ESSAYS ON HEALTH AND PUBLIC ECONOMICS

A Dissertation

by

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ABSTRACT

This dissertation introduces three essays on health and public economics. In the first essay, I reexamine how false ID laws with scanner provisions affect underage drinking. Yoruk uses data from the National Longitudinal Survey of Youth 1997 and finds that false ID laws with scanner provisions have large impacts on underage drinking. I first demonstrate that analyses based on NLSY97 data fail falsification exercises testing for significant pre-intervention effects, and that the estimated effects based on these data are highly sensitive to the inclusion of a lead term and to sample selection, which weakens confidence in the large estimated effects reported in the previous literature. I then use data from the Youth Risk Behavior Surveillance System for the analysis and show that estimates based on these data indicate that these policies have no effect on underage drinking behavior.

In the second essay, I take advantage of the 2005 Base Realignment and Closure process to analyze the effect of government spending on local economic conditions. Exploiting variation in the timing and amount of construction funding provided across counties, my analyses yield an estimated cost per job of $65,000 per year and a local fiscal multiplier of 1.21. Analyses of neighboring counties show little evidence of spillover effects. To further explore the mechanisms underlying these results, I investigate the effects of government spending on migration and show that the funding has positive effects on in-migration, but these effects are too small to explain the main results.

In the final essay, we examine how childbearing responds to changes in economic conditions. We exploit variation driven by the 2005 Base Realignment and Closure process, in which $25 billion of construction funding was distributed across the United States in different amounts and at different points in time. We show that this stimulus improved men's—but not women's—economic conditions, providing a rare opportunity to assess different theoretical models of childbearing. We find that the stimulus led to significant increases in birth rates. These results are consistent with models in which child quantity is a normal good and women's foregone earnings are a major component of the costs of having children.

DEDICATION

To My Parents and Grandpa,

I would not be the person I am today without your unconditional love and selfless devotion.

To Shaobo,

For your love, patience, and support.

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Contributors

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Chapter 2 was conducted with restricted access to Bureau of Labor Statistics (BLS) data. The views expressed here do not necessarily reflect the views of the BLS. Centers for Disease Control and Prevention also provided me with the data necessary to conduct Chapter 2. The data analyzed for Chapter 4 was provided by National Center for Health Statistics.

The analyses depicted in Chapter 4 were conducted in part by Dr. Jason Lindo of the Department of Economics. All other work conducted for the dissertation was completed by the student independently.

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1. GENERAL INTRODUCTION

The main focus of my doctoral research is to apply causal inference technique to identify the effects of important public policies on measures of social wellbeing. Particularly, policy changes are employed as quasi-experiments to identify the health and social impacts of interventions.

In the first essay, I reexamine the effects of the false ID laws with scanner provisions on underage drinking. Recently, several states have passed false ID laws with scanner provisions (hereafter, FSP laws): these laws incentivize alcohol retailers and bar owners to use electronic scanners to ensure that customers are at least 21 years old and have valid identification.[1] The estimates reported in Yoruk (2014) suggest that the adoption of FSP laws significantly reduces youth alcohol use. Moreover, the magnitude of those estimates suggests that FSP laws are extremely effective compared to other alcohol control policies. These laws have the potential to reduce alcohol sales to youth through two channels. First, there may be a detection effect because an electronic scanner makes it easier to detect fake identification used to purchase alcohol. Second, there may be a deterrence effect as scanners may deter underage youth from trying to purchase alcohol. However, FSP laws may not be effective if few retailers choose to install scanners in their stores, underage youth substitute towards retailers that do not use scanners, borrow an ID from look-alikes who are over 21, or ask someone older than 21 to purchase alcohol on their behalf. In this essay, I first demonstrate that analyses based on NLSY97 data fail falsification exercises testing for significant pre-intervention effects, and that the estimated effects based on these data are highly sensitive to the inclusion of a lead term and to sample selection, which weakens confidence in the large estimated effects reported in [1]. I then use data from

[1] Prices for ID scanners range from $400 to $1,300. (www.idscanner.com)

the Youth Risk Behavior Surveillance System for the analysis and show that estimates based on these data indicate that false ID laws with scanner provisions have no effect on underage drinking behavior.

In the second essay, I take advantage of the 2005 Base Realignment and Closure process to identify how government spending affects local economies. Motivated by beliefs that the fiscal multiplier is relatively large, the federal government passed the American Recovery and Reinvestment Act (ARRA) to stimulate the economy in 2009 at a cost of more than $800 billion [2]. Many other countries adopted similar policies in response to the Great Recession, the worst economic downturn since the 1930s. However, economists and policy makers still have not reached a consensus on the effectiveness of government spending. In order to estimate the effect of government spending on local economies, I exploit variation in the timing and amount of construction funding provided by the 2005 BRAC across counties with military bases. The BRAC process realigned and closed some military installations to improve military efficiency and effectiveness. A BRAC Commission was created to provide an objective and non-partisan analysis of military installations. It then produced a final, non-amendable recommendation list. The commission gave priority to military value during its selection process, and commissioners recused themselves from participation in matters related to installations in their home states. Thus, to some extent, the funding awarded to each county most likely was motivated by military considerations and plausibly was unrelated to local economies. My analysis identifies the causal effect of the stimulus on local economies under the identifying assumption that, changes in local economic conditions would have been the same across military counties absent the 2005 BRAC funding. Using county-level economic data from the Bureau of Economic Analysis' Regional Economic Accounts and a novel dataset that contains 2005 BRAC construction funding information, I find an estimated cost per job of $65,000 and a local fiscal multiplier—the change in local per capita income produced by a one dollar change in per capita government spending—of 1.21. These estimates are robust to various model specifications and the empirical strategy passes falsification exercises. Furthermore, my industry-specific analysis reveals especially large effects on the construction industry, which is consistent with the nature of the program.

In the third essay, we further takes advantage of the 2005 Base Realignment and Closure process, an intervention that mostly affects men's labor market condition, to test Becker's (1960) contention that children are "normal." In his first pathbreaking work on the topic, Gary Becker developed an economic framework treating children as consumer durables that provide utility to parents over time [3]. Becker recognized that this model seemed at odds with the negative relationship between household income and family size in the United States at the time; he surmised that it was driven by differences in knowledge about birth control, not because children were "inferior goods." A great deal of subsequent research was devoted to expanding on Becker's initial framework so that it could predict this negative relationship while maintaining that children were normal goods. Most of these works involved one of two theoretical mechanisms that work in the opposite direction of the pure income effect: a quantity-quality tradeoff or parental time use. Models involving a tradeoff between child quantity and child quality can generate a reduction in the demand for quantity in response to increase in income if the income elasticity for quality is high relative to the income elasticity for quantity; having children is more costly when parents plan to invest more in each child. In models that incorporate parental time into the production function for children, an increase in parental wages generates an income effect that increases the demand for children and a substitution effect that reduces demand through the effect on the opportunity cost of devoting time to have and raise children. Models emphasizing parental time use can also recognize differences in household roles: the substitution effect is more likely to dominate for wage shocks to primary caregivers (usually women) while the income effect is more likely to dominate for wage shocks other caregivers (usually men). Since men account for more than 90% of all employment in the construction industry, the 2005 Base Realignment and Closure process provides a potentially exogenous shock to men's labor market condition, thus a good opportunity to test the implication based on parental time allocation model—men's labor market condition would have a positive effects on fertility since income effects are likely to dominate substitution effects in this case. Exploiting variation in the timing and amount of construction funding provided by the 2005 BRAC across counties with military bases, our analysis suggest that fertility increases with men's labor market condition, consistent with the theoretical prediction.

2. CAN TECHNOLOGY REALLY HELP TO REDUCE UNDERAGE DRINKING? NEW EVIDENCE ON THE EFFECTS OF FALSE ID LAWS WITH SCANNER PROVISIONS

2.1 Introduction

Youth alcohol use is a prominent public health issue in the United States. As the most commonly used and abused drug, alcohol is responsible for more than 4,300 deaths and 185,000 emergency room visits among minors every year.[1] Moreover, recent studies have linked underage alcohol consumption to a variety of undesirable outcomes, including risky sexual behavior [6, 7, 8], mortality [9, 10, 11, 12], morbidity [13], crime [14, 15], poor academic performance [16, 17], and unemployment [18]. The medical and social costs associated with underage drinking are estimated to be in the billions of dollars per year [19].

Given these alarming statistics and findings, how can we best address this problem? Recently, several states have passed false ID laws with scanner provisions (hereafter, FSP laws): these laws incentivize alcohol retailers and bar owners to use electronic scanners to ensure that customers are at least 21 years old and have valid identification.[2] [1] uses data from the National Longitudinal Survey of Youth 1997 and a differences-in-difference design to estimate the effects of these policies on underage drinking and conclude that the adoption of FSP laws significantly reduces youth alcohol use. Moreover, the magnitude of the estimates suggests that FSP laws are extremely effective compared to other alcohol control policies. Prior work has shown that increasing the Minimum Legal Drinking Age (hereafter, MLDA) reduces drinking participation and binge drinking participation by approximately 5% [9, 20]; zero tolerance laws have no effect on drinking participation but

[1] See [4] and [5]

[2] Prices for ID scanners range from \$400 to \$1,300. (www.idscanner.com)

reduce binge drinking participation by 13% for males [21]; social hosting laws have no effect on underage drinking [22]; vertical ID laws reduce drinking participation for 16 year olds by 10% but have no effect on binge drinking or drinking frequency [23].[3] In contrast, the estimates reported in [1] suggest that FSP laws reduce binge drinking participation by 15%, frequency of alcohol consumption by 20%, and binge drinking frequency by 30%.

What is the argument for FSP laws as an approach to reducing underage drinking? FSP laws provide an affirmative defense for retailers in prosecutions for sales to minors if they can show that the scanner was used properly. These laws have the potential to reduce alcohol sales to youth through two channels. First, there may be a detection effect because an electronic scanner makes it easier to detect fake identification used to purchase alcohol. Second, there may be a deterrence effect as scanners may deter underage youth from trying to purchase alcohol. However, FSP laws may not be effective if few retailers choose to install scanners in their stores, underage youth substitute towards retailers that do not use scanners, borrow an ID from look-alikes who are over 21, or ask someone older than 21 to purchase alcohol on their behalf.[4] Given this theoretical ambiguity, it is necessary to empirically evaluate the effectiveness of these laws, highlighting the importance of [1]. Moreover, if the laws have the large effects reported in [1], it may be efficient for policy makers in every state to consider adopting FSP laws.

In this paper, I reexamine the impact of FSP laws on underage drinking using a difference-in-differences method, exploiting within state variation induced by the timing of several states passing FSP laws. First, I use the restricted National Longitudinal Survey of Youth 1997 (NLSY97) to replicate and extend the estimates reported in [1]. I demonstrate that analyses based on NLSY97 data fail falsification exercises testing for significant pre-intervention effects, and the magnitude and statistical significance of the estimated effects

[3]Though not aiming at reducing underage drinking, [24] suggests that medical marijuana laws reduce drinking participation for 18–19 year olds by around 14%.

[4][1] also mentions reasons why FSP laws may not work.

based on these data are sensitive to the inclusion of a lead term in the specification, weakening confidence in the results originally reported in [1]. Moreover, a fair share of the significant estimates disappears when the 1997 wave of the NLSY97 is included in the analysis, casting further doubt on our ability to draw strong conclusions based on analyses of these data. I then turn to another reasonable data set for estimating the effects of FSP laws. In particular, I use the 1991–2013 national Youth Risk Behavior Surveillance System (YRBS), which offers a larger sample size and a longer sample period than NLSY97. Moreover, the YRBS was specifically designed to study youth behaviors, such as alcohol and other drug use, risky sexual behavior, and tobacco use. Estimates based on these data indicate that FSP laws have no effect on drinking participation, binge drinking participation, or drinking and binge drinking frequency. In contrast to the estimates based on the NLSY97, these results are robust to changes in specifications and do not fail falsification tests. Moreover, estimates allowing for dynamic treatment effects indicate that FSP laws have neither short-term nor long-term effects.

2.2 Reconsidering Evidence from NLSY97

To reexamine the effects of FSP laws, I first replicate and extend Yoruk's (2014) analysis using the restricted NLSY97 data—the same source of data as used by [1]. The NLSY97 surveys a nationally representative sample of approximately 9,000 youths who were 12–16 years old as of December 31, 1996 [25]. These youths have been interviewed annually since 1997.[5] Following [1], I begin by restricting my attention to NLSY97 data from the period 1998–2005. I then include data from 1997 in the analysis.[6]

NLSY97 asks respondents how many days did they consume alcohol and engage in

[5]NLSY97 starts to interview cohorts biennially after 2011.

[6]Following [1], I restrict the sample period to the year of 2005, because no interviewee in the NLSY97 sample is younger than 21 years old after 2005. A follow-up with Yoruk indicates that, while not mentioned in Yoruk (2014), those who do not have an exact interview date or exact birthday are dropped, along with those who reported drinking more than more than 30 drinks a day more than 30 drinks a day.

binge drinking (consuming five or more drinks in one sitting) in the past 30 days, respectively. Based on this information, I construct *Days of Alcohol Consumption* and *Days of Binge Drinking* to measure drinking and binge drinking frequency, respectively, and these variables have a value of zero if participants have not drunk or binge drunk in the past 30 days. Using information on drinking and binge drinking frequency, I also generate two dummy variables—*If Consumed Alcohol* and *If Binge Drank*—to measure unconditional drinking and unconditional binge drinking participation in the past 30 days, respectively. The remaining variable, *Average Drinks per Day*, measures drinking intensity, which is calculated as *Days of Alcohol Consumption* times average drinks per sitting divided by 30. Table 2.1 presents the summary statistics. In the table, we see that the means and standard deviations I calculate from the NLSY97 sample are close to those reported in [1].

Following [1], I use a difference-in-differences methodology in the analysis, exploiting variation in the timing of FSP law adoption across states. Specifically, I estimate the following model:

$$Y_{istm} = \beta^{'} X_{istm} + \alpha^{'} S_{stm} + \gamma FSP_{istm} + \mu_s + \eta_t + \lambda_m + \delta_{st} + \varepsilon_{istm}$$

where *i* indicates individuals, *s* indicates states, *t* indicates years, and *m* indicates months. In this model, Y_{istm} is a measure of drinking behavior; X_{istm} refers to a variety of individual-level controls, including age, gender, race, family size, income, marital status, employment status, educational attainment and being a student; S_{stm} is a vector of state-level time-varying controls, including unemployment rates, per capita income, state beer taxes rates, and indicators for several other alcohol policies, including BAC 0.08 laws, social hosting laws, Sunday alcohol sales, and vertical ID laws; δ_{st} indicates state-specific linear time trends.[7] The state, year and month fixed effects are captured by μ_s, η_t and λ_m, respectively.

[7] I follow [1] in constructing control variables. I define treatment using introduction dates of FSP laws from the [26], and law effective dates are listed in Table C.1. The data for state beer taxes are from the Beer

Variable	N	Mean	S.D.
Panel A: If Consumed Alcohol			
Replication: NLSY97 (98–05)	40,164	0.476	0.499
Extension: NLSY97 (97–05)	49,089	0.431	0.495
YRBS (98–05)	54,730	0.464	0.499
YRBS (91–13)	157,288	0.455	0.498
Panel B: If Binge Drank			
Replication: NLSY97 (98–05)	40,097	0.275	0.446
Extension: NLSY97 (97–05)	49,020	0.246	0.431
YRBS (98–05)	56,539	0.292	0.455
YRBS (91–13)	164,501	0.286	0.452
Panel C: Days of Alcohol Consumption			
Replication: NLSY97 (98–05)	40,164	2.685	4.938
Extension: NLSY97 (97–05)	49,089	2.373	4.688
YRBS (98–05)	54,730	2.616	5.116
YRBS (91–13)	157,288	2.507	4.926
Panel D: Days of Binge Drinking			
Yoruk(2014): NLSY97 (98–05)	40,249	1.283	3.308
Replication: NLSY97 (98–05)	40,097	1.249	3.214
Extension: NLSY97 (97–05)	49,020	1.108	3.042
YRBS (98–05)	56,539	1.299	3.465
YRBS (91–13)	164,501	1.233	3.332
Panel E: Average Drinks per Day			
Replication: NLSY97 (98–05)	39,883	0.548	1.562
Extension: NLSY97 (97–05)	48,786	0.481	1.465

Table 2.1: Summary Statistics for Underage Drinking

Notes: Sample weighted means are reported.

FSP_{istm} is the variable of interest, which equals one if a FSP law is in effect at month *m*, year *t* in state *s* for individual *i*. The estimate of γ identifies the causal effect of FSP laws on underage drinking under the identifying assumption that, in the absence of FSP laws,

Institute's [27]; unemployment rates are from the Bureau of Labor Statistics (BLS); state-level income per capita are from the Bureau of Economic Analysis (BEA); the introduction dates of BAC 0.08 laws, social hosting laws, and Sunday alcohol sales are from the Alcohol Policy Information System; and data on the introduction years of vertical ID laws are from [23].

the change in underage alcohol consumption in the states passing FSP laws would have been the same as the change in underage alcohol consumption in other states. Models with binary outcome variables are estimated as linear probability models, and standard errors are clustered at the state level [28].

Table 2.2 is a replication of Yoruk's main results. Column 1 shows my replication of the baseline estimates, from a model that includes state, year and month fixed effects, and Column 2 through 4 progressively add individual- and state-time-varying controls, a lead term (a dummy variable for the two years before the policy adoption), and state-specific linear time trends to the estimation strategy. Columns 5 through 8 are similar but additionally control for individual fixed effects. The estimated effects of the FSP laws reported in this table are often statistically significant, suggesting that FSP laws reduce underage drinking. However, the estimated effects of the lead term are statistically significant nearly as often. Convention would have us interpret these estimates as failed placebo tests that cast doubt on the main results.

Table 2.3 extends the analysis by also using data from the 1997 survey wave, which increases the sample size by about 20%. [1] does not discuss why these data were not included in his analyses. However, these data would appear to be particularly important because they disproportionately include young teens while his analysis of heterogeneity indicates that his main results are largely driven by effects on young teens.

The results shown in this table demonstrate that Yoruk's choice to omit data from 1997 leads to estimates that are statistically significant about twice as often as they are when this additional year of data is used in the analysis. This lack of robustness casts further doubt on our ability to draw any strong conclusion based on analyses of the NLSY97 data. I also note that the lead terms continue to be large and significant for a couple of outcomes when 1997 data are included in the analysis.

To conclude, the results from the NLSY97 are sensitive to the inclusion of a lead term,

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: If Consumed Alcohol								
FSP Laws	-0.004	-0.003	-0.006	0.024	0.003	-0.001	-0.002	0.015
	(0.011)	(0.014)	(0.018)	(0.024)	(0.011)	(0.013)	(0.015)	(0.021)
1–2 Years Prior to FSP Laws			-0.005	0.007			-0.001	-0.002
			(0.010)	(0.013)			(0.009)	(0.010)
N	40,164	40,070	40,070	40,070	40,164	40,070	40,070	40,070
R^2	0.039	0.106	0.106	0.108	0.068	0.077	0.077	0.082
Panel B: If Binge Drank								
FSP Laws	-0.023*	-0.023*	-0.038**	-0.011	-0.023***	-0.028**	-0.035**	-0.016
	(0.012)	(0.014)	(0.017)	(0.018)	(0.008)	(0.011)	(0.015)	(0.018)
1–2 Years Prior to FSP Laws			-0.026***	-0.018*			-0.014*	-0.016*
			(0.008)	(0.010)			(0.008)	(0.008)
N	40,097	40,004	40,004	40,004	40,097	40,004	40,004	40,004
R^2	0.032	0.095	0.095	0.097	0.051	0.060	0.060	0.064
Panel C: Days of Alcohol Consumption								
FSP Laws	-0.168*	-0.156*	-0.357***	-0.269**	-0.208*	-0.266**	-0.441***	-0.398***
	(0.093)	(0.087)	(0.088)	(0.103)	(0.121)	(0.116)	(0.118)	(0.116)
1–2 Years Prior to FSP Laws			-0.364***	-0.396***			-0.322***	-0.396***
			(0.079)	(0.106)			(0.083)	(0.099)
N	40,164	40,070	40,070	40,070	40,164	40,070	40,070	40,070
R^2	0.026	0.084	0.084	0.085	0.054	0.065	0.065	0.068
Panel D: Days of Binge Drinking								
FSP Laws	-0.125	-0.111	-0.251***	-0.082	-0.136*	-0.158**	-0.272***	-0.151*
	(0.091)	(0.071)	(0.087)	(0.101)	(0.073)	(0.077)	(0.092)	(0.077)
1–2 Years Prior to FSP Laws			-0.254***	-0.199***			-0.209***	-0.202***
			(0.054)	(0.065)			(0.046)	(0.038)
N	40,097	40,004	40,004	40,004	40,097	40,004	40,004	40,004
R^2	0.020	0.071	0.071	0.073	0.037	0.044	0.045	0.048
Panel E: Average Drinks per Day								
FSP Laws	-0.086**	-0.071	-0.128***	-0.100***	-0.121***	-0.124**	-0.173***	-0.138***
	(0.039)	(0.045)	(0.038)	(0.033)	(0.041)	(0.046)	(0.043)	(0.043)
1–2 Years Prior to FSP Laws			-0.104***	-0.099**			-0.091***	-0.091***
			(0.036)	(0.040)			(0.025)	(0.027)
N	39,883	39,790	39,790	39,790	39,883	39,790	39,790	39,790
R^2	0.013	0.051	0.051	0.052	0.022	0.027	0.027	0.029
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
1–2 Years Prior to FSP Laws	No	No	Yes	Yes	No	No	Yes	Yes
State-specific Linear Time Trends	No	No	No	Yes	No	No	No	Yes
Individual Fixed Effects	No	No	No	No	Yes	Yes	Yes	Yes

Table 2.2: Attempted Replication of [1]'s Main Results, Highlighting Significance of Leads

Note: *** p<0.01, ** p<0.05, * p<0.1 Sample is 1998–2005 NLSY97 survey. All regressions include state, year and month fixed effects. Individual-level controls include age, gender, race, family size, income, marital status, employment status, educational attainment and being a student. State-level controls include unemployment rate, log of income per capita, state beer tax per gallon, and dummy variables controlling for various state policies on youth alcohol access. Standard errors are clustered at the state level. Estimates are unweighted.

raising concerns about the validity of the common trends assumption with this dataset. Moreover, the results are sensitive to sample selection. When the entire available sample is used, a fair share of the previously significant results disappears, and almost all the estimates become smaller in magnitude when 1997 data are included in the analysis. Overall, this set of estimates substantially weakens the confidence in the results originally reported

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: If Consumed Alcohol								
FSP Laws	-0.002	0.007	0.007	0.028	0.003	0.007	0.010	0.031*
	(0.013)	(0.017)	(0.021)	(0.018)	(0.013)	(0.016)	(0.019)	(0.016)
1–2 Years Prior to FSP Laws			-0.001	0.009			0.007	0.011
			(0.011)	(0.010)			(0.011)	(0.010)
N	49,089	48,979	48,979	48,979	49,089	48,979	48,979	48,979
R^2	0.071	0.135	0.135	0.137	0.124	0.133	0.133	0.137
Panel B: If Binge Drank								
FSP Laws	-0.014	-0.009	-0.015	-0.013	-0.017*	-0.015	-0.015	-0.015
	(0.012)	(0.014)	(0.017)	(0.017)	(0.010)	(0.012)	(0.016)	(0.016)
1–2 Years Prior to FSP Laws			-0.014	-0.012			0.001	-0.003
			(0.010)	(0.007)			(0.011)	(0.008)
N	49,020	48,911	48,911	48,911	49,020	48,911	48,911	48,911
R^2	0.047	0.105	0.105	0.107	0.078	0.086	0.086	0.090
Panel C: Days of Alcohol Consumption								
FSP Laws	-0.140	-0.080	-0.190*	-0.238***	-0.161	-0.161	-0.235*	-0.315***
	(0.090)	(0.095)	(0.109)	(0.061)	(0.098)	(0.109)	(0.121)	(0.065)
1–2 Years Prior to FSP Laws			-0.269***	-0.311***			-0.182**	-0.252**
			(0.083)	(0.096)			(0.089)	(0.103)
N	49,089	48,979	48,979	48,979	49,089	48,979	48,979	48,979
R^2	0.043	0.095	0.096	0.097	0.080	0.091	0.091	0.094
Panel D: Days of Binge Drinking								
FSP Laws	-0.080	-0.052	-0.123	-0.124	-0.093	-0.102	-0.147	-0.171**
	(0.086)	(0.076)	(0.092)	(0.092)	(0.078)	(0.078)	(0.096)	(0.074)
1–2 Years Prior to FSP Laws			-0.174***	-0.163***			-0.110*	-0.128**
			(0.063)	(0.056)			(0.063)	(0.053)
N	49,020	48,911	48,911	48,911	49,020	48,911	48,911	48,911
R^2	0.027	0.072	0.072	0.074	0.047	0.054	0.054	0.058
Panel E: Average Drinks per Day								
FSP Laws	-0.065**	-0.036	-0.064**	-0.056*	-0.088***	-0.071**	-0.086**	-0.084***
	(0.029)	(0.032)	(0.031)	(0.029)	(0.027)	(0.033)	(0.033)	(0.026)
1–2 Years Prior to FSP Laws			-0.070	-0.053			-0.039	-0.030
			(0.046)	(0.048)			(0.042)	(0.040)
N	48,786	48,677	48,677	48,677	48,786	48,677	48,677	48,677
R^2	0.021	0.055	0.055	0.057	0.035	0.040	0.040	0.042
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
1–2 Years Prior to FSP Laws	No	No	Yes	Yes	No	No	Yes	Yes
State-specific Linear Time Trends	No	No	No	Yes	No	No	No	Yes
Individual Fixed Effects	No	No	No	No	Yes	Yes	Yes	Yes

Table 2.3: Extending [1] to Utilize Full NLSY97 Sample

Note: *** p<0.01, ** p<0.05, * p<0.1 Sample is 1997–2005 NLSY97 survey. All regressions include state, year and month fixed effects. Individual-level controls include age, gender, race, family size, income, marital status, employment status, educational attainment and being a student. State-level controls include unemployment rate, log of income per capita, state beer tax per gallon, and dummy variables controlling for various state policies on youth alcohol access. Standard errors are clustered at the state level. Estimates are unweighted.

in [1].[8]

[8]Table C3 presents my attempted replication and extension of Yoruk's (2014) dynamic analysis, with Panel A omitting 1 year before the FSP laws, and shows that none of the estimates are significant. These results are at odds with Yoruk's (2014) finding of an immediate effect on average drinks per day. Moreover, while the indicators for years leading up to the policies are insignificant in this specific analysis, this appears to be due to a lack of power associated with using a single year of baseline data (i.e. having only one year serve as the omitted category) when sample size is small. It is evident from my main results that the indicators for 1-2 years prior to the laws are significant when all prior years are used as the baseline. I have also performed an analysis that is closer in spirit to Yoruk's analysis of dynamics that enhances power by

2.3 New Evidence from YRBS

2.3.1 Main Results

In this section, I turn to an alternative data set for estimating the effects of FSP laws—the restricted national Youth Risk Behavior Surveillance System (YRBS) [29]. YRBS is a biennial survey spanning the years from 1991 to 2013. It surveys a nationally representative sample of youth about their health-risk behaviors, such as alcohol and drug use.[9] Primarily designed to monitor health-risk behavior that contributes significantly to death, disability, and social problems among youth, the YRBS provides comprehensive information on youth alcohol consumption. It has been used in a number of other studies on youth health behavior [23, 30, 31, 32, 33, 34, 35, 36]. Unlike the NLSY97, this survey focuses exclusively on ninth through twelfth grade students; however, this is arguably an attractive feature in this context, because Yoruk's (2014) estimates suggest 13–17 year olds are responsive to these policies whereas older individuals are not.[10]

One potential advantage of the NLSY97 dataset is that it allows for the inclusion of individual fixed effects. However, the results based on the NLSY97 with and without individual fixed effects do not differ, which is not surprising because it is unlikely that individual characteristics are correlated with the introduction of FSP laws. In contrast, the primary advantages of the YRBS dataset are a much larger sample size and a much

omitting 1-2 years prior to the policies instead of just one. I present the results of this analysis in Panel B of Table C3. These estimates also indicate that Yoruk's (2014) NLSY97 sample fails the falsification tests. As a whole, this set of results further weakens confidence in the conclusions drawn from Yoruk's analysis of FSP laws.

[9] YRBS codes 7 for people aged 18 years and over, so potentially the sample could include people over age 21. Inclusion of individuals aged over 21 will attenuate the results, since FSP laws only targets people under the age of 21. However, because YRBS targets ninth through twelfth graders, it is unlikely that they are over 21. According to 2013 Current Population Survey October supplements, people 21 years old and older account for only 2.5% of the entire high school population. Thus, I recode these people as 18 years old and include them in the main analysis. I drop individuals whose age information is missing, accounting for around 2% of the entire sample.

[10] See Table 6 in [1]. The reasons, as suggested by [1], could be that young teens have less chance of knowing someone older than 21 to purchase alcohol for them; they are more likely to be asked for identification in the stores; and they are not in college where alcohol is more accessible.

longer sample period. These advantages have important implications for the reliability of the analysis. First, larger samples are expected to more closely approximate the population and to enhance precision. Second, the longer sample period of the YRBS offers a longer pre-treatment period than the NLSY97, which is crucial for a difference-in-differences identification strategy. Having *few* years of pre-treatment data increases the risk that the estimates may be biased by anomalous spikes in the data prior to treatment, which might explain why the NLSY97 estimates vary considerably when controlling for a lead term.

Same as the NLSY97, YRBS also asks individuals about their frequency of alcohol use in the previous 30 days, including the number of days of alcohol consumption and the number of days of binge drinking (consuming five or more drinks in one sitting).[11] Following the same method as used in the NLSY97 sample, I construct *Days of Alcohol Consumption* and *Days of Binge Drinking* to measure drinking and binge drinking frequency, respectively, and also generate two dummy variables—*If Consumed Alcohol* and *If Binge Drank*—to indicate unconditional drinking and binge drinking participation in the past 30 days, respectively.[12]

[11]Note that for both males and females in YRBS binge drinking is defined as five drinks in one sitting, which is consistent with NLSY97. Other data sources may set the reference level at four drinks for females. For questions on drinking frequency, YRBS codes 1 as no drinking, 2 as drinking 1 or 2 days, 3 as drinking 3 to 5 days, etc. I recode no drinking to 0 and use the midpoint to recode the rest. That is, I recode 1.5 for drinking 1 or 2 days, 4 for drinking 3 to 5 days, 7.5 for drinking 6 to 9 days, 14.5 for drinking 10 to 19 days, 24.5 for drinking 20 to 29 days and 30 for drinking 30 days. The same applies for binge drinking frequency. I recode 0 for reporting no binge drinking, 1 for binge drinking 1 days, 2 for binge drinking 2 days, 4 for binge drinking 3 to 5 days, 7.5 for binge drinking 6 to 9 days, 14.5 for binge drinking 10 to 19 days, and 25.5 for binge drinking over 20 days. Table A4 presents an analysis I perform to show that measurement errors are not responsible for the discrepancy in results between YRBS and NLSY97 samples. Using Yoruk's (2014) NLSY97 sample, I recoded the responses following YRBS's coding scheme for the two variables that were interpolated based on mid-point of ranges, and present results using both the original and recoded responses. Estimates based on the two sets of responses are similar, indicating that measurement errors are not responsible for the differences in results.

[12]One thing to note about the YRBS data is that it does not contain the exact dates of the interview. However, all surveys took place between February and May of odd-number years. Since none of the policy changes happened during an interview window, treatment status can be assigned without error. I use all of the data available. [1] does not treat Nebraska and Utah as treatment states because no interviewee in NLS797 is under 21 after 2005, and these states passed FSP laws in 2009 and 2010, respectively. Moreover, the statistical significance and magnitude of the estimates remain robust if these states are excluded.

Summary statistics presented in Table 2.1 allows for a comparison of the NLSY97 and the YRBS. The summary statistics based on the YRBS and NLSY97 are remarkably similar for the four underage drinking indicators calculated from both samples, even though the size of the full sample of YRBS is more than three times of the NLSY97's.

Table 2.4 shows the main results from YRBS sample, and each underage drinking measure is presented panel by panel.[13] Column 1 presents estimates from the baseline specification, which simply controls for state and year fixed effects.[14] The results from this specification indicate that FSP laws have no effect on drinking participation, binge drinking participation, drinking frequency, or binge drinking frequency. As precise as the estimates based on the NLSY97 sample, these estimates can not rule out large positive or negative impact of the FSP laws though. In Column 2, I add controls for economic conditions and state-level underage alcohol control policies, as well as individual characteristics.[15] I note that the estimate for binge drinking participation becomes *positive* and significant at the 10% level once these controls are added; however, the point estimate is positive and is not robust across specifications. Overall, the results change little once time-varying controls are added, suggesting that there may be little scope for omitted, unobserved factors to bias the estimates, and supporting the conclusion that FSP laws have no effect on reducing any of the underage alcohol consumption measures.

In Column 3, I also include a lead term to the specification: a dummy variable for the two years before the policy changes. Under the common trends assumption, the coefficient on the lead term should be zero and the inclusion of this lead term should not

[13] The results are unweighted so that they are comparable to [1]'s main results. Weighted results are shown in Appendix.

[14] [1] also controls for month fixed effects, which is infeasible for the YRBS data. However, this should not influence the estimates, as the interview period of YRBS is from February to May, a period that does not exhibit much seasonal variation in alcohol consumption behavior [37].

[15] YRBS has information on age, gender and race. Other individual characteristics such as marital status, employment status and income level are not available on YRBS; however, it should not meaningfully affect the estimates since individual-level characteristics are unlikely to be correlated with whether states passed FSP laws. Also, only a small fraction of underage minors are married or have jobs.

	(1)	(2)	(3)	(4)
Panel A: If Consumed Alcohol				
FSP Laws	0.006	0.013	0.016	-0.004
	(0.010)	(0.011)	(0.011)	(0.022)
1–2 Years Prior to FSP Laws			0.017	0.004
			(0.015)	(0.019)
N	157,288	155,480	155,480	155,480
Panel B: If Binge Drank				
FSP Laws	0.007	0.019*	0.023**	0.009
	(0.011)	(0.010)	(0.011)	(0.026)
1–2 Years Prior to FSP Laws			0.022*	0.016
			(0.011)	(0.017)
N	164,501	162,585	162,585	162,585
Panel C: Days of Alcohol Consumption				
FSP Laws	0.116	0.224	0.253*	0.192
	(0.118)	(0.136)	(0.130)	(0.276)
1–2 Years Prior to FSP Laws			0.146	0.133
			(0.135)	(0.176)
N	157,288	155,480	155,480	155,480
Panel D: Days of Binge Drinking				
FSP Laws	0.040	0.125	0.142	0.125
	(0.075)	(0.087)	(0.086)	(0.221)
1–2 Years Prior to FSP Laws			0.085	0.092
			(0.069)	(0.113)
N	164,501	162,585	162,585	162,585
Control Variables	No	Yes	Yes	Yes
1–2 Years Prior to FSP Laws	No	No	Yes	Yes
State-specific Linear Time Trends	No	No	No	Yes

Table 2.4: Estimated Effects of FSP Laws on Underage Drinking Using YRBS Data

Notes: *** p<0.01, ** p<0.05, * p<0.1

Sample is 1991–2013 YRBS survey. All regressions include state and year fixed effects. Individual-level controls include gender, race and age. State-level controls include unemployment rate, log of income per capita, state beer tax per gallon, and dummy variables controlling for various state policies on youth alcohol access. Standard errors are clustered at the state level. Estimates are unweighted.

meaningfully change the estimated effects. Unlike the results from the NLSY97 sample, the coefficients for the lead term are not significant, which provides support for the validity of the analysis using these data. Moreover, the estimates with and without a lead term are very similar, all suggesting that FSP laws do not reduce underage drinking. In Column 4, I add state-specific linear time trends to the specification, thereby allowing each state to follow a different trend. To conclude, the estimates are largely robust under various model specifications, and provide little evidence that FSP laws have reduced any of the underage alcohol consumption behavior.[16]

As an additional way of estimating the effects of FSP laws on drinking, I investigate dynamic responses in youth drinking behavior to the adoption of FSP laws. I do so by replacing the indicator for having a FSP law in place with a set of indicator variables corresponding to 5–6, 3–4, 1–2 years prior to adoption, the year of adoption, and 1–2, 3–4, 5-or-more years after policy adoption.[17] As before, I continue to control for individual- and state-time-varying controls and state-specific linear time trends. Figure 2.1 plots coefficient estimates and 95% confidence intervals from the dynamic analysis. This figure shows that none of the coefficient estimates for years leading up to the policy changes are statistically significant at the 5% level, supporting the common trends assumption. Moreover, the same is true for the lags, indicating that FSP laws have no effect on reducing underage drinking behavior in the short term or in the long term.

2.4 Conclusion

Youth alcohol use imposes substantial costs on society and has long been a major public health concern in the United States. In this paper, I employ a difference-in-differences design to reexamine the effect of false ID laws with scanner provisions on underage drink-

[16]Figure B.1 presents a graphical analysis to explore if the estimates are sensitive to the treated states considered and if there are heterogeneous treatment effects across states. I continue to use a model with state and year fixed effects, controls, and state-specific linear time trends, but drop 1, 2 or 3 treatment states in this analysis. Figure A1 plots the coefficient estimates and the 95% confidence interval against the ranking of coefficient estimates from this analysis. As estimates are rarely significant in Figure B.1, it shows that there are no heterogeneous treatment effects across states and, further, FSP laws do not reduce underage alcohol use. Yoruk (2014) also shows that the estimated effects he documents are not driven by any particular states. Noting that the YRBS does not cover all states in all years, these results provide reassuring evidence that the differences between the estimates reported in Yoruk (2014) and those documented here are unlikely to be driven by differences in the composition of states included in the NLSY97 and YRBS.

[17]Since YRBS is a biennial survey, I combine two years (1-2 etc.) together instead of estimating year-by-year effects.

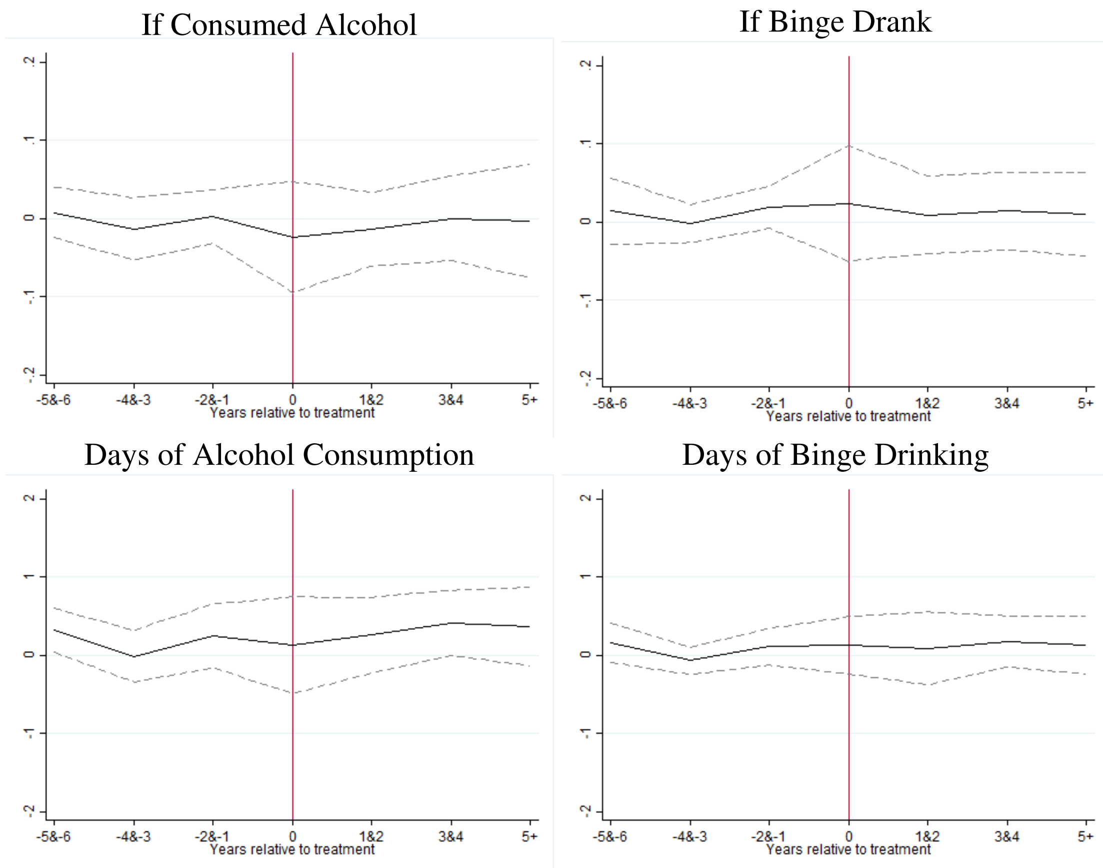

Figure 2.1: Estimated Effects of FSP Laws, Lags and Lead

Notes: Sample is 1991–2013 YRBS survey. Coefficient estimates are from a model controlling for state and year fixed effects, controls and state-specific linear time trends. Controls include gender, race, unemployment rate, log of income per capita, state beer tax per gallon, and dummy variables controlling for various state policies on youth alcohol access. Standard errors are clustered at the state level, and 95% confidence intervals are shown in dash line. The omitted years are seven and more years prior to law adoption. Estimates are unweighted.

ing using 1991–2013 national Youth Risk Behavior Surveillance System data. In contrast to previous findings based on the NLSY97, the results using these data indicate that these laws have no effect on reducing underage drinking behavior, and that conclusion is robust under various model specifications. Estimates allowing for dynamic treatment effects lead to the same conclusion. Moreover, a replication and extension of prior work using the NLSY97 suggests that previously reported estimates based on those data are not reliable.

As a whole, my analyses suggest that a stricter false ID law with enforced use of electronic scanners in alcohol sales is not an effective approach to reducing underage drinking.

3. NEW EVIDENCE ON THE LOCAL FISCAL MULTIPLIER AND EMPLOYMENT FROM MILITARY CONSTRUCTION SPENDING

3.1 Introduction

One cannot overemphasize the need to better understand the effects of government spending. Is government spending really effective at stimulating the economy? How much does it cost to create a job? And, how large is the multiplier? Motivated by beliefs that the fiscal multiplier is relatively large, the federal government passed the American Recovery and Reinvestment Act (ARRA) to stimulate the economy in 2009 at a cost of more than $800 billion [2]. Many other countries adopted similar policies in response to the Great Recession, the worst economic downturn since the 1930s. However, economists and policy makers still have not reached a consensus on the effectiveness of government spending.

The debate arises partly because theoretical models offer contradictory predictions. Simple neoclassical models usually yield a small multiplier (typically less than 0.5) while New Keynesian models can yield predictions larger than 2 [38]. Difficulty in measuring the counterfactual in the macroeconomy only adds to the debate. To tackle this problem, a recent stream of the empirical literature utilizes cross-sectional variation to estimate the income multiplier and cost per job. However, this line of empirical evidence also offers a wide range of estimates for the cost per job ($25,000–$125,000) and the local income multiplier (0.4–2.2). This wide range of estimates may be due to differential effects of government spending across different periods of time, across different places or to heterogeneous treatment effects across various types of government spending [39].

This paper offers new evidence on the effectiveness of government spending. It is the first to estimate the cost per job and the local multiplier associated with construction spending.[1] It is also the first paper to examine the effects of the $25 billion Base Realignment and Closure (hereafter, BRAC) military construction program on local economies.[2] As such, it is quite relevant to the ongoing discussions about the Department of Defense's proposal of another BRAC in the near future.

In order to estimate the effect of government spending on local economies, I exploit variation in the timing and amount of construction funding provided by the 2005 BRAC across counties with military bases. The BRAC process realigned and closed some military installations to improve military efficiency and effectiveness.[3] A BRAC Commission was created to provide an objective and non-partisan analysis of military installations. It then produced a final, non-amendable recommendation list. The commission gave priority to military value during its selection process, and commissioners recused themselves from cases related to installations in their home states. Thus, to some extent, the funding awarded to each county most likely was motivated by military considerations and plausibly was unrelated to local economies.

My analysis identifies the causal effect of the stimulus on local economies under the identifying assumption that, changes in local economic conditions would have been the same across military counties absent the 2005 BRAC funding. Using county-level economic data from the Bureau of Economic Analysis' Regional Economic Accounts and a novel dataset that contains 2005 BRAC construction funding information, I find an estimated cost per job of $65,000 and a local fiscal multiplier—the change in local per

[1] A couple of studies have investigated the effects of public highway spending on economic outcomes. [40] studies the impulse response of state output to the Federal-Aid Highway Program, a program to fund construction, maintenance, and other improvements on a large array of public roads. [41] examines the effects of public infrastructure investment on output using a structural VAR and aggregate U.S. data.

[2] [42] also studies the effects of 2005 BRAC on local economies, but he focuses on the effects of personnel relocation as opposed to construction funding. As shown in Section 3.4 of this paper, the observed effects are driven by construction funding, not personnel relocation. This paper explicitly examines the identifying assumption and explores the treatment effects dynamics, which are not feasible for [42] as it only uses one year of pre-treatment data (2005) and one year of post-treatment data (2011).

[3] I restrict the analysis to counties that did not experience any closure during the process in order to better investigate the effects of government stimulus, excluding disinvestment.

capita income produced by a one dollar change in per capita government spending—of 1.21. These estimates are robust to various model specifications and the empirical strategy passes falsification exercises. Furthermore, my industry-specific analysis reveals especially large effects on the construction industry, which is consistent with the nature of the program.

Traditional Keynesian model usually yields a larger prediction on multiplier when the economy is in slack; that is, when some factors of production are in idle. In this case, counties in slack may benefit more from government spending compared to other counties. To test this hypothesis, I use unemployment rate as a measure of "slack" in an economy and divide the funded counties into counties with higher and lower unemployment rates. Results from this analysis provide mixed evidence. The estimates suggest larger effects on income for counties with higher unemployment rates. However, the effects on employment are larger for counties with lower unemployment rates.

To further understand the regional impacts of government spending, I extend the analysis to directly measure the extent to which there are spillovers across neighboring counties. I find little evidence of any likely spillover effects. Furthermore, I investigate the extent to which there are spillovers on the construction industry for nearby counties, since the construction industry is directly affected by the program. These estimates are uniformly positive and larger, providing suggestive evidence that the BRAC funding leads to higher employment for construction workers nearby but has no effect on income for those workers.

In order to speak to the mechanisms at work, I also examine the extent to which the BRAC stimulus affects in-migration and out-migration. What if government spending attracts many high-ability migrants and they take the employment opportunities and drive up the average income? In this case, local residents may not gain from increased government funding because the benefits of the stimulus instead accrue to migrants. My analysis suggests that government spending has positive effects on in-migration and no effects on out-migration to the funded and nearby counties. However, the effects on migration are too small to explain the main results. This suggests that residents of the funded counties do benefit from the government spending.

It is important to note that the multiplier estimated in this paper is *local*, as opposed to the extensively studied *national* multiplier [43, 44, 45, 46, 47, 48, 49]. Estimating the local multiplier does not inform us directly about the magnitude of the national multiplier, which has been the focus of the literature and is of great policy concern. Indeed, spillovers and migration are more likely to occur within a country rather than across country borders [50]. When a stimulus does lead to spillovers, or attracts migration, the local multipliers may be larger or smaller than the national multiplier depending on the direction and magnitude of the effects. For instance, if there are positive spillovers across counties, then the local multiplier will not fully capture the effects of government spending and thus will be smaller than the national multiplier. On the other hand, if government spending attracts many migrants who take employment opportunities and drive up average income, then the local multiplier will be larger than the national multiplier. The fact that there is not a one-to-one mapping between local and national multipliers does not negate the policy relevance of local parameters. State and local governments spend over 3 trillion dollars every year, and the effects of regional spending are certainly of interest to local policy makers and residents. Thus, while this paper is only informative about national stimulus, it provides a direct estimate of the effectiveness of regional investment.

The rest of the paper is organized as follows. In the next section I discuss related literature. Section 3.3 provides some background on the 2005 Base Realignment and Closure. Section 3.4 describes the methodology and data used to analyze the causal impact of government spending on local economics. Section 3.5 presents empirical results of my analyses. Section 3.6 concludes and provides a discussion of the implications of the results.

3.2 Related Literature

For decades, macroeconomists have tried to model and estimate the national fiscal multiplier, a parameter that summarizes the effects of government spending at the national level. Yet, there is still considerable debate over its magnitude. The debate arises in part because theoretical models offer contradictory predictions.[4] Simple neoclassical models often yield a small multiplier (usually smaller than 0.5); [51] find that temporary spending financed by a distortionary tax could lead to a multiplier as small as -2.5. New Keynesian models, on the other hand, yield a wide range of predictions, depending crucially on the monetary policy. Less responsive monetary policy, such as the case of “zero lower bound," could yield a multiplier as large as 2.3 [52, 53, 54].[5] Difficulty in measuring the counterfactual only adds to the debate. To tackle this problem, this line of literature employs two major approaches. The first is to use a structural VAR model, which relies on structural assumptions and small changes in the assumptions can lead to large differences in the estimated multiplier [45, 55]. The second approach uses military spending associated with wars as potentially exogenous shocks [48, 43, 46, 56]. However, major episodes of war are rare, and other fiscal policies, patriotism, or capacity constraints that occur during the war could confound the estimates.

A recent stream of the empirical literature uses cross-sectional variation to estimate the local income multiplier and cost per job.[6] A number of these studies have used variation in the geographic distribution of funds under the ARRA. [58] use state-level formula-driven variation in the allocation of ARRA Medicaid spending; they estimate a cost per job of

[4][38] provides a thorough review of the leading theories on the effects of government spending and related empirical work.

[5]At the zero lower bound, the nominal interest rate is held constant while inflation drives the real interest rate down [38].

[6]See [57] for a thorough review on this line of literatures.

around \$25,000 and a local income multiplier of about 2. [59] uses a similar approach to estimate the overall employment effects of the total ARRA spending, but finds a state-level cost per job of around \$125,000 per year. The reason for differences in results between these two papers may be that medical spending and other types of expenditures have different effects on the economy. [39] also use state-level variation in the allocation of ARRA funding; they estimate a cost per job of \$107,000 with an implied income multiplier from 0.5 to 1. They also suggest that the overall results mask considerable variation for different types of spending. Low income assistance programs and infrastructure spending are found to be highly expansionary while grants to states for education do not seem to have created any additional jobs.

Other studies have explored different sources of variation to estimate these parameters. [60] use variation in federal spending that is induced by the fact that much direct federal spending, and transfer programs to a local area depend on population estimates which are exogenously “shocked" after each Decennial Census. They estimate a county-level cost per job of \$30,000 per year and income multiplier of 1.7 to 2. [61, 62] uses differences in returns to state pension funds as “windfall" shocks to state spending; he estimates a state-level income multiplier of 2.2 and a cost per job of around \$35,000 per year. [63] use variation across states in federal spending during the Great Depression and estimate an income multiplier of 1.1 and no significant effect on employment. [64] use state spending cuts induced by institutional rules on budget deficits to estimate a spending multiplier of 0.4. [65] use regional variation in military procurement spending to estimate a local multiplier of 1.5. Their theoretical model relates the estimated local multiplier to the traditional national multiplier; and their estimates fit well with an open economy New Keynesian model in which consumption and labor are complements. Finally, [66] use province-level variation in the temporary contractions in local public spending directed at combatting the Mafia in Italy to estimate a local multiplier of 1.5.[7] Overall, this line of literature offers a wide range of estimates for cost per job ($25,000–$125,000) and the income multiplier (0.4–2.2).

This paper complements the existing literature by providing the first estimates of cost per job and local multiplier based on a military construction program. This is of particular significance if heterogeneous treatment effects across various types of government spending may explain why we observe such a wide range of empirical estimates. Moreover, it provides guidance to policy makers as to how limited funding should be allocated across various types of expenditures. Furthermore, I use a recent spending episode (2006–2011) which is relevant to current policy if the effects of public spending vary across periods. In addition, this research adds to the literatures on the effects of the BRAC process on various outcomes [68, 69, 70, 71]. Because the Department of Defense is proposing another round of the BRAC in the near future and a sizable amount of money is at stake, it is valuable that this paper sheds light on the stimulus benefits of the program itself.

3.3 Base Realignment and Closure

Base Realignment and Closure (BRAC) is a congressionally authorized process that the Department of Defense has used to reorganize its base structure [72]. Its goal is to more efficiently and effectively support the armed forces and to enhance operational readiness. The Defense Authorization Amendments and Base Closure and Realignment Act, enacted on October 24, 1988, provided the basis for implementation of the 1988 BRAC recommendations. On November 5, 1990, President George H. W. Bush signed the Defense Base Closure and Realignment Act of 1990, an attempt to isolate political influence from military activity. This act established an independent commission, the Defense Base

[7] [67] uses an instrumental variable approach to investigate how stimulus-motivated federal funding directed to universities affects the economies of the counties in which the institutions are located, but they find little evidence of a stimulus effect, which could be due to the fact that only a small fraction of the funding “stuck where they hit".

Closure and Realignment Commission, to ensure a timely, independent, and fair process for closing and realigning U.S. military installations. Since then, there have been four additional BRACs in 1991, 1993, 1995, and 2005. The 2005 BRAC cost around 35 billion dollars, more than the sum of the previous 4 BRACs, and thus provides a good opportunity for examining the local effects of stimulus.

On May 13, 2005, the Department of Defense issued the initial recommendation list for the 2005 Base Realignment and Closure. An independent panel of nine commissioners was created to provide an objective and non-partisan review and analysis of that list. It then produced a final non-amendable recommendation list.[8] During this selection process, the commission followed eight selection criteria and it gave priority to military value.[9] To enhance the impartiality and integrity of the BRAC process, commissioners recused themselves from cases related to installations in their home states.[10] President George W. Bush approved the 2005 BRAC commission's recommendation on September 15, 2005 with a statutory deadline of September 15, 2011. Given the process by which decisions were made, it is likely that the funding awarded to each county was motivated by military considerations and not local economic conditions. I provide graphical evidence and conduct a robustness check to support this hypothesis in Section 3.4. First, I show that funded and non-funded military counties did not have divergence in economic conditions prior to the first year of funding. Then, I perform a robustness check to show that the estimates are robust to the exclusion of states that were connected to the 2005 BRAC Commission; that is, the estimates remain robust after I drop states in which any of the commissioners were

[8] The 2005 BRAC commission consists of Anthony J. Principi, James H. Bilbray, Philip Coyle, Harold W. Gehman, Jr., James V. Hansen, T. Hill, Lloyd W. Newton, Samuel K. Skinner, Sue E. Turner.

[9] See Appendix A.1 for a full list of selection criteria.

[10] Four commissioners have recused themselves from cases relating to installations in their home states. Commissioners Coyle and Gehman recused themselves because of their participation in BRAC-related activity in California and Virginia respectively. Commissioners Bilbray and Hansen recused themselves because of their long-time representation in the Congress and other public offices of Nevada and Utah respectively (www.brac.gov).

born or have worked.

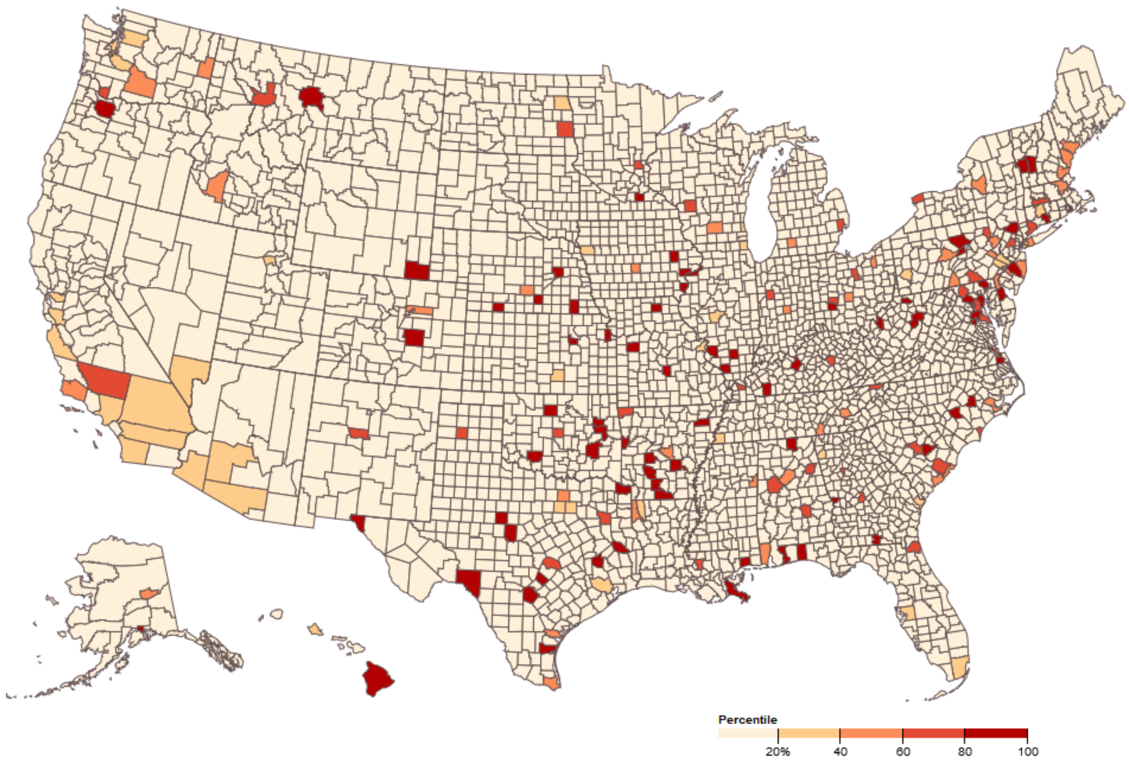

Figure 3.1: Geographic Distribution of Annual BRAC Funding per Capita

Source: 2013 Base Realignment and Closure Commission Execution Report

The 2005 BRAC cost over 35 billion dollars; military construction alone cost around 25 billion dollars. Figure 3.1 presents the geographic distribution of the average annual per capita construction funding across the United States. The mean of the average annual per capita funding is roughly \$122 for the funded counties. Almost every state has counties that received construction funding. In addition to funding construction across the nation, the 2005 BRAC relocated around 200,000 military and civilian personnel. The average net change in direct jobs was a net loss of 27 jobs per installation throughout the process

[42]. In Section 3.4, I conduct a robustness check and show that the main results are not due to personnel relocation. This is not surprising, as net personnel change only accounts for a tiny portion of county population.

3.4 Data and Methodology

3.4.1 Data

The 2005 Base Realignment and Closure construction funding data come from the 2013 Base Realignment and Closure Commission Execution Report [73]. It summarizes the costs and savings for each the Department of Defense Component throughout the six-year BRAC implementation period (2006–2011). It also lists detailed annual construction funding information at the installation level. I geocode and aggregate this information into county-level funding information. To distinguish between the effects of personnel relocation and construction funding, I obtain county-level annual personnel counts for the years 2002 through 2011 from the U.S. Base Structure Reports. This administrative report is published annually by the Department of Defense, and lists annual installation-level personnel counts for installations of at least ten acres and at least $10 million in "Plant Replacement Value."[11] The personnel counts include military personnel, federal civilian employees and other non-military personnel, such as contractor personnel. I geocode and aggregate these personnel counts to county-level counts.

The data on county-level economic activity come from the Bureau of Economic Analysis' (BEA) Regional Economic Accounts [74]. These data provide a wealth of economic statistics at the county level, including per capita income and employment.[12] The dataset

[11] I use 2003–2012 Base Structure Reports because reports published in year t+1 would provide information on the personnel counts in year t. After 2012, the annual report changes format and stops listing personnel counts at the base level. "Plant Replacement Value" represents the Military Service or Washington Headquarter Service calculated cost to replace the current physical plant (facilities and supporting infrastructure) using today's construction cost (labor and materials) and standards (methodologies and codes).

[12] BEA reports data on a calendar year basis, whereas Department of Defense reports funding on a fiscal year basis. For instance, fiscal year 2005, begins on October 1, 2004 and ends on September 30, 2005. I match fiscal year directly with calendar year in the empirical analysis. This matching procedure should yield

comes from a variety of administrative sources. Employment and wage data are from the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW) [75]. It reports on workers covered by the state Unemployment Insurance (UI) program and federal workers covered by the Unemployment Compensation for Federal Employees (UCFE) program. The BEA then adjusts these data for employment and wages not covered, or not fully covered, by these programs to provide more comprehensive measures of income and employment. The Regional Economic Accounts also have annual information on economic activity for large industries, such as construction and manufacturing, at the county level.

To further investigate whether migrants drive the main results, I use county-level migration data from the Internal Revenue Service (IRS) Statistics of Income [76]. The IRS tracks inflows and outflows based on address changes of individual tax filers. I use these data to construct measures of the number of individuals and households moving into and out of a particular county and to evaluate how the 2005 BRAC construction funding affects in-migration and out-migration.

Table 3.1 presents summary statistics based on these data. County-level per capita income and employment average around $33,000 and 120,000 across treatment and control counties respectively over the sample period. Construction employment accounts for around 6% of all employment in these counties. Military personnel account for about 1.24% of the overall population in those counties, more than the U.S. average of around 0.78%.[13]

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a relatively conservative estimate.

[13] Currently, there are 1.4 million active military personnel and 1.1 million reserve personnel in the United States.

Variable	N	Mean	S.D.
Panel A: Outcome Variables			
Employment	5,370	127,161	199,438
per Capita Income	5,370	33,917	8,235
Employment: Construction Industry	5,163	7,630	11,854
Personal Income: Construction Industry	5,168	404,793	742,842
Number of Households: In-migration	5,449	5,429	7,879
Number of Individual: In-migration	5,449	10,072	14,219
Number of Households: Out-migration	5,449	5,334	7,840
Number of Individuals: Out-migration	5,449	9,941	14,476
Panel B: Control Variables			
Population	5,370	214,354	32,6572
Percentage of Females	5,370	0.504	0.020
Percentage of Hispanics	5,370	0.093	0.136
Percentage of African Americans	5,370	0.105	0.136
Number of Personnel	4,108	2,654	7,809

Table 3.1: Descriptive Statistics

Note: The sample includes counties receiving the construction funding and counties with at least one military base reported in 2005 Base Structure Report.

3.4.2 Methodology

To identify the effects of government spending on local economies, I exploit variation in the timing and amount of BRAC construction funding across counties. Specifically, I estimate the following model:

$$Y_{it} = \alpha + \beta perCapitaFunding_{it} + \mu_i + \eta_t + X_{it} + \delta_{st} + u_{it}$$

where *i* indicates counties, *t* indicates years, and *s* indicates states. In this model, Y_{it} is a measure of the county economic condition; X_{it} refers to county-level time-varying demographic controls, including population, percentage Hispanic, African American, and female; and δ_{st} represents state-by-year fixed effects.[14] The county and year fixed effects are captured by μ_i and η_t, respectively. The county fixed effects control for county-level time-invariant characteristics and year fixed effects controls for nationwide economic shocks in any year. Moreover, I also include state-by-year fixed effects to capture state-level economic shocks in any given year. The inclusion of state-by-year fixed effects allows counties in different states to follow different trajectories and account for differential shocks by state over time. In this case, the crucial assumption is that in the absence of the 2005 BRAC construction funding, changes in economic condition would have been the same across all military counties *in the same state*. The variable of interest is $perCapitaFunding_{it}$, which measures 2005 BRAC construction funding at year *t* in county *i*.

Noted that while the BRAC report provides data on funding awarded, information on when the funding was spent is not available. Thus, I assume that all funding received by a county was spent linearly beginning in the year of receipt. That is, $perCapitaFunding_{it}$ equals zero prior to the receipt of any funding for county i and equals $\frac{\overline{Funding_{it}}}{Population_{i,2005}}$ for years after the initial funding receipt.[15] I make that assumption for two reasons. First, according to the Department of Defense's policy, military construction funding can remain available for up to five years. Second, most installations completed their projects in 2011, even though the Department of Defense distributed most of the funding between 2007 and 2009, and few counties received funding in 2011 [42].[16] The estimate of β identifies the

[14] The county-level demographic data come from the U.S. Census Bureau.

[15] Each BRAC construction project should be at least 35-percent design complete to request funding from the Department of Defense, which generates variation in the timing of counties' first funding receipt. And $\overline{Funding_{it}}$ is defined as $\frac{TotalFunding_i}{2011-firstyearoffundingreceipt+1_i}$ for years after the initial funding receipt. I use population counts in 2005 to generate the treatment variable because population could be affected by government spending and 2005 is the last year prior to the BRAC construction program. I also present estimates where $perCapitaFunding_{it}$ equals zero prior to the receipt of any funding for county i and equals $\frac{\overline{Funding_{it}}}{Population_{it}}$ for years after the first year of funding receipt in Appendix Table A1. The results are robust to this exercise.

[16] Another assumption could be that each individual funding awarded to a county at a given year is spent equally between that year and 2011. For instance, County A receive M million dollars in 2008 and N million dollars in 2010, so spending for County A would be $\frac{M}{4}$ million dollars in 2008 and 2009, and $\frac{M}{4}+\frac{N}{2}$ million dollars in 2010 and 2011. The results are robust to this alternative assumption of spending pattern and are

causal effect of 2005 BRAC funding under the identifying assumption that, in the absence of the BRAC construction funding, the change in outcomes across counties would have been the same. Finally, standard errors are clustered at the state level to allow for arbitrary correlation of the error term at the state level across counties and years.

In reality, counties with military installations are likely to be systematically different from counties that do not have installations. For this reason, I restrict my sample of unfunded counties to those with at least one military installation in the 2005 Base Structure Report, the administrative report on military installations that is published annually by the Department of Defense.[17] I further restrict the overall sample to counties that did not experience any closure during the 2005 BRAC to better investigate the effects of government spending, excluding disinvestment. The sample period is 2002–2011 for the main analysis. I use 2002 because it is the first year after the completion of the previous BRAC and 2011 because it is the statutory deadline for completion of the 2005 BRAC. When I explore treatment effects over time, I extend the sample period through 2013 to investigate whether the funding has lingering effects.

3.5 Results

In this section, I begin by presenting my main results. They are followed by robustness checks verifying that these results are robust under alternative identification strategies, to the exclusion of states linked to the 2005 BRAC Commission, and that they are not driven by personnel relocation. Then, I test the hypothesis that the effects of government spending are larger during periods of slack and examine the heterogeneous treatment effects of government spending across high and low unemployment counties. Next, I extend my analysis to explore the extent to which there are spillovers on the nearby counties. Finally, I examine the effects on migration to investigate whether migrants are responsible for the

shown in Appendix Table A2.

[17] I use the 2005 report because it is the last one published prior to the 2005 BRAC.

main results.

3.5.1 Main Results

	(1)	(2)	(3)	(4)
Panel A: Log(Employment)				
BRAC Funding per Capita ($1,000s)	0.036***	0.038***	0.030**	0.033***
	(0.007)	(0.007)	(0.013)	(0.009)
N	5,370	5,370	5,370	5,370
Panel B: Log(Per Capita Income)				
BRAC Funding per Capita ($1,000s)	0.056***	0.050**	0.045***	0.038***
	(0.019)	(0.020)	(0.011)	(0.012)
N	5,370	5,370	5,370	5,370
Cost per Job	57,563	54,533	69,075	64,795
Income Multiplier	1.79	1.59	1.44	1.21
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table 3.2: Estimated Effects of 2005 BRAC Construction Funding on Local Economic Conditions

Note: *** p<0.01, ** p<0.05, * p<0.1 All regressions include county and year fixed effects. County-level controls include population, percentage of female, Hispanics and African Americans. Standard errors are clustered at the state level. Control counties are counties with at least one military base reported in base structural report in 2005, the last report published prior to the 2005 BRAC. Estimates are unweighted. The cost per job is calculated as $\frac{\$1,000 \times \text{pre-funding average of population size for funded counties}}{\text{estimated percentage change} \times \text{pre-funding average of employment for funded counties}}$ and the local fiscal multiplier is calculated as $\frac{\text{estimated percentage change} \times \text{pre-funding average of per capita income for funded counties}}{\$1,000}$.

Table 3.2 presents my main results with Panel A showing the estimated effects on employment and Panel B on income. Column 1 shows estimates from the baseline specification, simply controlling for county and year fixed effects. The results from this specification suggest that a $1,000 increase in annual per capita BRAC construction funding would increase employment by 3.6%, or roughly 5,100 jobs.[18] This implies a cost per job

[18]This number is calculated by multiplying 3.8% by 134,580, the pre-funding average of employment for funded counties.

of approximately $57,563, because it takes roughly 279 million dollars ($1,000 per capita spending multiplied by 278,884, which is the pre-funding population average for funded counties) to create these jobs. Similarly, the estimate implies that a $1,000 increase in annual per capita BRAC funding would increase per capita income by 5.6%. Multiplying this number by $31,891—the pre-funding average of per capita income in the funded counties—yields an increase of roughly $1,790. This estimate implies a fiscal multiplier of 1.79.

Column 2 adds county-level time-varying demographic controls to the model. Adding these covariates may be over-controlling, because county-level demographic characteristics could be affected by government spending and thus a causal path between local stimulus and economic conditions. So it is unclear whether the estimates in Column 2 are superior to those in Column 1. Nonetheless, the results change little after adding these controls. These estimates imply a cost per job of $54,533, and a fiscal multiplier of 1.59.

In Column 3, I present the results of a specification in which I control for county, year, and state-by-year fixed effects. Adding state-by-year fixed effects to the model controls for statewide economic shocks. The estimates from this specification suggest that a $1,000 increase in annual per capita funding would increase employment by 3.0% and per capita income by 4.5%, implying an estimated cost per job of around $69,075 and income multiplier of 1.44. Finally, in Column 4 I present a specification in which I control for state-by-year fixed effects and county-level time-varying controls. The results change little, with an estimated cost per job of approximately $64,795 and an income multiplier of 1.21. To summarize, the estimates in Table 3.2 provide strong evidence that the BRAC construction funding had a significant impact on employment and income for funded counties, and these estimates are robust to various model specifications.

As an additional way to estimate the effects of government spending on local economies, I investigate the dynamic responses of economic conditions to 2005 BRAC construction

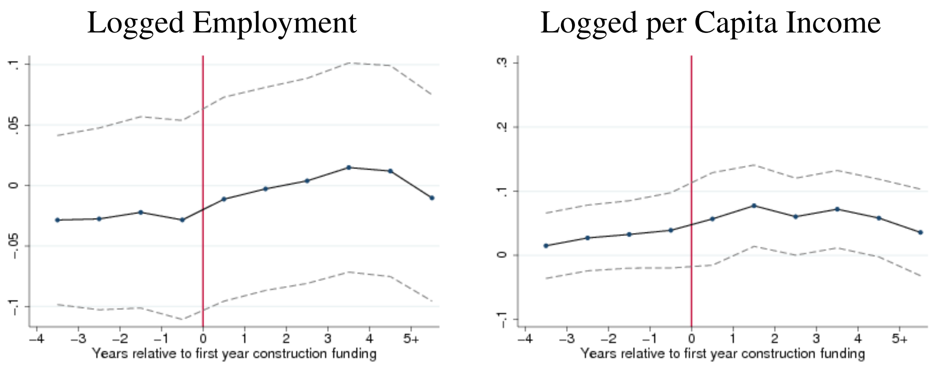

Figure 3.2: Estimated Effects of 2005 BRAC Construction Funding, Lags and Lead

Note: Data to generate these figures are from BEA. These figures present the coefficient estimates from a model that includes county fixed effects, year fixed effect, state-by-year fixed effects and county-level time-varying controls. County-level controls include population, percentage of female, Hispanics and African Americans. Standard errors are clustered at the state level. Control counties are counties with at least one military base reported in base structural report in 2005, the last report published prior to the 2005 BRAC. Estimates are unweighted.

funding. To do that, I interact average per capita funding with a set of indicator variables that correspond to 1, 2, 3, 4 years prior to the first year of funding receipt, the first year of funding receipt, and 1, 2, 3, 4, 5-or-more years after the first year of funding receipt. As in Column 4 of Table 3.2, I continue to control for county, year, state-by-year fixed effects and county-level time-varying demographic characteristics. Figure 3.2 plots the coefficient estimates and 95% confidence intervals from this analysis. None of the coefficient estimates for the years leading up to the funding receipt are statistically significant at the 5% level, supporting the common trends assumption. Furthermore, the estimates for years after the first year of funding receipt provide some evidence that the effects on employment and income are concentrated in the short term and it fades away once the funding is discontinued.

Finally, because the construction industry is directly affected by BRAC, we might expect there to be larger effects on this industry. I investigate this hypothesis and present the results in Table 3.3. Estimates from this industry-specific analysis are uniformly larger

	(1)	(2)	(3)	(4)
Panel A: Log(Employment)				
Annual BRAC Funding per Capita	0.139***	0.138***	0.111***	0.108***
	(0.027)	(0.024)	(0.036)	(0.033)
Observations	5,163	5,163	5,163	5,163
Panel B: Log(Personal Income)				
Annual BRAC Funding per Capita	0.203***	0.202***	0.142**	0.138***
	(0.038)	(0.036)	(0.053)	(0.049)
N	5,168	5,168	5,168	5,168
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table 3.3: Estimated Effects of 2005 BRAC Construction Funding on Construction Industry

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. All regressions include county and year fixed effects. County-level controls include population, percentage of female, Hispanics and African Americans. Standard errors are clustered at the state level. Control counties are counties with at least one military base reported in base structural report in 2005, the last report published prior to the 2005 BRAC. Estimates are unweighted.

than the main results, suggesting that BRAC construction funding does have an especially large effect on the construction industry. Results from the specification where I control for county, year, state-by-year fixed effects and county-level time-varying demographic characteristics indicate that a $1,000 increase in per capita funding increases construction employment by 10.8% and personal income by 13.8%.[19] Compared with the main results—a 6.8% increase in overall employment and 3.8% increase in per capita income—these estimates are consistent with the hypothesis that the BRAC funding has a larger impact on the construction industry.

3.5.2 Robustness Checks

This section presents several tests to check the robustness of the main results. I begin by presenting results where I drop all unfunded counties from the analysis and only use

[19]BEA publish personal income as opposed to per capita income for large industries and population counts by industry is unavailable, so I focus on personal income instead of per capita income in the industry-specific analysis.

variation in the timing and amount of funding among funded counties. Next, I extend the analysis to explore the extent to which dropping states linked to the 2005 BRAC Commission affects the results. Finally, I investigate whether the effects are driven by military personnel relocation or the stimulus.

	(1)	(2)	(3)	(4)
Panel A: Log(Employment)				
BRAC Funding per Capita ($1,000s)	0.026***	0.030***	0.029**	0.034***
	(0.009)	(0.007)	(0.015)	(0.008)
N	1,250	1,250	1,250	1,250
Panel B: Log(Per Capita Income)				
BRAC Funding per Capita ($1,000s)	0.057***	0.048**	0.049***	0.039***
	(0.021)	(0.018)	(0.010)	(0.007)
N	1,250	1,250	1,250	1,250
Cost per Job	79,701	69,075	71,457	60,948
Income Multiplier	1.82	1.53	1.56	1.24
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table 3.4: Estimated Effects of 2005 BRAC Construction Funding on Local Economic Conditions: Restricting Analysis to Funded Counties

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$ These estimates utilize variation in the timing and amount of funding awarded within funded counties to estimate the results. All regressions include county and year fixed effects. County-level controls include population, percentage of female, Hispanics and African Americans. Standard errors are clustered at the state level. Estimates are unweighted. The cost per job is calculated as $\frac{\$1,000 \times \text{pre-funding average of population size for funded counties}}{\text{estimated percentage change} \times \text{pre-funding average of employment for funded counties}}$ and the local fiscal multiplier is calculated as $\frac{\text{estimated percentage change} \times \text{pre-funding average of per capita income for funded counties}}{\$1,000}$.

In Table 3.4, I present the estimates based only on funded counties. This estimation strategy compares counties receiving lower levels of construction funding per capita to counties that receive more funding per capita. Each column in Table 3.4 follows the same specification as Table 2. The estimates are similar to those presented in Table 2; Column 4 indicates a 3.4% increase in employment and a 3.9% increase in per capita income for a

$1,000 increase in annual per capita funding. That implies a cost per job of around $60,948 and an income multiplier of 1.24. The estimates remain robust when I use an alternative source of variation, lending further support to the main results.

	(1)	(2)	(3)	(4)
Panel A: Log(Employment)				
BRAC Funding per Capita ($1,000s)	0.032***	0.031***	0.034***	0.035***
	(0.008)	(0.006)	(0.010)	(0.008)
N	3,390	3,390	3,390	3,390
Panel B: Log(Per Capita Income)				
BRAC Funding per Capita ($1,000s)	0.050**	0.042**	0.045***	0.036***
	(0.019)	(0.018)	(0.013)	(0.013)
N	3,390	3,390	3,390	3,390
Cost per Job	70,637	72,915	66,482	64,582
Income Multiplier	1.65	1.39	1.49	1.19
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table 3.5: Estimated Effects of 2005 BRAC Construction Funding on Local Economic Conditions: Omitting States Linked to the 2005 BRAC Commission

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$ All regressions include county and year fixed effects. County-level controls include population, percentage of female, Hispanics and African Americans. Standard errors are clustered at the state level. Control counties are counties with at least one military base reported in base structural report in 2005, the last report published prior to the 2005 BRAC. Estimates are unweighted. Counties in state linked to the 2005 BRAC Commission are dropped from the analysis. The cost per job is calculated as $\frac{\$1,000 \times \text{pre-funding average of population size for funded counties}}{\text{estimated percentage change} \times \text{pre-funding average of employment for funded counties}}$ and the local fiscal multiplier is calculated as $\frac{\text{estimated percentage change} \times \text{pre-funding average of per capita income for funded counties}}{\$1,000}$.

In Section 3.3, I discussed the institutional background of the 2005 BRAC program, arguing that political factors played a small role in the BRAC process and that funding was mainly motivated by military considerations. The analysis above supports this argument: it shows no evidence of economic divergence *prior to* the receipts of BRAC construction funding. None of the coefficient estimates for the years leading up to the funding award are statistically significant. However, this does not rule out the possibility that commissioners may have voted in favor of their connected states and that political factors may affect the BRAC process in a significant way. It is important to note that this sort of behavior would only bias the estimates if it was related to expected economic outcomes.

In any case, to address this concern, I conduct a robustness check by dropping states where any of the commissioners were born or had worked at any time throughout their career.[20] If the estimates change substantially after dropping those counties, then it would raise concerns about political involvement during the BRAC process. The estimates presented in Table 3.5 remain close to those shown in Table 2: estimates from Column 4 imply a cost per job of around $64,582 and a local fiscal multiplier of 1.19, This suggests little evidence of political manipulation that is systematically related to economic conditions.[21]

Because military personnel were relocated during 2005 BRAC process, the main results could be driven by more military personnel moving to the funded counties. To address this concern, I include logged military personnel counts as a covariate in my analysis. I present the estimates in Table 3.6. When I control for military personnel, the estimated effects of BRAC construction funding change little. Estimates from Column 4 imply a cost per job of approximately $57,166 and an income multiplier of 1.21. Furthermore, the estimates on logged personnel counts are small and never statistically significant, reassuring us that the main effects indeed are driven by funding, not personnel relocation. This finding is consistent with the fact that the number of military personnel in a county only accounts for a small portion of the population, and on average there is only a net loss of 27 jobs per installation throughout the process [42].[22]

[20] I drop the following states in this analysis: Arizona, California, Georgia, Hawaii, Illinois, Maryland, Mississippi, Nebraska, Nevada, New York, Ohio, South Carolina, Texas, Utah, Virginia, Washington.

[21] The estimated cost per job is calculated as $\frac{\$1{,}000\times 258{,}471}{3.5\%\times 140{,}167}$, and the local fiscal multiplier is calculated as $\frac{\$33{,}070\times 3.6\%}{\$1{,}000}$.

[22] The estimated cost per job is calculated as $\frac{\$1{,}000\times 343{,}123}{3.8\%\times 167{,}397}$, and the local fiscal multiplier is calculated as $\frac{\$32{,}672\times 3.7\%}{\$1{,}000}$.

	(1)	(2)	(3)	(4)
Panel A: Log(Employment)				
BRAC Funding per Capita ($1,000s)	0.037***	0.038***	0.038***	0.038***
	(0.007)	(0.007)	(0.008)	(0.007)
Logged(Personnel)	-0.001	-0.002	-0.002	-0.003
	(0.002)	(0.001)	(0.002)	(0.002)
N	4,108	4,108	4,108	4,108
Panel B: Log(Per Capita Income)				
BRAC Funding per Capita ($1,000s)	0.052***	0.045**	0.044***	0.037***
	(0.018)	(0.018)	(0.012)	(0.013)
Logged(Personnel)	0.002	0.002	0.002	0.002
	(0.002)	(0.001)	(0.002)	(0.002)
N	4,108	4,108	4,108	4,108
Cost per job	58,710	57,166	57,166	57,166
Income Multiplier	1.70	1.47	1.44	1.21
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table 3.6: Estimated Effects of 2005 BRAC Construction Funding on Local Economic Conditions: Considering Personnel Relocation

Note: *** p<0.01, ** p<0.05, * p<0.1 All regressions include county and year fixed effects. County-level controls include population, percentage of female, Hispanics and African Americans, and natural log of military personnel counts. Standard errors are clustered at the state level. Control counties are counties with at least one military base reported in base structural report in 2005, the last report published prior to the 2005 BRAC. Estimates are unweighted. The cost per job is calculated as $\frac{\$1,000 \times \text{pre-funding average of population size for funded counties}}{\text{estimated percentage change} \times \text{pre-funding average of employment for funded counties}}$ and the local fiscal multiplier is calculated as $\frac{\text{estimated percentage change} \times \text{pre-funding average of per capita income for funded counties}}{\$1,000}$.

Overall, these robustness checks indicate that the main results are robust under alternative identification strategies. Also, we observe little political manipulation related to economic conditions during the BRAC process, which lends further support to the identification strategy. Also, the main results are driven by the funding, not by relocation of military personnel. All of this evidence supports an interpretation of the main results as cost per job and income multiplier.

3.5.3 Heterogeneous Effects

The traditional Keynesian model implies larger multipliers when the economy is in slack; that is, when some production factors are in idle.[23] This theoretical prediction implies that we shall observe larger effects on income and employment for counties that have idle productive capacity. If so, government spending may have a redistributional effect in addition to its stimulating effects: areas in slack would benefit more from the same amount of government spending than other areas. In this section, I investigate this issue by examining the degree to which the effects of BRAC funding on income and employment differ by the amount of slack in the local economy as measured by the unemployment rate. Specifically, I divide the funded counties into two groups based on their unemployment rates in the year of 2005, the last year prior to any counties receiving the BRAC construction funding. Table 3.7 presents results from this analysis. Panel A presents the results for counties with higher unemployment rates while Panel B for those with lower unemployment rates in 2005. Contracted to the theoretical prediction, the point estimates for employment effects are larger for counties with lower unemployment rates, which could be due to the fact that counties with lower unemployment rates in the sample are more likely to be larger counties with more job opportunities. The effects on income, on the other hand, provide some evidence to support the slack hypothesis—economies in slack seems to gain more from federal spending. The estimated multiplier for counties with lower unemployment rates is smaller relative to the counties with higher unemployment rates and less robust to alternative model specifications.

[23]Empirical evidence from the macroeconomics literature yields contrasting results on whether government spending multipliers are larger during periods of slack. [77] and [78] find evidence of larger multipliers during periods of slack, while [79], [80], and [81] do not observe higher multipliers during times of slack.

	(1)	(2)	(3)	(4)
Panel A: High Unemployment Rate				
Panel A1: Log(Employment)				
BRAC Funding per Capita ($1,000s)	0.032***	0.031***	0.029*	0.030**
	(0.006)	(0.005)	(0.015)	(0.012)
N	4,750	4,750	4,750	4,750
Panel A2: Log(Per Capita Income)				
BRAC Funding per Capita ($1,000s)	0.058**	0.048**	0.047***	0.038***
	(0.023)	(0.021)	(0.014)	(0.013)
N	4,750	4,750	4,750	4,750
Cost per Job	70,660	72,939	77,968	75,371
Income Multiplier	1.72	1.42	1.39	1.12
Panel B: Low Unemployment Rate				
Panel B1: Log(Employment)				
BRAC Funding per Capita ($1,000s)	0.074**	0.089***	0.052*	0.063***
	(0.032)	(0.027)	(0.028)	(0.022)
N	4,740	4,740	4,740	4,740
Panel B2: Log(Per Capita Income)				
BRAC Funding per Capita ($1,000s)	0.036	0.060*	0.025	0.029
	(0.033)	(0.033)	(0.015)	(0.018)
N	4,740	4,740	4,740	4,740
Cost per Job	25,931	21,561	36,903	30,459
Income Multiplier	1.23	2.06	0.86	0.99
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table 3.7: Estimated Effects of 2005 BRAC Construction Funding on Local Economic Conditions: High vs Low Unemployment Rate Counties

Note: *** p<0.01, ** p<0.05, * p<0.1 . Data on migration pattern are from IRS county migration data. All regressions include county and year fixed effects. County-level controls include population, percentage of female, Hispanics and African Americans. Standard errors are clustered at the state level. Control counties are counties with at least one military base reported in base structural report in 2005, the last report published prior to the 2005 BRAC. Estimates are unweighted. The cost per job is calculated as $\frac{\$1,000 \times \text{pre-funding average of population size for funded counties}}{\text{estimated percentage change} \times \text{pre-funding average of employment for funded counties}}$ and the local fiscal multiplier is calculated as $\frac{\text{estimated percentage change} \times \text{pre-funding average of per capita income for funded counties}}{\$1,000}$.

3.5.4 Spillover Effects

To further examine the effects of the BRAC construction funding, I consider the impact on neighboring counties.[24] This extension of the baseline analysis helps to capture the total regional impact of the stimulus; government spending may create externalities for neighboring counties that did not directly receive BRAC funding. Positive spillovers across counties would suggest that the main analyses understate the total regional effects of the BRAC funding. Spillovers might arise, for example, if some construction materials are purchased from neighboring counties: this increase in demand for input would have positive effects on those counties. On the other hand, if we find negative spillovers on nearby counties, then the main results may overstate the regional impact of the stimulus. For example, if the BRAC funding leads to higher wages and attracts migrants from neighboring counties, then decreases in population could have negative effects on businesses in those counties, ultimately resulting in negative spillovers.

In order to estimate the spillover effects, I define neighboring counties as the 10 nearest counties based on highway distance between county centroids for every county in the main analysis.[25] Comparing counties near the funded counties to those near the unfunded military counties, my results are shown in Table 3.8. None the estimates are significant and there is little evidence of spillovers. The estimates are precise enough to rule out effects on employment on the order of 1% at the 5 percent significance level for a \$1,000 increase in per capita funding. Also the estimates on per capita income can rule out meaningful negative impact smaller than -0.6% at the 5 percent level.

Because the construction industry is directly affected, it is likely that the BRAC funding would lead to higher demand for construction workers in the funded counties, and would also drive up the income and employment for construction workers in the nearby

[24][60] also finds little evidence of spillovers across neighboring counties.

[25]The County-to-County Distance data are from the Center for Transportation Analysis.

	(1)	(2)	(3)	(4)
Panel A: Log(Employment)				
BRAC Funding per Capita ($1,000s)	-0.001	0.000	-0.008	-0.006
	(0.010)	(0.010)	(0.006)	(0.006)
N	24,010	24,010	24,010	24,010
Panel B: Log(Per Capita Income)				
BRAC Funding per Capita ($1,000s)	0.013	0.012	0.012	0.010
	(0.012)	(0.012)	(0.009)	(0.008)
N	24,010	24,010	24,010	24,010
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table 3.8: Spillover Effects of 2005 BRAC Construction Funding on Neighboring Counties

Note: *** p<0.01, ** p<0.05, * p<0.1 . All regressions include county and year fixed effects. County-level controls include population, percentage of female, Hispanics and African Americans. Standard errors are clustered at the state level. Control counties are counties with at least one military base reported in base structural report in 2005, the last report published prior to the 2005 BRAC. Estimates are unweighted. Nearby counties are selected according to highway distance.

	(1)	(2)	(3)	(4)
Panel A: Log(Employment)				
BRAC Funding per Capita ($1,000s)	0.033	0.031	0.030**	0.031**
	(0.021)	(0.020)	(0.014)	(0.013)
N	21,951	21,951	21,951	21,951
Panel B: Log(Personal Income)				
BRAC Funding per Capita ($1,000s)	0.048	0.045	0.030	0.031
	(0.039)	(0.038)	(0.028)	(0.027)
N	21,951	21,951	21,951	21,951
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table 3.9: Spillover Effects of 2005 BRAC Construction Funding on Construction Industry

Note: *** p<0.01, ** p<0.05, * p<0.1 . All regressions include county and year fixed effects. County-level controls include population, percentage of female, Hispanics and African Americans. Standard errors are clustered at the state level. Control counties are counties with at least one military base reported in base structural report in 2005, the last report published prior to the 2005 BRAC. Estimates are unweighted. Nearby counties are selected according to highway distance.

counties. For this reason, I also explore the spillover effects on the construction industry specifically. These results are shown in Table 3.9. The effects on employment are not robust across specifications, but the signs are uniformly positive, suggesting potential positive spillovers on employment in the construction industry.

3.5.5 Are Migrants Responsible for the Effects?

Are migrants are responsible for the main effects on employment and income?[26] If government spending does not lead to changes in migration, it would suggest that the effects we observe for the funded counties are likely driven by local residents, who benefit from increased government funding through higher incomes and more job opportunities. On the other hand, if government funding leads to higher wages and thus attracts many high-ability migrants, the observed effects on employment and income could be mainly driven by migrants. If this is the case, then the benefits of government spending accrue to the migrants instead of the local residents. To explore this question, I use IRS migration data and separately investigate the effects on in-migration and out-migration.

Table 3.10 shows the results of this analysis. The results suggest that government spending has a positive impact on in-migration to the funded counties, but little effect on out-migration. The estimate from a specification where I control for county, year, state-by-year fixed effects and county-level time-varying demographic characteristics indicate that a $1,000 increase in annual BRAC funding per capita attracts around 1,000 additional migrants.[27] Given the average population size of the funded counties (200,000), these additional migrants are not likely to have created an increase in per capita income of 4% as estimated in the main results. Similarly, because a $1,000 increase in per capita funding could increase employment by more than 5,000 as calculated from the main results,

[26] [82] finds positive migration effects of government spending, and [62] find little evidence of migration effects.

[27] This number is calculated as $6.7\% \times 13,807.22$, the pre-funding average of in-migrations for the funded counties.

	(1)	(2)	(3)	(4)
Panel A: Inflow				
Panel A1: Log(Households)				
BRAC Funding per Capita ($1,000s)	0.075***	0.074***	0.057*	0.060**
	(0.028)	(0.025)	(0.033)	(0.029)
N	5,369	5,369	5,369	5,369
Panel A2: Log(Individuals)				
BRAC Funding per Capita ($1,000s)	0.084**	0.080***	0.065*	0.067**
	(0.032)	(0.028)	(0.036)	(0.032)
N	5,369	5,369	5,369	5,369
Panel B: Outflow				
Panel B1: Log(Households)				
BRAC Funding per Capita ($1,000s)	0.023	0.029	0.022	0.028
	(0.022)	(0.020)	(0.025)	(0.023)
N	5,369	5,369	5,369	5,369
Panel B2: Log(Individuals)				
BRAC Funding per Capita ($1,000s)	0.027	0.031	0.028	0.033
	(0.024)	(0.022)	(0.028)	(0.025)
N	5,369	5,369	5,369	5,369
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table 3.10: Estimated Effects of 2005 BRAC Construction Funding on Migration

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Data on migration pattern are from IRS county migration data. All regressions include county and year fixed effects. County-level controls include population, percentage of female, Hispanics and African Americans. Standard errors are clustered at the state level. Control counties are counties with at least one military base reported in base structural report in 2005, the last report published prior to the 2005 BRAC. Estimates are unweighted.

the observed effects on employment are unlikely to be driven by migrants to the funded counties.

BEA measure employment and income based on place of work, but IRS migration files measure migration pattern based on change of addresses, a place of residence measure. So it is possible that migrants move to counties near funded counties and take the employment opportunities in the funded counties. To investigate this hypothesis, I examine the impact of the BRAC funding on migration for the nearby counties. Similar to my analysis on the

	(1)	(2)	(3)	(4)
Panel A: Inflow				
Panel A1: Log(Households)				
BRAC Funding per Capita ($1,000s)	0.026**	0.023*	0.017*	0.017*
	(0.012)	(0.013)	(0.010)	(0.009)
N	23,980	23,980	23,980	23,980
Panel A2: Log(Individuals)				
BRAC Funding per Capita ($1,000s)	0.035**	0.031**	0.026**	0.025**
	(0.014)	(0.015)	(0.013)	(0.012)
N	23,980	23,980	23,980	23,980
Panel B: Outflow				
Panel B1: Log(Households)				
BRAC Funding per Capita ($1,000s)	0.012	0.014	-0.002	0.002
	(0.010)	(0.010)	(0.013)	(0.012)
N	24,078	24,078	24,078	24,078
Panel B2: Log(Individuals)				
BRAC Funding per Capita ($1,000s)	0.016	0.016	-0.000	0.003
	(0.012)	(0.012)	(0.015)	(0.015)
N	24,078	24,078	24,078	24,078
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table 3.11: Spillover Effects of 2005 BRAC Construction Funding on Migration for Neighboring Counties

Note: *** p<0.01, ** p<0.05, * p<0.1 . Data on migration pattern are from IRS county migration data. All regressions include county and year fixed effects. County-level controls include population, percentage of female, Hispanics and African Americans. Standard errors are clustered at the state level. Control counties are counties with at least one military base reported in base structural report in 2005, the last report published prior to the 2005 BRAC. Estimates are unweighted.

spillover effects, I define neighboring counties as the 10 nearest counties based on highway distance between county centroids. Table 3.11 presents the results from this analysis. The results show that the stimulus has a positive effect on in-migration to the neighboring counties, but no effects on out-migration. The estimates on in-migration shows that a $1,000 increase in annual BRAC funding attracts around 2,000 additional migrants to the ten nearest counties.[28] Combining these results with those on the funded counties, a \$1,000 increase in annual BRAC funding attracts around 3,000 additional migrants in total. However, the effect on migration is still too small to have created an increase in per capita income of 4% and an additional 5,000 jobs for the funded counties. Thus, local residents do benefit from the stimulus.

3.6 Discussion and Conclusion

In this paper, I exploit variation across counties in the timing and amount of construction funding provided by the 2005 BRAC to estimate the effects of government spending on local economic conditions. My analysis yields an estimated cost per job of approximately \$65,000 and a local fiscal multiplier of around 1.21, and these estimates are robust to various model specifications. Furthermore, an industry-specific analysis finds especially large effects on the construction industry, which is consistent with the nature of the spending. To better understand the regional impact of the BRAC funding, I directly estimate spillover effects on neighboring counties. I find little evidence of spillovers; however, there is suggestive evidence of positive spillovers on construction employment for neighboring counties.

To test traditional Keynesian prediction that economies with higher amount of slack would gain more from government spending, I investigate if counties with higher unemployment rates benefit more from the stimulus. Results from this analysis are mixed. While the effects on income are larger for counties with higher unemployment rates, implying a larger multiplier for those counties, the effects on employment are larger for counties with lower unemployment rates.

Finally, to better understand how stimulus affects relocation decision, I also examine the effects of government spending on in-migration and out-migration. Government

[28]This number is calculated as $10 \times 2.6\% \times 78,00$, where 78,00 is the pre-funding average of in-migration for each of the nearby counties.

spending potentially could attract more in-migration into the funded areas, so the main estimated effects could be driven mainly by migrants. My analysis shows that government spending has positive effects on in-migration to funded counties and their neighboring counties but no effects on out-migration. In any case, the migration effects are too small to bring about the results I observe in the main analyses. Thus we can conclude that residents of the funded counties do benefit from the government spending.

This paper complements the existing literature on the regional impacts of stimulus by estimating the effects of a quite recent military construction program. The magnitude of the estimated cost per job falls in the middle of the distribution on the employment effects of government spending. Still, the income effects I estimate are modest compared to estimates based on other stimulus programs. I therefore conclude that government spending, especially construction spending, could play a significant role in creating jobs and increasing income. Because the construction industry is usually one of the hardest-hit during economic downturns [83], and my results suggest especially large effects for this sector, it seems that government investment in construction could effectively mitigate economic slowdowns. Furthermore, heterogeneous treatment effect analysis shows that this program has a larger income effects on counties with higher unemployment rates. Thus, to the extent that the federal government wants to redistribute resources to counties with more slack, the BRAC construction program provides an attractive approach for its impact on those counties and the fact that it allows the federal government to directly engage in stimulating the economy.

4. BETTER ECONOMY, MORE BABIES? NEW EVIDENCE ON THE EFFECTS OF ECONOMIC CONDITION ON CHILDBEARING

4.1 Introduction

Dating back to [84], economists have had an interest in understanding decisions associated with childbearing and how economic factors affect this decision-making process. In his first pathbreaking work on the topic, Gary Becker developed an economic framework treating children as consumer durables that provide utility to parents over time [3]. Becker recognized that this model seemed at odds with the negative relationship between household income and family size in the United States at the time; he surmised that it was driven by differences in knowledge about birth control, not because children were "inferior goods." A great deal of subsequent research was devoted to expanding on Becker's initial framework so that it could predict this negative relationship while maintaining that children were normal goods. Most of these works involved one of two theoretical mechanisms that work in the opposite direction of the pure income effect: a quantity-quality tradeoff or parental time use. Models involving a tradeoff between child quantity and child quality can generate a reduction in the demand for quantity in response to increase in income if the income elasticity for quality is high relative to the income elasticity for quantity; having children is more costly when parents plan to invest more in each child. In models that incorporate parental time into the production function for children, an increase in parental wages generates an income effect that increases the demand for children and a substitution effect that reduces demand through the effect on the opportunity cost of devoting time to have and raise children. Models emphasizing parental time use can also recognize differences in household roles: the substitution effect is more likely to dominate for wage shocks to primary caregivers (usually women) while the income effect is more likely to dominate for wage shocks other caregivers (usually men).

These types of models have been extremely influential in the way that economists think about household decision-making and the family. At the same time, there does not appear to be a clear consensus on which of the proposed mechanisms explains the observed negative relationship between household incomes and fertility, which at this point has been documented across households in various countries, across countries with regards to GDP per capita, and within countries over time as they experience economic development [85]. The main challenge, as pointed out by [86], is a lack of exogenous shock to household income.[1]

In this paper, we examine a recent large economic shock that affects mostly men's employment and wage. Specifically, we exploit variation in the timing and the amount of construction funding provided by the 2005 Base Realignment and Closure (BRAC) process. The 2005 BRAC spent 35 billion dollars between the years 2006 and 2011 to realign and close military installations to improve military efficiency and effectiveness, and 25 billion dollars are dedicated as construction funding.[2] Because of the institutional characteristics of the program, the funding awarded to each county most likely was motivated by military considerations and plausibly was unrelated to local economies. [87] provides evidence to support this hypothesis and shows that the program has a large impact on county-level employment and per capita income. Moreover, [87] shows that the effects are particularly large for the construction industry. Since men account for more than 90% of construction employment, this intervention provides a potentially exogenous shock to men's labor market condition, providing a good opportunity to test whether children are "normal." Exploiting variation in the timing and amount of construction funding provided

[1] [86] also highlights that exogenous variation in the "price" of children is necessary to determine whether any quantity-quality tradeoff exists.

[2] We restrict the analysis to counties that did not experience any closure during the process because base closure may have its own impacts on birth rates through mental stress.

by the 2005 BRAC across counties with military bases, our analysis suggest that fertility increases with men's labor market condition, consistent with the theoretical prediction.[3]

Our analysis identifies the causal effect of the stimulus on childbearing under the identifying assumption that, changes in birth rates would have been the same across military counties absent the 2005 BRAC funding. Using gender-specific county-level employment and wage information from the Longitudinal Employer-Household Dynamics (LEHD) program's Quarterly Workforce Indicators (QWI) in conjunction with a novel dataset that contains 2005 BRAC construction funding information, we first demonstrate that a \$1,000 increase in per capita spending is associated with a 4.5% increases in men's employment and 2% increase in men's wage, but it has no effects on women's employment or wage. Then, using county-level birth rates data calculated from the National Center for Health Statistics' restricted-use natality file and population counts from the Surveillance, Epidemiology, and End Results Program (SEER), we find that a \$1,000 increase in per capita spending would lead to a 4.9% increase in birth rates for women aged 15 to 44. Taking together with the results on gender-specific labor market outcomes, these results suggest that a 4.5% increases in men's employment and 2% increase in men's wage would increase contemporaneous birth rates by 4.9%, consistent with the contention that children are "normal." These estimates are robust to various model specifications and the empirical strategy passes falsification exercises. Further investigation into the heterogeneous responses across age groups and racial groups suggests that the effects are larger for females aged 20 to 34 and Hispanics.

This paper joins a handful of studies that attempted to use exogenous shocks to disentangle the causal link. [88] uses father's job loss as a potential exogenous shock to household income and finds that father's income decreases reduce total fertility, supporting the

[3]Due to sample restriction, we only investigate the effects on contemporaneous fertility but not completed fertility. Therefore, it is possible that people would change the timing of fertility but not the total completed fertility in response to this intervention.

hypothesis that children are “normal."[4] [90] creates gender-specific shift-share indices to instrument for men’s and women’s labor market opportunity and concludes that improvement in men’s labor market condition has a positive effects on fertility while improvement in women’s has smaller negative effects on fertility, consistent with the prediction based on parental time allocation model. [91] uses household wealth shocks generated by the recent housing boom and bust to investigate the fertility effects of family wealth, and it shows that increased wealth has a positive effect on fertility. [92] uses the mid-1970s’ increase in world coal prices as an exogenous shock to men’s income in the Appalachian coal-mining region to investigate how father’s income increases affects fertility, and it shows that fertility increases with father’s income, suggesting that children are “normal". [93] exploits variation in the localized “fracking booms" across the U.S. and shows that it leads to higher wage for non-college-educated men. They then present evidence that the boom is associated with increased births, consistent with the hypothesis that children are “normal." [5]

The first advantage of our empirical strategy is the transparent and plausibly exogenous variation in men’s labor market conditions. As mentioned earlier, the 2005 BRAC process aims to improve military efficiency and effectiveness. To achieve this goal, a BRAC commission was created to conduct an objective and non-partisan review of military installations and produce a final, non-amendable recommendation lists. [87] conduct various falsification exercises to provide strong evidence that the funding decision was likely driven by military consideration instead of economic concerns. For instance, the

[4] [89] also uses father’s job loss as a potential exogenous shock to family income to investigate its impact on the timing of fertility and it concludes that father’s displacement delays first and second births but parents adjust their fertility in the long run.

[5] There also exists other empirical work that assesses the relationship between economic condition and fertility without an apparent source of exogenous variation. For instance, [94]study how fertility responds to the business cycle and finds that fertility is procyclical. Similarly, [95] uses cohort fixed effects to investigate the relationship between unemployment and fertility, and they finds that increase in unemployment rate is associated with a reduction in fertility.

results remain largely unchanged when commissioners-related states were dropped from the analysis, lending strong support to the claim. Moreover, empirical evidence shows no divergence in economic condition between military counties that received funding and military counties that did not receive funding. Due to the natural of the program and the fact that men accounts for approximately 90% of construction employment, this intervention thus provides a large and potentially exogenous shock to men's labor market outcomes.

Another advantage of this work is that we explicitly estimate how men's and women's labor market outcomes respond to the 2005 BRAC construction program, and concludes that men's employment and wage were affected while women's were not. Thus, it allows us to investigate how fertility changes with men's labor market condition, holding women's constant. This is fundamentally different from a scenario in which an economic shock impacts men *more than* women. If women also benefit from an exogenous economic shock, substitution effects are likely to attenuate the income effects assuming that females are the primary caregiver.

The rest of the paper is organized as follows. Section 4.2 provides some background on the 2005 Base Realignment and Closure. Section 4.3 describes the methodology and data used to analyze the causal impact of males' labor market opportunity on fertility. Section 4.4 presents empirical results of our analyses. Section 4.5 concludes and provides a discussion of the implications of the results.

4.2 Base Realignment and Closure

Base Realignment and Closure (BRAC) is a congressionally authorized process that the Department of Defense has used to reorganize its base structure [72]. Its goal is to more efficiently and effectively support the armed forces and to enhance operational readiness. On November 5, 1990, President George H. W. Bush signed the Defense Base Closure and Realignment Act of 1990, an attempt to isolate political influence from military activity. This act established an independent commission, the Defense Base Closure and Realignment Commission, to ensure a timely, independent, and fair process for closing and realigning U.S. military installations. Since then, there have been four additional BRACs in 1991, 1993, 1995, and 2005. The 2005 BRAC cost around 35 billion dollars, more than the sum of all previous rounds of BRACs, and 25 billion dollars are dedicated as construction funding.

On May 13, 2005, the Department of Defense issued the initial recommendation list for the 2005 Base Realignment and Closure. An independent commission of nine members was created to provide an objective and non-partisan review and analysis of that list. It then produced a final non-amendable recommendation list.[6] During this selection process, the commission followed eight selection criteria and it gave priority to military value.[7] To enhance the impartiality and integrity of the BRAC process, commissioners recused themselves from participation in matters related to installations in their home states.[8] President George W. Bush approved the 2005 BRAC commission's recommendation on September 15, 2005 with a statutory deadline of September 15, 2011. Figure 4.1 presents the geographic distribution of the average annual per capita construction funding across the United States. The mean of the average annual per capita funding is roughly $122 for the funded counties. Almost every state has counties that received construction funding.

Given the decision process, it is likely that the funding awarded to each county was motivated by military considerations and not local economic conditions. Zheng (2017) provides convincing evidence to support this hypothesis. Furthermore, since more than

[6] The 2005 BRAC commission consists of Anthony J. Principi, James H. Bilbray, Philip Coyle, Harold W. Gehman, Jr., James V. Hansen, T. Hill, Lloyd W. Newton, Samuel K. Skinner, Sue E. Turner.

[7] See Appendix A.1 for a full list of selection criteria.

[8] Four commissioners have recused themselves from participation in matters relating to installations in their home states. Commissioners Coyle and Gehman recused themselves because of their participation in BRAC-related activity in California and Virginia respectively. Commissioners Bilbray and Hansen recused themselves because of their long-time representation in the Congress and other public offices of Nevada and Utah respectively.

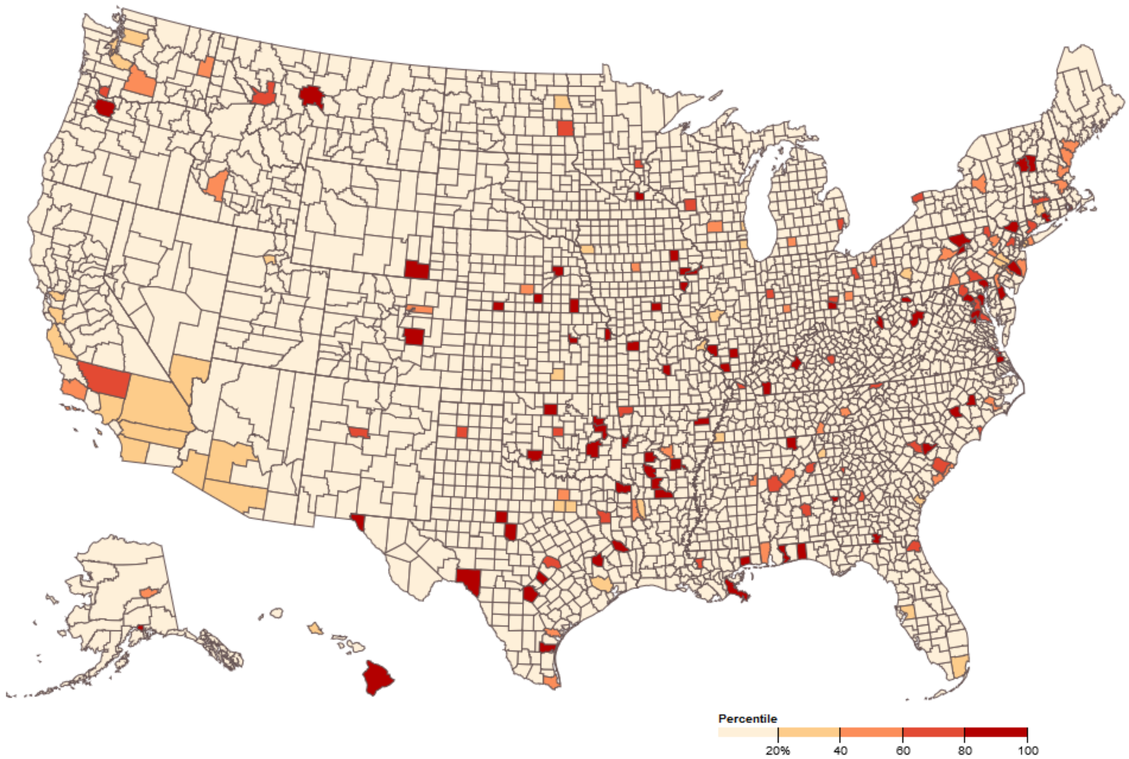

Figure 4.1: Geographic Distribution of Annual BRAC Funding per Capita by County

Source: 2013 Base Realignment and Closure Commission Execution Report

70% of all funding was dedicated as construction funding, and men accounts for approximately 90% of all employment in this industry, the intervention is likely to have larger impacts on men's labor market opportunities. Thus, this intervention provides a good opportunity to examine the theoretical prediction that children are “normal."

4.3 Data and Methodology

4.3.1 Data

The 2005 Base Realignment and Closure construction funding data come from the 2013 Base Realignment and Closure Commission Execution Report. It provides an overview of the costs and savings for each the Department of Defense Component throughout the six-year BRAC implementation period (2006–2011). It also lists detailed annual construction funding information at the installation level. We geocode and aggregate this information into county-level funding information.

The data on county-level economic activity come from the Longitudinal Employer-Household Dynamics (LEHD) program's Quarterly Workforce Indicators (QWI) [96]. The LEHD data is a massive longitudinal database covering over 95% of U.S. private sector jobs. It reports on workers covered by the state Unemployment Insurance (UI) program. Then, several data sources, including the 2000 Census, Social Security Administrative records, and individual tax return files, are used to collect demographic information about the worker, such as gender, race and ethnicity. Generated by LEHD's linked employer-employee microdata, the QWI provides a wealth of economic statistics at the county level, including employment and wage, by worker's gender, race and age. We will use these data to separately assess the effects of the 2005 BRAC construction program on men's and women's labor market outcomes and test if the intervention has larger effects on men as predicted.

To estimate the effects of economic condition on fertility, we use restricted-use natality files from the National Center for Health Statistics from 2002-2013 [97]. These data consists of a record for each birth that occurred in the United States over this time period. These data files include information on mother's age, race and county of residence, which are used in conjunction with population counts from the Surveillance, Epidemiology, and End Results Program (SEER) in order to calculate birth rates. We assign births to the year of conception based on the mothers' last menstrual period where available; otherwise we assume a gestation period of nine months.

Table 4.1 presents summary statistics based on these data. Birth rates per thousand females aged between 15 and 44 averages around 67 across treatment and control counties over the sample period. Consistent with our intuition, birth rates are higher for females

Variable	N	Mean	S.D.
Panel A: Outcome Variables			
Birth rates per 1,000 females (15-44)	5,450	66	13
Birth rates per 1,000 non-Hispanic white females (15-44)	5,450	62	12
Birth rates per 1,000 black females (15-44)	5,450	72	32
Birth rates per 1,000 Hispanic females (15-44)	5,450	98	33
Birth rates per 1,000 females (15-19)	5,450	43	21
Birth rates per 1,000 females (20-24)	5,450	113	42
Birth rates per 1,000 females (25-29)	5,450	128	23
Birth rates per 1,000 females (30-34)	5,450	91	19
Birth rates per 1,000 females (35-39)	5,450	38	11
Birth rates per 1,000 females (40-44)	5,450	7	3
Panel B: Control Variables			
Population	5,450	214,354	32,6572
Percentage of Females	5,450	0.504	0.020
Percentage of Hispanics	5,450	0.093	0.136
Percentage of African Americans	5,450	0.105	0.136

Table 4.1: Summary Statistics

Note: The sample includes counties receiving the construction funding and counties with at least one military base reported in 2005 Base Structure Report.

aged 20 to 29, and compared with non-Hispanic whites, blacks and Hispanics have higher birth rates.

4.3.2 Methodology

To identify the effects of this intervention on fertility, we exploit variation in the timing and amount of BRAC construction funding across counties. Given the discrete nature of births and the fact that sometimes county-year cells are zero, especially for subgroup counts, our preferred specification is to use a Poisson model. While Poisson models are often used to consider counts instead of rates, we note that this model is equivalent to estimating the natural log of the expected counts of births while controlling for the corresponding population and restricting its coefficient to be equal to one. Specifically, we estimate the following model:

$$E[FR_{it}|perCapitaFunding_{it}, \mu_i, \eta_t, X_{it}, \delta_{st}]$$
$$= exp(\beta perCapitaFunding_{it} + \mu_i + \eta_t + X_{it} + \delta_{st} + u_{it})$$

where *i* indicates counties, *t* indicates years, and *s* indicates states. In this model, FR_{it} measures annual county-level birth rates per thousand females aged 15 to 44; X_{it} refers to county-level time-varying demographic controls, including population, percentage of Hispanics, African Americans, and females; and δ_{st} represents state-by-year fixed effects.[9] The county and year fixed effects are captured by μ_i and η_t, respectively. The county fixed effects control for county-level time-invariant characteristics and year fixed effects controls for nationwide shocks in any year. Moreover, we also include state-by-year fixed effects to capture state-level shocks in any given year. The inclusion of state-by-year fixed effects allows counties in different states to follow different trajectories and account for differential shocks by state over time. In this case, the crucial assumption is that in the absence of the 2005 BRAC construction funding, changes in birth rates would have been the same across all military counties *in the same state*. The variable of interest is $perCapitaFunding_{it}$, which measures 2005 BRAC construction funding at year *t* in county *i*. Noted that while the BRAC report provides data on funding awarded, information on when the funding was spent is not available. Thus, we assume that all funding received by a county was spent linearly beginning in the year of receipt. That is, $perCapitaFunding_{it}$ equals zero prior to the receipt of any funding for county *i* and equals $\frac{\overline{Funding_{it}}}{Population_{i,2005}}$ for years after the initial funding receipt.[10] Finally, standard errors are clustered at the state level to allow for

[9]The county-level demographic data come from the U.S. Census Bureau.

[10]Each BRAC construction project should be at least 35-percent design complete to request funding from the Department of Defense, which generates variation in the timing of counties' first funding receipt. And $\overline{Funding_{it}}$ is defined as $\frac{TotalFunding_i}{2011-firstyearoffundingreceipt+1_i}$ for years after the initial funding receipt. We use population counts in 2005 to generate the treatment variable because population could be affected by

arbitrary correlation of the error term at the state level across counties and years.

In reality, counties with military installations are likely to be systematically different from counties that do not have installations. For this reason, we restrict the sample of unfunded counties to those with at least one military installation in the 2005 Base Structure Report, the administrative report on military installations that is published annually by the Department of Defense.[11] We further restrict the overall sample to counties that did not experience any closure during the 2005 BRAC because closure of bases may have its own effects on birth rates through mental distress. The sample period is 2002–2011 for the main analysis. We use 2002 because it is the first year after the completion of the previous BRAC and 2011 because it is the statutory deadline for completion of the 2005 BRAC. When we explore treatment effects over time, we extend the sample period through 2013 to investigate whether there are lingering effects.

4.4 Results

In this section, we begin by presenting our results on the general labor market effects and gender-specific labor market effects to examine if this intervention has a particularly large effect on men. Then, we proceed to show our main results on fertility. They are followed by treatment effect dynamic analyses verifying the identifying assumption and exploring how the effects evolve over time. Finally, we assess the degree to which there are heterogeneous treatment effects across various racial and age groups.

government spending and 2005 is the last year prior to the BRAC construction program. We make that assumption for two reasons. First, according to the Department of Defense's policy, military construction funding can remain available for up to five years. Second, most installations completed their projects in 2011, even though the Department of Defense distributed most of the funding between 2007 and 2009, and few counties received funding in 2011 [42].

[11] We use the 2005 report because it is the last one published prior to the 2005 BRAC.

4.4.1 Effects on Labor Market Conditions

Tables in this section are organized in the following way: Column 1 shows estimates from the baseline specification in which we simply control for county and year fixed effects. Column 2 further adds county-level time-varying demographic controls to the model to test if the estimates are sensitive to the inclusion of additional controls. Column 3 present estimates from a specification in which we control for county, year and state-by-year fixed effects. The inclusion of state-by-year fixed effects controls for statewide shocks that are common to counties in the same state at the same year, which further relaxes the identifying assumption. And Column 4 controls for all these fixed effects and time-varying characteristics.

Table 4.2 presents our results on the general labor market effects with Panel A showing the estimated effects on employment and Panel B on average monthly wage. Estimates from this analysis suggest that this intervention has a significant effect on overall employment and average monthly wage, with our preferred specification in Column 4 indicating that a $1,000 increase in annual per capita BRAC construction funding would increase employment by 3.4% and increase average monthly wage by 1.4%.[12]

While the analysis on overall employment and wage indicates a significant positive effect, the sign of the fertility effects depends on whether income effects or substitution effects dominate. Assuming that females are the primary caregiver, improvement in women's employment and wage would lead to a decrease in fertility because the price of having an additional child is likely to outweigh the income effects. But if the labor market effects fall mostly on men, the income effects would dominate the substitution effects and we would expect to observe an increase in fertility. Thus, in Table 4.3, we present estimates that separately investigate the effects of the 2005 BRAC intervention by gender.

[12] These estimates are comparable in magnitude to what Zheng (2017) calculated based on the Bureau of Economic Analysis' Regional Economic Information System dataset.

	(1)	(2)	(3)	(4)
Panel A: Log(Employment)				
Annual BRAC Funding per Capita	0.033***	0.037***	0.031**	0.034***
	(0.010)	(0.009)	(0.012)	(0.011)
N	21,333	21,013	21,333	21,013
Panel B: Log(Average Monthly Earning)				
Annual BRAC Funding per Capita	0.019**	0.018*	0.014***	0.014***
	(0.009)	(0.009)	(0.005)	(0.004)
N	21,281	20,961	21,281	20,961
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table 4.2: Estimated Effects of 2005 BRAC Construction Funding on Labor Market Outcomes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$ Data are from Longitudinal Employer-Household Dynamics. All regressions include county, year and quarter fixed effects. Control variables include population, percentage of female, percentage of African Americans and percentage of Hispanics. Standard errors are clustered at the state level.

Panel A of Table 4.3 presents the effects on men and Panel B presents the effects on women. Given the nature of the intervention and the fact that over 90% of construction workers are men, ex ante, this program is likely to have larger effects on men's labor market outcomes. And results from Table 4.3 are consistent with this hypothesis. Results based on our preferred specification in Column 4 shows that an additional $1,000 increase in annual per capita BRAC construction funding would increase employment by 4.5% and wage by 1.8% for men. However, there is no evidence that this intervention has an effect on women.

To sum up, results from these analyses indicate that the 2005 BRAC construction program has significant effects on average wage and employment for funded military counties. In particular, it has large effect on men but no effects on women. Based on the parental time allocation model, we would expect to observe an increase in fertility assuming that children are "normal" and burden of child-raising mostly fall on women. We will explore this issue in the next section.

	(1)	(2)	(3)	(4)
Panel A: Male				
Panel A1: Log(Employment)				
Annual BRAC Funding per Capita	0.046***	0.049***	0.043***	0.045***
	(0.011)	(0.011)	(0.010)	(0.009)
Panel A2: Log(Average Monthly Earning)				
Annual BRAC Funding per Capita	0.024**	0.023**	0.018***	0.018***
	(0.009)	(0.010)	(0.004)	(0.004)
Panel B: Female				
Panel B1: Log(Employment)				
Annual BRAC Funding per Capita	0.019	0.023*	0.018	0.022
	(0.016)	(0.013)	(0.018)	(0.017)
Panel B2: Log(Average Monthly Earning)				
Annual BRAC Funding per Capita	0.007	0.006	0.006	0.005
	(0.006)	(0.006)	(0.005)	(0.004)
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table 4.3: Estimated Effects of 2005 BRAC Construction Funding on Labor Market Outcomes by Gender

Note: *** p<0.01, ** p<0.05, * p<0.1 Data are from Longitudinal Employer-Household Dynamics. All regressions include county, year and quarter fixed effects. Control variables include population, percentage of female, percentage of African Americans and percentage of Hispanics. Standard errors are clustered at the state level.

4.4.2 Main Results on Fertility

Before presents our empirical results, we would like to highlight how this intervention differs from others and its implication on estimated results. As shown in previous section, the 2005 BRAC construction program generates a natural experiment in which a plausibly exogenous shock impacts men's employment and wage, but not women's. Therefore, this program provides an opportunity for us to investigate how fertility changes with men's employment and wage, holding women's constant. This is essentially difficult from an experiment that affects men *more than* women. Assuming that women are the primary caregiver for children, in the latter case, substitution effects from women's employment

	(1)	(2)	(3)	(4)
ln(Birth Rate per 1,000) (Mean: 67 S.D.:13)				
Annual BRAC Funding per Capita	0.059***	0.052***	0.060***	0.049***
	(0.012)	(0.010)	(0.012)	(0.009)
N	5,450	5,450	5,450	5,450
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table 4.4: Estimated Effects of 2005 BRAC Construction Funding on Fertility

Note: *** p<0.01, ** p<0.05, * p<0.1 All regressions include county and year fixed effects. Control variables include population, percentage of female, percentage of African Americans and percentage of Hispanics. Standard errors are clustered at the state level.

and wage increases are more likely to attenuate the income effects.

Table 4.4 presents the main results on fertility. Column 1 shows estimates from the baseline specification in which we control for county and year fixed effects. Estimates from this specification suggest that an additional $1,000 increases in per capita BRAC construction funding would increase birth rates by 5.9%. Column 2 further adds a rich set of time-varying demographic controls to the model, including population, percentage of females, blacks and Hispanics. Results from this specification largely unchanged, suggesting that there is little scope for omitted variables bias. In Column 3, we present estimates from a specification in which we control for county, year and state-by-year fixed effects in order to address the potential concern that counties in different states may follow different trajectories and there could be state-level policies that impacts fertility during the sample period. Adding state-by-year fixed effects, in this case, would control for statewide shocks to fertility that are common to counties in the same state at the same year. Estimates based on this specification remain similar to the previous results. Finally, our preferred specification in Column 4 controls for all these fixed effects and time-varying characteristics, and the results suggests that a $1,000 increase in annual per capita BRAC construction funding would increase fertility by 4.9%. To put these results into context, it suggest that a 4.5% increase in men's employment and 1.8% increase in men's wage would lead to a 4.9% increases in birth rates.

As an additional way to estimate the effects of this program on fertility, we investigate the dynamic responses of birth rates to 2005 BRAC construction funding. We progressively make changes to the models to investigate how the effects evolve over time and verify that birth rates in military counties that received funding did not diverge from unfunded military counties in the same state. Column 1 of Table 4.5 presents estimates from the specification that we control for county, year and state-by-year fixed effects. Estimates from this column indicate that larger effects in the short run, and the effects disappear once the funding is discontinued after 2011. In Column 2, we further add county level time-varying demographic controls to the model. Estimates from this model remain similar to those in Column 1. Column 3 and Column 4 separately adds one- and two-year leads to the specifications to check if birth rates in funded military counties tracks the trends of birth rates in unfunded military counties in the same state, which otherwise would cast doubt on the identifying assumption. Estimates on the lead terms are close to zero and never statistically significant, providing strong support to the identification strategy. Furthermore, the estimated effects of 2005 BRAC construction funding are robust to the inclusion of those leads, lending further support to the research design.

4.4.3 Heterogeneous Effects

In this section, we explore the degree to which there are heterogeneous treatment effects across different demographic groups to help capture a better picture of the effects. First, we will examine the heterogeneous effects across racial groups and then across age groups.

Summary statistics in Table 4.1 suggests that birth rates differ by racial groups. Thus,

	(1)	(2)	(3)	(4)
ln(Birth Rate per 1,000) (Mean: 67 S.D.:13)				
Effects of BRAC Funding in the First Year	0.068***	0.058***	0.060***	0.047***
	(0.011)	(0.010)	(0.013)	(0.016)
Effects of BRAC Funding in the Second Year	0.097***	0.086***	0.089***	0.076***
	(0.013)	(0.013)	(0.014)	(0.019)
Effects of BRAC Funding in the Third Year	0.058***	0.046***	0.049***	0.036**
	(0.016)	(0.012)	(0.015)	(0.015)
Effects of BRAC Funding in the Fourth Year	0.092***	0.078***	0.080***	0.068***
	(0.014)	(0.012)	(0.015)	(0.017)
Effects of BRAC Funding in the Fifth Year	0.007	-0.003	-0.000	-0.013
	(0.015)	(0.012)	(0.015)	(0.015)
Effects of BRAC Funding After Fifth Year	0.024*	-0.003	-0.001	-0.013
	(0.015)	(0.012)	(0.015)	(0.015)
One Year Before the Initial BRAC Receipt			0.010	-0.002
			(0.017)	(0.014)
Two Years Before the Initial BRAC Receipt				-0.041
				(0.032)
N	6,516	6,516	6,516	6,516
Controls	No	Yes	Yes	No
State-by-Year Fixed Effect	Yes	Yes	Yes	Yes

Table 4.5: Estimated Effects of 2005 BRAC Construction Funding on Childbearing, Lags and Leads

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$ All regressions include county and year fixed effects. Control variables include population, percentage of female, percentage of African Americans and percentage of Hispanics. Standard errors are clustered at the state level.

it is also possible that various racial group would respond differently towards the 2005 BRAC economic shock. In Table 4.6, we present results that separately estimate the effects for non-Hispanic whites, blacks and Hispanics. Panel A presents the effects on non-Hispanic white and shows that an additional thousand per capita BRAC spending would increase fertility for this group by approximately 4.6%. Panel B presents the results for Blacks, and while the estimates are less robust to alternative specification as compared with the ones for non-Hispanic whites, the magnitude of the estimates for this group are uniformly larger. Panel C presents the results for Hispanics. It appears that the fertility

	(1)	(2)	(3)	(4)
Panel A: ln(Non-Hispanic White Birth Rate per 1,000) (Mean: 62 S.D.:12)				
Annual BRAC Funding per Capita	0.039***	0.046***	0.042***	0.046***
	(0.012)	(0.012)	(0.012)	(0.010)
N	5,450	5,450	5,450	5,450
Panel B: ln(Black Birth Rate per 1,000) (Mean: 72 S.D.:32)				
Annual BRAC Funding per Capita	0.150***	0.104***	0.112***	0.077***
	(0.038)	(0.036)	(0.028)	(0.026)
N	5,450	5,450	5,450	5,450
Panel C: ln(Hispanic Birth Rate per 1,000) (Mean: 98 S.D.:33)				
Annual BRAC Funding per Capita	0.150***	0.125***	0.121***	0.083**
	(0.035)	(0.036)	(0.035)	(0.033)
N	5,450	5,450	5,450	5,450
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table 4.6: Estimated Effects of 2005 BRAC Construction Funding on Fertility by Racial Groups

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$ All regressions include county and year fixed effects. Control variables include population, percentage of female, percentage of African Americans and percentage of Hispanics. Standard errors are clustered at the state level.

effects for this group are the largest among all three, which is consistent with our intuition because approximately 30% of construction employment are Hispanics while they only constitute less than 20% of the total population. Based on these results, we conclude that the fertility effects are larger for blacks and Hispanics while the magnitude of the overall effects are similar to the effects on non-Hispanic white as they account for approximately 80% of the total population.

Table 4.7 presents the effects by age groups. Ex ante, since teen births are less likely to be affected by household financial conditions, we should expect to see smaller effects on this age group. And results from Panel A in Table 6 is consistent with the intuition and there are little effects on teen birth rates. On the other hand, We find large and consistent effects for females aged 20 to 34.

In conclusion, results from this panel indicate that there are heterogeneous responses across racial groups and the effects are larger for blacks and Hispanics. Similarly, while

	(1)	(2)	(3)	(4)
Panel A: 15-19 (Mean: 43 S.D.:21)				
Annual BRAC Funding per Capita	0.011	0.014	0.003	0.001
	(0.029)	(0.030)	(0.024)	(0.025)
Panel B: 20-24 (Mean: 113 S.D.:42)				
Annual BRAC Funding per Capita	0.116***	0.116***	0.109**	0.105**
	(0.034)	(0.034)	(0.041)	(0.041)
Panel C: 25-29 (Mean: 128 S.D.:23)				
Annual BRAC Funding per Capita	0.146***	0.141***	0.139***	0.131***
	(0.020)	(0.019)	(0.025)	(0.024)
Panel D: 30-34 (Mean: 91 S.D.:19)				
Annual BRAC Funding per Capita	0.089***	0.086***	0.084***	0.079***
	(0.022)	(0.019)	(0.019)	(0.018)
Panel E: 35-39 (Mean: 38 S.D.:11)				
Annual BRAC Funding per Capita	0.029	0.026	0.030	0.026
	(0.035)	(0.036)	(0.039)	(0.041)
Panel F: 40-44 (Mean: 7 S.D.:3)				
Annual BRAC Funding per Capita	-0.074*	-0.085**	-0.070	-0.077*
	(0.043)	(0.040)	(0.044)	(0.041)
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table 4.7: Estimated Effects of 2005 BRAC Construction Funding on Fertility by Mother's Age Groups

Note: *** p<0.01, ** p<0.05, * p<0.1 All regressions include county and year fixed effects. Control variables include population, percentage of female, percentage of African Americans and percentage of Hispanics. Standard errors are clustered at the state level.

teen births and female over 35 are unaffected by the intervention, females aged 20 to 34 are responsible for the overall effects.

4.5 Discussion and Conclusion

In this paper, we use a potentially exogenous shock to localized economic condition, especially men's labor market condition, to test the parental time allocation model. Specifically, we exploit variation across counties in the timing and amount of construction funding provided by the 2005 BRAC to show that this program has a large effects on men's labor market opportunity but not women's. Then, we use the same variation to test the theoretical prediction that father's labor market outcomes have a positive effect on fertility. Our analyses indicates that a 4.5% increase in men's employment and 1.8% increase in income would lead to a 4.9% increase in contemporaneous fertility, which is consistent with theoretical prediction. Furthermore, we investigate the extent to which there are heterogeneous treatment effects across various demographic groups and conclude that the effects are mostly driven females aged 20 to 34.

Noted that [87] finds significant effects of the 2005 BRAC construction program on in-migration, it is possible that the fertility effects are driven by migrants. In this case, compositional changes in population may be responsible for the observed increases in fertility. However, as shown in [87], an $1,000 increase in per capita BRAC spending is associated with an additional 1,000 migrants, and they are unlikely to have increased birth rates by 4.9% for counties averaging around 200,000 people. Thus, it does not seem that migrants are driving the observed effects.

This paper complements a growing body of empirical literatures that try to disentangle the causal link between parental labor market condition and fertility. Specifically, it adds to this line of literature by estimating a recent potentially exogenous shock that affect men's labor market condition on fertility. And we find evidence that father's improved labor market condition has large and consistent effects on fertility. Understanding the causal link between parental income shocks and fertility have important policy implications as society may have a targeted or optimal birth rates. The positive relationship between men's labor market opportunity and fertility suggests that programs aiming at improving labor market condition for men may have unintended fertility effects. However, the results should be interpreted with caution for policy implications. Our results indicate a contemporaneous fertility increase following improved labor market conditions for men and provide some evidence of an increase in completed fertility during the sample period, but it is possible that these shocks have no effects on total completed fertility in the long run if people simply change the timing of fertility. Therefore, further research to investigate long run effects of this intervention would be warranted to policies that aim to increase total completed fertility.

5. CONCLUSION

This dissertation applies causal inference method to study how policies affect the general welfare of the public; whether intentionally or unintentionally.

In the first essay, I employ a difference-in-differences design to reexamine the effect of false ID laws with scanner provisions on underage drinking using 1991–2013 national Youth Risk Behavior Surveillance System data. In contrast to previous findings based on the NLSY97, the results using these data indicate that these laws have no effect on reducing underage drinking behavior, and that conclusion is robust under various model specifications. Estimates allowing for dynamic treatment effects lead to the same conclusion. Moreover, a replication and extension of prior work using the NLSY97 suggests that previously reported estimates based on those data are not reliable. As a whole, my analyses suggest that a stricter false ID law with enforced use of electronic scanners in alcohol sales is not an effective approach to reducing underage drinking.

In the second essay, I exploit variation across counties in the timing and amount of construction funding provided by the 2005 BRAC to estimate the effects of government spending on local economic conditions. My analysis yields an estimated cost per job of approximately $65,000 and a local fiscal multiplier of around 1.21, and these estimates are robust to various model specifications. Furthermore, an industry-specific analysis finds especially large effects on the construction industry, which is consistent with the nature of the spending. To better understand the regional impact of the BRAC funding, I directly estimate spillover effects on neighboring counties. I find little evidence of spillovers; however, there is suggestive evidence of positive spillovers on construction employment for neighboring counties. This paper suggests that government spending, especially construction spending, could play a significant role in creating jobs and increasing income.

In the third essay, we used a potentially exogenous shock to localized economic condition, especially men's labor market condition, to test the parental time allocation model. Specifically, we exploit variation across counties in the timing and amount of construction funding provided by the 2005 BRAC to show that this program has a large effects on men's labor market opportunity but not women's. Then, we use the same variation to test the theoretical prediction that father's labor market outcomes have a positive effect on fertility. Our analyses indicates that a 4.5% increase in men's employment and 1.8% increase in income would lead to a 4.9% increase in contemporaneous fertility, which is consistent with theoretical prediction. Furthermore, we investigate the extent to which there are heterogeneous treatment effects across various demographic groups and conclude that the effects are mostly driven females aged 20 to 34. This paper provides strong evidence that children are "normal" and that policies aiming at improving men's labor market outcomes would increase fertility, at least in the short-run.

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APPENDIX A

2005 BASE REAGLINMENT AND CLOSURE SELECTION CRITERIA

In selecting military installations for closure or realignment, the Department of Defense, giving priority consideration to military values, the first four criteria listed below, will consider:

Military Value

1. The current and future mission capabilities and the impact on operational readiness of total force of the Department of Defense, including the impact on joint warfighting, training, and readiness.

2. The availability and condition of land, facilities, and associated airspace (including training areas suitable for maneuver by ground, naval, or air forces throughout a diversity of climate and terrain areas and staging areas for the use of the Armed Forces in homeland defense missions) at both existing and potential receiving locations.

3. The ability to accommodate contingency, mobilization, surge, and future total force requirements at both existing and potential receiving locations to support operations and training.

4. The cost of operations and the manpower implications.

Other Considerations

5. The extent and timing of potential costs and savings, including the number of years, beginning with the date of completion of the closure or realignment, for the savings to exceed the costs.

6. The economic impact on existing communities in the vicinity of military installations.

7. The ability of the infrastructure of both the existing and potential receiving commu-

nities to support forces, missions, and personnel.

8. The environmental impact, including the impact of costs related to potential environmental restoration, waste management, and environmental compliance activities.

APPENDIX B

FIGURES

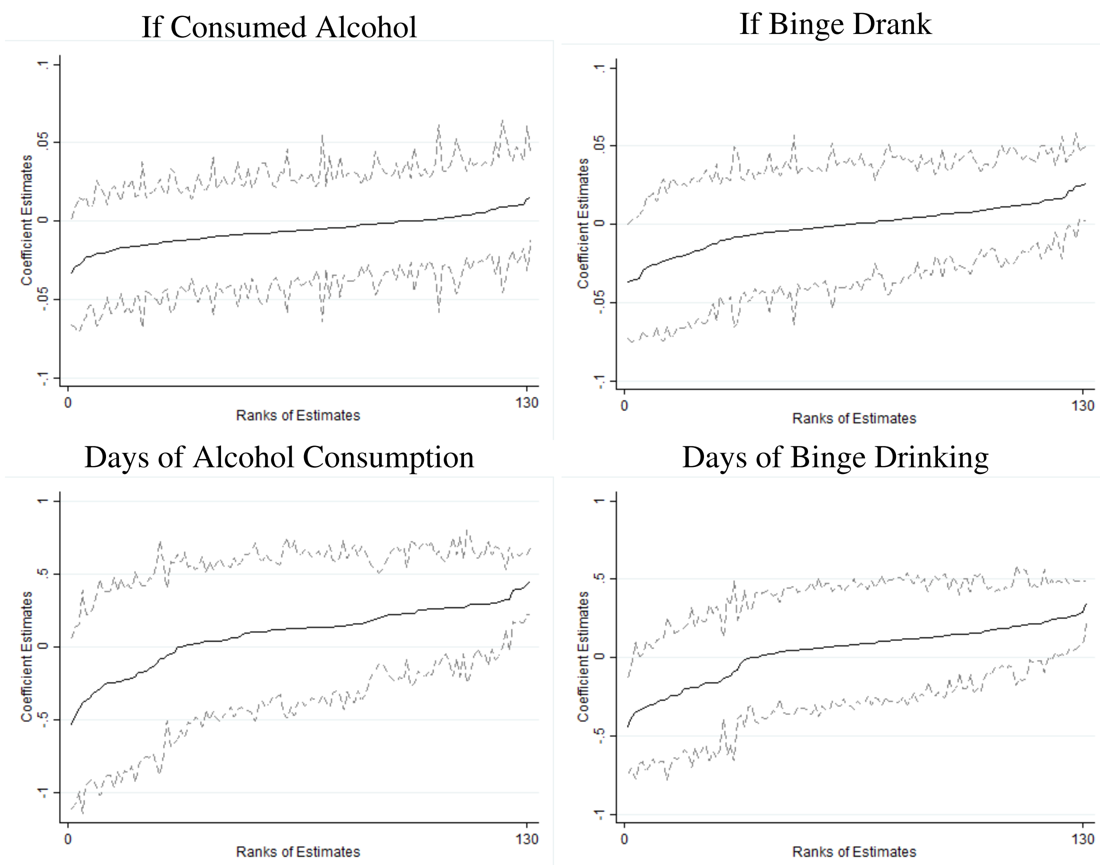

Figure B.1: Sensitivity Analysis of Estimates to Treated States Considered

Notes: Sample is 1991–2013 YRBS surveys. These figures plot the coefficient estimates and the 95% confidence interval against ranking of the coefficient estimates. These estimates are from models with state and year fixed effects, controls and state-specific linear time trends, dropping 1, 2 or 3 treatment states in the analysis. Standard errors are clustered at the state level. Estimates unweighted.

APPENDIX C

TABLES

State	Law Effective Date
Arizona	8/12/2005
Connecticut	10/1/2001
Nebraska	7/15/2010
New York	9/1/1999
North Carolina	11/14/2001
Ohio	9/21/2000
Oregon	1/1/2000
Pennsylvania	12/16/2002
Texas	9/1/2005
Utah	7/1/2009
West Virginia	6/6/2003

Table C.1: Law Effective Dates of False ID Laws with Scanner Provisions

Source: Alcohol Policy Information System (APIS)

	Full Sample	Male	Female
Panel A: If Consumed Alcohol			
FSP Laws	-0.027	-0.041	-0.006
	(0.023)	(0.038)	(0.024)
N	155,480	76,026	79,454
Panel B: If Binge Drank			
FSP Laws	-0.011	-0.015	0.000
	(0.023)	(0.033)	(0.022)
N	162,585	79,713	82,872
Panel C: Days of Alcohol Consumption			
FSP Laws	-0.114	-0.181	0.010
	(0.222)	(0.294)	(0.227)
N	155,480	76,026	79,454
Panel D: Days of Binge Drinking			
FSP Laws	-0.090	-0.121	-0.020
	(0.191)	(0.246)	(0.189)
N	162,585	79,713	82,872

Table C.2: Weighted Least Square Estimates of FSP Laws on Underage Drinking

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$ Sample is 1991–2013 YRBS survey. All regressions include state and year fixed effects, individual- and state-level controls, lead term and state-specific linear time trend. Standard errors are clustered at the state level. Individual-level controls include gender, race and age. State-level controls include unemployment rate, log of income per capita, state beer tax per gallon, and dummy variables controlling for various state policies on youth alcohol access. Estimates weighted by survey weights.

	If Binge Drank	Days of Alcohol Consumption	Days of Binge Drinking	Average Drinks per Day
Panel A				
4+ years before	0.033	0.022	0.159	0.053
	(0.030)	(0.227)	(0.198)	(0.104)
3 years before	0.006	0.102	0.010	0.043
	(0.023)	(0.148)	(0.180)	(0.074)
2 years before	-0.003	-0.072	-0.250	-0.050
	(0.022)	(0.142)	(0.197)	(0.071)
1 year before (omitted)				
1 year after	0.002	0.148	0.032	-0.021
	(0.017)	(0.201)	(0.107)	(0.057)
2 years after	0.013	0.183	0.130	0.035
	(0.015)	(0.334)	(0.118)	(0.085)
3 years after	0.005	0.432	0.145	0.091
	(0.031)	(0.289)	(0.164)	(0.092)
4+ years after	0.031	0.821	0.291	0.161
	(0.030)	(0.702)	(0.298)	(0.147)
N	40,004	40,070	40,004	39,790
Panel B				
4+ years before	0.035***	0.094	0.409***	0.103
	(0.012)	(0.171)	(0.087)	(0.063)
3 years before	0.008	0.161*	0.215***	0.084**
	(0.009)	(0.088)	(0.056)	(0.038)
1-2 years before (omitted)				
1 year after	0.002	0.148	0.032	-0.021
	(0.017)	(0.201)	(0.105)	(0.057)
2 years after	0.013	0.169	0.083	0.026
	(0.014)	(0.322)	(0.090)	(0.078)
3 years after	0.004	0.406	0.055	0.073
	(0.027)	(0.280)	(0.123)	(0.078)
4+ years after	0.029	0.782	0.154	0.134
	(0.025)	(0.676)	(0.221)	(0.127)
N	40,004	40,070	40,004	39,790

Table C.3: Attempted Replication and Extension of [1]'s Dynamic Analysis

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$ All regressions include state, year and month fixed effects, controls and state-specific linear time trends. Individual-level controls include age, gender, race, family size, income, marital status, employment status, educational attainment and being a student. State-level controls include unemployment rate, log of income per capita, state beer tax per gallon, and dummy variables controlling for various state policies on youth alcohol access. Standard errors are clustered at the state level. Data are from 1998–2005 NLSY97 sample. Estimates unweighted.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Days of Alcohol Consumption, Original Responses								
FSP Laws	-0.168*	-0.156*	-0.357***	-0.269**	-0.208*	-0.266**	-0.441***	-0.398***
	(0.093)	(0.087)	(0.088)	(0.103)	(0.121)	(0.116)	(0.118)	(0.116)
1-2 Years Prior to FSP laws			-0.364***	-0.396***			-0.322***	-0.396***
			(0.079)	(0.106)			(0.083)	(0.099)
N	40,164	40,070	40,070	40,070	40,164	40,070	40,070	40,070
R^2	0.026	0.084	0.084	0.085	0.054	0.065	0.065	0.068
Panel B: Days of Alcohol Consumption: Variables Recoded								
FSP Laws	-0.177*	-0.160*	-0.365***	-0.261**	-0.203	-0.251*	-0.427***	-0.376***
	(0.097)	(0.094)	(0.105)	(0.100)	(0.136)	(0.129)	(0.131)	(0.113)
1-2 Years Prior to FSP laws			-0.372***	-0.400***			-0.322***	-0.407***
			(0.094)	(0.124)			(0.099)	(0.121)
N	40,164	40,070	40,070	40,070	40,164	40,070	40,070	40,070
R^2	0.025	0.084	0.084	0.085	0.054	0.064	0.064	0.067
Panel C: Days of Binge Drinking, Original Responses								
FSP Laws	-0.125	-0.111	-0.251***	-0.082	-0.136*	-0.158**	-0.272***	-0.151*
	(0.091)	(0.071)	(0.087)	(0.101)	(0.073)	(0.077)	(0.092)	(0.077)
1-2 Years Prior to FSP laws			-0.254***	-0.199***			-0.209***	-0.202***
			(0.054)	(0.065)			(0.046)	(0.038)
N	40,097	40,004	40,004	40,004	40,097	40,004	40,004	40,004
R^2	0.020	0.071	0.071	0.073	0.037	0.044	0.045	0.048
Panel D: Days of Binge Drinking: Variables Recoded								
FSP Laws	-0.118	-0.106	-0.252***	-0.077	-0.125	-0.157*	-0.270***	-0.156
	(0.099)	(0.075)	(0.092)	(0.117)	(0.077)	(0.080)	(0.098)	(0.096)
1-2 Years Prior to FSP laws			-0.264***	-0.202**			-0.208***	-0.203***
			(0.072)	(0.081)			(0.066)	(0.061)
N	40,097	40,004	40,004	40,004	40,097	40,004	40,004	40,004
R^2	0.020	0.069	0.069	0.071	0.035	0.043	0.043	0.046
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
1–2 Years Prior to FSP Laws	No	No	Yes	Yes	No	No	Yes	Yes
State-specific Linear Time Trends	No	No	No	Yes	No	No	No	Yes
Individual Fixed Effects	No	No	No	No	Yes	Yes	Yes	Yes

Table C.4: Estimated Effects on Underage Drinking, Adding Measurement Errors to NLSY97 to Be Comparable to YRBS

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$ All regressions include state, year and month fixed effects. Individual-level controls include age, gender, race, family size, income, marital status, employment status, educational attainment and being a student. State-level controls include unemployment rate, log of income per capita, state beer tax per gallon, and dummy variables controlling for various state policies on youth alcohol access. Standard errors are clustered at the state level. Data are from 1998–2005 NLSY97 sample. Estimates unweighted.

	(1)	(2)	(3)	(4)
Panel A: Log(Employment)				
BRAC Funding per Capita ($1,000s)	0.037***	0.041***	0.030*	0.033**
	(0.009)	(0.008)	(0.017)	(0.014)
N	5,370	5,370	5,370	5,370
Panel B: Log(Per Capita Income)				
BRAC Funding per Capita ($1,000s)	0.073***	0.074***	0.058***	0.058***
	(0.012)	(0.010)	(0.021)	(0.018)
N	5,370	5,370	5,370	5,370
Cost per Job	56,007	50,543	69,075	62,796
Income Multiplier	2.33	2.36	1.85	1.85
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table C.5: Estimated Effects of 2005 BRAC Construction Funding on Local Economic Conditions: Alternative Construction of per Capita Funding

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$ All regressions include county and year fixed effects. County-level controls include population, percentage of female, Hispanics and African Americans. Standard errors are clustered at the state level. Control counties are counties with at least one military base reported in base structural report in 2005, the last report published prior to the 2005 BRAC. Estimates are unweighted. $perCapitaFunding_{it}$ equals to zero prior to the receipt of any funding for county i and equals to $\frac{\overline{Funding_{it}}}{Population_{it}}$ for years after the first year of funding receipt. The cost per job is calculated as $\frac{\$1,000\times\text{pre-funding average of population size for funded counties}}{\text{estimated percentage change}\times\text{pre-funding average of employment for funded counties}}$ and the local fiscal multiplier is calculated as $\frac{\text{estimated percentage change}\times\text{pre-funding average of per capita income for funded counties}}{\$1,000}$.

	(1)	(2)	(3)	(4)
Panel A: Log(Employment)				
BRAC Funding per Capita ($1,000s)	0.033***	0.034***	0.028**	0.031***
	(0.006)	(0.005)	(0.011)	(0.008)
N	5,370	5,370	5,370	5,370
Panel B: Log(Per Capita Income)				
BRAC Funding per Capita ($1,000s)	0.042***	0.038**	0.036***	0.031***
	(0.015)	(0.016)	(0.009)	(0.010)
N	5,370	5,370	5,370	5,370
Cost per Job	62,795	60,949	74,009	66,847
Income Multiplier	1.34	1.21	1.15	0.99
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table C.6: Estimated Effects of 2005 BRAC Construction Funding on Local Economic Conditions: Alternative Assumption on Spending Pattern

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$ All regressions include county and year fixed effects. County-level controls include population, percentage of female, Hispanics and African Americans. Standard errors are clustered at the state level. Control counties are counties with at least one military base reported in base structural report in 2005, the last report published prior to the 2005 BRAC. Estimates are unweighted. Assume each individual funding awarded to a county at a given year is spent equally between that year and 2011. For instance, County A receive M million dollars in 2008 and N million dollars in 2010, so the spending for County A would be $\frac{M}{4}$ million dollars in 2008 and 2009, and $\frac{M}{4}+\frac{N}{2}$ million dollars in 2010 and 2011. The cost per job is calculated as $\frac{\$1{,}000\times\text{pre-funding average of population size for funded counties}}{\text{estimated percentage change}\times\text{pre-funding average of employment for funded counties}}$ and the local fiscal multiplier is calculated as $\frac{\text{estimated percentage change}\times\text{pre-funding average of per capita income for funded counties}}{\$1{,}000}$.

	(1)	(2)	(3)	(4)
Panel A: Log(Average Wage)				
BRAC Funding per Capita ($1,000s)	0.061***	0.059***	0.054***	0.052***
	(0.007)	(0.007)	(0.005)	(0.004)
N	5,370	5,370	5,370	5,370
Panel B: Log(Unemployment Rate)				
BRAC Funding per Capita ($1,000s)	-0.047*	-0.049**	-0.005	-0.008
	(0.025)	(0.023)	(0.010)	(0.010)
N	63,745	62,809	63,745	62,809
Panel C: Log(Population)				
BRAC Funding per Capita ($1,000s)	0.013	0.014	0.012	0.017
	(0.019)	(0.019)	(0.022)	(0.023)
N	5,370	5,370	5,370	5,370
Panel C: Log(Average Monthly Employment)				
BRAC Funding per Capita ($1,000s)	0.040***	0.045***	0.034***	0.039***
	(0.008)	(0.007)	(0.011)	(0.009)
N	21,255	20,943	21,255	20,943
Controls	No	Yes	No	Yes
State-by-Year Fixed Effect	No	No	Yes	Yes

Table C.7: Estimated Effects of 2005 BRAC Construction Funding on Other Measures of Local Economic Conditions

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Information on average wage and population are taken from BEA's REIS. Data on unemployment rate are from BLS. Control variables include population, percentage of female, percentage of African Americans and percentage of Hispanics. Standard errors are clustered at the state level. Estimates are unweighted.