in the wider population). After reviewing the literature which identified numerous factors, the author concludes that increased indebtedness (and inability to service debt in the face of income fluctuations) played the predominant role in farmer suicides.

sion. [Frasquilho et al. \(2015\)](#) demonstrate that periods of economic recession are associated with a higher prevalence of mental health problems, including common mental disorders, substance disorders, and ultimately suicidal behaviour thorough analysis of the selected investigations. One financial stress that were identified as contributing to mental health problem and suicidal behaviour is debt from loans, mortgage/rent arrears, and debt to friends and family in 2008 Global Financial Crisis ([Gunnell et al. \(1998\)](#)). [Coope et al. \(2014\)](#) investigated trends in indicators of the 2008 economic recession and trends in suicide rates in England and Wales and find that the indicators of economic strain other than unemployment and redundancies, such as personal debt and house repossessions contribute to increased suicide rates in younger-age men.

There is a well-documented statistical association between financial stress in within families and divorce. Divorce is determined by many factors, while financial issues play a more significant role relative to other common marital disagreements. [Papp et al. \(2009\)](#) analyze diary reports of 748 marital conflict instances from 200 married people. They find that compared to non-monetary issues, marital conflicts about money were more pervasive, problematic, and recurrent, and more likely to remain unresolved. In addition, [Dew et al. \(2012\)](#) claim that financial disagreements are more salient to couples, are less easily resolved, and lead to more problematic conflicts compared to other common disagreements. Financial satisfaction is strongly linked to marital satisfaction. [Grable et al. \(2007\)](#) investigate the role financial satisfaction has on the thought of subsequent divorce and find that individuals with high levels of financial satisfaction were significantly less likely to have thought about di-

divorce during the past three years. Financial distress such as debt is likely to induce divorce. For example, consumer debt could lead to marital conflict. [Dew \(2007\)](#) finds that consumer debt directly predicts higher frequency in marital conflict. Spouses may disagree on the desirability, necessity, and practicality of assuming debt and these disagreements may strain marriages.⁷

3.3 Setting

Historically almost all US states restricted banks from expanding geographically both within and across state borders. With regards to intrastate restrictions, some states allowed bank holding companies to own separately capitalized and licensed banks throughout the state, while others were “unit banking states” in which each bank was permitted to operate only one office. Between the 1970s and mid-1990s these restrictions were removed state by state. This is widely regarded a wholesale relaxation of regulation in the sector, and drove substantial changes in the organization and operation of the sector and the supply of credit to households.

Consistent with most existing studies ([Beck et al. \(2010\)](#), [Sun and Yannelis \(2016\)](#)) we will define our date of deregulation as the year in which a state legalized intrastate branching via mergers and acquisitions through the holding company structure.⁸

⁷An alternative view, and one implying as more upbeat view of divorce, is that the availability of credit could facilitate an amicable break-up in a setting in which finances would otherwise oblige a couple to remain married. While we will treat marital breakdown as a metric for social distress we recognize the need to be more cautious in interpreting the welfare impacts of an increase in divorce rates. The necessarily negative implication of suicide is less contentious.

⁸This was the first major deregulatory step and in most cases was followed by lifting of restrictions on *de novo* branching (allowing a bank holding company to establish a new branch in a different

Our identification will exploit the fact that the timing of deregulation was staggered across states. The progress of deregulation is summarized in figure 3.1 and table 3.1. At the start of the 1970s only 12 states allowed intrastate branching, by 1996 only Arkansas, Iowa and Minnesota had not lifted restrictions. The important paper by [Jayaratne and Strahan \(1996\)](#) was the first to provide compelling evidence that dates of deregulation were not explained by state-level variations in economic conditions (actual or anticipated) and so for our analysis can plausibly be treated as exogenous.

3.4 Data

To investigate the effect of branch deregulation on suicide and divorce, we gather data on the timing of bank deregulations, suicide rates, divorce rates and other state level characteristics. We derive data from several sources. We study the twenty year period from 1976 to 1996.

Dates of deregulation were taken from [Kroszner and Strahan \(1999\)](#). Certain peculiarities of the banking sector in Delaware and South Dakota lead us to drop those two states from our main sample, consistent with most other studies in the literature ([Beck et al. \(2010\)](#), [Dick and Lehnert \(2010\)](#)). As a robustness check we will confirm that reinserting them has little impact on results.

Data on suicide are from National Center for Health Statistics (NCHS). The NCHS is the primary health statistics agency in the US and compiles and disseminates statistical information on a plethora of health issues. The suicide data we use (by geographical area). See [Sun and Yannelis \(2016\)](#) for a more thorough review.

are derived from their ongoing census of death certificates (which record causes of death). We obtain measures of suicide per capita for each state for each year in our study period. Data on completed divorces on the same basis also come from the NCHS.

To account for time-varying changes in a state-level economic conditions we will use a number of control variables, including state unemployment rate, state personal income, age composition and racial composition. Personal income is the natural log of state personal income per capita. Age composition variables measure the share of the population of a state in a series of ten-year cohort ‘bins’. Racial composition is the share of the population in the state that is black, white, or other. Age and race data are constructed from Unicon’s March CPS files, and refer to the population aged fourteen years or greater.

Table 3.2 provide summary statistics. With respect to our outcome variables of interest, the average number of suicides in our sample across the whole period is 130 per million of population per annum. The average number of divorces is 5225 per million of population per annum.

3.5 Empirical Strategy

Our source of identifying variation is the plausibly exogenous variation in timing of bank branching deregulation across states.

We use this natural variation to explore causal effects from deregulation to our chosen metrics of social distress (suicide and divorce) using state-based panel esti-

mation, including both state and time fixed effects in all regressions. To capture exposure to our treatment, deregulated banking, we use a dummy. We estimate

$$Y_{it} = \beta Deregulation_{it} + \gamma X_{it} + \alpha_i + \tau_t + \varepsilon_{it} \quad (3.1)$$

The dependent variable Y_{it} is the suicide or divorce rate in state i in year t . The binary variable $Deregulation_{it}$ takes the value 1 if intrastate branching was allowed in state i in year t . X_{it} is a vector of other regressors and includes controls for state unemployment rate, state personal income, and indicator variables capturing age composition and racial composition of population at state level. The specification also includes year fixed effects (τ_t) that control for national trends in suicide and divorce, and any time-varying factors that influence suicide and divorce at the national level, and state fixed effects (α_i) that control for any time-invariant factors that influence suicide and divorce at state level.

Standard errors are calculated using the block bootstrap method to allow for potential serial correlation. Block bootstrap maintains the autocorrelation structure by keeping all the observations that belong to the same state together. Block bootstrap methods are widely applied in the difference-in-differences literature ([Sun and Yan-nelis \(2016\)](#), [Bertrand et al. \(2004\)](#)). Using Monte Carlo simulation, [Bertrand et al. \(2004\)](#) demonstrate that block bootstrap (taking into account the autocorrelation of the data) works well when the number of states is large (50 in their case).

The specification is a generalization of the difference-in-differences approach [Bertrand et al. \(2004\)](#), [Imbens and Wooldridge \(2009\)](#). The coefficient of interest in each case is β . The difference-in-differences method is appropriate when the timing of inter-

vention is as good as random, conditional on time, and group fixed effects. Validity of our approach requires that timing of deregulation by state not be systematically related to pre-existing levels suicide or divorce. While such a relationship seems unlikely, to dispel any residual concern we follow the previous literature (Beck et al. (2010)) by generating scatter-plots from year of deregulation to each of our outcome variables. These are presented in figure 3.2 and figure 3.3. Visually there is no discernible pattern to either scatter, and the lack of correlation is confirmed by the calculations reported in the figure notes.

3.6 Results

3.6.1 Main Results: Suicide

Table 3.3 reports results with state-level suicide rate as the dependent variable.

Column (1) includes no controls except state and year fixed effects. The coefficient on the deregulation regressor is positive and statistically significant at 5% (p value = 0.023). In columns (2) through (4) we introduce, individually and together, standard proxies for state-level economic conditions (unemployment rate and personal income). Unemployment is positively associated with suicide, income negatively. However, of interest to us is that the estimated coefficient on deregulation is largely undisturbed by their inclusion.

We use annual data throughout. However the date *within* a year at which regulation is relaxed varies between states. Further, it is likely that it would take at least several months following the date of deregulation for practical consequences to be

felt. To account for this we re-estimate the specification in column (4) but excluding data relating to the year in which reform occurred and the first year calendar year after reform. The results of this exercise are reported in column (5). The coefficient remains positive and significant at 1% (p value = 0.004).

Column (5) represents our preferred specification. The suicide rate average across our sample is 130 per million implying that bank deregulation causes a 3.3% increase in the state level suicide rate.

3.6.2 Main Results: Divorce

Table 3.4 reports results from conducting the same exercises but with state-level suicide rate replaced by state-level divorce rate as the dependent variable.

In each column the estimated coefficient on the deregulation regressor is positive and significant at better than 1%. The estimates can again be seen to be comparatively insensitive to inclusion or exclusion of proxies for time-varying state-level economic conditions.

Our preferred specification is again column (5), which generates an estimated coefficient of interest of 243.6, significant at better than 1% (p value = 0.000). The average state level divorce rate in our sample is 5224 per million implying that deregulation causes a 4.7% increase in the state-level divorce rate.

3.6.3 Robustness

Table 3.3 and table 3.4 presented our preferred specifications. In addition to the preferred specifications other columns provided some evidence of the stability of our

estimated coefficients of interest to the inclusion or exclusion of a key state level controls (proxies for time-varying, state-specific economic conditions).

In this section we subject our main results to a number of other robustness challenges.

Demographics Suicide and divorce propensities have been shown to vary with age and race (Preti and Miotto (1998), Kubrin and Wadsworth (2009), Michael (1978), Bulanda and Brown (2007)). These demographic profiles vary across state and evolve through time, which could lead to changing patterns of suicide and divorce. We have no reason to think those variations would correlate with timing of bank deregulation. However to investigate any potential confounding factors we re-estimate our main specifications but now including a set of indicator variables for age and race composition by state, by year (described in data section). The results of this are reported in column (1) of each of table 3.5 and table 3.6. The estimated coefficient on the bank deregulation variable remain positive and significant at better than 1% with value little disturbed.

Outliers A legitimate question in any analysis of this sort is the role of outliers - to what extent are the results of our main regressions driven by things happening in a single or small number of states.

We address this in two ways. In Appendix table 3.7 and Appendix table 3.8 we re-estimate our preferred specification but excluding in turn each of the five most populous states (in order, California, New York, Texas, Pennsylvania and Florida). It can be seen that in few case is sign or statistical significance lost. In 9 out of the

10 exercises the coefficient value is little changed, though when considering suicide as the outcome excluding California reduces the coefficient estimate by around a quarter.

Separately we execute a 99% winsorization of data. By winsorizing the impact of outliers is reduced, without losing observations. Column (2) of table 3.5 and table 3.6 summarize the results of reestimating of our preferred specification with the adjusted data. Results are not substantially impacted (though as expected implied effect sizes are muted somewhat) allaying concerns that inference is based on outliers.

Delaware and South Dakota Following [Beck et al. \(2010\)](#), [Dick and Lehnert \(2010\)](#) and others we excluded Delaware and South Dakota from our dataset. Historically they had quite different banking policy environments (for example, tax incentivized credit card lending by banks) such that banking reform in these states had a different structure, and can be expected to have had different impacts. While these exclusions are warranted, for each outcome variable we reestimate our preferred specification but reinserting these two states into the panel. The results of this exercise are reported in column (3) of each of table 3.5 and table 3.6. The results are similar to our main results.

Divorce law Another threat to identification arises from the recognition that, in addition to deregulating banking, many states during this same period reformed divorce laws. In particular they enacted unilateral divorce legislation that allowed for either party to initiate dissolution of a marriage without seeking the approval of the other. [Stevenson and Wolfers \(2006\)](#) and [Friedberg \(1998\)](#) present evidence of

a causal effect of such reform on suicide and divorce. As such we want to address concerns that the state-level changes in suicide and divorce rates that we attribute to bank reform are not in fact a result of changes to divorce laws happening around the same period.

We do this in two ways. The first is to confirm that there is little or no correlation between the timing of bank deregulation and that of divorce law reform. This we do in Appendix figure 3.6. Visually there is no discernible pattern in the scatter-plot, and the correlation coefficient is -0.04. This makes divorce law reform unlikely to be an important confounder in our exercise. Nonetheless in column (4) of table 3.5 and table 3.6 we re-estimate our preferred specifications but now inserting a dummy variable that takes the value 1 if in state i in period t prevailing divorce law allowed for unilateral divorce. In each case our estimated coefficients on bank regulation changes only slightly but the precision of the estimate (and level of statistical significance) is improved.

Lagged unemployment Unemployment has been shown to be an important contributor to both of our outcomes of interest, with effects sometimes spilling over between years ([Beck et al. \(2010\)](#)). In Appendix table 3.9 and Appendix table 3.10 we add three lags of the unemployment rate, both individually and as a suite, to our main specification. Again, our main results are not significantly disturbed - the implied effect sizes are if anything somewhat larger.

3.6.4 Falsification: Evidence from 6000 Placebos

While our interest is the uncovering of a causal effect (should one exist) of bank deregulation on suicide or divorce, the variation that we exploit is with respect to date of treatment.

Inclusion of year fixed effects controls for any time-varying, across-state factors that might have been influencing suicide or divorce rates during our study period. This includes factors particular to a single year as well as longer-term trends or cycles. We have also verified that timing of deregulation was not systematically related to pre-deregulatory levels of our outcome variables.

To further challenge our identification, in this section we report the results of running a large number of placebo tests. In each of these we falsely assign other, counterfactual treatment dates.

Placebo exercises are a test of design ([Chetty et al. \(2016\)](#), [Card et al. \(2012\)](#)). If when we work with a vector of state-level deregulation dates that we know to be false the regression exercise generates a significant coefficient, then we know that ‘result’ to be spurious. Of course in any given such exercise such significance could be generated by chance. To endorse our design we want to be convinced that such spurious outcomes do not occur too often.⁹

With this in mind, for each of three different ways of randomly-assigning false treatment dates to states, for each outcome variable, we run 1000 placebo regressions and from each regression harvest the estimated coefficient on the deregulation

⁹Of the papers that we have cited so far on the effects of deregulation none presents the results of any placebo exercises.

regressor and associated t values. In other words we end up with 6000 $\hat{\beta}$'s and 6000 t values. From this we can assess how likely it is that the values from our preferred specifications (with correctly assigned treatment dates) would have been generated spuriously.

Placebo 1 (Preferred) In the first exercise we construct a list of dates at which a state deregulated, allowing repetition. So for example the year 1988 appears on the list four times, because four states (Illinois, Louisiana, Oklahoma and Texas) deregulated in that year, while 1979 (Ohio) appears only once. To construct a placebo vector of deregulation dates, for each state we draw at random a date from that list. If the date happens to coincide with the true date for a particular state it is discarded and another date is drawn. We do this repeatedly to construct 1000 different placebo vectors. For each placebo vector we reestimate our preferred specifications (corresponding to column (5) in table 3.3 and table 3.4).

Placebo 2 In the second exercise, to construct a placebo vector of deregulation dates for each state we draw a year at random from the dates 1960 to 1999 (the whole period of the bank deregulation wave). If the date happens to coincide with the true date for a particular state it is discarded and another date is drawn. We do this repeatedly to construct 1000 different placebo vectors then proceed as above.

Placebo 3 In placebo 3 we proceed as in placebo 1, but not allowing repetition. In other words any particular date can appear at most once on the list. To construct a placebo vector of deregulation dates, for each state we draw at random a date from

that list. If the date happens to coincide with the true date for a particular state it is discarded and another date is drawn. We do this repeatedly to construct 1000 different placebo vectors then proceed as above.

Falsification Results

The results of these exercises for suicide as outcome variable are presented in figure 3.4.

The barchart in the left-hand of the top panel plots the 1000 coefficients derived from placebo exercise 1. The right-hand panel plot the T values. The vertical line in each case denotes the coefficient from the true assignment. Only 12/1000 (or 1.2%) placebo runs deliver a coefficient estimate bigger than the 4.28 under true assignment, and 14/1000 (1.4%) deliver a bigger T value.

The middle and bottom panels present equivalent barcharts corresponding to placebos 2 and 3 respectively. In each case it can be seen that the coefficients and T values from the regressions under true assignment are far in the right-hand tail of the distributions. The numbers of cases are summarized in the table underneath. In the least favorable case (here placebo 2) the coefficient value and t value under true assignment lie above 97.0% and 97.1% of those from the placebo regressions.

Figure 3.5 presents the results of the same falsification exercises but with divorce as the outcome variable. Again the results are similar. In the least favorable case (placebo 2) the coefficient value and t value under true assignment lie above 97.6% and 97.8% of those from the placebo regressions.

Taken together we believe that these results make it unlikely that the pattern

characterized in our main tables are spurious or underpinned by shortcomings with design.

3.7 Conclusion

Banks play a vital role in supporting economic and social activity by extending credit and other services to individuals and businesses. The relaxation of branch banking restrictions in most American states between the 1970s and 1990s led to increased competition between banks ([Jayaratne and Strahan \(1998\)](#)) and lower prices and higher quantities of credit. As a result individuals borrow more to buy homes ([Tewari \(2014\)](#)), start businesses ([Black and Strahan \(2002\)](#)) and pay for education ([Sun and Yannelis \(2016\)](#)). Total private loan volume increases, bank fees decrease, and mortgage loan interest rates decrease after branching deregulation takes effect ([Sun and Yannelis \(2016\)](#)).

However as gatekeepers to credit, banks are also central participants in the creation of debt. What are the implications of loosening regulation of banks on social outcomes? [Garmaise and Moskowitz \(2006\)](#) found a significant negative effect on property crime. [Dick and Lehnert \(2010\)](#) found a significant positive effect on personal bankruptcy. The important impacts that access to credit can have on individual and familial distress motivated us to investigate a possible causal influence of deregulation on patterns of suicide and divorce. Since these impacts can be positive or negative, the overall impact is an empirical question.

The analysis reported here points to a significant and robust causal impact of

deregulation of branching rules on state-level measures of suicide and divorce rates. The effect size is substantial. In our preferred specification it causes a 3.3% increase in state-level suicide rate and a 4.7% increase in the state-level divorce rate. Results are in most cases statistically significant at levels much higher than 1%, and prove resilient in a variety of robustness checks and falsification exercises.

These empirical findings are consistent with the relaxation credit supply being the primary mechanism through which this financial deregulation affected suicide and divorce. We did not provide direct test on the credit availability as the mechanism, but the banking deregulation literature suggest that banks in general allowed previously excluded households to enter the market and increased overall personal borrowing, including credit card loans ([Dick and Lehnert \(2010\)](#), [Sun and Yannelis \(2016\)](#))).

The analysis adds further nuance to the intricate inter-relationship between retail banking sector and social lives, and that care is needed in thinking about how the provision of credit should be managed and regulated.

Table 3.1: Year Intrastate Bank Branching First Allowed

State	Year of Deregulation	State	Year of Deregulation
Alabama	1981	Monatana	1990
Alaska	1960	Nebraska	1985
Arizona	1960	Nevada	1960
Arkansas	1994	New Hampshire	1987
California	1960	New Jersey	1977
Colorado	1991	New Mexico	1991
Connecticut	1980	New York	1976
Delaware	1960	North Carolina	1960
District of Columbia	1960	North Dakota	1987
Florida	1988	Ohio	1979
Georgia	1983	Oklahoma	1988
Hawaii	1986	Oregon	1985
Idaho	1960	Pennsylvania	1982
Illinois	1988	Rhode Island	1960
Indiana	1989	South Carolina	1960
Iowa	1999	South Dakota	1960
Kansas	1987	Tennessee	1985
Kentucky	1990	Texas	1988
Louisiana	1988	Utah	1981
Maine	1975	Vermont	1970
Maryland	1960	Virginia	1978
Massachusetts	1984	Washington	1985
Michigan	1987	West Virginia	1987
Minnesota	1993	Wisconsin	1990
Mississippi	1986	Wyoming	1988
Missouri	1990		

Table 3.2: Summary Statistics

	Mean (1)	Std. Deviation (2)	Min (3)	Max (4)
Suicide Rate (per 1,000,000)	130.229	33.617	51.245	288.133
Divorce Rate (per 1,000,000)	5224.848	1813.538	2037.666	17600
Unemployment Rate	0.07206	0.02337	0.02141	0.18771
Personal Income	9.5339	0.4081	8.4674	10.3974
Share 14-19	0.1226	0.0220	0.0645	0.1926
Share 20-29	0.2111	0.02944	0.1303	0.3370
Share 30-39	0.1995	0.02717	0.1298	0.2910
Share 40-49	0.1493	0.0237	0.0935	0.2298
Share 50-59	0.1204	0.0138	0.0758	0.1647
Share 60-69	0.1035	0.0150	0.0318	0.1722
Share 70-79	0.0662	0.0142	0.0126	0.1140
Share 80-89	0.0244	0.0071	0.0005	0.0497
Share 90plus	0.0031	0.0017	0.0000	0.0096
White Share	0.8620	0.1414	0.2323	0.9994
Black Share	0.1005	0.1199	0.0000	0.7399
Other Share	0.0374	0.0953	0.0000	0.7567

Note: Suicide and divorce rate is per 1,000,000. Personal income is the natural log of state personal income per capita. Age composition variables indicate the share of states' populations aged 14 to 19, and then ten-year cohorts beginning with age 20 up to a variable for 90+. Racial composition refers to the share of the state's population that is Black, White, and other. Age and race data are constructed from Unicon's March CPS files, and refer to the population aged fourteen years or greater.

Table 3.3: Main Result: Suicide

	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)
Bank Deregulation	2.972** (1.303)	3.308** (1.314)	3.015** (1.320)	3.227** (1.451)	4.281*** (1.502)
Unemployment Rate		74.831*** (25.582)		48.523 (31.455)	55.478** (27.053)
Personal Income			-32.879** (15.290)	-28.171* (16.347)	-32.815* (18.250)
State FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Observations	1,029	1,029	1,029	1,029	956
R-squared	0.8772	0.8782	0.8791	0.8795	0.8804

Note: Dependent variable is state suicide rate per annum per million of population. Bank deregulation equals one during all years in which a state permits intrastate branching and equals zero otherwise. All models control for state and year fixed effects. Standard errors calculated using block bootstrap method in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Main Result: Divorce

	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)
Bank Deregulation	202.426*** (68.979)	197.831*** (73.329)	204.361*** (61.691)	209.719*** (64.962)	243.607*** (67.251)
Unemployment Rate		-1507.890 (1049.693)		1728.659* (1035.523)	2224.124* (1159.11)
Personal Income			3415.894*** (417.696)	3576.127*** (434.733)	3727.999*** (406.790)
State FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Observations	986	986	986	986	915
R-squared	0.9197	0.9199	0.9269	0.9271	0.9281

Note: Dependent variable is state divorce rate per annum per million of population. Bank deregulation equals one during all years in which a state permits intrastate branching and equals zero otherwise. All models control for state and year fixed effects. Standard errors calculated using block bootstrap method in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 3.5: Robustness Check: Suicide

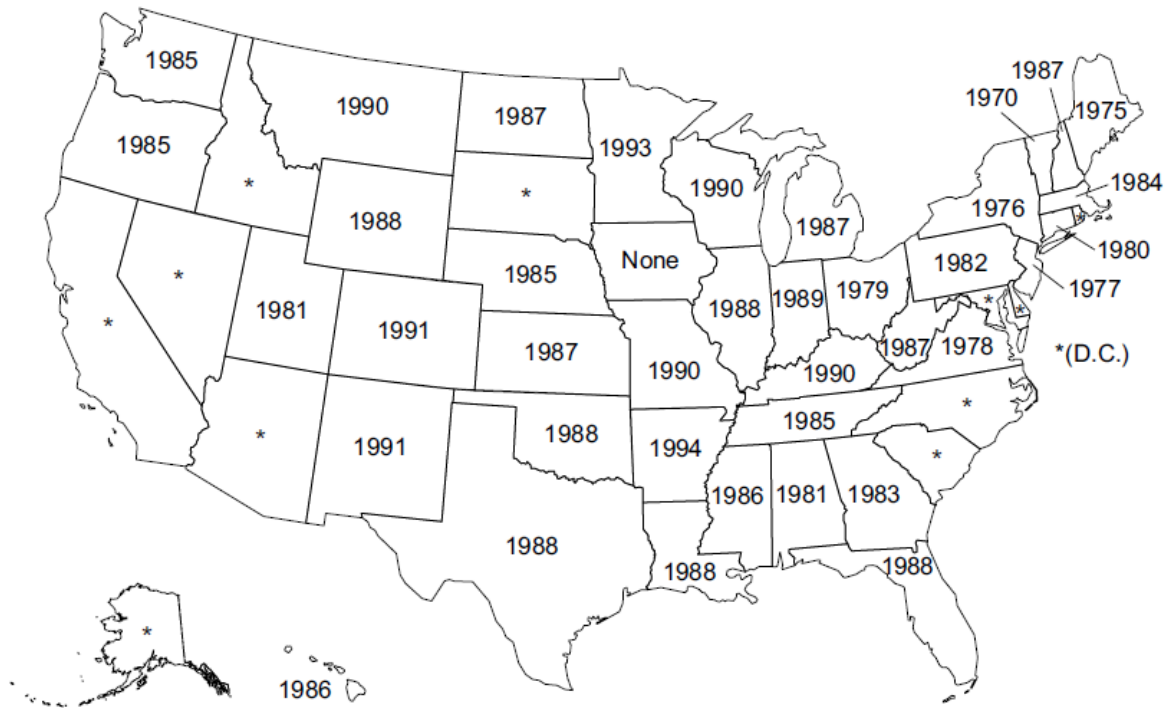
	Age and Race Profile	Data Winsorization	Include DE and SD	Control Divorce Law
	(1)	(2)	(3)	(4)
Bank Deregulation	4.020*** (1.557)	3.718** (1.695)	3.336** (1.687)	4.387*** (1.583)
Unemployment Rate	61.244* (32.704)	50.914 (31.270)	46.940 (30.227)	54.302 (34.340)
Personal Income	-29.239 (17.840)	-31.867* (17.207)	-27.918** (13.509)	-33.147* (17.514)
Divorce Law				-3.882 (4.695)
Age & Race Controls	✓			
State FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Observations	956	956	1022	956
R-squared	0.8828	0.8817	0.8700	0.8806

Note: Dependent variable is state suicide rate per annum per million of population. Bank deregulation equals one during all years in which a state permits intrastate branching and equals zero otherwise. All models control for state and year fixed effects. Standard errors calculated using block bootstrap method in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Robustness Check: Divorce

	Age and Race Profile (1)	Data Winsorization (2)	Include DE and SD (3)	Control Divorce Law (4)
Bank Deregulation	240.773*** (73.418)	198.229*** (67.296)	195.283*** (70.227)	237.642*** (78.865)
Unemployment Rate	1709.481 (1218.048)	2368.769** (1022.257)	1724.374 (1112.672)	2293.591* (1022.114)
Personal Income	3502.793*** (415.632)	3511.861*** (411.321)	3328.583*** (472.776)	3747.979*** (429.582)
Divorce Law				215.817** (103.153)
Age & Race Controls	✓			
State FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Observations	915	915	981	915
R-squared	0.9298	0.9356	0.9292	0.9283

Note: Dependent variable is state divorce rate per annum per million of population. Bank deregulation equals one during all years in which a state permits intrastate branching and equals zero otherwise. All models control for state and year fixed effects. Standard errors calculated using block bootstrap method in parentheses *** p<0.01, ** p<0.05, * p<0.1



* means permitted intrastate branching before 1970
Source: Kroszner and Strahan (1999).

Figure 3.1: Timing of intrastate branching deregulation

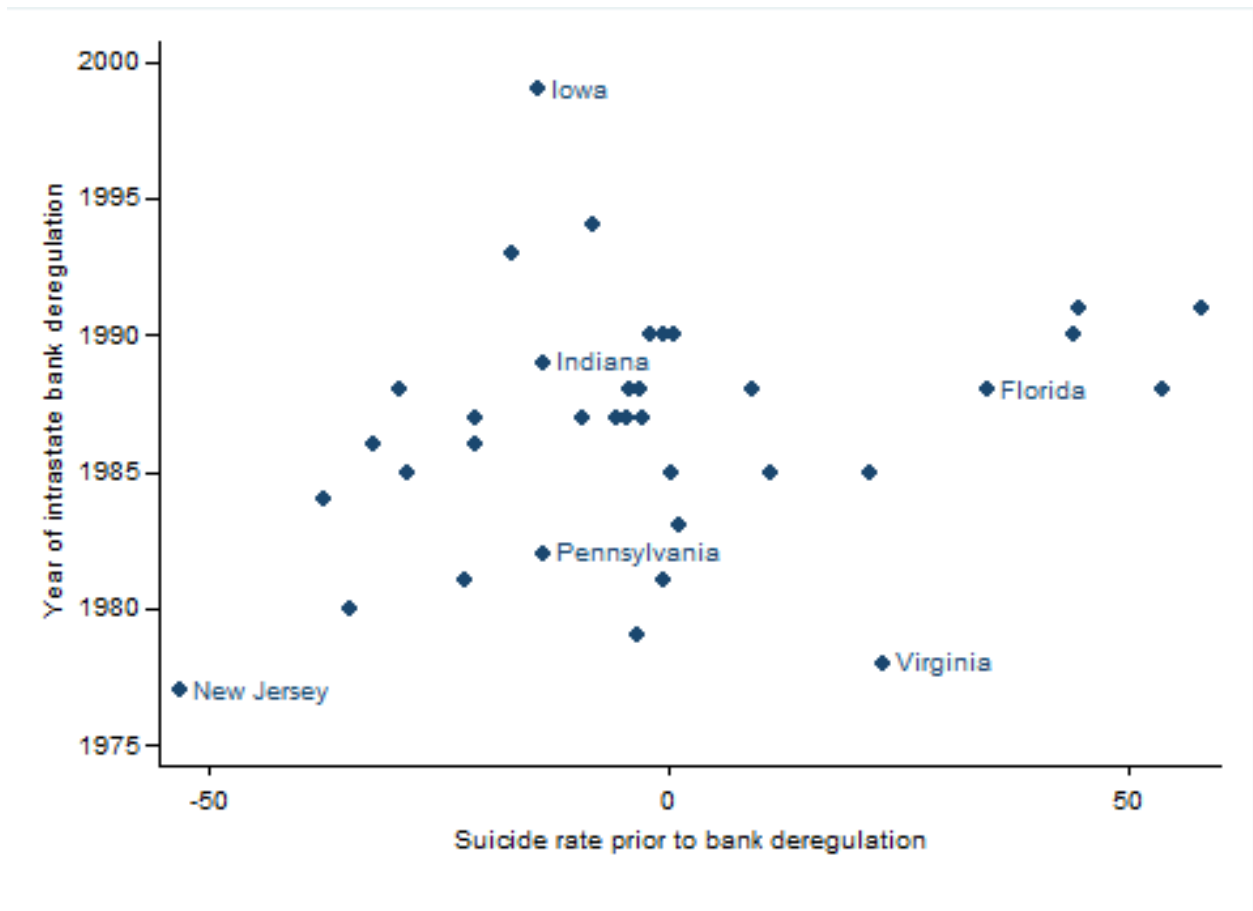


Figure shows a scatter plot of the average suicide rate prior to bank deregulation and the year of bank deregulation. The t-statistics for the correlation is 1.08.

Figure 3.2: Timing of bank deregulation and pre-existing suicide rate

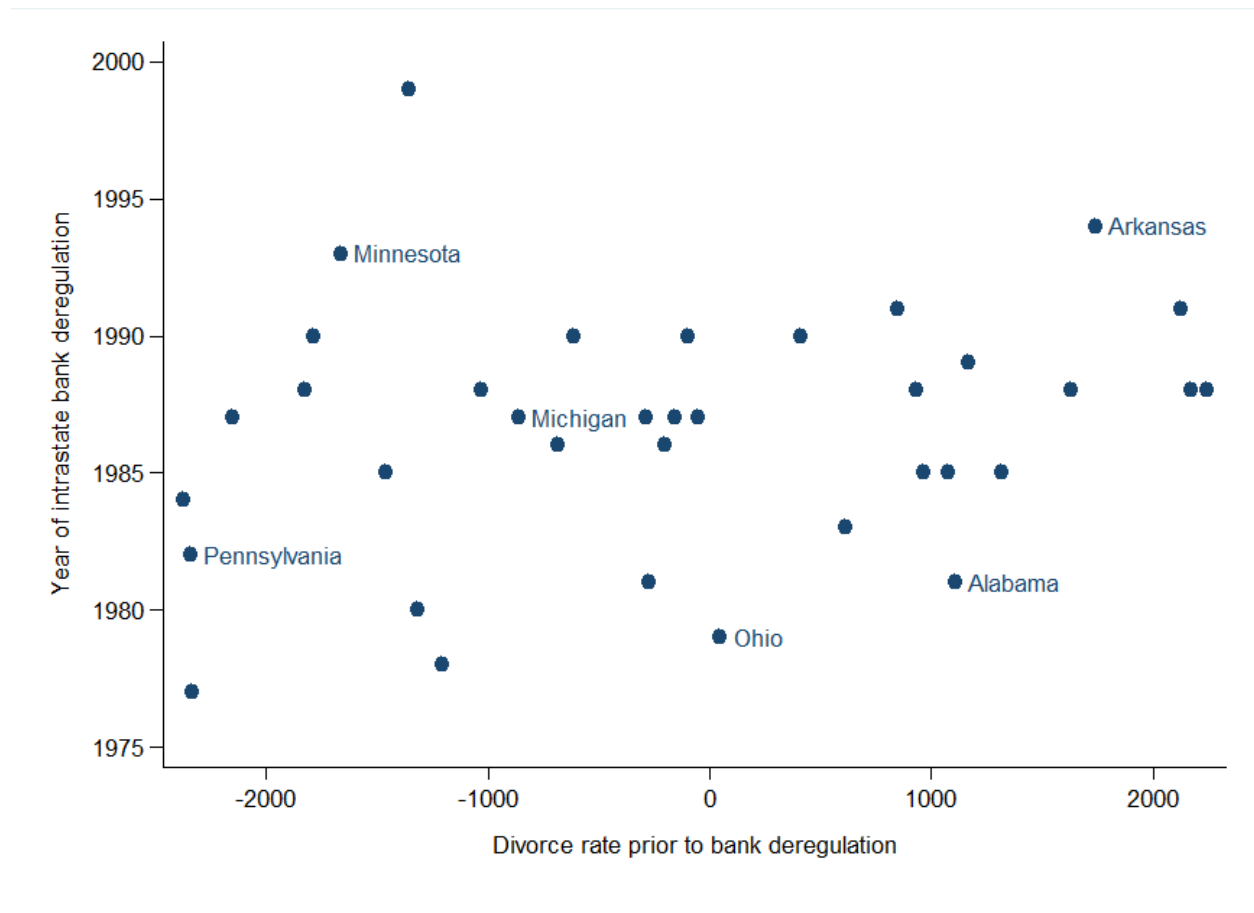
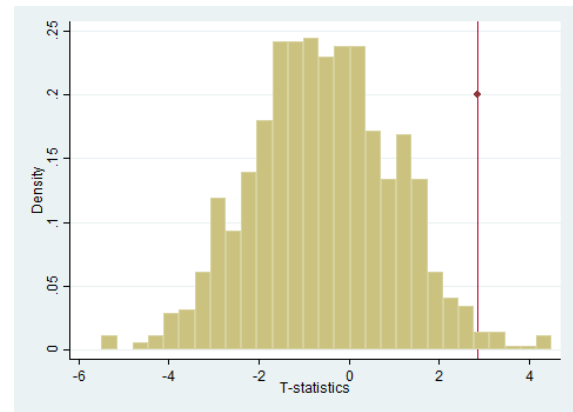
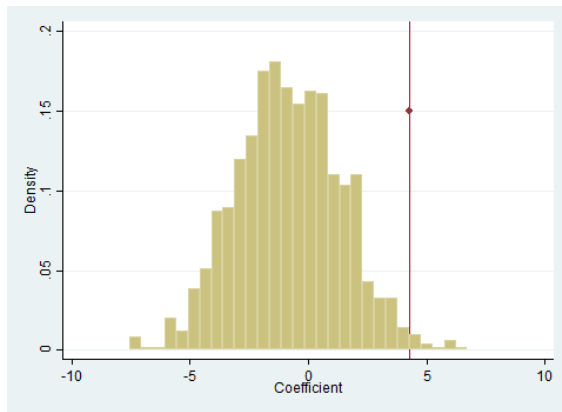
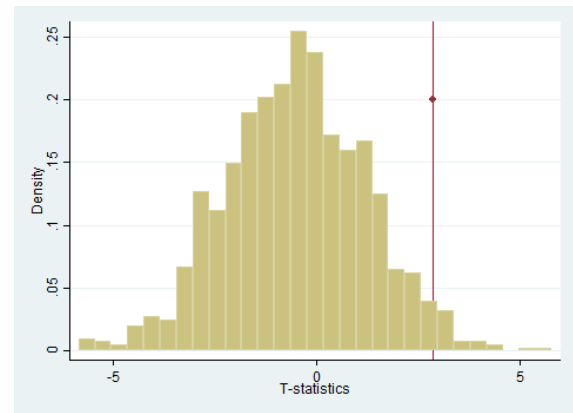
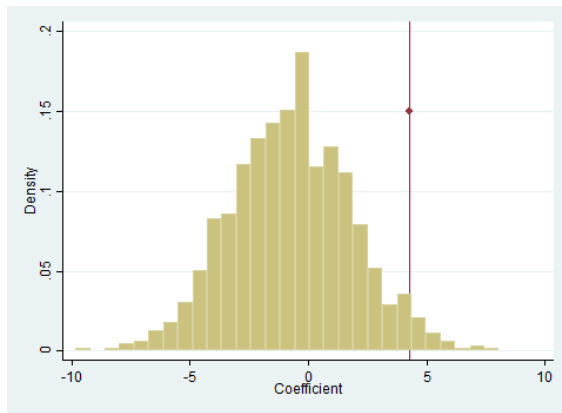


Figure shows a scatter plot of the average divorce rate prior to bank deregulation and the year of bank deregulation. The t-statistics for the correlation is 1.33.

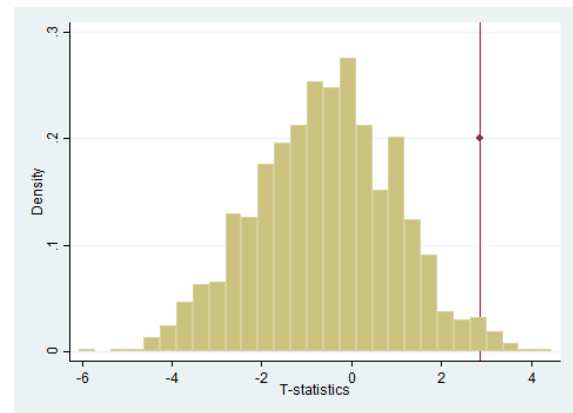
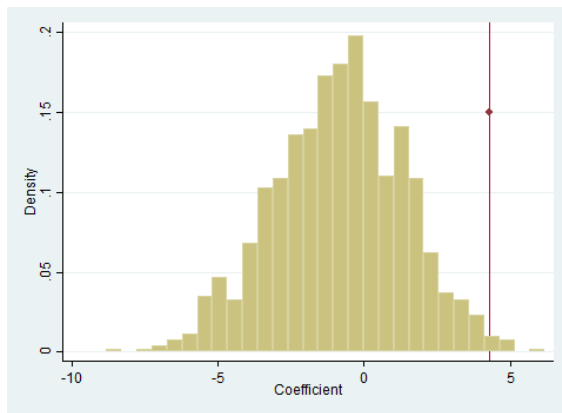
Figure 3.3: Timing of bank deregulation and pre-existing divorce rate



Suicide Placebo 1



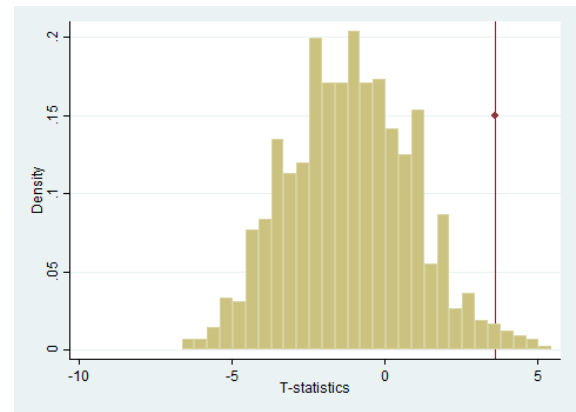
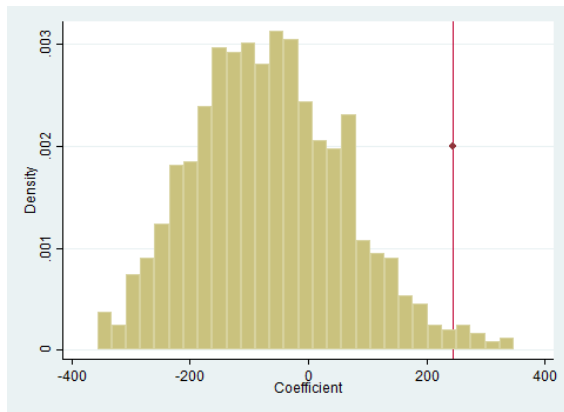
Suicide Placebo 2



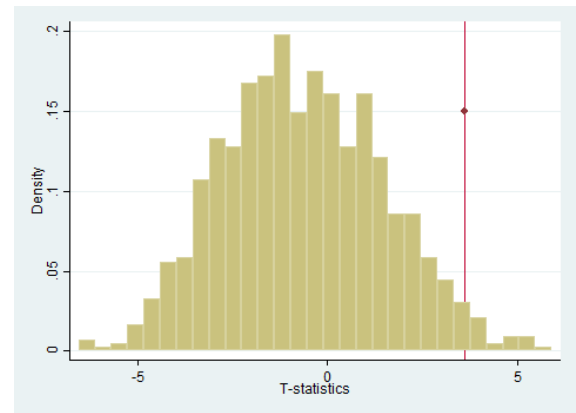
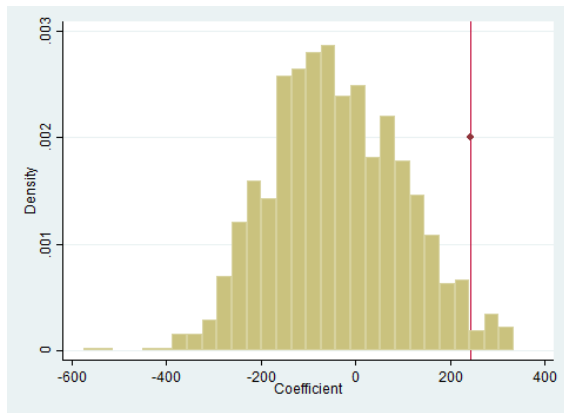
Suicide Placebo 3

	Placebo 1	Placebo 2	Placebo 3
Greater than "True" T-value	14/1000=1.4%	29/1000=2.9%	15/1000=1.5%
Greater than "True" Coefficient	12/1000=1.2%	30/1000=3%	8/1000=0.8%
Greater than 1% T-value	25/1000=2.5%	39/1000=3.9%	25/1000=2.5%

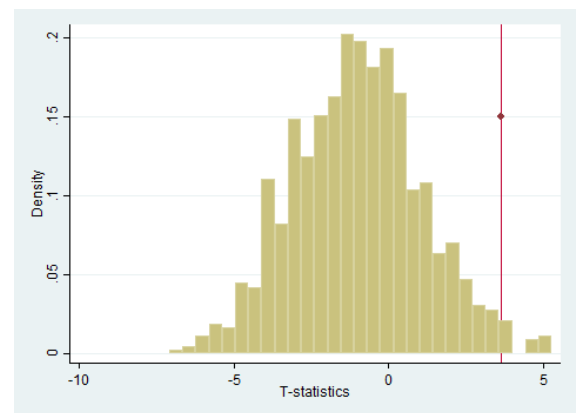
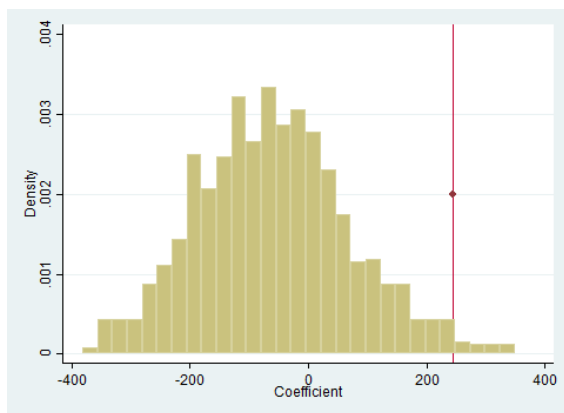
Figure 3.4: Suicide placebo tests



Divorce Placebo 1



Divorce Placebo 2



Divorce Placebo 3

	Placebo 1	Placebo 2	Placebo 3
Greater than "True" T-value	16/1000=1.6%	22/1000=2.2%	17/1000=1.7%
Greater than "True" Coefficient	15/1000=1.5%	24/1000=2.4%	16/1000=1.6%
Greater than 1% T-value	41/1000=4.1%	71/1000=7.1%	49/1000=4.9%

Figure 3.5: Divorce placebo tests

3.8 Appendix

Table 3.7: Exclude Large States - Suicide

State Excluded	California (1)	New York (2)	Texas (3)	Pennsylvania (4)	Florida (5)
Bank Deregulation	2.722* (1.613)	4.134*** (1.591)	4.488*** (1.506)	4.251*** (1.552)	4.634*** (1.714)
Unemployment Rate	52.582* (31.520)	49.344* (29.008)	57.752** (27.584)	56.342* (32.838)	55.903* (30.698)
Personal Income	-39.543** (17.928)	-31.494* (16.257)	-34.071** (14.797)	-32.819** (17.259)	-32.270* (17.441)
State FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Observations	935	937	937	937	937
R-squared	0.8860	0.8767	0.8810	0.8803	0.8793

Note: Dependent variable is state suicide rate per annum per million of population. Bank deregulation equals one during all years in which a state permits intrastate branching and equals zero otherwise. All models control for state and year fixed effects. Standard errors calculated using block bootstrap method in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 3.8: Exclude Large States - Divorce

State Excluded	California (1)	New York (2)	Texas (3)	Pennsylvania (4)	Florida (5)
Bank Deregulation	232.625*** (82.621)	248.586*** (84.943)	249.704*** (77.808)	228.430*** (77.167)	264.036*** (84.595)
Unemployment Rate	2022.255** (1023.878)	2295.691** (1039.677)	2309.253** (1066.435)	2302.403** (1161.238)	2196.845* (1143.565)
Personal Income	3706.191*** (439.886)	3698.401*** (461.012)	3703.733*** (391.169)	3714.878*** (461.600)	3777.605*** (455.718)
State FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Observations	900	896	897	896	896
R-squared	0.9286	0.9271	0.9279	0.9270	0.9280

Note: Dependent variable is state divorce rate per annum per million of population. Bank deregulation equals one during all years in which a state permits intrastate branching and equals zero otherwise. All models control for state and year fixed effects. Standard errors calculated using block bootstrap method in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 3.9: Inclusion of Lagged Unemployment Rate - Suicide

	Model (1)	Model (2)	Model (3)	Model (4)
Bank Deregulation	4.281*** (1.502)	5.330*** (1.704)	4.901*** (1.907)	5.988*** (2.001)
Personal Income	-32.815* (18.250)	-46.952*** (17.463)	-63.598*** (16.159)	-63.365*** (18.531)
Unemployment Rate	55.478** (27.053)	49.425 (35.052)	15.775 (38.536)	10.967 (41.980)
Unemployment Rate _{t-1}		-38.527 (41.337)	-51.123 (49.487)	-67.365 (48.256)
Unemployment Rate _{t-2}			2.601 (32.009)	-36.256 (42.560)
Unemployment Rate _{t-3}				47.631 (45.036)
State FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Observations	956	872	790	710
R-squared	0.8804	0.8870	0.8943	0.8977

Note: Dependent variable is state suicide rate per annum per million of population. Bank deregulation equals one during all years in which a state permits intrastate branching and equals zero otherwise. All models control for state and year fixed effects. Standard errors calculated using block bootstrap method in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 3.10: Inclusion of Lagged Unemployment Rate - Divorce

	Model (1)	Model (2)	Model (3)	Model (4)
Bank Deregulation	243.607*** (67.251)	277.279*** (82.002)	290.076*** (104.133)	312.203*** (111.877)
Personal Income	3727.999*** (406.790)	4028.594*** (503.095)	4122.352*** (539.112)	4187.31*** (596.269)
Unemployment Rate	2224.124* (1159.11)	-48.755 (1515.292)	-973.148 (1702.879)	-622.825 (1656.402)
Unemployment Rate _{t-1}		3745.563** (1647.981)	1998.426 (2350.715)	1225.535 (2105.452)
Unemployment Rate _{t-2}			3503.71** (1598.212)	2268.359 (2225.285)
Unemployment Rate _{t-3}				2457.054 (1768.145)
State FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Observations	915	833	755	678
R-squared	0.9281	0.9293	0.9293	0.9286

Note: Dependent variable is state divorce rate per annum per million of population. Bank deregulation equals one during all years in which a state permits intrastate branching and equals zero otherwise. All models control for state and year fixed effects. Standard errors calculated using block bootstrap method in parentheses *** p<0.01, ** p<0.05, * p<0.1

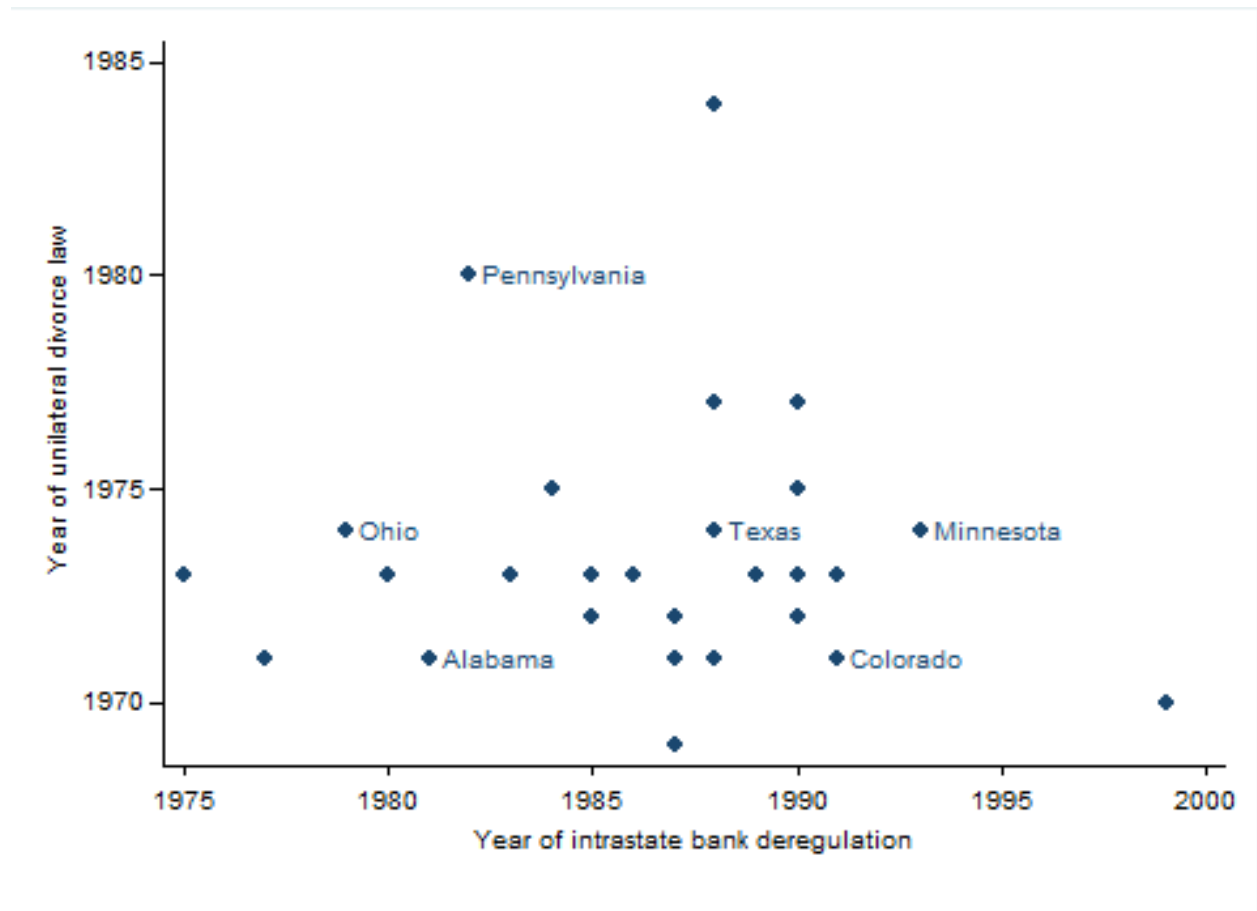


Figure shows a scatter plot of the year of bank deregulation and the year of unilateral divorce law. The t-statistics for the correlation is 1.33.

Figure 3.6: Timing of bank deregulation and unilateral divorce law

Bibliography

Abiad, A. and Mody, A. (2005). Financial reform: What shakes it? what shapes it? *The American Economic Review*, 95(1):66–88.

Acemoglu, D., Johnson, S., and Robinson, J. A. (2001). The colonial origins of comparative development: An empirical investigation. *American economic review*, 91(5):1369–1401.

Acemoglu, D., Robinson, J., et al. (2008). The role of institutions in growth and development. *World Bank, Washington DC*.

Acemoglu, D. and Robinson, J. A. (2008). Persistence of power, elites, and institutions. *American Economic Review*, 98(1):267–93.

Alesina, A., Ardagna, S., and Trebbi, F. (2006). Who adjusts and when? on the political economy of reforms. Technical report, National Bureau of Economic Research.

Alesina, A. F., Troiano, U., and Cassidy, T. (2015). Old and young politicians. Technical report, National Bureau of Economic Research.

- Andersen, J. J. and Aslaksen, S. (2013). Oil and political survival. *Journal of Development Economics*, 100(1):89–106.
- Angrist, J. D. and Pischke, J.-S. (2008). *Mostly harmless econometrics: An empiricist's companion*. Princeton university press.
- Arellano, M. and Bond, S. (1991). Some tests of specification for panel data: Monte carlo evidence and an application to employment equations. *The review of economic studies*, 58(2):277–297.
- Bazzi, S. and Blattman, C. (2014). Economic shocks and conflict: Evidence from commodity prices. *American Economic Journal: Macroeconomics*, 6(4):1–38.
- Beck, T. and Levine, R. (2004). Stock markets, banks, and growth: Panel evidence. *Journal of Banking & Finance*, 28(3):423–442.
- Beck, T., Levine, R., and Levkov, A. (2010). Big bad banks? the winners and losers from bank deregulation in the united states. *The Journal of Finance*, 65(5):1637–1667.
- Beland, L.-P. (2015). Political parties and labor market outcomes: Evidence from US states. *American Economic Journal: Applied Economics*, 7(4):198–220.
- Bertrand, M., Duflo, E., and Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics*, 119(1):249–275.

- Besley, T. and Case, A. (1995). Does electoral accountability affect economic policy choices? evidence from gubernatorial term limits. *The Quarterly Journal of Economics*, 110(3):769–798.
- Black, S. E. and Strahan, P. E. (2002). Entrepreneurship and bank credit availability. *The Journal of Finance*, 57(6):2807–2833.
- Blinder, A. S. and Watson, M. W. (2016). Presidents and the us economy: An econometric exploration. *The American Economic Review*, 106(4):1015–1045.
- Bond, P., Musto, D. K., and Yilmaz, B. (2009). Predatory mortgage lending. *Journal of Financial Economics*, 94(3):412–427.
- Bonfiglioli, A. and Gancia, G. A. (2015). Economic uncertainty and structural reforms.
- Bridges, S. and Disney, R. (2010). Debt and depression. *Journal of Health Economics*, 29(3):388–403.
- Brodeur, A., Lé, M., Sangnier, M., and Zylberberg, Y. (2016). Star wars: The empirics strike back. *American Economic Journal: Applied Economics*, 8(1):1–32.
- Brown, S., Taylor, K., and Price, S. W. (2005). Debt and distress: Evaluating the psychological cost of credit. *Journal of Economic Psychology*, 26(5):642–663.
- Brückner, M. and Ciccone, A. (2010). International commodity prices, growth and the outbreak of civil war in sub-saharan africa. *The Economic Journal*, 120(544):519–534.

- Brückner, M., Ciccone, A., and Tesei, A. (2012). Oil price shocks, income, and democracy. *Review of Economics and Statistics*, 94(2):389–399.
- Bulanda, J. R. and Brown, S. L. (2007). Race-ethnic differences in marital quality and divorce. *Social Science Research*, 36(3):945–967.
- Burke, P. J. and Leigh, A. (2010). Do output contractions trigger democratic change? *American Economic Journal: Macroeconomics*, 2(4):124–157.
- Calderon, C. A., Chong, A., and Loayza, N. V. (2002). Determinants of current account deficits in developing countries. *Contributions in Macroeconomics*, 2(1).
- Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6):2295–2326.
- Calonico, S., Cattaneo, M. D., and Titiunik, R. (2015). rdrobust: An r package for robust nonparametric inference in regression-discontinuity designs. *R Journal*, 7(1):38–51.
- Card, D., Mas, A., Moretti, E., and Saez, E. (2012). Inequality at work: The effect of peer salaries on job satisfaction. *American Economic Review*, 102(6):2981–3003.
- Caselli, F. and Tesei, A. (2016). Resource windfalls, political regimes, and political stability. *Review of Economics and Statistics*, 98(3):573–590.
- Cattaneo, M. D., Jansson, M., and Ma, X. (2015). Simple local regression distribution estimators with an application to manipulation testing. Technical report, working paper, University of Michigan.

- Caughey, D., Warshaw, C., and Xu, Y. (2016). Incremental democracy: The policy effects of the partisan composition of state government. *Journal of Politics* (forthcoming).
- Chetty, R., Hendren, N., and Katz, L. F. (2016). The effects of exposure to better neighborhoods on children: New evidence from the moving to opportunity experiment. *American Economic Review*, 106(4):855–902.
- Coope, C., Gunnell, D., Hollingworth, W., Hawton, K., Kapur, N., Fearn, V., Wells, C., and Metcalfe, C. (2014). Suicide and the 2008 economic recession: who is most at risk? trends in suicide rates in england and wales 2001–2011. *Social Science & Medicine*, 117:76–85.
- de Benedictis-Kessner, J. and Warshaw, C. (2016). Mayoral partisanship and municipal fiscal policy. *The Journal of Politics*, 78(4):1124–1138.
- de la Cuesta, B. and Imai, K. (2016). Misunderstandings about the regression discontinuity design in the study of close elections. *Annual Review of Political Science*, 19:375–396.
- Deaton, A. (1999). Commodity prices and growth in africa. *The Journal of Economic Perspectives*, 13(3):23–40.
- Dew, J. (2007). Two sides of the same coin? the differing roles of assets and consumer debt in marriage. *Journal of Family and Economic Issues*, 28(1):89–104.
- Dew, J., Britt, S., and Huston, S. (2012). Examining the relationship between financial issues and divorce. *Family Relations*, 61(4):615–628.

- Dick, A. A. and Lehnert, A. (2010). Personal bankruptcy and credit market competition. *The Journal of Finance*, 65(2):655–686.
- Drazen, A. and Easterly, W. (2001). Do crises induce reform? simple empirical tests of conventional wisdom. *Economics & Politics*, 13(2):129–157.
- Dunning, J. H. (1998). Location and the multinational enterprise: a neglected factor? *Journal of international business studies*, 29(1):45–66.
- Eggers, A. C., Fowler, A., Hainmueller, J., Hall, A. B., and Snyder, J. M. (2015). On the validity of the regression discontinuity design for estimating electoral effects: New evidence from over 40,000 close races. *American Journal of Political Science*, 59(1):259–274.
- Eggers, A. C. and Hainmueller, J. (2009). Mps for sale? returns to office in postwar british politics. *American Political Science Review*, 103(04):513–533.
- Erikson, R. S., Folke, O., and Snyder Jr, J. M. (2015). A gubernatorial helping hand? how governors affect presidential elections. *The Journal of Politics*, 77(2):491–504.
- Faccio, M. (2006). Politically connected firms. *American economic review*, 96(1):369–386.
- Fan, J. and Gijbels, I. (1996). *Local polynomial modelling and its applications: monographs on statistics and applied probability 66*, volume 66. CRC Press.
- Fernandez, R. and Rodrik, D. (1991). Resistance to reform: Status quo bias in the presence of individual-specific uncertainty. *The American Economic Review*, pages 1146–1155.

- Ferreira, F., Gyourko, J., et al. (2009). Do political parties matter? evidence from us cities. *The Quarterly Journal of Economics*, 124(1):399–422.
- Fitch, C., Chaplin, R., Trend, C., and Collard, S. (2007). Debt and mental health: the role of psychiatrists. *Advances in Psychiatric Treatment*, 13(3):194–202.
- Fox, G. L., Benson, M. L., DeMaris, A. A., and Wyk, J. (2002). Economic distress and intimate violence: Testing family stress and resources theories. *Journal of Marriage and Family*, 64(3):793–807.
- Fox, S. E. (1996). The influence of political conditions on foreign firm location decisions in the american states (1974-1989). *Political Research Quarterly*, 49(1):51–75.
- Frasquilho, D., Matos, M. G., Salonna, F., Guerreiro, D., Storti, C. C., Gaspar, T., and Caldas-de Almeida, J. M. (2015). Mental health outcomes in times of economic recession: a systematic literature review. *BMC public health*, 16(1):115.
- Friedberg, L. (1998). Did unilateral divorce raise divorce rates? evidence from panel data. *The American Economic Review*, 88(3):608–627.
- Fry, E. H. (1998). *The expanding role of state and local governments in US foreign affairs*. New York (USA) Council on Foreign Relations Press.
- Garmaise, M. J. and Moskowitz, T. J. (2006). Bank mergers and crime: The real and social effects of credit market competition. *The Journal of Finance*, 61(2):495–538.
- Gathergood, J. (2011). Debt and depression: Evidence on causal links and social stigma effects.

- Gathergood, J. (2012). Self-control, financial literacy and consumer over-indebtedness. *Journal of Economic Psychology*, 33(3):590–602.
- Gelman, A. and Imbens, G. (2014). Why high-order polynomials should not be used in regression discontinuity designs. Technical report, National Bureau of Economic Research.
- Gerber, E. R. and Hopkins, D. J. (2011). When mayors matter: estimating the impact of mayoral partisanship on city policy. *American Journal of Political Science*, 55(2):326–339.
- Giuliano, P., Mishra, P., and Spilimbergo, A. (2013). Democracy and reforms: evidence from a new dataset. *American Economic Journal: Macroeconomics*, 5(4):179–204.
- Grable, J. E., Britt, S., and Cantrell, J. (2007). An exploratory study of the role financial satisfaction has on the thought of subsequent divorce. *Family and Consumer Sciences Research Journal*, 36(2):130–150.
- Grimmer, J., Hersh, E., Feinstein, B., and Carpenter, D. (2011). Are close elections random? *Unpublished manuscript*.
- Grossman, G. M. and Helpman, E. (2001). *Special interest politics*. MIT press.
- Gunnell, D., Donovan, J., Barnes, M., Davies, R., Hawton, K., Kapur, N., Hollingworth, W., and Metcalfe, C. (1998). The 2008 global financial crisis: effects on mental health and suicide. *Age*, 2003(2008):2013.

- Halvorsen, T. and Jakobsen, J. (2013). Democrats, republicans or both? an empirical analysis of the effects of the composition of state governments on fdi, 1977–2004. *International Interactions*, 39(2):167–191.
- Hansen, S. (1989). *The Political Economy of State Industrial Policy*. University of Pittsburgh Press.
- Hatcher, S. (1994). Debt and deliberate self-poisoning. *The British Journal of Psychiatry*, 164(1):111–114.
- Head, C. K., Ries, J. C., and Swenson, D. L. (1999). Attracting foreign manufacturing: Investment promotion and agglomeration. *Regional Science and Urban Economics*, 29(2):197–218.
- Hintikka, J., Kontula, O., Saarinen, P., Tanskanen, A., Koskela, K., and Viinamäki, H. (1998). Debt and suicidal behaviour in the finnish general population. *Acta Psychiatrica Scandinavica*, 98(6):493–496.
- Høj, J., Galasso, V., Nicoletti, G., and Dang, T.-T. (2006). The political economy of structural reform.
- Imbens, G. and Kalyanaraman, K. (2011). Optimal bandwidth choice for the regression discontinuity estimator. *The Review of Economic Studies*, page rdr043.
- Imbens, G. W. and Wooldridge, J. M. (2009). Recent developments in the econometrics of program evaluation. *Journal of Economic Literature*, 47(1):5–86.
- Innes, R. and Mitra, A. (2015). Parties, politics, and regulation: Evidence from clean air act enforcement. *Economic Inquiry*, 53(1):522–539.

- Jayaratne, J. and Strahan, P. E. (1996). The finance-growth nexus: Evidence from bank branch deregulation. *The Quarterly Journal of Economics*, 111(3):639–670.
- Jayaratne, J. and Strahan, P. E. (1998). Entry restrictions, industry evolution, and dynamic efficiency: Evidence from commercial banking. *The Journal of Law and Economics*, 41(1):239–274.
- Klašnja, M. and Titunik, R. (2014). The incumbency curse: Weak parties, term limits, and unfulfilled accountability. *American Political Science Review*.
- Kroszner, R. S. and Strahan, P. E. (1999). What drives deregulation? economics and politics of the relaxation of bank branching restrictions. *The Quarterly Journal of Economics*, 114(4):1437–1467.
- Kubrin, C. E. and Wadsworth, T. (2009). Explaining suicide among blacks and whites: How socioeconomic factors and gun availability affect race-specific suicide rates. *Social Science Quarterly*, 90(5):1203–1227.
- Lee, D. S. (2008). Randomized experiments from non-random selection in us house elections. *Journal of Econometrics*, 142(2):675–697.
- Lee, D. S. and Lemieux, T. (2010). Regression discontinuity designs in economics. *Journal of Economic Literature*, 48(2):281–355.
- Lee, D. S., Moretti, E., and Butler, M. J. (2004). Do voters affect or elect policies? evidence from the us house. *The Quarterly Journal of Economics*, 119(3):807–859.

- Leigh, A. (2008). Estimating the impact of gubernatorial partisanship on policy settings and economic outcomes: A regression discontinuity approach. *European Journal of Political Economy*, 24(1):256–268.
- Leip, D. (2008). *Atlas of US Presidential Elections*. Online document: <http://www.uselectionatlas.org>.
- List, J. A. and Sturm, D. M. (2006). How elections matter: Theory and evidence from environmental policy. *The Quarterly Journal of Economics*, 121(4):1249–1281.
- McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, 142(2):698–714.
- McMillan, S. L. (2009). Looking beyond the national level: foreign direct investment attraction in us states. *International Interactions*, 35(2):155–178.
- Melzer, B. T. (2011). The real costs of credit access: Evidence from the payday lending market. *The Quarterly Journal of Economics*, 126(1):517–555.
- Merriott, D. (2016). Factors associated with the farmer suicide crisis in india. *Journal of Epidemiology and Global Health*, 6(4):217–227.
- Mian, A., Sufi, A., and Trebbi, F. (2014). Resolving debt overhang: political constraints in the aftermath of financial crises. *American Economic Journal: Macroeconomics*, 6(2):1–28.
- Michael, R. T. (1978). The rise in divorce rates, 1960–1974: Age-specific components. *Demography*, 15(2):177–182.

- Nickell, S. (1981). Biases in dynamic models with fixed effects. *Econometrica: Journal of the Econometric Society*, pages 1417–1426.
- Office of Trade and Economic Analysis (2016). Jobs attributable to foreign direct investment in the united states.
- Papp, L. M., Cummings, E. M., and Goeke-Morey, M. C. (2009). For richer, for poorer: Money as a topic of marital conflict in the home. *Family Relations*, 58(1):91–103.
- Persson, T. and Tabellini, G. (2006). Democratic capital: The nexus of political and economic change. Technical report, National Bureau of Economic Research.
- Pinto, P. M. and Pinto, S. M. (2008). The politics of investment partisanship: And the sectoral allocation of foreign direct investment. *Economics & Politics*, 20(2):216–254.
- Prati, A., Onorato, M. G., and Papageorgiou, C. (2013). Which reforms work and under what institutional environment? evidence from a new data set on structural reforms. *Review of Economics and Statistics*, 95(3):946–968.
- Preti, A. and Miotto, P. (1998). Seasonality in suicides: the influence of suicide method, gender and age on suicide distribution in italy. *Psychiatry research*, 81(2):219–231.
- Quinn, D. P. and Shapiro, R. Y. (1991). Economic growth strategies: The effects of ideological partisanship on interest rates and business taxation in the united states. *American Journal of Political Science*, pages 656–685.

- Ranci re, R. G. and Tornell, A. (2016). Why do reforms occur in crises times?
- Reading, R. and Reynolds, S. (2001). Debt, social disadvantage and maternal depression. *Social Science & Medicine*, 53(4):441–453.
- Reed, W. R. (2006). Democrats, republicans, and taxes: Evidence that political parties matter. *Journal of Public Economics*, 90(4):725–750.
- Resmini, L. (2000). The determinants of foreign direct investment in the ceecs: New evidence from sectoral patterns. *Economics of transition*, 8(3):665–689.
- Roodman, D. (2009). A note on the theme of too many instruments. *Oxford Bulletin of Economics and statistics*, 71(1):135–158.
- Skovron, C. and Titiunik, R. (2015). A practical guide to regression discontinuity designs in political science. *American Journal of Political Science*.
- Snowberg, E., Wolfers, J., and Zitzewitz, E. (2007). Partisan impacts on the economy: Evidence from prediction markets and close elections. *The Quarterly Journal of Economics*, 122(2):807–829.
- Stevenson, B. and Wolfers, J. (2006). Bargaining in the shadow of the law: Divorce laws and family distress. *The Quarterly Journal of Economics*, 121(1):267–288.
- Sun, S. T. and Yannelis, C. (2016). Credit constraints and demand for higher education: Evidence from financial deregulation. *Review of Economics and Statistics*, 98(1):12–24.

- Tewari, I. (2014). The distributive impacts of financial development: Evidence from mortgage markets during us bank branch deregulation. *American Economic Journal: Applied Economics*, 6(4):175–96.
- Thistlethwaite, D. L. and Campbell, D. T. (1960). Regression-discontinuity analysis: An alternative to the ex post facto experiment. *Journal of Educational Psychology*, 51(6):309.
- Tommasi, M. and Velasco, A. (1996). Where are we in the political economy of reform? *The Journal of Policy Reform*, 1(2):187–238.
- Watson, D. J. (1995). *The new civil war: government competition for economic development*. Praeger Publishers.
- Whatley, C. (2003). *State official's guide to international affairs*. Council of State Governments.
- Wheeler, D. and Mody, A. (1992). International investment location decisions: The case of us firms. *Journal of international economics*, 33(1-2):57–76.
- Yip, P. S., Yang, K. C., Ip, B. Y., Law, Y., and Watson, R. (2007). Financial debt and suicide in hong kong sar. *Journal of Applied Social Psychology*, 37(12):2788–2799.
- Yu, J. and Walsh, M. J. P. (2010). *Determinants of foreign direct investment: A sectoral and institutional approach*. Number 10-187. International Monetary Fund.