

Three Essays in Public Economics

A Dissertation Presented for the
Doctor of Philosophy
Degree

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I dedicate this dissertation to my loving wife
Seo Hee Choi
who has been the biggest support to my journey.

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Abstract

My dissertation consists of three essays in public economics. The first essay explores the role of parental wealth on school-to-work transitions and labor market outcomes of college graduates. I rely on quasi-experimental variation generated by short-run housing market fluctuations in South Korea during the 2000s. I find that a one percent increase in parental housing wealth reduces the employment hazard of their children by one percent. The result is mainly driven by young individuals who took additional years of schooling after college graduation. I also find some evidence that a greater level of parental wealth improves job security. The second essay, which is a joint work, examines whether the U.S. Department of Defense 1033 program crowds out police protection expenditure of localities. We exploit plausibly exogenous variation in item availability across time interacted with cross-sectional variation in transaction costs and land area. We find that items received through the 1033 program do not crowd out local police spending. The third essay documents evidence of consumption externalities from upper-income households to lower-income households using a series of tax reforms during the Bush administration as a natural experiment. I find that consumption externalities generated by the tax reforms raised quarterly consumption of lower-income households by more than 400 U.S. dollars. The increase was mainly driven by consumption of visible goods such as housing and vehicle. The effect of consumption externalities was stronger for lower-income households who were closer to upper-income households in the income distribution.

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Chapter 1

Parental Wealth, Time to First Job, and Labor Market Outcomes: Evidence from Housing Wealth Shocks in South Korea

I Introduction

For young adults who make transitions from school to work, financial resources of parents can work as insurance against labor market risks [35]. When students enter the labor market, they usually have limited resources to manage expenses that are incurred from the job search. Monetary support from parents can help first-time job seekers smooth consumption while searching for a job, allowing them to be more selective in accepting job offers. This selectivity margin prolongs job search durations [27]. On the other hand, a longer search has its own costs in terms of human capital depreciation, foregone on-the-job training, and/or a reduction in job availability. These factors render so-called negative duration dependence, which means that a longer search duration lowers the probability of finding a job (for example, Kroft et al.

[61]). The adverse effects of long-term unemployment may prevent young job seekers from being overly selective.¹

This paper explores whether parental wealth affects the time that it takes for college graduates to land a first job and corresponding labor market outcomes. While economists pay enormous attention to the role of education as a vehicle for intergenerational mobility (see Lefgren et al. [66], Hilger [48], Richey and Rosburg [85] for recent evidence), a better education alone does not guarantee a quality job, which finally turns human capital into a paycheck. The role of parental wealth as a private safety net against labor market risks can be an important but rarely investigated channel of intergenerational transmission of wealth. Additionally, one's early labor market experience could have a long-lasting impact on subsequent labor market outcomes Burgess et al. [17], Kondo [58] and the negative impact is not easily mitigated by additional human capital accumulation in the following periods [78].

In the absence of a randomized experiment, it is challenging to identify the causal effect of parental wealth on job search and labor market outcomes of their children. Any correlation between parental wealth and unobserved factors such as abilities [93], time preferences [28], or qualities of informal networks [53, 9] gives rise to endogeneity. To address potential endogeneity, I exploit quasi-experimental variation in parental housing wealth generated by short-run housing market fluctuations within municipalities. The approach is similar to Lovenheim [71] and Lovenheim and Reynolds [73], except that I use variation in home prices per square meter rather than total home prices. In other words, I control for variation in home size across households because it might be correlated with factors mentioned above. Considering the idiosyncratic nature of the housing market, the variation that I exploit is arguably exogenous, especially conditional on municipality fixed effects as well as individual and parental characteristics. Indeed, the data supports that the variation is not associated with the ranking of college from which an individual graduated or the probability of using informal networks to find a job.

¹Nekoei and Weber [79] argue that the effect of unemployment insurance on wages, not on durations, depends on the relative importance between the selectivity effect and the duration dependence effect.

To answer this question, I link unique survey data from the Graduates Occupational Mobility Survey (GOMS) to administrative data on assessed housing values from South Korea. Using a two-step control function approach, I find that a one percent increase in estimated parental housing wealth reduces the employment hazard of their children by about one percent. I also find that the effect is concentrated on male young adults and those from high-income households. Consistent with many empirical studies, parental housing wealth does not affect wages at the first job, or even wages in the longer run [18, 35]. However, a greater level of parental wealth lowers the probability of working in a large firm and raises the probability of working in the public sector. I further explore the mechanisms through which parental housing wealth affects search spells. Results indicate that an exogenous increase in parental housing wealth encourages young adults to receive post-college education, in particular those who had a time gap between college graduation and post-college enrollment.² The estimate implies that the effect of parental housing wealth accounts for one third of additional schooling after graduating from college. This evidence mirrors Barr and Turner [7] who show that extending UI benefits increases college enrollment in the United States.

Studying the case of South Korea has several benefits. First, the GOMS data provides a relatively large sample size and contains detailed information on education, school-to-work transitions, and early career development of college graduates. The GOMS also collects data on self-reported reservation wages and job-finding routes, which are rarely available from other data sources. Second, the role of family support, if it exists, should be stronger in South Korea where about 69 percent of 25-34 years-olds completed tertiary education according to the Organisation for Economic Co-operation and Development [81]. Third, due to fierce competition for jobs and a tight labor market, the demand for post-college investment in job preparation is rapidly growing [80], making young adults' reliance on their parents stronger. Finally, South Korea has recently launched a program that provides financial support of 300,000 Korean Won (slightly less than \$260 U.S. dollars) for up to three months to young individuals who participate in a government training program called the Youth Employment Success Package. While a government subsidy can have a different

²Most young individuals in my sample enrolled in graduate schools, but about 4.4 percent of those who received post-college education enrolled in college.

impact than parental wealth, the findings of this paper shed light on whether the existence of insurance affects job search behavior of young individuals and labor market outcomes.

This paper contributes to the literature in several ways. First of all, I provide the first causal evidence of the effect of parental wealth on time to a first job of their children. Fradkin et al. [35] also consider the role of family insurance for first-time job seekers in Belgium, but the authors focus on the labor supply of young adults measured by total days worked rather than search durations. I employ a different identification strategy with different data in a very different context. Second, the rich information in the GOMS data allows me to explore potential mechanisms through which parental wealth influences search spells of young adults. More broadly, I document new evidence of the effects of insurance on labor market outcomes. Until recently, researchers have not found evidence that extending UI benefits improves employment outcomes, mostly measured by wages [18, 64, 102]. However, a couple of recent studies find that the UI benefit extensions affect reemployment wages negatively [91] or positively [79]. The finding of this paper adds more evidence regarding the role of insurance in shaping search outcomes.

This paper is not without limitations. First, although the GOMS collects data on some characteristics of a residential home of parents, it does not collect data on housing wealth. To overcome this limitation, I estimate parental housing values based on household characteristics, housing characteristics, and residential locations of households. Results from the reduced-form estimation, which do not rely on the imputation, are qualitatively similar to results from the control function approach. It is also worth emphasizing that measurement errors generated from the imputation would work against finding the effect of parental housing wealth on outcomes of interest. Second, I focus on a selected subsample of young adults, especially those who were living with their parents three to four years after college graduation. However, these individuals could be the population of policy interest if they moved back to their parents' home after failing to find a job [55]. I also show that individuals in my estimation sample are similar to those in the full sample in terms of observed characteristics.

The paper proceeds as follows. Section II develops a simple framework to motivate the empirical analysis of this paper. Section III provides background information regarding the

youth labor market in South Korea in 2000s. Section IV describes data sources, housing wealth imputation process, duration calculations, and sample selection criteria. Section V proposes an empirical strategy. Section VI explores the effect of parental wealth on time-to-first-job of college graduates as well as robustness and heterogeneity. Section VII examines the role of parental housing wealth in determining labor market outcomes and Section VIII investigates potential channels through which parental housing wealth influences job search behaviors of their children. Section IX concludes.

II A simple framework

To motivate my empirics, I develop a simple model that describes a first job search process, based on Card et al. [18] and Chetty [22]. The authors greatly simplify the model suggested by Lentz and Trans [67] by assuming that (1) once an individual finds a job, it lasts forever as in Danforth [27] and (2) wages are exogenously given. Since I only consider a single unemployment spell for each individual, which is the time to a first job, this simplification is well-suited for the purpose of this study.

Consider a discrete time finite horizon model where δ represents a subjective rate of time preference and r represents an interest rate, which is time-invariant. The utility function is separable in consumption, c_t , and search effort, s_t . Unlike experienced workers, first-time job seekers are not eligible for unemployment insurance (UI) benefits. Instead, I assume that an individual receives a fixed amount of unconditional transfer, b , from his parents in every period until he finds a job. While the transfer provides consumption smoothing benefits, suppose that it also create disutility from being dependent. For example, if an individual lives together with his parents, the costs come from the shared residence [55]. If an individual does not live with his parents, the disutility comes from reduced consumption of his parents under other regarding preferences or any discomfort from “not standing on his feet.” The utility function is given as:

$$u(c_t, s_t, b) = \phi(c_t) - \psi(s_t) - \eta(b) \tag{1.1}$$

where $\phi'(\cdot) > 0, \phi''(\cdot) < 0, \psi'(\cdot) > 0, \psi''(\cdot) > 0, \eta'(\cdot) > 0, \eta''(\cdot) > 0$, by assumption. For further simplification, search effort s_t is normalized to the probability of getting a job in the current period t .

Suppose that an individual enters the labor market and starts looking for a job at $t = 0$. In each period, he chooses the intensity of search s_t . If search is successful, he begins working immediately and earns wage rate of w_t . Otherwise, he receives financial support from his parents. Let c_t^e and c_t^u denote the optimal consumption choice under employment and unemployment, respectively.

The value function for an individual who finds a job is then

$$V_t^e(a_t) = \max_{a_{t+1} \geq L} \phi(a_t - \frac{a_{t+1}}{1+r} + w_t) + \frac{1}{1+\delta} V_{t+1}(a_{t+1}) \quad (1.2)$$

where L denotes the borrowing limit imposed by the capital market and a denotes his own assets. The first term on the right hand side represents the utility of consumption in period t where c_t^e is replaced with $a_t - \frac{a_{t+1}}{1+r} + w_t$ using the recursive budget constraint. The second term implies the present value of optimal lifetime utility from $t + 1$ up to T .

The value function for an individual who fails to find a job is

$$V_t^u(a_t) = \max_{a_{t+1} \geq L} \phi(a_t - \frac{a_{t+1}}{1+r} + b) + \frac{1}{1+\delta} J_{t+1}(a_{t+1}) \quad (1.3)$$

where

$$J_{t+1}(a_{t+1}) = \max_{s_{t+1}} s_{t+1} V_{t+1}^e(a_{t+1}) + (1 - s_{t+1}) V_{t+1}^u - \psi(s_{t+1}) - \eta(b) \quad (1.4)$$

is the expected utility of entering period $t + 1$ without having a job.

To maximize his expected utility at the beginning of period t , an unemployed individual should choose the optimal search effort s_t^* that satisfies the following optimal condition:

$$\psi'(s_t^*) = V_t^e(a_t) - V_t^u(a_t) \quad (1.5)$$

The condition implies that the marginal cost of search should be equal to the utility difference from the employment and unemployment status, or to put it differently, the

marginal utility of being employed at the optimal level of search. A simple comparative static produces a testable hypothesis regarding the effect of parental transfers on one's search effort. That is,

$$\frac{\partial s_t^*}{\partial b} = -\frac{\phi'(c_t^u) - \eta'(b)}{\psi''(s_t^*)} \leq 0 \quad (1.6)$$

Equation (1.6) suggests that the effect of parental transfers depends on two competing forces, the consumption smoothing benefit while searching for a job and the psychological cost of being dependent on his parents. While Chetty [22] predicts that an exogenous increase in individual wealth leads to a reduction in search effort, my model implies that the role of parental support on job search effort of their children is uncertain. When one's disutility from receiving parental support is completely insensitive to changes in the level of the transfer (i.e., $\eta'(b) = 0$), the result is exactly the same as Chetty [22]. On the other hand, if the disutility from being dependent dominates the consumption smoothing benefit, an increase in parental transfer can facilitate one's school-to-work transition.

III Youth labor market in South Korea in 2000s

South Korea is famous for its education fever [82]. The college enrollment rate in South Korea jumped from 33.2 percent in 1990 to 79 percent in 2010 [59]. Despite the surge in the supply of highly educated labor force, the economy could not keep up with the trend, failing to generate a sufficient number of decent jobs. Figure 1.1 depicts the trend in unemployment among young adults aged between 15 and 29, which does not seem particularly troublesome compared to other countries. But the trend in the labor force participation rates reveals the problem: a nontrivial share of South Korean youth does not actively look for a job.

There are several factors that explain a low youth labor force participation in South Korea. Young individuals attending schools or preparing for job-related exams account for a large share of them [52]. In addition to the high college enrollment rate, 8.9 percent of 4-year university graduates went to graduate school in 2010. At the same time, roughly ten percent of young adults (aged between 15 and 29) who were not in the labor force prepared

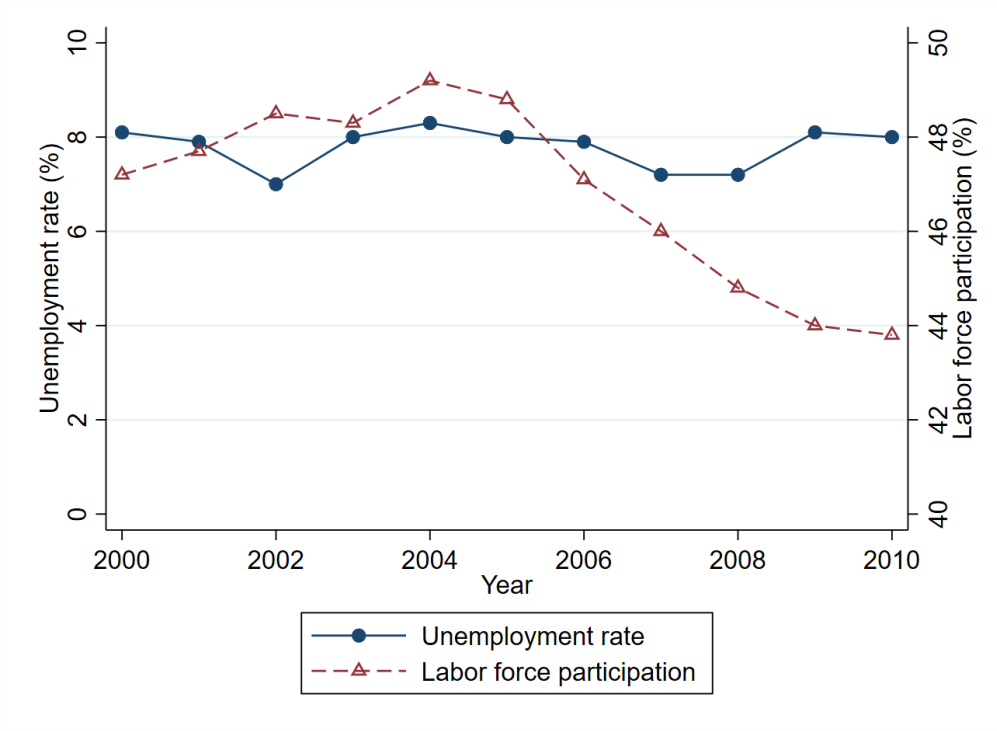


Figure 1.1: Trends in unemployment and labor force participation in South Korea

Note: The figure depicts trends in unemployment and labor force participation among individuals aged between 15 to 29. *Source:* Economically Active Population Survey, 2000-2010

for job-related exams in 2010 [60].³ However, only a very small number of individuals can pass the exams. For example, the pass rate of civil service exams for general public official positions was 1.35 percent in 2011 [76]. Despite the low chance of success, many South Korean youth still prefer to work in the public sector due to high job security and other benefits it provides [65]. Substantial anecdotal evidence suggests that it is common for exam takers to prepare for an exam more than a year [56]. Because both additional schooling and preparing for job-related exams are so costly that young individuals cannot afford them by themselves, parental support can be crucial in determining time to a first job and labor market outcomes.

³Statistics Korea defines six categories of job-related exams: exams for enterprises, media and public enterprises, teachers, general public officials, high-ranking public officials and professional jobs, and technicians and others. In 2010, young individuals who prepare for civil servant exams for general public official positions account for 32.3 percent among the population of youth preparing for job entrance exams [98].

IV Data

IV.A The Graduates Occupational Mobility Survey (GOMS)

The Graduates Occupational Mobility Survey (GOMS) was first implemented in 2006. The survey is designed to alleviate mismatch between labor supply and demand, and to help students make major choices by providing up-to-date information on Korean labor markets. The GOMS samples about 18,000 college graduates each year, which cover approximately 3-4 percent of the population of interest. Individuals are interviewed either 18 months or 24 months after their graduation, depending on whether they graduate in August or in February. By the time of the survey, most graduates have completed transitions from school to work.⁴ At the beginning, the GOMS implemented a follow-up survey two years after the initial survey, but it has become a cross-sectional survey since 2011. For the purpose of this study, I use the panel waves surveyed from 2009 to 2011, which cover individuals who graduated from college between August 2007 to February 2010.

The survey collects data on school experience, job preparation, and early employment history of recent college graduates. In particular, the GOMS asks survey participants about their reservation wages and routes through which they obtained the jobs. The richness of information allows me to explore mechanisms through which parental housing wealth might influence job search and outcomes of the graduates. The GOMS also contains detailed information on parental characteristics, including their combined income levels, occupations, and educational attainments.⁵

Although the GOMS is well-suited for the purposes of my research in many dimensions, there are limitations of using the data as well. First, the GOMS does not have information on parental housing wealth, which is the variable of interest. To overcome this limitation, I estimate the housing wealth using data on household characteristics, housing characteristics, and residential locations of households. Section [IV.B](#) discusses the estimation procedure in

⁴For example, 83.2 percent of the GOMS participants between the survey year 2009 to 2011 were either employed (74.6 percent) or enrolled in schools (8.6 percent) when they were interviewed.

⁵The survey asks combined income levels of parents at two different points in time: (i) when their children entered college and (ii) when they were interviewed. In the analysis, I only use the former information.

detail. Moreover, I can observe home ownership status of parents and their housing types only for young adults who lived with their parents three and a half years (or four years for those graduated in August) after graduation.⁶⁷ This raises a concern regarding sample selection bias. I discuss issues related to sample selection in Section IV.D.

IV.B Parental housing wealth estimation

To impute parental housing wealth, I use data from the Korea Housing Survey (KHS). Starting from 2006, the KHS interviews approximately 30,000 households every two years and asks questions regarding characteristics of past/current residential homes, moving history, housing preferences, etc. The survey also collects household characteristics including information on various types of household assets.

The imputation procedure consists of two steps. First, I estimate the following linear equation using the KHS data:

$$\ln(size_i) = \alpha_0 + \beta Location_i + \gamma Type_i + \delta Inc_i + \eta Edu_i + \theta Year_t + \varepsilon_i \quad (1.7)$$

where $size_i$ is the size (in m^2) of residential home of household i and $Location$, $Type$, Inc , and Edu are vectors of dummies for residential locations, housing types, income brackets, and education levels of a household head, respectively. $Year$ is a vector of year dummies.⁸ Once equation (1.7) is estimated, I plug the GOMS data into the estimated equation to predict the house size. Then, I multiply the predicted house size by the municipality-specific per-unit (m^2) home price for a given housing type to obtain synthetic housing values. Because data on per-unit home price is not publicly available at the municipality level, I construct per-unit home prices of municipalities using administrative data on assessed housing values provided by the Korea National Spatial Data Infrastructure Portal. The data covers the universe of housing units in South Korea and contains information on housing values assessed for tax purposes as well as house size and location.

⁶⁷Data on home ownership status and housing types are only available in the follow-up surveys of the earlier GOMS. If a young adult lived with his parents, I assume that he provided housing characteristics of the parents, unless he was a head of the household.

⁷I consider three housing types: house, apartment, and multi-family residential housing.

⁸See Table A.1 in Appendix A for variable descriptions.

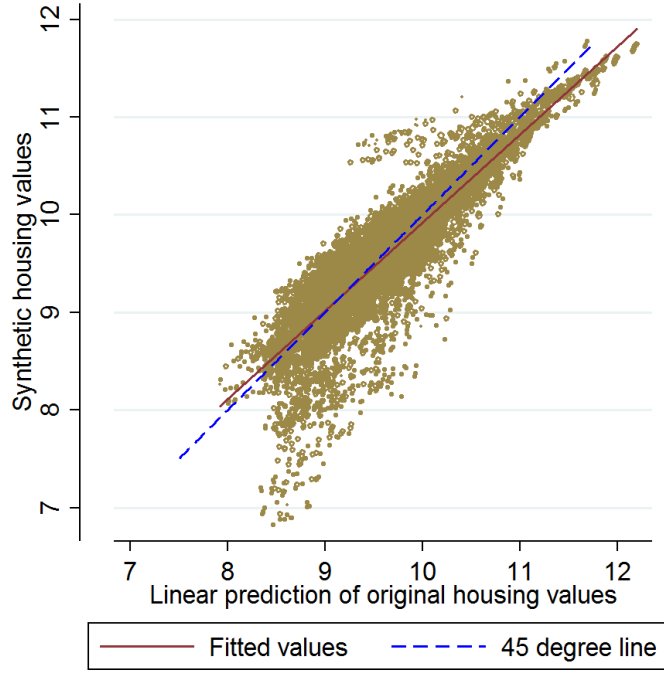


Figure 1.2: Correlation between original housing values and imputed housing values

Note: The x-axis represents the log of the linear predication of original housing values from the KHS data using the covariates in equation (1.7) as predictors. The y-axis represents the log of imputed housing values obtained following the procedure that I describe above. Both values are expressed in thousands of 2010 Korean Wons.

One way to test the performance of the imputation is to plot the correlation between original housing values and imputed housing values. Because I discard variation that is unexplained by covariates in equation (1.7), it is appropriate to compare the synthetic housing values to the corresponding first-stage linear prediction of the original housing values. As shown in Figure 1.2, the imputed housing values closely follow the original housing values along the 45 degree line. The correlation between the two values is 0.924.

Thus far, I use housing wealth and housing value interchangeably. However, to be precise, a housing value does not necessarily represent one's housing wealth. If an individual purchases a house using significant debt, the discrepancy between the two will be nontrivial. Nevertheless, I argue that the distinction should not make a big difference in this analysis. First, the correlation between the home value and the home equity in level terms is 0.974

in the KHS sample.⁹ Second, regardless of the size of housing debt, homeowners will fully enjoy gains and losses due to changes in their home values. Third, Bostic et al. [12] show that the elasticity of consumption with respect to housing value is similar to the elasticity of consumption with respect to housing wealth. Fourth, for households who own multiple houses, which consist of 13.6 percent of the KHS sample, the difference between the value of the residential home and the housing wealth is larger. Note that the underrepresentation of housing wealth for multi-home owners will bias the estimates upward.

IV.C Duration calculation

To calculate job search durations of young adults, I need to identify the time of the job market entry and first employment. I set the time of job market entry to a year before graduation. First, many students start searching for jobs when they become seniors. Second, a smaller number of observations are left-censored under this definition, compared to using the time of graduation as an alternative definition. Third, Light [69] shows that the choice of career starting time affects returns to schooling as a different starting time is associated with a different level of “pre-career” work experience. My definition of the job market entry would be more inclusive of the “pre-career” by allowing individuals to begin their careers a year before graduation.

For identification of a first job, I use questionnaires from the GOMS. Under the current employment section, the survey asks participants whether their current job is a first job. The survey also has a separate section for first employment experiences and asks participants whether they have had a first job and when they started working on that job. For those who have never been employed at the time of the initial survey, I use data on the employment history available in the follow-up survey to figure out the time of the first employment.

Based on the above discussion, I define the primary measure of duration as the number of months from the time of job market entry to a first job. Figure 1.3 shows the distribution of search spells. Two features are noticeable. First, there exists a spike in the number of individuals who found a job around the time of graduation, which is not surprising. The

⁹I weight observations using the KHS sampling weights in calculating the correlation. However, the unweighted correlation is very similar.

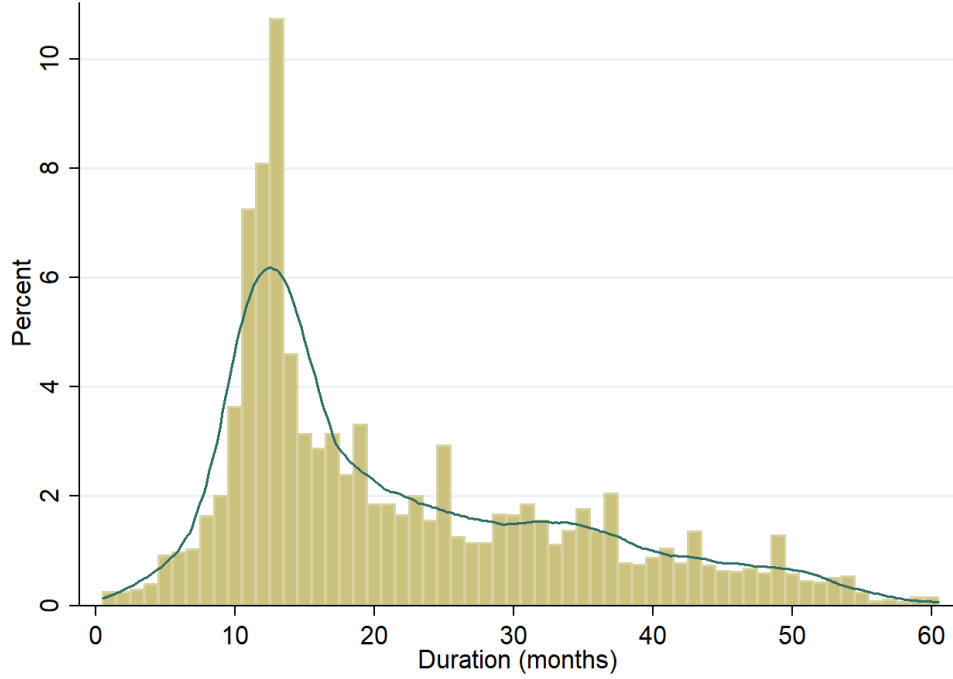


Figure 1.3: The distribution of search spells

Note: The duration is the number of months from the time of job market entry to a first job. The time of job market entry is set to a year before graduation. Observations with negative duration are dropped. I use the optimal kernel width to plot the kernel density estimates.

majority of graduates made the transition from school to work within 3 months of graduation and about 72 percent of graduates found a job within a year of graduation. Second, the distribution has a very long right-tail. Observations are right-censored for individuals who remain non-employed by the time of the follow-up survey.

IV.D Sample selection

The estimation sample consists of young adults (i) who attended high schools in municipalities with school equalization policy, (ii) who enrolled in 4-year college within 2 years after graduating from high schools, (iii) who did not have a first job until a year before graduation (i.e., job market entry), and (iv) who were living with their parents at the time of the follow-up survey.

The first criterion is required since I approximate municipality of parental residence based on high school locations given that the secondary school equalization policy in South Korea forces students to attend schools within certain distance of their municipality of residence.¹⁰ For individuals who graduated from schools to which the equalization policy did not apply, high school location is a poor proxy for their residence. Moreover, if students were able to sort into high-quality schools and home prices of municipalities in which good schools were located grew faster, it would render spurious correlation between school quality and search spells.

I focus on 4-year college students which account for about two thirds of the GOMS sample. The sample of 2-year college shrinks further when I restrict the sample to general education high school graduates, which makes identification difficult.¹¹ In addition, because 4-year college education is less directly attached to developing skills that industry demands, school-to-work transitions of 4-year college graduates also has an important policy implication. I also focus on students who do not have more than a 2-year gap between high school graduation and college enrollment.

I exclude individuals who had worked full-time before the labor market entry since they are different than their counterpart in several aspects. For example, the average graduate who had a full-time job before the job market entry is about 9 years older and less likely to live with their parents than their counterpart. Furthermore, their work experience might facilitate job search either through more information, a wider network, and/or accumulated wealth.

Finally, I only use a subsample of individuals who lived with their parents at the time of the follow-up survey. GOMS only asked information on home ownership and housing types in the follow-up survey and that information is only relevant when young adults cohabitated

¹⁰In general, students who plan to attend general education high schools first apply for schools within their *Hakgun*, which consists of multiple municipalities. Schools admit students based on their preferences to some extent and then remaining slots are randomly assigned based on proximity to schools from students' residence. The school equalization policy applies to both public and private general education high schools, but the policy does not apply to vocational and specialized high schools.

¹¹GOMS also surveys individuals who graduate from education college. I exclude those individuals, which make up less than 2 percent of the sample.

with parents.¹² Rosenzweig and Wolpin [86] and Kaplan [55] show that parental assistance in the form of shared residence helps young individuals insure themselves from labor market risks. The evidence suggests that job search durations might be endogenous to the living arrangements of young individuals. In other words, individuals who were not successful in finding jobs could sort into coresidence. Ignoring the selection would overestimate the effect of parental housing wealth on search spells. On the other hand, the selection can also result in underestimation of the wealth effect if coresidence and monetary support are substitutes as Rosenzweig and Wolpin [86] assume. Lastly, I drop individuals with missing covariates.

Table 1.1 shows descriptive statistics for each stage of sample selection. Starting from the full sample in column (1), I add an additional restriction from one column to the next. The resulting estimation sample consists of 8,250 college graduates, covering about 15.4 percent of the full sample. On average, individuals in the estimation sample remained nonemployed longer, and had richer, and better educated parents. The sample also contains more female individuals. While most of the differences are due to the selection of 4-year college students, restricting the sample to young adults who cohabitated with their parents leaves a population whose parents experienced relatively large gains in their housing wealth. It is possible that endogenous living arrangements can confound the effect of parental housing wealth on search durations. The average municipality-level 3-year home price difference per square meter is 0.34 MKRW. This leads to a 31,089 MKRW (or a 10.7 percent) increase in imputed parental housing wealth net of inflation.

¹²To be precise, GOMS lacks data on homeowners. However, it is very rare for a young individual to become a homeowner in three years after graduating from college and to live with his parents. Thus, I simply assume that parents are homeowners when they cohabitate with their children.

Table 1.1: Descriptive statistics

	(1) Full Sample		(2) Sample 1		(3) Sample 2		(4) Sample 3		(5) Sample 4		(6) Estimation sample	
Duration (month)	12.48	(35.92)	18.90	(14.10)	18.93	(14.20)	20.13	(11.85)	21.75	(12.52)	21.71	(12.48)
Duration (month, censored)	13.85	(34.43)	19.69	(13.08)	19.71	(13.16)	20.83	(10.73)	21.47	(10.63)	21.41	(10.59)
Housing wealth (MKRW)	-	-	-	-	-	-	-	-	278.10	(228.82)	283.56	(229.80)
3-year changes in housing wealth (MKRW/ m^2)	0.25	(0.59)	0.25	(0.63)	0.29	(0.67)	0.29	(0.67)	0.33	(0.70)	0.34	(0.71)
Parental income, 0 to 1MKRW	0.09	(0.29)	0.04	(0.19)	0.04	(0.19)	0.04	(0.19)	0.03	(0.17)	0.03	(0.17)
Parental income, 1 to 2MKRW	0.13	(0.34)	0.11	(0.31)	0.10	(0.30)	0.10	(0.30)	0.09	(0.29)	0.09	(0.28)
Parental income, 2 to 3MKRW	0.25	(0.43)	0.24	(0.43)	0.24	(0.43)	0.24	(0.43)	0.24	(0.43)	0.24	(0.43)
Parental income, 3 to 4MKRW	0.23	(0.42)	0.25	(0.43)	0.25	(0.43)	0.25	(0.44)	0.26	(0.44)	0.26	(0.44)
Parental income, 4 to 5MKRW	0.14	(0.35)	0.16	(0.37)	0.16	(0.37)	0.16	(0.37)	0.17	(0.37)	0.17	(0.38)
Parental income, 5 to 7MKRW	0.09	(0.29)	0.11	(0.32)	0.12	(0.32)	0.12	(0.32)	0.12	(0.32)	0.12	(0.33)
Parental income, over 7MKRW	0.07	(0.25)	0.09	(0.28)	0.09	(0.29)	0.09	(0.29)	0.09	(0.28)	0.09	(0.29)
Share of fathers received higher education	0.32	(0.46)	0.39	(0.49)	0.41	(0.49)	0.41	(0.49)	0.42	(0.49)	0.42	(0.49)
Share of mothers received higher education	0.16	(0.37)	0.21	(0.41)	0.23	(0.42)	0.23	(0.42)	0.23	(0.42)	0.23	(0.42)
Male	0.55	(0.50)	0.57	(0.49)	0.57	(0.50)	0.56	(0.50)	0.50	(0.50)	0.50	(0.50)
Age (at the time of survey)	27.18	(5.46)	26.37	(2.08)	26.40	(2.10)	26.37	(2.07)	26.05	(1.84)	26.07	(1.82)
Number of household members	3.08	(1.39)	2.96	(1.40)	3.11	(1.35)	3.11	(1.35)	3.65	(1.05)	3.65	(1.04)
Rank	-	-	60.56	(47.55)	58.74	(46.89)	58.57	(46.83)	63.07	(46.91)	62.51	(46.74)
GPA (100)	80.30	(18.14)	79.77	(15.73)	79.70	(15.83)	79.77	(15.77)	79.77	(15.73)	81.39	(8.52)
Student loan take-up	0.11	(0.31)	0.10	(0.31)	0.10	(0.31)	0.10	(0.30)	0.09	(0.29)	0.09	(0.29)
Dual degree	0.16	(0.36)	0.22	(0.42)	0.23	(0.42)	0.23	(0.42)	0.23	(0.42)	0.24	(0.43)
Work experience	0.65	(0.48)	0.73	(0.45)	0.73	(0.45)	0.73	(0.45)	0.74	(0.44)	0.75	(0.43)
TOEIC (takers only)	742.81	(149.40)	758.91	(139.25)	762.44	(137.98)	763.02	(137.57)	749.01	(143.86)	751.07	(142.68)
Study Abroad	0.17	(0.37)	0.23	(0.42)	0.24	(0.43)	0.24	(0.43)	0.23	(0.42)	0.24	(0.42)
Qualification	0.51	(0.50)	0.50	(0.50)	0.50	(0.50)	0.50	(0.50)	0.52	(0.50)	0.52	(0.50)
4-year college graduates			✓		✓		✓		✓		✓	
School equalization policy					✓		✓		✓		✓	
Non-negative durations							✓		✓		✓	
Coresidence and home ownership									✓		✓	
No missing covariates											✓	
Observations	53626		32412		26218		25523		9356		8250	

Note: See Table A.1 in Appendix A for variable descriptions.

V Empirical strategy

The primary goal of this paper is to estimate the effect of parental housing wealth on job search durations of their children. Since the outcome of interest is time to employment and it is right-censored for individuals who fail to get a job during the estimation window, I rely on a survival analysis framework. Specifically, I estimate the following Cox proportional hazard (PH) model stratified by college major:

$$\ln(h_{i,h,j,t}) = \beta_0^g + \beta_1 \ln(hwealth_{i,h,j,t}) + \Gamma X_{i,j,t} + \phi \Omega_{k,t} \quad (1.8)$$

where $h_{i,h,j,t}$ is the employment hazard of individual i in month t whose parents own a home of type h in municipality j . β_0^g is the baseline hazard specific to major g .¹³ $hwealth_{i,h,j,t}$ denotes the imputed parental housing wealth of individual i in month t . $X_{i,j,t}$ includes individual i 's age, sex, major, college ranking, time of graduation, and high school types. $X_{i,j,t}$ also includes combined income, educational attainments, occupations, residential location, home ownership status, and housing type of i 's parents. $\Omega_{k,t}$ is a vector of provincial-level macroeconomic factors, including unemployment rates, the number of employees per establishment, and GDP per capita at the time of graduation.¹⁴

The coefficient of interest is β_1 . To obtain an unbiased estimate for β_1 , parental housing wealth should not be correlated with the error terms ε_i . However, endogeneity can arise for several reasons. The classical example is unobserved abilities transmitted from parents to their offspring [93, 19]. For example, β_1 would underestimate the wealth effect if graduates from wealthier families tend to have greater abilities, and thus exit unemployment status quicker. In a similar vein, a greater level of housing wealth may be correlated with time preference of individuals [28], or parents who live in a rich neighborhood may have a better informal network [9].

I address concerns regarding endogeneity using a 3-year difference in per-unit home price for a given housing type. With a deregulation of the housing finance market in late 1990s and the recovery from the Asian Crisis in 1997, South Korea's housing market expanded

¹³The GOMS is primarily stratified by college major.

¹⁴See Table A.1 in Appendix A for variable descriptions.

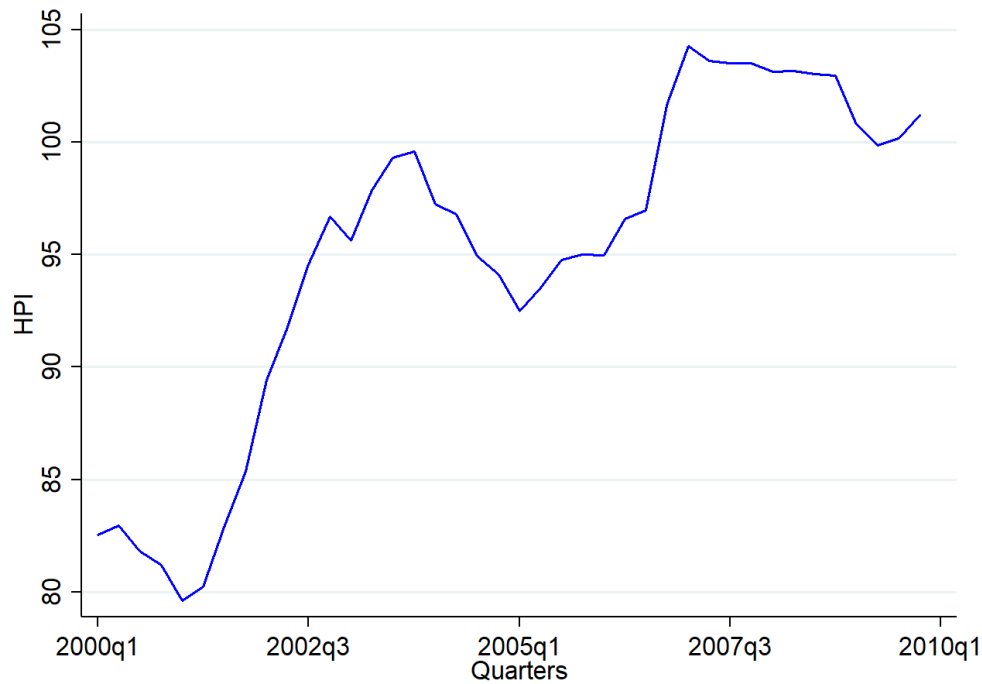


Figure 1.4: The trend in quarterly real housing price index in South Korea

Source: Bank for International Settlements, Real Residential Property Prices for Republic of Korea, retrieved from FRED, Federal Reserve Bank of St. Louis

dramatically [40]. Figure 1.4 shows the trend in real housing price index (HPI) in South Korea during 2000s. In general, there exists an upward trend in the HPI. A drop in HPI started from the first quarter of 2004 was a result of the government policy to curb the soaring housing price. Housing price declined once again at the time of Great Recession, but the dip was not as dramatic as the collapse of the U.S. housing market. The rise and fall of the Korean housing market during 2000s provides a natural experiment to test the effect of housing wealth on outcomes of interest.

Lovenheim [71] also uses a short-run change in housing wealth as an instrument for the level of parental housing wealth. There are several mechanisms through which housing wealth effects manifest. One channel is so-called pure wealth effect [24], suggesting that increased household wealth would lead to greater spending, regardless of whether the housing wealth is liquidated. Another channel is home equity extraction [77, 71]. The Bank of Korea [101]

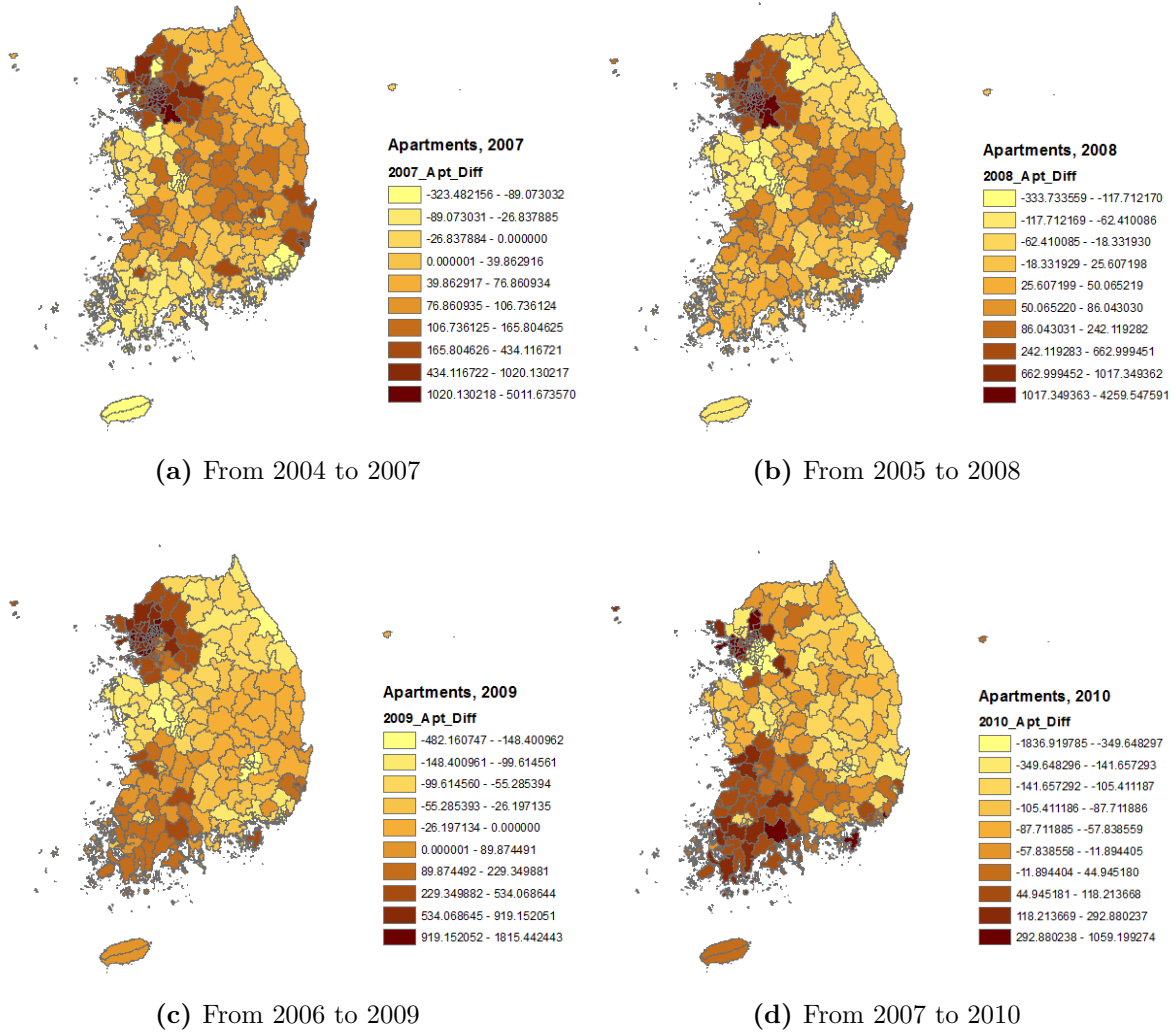


Figure 1.5: 3-year changes in home prices per unit (m^2): Apartments

Note: I construct each map by grouping municipalities into ten deciles of 3-year changes in home prices per square meter. See Section IV.B for home price construction details.

documents that South Korean households use about 40 percent of mortgage lending for purposes other than home purchases.

Although the strategy mitigates potential biases due to endogeneity, wealth differentials are still a function of wealth itself and there might be mechanical correlation between the two that leads to violation of the exclusion restriction. However, I exploit variation in parental housing wealth picked up by within-municipality differences in per-square-meter home prices across years and housing types, which is less prone to endogeneity as it does not encompass variation in housing sizes. Figure 1.5 shows the evolution of the 3-year changes in home prices for apartments in South Korea. Although the home wealth appreciation is usually higher in Seoul Metropolitan City and other cities around it, the figures clearly show that within-municipality variation is also substantial.

Due to nonlinearity in the structural equation (1.8), I use a two-step control function approach instead of relying on an instrumental variable approach. In particular, the first-stage equation is given as

$$\ln(\widehat{hwealth}_{i,h,j,t}) = \alpha_0 + \alpha_1 \Delta hprice_{h,j,t} + \Lambda X_{i,j,t} + \psi \Omega_k + \mu_{i,h,j,t} \quad (1.9)$$

where $\Delta hprice_{h,j,t} \equiv hprice_{h,j,t} - hprice_{h,j,t-36}$ and $hprice_{h,j,t}$ is a home price per m^2 for housing type h in municipality j . I first estimate equation (1.9) using OLS in order to obtain the residual, $\mu_{i,h,j,t}$. Then, I add $\mu_{i,h,j,t}$ in equation (1.8) as an additional covariate to directly control for the endogeneity. Since the residual is a generated regressor, the usual standard errors are incorrect. Therefore, I adjust the standard errors using bootstrap.

VI The effect of housing wealth on search spells

VI.A Baseline results

I begin by plotting the Kaplan-Meier survival curves for young adults who received parental housing shocks of different intensity. Since housing wealth can be correlated with factors such as abilities or neighborhood, simply plotting the survival curves for two different levels of housing wealth would fail to show the wealth effect. Instead, I use variation in the

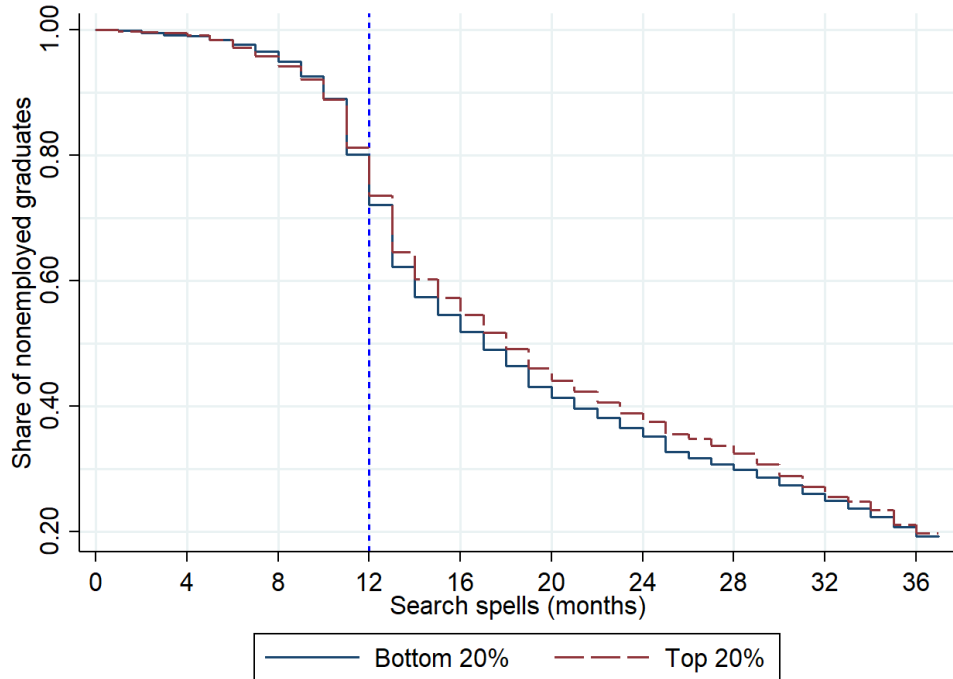


Figure 1.6: Kaplan-Meier survival curves

Note: The solid line represents the survival curve for individuals who belong to the bottom 20 percent of the parental housing shock distribution and the dashed line represents the survival curve for individuals who belong to the top 20 percent of the parental housing shock distribution. See the text for housing shock construction details.

home price instrument, $\Delta hprice_{h,j,t}$. More specifically, I regress $\Delta hprice_{h,j,t}$ on dummies for municipalities, year, and housing types, and obtain the residuals. The variation in the residuals is plausibly exogenous and can be used to demonstrate the causal effect of housing wealth on job search durations.

Figure 1.6 shows the Kaplan-Meier survival curves for individuals who belong to the bottom 20 percent and the top 20 percent of the housing wealth residuals distribution in a given year. While individuals in the top 20 percent group experienced gains of about 1.14 MKRW per m^2 of their parents' homes, those in the bottom 20 percent group experienced only losses of 0.23 MKRW. The survival rates for the two groups look similar before graduation. However, graduates who received a stronger housing wealth “treatment” tend to stay unemployed longer than their counterparts once they left college. The two curves diverge over time until around month 28, then the gap starts shrinking. This pattern becomes more pronounced when comparing graduates in the bottom 5 percent of the treatment

distribution to those in the top 5 percent, but less pronounced when comparing the bottom 50 percent group to the top 50 percent group (see Figure A.1 in the appendix). Non-parametric estimation results presented in Figure 1.6 and Figure A.1 suggest that an increase in parental housing wealth leads to a longer search duration of their children, which is consistent with evidence from the UI literature.

To further complement the graphical evidence, I estimate the Cox PH model using the control function approach. Table 1.2 summarizes estimation results. Column (1) estimates equation (1.8), which serves as a baseline specification. The coefficient estimate for $\ln(hwealth)$ is -0.97 and statistically significant at 5 percent. It implies that a one percent increase in parental housing wealth reduces the employment hazard of college graduates by 0.97 percent. In other words, a greater level of parental housing wealth allows individuals to search longer. The residual obtained from the first-stage equation is positive and statistically significant at 5 percent. Given that I control for all predictors of parental housing wealth in the second-stage equation except the instrument, the significant estimate for the residual term indicates that even within-municipality variation in per-unit home price level is endogenous and, therefore, should be accounted for.

The employment hazard for students whose parents earned 1MKRW to 2MKRW per month at the time of college entrance was 17 percent lower than students whose parents earned 0 to 1MKRW. For all other income groups, however, I do not find any evidence of parental income effect. This may indicate that the income effect only exists for those who are severely credit-constrained. It is also possible that parental income serves as a sufficient statistic that encompasses unobserved individual characteristics such as abilities passed on to their offspring to some extent. The positive coefficient for the top two income brackets may suggest that this non-financial aspect dominates the pure income effect for those at the right-tail of the income distributions. Male students tend to search longer than female students, but for only up to the age of around 29. While an increase in the number of household members is associated with a 5 percent decrease in the hazard rate, a better school quality measured by college ranking facilitates transitions from college to work. Among provincial-level macroeconomic factors, only GDP per capita negatively affects the transitions.

Table 1.2: The effect of parental housing wealth on search durations: Baseline estimates

	Cox proportional hazard		Accelerated failure time	
	(1)	(2)	(3)	(4)
Housing wealth (in log)	-0.9774** (0.4842)	-1.0580** (0.4824)	0.5370* (0.2902)	0.4860* (0.2757)
	[-1.8779, 0.0855]	[-1.9233, -0.0089]	[-0.0529, 1.0648]	[-0.0617, 1.0162]
Male	-0.8124 (0.5107)	-0.7973 (0.5108)	0.5855 (0.3108)	0.6085** (0.2995)
	[-1.7950, 0.1991]	[-1.7963, 0.2287]	[-0.0340, 1.1943]	[0.0004, 1.1910]
Age	-0.0019 (0.0149)	-0.0044 (0.0152)	0.0092 (0.0094)	0.0119 (0.0093)
	[-0.0291, 0.0290]	[-0.0313, 0.0280]	[-0.0087, 0.0280]	[-0.0060, 0.0302]
Male $\times$ Age	0.0286 (0.0198)	0.0298 (0.0199)	-0.0219 (0.0121)	-0.0236** (0.0117)
	[-0.0108, 0.0658]	[-0.0100, 0.0680]	[-0.0458, 0.0022]	[-0.0459, 0.0000]
Number of household members	-0.0523*** (0.0146)	-0.0509*** (0.0148)	0.0301*** (0.0082)	0.0294*** (0.0080)
	[-0.0784, -0.0221]	[-0.0781, -0.0207]	[0.0130, 0.0451]	[0.0127, 0.0437]
College ranking	0.0006 (0.0004)	0.0005 (0.0004)	-0.0004 (0.0002)	-0.0004 (0.0002)
	[-0.0001, 0.0013]	[-0.0002, 0.0013]	[-0.0009, 0.0000]	[-0.0009, 0.0001]
Parental income, 1 to 2MKRW	-0.1696 (0.1241)	-0.1993 (0.1277)	0.1197 (0.0822)	0.1177 (0.0800)
	[-0.3871, 0.0985]	[-0.4193, 0.0861]	[-0.0469, 0.2738]	[-0.0463, 0.2709]
Parental income, 2 to 3MKRW	-0.1632 (0.1197)	-0.1767 (0.1220)	0.1090 (0.0816)	0.0998 (0.0793)
	[-0.3705, 0.0903]	[-0.3923, 0.0773]	[-0.0490, 0.2678]	[-0.0474, 0.2539]
Parental income, 3 to 4MKRW	-0.0997 (0.1048)	-0.1159 (0.1065)	0.0683 (0.0703)	0.0658 (0.0681)
	[-0.2954, 0.1117]	[-0.3194, 0.1038]	[-0.0641, 0.2098]	[-0.0641, 0.2046]
Parental income, 4 to 5MKRW	-0.0629 (0.1167)	-0.0687 (0.1176)	0.0532 (0.0761)	0.0457 (0.0741)
	[-0.2901, 0.1702]	[-0.2966, 0.1652]	[-0.0957, 0.2055]	[-0.0965, 0.2013]
Parental income, 5 to 7MKRW	0.1498 (0.1263)	0.1512 (0.1265)	-0.0669 (0.0789)	-0.0712 (0.0763)
	[-0.0909, 0.3983]	[-0.0885, 0.4015]	[-0.2207, 0.0900]	[-0.2225, 0.0788]
Parental income, 7MKRW over	0.2929* (0.1571)	0.2958* (0.1552)	-0.1653* (0.0949)	-0.1518* (0.0911)
	[-0.0079, 0.6066]	[-0.0013, 0.6013]	[-0.3487, 0.0133]	[-0.3276, 0.0199]
Unemployment rate	-0.0256 (0.0384)	-0.0271 (0.0402)	-0.0041 (0.0228)	-0.0000 (0.0235)
	[-0.1012, 0.0473]	[-0.1097, 0.0494]	[-0.0465, 0.0406]	[-0.0438, 0.0447]
Employee per establishment	0.3607 (0.2843)	0.3146 (0.2877)	-0.1734 (0.1805)	-0.1241 (0.1774)
	[-0.1745, 0.9401]	[-0.2018, 0.9295]	[-0.5141, 0.1755]	[-0.4678, 0.2096]
GDP per capita	-0.0000* (0.0000)	-0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
	[-0.0001, 0.0000]	[-0.0001, 0.0000]	[-0.0000, 0.0001]	[-0.0000, 0.0000]
GPA		0.0065*** (0.0024)		-0.0043*** (0.0015)
		[0.0018, 0.0113]		[-0.0072, -0.0014]

Table 1.2: The effect of parental housing wealth on search durations: Baseline estimates (continued)

	Cox proportional hazard		Accelerated failure time	
	(1)	(2)	(3)	(4)
Loan take-up		0.1154*** (0.0366) [0.0490, 0.1908]		-0.0699*** (0.0206) [-0.1122, -0.0320]
Dual degree		0.1076*** (0.0290) [0.0501, 0.1644]		-0.0644*** (0.0161) [-0.0977, -0.0354]
Toeic		0.0004*** (0.0002) [0.0001, 0.0007]		-0.0003*** (0.0001) [-0.0005, -0.0001]
I(Toeic > 0)		-0.4459*** (0.1166) [-0.6914, -0.2281]		0.3081*** (0.0745) [0.1627, 0.4549]
Working experience		0.1188*** (0.0327) [0.0464, 0.1790]		-0.0810*** (0.0200) [-0.1180, -0.0381]
Study abroad		0.0968*** (0.0358) [0.0230, 0.1637]		-0.0656*** (0.0209) [-0.1075, -0.0254]
Qualification		0.1444*** (0.0248) [0.0942, 0.1913]		-0.0926*** (0.0152) [-0.1234, -0.0634]
Residual	1.0366** (0.5019) [-0.0218, 1.9752]	1.1122** (0.4996) [0.0456, 2.0232]	-0.5745 (0.2998) [-1.1258, 0.0208]	-0.5250 (0.2850) [-1.0751, 0.0410]
PH-test (p-value)	0.9865	0.5269		
F-statistics	55.19	55.16	55.19	55.16
Observations	8250	8250	8250	8250

Note: All specifications include fixed effects for graduation year-month, municipality, education and occupation of both parents, housing type, and high school type. P-values for the PH-test demonstrate the validity of the proportionality assumption. The Kleibergen-Paap Wald F statistic shows the relevance of the instrument. Standard errors given in parentheses are clustered at the municipality level and obtained via 2,000 bootstrap replications.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In column (2), I add variables on which housing wealth might have impact. These variables include one's grade point average (GPA), English test score (TOEIC), dummy variables for student loan take-up, dual degree, work experience while in college, and study abroad. Note that the regressors would be endogenous if longer durations allow individuals to improve their qualifications for a job. On the other hand, omitting those controls can underestimate the insurance aspects of parental housing wealth on search, assuming that the wealth is positively correlated with overall quality of job seekers and a more qualified individual gets a job faster. The coefficient estimate for logged housing wealth indicates that a one percent increase in parental housing wealth reduces the employment hazard by 1.06 percent. Adding additional controls increases the size of the wealth effect. In addition, not surprisingly, the coefficient estimates for most variables that are newly added to the model are statistically significant at the one percent level with expected signs, suggesting that controlling for individual quality is important in explaining individual job search behavior. It is worth emphasizing that student loan takers who are likely to be credit-constrained tend to get a job quicker.

I prefer to present the Cox PH estimates because the model does not require an assumption regarding the baseline hazard, $\lambda_0(t)$, in equation (1.8). But it is more straightforward to interpret results from alternative parametric models because the dependent variable of those models is a duration rather than a hazard rate. Moreover, exploring whether the main finding holds in another specification is an interesting exercise on its own. Therefore, I estimate the accelerated failure time (AFT) model in column (3) and column (4). Based on the Akaike Information Criterion, I present the estimates from the AFT model where the hazard function is assumed to follow the log-logistic distribution.¹⁵ In column (3), the coefficient estimate for logged housing wealth is 0.537 and it is statistically significant at the 10 percent level. It implies that a one percent increase in parental housing wealth leads to a 0.7 ($= e^{0.537} - 1$) percent increase in search durations. The direction of the effect implied by the estimate is consistent with the estimates from the Cox PH model. When I add individual qualification controls as well as a loan take-up indicator in column (4), the estimate for logged

¹⁵I also estimate the ATF models with log-normal, Weibull, Gompertz, and exponential distributions. I find that estimates from those alternative models are qualitatively similar to the results presented in Table 1.2.

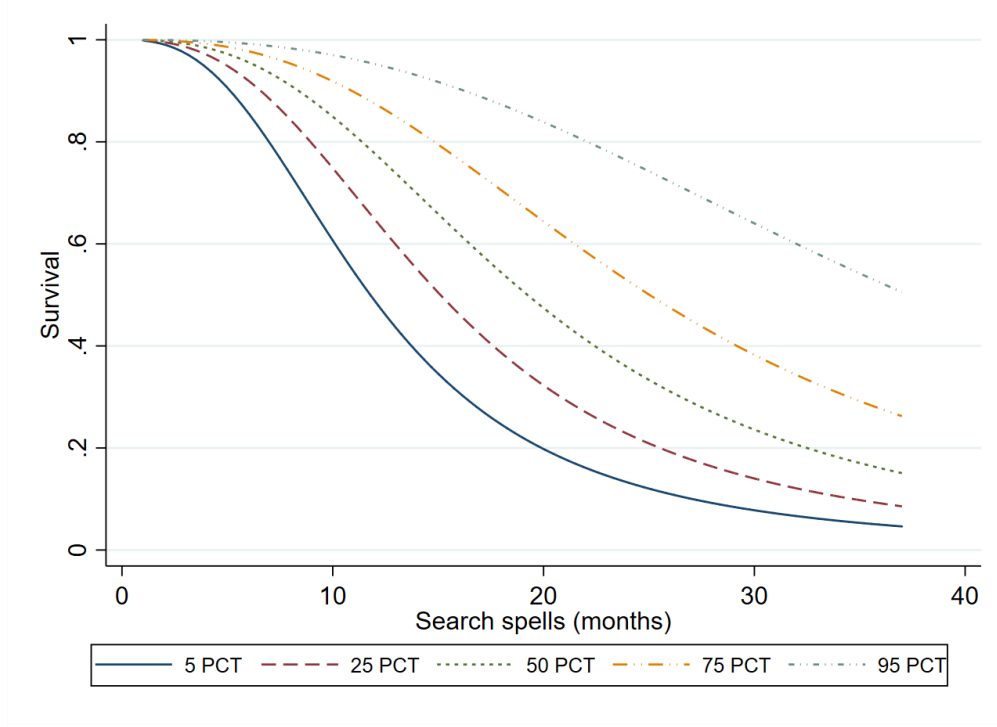


Figure 1.7: Survival curves by housing wealth percentiles

Note: This figure shows survival curves across different housing wealth percentile, implied by the result in column (4) of Table 1.2. All curves are evaluated at the average values of the covariates, except the housing wealth.

housing wealth is reduced to 0.486 with an implied elasticity of 0.63. Again, the results are qualitatively similar to the ones presented in column (2).

To evaluate economic significance of my results, I calculate how much shorter the average duration would have been, had parental housing wealth grown at the inflation rate before graduating from college. Specifically, I calculate the difference between the average duration implied by the AFT model given in column (4) of Table 1.2 evaluated at housing wealth at t and the average duration implied by the same model evaluated at housing wealth in $t - 36$. This exercise suggests that the average duration spell of college graduates would have shortened by roughly a month in the absence of housing wealth gains.

Remember that the results presented in Table 1.2 use imputed housing wealth. Since I control for all the predictors that are used to impute the size of parental housing, the variation that I use for identification comes exactly from the covariation between $hprice_{h,j,t}$ and $\Delta hprice_{h,j,t} \equiv hprice_{h,j,t} - hprice_{h,j,t-36}$. Although the two-step control function estimator

Table 1.3: Reduced-form survival model estimates

	Cox proportional hazard		Accelerated failure time	
	(1)	(2)	(3)	(4)
$\Delta hprice_{h,j,t}$	-0.0495** (0.0196)	-0.0535*** (0.0191)	0.0272** (0.0121)	0.0246** (0.0120)
PH-test (p-value)	1.0000	0.9435	N/A	N/A
Observations	8250	8250	8250	8250

Note: All specifications include individual characteristics, qualifications, and fixed effects for graduation year-month, municipality, education and occupation of both parents, housing type, and high school type. P-values for the PH-test demonstrate the validity of the proportionality assumption. Standard errors are clustered at the municipality level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

enables interpretation of the treatment effect in terms of housing wealth, it may be more efficient to fully exploit variation in short-run home prices, provided that the variation is exogenous. Thus, I estimate reduced-form survival models by replacing $\ln(\widehat{hwealth}_{i,t})$ in equation (1.8) with $\Delta hprice_{h,j,t}$, which does not rely on imputation.¹⁶

I present reduced-form estimation results in Table 1.3. I only report the coefficient estimates for $\Delta hprice_{h,j,t}$ to save space.¹⁷ The signs of the coefficient estimates for $\Delta hprice_{h,j,t}$ are consistent with the previous results. The Cox PH model estimates suggest that 1MKRW appreciation in home price per square meter of a municipality reduces the hazard rate of college graduates by 5 to 5.4 percent. Given that the average 3-year home price changes per square meter is 0.36MKRW in the estimation sample, the implied hazard rate reduction ranges from 1.8 to 1.94 percent. Results from the ATF models in column (3) and column (4) show that when per-unit home price of a municipality goes up by 1MKRW, it prolongs the duration of job search by 2.4 to 2.7 percent.

VI.B Tests of the proportionality assumption

The Cox PH model assumes that the effect of any covariate on the hazard ratio is constant over time. I test the validity of the assumption in two ways. First, I perform an analytic

¹⁶[73] and [72] provide similar reduced-form estimates, claiming that the short-run changes in housing wealth is conditionally exogenous and that any home price appreciation should be capitalized into housing wealth.

¹⁷Coefficient estimates for other covariates are qualitatively similar to those presented in Table 1.2.

test proposed by [42].¹⁸ As shown at the end of the Tables 1.2 and 1.3, the test fails to reject the null hypothesis of the constant hazard ratio in these specifications.

I also test whether the proportionality assumption is valid by plotting the log-log transformation of the survival function for major variables. The results are shown in Figure A.2. While the patterns are quite noisy up to the first three months of job search, the figures clearly evidence that the employment hazards are proportional across different values of a control variable, conditional on other covariates in the model. Because less than one percent of young adults in the estimation sample completed their transitions in the first three months, it is unlikely that any deviation from the proportionality assumption occurred in the early stage of job search affects my results.

VI.C Instrument exogeneity

The use of the instrument, $\Delta hprice_{jt}$, is only valid when (i) there exists enough correlation between the instrument and the endogenous variable and (ii) the instrument is uncorrelated with the error term of the structural equation (1.8), ε_i . The F-statistics presented at the end of Table 1.2 clearly show that the 3-year difference in home price at the municipal level is sufficiently correlated with the level of parental housing wealth. Although the validity of the second assumption, which is often called the exclusion restrictions, cannot be formally tested in a just-identified model, this section aims at providing some evidence that supports the exogeneity of the instrument.

As Section V describes, unobserved individual ability is one major source of endogeneity in studies exploring the intergenerational wealth effects [93, 19, 71]. Given that I control for several variables that must be strongly correlated with both individual and parental abilities, this particular type of endogeneity is unlikely to arise. Nevertheless, I test whether the instrument is associated with the ranking of the college from which an individual graduated using OLS. In South Korea where the college admission is based on a standard test score called *Suneung*, the ranking of the college serves as a proxy for individual ability. I present

¹⁸The idea of the test is to check whether the scaled Schoenfeld residual, which is the difference between observed values of a covariate and its expectation evaluated at each survival time, do not systematically evolve over time. In Table 1.2 and Table 1.3, I report p-values for the global test. For more details, see [92].

Table 1.4: Some tests of validity of exclusion restrictions

Dependent variable	(1) College ranking	(2) I(Informal)	(3) Hazard rate
Panel A: CF estimates			
Housing wealth (in log)	25.4053 (19.3183) [-11.4053, 65.1663]	-0.1482 (0.1623) [-0.4630, 0.1700]	0.9058 (0.8025) [-0.4302, 2.4483]
Panel B: RF estimates			
$\Delta hprice_{h,j,t}$	1.237 (0.837)	-0.007 (0.007)	0.034 (0.041)
PH-test (p-value)	N/A	N/A	0.6273
Observations	8250	8250	3702
R^2	0.245	0.071	N/A

Note: All specifications include individual characteristics, qualifications, local macroeconomic factors, and fixed effects for graduation year-month, municipality, education and occupation of both parents, housing type, and high school type. P-values for the PH-test demonstrate the validity of the proportionality assumption. Standard errors are clustered at the municipality level and given in parentheses. For CF estimates, standard errors are obtained via 2,000 bootstrap replications.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the results in column (1) of Table 1.4. Panel A summarizes the two-step control function estimates and Panel B summarizes reduced-form estimates. Both estimates imply that the correlation between college rankings and the instrument is not statistically significant.

Another threat to identification stems from the use of informal network to obtain a job [53, 9]. If the average quality of informal network of residents of a municipality evolved together with the housing price, the previous estimates would be biased. GOMS asks how individuals get into their job. About 13.3 percent of individuals who found a job answered that they got that job using family members, friends, and relatives. In column (2), I test whether young adults whose parents experienced larger short-run gains in housing wealth were more likely to get a job through informal networks. I estimate a linear probability model where the dependent variable takes 1 if an individual used an informal network and 0 otherwise. Again, I find no evidence that the local housing price predicts the likelihood of finding a job using informal networks in both Panel A and Panel B.¹⁹

Lastly, I implement a placebo test that is employed by [71]. The intuition is that the short-run home price changes only affect the housing wealth of homeowners. Renters would

¹⁹Reduced-form estimates from logit and probit models are qualitatively similar and available upon request.

gain nothing or even lose some portion of wealth if they were supposed to pay higher rents due to local home price appreciation. If the sign of the coefficient for parental housing wealth is the same for homeowners and renters, it might indicate that the results are driven by a spurious correlation rather than the intergenerational wealth effect. In contrast to the results from the homeowner sample, the coefficient estimates presented in column (3) are positive and statistically insignificant, reassuring that the relationship between parental housing wealth and time to first job is not driven by a spurious correlation.

VI.D Robustness checks

This section explores whether my results are robust to different specifications and alternative empirical setups. I summarize the results in Table 1.5. Column (1) presents control function estimates and column (2) presents reduced-form estimates. When it is feasible and relevant, I present both estimates.²⁰

I first examine sensitivity of the results to an alternative duration censoring. Previously, I censor search spells at 36 months. Although I provide some rationale behind the choice, it is still arbitrary and might influence the estimates. When the spells are censored at month 60, which is the maximum spell that I can observe in the data, economic and statistical significance of both estimates become smaller than the baseline estimates. This is potentially due to additional noise introduced by individuals who had very long spells, which are unexplained by the parental wealth effect. On the contrary, censoring the spells at month 30 results in larger estimates than the baseline estimates. Together with the results from specification (1), these results suggest that the role of parental housing wealth in supporting job search of their children is concentrated in the short run. Note that the Kaplan-Meier survival curve estimates in Figure 1.6 and Figure A.1 show similar patterns.

Specifications (3) and (4) test whether the functional form of the control function matters. The baseline model uses a simple linear control function, but this might not be sufficient if unobserved factors that are correlated with parental housing wealth nonlinearly affect

²⁰I only present two-step estimates in specification (3) and (4) because the choice of a functional form of the control function is not relevant in the reduced-form context. For specification (7), I only estimate the reduced-form model because the control function estimator fails to converge.

Table 1.5: Robustness checks

Specifications	Statistic	Control function (1)	Reduced-form (2)
(0) Baseline (N = 8250)	β_1 SEs PH-test 95percent CI	-1.0580** (0.4824) 0.5269 [-1.9233, -0.0089]	-0.0535*** (0.0191) 0.9727
(1) Censor durations at 61 months (N = 8250)	β_1 SEs PH-test 95percent CI	-0.7965* (0.4440) 0.7204 [-1.6575, 0.1040]	-0.0442*** (0.0170) 0.9727
(2) Censor durations at 30 months (N = 8250)	β_1 SEs PH-test 95percent CI	-1.1044** (0.5203) 0.9288 [-2.0768, 0.0176]	-0.0559*** (0.0209) 0.9948
(3) Quadratic control function (N = 8250)	β_1 SEs PH-test 95percent CI	-1.0453** (0.4825) 0.6229 [-1.9004, 0.0186]	
(4) Cubic control function (N = 8250)	β_1 SEs PH-test 95percent CI	-1.0456** (0.4845) 0.5996 [-1.8972, 0.0259]	
(5) Exclude individuals who attended college more than 6 years (N = 7504)	β_1 SEs PH-test 95percent CI	-1.2218** (0.5664) 0.3537 [-2.3342, -0.1461]	-0.0610** (0.0239) 0.9094
(6) School fixed effects (N = 8250)	β_1 SEs PH-test		-0.0578*** (0.0197) 0.0000

Note: All specifications include individual characteristics, qualifications, local macroeconomic factors, and fixed effects for graduation year-month, municipality, education and occupation of both parents, housing type, and high school type. P-values for the PH-test demonstrate the validity of the proportionality assumption. Standard errors are clustered at the municipality level and given in parentheses. For CF estimates, standard errors are obtained via 2,000 bootstrap replications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

job search of young adults. As alternatives, I use quadratic and cubic control functions and confirm that the results are robust to the choice of the functional form of the control function.

It is possible that a larger housing wealth allows individuals to postpone graduation so that they can have more time to prepare for the job market. Or, some students would not leave schools until they find a job to avoid the gap in their résumés. Since including those individuals in the estimation sample may underestimate the parental wealth effect on search spells, I exclude female (male) students who attended college more than six years (eight years) and estimate the baseline model in specification (5).²¹ Indeed, the implied wealth effect is about 15 percent larger than the estimate presented in column (2) of Table 1.2. The evidence suggests that parental wealth as insurance against job market risk in part works through a longer time to college completion.

Lastly, I estimate specification (6) to examine whether replacing the college ranking variable with institution fixed effects. The baseline equation controls for the school ranking as it captures the effect of institution quality on the employment hazards in a more parsimonious way than controlling for school fixed effects. However, school fixed effects account not only for the average school quality, but also for any unobserved school-specific characteristics fixed in time, which can affect the result. The reduced-form estimate with institution fixed effects is slightly larger than the baseline estimate and statistically significant at the 1 percent level.

Overall, the results presented Table 1.5 confirm that the negative relationship between parental housing wealth and search spells is robust to alternative specifications.

VI.E Heterogeneous treatment effects

This section investigates whether parental housing wealth varies across different population groups. As a first exercise, I divide the sample into individuals whose parental income is below 4MKRW or above 4MKRW monthly. Search theory predicts that an increase in wealth leads to a longer search duration by relaxing one's credit constraint [22]. If this is the mechanism that explains the results, assuming that intergenerational transfers are

²¹I set an eight-year restriction for male students to account for two years of military service, which is mandatory for male adults in South Korea.

Table 1.6: Heterogeneous treatment effects

	By income group		By gender	
Panel A: CF estimates				
	(1)	(2)	(3)	(4)
	Low income	Female	Male	High income
Housing wealth (in log)	-0.7363	-1.4602**	-0.9702	-1.4024**
	(0.7142)	(0.6787)	(0.8295)	(0.6490)
	[-2.1288, 0.7479]	[-2.7457, -0.0804]	[-2.9872, 0.4250]	[-2.5110 0.2567]
PH-test (p-value)	0.1125	0.6263	0.7466	0.0800
Observations	5096	3154	4140	4110
Panel B: RF estimates				
	(1)	(2)	(3)	(4)
	Female	Male	Female	Male
Housing wealth (in log)	-0.0351	-0.0730**	-0.0520	-0.0670*
	(0.0393)	(0.0358)	(0.0371)	(0.0360)
PH-test (p-value)	0.1125	0.6263	0.7466	0.0800
Observations	5096	3154	4140	4110

Note: All specifications include individual characteristics, qualifications, local macroeconomic factors, and fixed effects for graduation year-month, municipality, education and occupation of both parents, housing type, and high school type. P-values for the PH-test demonstrate the validity of the proportionality assumption. Standard errors are clustered at the municipality level and given in parentheses. For CF estimates, standard errors are obtained via 2,000 bootstrap replications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

proportional to all financial resources available to households, individuals whose parents make high income are less likely to be credit-constrained and thus affected less by exogenous parental wealth shocks.

The results are presented in Panel A of Table 1.6. The control function estimates presented in first two columns of Panel A suggest that the effect of parental housing wealth on job search of their children is stronger for young individuals from upper income households than those from lower income households in terms of both economic and statistical significance. The reduced-form estimates presented in the second two columns of Panel B corroborate the finding. These results are inconsistent with the idea that lower-income individuals are forced to get any job quicker to smooth consumption intertemporally.

However, note that two groups can be systematically different in many other aspects. For example, low income households might prioritize additional wealth differently than high income households. If low income households have higher discount rates than high income households [46], they may underestimate the value of the search and use additional resources

to satisfy more immediate needs.²² In other words, family support for job search might be a luxury good that low income households cannot afford unless the wealth gain from a housing boom is substantial. Furthermore, on average, the level of housing wealth gain was larger in high-income groups with greater variation (Figure A.3). This difference in treatment intensity and variation can also contribute to different responses across income groups.

As a second exercise, I test whether parental housing wealth affects male and female populations asymmetrically. Traditionally, Korean parents favor boys over girls. This phenomenon is dubbed son preference. While the sex imbalance at birth becomes less noticeable in terms of the sex ratio, Choi and Hwang [23] show that Korean parents devote more time and money to their first-born sons than first-born daughters. The persistence of son preference can result in heterogeneity in the effect of parental housing wealth on job search.

Column (3) and column (4) of Table 1.6 summarizes the results. Both control function and reduced-form estimates suggest that parental housing wealth reduces the employment hazard rate for male graduates and the effect is at least marginally statistically significant. Although the estimate for female graduates is also negative, it is not statistically significant. While the evidence is consistent with [23], it is difficult to draw a firm conclusion out of the results for two reasons. First, the upper limit of the bias-corrected confidence interval in column (4) of Panel A is positive and large relative to the previous estimates. Second, the reduced form estimates are not very different from each other. While it is not straightforward to test equality of two coefficient estimates from a nonlinear model, I can check whether an interaction between sex of individuals and the instrument matters. The coefficient estimate for the interaction term is not statistically significant with p-value of 0.869, implying that the heterogeneity in the effect of parental housing wealth on job preparation might be small at best.

²²Note that my argument is distinct from DellaVigna and Paserman [28] who focus on the effect of time preference on search intensity rather than on intra-household resource allocation.

Table 1.7: Labor market outcomes

	(1)	(2)	(3)	(4)
	ln(wage)	ln(wage3)	wage3 - wage	1(contract)
$ln(hwealth)$	-0.1907 (0.1960) [-0.5834, 0.2108]	-0.0423 (0.1619) [-0.3459, 0.2959]	6.9441 (42.4946) [-65.3102 98.1313]	-0.1070 (0.1621) [-0.4498, 0.1912]
F-statistic	60.424	59.830	60.055	61.062
Observations	7765	6869	6845	7578
R^2	0.0725	0.1091	0.0141	0.0199
	(5)	(6)	(7)	(8)
	1(big firm)	1(public)	ln(workhour)	1(start-up)
$ln(hwealth)$	-0.4798** (0.2075) [-0.9536, -0.1203]	0.3053** (0.1363) [0.0817, 0.6142]	0.0210 (0.1571) [-0.2906, 0.3275]	0.0583 (0.0451) [-0.0335, 0.1544]
F-statistic	55.164	55.164	59.705	59.410
Observations	8250	8250	7815	7810
R^2	0.0172	0.0072	0.0178	0.0016

Note: All specifications include individual characteristics, qualifications, local macroeconomic factors, and fixed effects for graduation year-month, municipality, education and occupation of both parents, housing type, and high school type. P-values for the PH-test demonstrate the validity of the proportionality assumption. The Kleibergen-Paap Wald F statistic shows the relevance of the instruments. Standard errors given in parentheses are clustered at the municipality level and obtained via 2,000 bootstrap replications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

VII Does larger parental housing wealth improve labor market outcomes?

I document that an exogenous increase in parental housing wealth reduces the employment hazard of college graduates. I also show that my finding is not driven by other confounding factors. Given the robustness of the finding, it is natural to ask whether parental housing wealth has any bearings on labor market outcomes. Search theory suggests that a longer search spell leads to a higher wage [27], whereas the argument is barely supported by empirical evidence [18, 64], even for first-time job seekers [35]. One notable exception is Nekoei and Weber [79], who find that individuals who are marginally eligible for the UI benefit extension receive a higher wage at a new job than those who are marginally ineligible for the UI benefit extension. The authors also find that individuals are more likely to get into firms that pay higher wages to their peer employees.

This section explores whether parental housing wealth improves the quality of the match measured by several labor market outcomes. Table 1.7 presents the estimates from the two-stage least squares. Column (1) examines the effect of parental housing wealth on wages at the first job. Consistent with most empirical studies, I do not find any evidence that a greater level of parental housing wealth leads to higher wages at the first job. It is possible that some college graduates from wealthier families may use a low-paid first job (such as internships) to accumulate skills that are required to get a better job and make more money in the longer run. The dependent variable in column (2) is the current wage at the time of the follow-up survey, which was implemented about three to four years after college graduation. The estimate is not statistically significant, but the magnitude of the estimate is reduced in the longer run, compared to the estimate presented in column (1). This motivates me to test whether parental housing wealth contributes to the wage growth, similar to Nekoei and Weber [79]. The estimate presented in column (3) is positive, but it is not statistically significant.

Among other outcomes, I find that a one percent increase in parental housing wealth reduces the probability of working in a firm that has 300 employees or more by 0.48 percentage points in column (5) and increases the probability of working in the public sector by 0.3 percentage points in column (6). These effects are statistically significant at the 5 percent level. In particular, the result in column (6) mirrors the finding of Rothstein and Rouse [88] who document that a rise in debt discourages U.S. college students from working in nonprofit, government, education sectors. A larger parental housing wealth has a negative impact on the likelihood of finding a fixed-term contract job and a positive impact on the likelihood of starting a business, but these effects are not statistically significant.

VIII Potential mechanisms

In this section, I investigate the mechanisms behind the negative causality between parental housing wealth and job search duration. A simple model proposed by Chetty [22] implies that an exogenous parental wealth increase leads to a longer search duration because it reduces the utility gap between the employment state and the unemployment state. On

Table 1.8: Potential mechanisms

Dependent variable	(1) ln(reservation wage)	(2) I(exam preparer)	(3) I(in school)	(4) I(unplanned schooling)
Panel A: CF estimates				
Housing wealth (in log)	-0.0767 (0.0883) [-0.2629, 0.0825]	0.0309 (0.1318) [-0.2198, 0.2985]	0.4517** (0.2004) [0.0758, 0.8577]	0.2610** (0.1266) [0.0283, 0.5386]
Panel B: RF estimates				
$\Delta hprice_{h,j,t}$	-0.0039 (0.0042)	0.0016 (0.0058)	0.0228*** (0.0083)	0.0132** (0.0054)
Observations	8202	8250	8250	8250

Note: All specifications include individual characteristics, qualifications, local macroeconomic factors, and fixed effects for graduation year-month, municipality, education and occupation of both parents, housing type, and high school type. P-values for the PH-test demonstrate the validity of the proportionality assumption. The Kleibergen-Paap Wald F statistic shows the relevance of the instruments. Standard errors are clustered at the municipality level and given in parentheses. For CF estimates, standard errors are obtained via 2,000 bootstrap replications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the other hand, Danforth [27] shows that a greater level of wealth raises one's reservation wage, and it is an increased reservation wage that prolongs the search. Empirical studies explore either the effect of relaxing credit constraints on search durations or the mechanism through which additional credits work. But only a paucity of studies consider both aspects at the same time, largely due to data limitations.²³

I begin by testing whether parental housing wealth negatively affects the hazard rate by raising one's reservation wage. The result is given in column (1) of Table 1.8. In contrast to the theory and empirical evidence that Bloemen and Stancanelli [11] provide, I do not find that an exogenous gain in parental housing wealth increases reservation wages of their children systematically. In fact, the sign of the estimate suggests that the causality might be negative, not positive. Using nonlinearities of the UI benefit schedule in New Jersey, Krueger and Mueller [62] also find a negative but statistically insignificant relationship between the size of the UI benefits and the ratio of the reservation wage to the previous wage.

Because GOMS does not collect data on consumption, I cannot test whether parental housing wealth changes individual consumption on food or clothing. Instead, I look at the effect of parental housing wealth on exam preparation, which can be considered as a

²³See Bloemen and Stancanelli [11] and DellaVigna and Paserman [28].

particular type of consumption. There are plenty of anecdotes that as many as half of young job seekers engage in some type of exam preparation, mostly for civil service jobs [56]. Because of the fierce competition, it is common for young Korean individuals to spend more than a year preparing for a test. An exogenous increase in parental housing wealth may encourage college graduates who otherwise would have found a job to study for exams. As shown in column (2), however, the hypothesis is not supported by the data.

Lastly, I examine the role of parental housing wealth on schooling decisions. Barr and Turner [7] show that extending UI benefit duration encourages unemployed workers to enroll in college in the United States. Similarly, I test if family insurance proxied by parental housing wealth induces enrollment in a new school after college graduation. For young adults who never had a job, additional schooling can be a continuation of human capital accumulation. Or, it can be a retreat from the labor market if individuals feel that they are not quite ready for the transition or decide to go back to school after an unsuccessful job search. In column (3), I do not make a distinction between the two motivations for schooling. The result suggests that a one percent increase in parental housing wealth raises the probability of going to school by 0.45 percentage point and it is statistically significant at the 5 percent level. Considering that the average housing price went up by 11.8 percent in the estimation sample, the estimate implies that the effect of parental housing wealth accounts for about one third of additional schooling after graduating from college ($= (11.8 \times 0.45) / 15.78$). This evidence is consistent with Lovenheim [71] who shows that an increase in parental housing wealth raises college enrollment.

Note that the sample in column (3) includes students who originally planned to continue their study. In general, it is unclear whether one should view schooling after college as a part of career preparation or pure human capital accumulation. However, if an individual has a time gap between college graduation and new school enrollment, his schooling decision is more likely to be accidental. So, I examine the effect of parental housing wealth on the incidence of unplanned schooling in column (4). The size of the estimate is reduced substantially, but it is still statistically significant at the 5 percent level, indicating that the probability of going back to school unexpectedly rises by 0.26 percentage point when

Table 1.9: The effect of parental housing wealth on time to first job: Revisited

	(1)	(2)
	Exclude all students	Exclude students w/o a time gap
Panel A: CF estimates		
ln(housing wealth)	-0.7374	-0.9337**
	(0.4499)	(0.4486)
	[-1.4987, 0.2887]	[-1.7055, 0.1098]
Panel B: RF estimates		
$\Delta hprice_{h,j,t}$	-0.0402	-0.0512*
	(0.0269)	(0.0263)
PH-test	206.8676	212.1750
Observations	6948	7429

Note: All specifications include individual characteristics, qualifications, local macroeconomic factors, and fixed effects for graduation year-month, municipality, education and occupation of both parents, housing type, and high school type. P-values for the PH-test demonstrate the validity of the proportionality assumption. Standard errors are clustered at the municipality level and given in parentheses. For CF estimates, standard errors are obtained via 2,000 bootstrap replications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

parental housing wealth increases by one percent. The result strongly supports the role of parental wealth in securing their children from potential labor market risks.

Given the evidence that a greater level of parental housing wealth increases the chance of getting additional schooling, I revisit the parental wealth effect on time to first job in Table 1.9 and see whether it is the school-to-school-to-work transitions that actually prolong time to first job. In column (1), I exclude all individuals who were in school at the time of initial surveys. Both the CF estimate and the RF estimate are not statistically significant and the size of the estimates become smaller relative to the baseline estimate presented in column (2) of Table 1.2. In column (2), I exclude students who did not have a time gap between college graduation and new school enrollment, but retain those who did have a time gap. The latter individuals are likely to enroll in school for reasons other than academic study. Now the estimates presented in both panels are at least marginally significant and their magnitudes are comparable to the baseline.

IX Conclusion

This study explores the effect of parental housing wealth on the school-to-work transitions of college graduates. I exploit within-municipality variation in 3-year difference in per-unit house prices across years and housing types, which is arguably exogenous to search behaviors of college graduates. I first look at whether exogenous gains in parental housing wealth prolong time-to-first-job of Korean youth and find that a one percent increase in parental housing wealth reduces the employment hazard of their children by roughly one percent. Other unobserved factors such as abilities or the quality of informal network cannot explain the result. Parental housing wealth also decreases the probability of working in firms that have 300 or more employees and increases the probability of getting a job in public sectors, probably through a longer job preparation. Given the popularity of public sector jobs among South Korean youth, the results can be interpreted that young individuals from wealthier families find a better job. Consistent with other empirical studies, I do not find that parental housing wealth has an impact on wages at the first job. However, there might be differences in fringe benefits or job tenure.

Further analysis shows that the wealth effect on school-to-work transitions is driven by young individuals who moved to school, in particular those who had a time gap between college graduation and new school enrollment. The evidence might suggest that the role of parental wealth as insurance to first-time job seekers is mainly to prevent them from having a gap between careers when a job search turns out to be unsuccessful. When this particular type of school-to-work transition is ignored, the parental wealth effect becomes small and insignificant, implying that the parental wealth effect on extending a narrowly-defined search duration is weak.

Overall, my findings imply that a greater level of parental wealth provides a wider range of options available to young individuals transitioning from school to work. A better family insurance would allow their young adult children to be more selective in accepting job offers or to receive further education for the purpose of either advancing human capital or waiting for future opportunities without damaging their careers. A direct deposit from the government to youth may have a very different impact than exogenous changes in parental

wealth, especially when low-income families and high-income families have heterogeneous consumption preferences. In general, a government subsidy to young job seekers would provide consumption smoothing benefits. Nevertheless, it is uncertain whether the subsidy affects job market outcomes given its small amount and limited availability.

Chapter 2

Do In-Kind Grants Stick? The Department of Defense 1033 program and local government spending

I Introduction

Intergovernmental transfers are a key source of funds to local governments. The National League of Cities reports that approximately 5 percent of municipal budgets comes through the federal government, while an additional 30-40 percent of local government revenues comes from states.¹ The motivation for and structure of grant programs are both important because this external funding will affect the allocation of local public resources, overall local spending, and subsequent welfare. In some cases grants may be used to redistribute resources from high-income to low-income areas, in turn addressing differentials in local tax capacity. In other instances, local government provision of a public good may be preferable to state or federal provision, either due to diseconomies of scale or heterogeneity in preferences between local jurisdictions. In these circumstances, providing grants to local governments for the provision of a particular service may generate more welfare than the federal government providing the service directly. Grants are also widely used to address interjurisdictional spillovers (as

¹<http://www.nlc.org/revenue-from-intergovernmental-transfers>

with transportation infrastructure), and they may serve as a lever for paternalistic oversight from higher levels of government in a federalist system. If local preferences lead to too little spending on a public good, state or national agencies may choose to encourage more provision through grants.

The underlying motivation for a grant matters because it determines how the grant should be structured to maximize welfare. At the heart of this issue is the extent to which grants from higher levels of government replace, or crowd out, lower-level government spending. For example, if the goal is simply to redistribute income, a lump sum grant would yield the largest welfare gain to local citizens. Such a grant, however, could crowd out a substantial degree of funds that local budget-setters would have committed in the absence of external support. Thus, if the impetus for a grant is to correct the under-provision of a public good or to elicit spillovers, the grant should be designed to minimize crowd-out and promote stickiness. In this case, crowding out is undesirable and may defeat the objectives of the grant program.

Intergovernmental grants typically crowd out some degree of recipient spending no matter how grant funds are earmarked, which is consistent with theoretical expectations outlined by Bradford and Oates [13]. For example, Lutz [74] examines the fiscal consequences of New Hampshire’s 1999 school finance reform and shows that the tax burden of local residents falls by 90 cents per grant dollar. Lutz attributes this finding to the context surrounding the reform, including the state’s direct democracy that allows for perfect reflection of median voter preferences, perfect information on the part of the voters regarding the reform, fiscal autonomy of the state, and fungibility of the grants. Crowding out is rarely perfect in settings that have been studied: most of the literature on intergovernmental grants shows that some share of grant proceeds translates into higher spending, a result dubbed the “flypaper effect” [49]. The extent of crowd out has been studied for a number of funding sources and funding intentions, including matched Title I education grants [41, 21], health care [6], highway funding [57], and law enforcement [5, 33]. A related literature describes agency or grant features that make income equivalence unlikely, usually highlighting institutional features that differ from the conditions considered by Bradford and Oates [13]. See, for example, Filimon et al. [34], Strumpf [99], Payne [83], Brooks et al. [14], Glaeser [39].

While much is known about the effect of intergovernmental funding transfers on recipient spending, the literature is much quieter, to date, on the effect of intergovernmental grants in-kind. There are numerous examples of in-kind transfers from the public sector directly to individuals, such as food assistance, health care, and housing vouchers [100, 26]. Such transfers have been shown to raise targeted consumption (i.e., supplement would-be spending in target areas) but perhaps not to the same extent as cash [25] and not without some substitution between private purchases and government provision [44]. In a related vein of research, Carruthers and Wanamaker [20] detect an incomplete flypaper effect arising from a pre-War school building campaign in the segregated South, which amounted to a series of large capital transfers from private philanthropies to public school districts. The question of whether intergovernmental in-kind transfers crowd out local government spending, however, is open and unstudied. Examples of in-kind transfers from one level of government to another include the Morrill Act establishing land-grant universities, emergency response equipment provided to local governments following natural disasters, and the Department of Defense 1033 program considered here.

We contribute to our understanding of the public finance implications of external grants by exploring local effects of the federal 1033 program, which provides surplus military gear and vehicles to local governments. This application is the first to our knowledge that investigates the fiscal impacts of intergovernmental grants taking the form of goods rather than income. Since 1997, Section 1033 of the National Defense Authorization Act has allowed for the transfer of surplus or decommissioned U.S. military equipment to local law enforcement agencies at a nominal price of zero. Decommissioned capital initially used to provide national defense is re-purposed for the provision of public safety. From 1997 through 2014, the department of defense transferred over \$5.2 billion dollars in equipment to local law enforcement agencies, making it the largest federal-to-local grant of capital goods of which we are aware.

The 1033 program offers a unique context in which to examine the effects of intergovernmental grants; in short, this setting is one where we would expect little to no crowding out. Indeed, transfers through the 1033 program are not intended to modify local spending or preferences, or to transfer resources from rich to poor. Items received through the 1033

program are physical goods and are thus less fungible than cash. They cannot be sold or transferred to other local governments. Crowding out is still possible, of course, and could manifest as intra-agency substitution within or across functions related to 1033 equipment. This is most likely to arise for in-kind goods that have close substitutes within the local government’s public safety budget. Additionally, decisions to acquire items through the 1033 program are made solely by police chiefs, typically without local institutional oversight, public input, or even a signature from a city or county government official. The preferences of law enforcement executives are thus pivotal and may or may not reflect those of other bureaucrats or the voting populace. As a result, the median voter is less relevant to the acquisition of goods. The opacity of the process implies that city or county officials with presumptive budgetary authority may have incomplete information about the amount of equipment that police chiefs acquire through the 1033 program. These features of the 1033 program are the inverse of the New Hampshire school finance reform that resulted in almost complete crowd out [74]. Accordingly, we expect that 1033 transfers will result in little to no crowding out.

To empirically evaluate the relationship between 1033 program receipts and local public spending on police protection, we use panel data on county expenditure accounts from the Annual Survey of Governments matched to the value of 1033 equipment transfers from the Defense Logistics Agency. Exploiting within-county intertemporal variation in 1033 acquisitions over time, we find that the value of 1033 receipts has no significant effect on local spending (i.e., a complete flypaper effect with no crowding out and no crowding in). In our baseline specification, the effect of 1033 acquisitions on local police spending is a relatively precise zero. Confidence intervals imply that a one percent increase in 1033 transfers affects police spending in the following year by an absolute value of no more than 0.02 percent. In no specification do we find statistically significant evidence that receiving goods through the 1033 program reduces spending. The robust lack of crowding out of intergovernmental transfers stands in sharp contrast to nearly all of the related grant literature.

While grants through the 1033 program are much less likely to reflect endogenous voter preferences than other intergovernmental transfers [57], local police spending might be correlated with other time-varying unobserved factors (e.g., preferences of police chiefs,

severity of departmental budget constraints, endogenous voter preferences, perhaps, or time-varying heterogeneity in law enforcement leadership). To address concerns about omitted variables driving both police budgets and 1033 receipts, we approach causal identification from a second angle exploiting exogenous variation in transaction costs faced by departments when acquiring 1033 items [45]. Instrumental variables estimates are less precise, but estimated coefficients for the effect of 1033 receipts on local police expenditures are positive, nearly significant, and more consistent with crowding in rather than crowding out.

Within the limitations of our data, we examine the possibility that null results mask heterogeneous effects by county type or equipment. Acquiring items through the 1033 program may raise spending (i.e., crowd in additional resources) if the items in question are not regular purchases and require complementary inputs. Of the various types of equipment considered, vehicles are the largest and most likely to fit this description. A tactical truck, for example, may require a new storage facility, specialized training, and unforeseen maintenance. To examine this possibility, we divide 1033 receipts into vehicle and non-vehicle items. The estimated coefficients on 1033 receipts are greater than zero, favoring crowding in, we do not find statistically significant evidence for either category of goods. Additionally, we might expect the effects of 1033 receipts on local police spending to vary by areas' fiscal or social conservatism. To evaluate this possibility, we divide the county panel according to political leanings in the 2008 presidential election. We find no evidence of heterogeneous effects by aggregate partisanship. We also split the sample into counties with above or below median population to examine whether crowding in is more or less prominent in smaller counties. We find statistically significant crowding in among smaller counties, but the precision of this result is not robust to minor specification changes.

The absence of crowding out is likely attributable to the design of the 1033 program rather than special features of law enforcement. Evans and Owens [33] and Baicker and Jacobson [5] examine intergovernmental financial transfers to law enforcement and find significant but partial crowding out, in accord with the body of research on intergovernmental grants. The 1033 program transfers capital goods, often large and rarely purchased by agencies on their own, with decision makers positioned at the end-user department rather than higher levels or inter-departmental levels of local authority. On the surface, the absence

of a significant crowd-out (or crowd-in) effect of 1033 transfers on local spending can be viewed as a positive outcome, given that the program is not designed to replace or amplify local spending. More importantly, the program-specific circumstances offer lessons for how intergovernmental transfers could be designed to minimize crowd out. In cases where higher levels of government wish to increase overall local spending or one reason or another, policy makers might look to the structure of the 1033 program for guidance.

II Relevant Background on the 1033 program

The 1033 program was created as a part of the National Defense Authorization Act of 1997. The stated purpose of section 1033 was to enable the Department of Defense to transfer military equipment no longer in use to local Law Enforcement Agencies (LEAs) to assist in drug interdiction. While the 1033 program has garnered considerable attention for transferring tactical equipment (e.g., assault rifles and armored personnel carriers) to local law enforcement, it has also facilitated transfers of clothing, ice chests, first aid kits, flashlights, etc. Over 70 percent of the items transferred were of a non-tactical nature. The 1033 program was not designed to be a grant-in-kind program, but rather an avenue for surplus defense items.

Several features of the 1033 program stand in sharp contrast to most intergovernmental transfers. First, the items transferred are relatively non-fungible goods of a very specific nature. The Defense Logistics Agency (DLA) forbids secondary transfers of items acquired through the 1033 program to other agencies.² Second, there is very little, if any, administrative cost to participating in this program. There is a simple two-page form to register as a receiving agency. There is then a one page form to request non-tactical items, a one page form to request an armored vehicle, a one-page form to request an aircraft, and so forth. The highest-ranking signature on these forms is that for the chief of police or a law enforcement officer of similar rank. Unlike Title 1 grants, for example, these transfers

²For example, a police department cannot request utility trucks and gift them to the county ambulance service.

are not overseen or administered by the state.³ Similarly, the acquisition process does not operate under the auspices of county or municipal governments who make funding decisions, nor is there any other form of public oversight. Rather, 1033 acquisitions may occur with or without the knowledge of the local budgetary authority and likely without the knowledge of voters. While federal monetary grants to local governments must be accounted for and therefore included in the budgetary process, that is not the case with the 1033 program.⁴

These features create conditions under which transfers can have an ambiguous effect on local budgets for law enforcement. If local voters and elected officials are aware of equipment transfers through the 1033 program, they may choose to reduce police budgets in subsequent years as is the case with receipts from civil asset forfeitures [5]. This would be especially likely for transfers that are a close substitute for items that could be acquired from private sector vendors. Alternatively, if voters and elected officials are (relatively) unaware of 1033 transfers, these transfers may have no effect on police budgets. However, there are at least two mechanisms by which transfers through the 1033 program may actually increase police budgets, or lead to crowding in. First, many state programs contain clauses that equipment must be used in some form, or returned to the DLA. While these clauses are essentially toothless (no burden of proof is required), they would give law enforcement officials leverage to request budget increases. Second, for certain sorts of equipment there may be a complementary inputs effect, where the items cannot be engaged to produce public safety without additional inputs. Therefore, the receipt of items through the 1033 program increases the marginal returns to discretionary resources allocated to police departments, thus justifying higher spending until net marginal benefits equalize across funding categories.⁵

³While each state does have a coordinator, the function of this role is to facilitate communication rather than control or coordinate the use of funds or gather data.

⁴The field on the ASG questionnaire explicitly asks respondents to exclude depreciation and other capital asset accounting from their reported expenditure figures. The survey is designed to capture operating costs rather than the depreciation of buildings or in this case, military surplus.

⁵For example, if a police department receives a large military vehicle, they may need a storage facility and a diesel mechanic. With these inputs in place, the vehicle may contribute substantially toward public safety; without the complementary inputs, the vehicle will simply sit idle. The same analogy can be drawn for guns requiring ammunition and training time, or non-tactical items requiring storage space.

II.A Program Logistics and the Role of Land Area and Proximity

Due to the unique structure of the 1033 program, receipts through this program are less vulnerable to the sorts of endogeneity concerns first addressed by Knight [57]. Nevertheless, one arm of our identification strategy relies on a fixed effects instrumental variable (FE-IV) specification previously discussed by Harris et al. [45]. An abbreviated outline of FE-IV methods follows. we refer the reader to Harris et al. [45] for further discussion.

Specifically we draw on two sources of exogenous variation. First, the amount of equipment available for distribution through the 1033 program varies exogenously over time. Disbursements of tactical items increased sharply in 2006 when the M-16 was replaced by the M-4 carbine as the standard issue weapon for the Army and Marine Corps. After 2009, the draw down from Iraq and Afghanistan steeply increased the availability of all types of equipment. More generally, the supply of equipment is determined by national military spending and need rather than any facet of local law enforcement. Second, law enforcement agencies face plausibly exogenous differences in the transportation costs associated with acquiring 1033 items. The interaction of these two factors (variation in availability and transaction costs) yields variables that affect the cost of acquiring goods through the 1033 program but are uncorrelated with bureaucratic and voter preferences for public safety or other unobservable factors that determine police budgets.

The DLA states that, particularly for vehicles, preference will be given to jurisdictions with larger land areas. There is a field for “land area” on the 1033 vehicle request form. Counties whose law enforcement officers have more ground to cover will likely gain greater benefits from the use of vehicles. This is an important source of variation, given that vehicles account for approximately half of the total value of goods distributed through the program. While DLA relayed that they were able to meet the needs of local enforcement agencies over time (meaning there was no long-term two-way selection problem), land area was a consideration in determining which claims were prioritized at a given point in time. The interaction of land area with the total value of equipment distributed through the 1033 program in a given year serves as our main instrument in the empirical specification. Additionally, local LEAs were responsible for their own costs in identifying, evaluating, and

acquiring equipment through the 1033 program. There are 18 Field Activity Centers (FAC) that are in charge of distributing 1033 items. All decommissioned items, particularly those of a sensitive nature, are sent to the FAC nearest the military unit from which the decommissioned item originated for processing. LEAs that acquire 1033 items must pay for all transaction costs, including evaluation and shipping costs from the FAC where the decommissioned items are processed. The location of these FAC's predates the 1033 program by at least 20 years, meaning that their placement is completely independent of whether nearby police departments are substantial users of surplus equipment. Additionally, being near a FAC does not equate to being near a military base. All else equal, LEAs that are farther from a FAC will face higher evaluation and acquisition costs than LEAs in close proximity to a FAC. Given that items are only made available for claim by local law enforcement in a 14-day window and that claimed items must be immediately taken into possession, the difference between traveling 100 miles or 200 miles to evaluate goods is non-trivial. We therefore also use inverse distance to the nearest FAC as an instrument in some specifications and sensitivity analyses.

III Data

The main source of local finance data is the Census of Governments and the Annual Survey of Government Finances (ASG) from 2006 to 2014, collected by the U.S. Census Bureau. While the Census Bureau canvassed the universe of government units in 2007 and 2012, data for other years are based on voluntary surveys of the same government units. These data provide detailed information on the revenues and expenditures of different levels of government. We primarily focus on police expenditures of county governments since this is the finest unit of government for which all necessary data are available. Police protection expenditures comprise spending on “police patrols and communications, crime prevention activities, detention and custody of persons awaiting trial, traffic safety, and vehicular inspection.”⁶

⁶<https://www.census.gov/govs/local/definitions.html>.

Although ASG data are widely used in the public finance literature, there are some known issues that must be acknowledged. First, any zero values in ASG data can either represent a true zero or a missing figure; differentiating between the two is not always possible. Fortunately, only 1.3 percent of the sample reported zero expenditure for police protection. Assuming that data from counties that consistently reported zeros for police protection are valid, those dubious zeros account for less than one percent of the observations. Second, the county-year panel in the ASG data is not balanced. If survey participation decisions of governments are not random conditional on control variables, our estimates would be biased. While we use the unbalanced panel as our primary analytical sample, we show in Appendices [B.A](#) and [B.C](#) that results are similar when using the balanced panel.

Data on transfers of military equipment to local law enforcement agencies come from the DLA. These data contain information on agencies to which transfers were made, item name and corresponding National Stock Number (NSN), shipping date, quantity, and the acquisition value of the item. The data that we use in the analysis were last updated in September 2015 and coded at the individual agency level. Thus, we infer county information from the agency name and the state to which the agency belongs by combining DLA data with the Law Enforcement Agency Identifiers Crosswalk file created by the Bureau of Justice Statistics and the National Archive of Criminal Justice Data (NACJD). We first combine the two datasets using character-merge and then manually match observations that have low matching scores or cannot be matched automatically. Observations from agencies whose county information cannot be identified are dropped from the sample.⁷

We also collect information on county characteristics from other sources. Data on crime rates are obtained from the county-level Uniform Crime Reports, published by the Federal Bureau of Investigation and reproduced by the NACJD. Using agency-level data provided by the FBI, the NACJD imputes missing data and aggregates the data by county.⁸ We use aggregate rates of arrests for murder, rape, robbery, aggravated assault, burglary, and other assaults. The Census provides intercensal population estimates, which includes demographic

⁷In particular, because Berkshire County Sheriffs Office (MA), Northern York County Police (PA), and York-Poquoson Sheriffs Office (VA) belong to multiple-jurisdictions, we are unable to uniquely identify county information for those agencies.

⁸For more details regarding the imputation, refer to the NACJD's Uniform Crime Reporting Program Resource Guide (<https://www.icpsr.umich.edu/icpsrweb/content/NACJD/guides/ucr.html>).

information for counties. Using these estimates, we calculate the male population share, the share of population aged 15 to 24, and an index of racial diversity similar to one used by [1].⁹ Data on household median income and unemployment rates are obtained from the Small Area Income and Poverty Estimates of the Census Bureau and Bureau of Labor Statistics, respectively. Finally, we collect data on the 2008 U.S. presidential election from the Guardian.¹⁰ The data contain the number of votes that each presidential candidate received by county.

The counties in our analytical sample constitute a substantial portion of county-level law enforcement in the United States. The Bureau of Justice Statistics reported that the U.S. as a whole spends \$100 billion on the policing aspect of law enforcement per year.¹¹ County Sheriff's Offices - the focus of our analysis - account for approximately \$30 billion of the \$100 billion spent nationally. In 2007, counties in our sample accounted for 73.5% of that \$30 billion [16]. Additionally, county government entities in our sample absorbed a significant portion of the items distributed through the 1033 program. In 2012, county governments in our sample received 38 percent of the total value of goods transferred through the 1033 program.

Table 2.1 lists summary statistics. The first two columns show the mean and standard deviation of each variable in the unbalanced sample and the last two columns show the same statistics for a balanced subset of our main panel. Note that lagged 1033 item values and police expenditures are expressed in per capita terms and crime rates are expressed per 100,000 population. All monetary values are adjusted for inflation using CPI-U-RS and given in thousands of logged 2012 U.S. dollars. The unbalanced panel contains about 30 percent more observations than the balanced panel. While statistics of most variables look quite similar across the panels, the average population size of the unbalanced panel is substantially smaller than the average population size of the balanced panel. Since small

⁹The racial diversity measure is defined as $diversity_{jt} = 1 - \sum_k race_{kjt}^2$, where $race_{kjt}$ represents the population share of a particular race k , where k consists of white, black or African American, American Indian and Alaska Native, Asian and Native Hawaiian/Other Pacific Islander, two or more races, in county j in year t .

¹⁰The data can be obtained from the following webpage (<https://www.theguardian.com/news/datablog/2009/mar/02/us-elections-2008>).

¹¹The 'policing aspect' reflects that this estimate does not include funds spent on corrections. Source: <http://www.justicepolicy.org/research/3906>

Table 2.1: Summary statistics

	Unbalanced		Balanced	
	Mean	Std. Errors	Mean	Std. Errors
Lagged 1033 item value per capita (\$)	0.24	(2.04)	0.19	(1.73)
Lagged 1033 value per capita — value ₀	1.46	(4.82)	1.144	(4.17)
ln(Lagged 1033 item value per capita) (\$)	0.08	(0.34)	0.06	(0.30)
ln(Lagged 1033 vehicle item value per capita) (\$)	0.06	(0.32)	0.05	(0.28)
Police expenditure per capita (\$)	96.75	(103.49)	91.34	(76.33)
ln(Police expenditure per capita) (\$)	4.29	(0.79)	4.24	(0.79)
Population (1,000)	130.42	(374.86)	189.13	(462.28)
Land Area (sq. mi.)	1084.43	(1510.55)	1220.38	(1701.70)
Median income	45.45	(11.91)	47.73	(12.90)
Male (%)	49.80	(1.84)	49.57	(1.48)
Age 15 to 24 (%)	13.38	(3.40)	13.77	(3.58)
Diversity index (% point)	20.80	(15.82)	22.81	(15.76)
Poverty (%)	15.32	(6.39)	15.89	(6.38)
Unemployment rate (%)	7.00	(2.94)	6.90	(2.95)
Lagged murder per 100,000 population	1.17	(4.31)	1.03	(3.07)
Lagged rape per 100,000 population	2.34	(5.92)	2.09	(4.96)
Lagged robbery per 100,000 population	3.46	(8.77)	3.80	(9.17)
Lagged aggravated per 100,000 population	34.98	(60.83)	31.50	(51.80)
Lagged burglary per 100,000 population	30.18	(46.24)	27.22	(42.07)
Lagged assault per 100,000 population	116.76	(162.73)	112.16	(162.47)
Observations	17,539		10,998	

Note: The unbalanced panel consists of all county governments that at least once participated in the Annual Survey of State and Local Government Finances from 2006 to 2014 (including census years 2007 and 2012) and the balanced panel consists of county governments that fully participated in the survey. Both samples consist of county-level law enforcement agencies that are matched to county governments. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars.

county governments have greater participation rates in the census years (2007 and 2012), the difference is not surprising. In the unbalanced panel, the average county spends about \$96.80 per capita for police protection and receives 1033 items that amount to 24 cents per capita. While that number appears small, it is an artifact of irregular participation in the 1033 program. In the balanced panel, 61.3 percent of counties participated in the 1033 program in at least one year, whereas only 16.7 percent of counties received any 1033 item in a given year. Conditional on receipt, the average value of receipts in the unbalanced panel increases to 1.47 dollars per capita, or 1.5 percent of total police spending.¹²

¹²Conditional on receipt, the average police spending is \$99.80 in the unbalanced panel.

Table 2.2: National county average vs. counties in our estimation sample (in 2012)

	National average			Estimation sample		
	Obs.	Mean	Std. Errors	Obs.	Mean	Std. Errors
ln(Lagged 1033 item value per capita) (\$)	3002	0.12	(0.43)	2656	0.13	(0.45)
ln(Lagged 1033 vehicle item value per capita) (\$)	3002	0.11	(0.42)	2656	0.12	(0.45)
ln(Police expenditure per capita) (\$)	2992	4.37	(0.74)	2656	4.37	(0.74)
Population (1,000)	3118	99.44	(319.95)	2656	103.18	(330.58)
Median income (\$1,000)	3118	44.75	(11.32)	2656	44.94	(11.31)
Male (%)	3118	50.04	(2.24)	2656	50.00	(2.06)
Age 15 to 24 (%)	3118	13.03	(3.46)	2656	13.00	(3.26)
Diversity index (% point)	3118	20.47	(16.16)	2656	20.27	(15.76)
Poverty (%)	3118	17.22	(6.57)	2656	17.12	(6.49)
Unemployment rate (%)	3118	7.85	(2.77)	2656	7.88	(2.74)
Lagged murder per 100,000 population	3007	1.12	(5.76)	2656	1.16	(3.98)
Lagged rape per 100,000 population	3007	2.26	(8.59)	2656	2.39	(5.85)
Lagged robbery per 100,000 population	3007	4.22	(70.66)	2656	3.13	(7.86)
Lagged aggravated per 100,000 population	3007	32.66	(116.51)	2656	34.29	(55.85)
Lagged burglary per 100,000 population	3007	30.51	(59.73)	2656	33.31	(49.23)
Lagged assault per 100,000 population	3007	109.03	(197.08)	2656	118.84	(159.99)

Note: This table compares the average characteristics of the whole U.S. counties to the average characteristics of counties in our unbalanced panel as of 2012. We compute the county-level national average using data from various sources described in Section III. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars.

Counties in our unbalanced analytical sample are broadly representative of the U.S. as a whole. Table 2.2 compares summary statistics of our estimation sample in 2012 to the national average for each county. In no area are there significant differences.

IV Empirical Model

We begin with the following equation to evaluate the reduced-form effect of 1033 item acquisition on police protection expenditures of county-level LEAs:

$$\ln(\text{protection}_{j,s,t}) = \beta_0 + \beta_1 \ln(\text{value}_{j,s,t-1}) + \mathbf{\Gamma} \mathbf{X}_{j,s,t} + \theta_j + \delta_s t + \varepsilon_{j,s,t} \quad (2.1)$$

where $\ln(\text{protection}_{j,s,t})$ represents logged police protection expenditure of county j in state s in year t and $\ln(\text{value}_{j,s,t-1})$ represents a monetary value of items that the law enforcement agency received through the 1033 program in year $t-1$. Note that both $\ln(\text{protection}_{j,s,t})$ and $\ln(\text{value}_{j,s,t-1})$ are normalized to the county population and expressed in per capita terms. $\mathbf{X}_{j,s,t}$ is a vector of time-varying county-level characteristics including median household income, male population share, the share of the population aged 15 to 24, racial diversity index, poverty rate, and unemployment rate. Parameters θ_j and $\delta_s t$ are county fixed effects and state-specific time trends, respectively. Results are robust to inclusion of year fixed effects in lieu of state-specific time trends for both the unbalanced and balanced panel samples (see Appendices B.B and B.C). Robust standard errors are clustered at the county level. As a benchmark for interpretation, if $\beta_1 = 0$, that implies there is no crowding in or crowding out, but that the 1033 grants are perfectly sticky. If $\beta_1 < 0$, that implies there is some crowding out, that 1033 receipts displace some future county expenditures on law enforcement. If $\beta_1 > 0$, that result implies there is crowding-in, or that 1033 receipts attract additional resources to law enforcement. The recent economics literature on intergovernmental grants nearly always finds evidence of crowding out. The features of different grant programs determine the strength of the crowding out effect.

The fixed effects model presented in Equation 2.1 relies on within-county variation in the value of acquired 1033 items over time. Under the assumption that the error term $\varepsilon_{j,s,t}$ is

not systematically correlated with 1033 item values after controlling for other variables, we can obtain an unbiased estimate for β_1 . However, this assumption might not hold for several reasons. For example, Knight [57] shows that endogeneity arises due to a correlation between grant receipts and unobserved tastes for public goods. In general, any correlation between time-varying unobserved factors that affect both public expenditures on public safety and the value of receipts through the 1033 program (e.g., motivation by law enforcement leadership to court additional resources) can lead to biased estimates.

To address concerns about endogeneity, we estimate the FE-IV model using a subset of instrumental variables described above and by Harris et al. [45]. Most critically, we use the interaction between the total value of all items dispensed through the 1033 program in a given year and the land area of a county (value-land interaction).¹³ The first stage of the FE-IV expression can be expressed as:

$$\ln(value_{j,s,t}) = \alpha_0 + \alpha_1 \ln(landarea_{j,s} * V_t) + \alpha X_{j,s,t} + \theta_{j1} + \delta_{s1}t + \varepsilon_{j1,s,t} \quad (2.2)$$

where V_t represents the total value of all items disbursed through the 1033 program in year t . Our main specification includes state-specific time trends rather than year fixed effects to control for time-varying heterogeneity by state. This allows us to take full advantage of exogenous time-series variation in the disbursements of capital through the 1033 program. Several conversations with the DLA revealed that time-series variation in items disbursed is not from an unobservable source, but from observable military operational and logistical decisions. For example, in 2006, the Army and Marine Corps switched from the M-16 assault rifle to the M-4 carbine as their standard infantry weapon, creating a large number of assault rifles transferred through the 1033 program that year. The increase in items made available through the 1033 program from 2009-2013 was due to the drawdown from Iraq and Afghanistan, and the continued decommissioning of equipment as units returned. By omitting year fixed effects, we assume that nationwide intertemporal shifts in police spending are not correlated with shocks in 1033 disbursements. However, not only is the 1033 program a very small part of DLA's overall operation, but Law Enforcement Agencies are third or

¹³Land area and all other time-invariant characteristics are implicitly captured by county-level fixed effects, as are time-invariant unobservable factors that are correlated with land area or proximity to FAC.

fourth in the hierarchy of claimants of decommissioned equipment. These factors minimize the threat to identification from year-specific unobservable factors. However, for readers concerned about year fixed effects, results from specifications with year fixed effects are available in Appendices [B.B](#) and [B.C](#). In these specifications, we use the inverse distance to the nearest field activity center as the cross-sectional component of our instrument.

We use log-log specifications for our regressions due to the high level of skewness in both the distribution of police spending, but also 1033 receipts. The 1033 receipts in particular are highly skewed not only between, but within counties over time. As the data on 1033 receipts are administrative data, we do not suspect that this skewness is due to reporting errors. Therefore, any strategy involving the trimming of outliers would be arbitrary and unacceptable. Because level-level specifications are the standard in the flypaper literature, we have included a set of results using a level-level specification in Appendix [B.D](#). While the FE models and the FE-IV models with year fixed effects provide further evidence for the stickiness of the 1033 grants, the IV results are too imprecise to be informative. Therefore, the log-log specification, the results of which should be interpreted as an elasticity rather than a dollar-for-dollar displacement, is our preferred specification.

V Results

We first report results from the FE models to establish a baseline descriptive relationship. We then present results from IV specifications. Almost all versions of equation [2.1](#) return a null result for 1033 item value, implying that these particular in-kind grants do not crowd out or crowd in local spending on law enforcement. Finally, we investigate whether null results mask heterogeneous treatment effects by county or equipment characteristics.

Table [2.3](#) contains results of the baseline FE models. Estimated coefficients are generally close to zero with economically small confidence intervals, implying a perfect flypaper effect. Column (1) lists results from the baseline Equation [2.1](#) specification, and column (2) includes additional control variables for crime rates.^{[14](#)} In both cases, the 95 percent confidence interval

¹⁴While adding crime variables to the baseline specification improves the precision of the estimates, it is unclear whether the best specification includes or excludes crime rates. While arguments for including crime rates as control variables are obvious, studies have also shown that police protection expenditures

implies that a one percent increase in the value of receipts from the 1033 program leads to a change in local police expenditures between -0.012 and 0.004 percent. Because many of the items received through the 1033 program are durable capital goods, they may have budgetary effects beyond the next year, particularly if crowding in occurs due to the need for complementary inputs.¹⁵ Column (3) evaluates this possibility by examining whether items received in period $t - 2$ or $t - 3$ affect contemporaneous spending on public safety. While only the third lag is statistically significant, all estimates are positive, reinforcing that we find no evidence of crowding out. Column (4) represents a specification that includes item values for year $t + 1$ as a falsification test of the timing assumptions in our model. The estimate for the lead term is insignificant, indicating that pre-existing trends are not of primary concern in our setting. Columns (5) and (6) demonstrate that results are not sensitive to alternative ways of controlling for time-varying heterogeneity, such as year fixed effects and the inclusion of both state-specific time trends and year fixed effects. While we find no evidence of crowding out, confidence intervals indicate that if 1033 receipts do crowd out local police spending, they do with an elasticity less than 0.02.¹⁶

While the institutional features of the 1033 program make sources of endogeneity discussed by Knight [57] less likely, they do not eliminate all concerns about correlated unobservables that affect local spending on police protection and acquisition of items through the 1033 program. For example, if chiefs of police who participate in the 1033 program aggressively court resources from all sources (including the local government), our results will suffer from omitted variable bias. We therefore implement a fixed effects instrumental variables approach as described in Harris et al. [45], the results of which are found in Table 2.4. Panel A of Table 2.4 contains the results of the first-stage estimation, and Panel B contains FE-IV estimates of the effect of 1033 receipts on local police spending. For all specifications in Table 2.4, we use land area as a time invariant characteristic, interacted

are endogenous to crime [68, 29]. Therefore, results in (1) and (2) should not be viewed as a horse-race, but simply specification checks that our results are not sensitive to the inclusion/exclusion of crime rates as controls.

¹⁵For example, if a department acquires assault rifles, it will need additional funding for ammunition to train/qualify officers beyond the next calendar year.

¹⁶As a robustness check, we have run a specification that includes both leads and lags of 1033 receipts. The results are not substantively different in any way.

Table 2.3: The effects of 1033 program on police spending: FE model

	(1)	(2)	(3)	(4)	(5)	(6)
$\ln(\text{Item value per capita}_{t-1})$	-0.004 (0.008)	-0.004 (0.008)	0.009 (0.008)		0.001 (0.009)	-0.002 (0.008)
$\ln(\text{Item value per capita}_{t-2})$			0.005 (0.013)			
$\ln(\text{Item value per capita}_{t-3})$			0.031** (0.014)			
$\ln(\text{Item value per capita}_{t+1})$				0.002 (0.010)		
County characteristics	✓	✓	✓	✓	✓	✓
Crime controls		✓	✓	✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓		✓
Year fixed effects					✓	✓
Observations	17539	17539	9069	13101	17539	17539
R^2	0.098	0.098	0.053	0.110	0.046	0.102

Note: This analysis uses the unbalanced panel described in Table 2.1. The dependent variable is logged police protection expenditure of county governments per capita. We control for county-level time-varying characteristics such as median household incomes, male shares, shares of the population aged from 15 to 24, racial diversity, poverty rates, and unemployment rates in all specifications. In column (2) to column (6), we add arrest rates for the following crime types: murder, rape, robbery, aggravated assault, burglary, and assault. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

with the nationwide value of goods released through the 1033 program as an instrument for the value of 1033 receipts.¹⁷

Specifications in columns (1) and (2) are the same as columns (1) and (2) in Table 2.3, but with instrumental variables predicting 1033 item value. First stage F-statistics on the exclusion restrictions are greater than 20, which far exceeds the benchmark value of 10 [97]. While coefficients are larger in magnitude and less precise than those from reduced form fixed effects models, they are positive (favoring crowding in) and nearly significant (p-value ≈ 0.11). Columns (3) and (4) address concerns about whether results are driven by counties that are highly likely or unlikely to participate at the extensive margin. To do so, we estimated a propensity score for a county's participation in the 1033 program.¹⁸ Column

¹⁷We again refer readers to Harris et al. [45] for additional supporting evidence on the validity of these instruments.

¹⁸To obtain propensity scores, we estimate a logit model where the dependent variable is a binary indicator that takes 1 if a county received any 1033 item in a given year and 0 otherwise. The model controls for

Table 2.4: The effects of 1033 program on police spending: FE-IV model

	(1)	(2)	(3)	(4)
Panel A: First-stage				
$\ln(Value_{t-1} \times Land_j)$	0.023*** (0.005)	0.023*** (0.005)	0.022*** (0.006)	0.021*** (0.007)
Panel B: Second-stage				
$\ln(Item\ value\ per\ capita_{t-1})$	0.346 (0.217)	0.346 (0.223)	0.085 (0.240)	0.088 (0.301)
Kleibergen-Paap LM statistic	25.584	24.613	14.921	9.329
Kleibergen-Paap Wald F statistic	25.774	24.773	14.986	9.360
Endogeneity Test	0.090	0.097	0.728	0.791
Sample	Full	Full	$0.1 \leq p$	$0.15 \leq p$
County characteristics	✓	✓	✓	✓
Crime controls		✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓
Observations	17532	17532	13627	10618

Note: Column (1) and column (2) use the full unbalanced sample. Column (3) and column (4) restrict the sample based on the likelihood of receiving any items through the 1033 program, p . Panel A summarizes the first-stage estimates where the dependent variable is the value of items that a county-level agency has received through 1033 program. $Value_{t-1}$ is the total value of 1033 items released by the Department of Defense in a year $t - 1$. $Land_j$ is the land area of county j . Panel B summarizes the second-stage results where the dependent variable is logged police protection expenditure of county governments per capita. The Kleibergen-Paap Wald F statistic shows the relevance of the instruments. The Kleibergen-Paap LM statistic is used to test whether a model is under-identified. We also test the null hypothesis that the item value can actually be treated as exogenous using a GMM distance test proposed by [8] and present the results. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(3) contains results from a sample restricted to counties with a greater than 10 percent estimated propensity to participate. Column (4) also excludes counties with a less than 15 percent estimated propensity to participate. While these sample restrictions decrease the magnitude of estimates, they further support the inference that receipts from the 1033 program do not crowd out county expenditures on law enforcement.

V.A Heterogeneous effects of the 1033 program

We show in the previous section that grants-in-kind through the 1033 program do not crowd out subsequent local police spending. However, our null results may mask significant heterogeneous effects either by equipment type or subsample. For example, it is possible that certain ordinary items may result in substantial crowding out, while vehicles may require additional funds for maintenance and operation resulting in crowding in. Alternatively, if acquiring resources through the 1033 program gives chiefs additional political capital in the budget process, we may expect crowding-in effects to be more or less pronounced in counties with greater fiscal conservatism, for which we use the county’s vote in the 2008 presidential election as a proxy. Finally, the size of the county may also lead to heterogeneity in the effects of 1033 receipts. On one hand, larger counties may find it easier to shift resources to provide complementary inputs. Alternatively, the moving of resources between budget areas may simply be more visible in smaller counties.

To investigate heterogeneous effects by equipment type, we split the value of receipts into two categories: vehicle and non-vehicle. While it is not always clear which items will require complementary inputs, vehicles are the most likely candidates. In the absence of additional funds for storage, fuel, maintenance, and possibly personnel who can operate and maintain military vehicles, these items will not be productive inputs for public safety.¹⁹ Table 2.5 summarizes estimation results from a specification identical to column (1) in Tables 2.3 and

county population as well as all covariates included in the column (2) of Table 2.3. The average propensity score in the unbalanced sample is 0.188.

¹⁹We define an item as a vehicle if its FSG is 15 (aircraft and airframe structural components), 16 (aircraft components and accessories), 17 (aircraft launching, landing, and ground handling equipment), 19 (ships, small craft, pontoons, and floating docks), 20 (ship and marine equipment), 23 (ground effect vehicles, motor vehicles, trailers, and cycles), or 24 (tractors). Non-vehicle item values are obtained by simply subtracting the vehicle item values from the total item values at the agency level.

Table 2.5: The heterogeneous effects of 1033 program on police spending: by item types

	(1)	(2)	(3)	(4)
	Vehicle		Non-vehicle	
	FE	FE-IV	FE	FE-IV
$\ln(\text{Vehicle item value per capita}_{t-1})$	0.001 (0.007)	0.590 (0.391)		
$\ln(\text{Non} - \text{vehicle item value per capita}_{t-1})$			-0.049 (0.033)	0.174 (0.256)
Kleibergen-Paap LM statistic		8.553		242.324
Kleibergen-Paap Wald F statistic		8.535		270.915
Endogeneity test		0.077		0.385
Observations	17539	17532	17539	17532

Note: We use the unbalanced panel and estimate the baseline equation (2.1) using both FE and FE-IV models. In column (1) and column (2), the dependent variable is the logged value of vehicle items that a county has received through 1033 program. In column (3) and column (4), the dependent variable is the logged value of non-vehicle items. The FE-IV models use the value-land interaction to instrument for the item value. See the note in Table 2.4 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

2.4, except that the key covariate in this analysis is either the lagged values of vehicle items or the lagged values of non-vehicle items. Results imply that neither lagged 1033 vehicle nor non-vehicles values lead to crowding out in local police spending. The FE-IV estimates for both vehicles and non-vehicles, shown in columns (2) and (4) are both positive for vehicles and non-vehicles, but much larger and nearly significant for vehicles. While results are somewhat consistent with a complimentary inputs story for vehicles, they are ultimately too weak to be conclusive.

In Table 2.6, we question whether the fiscal impact of the 1033 program on local police expenditures depends on the political preferences of counties. We divide the sample into two groups, Democrat and Republican, based on the 2008 U.S. presidential election votes. If the number of votes for the candidate from the Democratic party is above the median in our sample, the county is considered as Democrat. Otherwise, the county is considered as Republican. We repeat both FE and FE-IV specifications on each subsample. Results in Table 2.6 do not suggest heterogeneous effects by party affiliation. For both subsamples, baseline results are a relatively precise zero, and IV results are weakly positive but imprecise.

Table 2.6: The heterogeneous effects of 1033 program on police spending: by political preferences

	(1)	(2)	(3)	(4)
	Republican		Democrat	
	FE	FE-IV	FE	FE-IV
$\ln(\text{Item value per capita}_{t-1})$	-0.004 (0.009)	0.308 (0.242)	-0.010 (0.012)	0.522 (0.494)
Kleibergen-Paap LM statistic		18.585		6.637
Kleibergen-Paap Wald F statistic		18.709		6.625
Endogeneity test		0.177		0.240
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓
Observations	10741	10734	6798	6798

Note: We split the unbalanced panel into two groups based on political preferences of counties and estimate the baseline equation (2.1) using both FE and FE-IV models. The sample is restricted to counties whose Democrat vote shares for 2008 U.S. presidential election are less than or equal to the median of the balanced panel (“Republican”) in the first two columns and to counties whose Democrat vote shares are greater than the median (“Democrat”) in the last two columns. The dependent variable is logged police protection expenditure of county governments per capita. The FE-IV models use the value-land interaction to instrument for the item value. See the note in Table 2.4 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Finally, we investigate heterogeneous effects of 1033 receipts on local police spending by the size of the county. We split the sample into large counties and small counties based on the population size and estimate FE and FE-IV specifications. Results are summarized in Table 2.7. Similar to prior tables, FE estimates in column (1) and column (3) are a relatively precise zero, reinforcing the null result. FE-IV results actually imply that 1033 receipts lead to crowding in among small counties, but are insignificant for larger counties. Larger counties may find it easier to move money between line items because of the breadth and size of the overall budget. Small counties are more likely to face sharper tradeoffs in reallocating resources, and are therefore more likely to seek additional money from the county government. However, conclusions that 1033 acquisitions crowd in more spending in small counties are not robust to sample or specification changes in Appendices B.B and B.C.

Table 2.7: The heterogeneous effects of 1033 program on police spending: by population size

	(1)	(2)	(3)	(4)
	Small		Large	
	FE	FE-IV	FE	FE-IV
$\ln(\text{Item value per capita}_{t-1})$	-0.010 (0.009)	0.655** (0.262)	0.006 (0.010)	-0.351 (0.468)
Kleibergen-Paap LM statistic		19.807		7.703
Kleibergen-Paap Wald F statistic		19.945		7.700
Endogeneity test		0.002		0.432
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓
Observations	10289	10282	7250	7250

Note: We split the unbalanced panel into two groups based on the population size of counties and estimate the baseline equation (2.1) using both FE and FE-IV models. The sample is restricted to counties whose population sizes are less than or equal to the mean of the balanced panel (“Small”) in the first two columns and to counties whose population sizes are greater than the median (“Large”) in the last two columns. The dependent variable is logged police protection expenditure of county governments per capita. The FE-IV models use the value-land interaction to instrument for the item value. See the note in Table 2.4 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

VI Discussion

We investigate the effect of aid-in-kind from the 1033 program on local expenditures for police protection. In sharp contrast to virtually all recent empirical literature on intergovernmental transfers, we find no evidence that receipts of 1033 goods crowd out local spending on public safety. In other words, we find evidence of a perfect flypaper effect. This lack of crowd out is robust to using unbalanced or balanced panel samples, year fixed effects, or a level-level, rather than a log-log specification. These specification checks are available in Appendices B.A-B.D.

While these results are not sufficiently precise to completely rule out any crowding out effects, they indicate that capital receipts through the 1033 program lead to much less crowding out than other studies of earmarked grants for transportation, law enforcement, and education. Estimated effects of intergovernmental transfers from recent economics papers

Table 2.8: Related research on intergovernmental transfers

Author(s)	Program/transfer type	Estimates	Grant size (%)
Knight (2002)	Federal Highway Aid	[-0.88(0.42), -1.33(0.53)]	33.59 - 34.90% ^a
Baicker and Jacobson (2007)	Police seizures	[-0.81(0.36), -1.3(0.39)]	3.61% ^b
Brooks and Phillips (2008)	Community Development Block Grant	[-0.66(0.31), -0.77(0.39)]	1.63%
Lutz (2010)	New Hampshire's school finance reform	[-0.75(0.12), -0.98(0.19)]	29.90% ^c
Cascio, Gordon, and Reber (2013)	Introduction of Title I	-0.5(0.08)	2.86% ^d
Vegh and Vuletin (2015)	Fiscal transfers in Argentina	[-1.64(0.19), -1.7(0.22)]	60%
Bruce et al (2018)	1033 Grants-in-Kind (FE)	[-0.004 (0.008)]	1.5% ^e
Bruce et al (2018)	1033 Grants-in-Kind (FE-IV)	[0.346 (0.217)]	
Bruce et al (2018)	1033 Grants-in-Kind (FE level-level)	[0.10 (0.21)]	

Note: Ranges of estimates are given in the bracket. Standard errors of estimates are given in the parentheses.

^a Figures presented in Table 1 of Knight [57] are used.

^b Figures presented in Table 1 of Baicker and Jacobson [5] are used.

^c Figures presented in Table 2 of Lutz [74] are used.

^d Figures presented in Table 1 of Cascio et al. [21] are used.

^e The figure presents the share of the 1033 grants conditional on the receipt.

are summarized in Table 2.8. While the first two estimates from this paper are elasticities rather than level-level effects, FE estimates are centered near zero, which is outside the 95 percent confidence interval for virtually all previous estimates. The FE-IV estimates, while imprecise, are positive with a p-value just over 0.12. We also include the FE result from our level-level specification in Appendix B.D. This point estimate is also above zero, which implies crowding in, and the confidence interval implies that there is at most 30 percent crowding out, which is much less than most prior point estimates in this literature. While we therefore cannot completely reject crowding out, results imply that the 1033 grants are considerably stickier than previously studied grants.

The stickiness of the 1033 grants may be attributable to unique features of the program. Unlike other intergovernmental aid programs, 1033 grants are provided in the form of less fungible, non-transferable capital goods and the take-up decision is made by a chief of police rather than local voters or budgetary personnel. The opacity of the process means that there is little if any public oversight associated with gear acquisition. While the stickiness of the grant program may be deemed positive when the goal of the grant is to increase the total amount of resources allocated for a public good in a specific category, the lack of local oversight raises questions regarding welfare consequences of the 1033 program.

Due to data limitations, we leave for future work the question of which specific features of the program are necessary to achieve perfect stickiness. Understanding which features (lack

of transparency, weak fungibility, or application below the level of budget authority) leads to stickiness or crowd in would be of great value in designing future grants in contexts where crowd out is highly undesirable. Each such feature, however, leads to some inefficiency and reduces total welfare compared to lump-sum cash transfers. However, when either paternalistic motivations or the desire to capitalize on positive externalities motivates the grant, programs that share some structural or administrative features of the 1033 program may be more effective in preventing the crowd out of intergovernmental transfers.

Chapter 3

Expenditure Cascades: Evidence from the Bush Administration Tax Reforms

I Introduction

One remarkable feature of recent growing income inequality in the U.S. is a sharp increase in real income of the top 1 percent in contrast to stagnant median income (for example, see Piketty and Saez [84], Autor et al. [4]). In a series of work [36, 37, 38], Frank has suggested that the very-rich would engage in more consumption as their income rises, increasing the standard of consumption. This in turn makes households whose income are just below them consume more. Frank refers to this chain of process as expenditure cascades.

This paper provides quasi-experimental evidence of consumption externalities along the income distribution. A series of tax reforms during the Bush Administration (2001-2009) changed purchasing power of U.S. households in an asymmetric way by reducing marginal tax rates (MTR) for households whose MTR was equal to or greater than 28 percent by at least three percentage points, while leaving the MTR for those at the 15 percent tax bracket unchanged. As a result, the after-tax income distribution of U.S. households has become more unequal. I exploit this exogenous shock in the income distribution to examine whether the middle-to-low-income households raise their consumption in response to increased consumption among high-income households.

Using the Consumer Expenditure Survey (CE) data ranging from 1994 to 2007, I show that quarterly consumption of households with high tax liabilities went up by \$520 after the tax reforms. I also show that households with low tax liabilities living in states with a greater share of high-income taxpayers increased their consumption more substantially than similar households living in states with a lower share of high-income taxpayers, after the tax reforms. My preferred specification estimates that consumption externalities generated by the tax reforms raised consumption of middle-to-low-income households by \$410 to \$443.

Further analysis reveals that increased spending on housing and vehicles primarily led to the consumption growth for both upper-income and lower-income households. This entailed a reduction in food consumption at home for lower-income households. On the contrary, food-at-home consumption of high-income households was constant over the period. This finding is consistent with the argument of Frank [36] that the growth in consumption of visible goods triggered by consumption of the top-income would hurt low-income households by reducing resources that can be spent on necessities such as food, unless their incomes rise accordingly.

This paper makes two distinct contributions to existing studies on household consumption. First, to my knowledge, this is the first paper that documents evidence of consumption externalities trickling down from upper-income households to lower-income households using a natural experiment. There exists a large body of literature that documents empirical evidence of consumption externalities [43, 2, 15, 63, 87]. However, the origin of such externalities is not necessarily the upper-income households in those studies, thus having very different policy implications. Drechsel-Grau and Schmid [30], Bertrand and Morse [10] examine whether consumption of richer households influences consumption of others who make less income. However, both studies rely on observational variation, which might not be ideal to test the causal relationship between reference consumption and own consumption. I exploit exogenous variation from the Bush Administration tax reforms, which is less vulnerable to the potential endogeneity including reverse causality. Second, the economic impact of tax reforms is interesting in itself. In particular, the distributional effects of the Bush tax reforms have garnered attention from both media outlets and academia [32, 51, 50].

The evidence provided in this paper suggests that policy makers should account for the welfare losses from the expenditure cascades when designing tax reform.

The paper proceeds as follows. Section II presents a simple framework that provides a testable hypothesis on household consumption under interdependent preference. Section III provides background on the Bush tax reforms and Section IV explains how I identify the evidence of consumption externalities from upper-income households to lower-income households in the aftermath of the tax reforms. Section V describes the data that I use in the study and Section VI presents the estimation strategy and results. Section VII concludes.

II Framework

I begin by presenting a simple discrete-time finite-horizon model based on Ljungqvist and Uhlig [70] to describe how changes in consumption of other households influence one's own consumption in the presence of interdependent preferences. Households live during periods $t \in \{0, 1, \dots, T\}$ for some finite $T \geq 1$. The utility function of households is given as

$$u(c_t, \bar{c}_t, l_t) = \frac{(c_t - \alpha \bar{c}_t)^{1-\gamma} - 1}{1-\gamma} - Al_t \quad (3.1)$$

where c_t denotes own household consumption in period t , l denotes own labor supply, and $\bar{c}$ denotes consumption of a reference group. The externality parameter $\alpha \in [0, 1)$ represents the sensitivity of own consumption to reference consumption and γ represents the inverse of the elasticity of intertemporal substitution. A is the relative importance of leisure. For simplicity, assume that $\bar{c}$ is exogenous. In other words, reference consumption is independent of consumption and labor supply decisions of household t .

I can write the household utility maximization problem as

$$\max_{\{c_t\}_{t=0}^T, \{l_t\}_{t=0}^T} \sum_{t=0}^T \beta^t \left\{ \frac{(c_t - \alpha \bar{c}_t)^{1-\gamma} - 1}{1-\gamma} - Al_t \right\} \quad (3.2)$$

$$\text{subject to } c_t + \frac{a_{t+1}}{1+r} = a_t + w_t l_t, \forall t$$

$$a_0 > 0$$

$$a_{T+1} \geq 0$$

where β is the discount factor, a_t is the level of assets in period t , r is the interest rate, and w is a wage. It can be easily shown that the optimal consumption of a household is

$$c_t^* = \left(\frac{w_t}{A}\right)^{\frac{1}{\gamma}} + \alpha \bar{c}_t. \quad (3.3)$$

The goal of this paper is to examine whether an exogenous increase in consumption of upper-income households (i.e., reference consumption) affects consumption of lower-income households (i.e., own consumption). A simple comparative static yields a testable hypothesis regarding the effect of reference consumption on own household consumption:

$$\frac{\partial c^*}{\partial \bar{c}} = \alpha \geq 0. \quad (3.4)$$

In a standard model where there is no consideration of others' consumption (i.e., $\alpha = 0$), own consumption is completely insensitive to reference consumption. On the other hand, if there exist interdependent preferences, an exogenous increase in reference consumption would also increase own household consumption by the externality parameter α .

III Bush Tax Reforms

A series of tax reforms, the Economic Growth and Tax Relief Reconciliation Act of 2001 (EGTRRA) and the Jobs and Growth Tax Relief Reconciliation Act of 2003 (JGTRRA), was implemented during the Bush Administration (2001-2009). EGTRRA and JGTRRA were designed to provide economic incentives to work and invest by reducing the distortion created by a high tax burden [3]. EGTRRA and JGTRRA contained many tax provisions, including raising the standard deduction for joint returns, raising the child tax credit from \$500 to \$1000, gradually repealing estate taxes, lowering taxes on dividends and capital

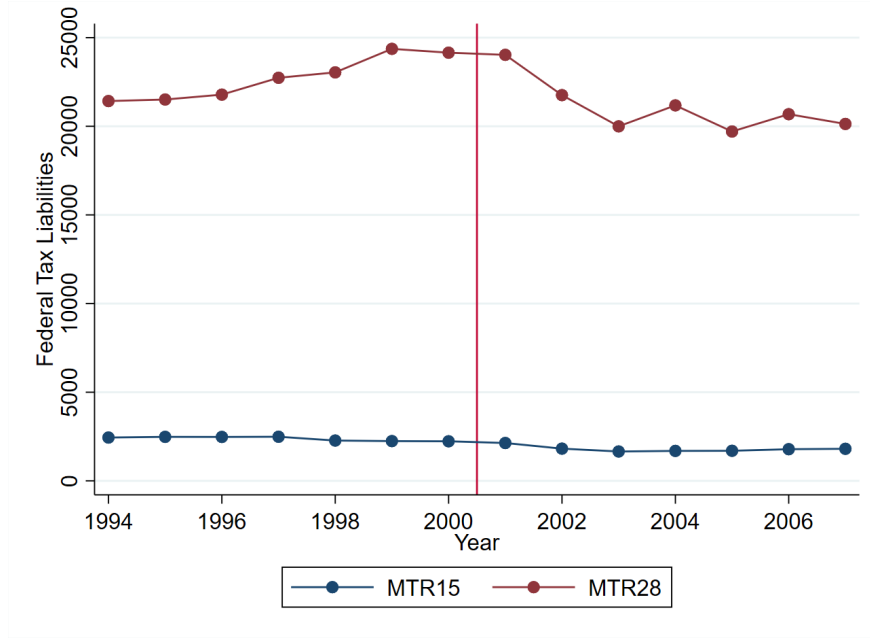


Figure 3.1: Average Federal Income Tax Liabilities

Note: MTR15 represents households whose marginal tax rates are 15 percent or below and MTR28 represents households whose marginal tax rates are 28 percent or above during the pre-reform periods. The average tax liabilities are calculated using the CPS weight. All figures are adjusted for inflation using CPI-U-RS and presented in 2013 U.S. dollars.

Source: Current Population Survey March microdata, 1995-2008.

gains, and so on.¹ Among the provisions, the most noticeable feature of the reforms is a reduction of marginal income tax rates (MTR).

Table 3.1 shows the MTRs and tax brackets for joint returns from 2000 to 2003. The MTRs were stable before and after the tax reforms. As shown in Table 1, the MTRs were reduced by at least three percentage points for households who faced the top four income tax brackets, while the reforms did not change the MTR for households whose taxable income belonged to the lowest tax bracket in the pre-reform period.² Throughout the paper, I define households in the former group as *MTR28* as their pre-reform MTRs were 28 percent or above and households in the latter group as *MTR15* for a similar reason.

Figure 3.1 summarizes the evolution of federal tax liabilities of both MTR15 and MTR28 households. It is clear that the average federal tax liabilities of MTR28 households reduced

¹Table A2 in Auten et al. [3] provides a summary of EGTRRA and JGTRRA provisions.

²The tax reforms also introduced a new 10 percent bracket, which affected all households whose taxable income was positive

Table 3.1: Marginal federal income tax rates for joint returns

MTR(%)	2000		MTR(%)	2001		MTR(%)	2002		MTR(%)	2003	
	Lower	Upper		Lower	Upper		Lower	Upper		Lower	Upper
			10.0	0	15,788	10.0	0	15,543	10.0	0	17,731
15.0	0	59,334	15.0	15,788	59,467	15.0	15,543	60,487	15.0	17,731	71,935
28.0	59,334	143,361	27.5	59,467	143,735	27.0	60,487	146,167	25.0	71,935	145,201
31.0	143,361	218,459	30.5	143,735	219,055	30.0	146,167	222,715	28.0	145,201	221,252
36.0	218,459	390,168	35.5	219,055	391,208	35.0	222,715	397,701	33.0	221,252	395,075
39.6	390,168		39.1	391,208	38.6	397,701		35.0	395,075		

Note: All figures are adjusted for inflation using CPI-U-RS and presented in 2013 U.S. dollars.

Source: www.taxfoundation.org

considerably after the reform. The average federal tax liabilities of *MTR15* also decreased for the same period, but this is due to the introduction of a new 10 percent bracket. It is worth noting that tax liabilities of *MTR28* are underestimated because the variable is top-coded at \$99,999 in the CPS data. Considering the fact that the reforms also included other tax provisions, the actual distributional effects of the tax reforms should be larger than shown in Figure 3.1. In fact, Elmendorf et al. [32] conclude that only households in the top quintile received substantial benefits from the tax reforms and the majority of households were made worse off. Sherman and Aron-Dine [94] estimate that the average after-tax income of the top one percent increased by \$146,000 (or 20 percent) from 2003 to 2004, compared to a \$1,700 (or 3.6 percent) increase for the the median households.

Assuming that the *MTR28* increased their consumption after the tax reforms, the expenditure cascades hypothesis implies that the *MTR15* would increase their consumption, not because their income went up, but because the increase in consumption of the *MTR28* shifted the consumption standard of society upward. Therefore, the overconsumption of the *MTR15* is an unintended consequence of the tax reforms, which might cause significant welfare loss to society (for example, see Frank [36]).

IV Identification

The main objective of this paper is to empirically examine whether the increased consumption of upper-income households trickled down to lower-income households. Non-experimental variation in household purchasing power can be correlated with unobserved

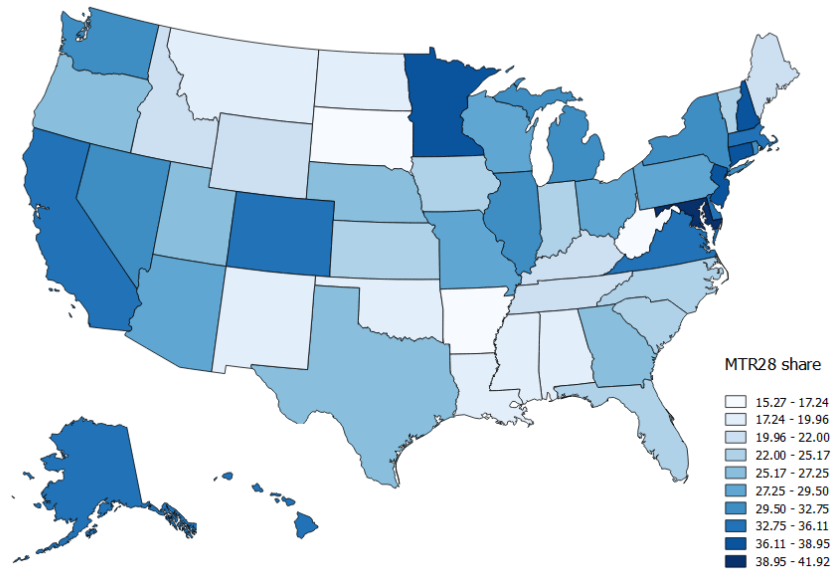
household characteristics such as preferences [54], which also influence household consumption behavior. The Bush tax reforms, which exogenously widened the after-tax income distribution of U.S. households, provide an unusual setting that one can exploit to test the expenditure cascade hypothesis.

Due to the introduction of a new 10 percent bracket and other changes in tax provisions, all households are effectively treated and this challenges the construction of an appropriate counterfactual. I overcome this challenge by exploiting state heterogeneity in the share of households who pay higher marginal tax rates. The idea is based on the notion of *demonstration effects* [31], saying that frequent exposure to a reference population that consumes goods of higher quality than their own urges them to imitate the reference consumption.

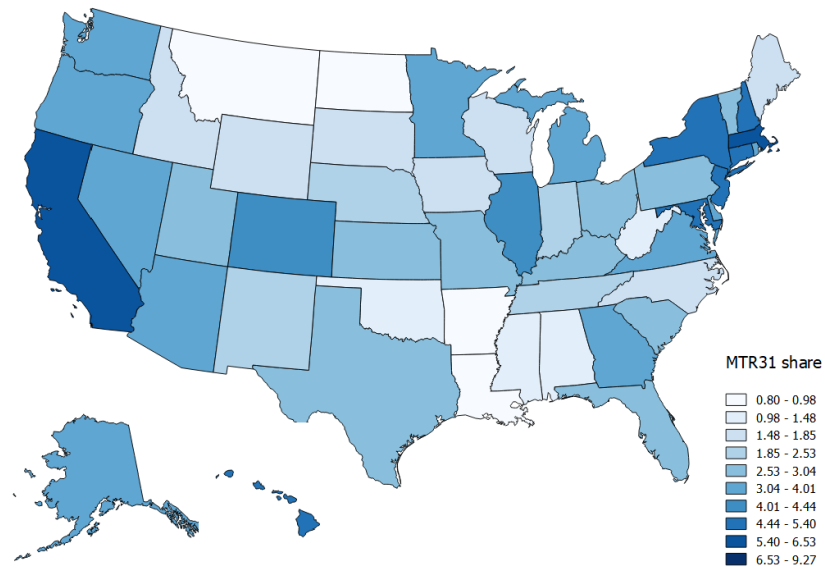
I measure the externality intensity using 1) the state share of MTR28 households in year 2000 and 2) the state share of households whose MTRs were equal to or greater than 31 percent (MTR31). The first measure is straightforward. It measures the chance of seeing a MTR28 household in a given state. This measure is intuitive, but considering that the tax reforms benefited households with the highest tax liabilities the most, the MTR28 share may not capture the externality intensity very well.³ On the other hand, the second measure would capture the share of higher income households better. If consumption of the highest-income households indeed trickled down to households below them in the income distribution, the MTR31 share would measure the externality intensity more accurately.

Figure 3.2 shows that there exists substantial variation in the state share of upper-income households. In general, the MTR28 share and the MTR31 share are strongly correlated, but they are not perfectly collinear. For example, Minnesota ranks the second among the states in terms of the MTR28 share, but its ranking in terms of the MTR31 share falls to 13th. By contrast, consumption externalities are assumed to be stronger in California when it is measured by the MTR31 share rather than the MTR28 share. I will provide further details regarding the construction of intensity measures in the next section.

³For example, the lower and upper tax bracket for married households who filed jointly were \$43,850 and \$105,950 in 2000. This means that the households whose pre-reform MTR was 28 would have gained around \$1,863 at most in nominal terms, relative to households below them in the distribution of adjusted gross income.



(a) MTR28 share



(b) MTR31 share

Figure 3.2: Externality intensity measures

Note: MTR15 represents households whose marginal tax rates are 15 percent or below. MTR28 (MTR31) represents households whose marginal tax rates are 28 percent (31 percent) or above during the pre-reform periods. The average tax liabilities are calculated using the CPS weight. All figures are adjusted for inflation using CPI-U-RS and presented in 2013 U.S. dollars.

Source: Current Population Survey March microdata, 1995-2008.

The identifying assumption is that, on average, MTR15 consumption in state i and state j for all $i \neq j$ would have grown similarly in the absence of EGTRRA and JGTRRA. This assumption would not hold if the average household in each state experiences different levels of income growth. To control for the income effect, I include the 6th order polynomial of after-tax household income in the model. It is also possible that states with a larger share of households with high tax liabilities underwent greater local inflation in the post-reform period. In this case, household spending increased even when they consumed more or less the same bundle of goods and services before and after the tax reforms. I control for heterogeneity in the growth of local prices by adding the state-level single-family housing price index (HPI) to the model.

V Data

The main source of information on household consumption is the Consumer Expenditure (CE) Survey microdata, which is by far the most comprehensive data regarding household consumption in the United States. The CE consists of the Interview survey and the Diary survey and I only use the former for the purpose of this study. Among several quarterly files from the Interview survey, I use FMLI files ranging from 1994 to 2007. In the Interview survey, each consumer unit (CU) provides information on demographics and household characteristics in the initial interview and information on expenditures in the second through the fifth interviews.

Although I also analyze household consumption for specific expenditure categories, the primary consumption measure is total household consumption. The total quarterly expenditure variable in the CE comprises household spending on food, alcohol and beverage, housing, apparel, transportation, health, entertainment, personal care, reading, education, tobacco, miscellanea, cash contribution, and personal insurance. Following existing literature using the CE data [75, 10], I convert expenditures on vehicle and housing purchases into service flows by merging a dataset created by Meyer and Sullivan [75] to the original CE data. Using the CE information on makes and models of vehicles, Meyer and Sullivan estimate values of the vehicles and annual depreciation rates to calculate vehicle service

flows. The authors also impute the rental equivalent for households in public or subsidized housing.

The CE also collects information on household income in the second and the fifth interviews. I use both before-tax income and after-tax income to control for any income effects. Before-tax income includes wage and salary, gain/loss from business, Social Security and Railroad Retirement income, benefits, workers' compensation and veterans' payments, unemployment compensation, public assistance/welfare, interest, child support, alimony, food stamps, income from dividends, royalties, estates, trusts, and other income. After-tax income is obtained by subtracting federal and state/local income taxes, property taxes, and other taxes from before-tax income.

To identify MTR28 and MTR15, I need information on taxable income and tax filing status of households. Unfortunately, tax filing status data is not available for the period that I study.⁴ Therefore, I impute household tax filing status and MTRs based on family characteristics and the total amount of federal tax paid. In particular, I define the total federal tax paid as a sum of annualized federal income tax reduction from earnings and additional federal income tax paid, less than additional federal income tax refund. For example, if the imputed tax filing status of a household is "married filing jointly" and the total amount of federal income tax paid is larger than \$9804.14, which is the federal tax liability at the maximum upper bound of the 15 percent bracket during the estimation window, the household is a MTR28.

A nontrivial portion of observations are dropped for several reasons. First of all, the CE suppresses state identifiers for observations that might be identifiable given state information. I drop those households.⁵ I also drop households that report total quarterly consumption below \$1,000, non-positive before-tax income, or non-positive after-tax income.⁶ In addition, I exclude households that could be a MTR28 in some years and MTR15 in another years due to changes in bracket endpoints or the "bracket creep" [90].⁷ Lastly, I drop households

⁴Tax filing status data in the CE becomes available in 2013.

⁵Observations from Mississippi, Montana, New Mexico, North Dakota, and Wyoming do not have state identifiers.

⁶[10] also drop households with zero income or with zero food consumption.

⁷For example, the MTR of households whose taxable income were between \$60,470 to \$71,941 in 2002 was reduced from 27 percent to 15 percent in 2003 (see Table 3.1).

Table 3.2: Summary statistics

	MTR28		MTR15	
	Mean	SD	Mean	SD
Quarterly consumption (\$)	18,906.58	(8,013.86)	11,655.57	(6,622.86)
Income before tax (\$)	120,706.51	(67,327.70)	55,650.99	(41,712.67)
Income after tax (\$)	99,618.91	(58,897.54)	54,773.71	(41,243.95)
Family size	2.52	(1.41)	2.66	(1.54)
Number of earners	1.75	(0.83)	1.33	(1.00)
Age	44.97	(11.72)	49.92	(17.63)
Male	0.64	(0.48)	0.51	(0.50)
White	0.88	(0.33)	0.82	(0.39)
Single	0.28	(0.45)	0.25	(0.43)
BA degree	0.45	(0.50)	0.22	(0.41)
Home ownership	0.79	(0.41)	0.66	(0.47)
Married filing jointly	0.59	(0.49)	0.53	(0.50)
Head of households	0.03	(0.18)	0.07	(0.26)
Observations	30,038		190,929	

Note: All monetary figures are adjusted for inflation using CPI-U-RS and presented in 2013 U.S. dollars.

Source: Consumer Expenditure Survey data, 1994-2007, and the author's calculation.

whose before-tax income is too high to be a MTR15 or too low to be a MTR28. Appendix C.B provides more details on the CE data management.

Table 3.2 provides summary statistics for the CE data. Not surprisingly, the average quarterly consumption of MTR28 households is higher than the average quarterly consumption of MTR15 households by \$7,251. Similarly, the annual pre-tax income of MTR28 households is larger than twice the the annual pre-tax income of MTR15 households. While family size is not very different across two samples, MTR28 households have a greater number of earners, which partially explains the income gap between two groups. The head of MTR28 households is almost five years younger, more likely to be male and white, and more likely to have a bachelor's degree. MTR28 households are also more likely to be married couples and homeowners, compared to the MTR 15 households.

VI Empirical Strategy and Results

This section can be divided into two parts. In the first part, I show that MTR28 households increased consumption as a result of the tax reforms using difference-in-difference and event-study specifications. Assuming that both consumption and savings are normal goods and the credit constraint of MTR28 households is binding, an exogenous increase in disposable income should raise consumption of MTR28 households. On the other hand, it is also possible that MTR28 households did not spend a penny out of extra money when they were already optimizing their consumption with nonbinding budget constraints. If the tax reforms did not spur consumption of MTR28 households significantly, the demonstration effect from MTR28 households to MTR15 households would be negligible, and thus consumption externalities would not exist in the first place. In the second part, I use another difference-in-difference model to estimate the effect of consumption externalities generated by the tax reforms and to examine whether the effect varies across income quartiles as the expenditure cascades hypothesis suggests.

VI.A Did MTR28 households increase their spending?

I first investigate whether the tax reforms led to higher consumption among MTR28 households by estimating the following difference-in-difference model:

$$C_{i,s,t} = \alpha + \delta MTR28_i + \beta Post_t \times MTR28_i + \gamma X_{i,t} + \varepsilon_{i,s,t} \quad (3.5)$$

where $C_{i,s,t}$ is a measure of consumption of household i who lives in state s in year-quarter t . For now, I only consider total quarterly consumption of households. $Post$ is a dummy variable that takes 1 if consumption took place after 2001, and 0 otherwise. $MTR28$ takes 1 if household i is MTR28, and 0 if the household is MTR15. X is a vector of household characteristics including family size, number of earners, home ownership status, age of a reference person, and dummy variables that indicate whether the reference person is male, white, single, or whether he has a bachelor's degree. X also includes dummies for tax filing status. All specifications include state fixed effects and year-quarter fixed effects. I weight

each observation using the weight variable that the CE provides and cluster standard errors by state to account for heteroskedasticity and any form of within-state correlation.

β is the coefficient of interest that captures the effect of the tax reforms on consumption of MTR28 households. The identifying assumption in this analysis is that the trends in consumption of MTR28 and MTR15 households should have been similar in the absence of the tax reforms. Since I hypothesize that there would be consumption externalities from MTR28 households to MTR15 households, the difference-in-difference model also assumes that the magnitude of the externalities remains unchanged in the post-treatment period. If the tax reforms somehow amplified the size of consumption externalities as I argue, the estimate would be biased downward. On the other hand, the estimate would be biased upward if the tax reforms reduced the effect of consumption externalities.

Table 3.3 summarizes the results. Column (1) presents the baseline result from estimating equation (3.5) using a full sample. The estimate implies that, on average, the tax reforms increased quarterly consumption of MTR28 households by \$266.3 and the estimate is statistically significant at the 5 percent level. In my sample, the average MTR28 household spent \$17,757 in the pre-treatment period and \$19,850 in the post-treatment period. This indicates that the tax reforms alone account for about 12.7 percent of the overall increase in consumption of MTR28 households. In column (2), I use the 6th order polynomial of before-tax income to allow for nonlinearity of consumption in household income. Surprisingly, the estimate becomes statistically significant at the 1 percent level and its magnitude is more than twice the previous estimate. This result implies that controlling for the nonlinear income effect can be very important. In column (3), I add lagged unemployment rates and the housing price index to account for macroeconomic conditions at the state-level. Incorporating macroeconomic controls does not change the result substantially. Column (4) further controls for any time-varying unobservables at the state level by including state-specific linear time trends. The estimate is slightly reduced to \$519.2, but it is still comparable to estimates presented in the previous columns.

Results presented in Table 3.3 are based on the assumption that MTR15 consumption would have trended similarly in the absence of the tax reforms. To show the validity of the identifying assumption as well as the treatment effect dynamics, I also estimate the following

Table 3.3: The effect of Bush tax reforms on consumption of MTR28 households

	(1)	(2)	(3)	(4)
MTR28	1188.6*** (76.39)	692.4*** (71.91)	704.6*** (74.08)	721.8*** (75.04)
Post $\times$ MTR28	266.3** (125.7)	553.6*** (116.1)	542.8*** (125.2)	519.2*** (131.6)
Before-tax income	0.0754*** (0.00132)	0.0846*** (0.00557)	0.0845*** (0.00558)	0.0838*** (0.00567)
Family size	242.5*** (60.29)	285.8*** (54.06)	286.5*** (53.84)	288.3*** (53.95)
Number of earners	509.3*** (33.45)	220.1*** (26.82)	219.1*** (26.57)	215.4*** (26.52)
Home ownership	2487.4*** (178.6)	2163.8*** (163.9)	2166.0*** (164.2)	2171.0*** (164.3)
Age	6.735*** (1.374)	10.88*** (1.296)	10.91*** (1.302)	10.82*** (1.314)
Male	-25.71 (38.53)	-79.30** (36.68)	-80.77** (37.13)	-79.82** (36.54)
White	757.2*** (91.97)	709.8*** (83.39)	712.3*** (83.58)	716.6*** (83.98)
Single	-825.8*** (83.07)	-742.3*** (77.96)	-742.7*** (77.71)	-740.7*** (75.37)
BA degree	1870.7*** (74.93)	1587.2*** (58.71)	1585.5*** (57.26)	1586.7*** (56.08)
Family	1170.6*** (57.41)	873.1*** (54.31)	874.8*** (53.77)	879.1*** (52.89)
Head	303.1*** (66.95)	390.0*** (63.55)	391.1*** (62.68)	389.6*** (62.57)
(Before-tax income) ²		0.000000312*** (6.69e-08)	0.000000313*** (6.72e-08)	0.000000319*** (6.78e-08)
(Before-tax income) ³		-2.30e-12*** (4.31e-13)	-2.29e-12*** (4.33e-13)	-2.32e-12*** (4.36e-13)
(Before-tax income) ⁴		4.51e-18*** (1.26e-18)	4.50e-18*** (1.26e-18)	4.56e-18*** (1.27e-18)
(Before-tax income) ⁵		-3.50e-24** (1.59e-24)	-3.48e-24** (1.59e-24)	-3.55e-24** (1.60e-24)
(Before-tax income) ⁶		8.98e-31 (7.00e-31)	8.89e-31 (7.02e-31)	9.16e-31 (7.06e-31)
HPI			6.356*** (1.831)	6.380*** (2.066)
Unemployment rate			-38.64 (46.90)	-6.087 (33.59)
Macro controls	No	No	Yes	Yes
Income polynomial	1st	6th	6th	6th
State specific time trends	No	No	No	Yes
Observations	220967	220967	220967	220967
R^2	0.627	0.646	0.646	0.646

Note: Standard errors are clustered at the state level and given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

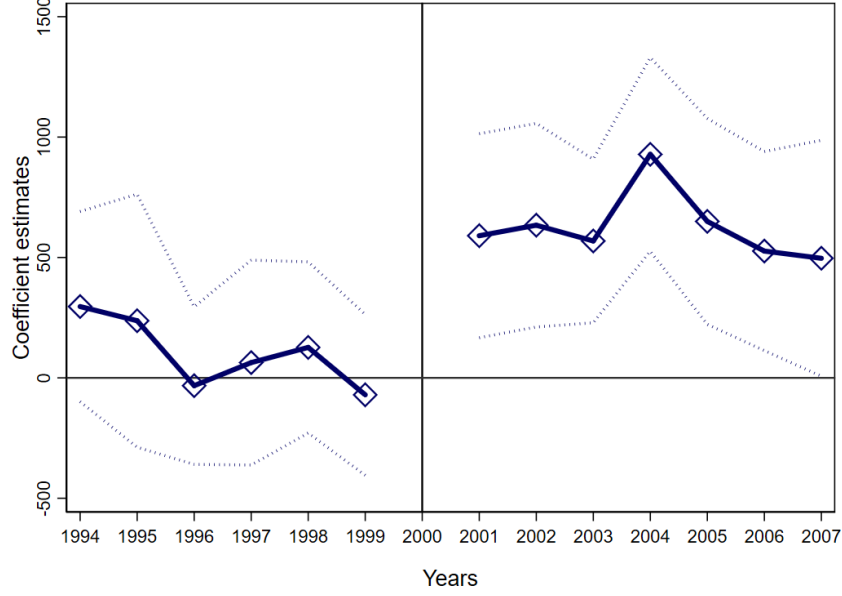


Figure 3.3: The effect of the Bush tax reforms: total consumption

Note: Each diamond in the figure represent the estimate for β_k in equation (3.6) for $k \in 1994, \dots, 2007$ and dotted lines connect confidence intervals associated with the estimates.

event study specification:

$$C_{i,s,t} = \alpha + \delta MTR28_i + \sum_{k=1994}^{2007} \beta_k \{\mathbb{1}(k = y) \times MTR28_i\} + \gamma X_{i,t} + \delta Z_{s,t} + \varepsilon_{i,s,t} \quad (3.6)$$

where $\mathbb{1}(\bullet)$ is an indicator that takes 1 if k equals year $y \in \{1994, \dots, 2007\}$, and 0 otherwise. All else is the same as given in equation (3.5).

Figure 3.3 summarizes how the treatment effect of the tax reforms evolved over time. Each diamond in the figure represents the estimate for β_k in equation (3.6) for $k \in 1994, \dots, 2007$ and dotted lines connect confidence intervals associated with the estimates. Prior to EGTRRA and JGTRRA, the estimates are not statistically different from zero. The evidence suggests that the parallel assumption is likely to hold in this context. On the contrary, the treatment effects are positive and statistically significant in years following EGTRRA, which is consistent with the previous results. In sum, the results generally evidence that the tax reforms raised consumption of MTR28 households by roughly 25 percent, by reducing tax

burden of those households substantially. It is worth noting that the estimates presented in Table 3.3 are likely to be biased downward because the CE data tend to underrepresent consumption of the high-income households and the problem is getting worse in recent years (for example, see Sabelhaus et al. [89]).

I further investigate the effect of the tax reforms on MTR28 household consumption for major consumption categories to provide a better picture of consumption responses of MTR28 households. While Figure 3.3 suggests that the trends in total quarterly consumption of MTR28 and MTR15 households were quite similar prior to the tax reforms, it is uncertain whether the parallel assumption still holds for each consumption category. To validate the use of MTR15 households as a counterfactual and to demonstrate the effect of tax reforms on MTR28 household consumption for a specific item, I again estimate the event study specification similar to equation (3.6), but the dependent variable is household consumption for each category.

Figure 3.4 clearly demonstrates that the increase in total consumption of MTR28 households after the implementation of EGTRRA was mainly driven by consumption on housing and vehicles, which are two types of consumption that economists view as visible [95, 47]. The results support the claim that the rich would spend more on positional goods such as houses and cars out of the money saved from the tax reforms [36]. Surprisingly, the tax reforms also increased MTR28 spending on utilities, which are less visible than housing and vehicles. Event study estimates for other items are quite noisy.

VI.B Did the Bush tax reforms generate consumption externalities from MTR28 to MTR15?

I show that the tax reforms increased consumption of MTR28 households, mainly in the domain of housing and vehicles. This section explores whether the increase in MTR28 consumption produced externalities to MTR15 households, thus spurring their consumption after the tax reforms. In other words, I estimate the average treatment effect on the untreated, similar to Angelucci and De Giorgi [2] and Roth [87].

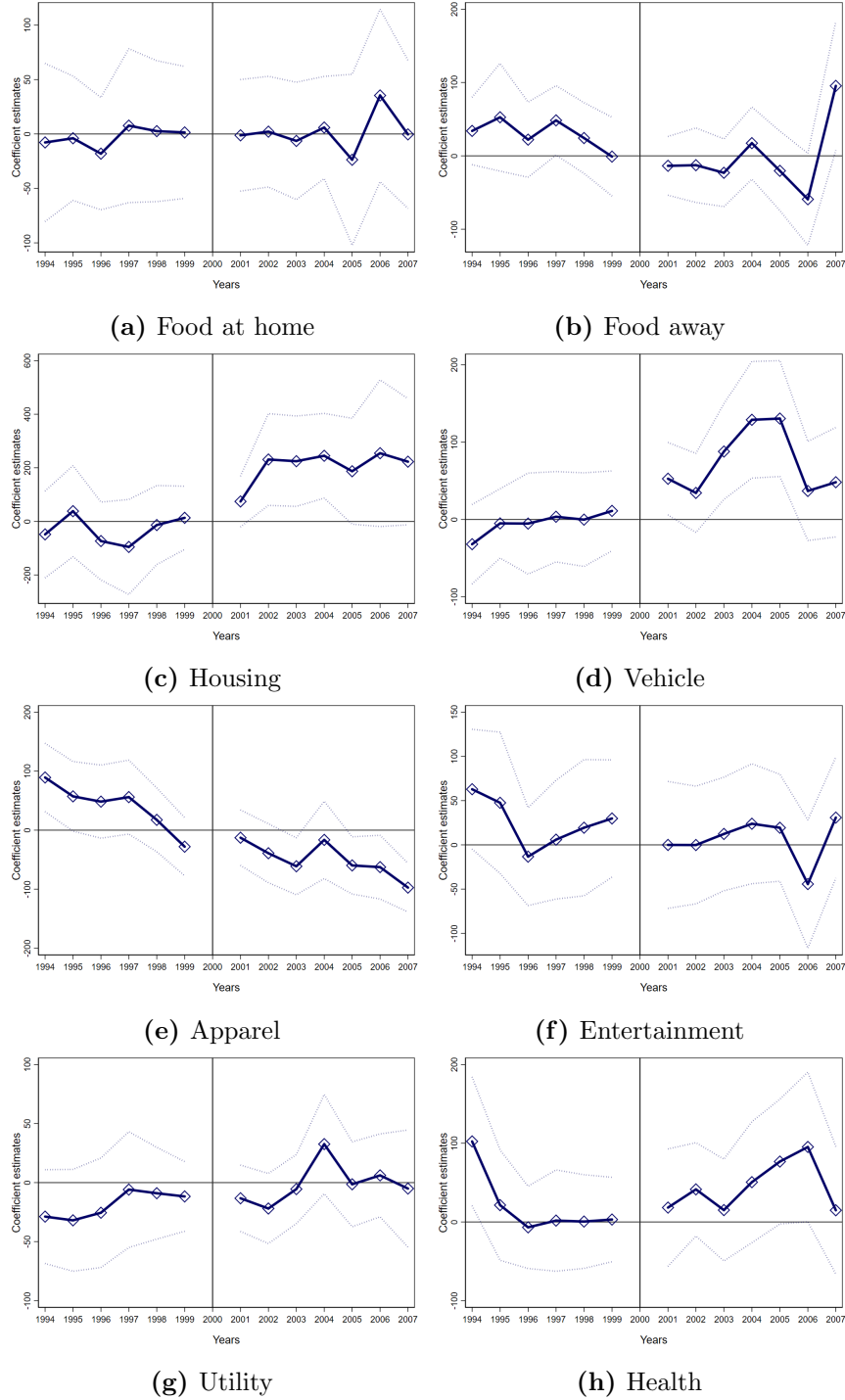


Figure 3.4: The effects of the Bush tax reforms: consumption categories

Note: Each diamond in the figure represent the estimate for β_k in equation (3.6) for $k \in 1994, \dots, 2007$ and dotted lines connect confidence intervals associated with the estimates.

Table 3.4: Consumption externalities on MTR15 households: baseline results

	MTR28		MTR31	
	(1)	(2)	(3)	(4)
$Post \times Intensity$	29.96 (20.28)	33.52* (19.79)	108.0** (42.92)	116.6*** (41.48)
Macro controls	Yes	Yes	Yes	Yes
Income polynomial	1st	6th	1th	6th
Observations	190929	190929	190929	190929
R^2	0.584	0.600	0.584	0.600

Note: Standard errors are clustered at the state level and given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

I begin by estimating the following difference-in-differences specification:

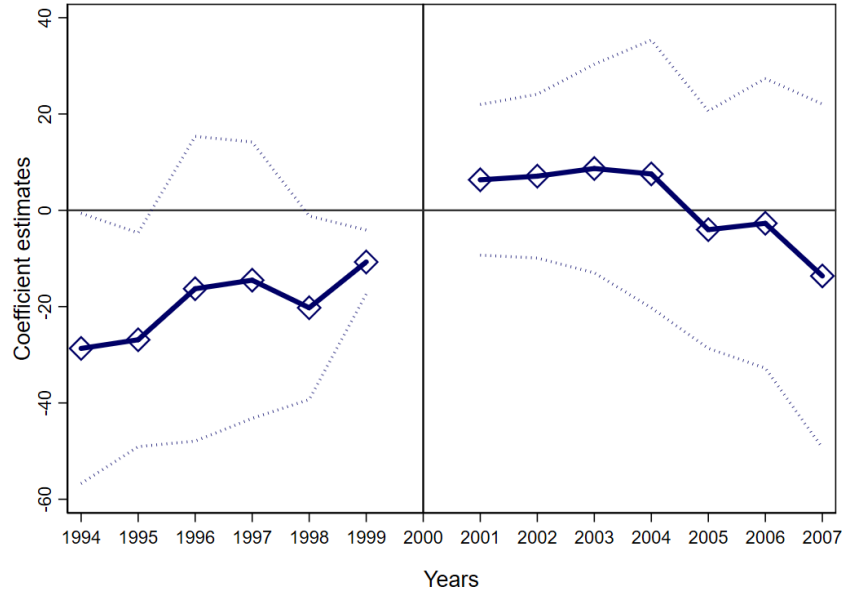
$$C_{i,s,t} = \alpha + \beta Post_t \times Intensity_s + \gamma X_{i,t} + \delta Z_{s,t} + \varepsilon_{i,s,t}. \quad (3.7)$$

where $Intensity_s$ is either the MTR28 share or the MTR31 share of state s . Unlike equation (3.5), I control for after-tax household income so that I can separate the effect of additional income which might be generated by the tax reforms from the externality effect. All other arguments are the same as before. For the purposes of this analysis, I restrict the sample to MTR15 households. I weight each observation by state-year cell size following the recommendation of Solon et al. [96].⁸

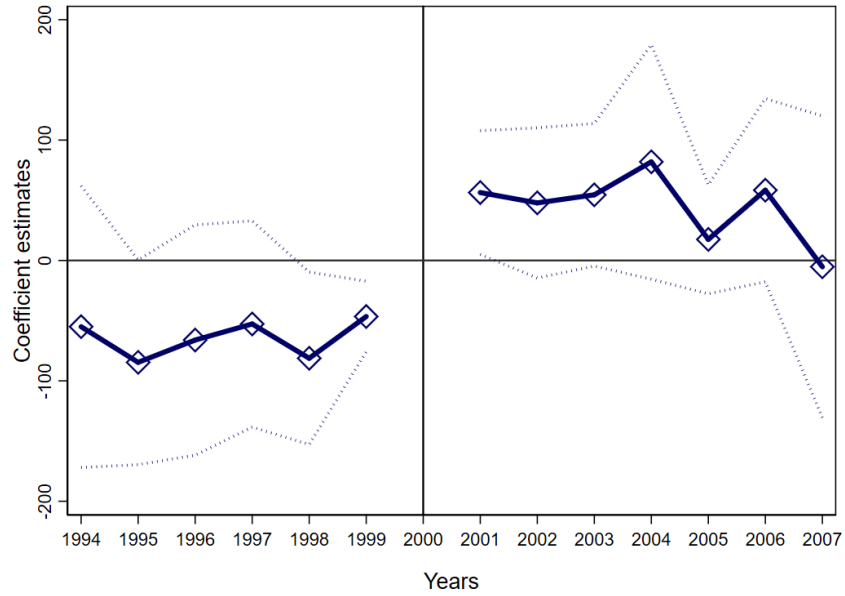
Table 3.4 summarizes estimation results. When I use the MTR28 share in column (1), the estimate is positive, but not statistically significant. Incorporating a more flexible before-tax income control improves the precision of the estimation in column (2). When evaluating the estimate at the mean, it implies that total quarterly consumption of MTR15 households increased by \$1,006 ($=33.52 \times 30$). Given that MTR28 households raised their spending by slightly more than \$500 after the tax reforms, the estimate is implausibly large.

In column (3) and column (4), I use the state-share of MTR31 as an alternative externality intensity measure. When I only control for after-tax household income linearly, the estimate is 108, implying that MTR15 households spent \$410.40 ($=108 \times 3.8$) after the

⁸Solon et al. [96] suggests that when the variance of group specific errors is small and the sample size of a unit of variation is highly variable and small in some units, weighting by the sample size of the unit can lead to more precise estimates.



(a) MTR28 share



(b) MTR31 share

Figure 3.5: Consumption externalities on MTR15 households: total consumption

Note: Each diamond in the figure represent the estimate for the treatment effect and dotted lines connect confidence intervals associated with the estimates.

tax reforms due to externalities from upper-income households. It is worth mentioning that the estimates presented in the previous section difference out the catching-up responses of MTR15 households in the post-reform period. Furthermore, the true effect of the tax reforms on MTR28 consumption would likely be higher after accounting for the underreporting in consumption of higher-income households in the CE data. Thus, I believe that the estimate given in column (3) is fairly reasonable. Once I replace a linear after-tax income control with a 6th order polynomial in column (4), the estimate becomes slightly larger and statistically significant at the 1 percent level.

I further explore the dynamic effects of consumption externalities using the event study framework, similar to equation (3.6). I present the results in Figure 3.5. Consistent with the results from static model estimation, none of estimates in the post-reform period are significant when I measure the externality intensity using the MTR28 share. On the other hand, confidence intervals for most estimates in the post-reform period indicate that households in states with a larger share of higher-income households spent more than their counterparts after the tax reforms.

The event study framework is also useful to check the validity of the identifying assumption. If indeed consumption externalities generated by the tax reforms drive the finding, coefficient estimates for year-intensity interaction terms in the pre-reform period should not be different from zero. However, both figures in Figure 3.5 raise concerns that there might be other factors that influence the consumption behaviors of households living in different states, even in the absence of the tax reforms. The figures suggest that MTR15 households who were more likely to encounter richer households spent less prior to the tax reforms, after controlling for other factors. To identify potential drivers of the pre-treatment effect, I estimate the event study specification for each consumption category. This exercise will also confirm whether consumption patterns across MTR28 and MTR15 households are consistent with the consumption externalities narrative. My preferred specifications use the MTR31 share to measure externality intensity. I present results from specifications that use the MTR28 share in Figure C.A.1 in the Appendix.

Figure 3.6 shows household consumption dynamics for specific items before and after the tax reforms. Figure 3.6c and Figure 3.6d suggest that the growth in total consumption of

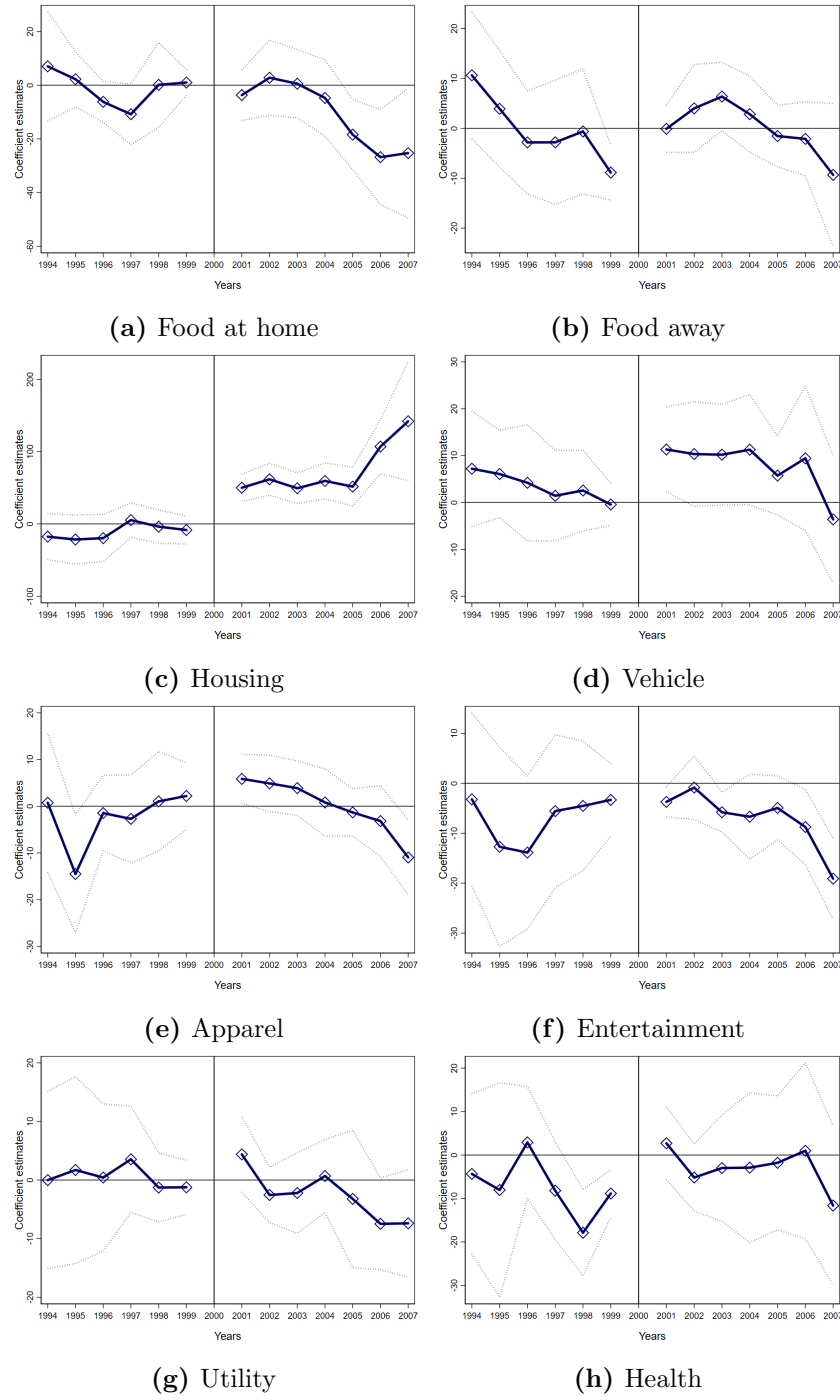


Figure 3.6: Consumption externalities on MTR15 households: consumption categories

Note: Each diamond in the figure represents the estimate for the treatment effect and dotted lines connect confidence intervals associated with the estimates. The intensity of consumption externalities is measured by the MTR31 share.

MTR15 households after the tax reforms is attributable to increases in housing and vehicle consumption, which are two items that led to higher consumption of MTR28 households. The symmetry is interesting, given that two analyses use different sources of identifying variation. However, the symmetry does not hold in food-at-home consumption. Figure 3.4a indicates that food-at-home consumption of MTR28 households was not affected by the tax reforms, whereas Figure 3.6a shows that MTR15 households who received stronger exposure to MTR28 consumption reduced food consumption at home.

The expenditure cascades hypothesis proposed by Frank et al. [38] not only states that consumption externalities trickle down from the high-income earners to the middle class, it also suggests that the impact of such externalities should be stronger for households who are closer to the reference households that generate the externalities. The effect of consumption of high-income would diminish as it moves down the income distribution. Now I test the existence of the gradient effect by estimating the following reduced-form equation:

$$C_{i,s,t} = \alpha + \sum_{k=1}^4 \beta_k Post_t \times Intensity_s \times Q_i^k + \gamma X_{i,t} + \delta Z_{s,t} + \varepsilon_{i,s,t}. \quad (3.8)$$

where Q^k takes 1 if household i belongs to the k th income quartile in a given year, and 0 otherwise. The expenditure cascades hypothesis implies that $Q^1 < Q^2 < Q^3 < Q^4$.

Table 3.5 summarizes key results from the analysis. Panel A presents coefficient estimates for each three-way interaction term and Panel B presents p-values for tests of equality across the β s. Column (1) estimates the baseline specification given in equation (3.8). Column (2) allows for the effect of the MTR31 share on household consumption to differ across income quartiles. Lastly, column (3) estimates a fully-specified triple difference model. In general, the estimates are larger for MTR15 households in a higher quartile, which is the population that is closer to the upper-income households on the income scale. This evidence strongly supports the expenditure cascades hypothesis. The gradient in the magnitude of the externality effect is stark in column (3) where the t-tests clearly reject the null hypotheses $H_0 : \beta_j = \beta_u$ for all $j \neq u$ at the 5 percent level.

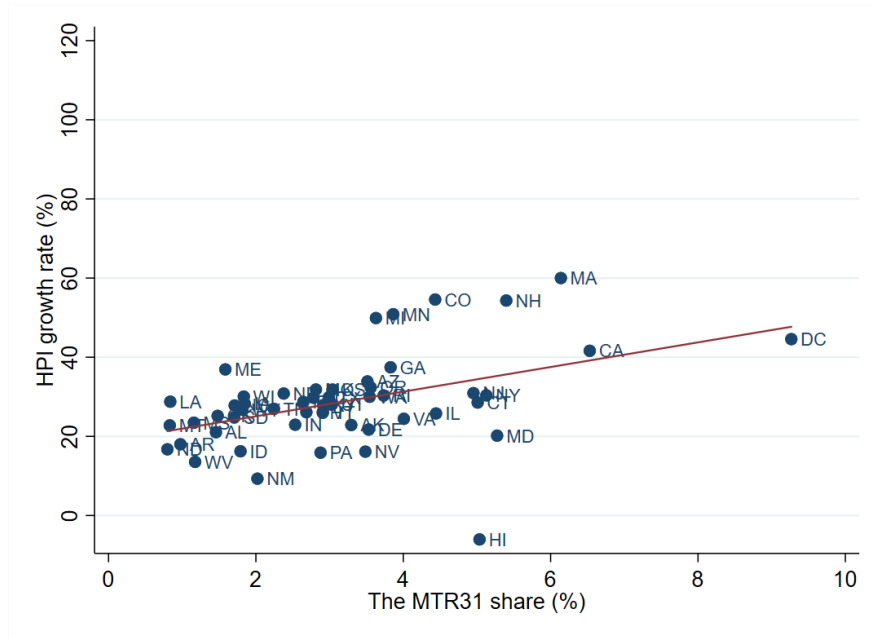
Table 3.5: Expenditure cascades

	(1)	(2)	(3)
Panel A: Estimates for βs			
$Post \times Intensity \times Q^1$	66.04** (32.63)	103.6*** (37.72)	4.759 (12.13)
$Post \times Intensity \times Q^2$	89.05** (35.83)	100.3** (38.55)	89.02** (34.58)
$Post \times Intensity \times Q^3$	123.8*** (40.91)	111.3*** (39.16)	140.1*** (48.79)
$Post \times Intensity \times Q^4$	180.5** (70.40)	147.1** (59.69)	235.6*** (81.94)
Panel B: Tests of equality across βs			
$\beta_4 = \beta_3$	0.0877	0.1897	0.0164
$\beta_3 = \beta_2$	0.0220	0.2846	0.0336
$\beta_2 = \beta_1$	0.2454	0.8468	0.0023
Intensity-quartile interactions	No	Yes	Yes
Post-quartile interactions	No	No	Yes
Observations	190929	190929	190929
R^2	0.602	0.602	0.602

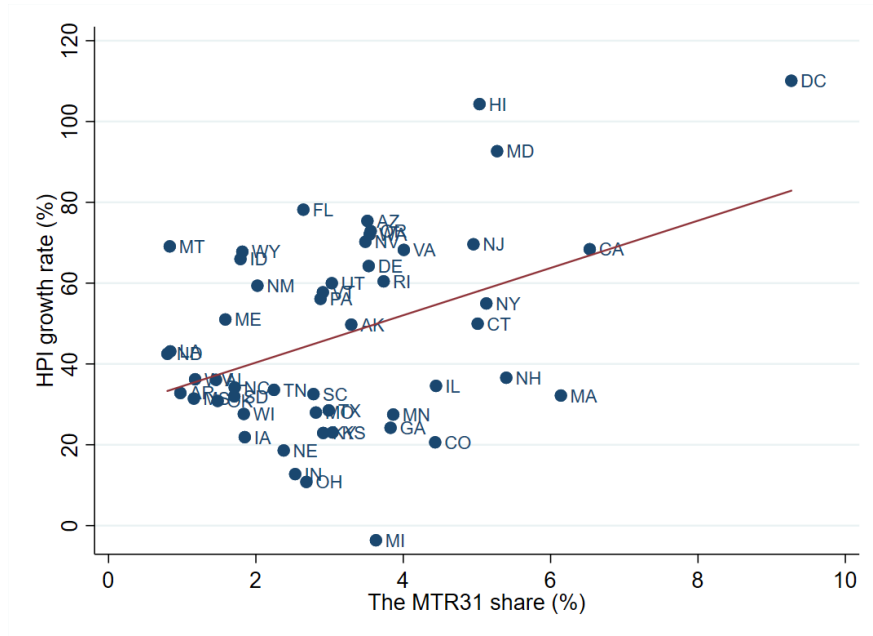
Note: Standard errors are clustered at the state level and given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

VII Prices or Quantities?

The evidence that I present in the previous section relies on the assumption that the MTR 31 share, which is the preferred measure of consumption externalities, is uncorrelated with other unobserved characteristics. However, event study estimates presented in Figure 3.5 suggest that the assumption might not hold. Among many possibilities, I examine the role of local prices in explaining different post-reform consumption behaviors across states. If there exists heterogeneity in local inflation, it would influence household spending differently even when they consume the same amount of goods and services [87, 10]. Indeed, Cooper [24] documents that the correlation between real consumption growth and real house price growth is about 0.7 during the 2000s. Since a household is unlikely to buy a house as a result of consumption externalities generated by the tax reforms, the local inflation channel can be one alternative mechanism that drives my findings.



(a) Pre-reform home price growth



(b) Post-reform home price growth

Figure 3.7: Correlation between the home price growth and the MTR31 share

Note: Pre-form home price growth is the rate of growth in the state-level single-family housing price index between 1994 to 2000. Post-form home price growth is the rate of growth in the same index between 2001 to 2007.

Figure 3.7 plots the unconditional correlation between the home price growth rate and the MTR 31 share for pre-reform and post-reform periods, respectively. In both periods, home prices in states with a greater share of MTR 31 grew faster than those in states with a lower MTR 31 share. However, the heterogeneity in the home price growth rate is much stronger in the post-reform period. See the cases of California, the District of Columbia, and Massachusetts, for example. While I control for the state-level housing price index in all specifications, it would not fully account for the time-varying variation in the home price growth rates across states. Separating the local inflation effect from the externality effect is left for future work.

VIII Discussion

This paper documents empirical evidence of consumption externalities trickling down from upper-income households to low-to-middle-income households using a series of tax reforms during the Bush Administration as a natural experiment. I first show that the tax reforms spurred consumption of households with higher tax liabilities. I also show that households with low tax liabilities living in states that feature a greater share of households with higher tax liabilities increased their consumption more substantially than their counterparts after the tax reforms. Finally, I find that the effect of consumption externalities was stronger in households whose income levels were similar to the reference households.

The existence of consumption externalities along the income distribution has an important policy implication. More than a decade ago, Frank [36] claims that tax reforms on the top-income earners would encourage them to spend more on positional goods and this in turn would generate so-called positional externalities to the middle class. Unless household income does not rise accordingly, such externalities would hurt the middle class by making them spend too much on visible goods and too less on necessities. I find that all households raised their spending on housing and vehicles after the tax reforms, but only low-to-middle-income households reduced their food-at-home consumption. The evidence

implies that government intervention designed to mitigate consumption externalities (e.g., raising taxes on positional goods) could improve social welfare.

The paper is not without limitations. The biggest threat to internal validity of my findings would be state-level changes in goods prices occurring concurrently with the tax reforms. In particular, the period that I analyze is exactly coincident with the remarkable changes in housing prices in the United States. Separating the externality effects from the price effect or the housing wealth effect is easier said than done. I acknowledge these concerns and will address them in future research.

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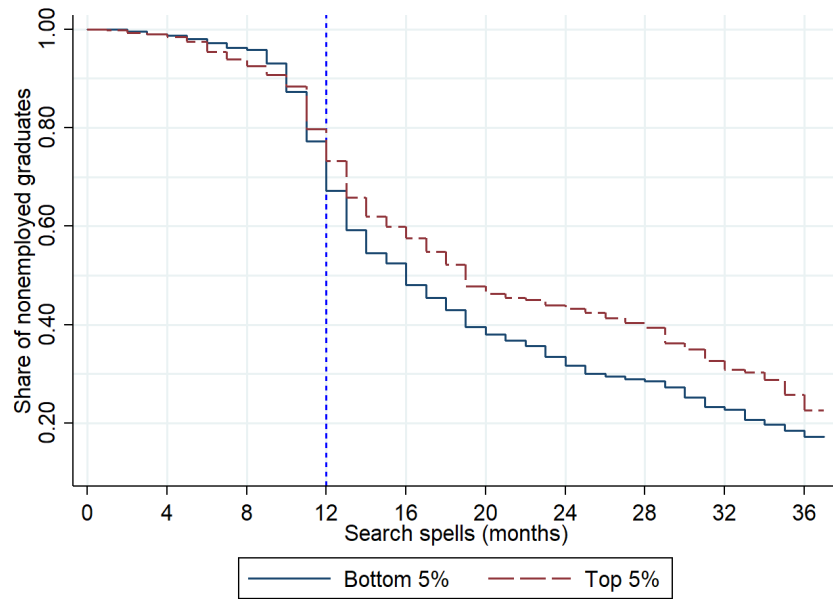
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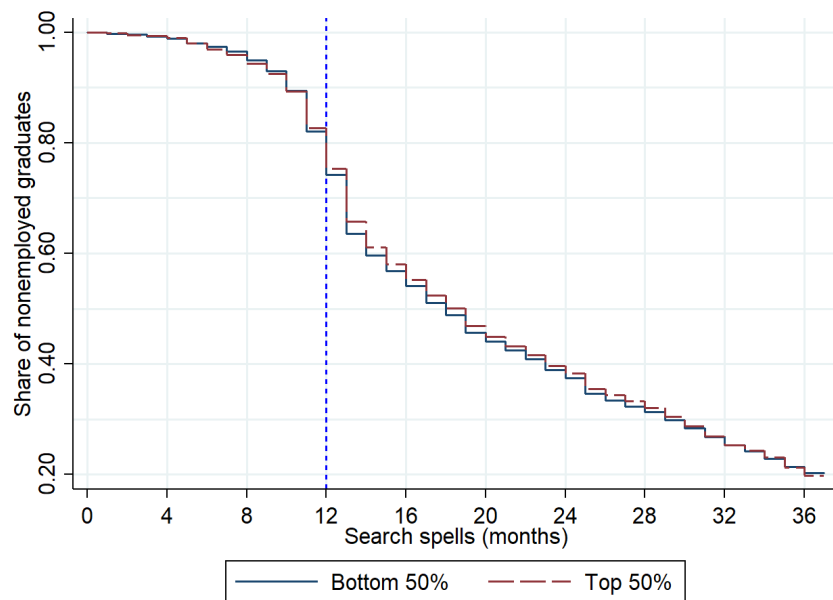
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Appendices

A Appendix for Chapter 1



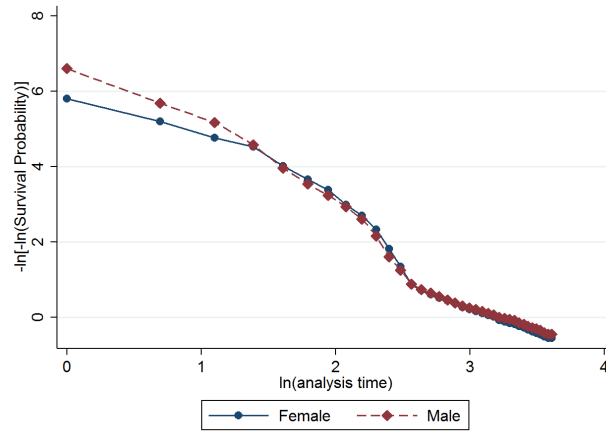
(a) Top 5percent vs. Bottom 5percent



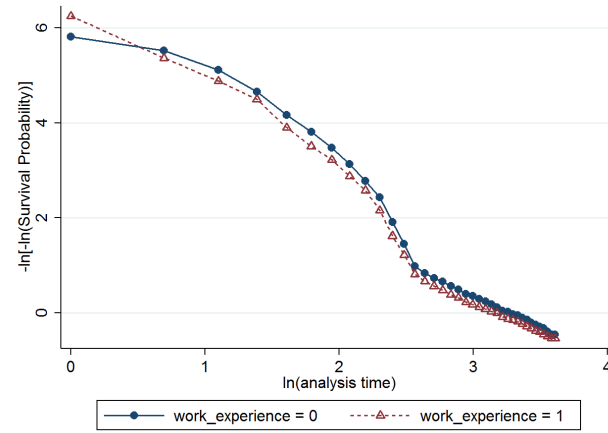
(b) Top 50percent vs. Bottom 50percent

Figure A.1: Kaplan-Meier survival curves

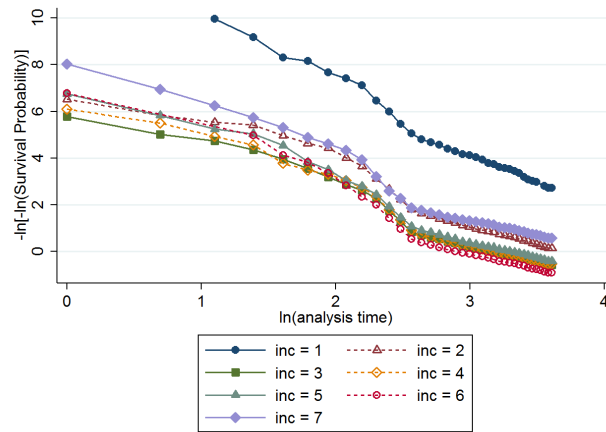
Figure A.2: Graphical tests of the proportionality assumption: Log-log plots



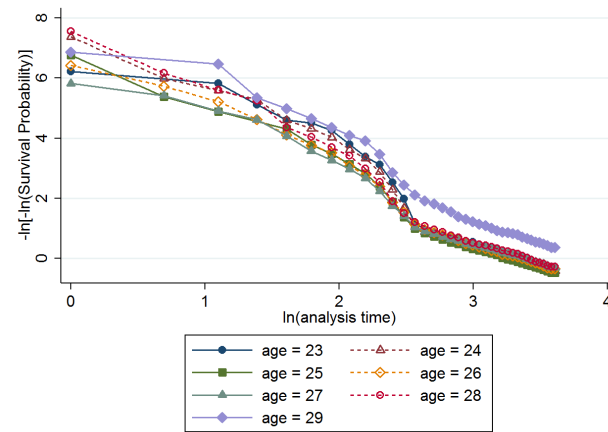
(c) Sex



(d) Working experience

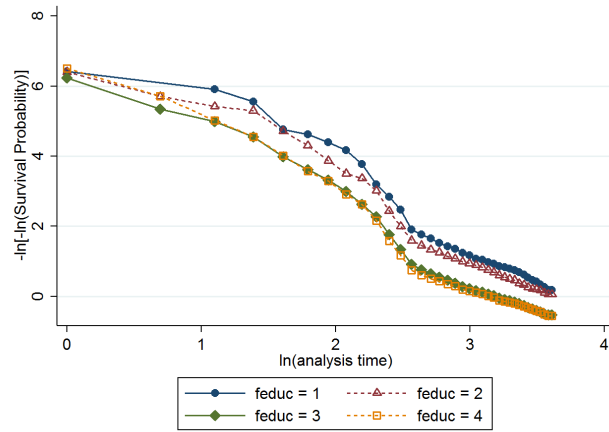


(e) Parental income

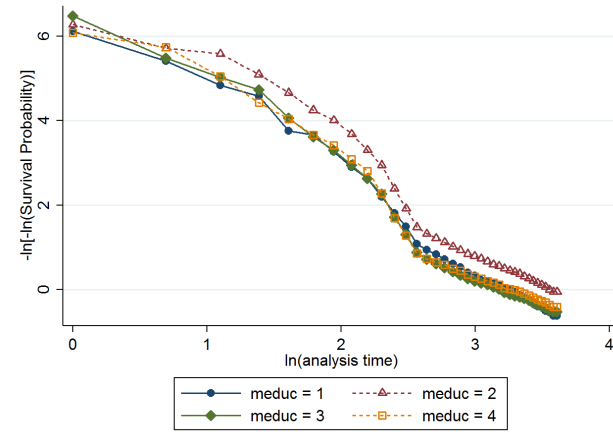


(f) Age

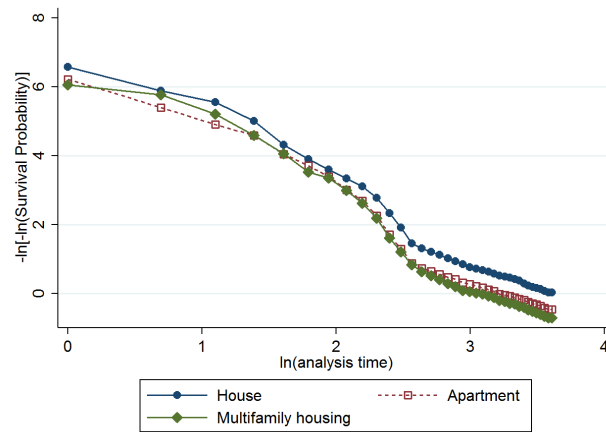
Figure A.2: Continued



(g) Fathers' education



(h) Mothers' education



(i) Housing type

Figure A.2: Continued

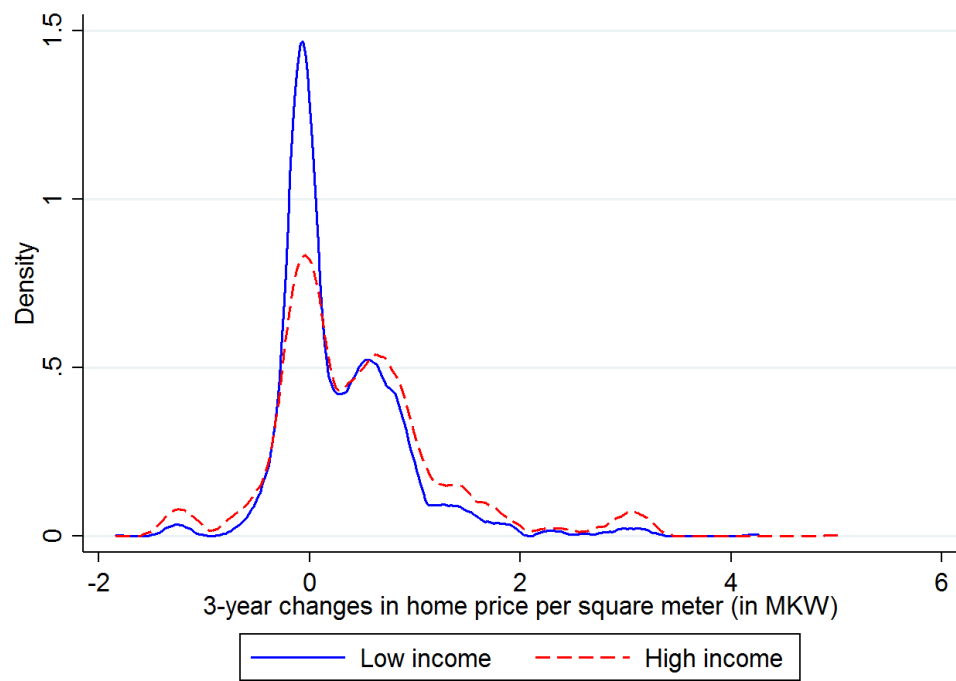


Figure A.3: The distribution of 3-year home price (KMW/m^2) changes by income groups

Table A.1: Variable definitions

Name	Description
Panel A: Control variables	
Male	A binary indicator for male individuals.
Age	Age at the time of the initial survey.
Major	Numeric codes for detailed college major identifier provided by the GOMS data. The Cox PH model in equation (1.8) is stratified by 121 unique college major categories.
Number of household members	The number of household members living together, including himself/herself.
Rank	The ranking of college from which an individual graduated.
Time of graduation	Dummy variables for each graduation year-month (from August 2007 to February 2010).
Parental income (<i>Inc</i>)	Dummy variables for parental income categories at the time of college entrance described in Table 1.1.
Parental education	Dummy variables for final education of each parent (less than middle school, middle school, high school, vocational college or more).
Parental occupations	Dummy variables for occupation of each parent at the time of college entrance based on Korean Standard Classification of Occupations (9 categories).
Home ownership	An indicator of parental home ownership that takes 1 if parents own a home, 0 otherwise.
Housing types (<i>Type</i>)	Dummy variables for parental housing types (house, apartment, multi-family residential housing).
GPA	A grouped GPA measure based on 20 quantiles (e.g., 1 = 0 to 5 percentiles of the GPA distribution in a given graduation year).
Loan take-up	An indicator of individuals who took out loans to pay for college.
Dual degree	An indicator of dual degree earners.
TOEIC	Scores for the test of English for International Communication (TOEIC).
I(<i>Toeic</i> > 0)	An indicator of individuals who had ever took the TOEIC test.
Working experience	An indicator of individuals who had ever worked during college years.
Study abroad	An indicator of individuals who had ever participated in language programs in other countries.
Qualification	An indicator of individuals who had earned at least one qualification.
High school types	Dummy variables for seven types of high schools
Panel B: Variables used in housing wealth imputation	
<i>Location</i>	Dummy variables for municipality of residence of households.
<i>Edu</i>	Final education of a parent who are more educated.
<i>Year</i>	Dummy variables for survey years of the Korea Housing Survey.

B Appendix for Chapter 2

B.A Log-log specifications, balanced panel, state-specific time trends

Table B.A.1: The effects of 1033 program on police spending: FE model, balanced panel, state-specific time trends

	(1)	(2)	(3)	(4)	(5)	(6)
$\ln(\text{Item value per capita}_{t-1})$	0.010 (0.012)	0.009 (0.012)	0.018* (0.010)		0.019 (0.014)	0.013 (0.013)
$\ln(\text{Item value per capita}_{t-2})$			-0.001 (0.016)			
$\ln(\text{Item value per capita}_{t-3})$			0.037** (0.017)			
$\ln(\text{Item value per capita}_{t+1})$				-0.003 (0.012)		
County characteristics	✓	✓	✓	✓	✓	✓
Crime controls		✓	✓	✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓		✓
Year fixed effects					✓	✓
Observations	10998	10998	7332	9776	10998	10998
R^2	0.124	0.125	0.061	0.132	0.055	0.130

Note: This analysis uses the balanced panel described in Table 2.1. The dependent variable is logged police protection expenditure of county governments per capita. We control for county-level time-varying characteristics such as median household incomes, male shares, shares of the population aged from 15 to 24, racial diversity, poverty rates, and unemployment rates in all specifications. In column (2) to column (6), we add arrest rates for the following crime types: murder, rape, robbery, aggravated assault, burglary, and assault. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.A.2: The effects of 1033 program on police spending: FE-IV model, balanced panel, state-specific time trends

	(1)	(2)	(3)	(4)
Panel A: First-stage				
$\ln(Value_{t-1} \times Land_j)$	0.021*** (0.005)	0.020*** (0.005)	0.019*** (0.006)	0.018*** (0.007)
Panel B: Second-stage				
$\ln(Item\ value\ per\ capita_{t-1})$	-0.069 (0.265)	-0.070 (0.274)	-0.320 (0.336)	-0.161 (0.380)
Kleibergen-Paap LM statistic	18.843	17.769	9.363	6.955
Kleibergen-Paap Wald F statistic	19.047	17.934	9.421	6.986
Endogeneity Test	0.766	0.773	0.289	0.612
Sample	Full	Full	$0.1 \leq p$	$0.15 \leq p$
County characteristics	✓	✓	✓	✓
Crime controls		✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓
Observations	10998	10998	8385	6878
R^2	-0.005	-0.005	-0.137	-0.047

Note: Column (1) and column (2) use the full balanced sample. Column (3) and column (4) restrict the sample based on the likelihood of receiving any items through the 1033 program, p . Panel A summarizes the first-stage estimates where the dependent variable is the value of items that a county-level agency has received through 1033 program. $Value_{t-1}$ is the total value of 1033 items released by the Department of Defense in a year $t - 1$. $Land_j$ is the land area of county j . Panel B summarizes the second-stage results where the dependent variable is logged police protection expenditure of county governments per capita. The Kleibergen-Paap Wald F statistic shows the relevance of the instruments. The Kleibergen-Paap LM statistic is used to test whether a model is under-identified. We also test the null hypothesis that the item value can actually be treated as exogenous using a GMM distance test proposed by [8] and present the results. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.A.3: The heterogeneous effects of 1033 program on police spending: by item types, balanced panel, state-specific time trends

	(1)	(2)	(3)	(4)
	Vehicle		Non-vehicle	
	FE	FE-IV	FE	FE-IV
$\ln(\text{Vehicle item value per capita}_{t-1})$	0.018*	-0.019		
	(0.011)	(0.781)		
$\ln(\text{Non-vehicle item value per capita}_{t-1})$			-0.072	0.111
			(0.052)	(0.365)
Kleibergen-Paap LM statistic		2.063		149.778
Kleibergen-Paap Wald F statistic		2.054		173.569
Endogeneity test		0.080		0.385
Observations	10998	10998	10998	10998
R^2	0.125	-0.001	0.125	-0.003

Note: We use the balanced panel and estimate the baseline equation (2.1) using both FE and FE-IV models. In column (1) and column (2), the dependent variable is the logged value of vehicle items that a county has received through 1033 program. In column (3) and column (4), the dependent variable is the logged value of non-vehicle items. The FE-IV models use the value-land interaction to instrument for the item value. See the note in Table 2.4 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.A.4: The heterogeneous effects of 1033 program on police spending: by political preferences, balanced panel, state-specific time trends

	(1)	(2)	(3)	(4)
	Republican		Democrat	
	FE	FE-IV	FE	FE-IV
$\ln(\text{Item value per capita}_{t-1})$	0.018 (0.017)	-0.083 (0.270)	-0.007 (0.014)	-0.060 (0.758)
Kleibergen-Paap LM statistic		13.609		3.590
Kleibergen-Paap Wald F statistic		13.778		3.570
Endogeneity test		0.710		0.944
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓
Observations	5499	5499	5499	5499
R^2	0.123	-0.009	0.153	-0.002

Note: We split the balanced panel into two groups based on political preferences of counties and estimate the baseline equation (2.1) using both FE and FE-IV models. The sample is restricted to counties whose Democrat vote shares for 2008 U.S. presidential election are less than or equal to the median of the balanced panel (“Republican”) in the first two columns and to counties whose Democrat vote shares are greater than the median (“Democrat”) in the last two columns. The dependent variable is logged police protection expenditure of county governments per capita. The FE-IV models use the value-land interaction to instrument for the item value. See the note in Table 2.4 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.A.5: The heterogeneous effects of 1033 program on police spending: by population size, balanced panel, state-specific time trends

	(1)	(2)	(3)	(4)
	Small		Large	
	FE	FE-IV	FE	FE-IV
$\ln(\text{Item value per capita}_{t-1})$	0.004 (0.016)	0.363 (0.334)	0.013 (0.012)	-0.959 (0.618)
Kleibergen-Paap LM statistic		11.954		5.878
Kleibergen-Paap Wald F statistic		12.060		5.867
Endogeneity test		0.261		0.049
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓
Observations	5445	5445	5553	5553
R^2	0.145	-0.108	0.127	-0.785

Note: We split the balanced panel into two groups based on the population size of counties and estimate the baseline equation (2.1) using both FE and FE-IV models. The sample is restricted to counties whose population sizes are less than or equal to the mean of the balanced panel (“Small”) in the first two columns and to counties whose population sizes are greater than the median (“Large”) in the last two columns. The dependent variable is logged police protection expenditure of county governments per capita. The FE-IV models use the value-land interaction to instrument for the item value. See the note in Table 2.4 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.B Log-log specifications, unbalanced panel, year fixed effects

Table B.B.1: The effects of 1033 program on police spending: FE model, unbalanced panel, year fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
$\ln(\text{Item value per capita}_{t-1})$	0.001 (0.009)	0.001 (0.009)	0.010 (0.009)		0.001 (0.009)	0.001 (0.009)
$\ln(\text{Item value per capita}_{t-2})$			0.012 (0.014)			
$\ln(\text{Item value per capita}_{t-3})$			0.044*** (0.014)			
$\ln(\text{Item value per capita}_{t+1})$				0.003 (0.010)		
County characteristics	✓	✓	✓	✓	✓	✓
Crime controls		✓	✓	✓	✓	✓
State-specific linear time trends					✓	✓
Year fixed effects	✓	✓	✓	✓		✓
Observations	17539	17539	9069	13101	17539	17539
R^2	0.045	0.046	0.006	0.048	0.046	0.046

Note: This analysis uses the unbalanced panel described in Table 2.1. The dependent variable is logged police protection expenditure of county governments per capita. We control for county-level time-varying characteristics such as median household incomes, male shares, shares of the population aged from 15 to 24, racial diversity, poverty rates, and unemployment rates in all specifications. In column (2) to column (6), we add arrest rates for the following crime types: murder, rape, robbery, aggravated assault, burglary, and assault. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.B.2: The effects of 1033 program on police spending: FE-IV model, unbalanced panel, year fixed effects

	(1)	(2)	(3)	(4)
Panel A: First-stage				
$\ln(Value_{t-1} \times \frac{1}{Dist_j})$	-0.077*** (0.020)	-0.076*** (0.019)	-0.087*** (0.021)	-0.095*** (0.023)
Panel B: Second-stage				
$\ln(Item\ value\ per\ capita_{t-1})$	0.475 (0.332)	0.450 (0.339)	0.232 (0.295)	0.365 (0.291)
Kleibergen-Paap LM statistic	14.801	14.422	18.596	20.563
Kleibergen-Paap Wald F statistic	15.700	15.432	17.454	17.021
Endogeneity Test	0.147	0.181	0.440	0.227
Sample	Full	Full	$0.1 \leq p$	$0.15 \leq p$
County characteristics	✓	✓	✓	✓
Crime controls		✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	17532	17532	13041	9615
R^2	(0.332)	(0.339)	(0.295)	(0.291)

Note: Column (1) and column (2) use the full unbalanced sample. Column (3) and column (4) restrict the sample based on the likelihood of receiving any items through the 1033 program, p . Panel A summarizes the first-stage estimates where the dependent variable is the value of items that a county-level agency has received through 1033 program. $Value_{t-1}$ is the total value of 1033 items released by the Department of Defense in a year $t-1$. $Dist_j$ measures distance between the centroid of county j and the closest Field Activity Center from the county. Panel B summarizes the second-stage results where the dependent variable is logged police protection expenditure of county governments per capita. The Kleibergen-Paap Wald F statistic shows the relevance of the instruments. The Kleibergen-Paap LM statistic is used to test whether a model is under-identified. We also test the null hypothesis that the item value can actually be treated as exogenous using a GMM distance test proposed by [8] and present the results. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.B.3: The heterogeneous effects of 1033 program on police spending: by item types, unbalanced panel, year fixed effects

	(1)	(2)	(3)	(4)
	Vehicle		Non-vehicle	
	FE	FE-IV	FE	FE-IV
<i>Vehicle item value per capita</i> $t-1$	0.005 (0.008)	0.626* (0.380)		
<i>Non – vehicle item value per capita</i> $t-1$			-0.039 (0.034)	0.641 (5.768)
Kleibergen-Paap LM statistic		16.250		0.230
Kleibergen-Paap Wald F statistic		17.126		0.243
Endogeneity test		0.100		0.904
Observations	17539	17532	17539	17532
R^2	0.046	-0.320	0.046	-0.048

Note: We use the unbalanced panel and estimate the baseline equation (2.1) using both FE and FE-IV models. In column (1) and column (2), the dependent variable is the logged value of vehicle items that a county has received through 1033 program. In column (3) and column (4), the dependent variable is the logged value of non-vehicle items. The FE-IV models use the value-distance interaction to instrument for the item value. See the note in Table 2.4 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.B.4: The heterogeneous effects of 1033 program on police spending: by political preferences, unbalanced panel, year fixed effects

	(1)	(2)	(3)	(4)
	Republican		Democrat	
	FE	FE-IV	FE	FE-IV
$\ln(\text{Item value per capita}_{t-1})$	-0.000 (0.011)	0.308 (0.638)	0.001 (0.014)	0.520 (0.357)
Kleibergen-Paap LM statistic		3.744		16.507
Kleibergen-Paap Wald F statistic		4.735		9.752
Endogeneity test		0.625		0.157
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	10741	10734	6798	6798
R^2	0.046	-0.093	0.051	-0.189

Note: We split the unbalanced panel into two groups based on political preferences of counties and estimate the baseline equation (2.1) using both FE and FE-IV models. The sample is restricted to counties whose Democrat vote shares for 2008 U.S. presidential election are less than or equal to the median of the balanced panel (“Republican”) in the first two columns and to counties whose Democrat vote shares are greater than the median (“Democrat”) in the last two columns. The dependent variable is logged police protection expenditure of county governments per capita. The FE-IV models use the value-distance interaction to instrument for the item value. See the note in Table 2.4 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.B.5: The heterogeneous effects of 1033 program on police spending: by population size, unbalanced panel, year fixed effects

	(1)	(2)	(3)	(4)
	Small		Large	
	FE	FE-IV	FE	FE-IV
$\ln(\text{Item value per capita}_{t-1})$	-0.002 (0.011)	-0.001 (1.538)	-0.005 (0.012)	0.148 (0.390)
Kleibergen-Paap LM statistic		0.422		13.994
Kleibergen-Paap Wald F statistic		0.472		11.399
Endogeneity test		0.999		0.695
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	10289	10282	7250	7250
R^2	0.060	0.000	0.032	-0.015

Note: We split the unbalanced panel into two groups based on the population size of counties and estimate the baseline equation (2.1) using both FE and FE-IV models. The sample is restricted to counties whose population sizes are less than or equal to the mean of the balanced panel (“Small”) in the first two columns and to counties whose population sizes are greater than the median (“Large”) in the last two columns. The dependent variable is logged police protection expenditure of county governments per capita. The FE-IV models use the value-distance interaction to instrument for the item value. See the note in Table 2.4 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.C Log-log specifications, balanced panel, year fixed effects

Table B.C.1: The effects of 1033 program on police spending: FE model, balanced panel, year fixed effects

	(1)	(2)	(3)	(4)
$\ln(\text{Item value per capita}_{t-1})$	0.019 (0.014)	0.019 (0.014)	0.016 (0.011)	
$\ln(\text{Item value per capita}_{t-2})$			0.004 (0.018)	
$\ln(\text{Item value per capita}_{t-3})$			0.049*** (0.018)	
$\ln(\text{Item value per capita}_{t+1})$				-0.000 (0.013)
County characteristics	✓	✓	✓	✓
Crime controls		✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	10998	10998	7332	9776
R^2	0.053	0.055	0.007	0.059

Note: This analysis uses the balanced panel described in Table 2.1. The dependent variable is logged police protection expenditure of county governments per capita. We control for county-level time-varying characteristics such as median household incomes, male shares, shares of the population aged from 15 to 24, racial diversity, poverty rates, and unemployment rates in all specifications. In column (2) to column (4), we add arrest rates for the following crime types: murder, rape, robbery, aggravated assault, burglary, and assault. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.C.2: The effects of 1033 program on police spending: FE-IV model, balanced panel, year fixed effects

	(1)	(2)	(3)	(4)
Panel A: First-stage				
$\ln(Value_{t-1} \times \frac{1}{Dist_j})$	-0.082*** (0.019)	-0.081*** (0.019)	-0.084*** (0.021)	-0.089*** (0.022)
Panel B: Second-stage				
$\ln(Item\ value\ per\ capita_{t-1})$	0.396 (0.331)	0.370 (0.338)	0.168 (0.329)	0.232 (0.358)
Kleibergen-Paap LM statistic	21.517	21.375	21.513	20.900
Kleibergen-Paap Wald F statistic	17.661	17.546	16.097	15.838
Endogeneity Test	0.262	0.307	0.644	0.549
Sample	Full	Full	$0.1 \leq p$	$0.15 \leq p$
County characteristics	✓	✓	✓	✓
Crime controls		✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	10998	10998	8477	6449
R^2	-0.111	-0.096	-0.022	-0.051

Note: Column (1) and column (2) use the full balanced sample. Column (3) and column (4) restrict the sample based on the likelihood of receiving any items through the 1033 program, p . Panel A summarizes the first-stage estimates where the dependent variable is the value of items that a county-level agency has received through 1033 program. $Value_{t-1}$ is the total value of 1033 items released by the Department of Defense in a year $t-1$. $Dist_j$ measures distance between the centroid of county j and the closest Field Activity Center from the county. Panel B summarizes the second-stage results where the dependent variable is logged police protection expenditure of county governments per capita. The Kleibergen-Paap Wald F statistic shows the relevance of the instruments. The Kleibergen-Paap LM statistic is used to test whether a model is under-identified. We also test the null hypothesis that the item value can actually be treated as exogenous using a GMM distance test proposed by [8] and present the results. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.C.3: The heterogeneous effects of 1033 program on police spending: by item types, balanced panel, year fixed effects

	(1)	(2)	(3)	(4)
	Vehicle		Non-vehicle	
	FE	FE-IV	FE	FE-IV
<i>Vehicle item value per capita</i> $t-1$	0.027** (0.013)	0.504 (0.386)		
<i>Non – vehicle item value per capita</i> $t-1$			-0.063 (0.057)	0.681 (2.176)
Kleibergen-Paap LM statistic		24.505		2.108
Kleibergen-Paap Wald F statistic		17.700		2.357
Endogeneity test		0.229		0.729
Observations	10998	10998	10998	10998
R^2	0.055	-0.162	0.055	-0.055

Note: We use the balanced panel and estimate the baseline equation (2.1) using both FE and FE-IV models. In column (1) and column (2), the dependent variable is the logged value of vehicle items that a county has received through 1033 program. In column (3) and column (4), the dependent variable is the logged value of non-vehicle items. The FE-IV models use the value-distance interaction to instrument for the item value. See the note in Table 2.4 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.C.4: The heterogeneous effects of 1033 program on police spending: by political preferences, balanced panel, year fixed effects

	(1)	(2)	(3)	(4)
	Republican		Democrat	
	FE	FE-IV	FE	FE-IV
$\ln(\text{Item value per capita}_{t-1})$	0.020 (0.020)	0.183 (0.821)	0.012 (0.017)	0.489 (0.343)
Kleibergen-Paap LM statistic		4.154		21.030
Kleibergen-Paap Wald F statistic		5.058		9.089
Endogeneity test		0.843		0.175
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	5499	5499	5499	5499
R^2	0.065	-0.023	0.047	-0.142

Note: We split the balanced panel into two groups based on political preferences of counties and estimate the baseline equation (2.1) using both FE and FE-IV models. The sample is restricted to counties whose Democrat vote shares for 2008 U.S. presidential election are less than or equal to the median of the balanced panel (“Republican”) in the first two columns and to counties whose Democrat vote shares are greater than the median (“Democrat”) in the last two columns. The dependent variable is logged police protection expenditure of county governments per capita. The FE-IV models use the value-distance interaction to instrument for the item value. See the note in Table 2.4 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.C.5: The heterogeneous effects of 1033 program on police spending: by population size, balanced panel, year fixed effects

	(1)	(2)	(3)	(4)
	Small		Large	
	FE	FE-IV	FE	FE-IV
$\ln(\text{Item value per capita}_{t-1})$	0.022 (0.020)	0.004 (1.497)	0.000 (0.014)	0.016 (0.383)
Kleibergen-Paap LM statistic		0.635		22.760
Kleibergen-Paap Wald F statistic		0.661		13.404
Endogeneity test		0.991		0.968
County characteristics	✓	✓	✓	✓
Crime controls	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	5445	5445	5553	5553
R^2	0.072	0.000	0.041	-0.000

Note: We split the balanced panel into two groups based on the population size of counties and estimate the baseline equation (2.1) using both FE and FE-IV models. The sample is restricted to counties whose population sizes are less than or equal to the mean of the balanced panel (“Small”) in the first two columns and to counties whose population sizes are greater than the median (“Large”) in the last two columns. The dependent variable is logged police protection expenditure of county governments per capita. The FE-IV models use the value-distance interaction to instrument for the item value. See the note in Table 2.4 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.D Level-level specifications estimates

Table B.D.1: FE model estimates with state-specific time trends

	Unbalanced Sample				Balanced Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Item value per capita</i> $t-1$	0.047 (0.200)	0.036 (0.200)	-0.062 (0.141)	0.090 (0.210)	0.062 (0.201)	0.094 (0.217)	0.091 (0.212)	0.008 (0.167)
<i>Item value per capita</i> $t-2$			0.215 (0.298)					-0.010 (0.231)
<i>Item value per capita</i> $t-3$			1.339*** (0.429)					1.590*** (0.579)
County characteristics	✓	✓	✓	✓	✓	✓	✓	✓
Crime controls		✓	✓	✓	✓		✓	✓
State-specific linear time trends	✓	✓	✓		✓	✓	✓	✓
Year fixed effects				✓	✓			
Observations	17539	17539	9069	17539	17539	10998	10998	7332
R^2	0.033	0.034	0.054	0.020	0.036	0.074	0.077	0.067

Note: Column (1) to column (5) in this table correspond to column (1) to column (5) in Table 2.3, except that the police expenditure and the item value are given in level terms. From column (6) to column (8), we re-estimate specifications given in column (1) to column (3) using balanced panel. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.D.2: FE-IV model estimates with state-specific time trends

	Unbalanced Sample				Balanced Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Item value per capita</i> $t-1$	-4.746 (7.369)	-4.640 (7.464)	-12.010 (9.606)	-14.724 (9.446)	-6.627 (10.138)	-5.858 (10.245)	-8.595 (10.353)	-7.771 (6.823)
County characteristics	✓	✓	✓	✓	✓	✓	✓	✓
Crime controls		✓	✓	✓		✓	✓	✓
State-specific linear time trends	✓	✓	✓	✓	✓	✓	✓	✓
Kleibergen-Paap LM statistic	4.748	4.664	4.364	6.956	2.130	1.928	2.233	8.186
Kleibergen-Paap Wald F statistic	4.746	4.659	4.358	6.970	2.122	1.920	2.223	8.235
Endogeneity Test	0.498	0.515	0.127	0.061	0.463	0.528	0.323	0.209
Sample	Full	Full	$0.1 \leq p$	$0.15 \leq p$	Full	Full	$0.1 \leq p$	$0.15 \leq p$
Observations	17532	17532	13627	10618	10998	10998	8390	6570
R^2	-0.044	-0.042	-0.313	-0.332	-0.173	-0.135	-0.332	-0.159

Note: Column (1) to column (4) in this table correspond to column (1) to column (4) in Table 2.4, except that the police expenditure and the item value are given in level terms. From column (5) to column (6), we re-estimate specifications given in column (1) to column (4) using the balanced panel. Our instrument in this analysis is the interaction between $Value_{t-1}$ and $\ln(land_j)$. See the note in Table 2.4 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.D.3: FE model estimates with year fixed effects

	Unbalanced Sample			Balanced Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Item value per capita</i> $t-1$	0.099 (0.208)	0.090 (0.210)	-0.037 (0.145)	0.148 (0.233)	0.146 (0.230)	0.010 (0.168)
<i>Item value per capita</i> $t-2$			0.362 (0.302)			0.122 (0.241)
<i>Item value per capita</i> $t-3$			1.536*** (0.469)			1.772*** (0.640)
County characteristics	✓	✓	✓	✓	✓	✓
Crime controls		✓	✓		✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Observations	17539	17539	9069	10998	10998	7332
R^2	0.019	0.020	0.009	0.043	0.045	0.012

Note: Column (1) to column (3) in this table correspond to the column (1) to column (3) in Table B.B.1, except that the police expenditure and the item value are given in level terms. From column (4) to column (6), we re-estimate specifications given in column (1) to column (3) using the balanced panel. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.D.4: FE-IV model estimates with year fixed effects

	Unbalanced Sample				Balanced Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Item value per capita</i> $t-1$	0.781 (8.242)	0.083 (8.975)	3.389 (7.989)	1.149 (6.743)	1.641 (7.122)	1.438 (7.220)	3.509 (5.949)	4.744 (6.333)
County characteristics	✓	✓	✓	✓	✓	✓	✓	✓
Crime controls		✓	✓	✓		✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Kleibergen-Paap LM statistic	7.500	6.877	6.959	13.746	11.646	11.641	11.331	13.079
Kleibergen-Paap Wald F statistic	7.985	7.412	7.853	11.819	10.403	10.256	9.502	9.431
Endogeneity Test	0.934	0.999	0.682	0.879	0.834	0.858	0.581	0.494
Sample	Full	Full	$0.1 \leq p$	$0.15 \leq p$	Full	Full	$0.1 \leq p$	$0.15 \leq p$
Observations	17532	17532	13627	10618	10998	10998	8390	6570
R^2	-0.001	0.000	-0.024	-0.002	-0.008	-0.006	-0.046	-0.045

Note: Column (1) to column (4) in this table correspond to column (1) to column (4) in Table B.B.2, except that the police expenditure and the item value are given in level terms. From column (5) to column (6), we re-estimate specifications given in column (1) to column (4) using the balanced panel. Our instrument in this analysis is the interaction between $Value_{t-1}$ and $\frac{1}{Dist_j}$. See the note in Table 2.4 for descriptions of test statistics. All dollars are inflation-adjusted using CPI-U-RS and expressed in 2012 U.S. dollars. Standard errors are clustered at the government level and given in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Appendix for Chapter 3

C.A Figures and Tables

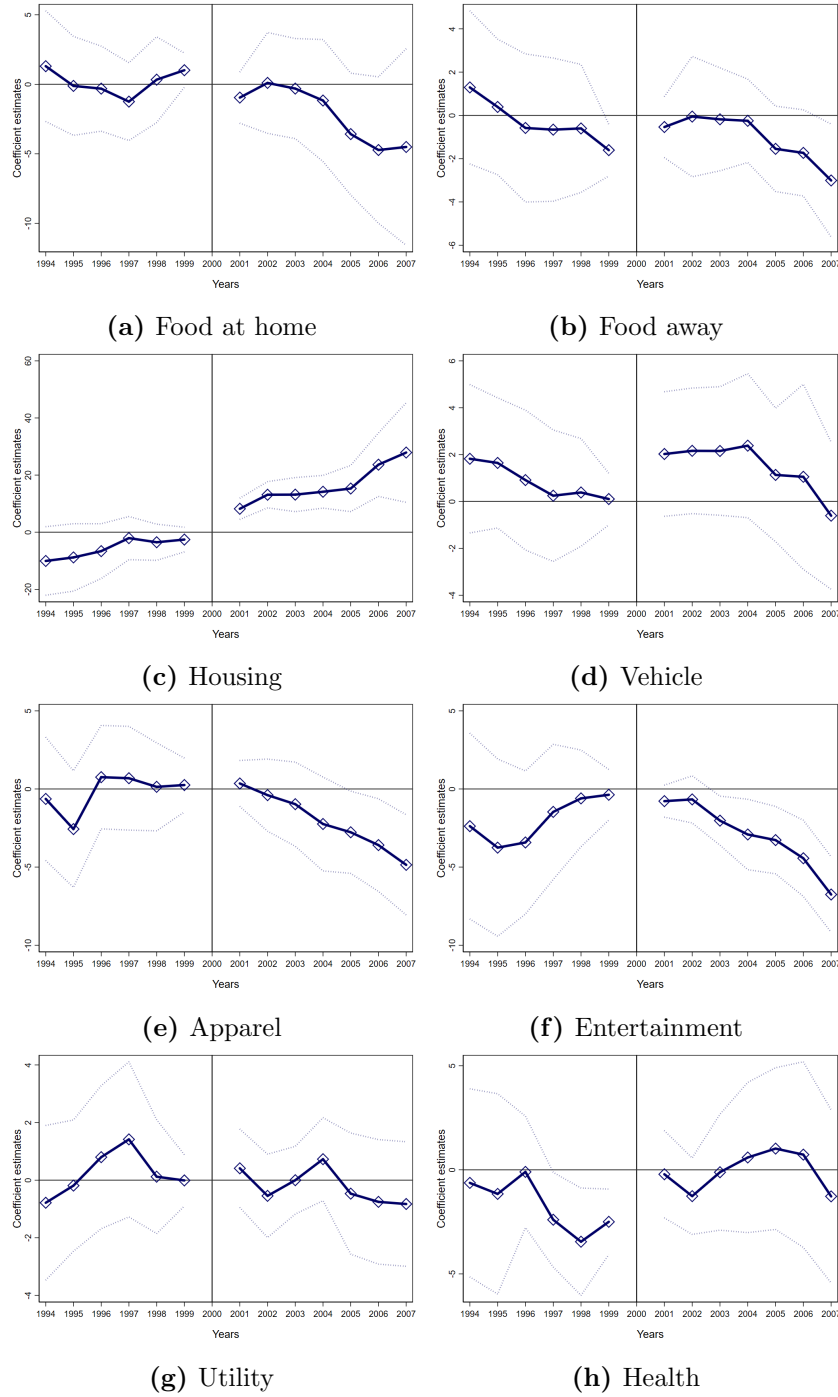


Figure C.A.1: Consumption externalities on MTR15 households: evidence from an alternative externality measure
Note: Each diamond in the figure represent the estimate for the treatment effect and dotted lines connect confidence intervals associated with the estimates. The intensity of consumption externalities is measured by the MTR28 share.

Table C.A.1: The effect of tax reforms on consumption of MTR28 households: by consumption category

	(1)	(2)	(3)	(4)
	Food at home	Food away	Housing	Vehicle
$Post \times MTR28$	3.855 (10.49)	-71.18** (28.04)	579.4*** (89.39)	88.53*** (20.56)
Observations	243551	202478	243613	211835
R^2	0.283	0.119	0.363	0.255
	(5)	(6)	(7)	(8)
	Apparel	Entertainment	Utility	Health
$Post \times MTR28$	-61.78*** (19.64)	-2.332 (27.67)	26.43*** (5.248)	-25.37 (21.30)
Observations	203422	220595	242627	202228
R^2	0.054	0.053	0.313	0.097

Note: Standard errors are clustered at the state level and given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Source: Consumer Expenditure survey data, 1994-2007, and the author's calculation.

Table C.A.2: The effect of tax reforms on consumption of MTR15 households: by consumption category

	(1)	(2)	(3)	(4)
	Food at home	Food away	Housing	Vehicle
$Post \times MTR28$	-2.882 (10.75)	3.604 (4.102)	81.22*** (12.47)	12.94*** (3.258)
Observations	189912	156134	184502	165512
R^2	0.281	0.126	0.286	0.234
	(5)	(6)	(7)	(8)
	Apparel	Entertainment	Utility	Health
$Post \times MTR28$	3.306* (1.892)	0.576 (3.445)	-0.278 (2.307)	5.303 (5.478)
Observations	157209	171307	189226	156217
R^2	0.133	0.150	0.328	0.0977

Note: Standard errors are clustered at the state level and given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Source: Consumer Expenditure survey data, 1994-2007, and the author's calculation.

C.B Identifying MTR28 and MTR15

The CPS contains information on individual taxable income (TAXINC) and household tax filing status (FILESTAT). To create a household taxable income variable, I sum taxable income of all individuals who have the same household identifier (SERIAL). Next, I convert the nominal household taxable income into 2013 U.S. dollars using CPI-U-RS. Finally, I determine whether a household is MTR28 or MTR15 based on taxable income and tax filing status of the household. If a household had taxable income greater than the maximum upper bound for 15% bracket as of 2000, then the household is MTR28. If a household had taxable income equal to or less than the minimum lower bound for 15% bracket as of 2000, then the household is MTR15. Table C.B.1 provides lower bounds and upper bounds for each tax bracket by tax filing status from 1994 to 2007.

Table C.B.1: Upper/lower bounds of federal tax liabilities at the 15% bracket, 1994 to 2007

Year	Married filing jointly	Single	Head of household
1994	8553.88	5121.08	6865.62
1995	8572.56	5132.55	6869.04
1996	8585.80	5138.63	6883.63
1997	8634.75	5166.18	6926.66
1998	8753.57	5239.74	7017.32
1999	8716.44	5213.66	6995.42
2000	8590.54	5142.57	6886.15
2001	8613.21	5154.59	6907.72
2002	8007.46	4866.36	6397.84
2003	9562.92	4781.46	6368.15
2004	9525.59	4762.80	6340.47
2005	9423.98	4711.99	6275.93
2006	9414.83	4707.42	6269.12
2007	9515.79	4757.89	6332.11
Maximum upper bound	9562.92	5239.74	7017.32
Minimum lower bound	8007.46	4707.42	6269.12

Note: All figures are adjusted for inflation using CPI-U-RS and presented in 2013 U.S. dollars.

Source: www.taxfoundation.org and the authors calculation.

Unlike the CPS, the CE does not contain information on household tax filing status. Thus, I have to impute tax filing status of households using information on family type (FAM

TYPE) and the composition of earners (EARNCOMP). Table C.B.2 provides description of the variables.

Table C.B.2: Codes for family type (FAM TYPE) and composition of earners (EARNCOMP)

Code	Family type
1	Husband and wife (H/W) only
2	H/W, own children only, oldest child under 6 years old
3	H/W, own children only, oldest child 6 to 17 years old
4	H/W, own children only, oldest child over 17 years old
5	All other H/W CUs
6	One parent, male, own children only, at least one child age under 18 years old
7	One parent, female, own children only, at least one child age under 18 years old
8	Single persons
9	Other CUs
Code	Composition of earners
1	Reference person only
2	Reference person and spouse
3	Reference person, spouse and others
4	Reference person and others
5	Spouse only
6	Spouse and others
7	Others only
8	No earners

Source: Interview Data Dictionary of the Consumer Expenditure Survey.

I assume that the tax filing status of households was married filing jointly if the family type of a household is coded 1, 2, 3, 4, or 5. Exceptions are the cases where 1) family type code is 3, 4, or 5, and 2) a household member other than the reference person and spouse works. Together with households whose family type is either 6 or 7, I code tax filing status for those households as head of household. Lastly, I code tax filing status for single-person households as single.

Vita

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